

DEMOGRAPHIC DESTINIES

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

PAA Oral History Project

Volume 1--Presidents

Number 3--From 1977 through 1993

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	
26	1962-63	Kingsley Davis	4/26/79, Philadelphia, Lunde and Ferriss; 5/1/89, Stanford, VDT	
27	1963-64	Donald J. Bogue	3/30/89, Baltimore, VDT	
28	1964-65	Ronald Freedman	4/56/79, Philadelphia, Lunde; 6/12/89, Ann Arbor, VDT	
29	1965-66	Calvin F. Schmid	No interview	
30	1966-67	Paul C. Glick	5/9/89, Phoenix, VDT	
31	1967-68	Ansley J. Coale	4/27/79, Philadelphia, Lunde; 5/11/88, Princeton, VDT	
32	1968-69	Otis Dudley Duncan	5/3/89, Santa Barbara, VDT	
33	1969-70	Everett S. Lee	6/28/79, Athens, GA, Ferriss	
34	1970-71	Nathan Keyfitz	12/31/88, Jarkata, Indonesia (self-interview re VDT)	
35	1971-72	Amos Hawley	4/6/88, Chapel Hill, VDT	
36	1972-73	Norman B. Ryder	5/11/88, Princeton, VDT	
37	1973-74	Arthur Campbell	2/16/88, Washington, DC, VDT	

38	1974-75	Charles F. Westoff	5/10/88, Princeton, VDT	
39	1975-76	Sidney Goldstein	12/14/89, Providence, RI, VDT	
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40	1977	Evelyn Kitagawa	No interview	
41	1978	Richard Easterlin	5/4/89, Los Angeles, VDT	6
42	1979	Charles B. Nam	4/22/88, New Orleans, VDT	31
43	1980	Jacob S. Siegel	6/21/88, Washington, DC, VDT	62
44	1981	Judith Blake	5/4/89, Los Angeles, VDT	94
45	1982	John F. Kantner	3/22/88, Bedford, PA, VDT	123
46	1983	George F. Stolnitz	1/20/88, Washington, DC, VDT	141
47	1984	Samuel H. Preston	6/14/88, Philadelphia, VDT	161
48	1985	Jane A. Menken	6/13/88, Philadelphia, VDT	195
59	1986	Paul Demeny	6/8/88, New York, VDT	217
50	1987	Ronald D. Lee	4/28/89, Berkeley, VDT	249
51	1988	Reynolds Farley	2/4/89, Washington, DC, VDT	284
52	1989	Harriett B. Presser	11/15/89, College Park, MD, VDT	324
53	1990	Larry L. Bumpass	3/21/91, Washington, DC, VDT	375
54	1991	Ronald R. Rindfuss	5/1/92, Denver, VDT	398
55	1992	Etienne van de Walle	2/17/93, Philadelphia, VDT	426
56	1993	Albert Hermalin	2/17/93, Philadelphia, VDT	461

RICHARD A. EASTERLIN

PAA President in 1978 (No. 41). Interview with Jean van der Tak in Dr. Easterlin's office at the Department of Economics, University of Southern California, Los Angeles, May 4, 1989.

CAREER HIGHLIGHTS: Born in New Jersey, Richard Easterlin received an M.E. (Mechanical Engineer) in 1945 from the Stevens Institute of Technology, and the A.M. in economics in 1949 and Ph.D. in economics in 1953 from the University of Pennsylvania. He was on the faculty at the University of Pennsylvania from 1953 to 1982 as Professor of Economics and, at various times, Chairman of the Department of Economics. Since 1982 he has been Professor of Economics at the University of Southern California in Los Angeles. From 1955 to 1962, he was also on the research staff of the National Bureau of Economic Research. He has been a visiting professor at Stanford and at the University of Washington and a fellow at the Center for Advanced Study in the Behavioral Sciences. He was president of the Economic History Association in 1979-80. Among his many publications, he is author of The American Baby Boom in Historical Perspective (1961), Population, Labor Force and Long Swings in Economic Growth: The American Experience (1968), Birth and Fortune: The Impact of Numbers on Personal Welfare (1980 and 1987), and coauthor of Population Redistribution and Economic Growth, United States, 1870-1950, Volumes 1 (1957) and 2 (1960), American Economic Growth: An Economist's History of the United States (1972), and The Fertility Revolution: A Supply-Demand Analysis (1985).

VDT [continuing "Career Highlights"]: Richard Easterlin's name has become a household word in U.S. demography, thanks to two models or frameworks he devised to explain trends in fertility and population growth. He is the progenitor of the "Easterlin hypothesis," the idea that the relative size of a cohort determines that cohort's life chances and, in turn, its fertility, which sets off self-generating waves of high and low fertility, which he feels have characterized the U.S. since at least the mid-19th century, through the post-World War II baby boom and bust, and the rising fertility he expects about now [late 1980s]. He has honed that hypothesis in many influential publications, beginning with his famous 1961 article, "The American Baby Boom in Historical Perspective" [American Review, December 1961], through his provocative 1978 PAA presidential address, "What Will 1984 be Like? Socioeconomic Implications of Recent Twists in Age Structure" [published in Demography, November 1978], and on to the popular bestseller, Birth and Fortune, of 1980, with a second edition in 1987.

In addition, he and his wife and research partner, Eileen Crimmins, are now also well known for the "Easterlin synthesis framework," or Easterlin-Crimmins model, which combines economic and sociological approaches to study the shift from high to low fertility as a country modernizes. The Fertility Revolution: A Supply-Demand Analysis, published in 1985, is one of their latest publications along this avenue of research.

Now, to begin at the beginning, how did you become interested in demography? You were a forerunner as a population economist, sort of in the tradition of Malthus and Spengler. They are still fairly rare in the field of demography.

EASTERLIN: I was recruited really by Simon Kuznets. He and Dorothy Thomas at the University of Pennsylvania had gotten funds from the Rockefeller Foundation for a project on population redistribution and economic growth in the United States, which aimed to mobilize economic and demographic data from census volumes, by state, since 1870. For workers on that project, they enlisted myself, Everett Lee, Ann Miller, and Carol Brainerd. That was 1953; right at the time I got my Ph.D.

VDT: Your Ph.D. was not yet in the direction of demography?

EASTERLIN: No, it was not. And actually, it was only in Kuznets's course that I got any exposure to population as a graduate student, and even then it was not very formal demography. So my real training in demography, such as it is, was as a worker on that project, and very largely the result of Dorothy Thomas's tutelage.

VDT: Had you had courses from her as a student?

EASTERLIN: No. She really didn't teach courses particularly in demography; she mainly ran a research seminar. Dorothy's forte was always the sort of one-on-one situation. She had a regular social setup, so a lot of what I learned about the field of demography came out of the course of conversations around coffee tables.

VDT: I've always heard that Dorothy Thomas mothered her students, the graduate students that were around. So in the course of conversation at these coffee klatches, she got you interested in demography?

EASTERLIN: Right. She guided me to the classics in the field and the great names. She got me to go to the meetings of the Population Association and attend the sessions and get acquainted with people, which I probably would not have done had it not been for her support and encouragement, because at the time there were hardly any economists at all in the Population Association. So, aside from the problem of my being a very junior person in the field, there was this additional problem of being in a different discipline and talking almost a different language. But as a result of her encouragement, I got to know people. I do the same now with my students. I make sure they go to the PAA meetings, try to give papers, and so on.

VDT: So you and Everett Lee and Ann Miller and others were taken on for this project with Simon Kuznets and Dorothy Thomas, funded by the Rockefeller Foundation, which came out eventually in two volumes, 1957 and 1960, on Population Redistribution and Economic Growth in the United States: 1870 to 1950. You wrote the chapters on manufacturing?

EASTERLIN: And income; I did the income estimates for the states of the United States.

VDT: You began working on that from 1953?

EASTERLIN: That's right. Ultimately, there were three volumes. The final volume [1964] was by Dorothy and Hope Eldridge, on migration in the United States.

VDT: Let's talk about some of the people who were at Penn at that time. You've mentioned Dorothy Thomas, who obviously was a great influence on you. You dedicated your 1978 PAA presidential address to her; she had died not long before then. And you've mentioned Simon Kuznets. What about John Durand; where does he come in?

EASTERLIN: I sort of recruited John to come to Penn. I'd known his work, of course, and had met him, but really I had not had much contact with John before the time that I recruited him, which I think was around 1960 or 61, when I was department chairman; the dates are a little hazy now. [Easterlin was chair of Penn's Department of Economics in 1958-60, 1961-62, and 1968. John Durand was

Professor of Economics and Sociology and director of Penn's Population Studies Center from 1965 to 1979.]

John, I think, felt that the United Nations was going in a different direction, and Dorothy and he had always been good friends. So I went to New York and talked to him about coming to Penn. He was head of the United Nations Population Division there and I asked him about giving that up and coming to Penn as professor of economics. Ultimately we worked it out and he came and then took over the directorship of the Population Studies Center at Penn.

It was after he came that I really had a lot of contact with him personally, in seminars and in supervising students. I learned a lot from John, in much the same way as I did from Dorothy, just sort of working firsthand with him. Those two people were the ones that for me were the role models of what great demographers should be. It involved a combination of both careful attention to data but also a concern about interpretation of the data--going beyond description and trying to get at explanation.

VDT: That's what the greats in the field seem to have been so good at. Kingsley Davis, among others, who always stresses that you have to talk about the causes, the background, to changes in fertility or whatever.

EASTERLIN: Yes.

VDT: Among others there at the time, what about Hope Eldridge? There's the sad story about Hope, who was hounded out of the United Nations during the McCarthy era [early 1950s]. I hadn't realized she'd come to Penn after that.

EASTERLIN: Yes. Dorothy was always a very loyal friend, and it was because of Dorothy that she came to Penn, first as a coworker on the project and eventually--I'm not sure what type of appointment she held, but she was teaching in sociology. I think she was quite happy. The situation that was worked out was great for Hope, because she loved to do research. I think her husband was on the faculty at Temple, so they were happy in Philadelphia; they had a pleasant setup. I think it was Dorothy's loyalty that really was responsible for sort of salvaging what could have been a very distressing ending to Hope's career.

VDT: Did she leave Penn before she had to retire? I had the impression they went south.

EASTERLIN: Yes, they did. I'm a little hazy now, but they did retire. I think Hope died shortly thereafter--I can't remember whether it was Hope or her husband who died shortly thereafter. It was a bit the same as in John's situation, because he retired and died within about a year or two. [Durand retired from Penn in 1979; died in 1981.]

VDT: Yes. Fortunately he was interviewed in this series for PAA, by Abbott Ferriss, who went to his retirement home [Spruce Pine, North Carolina] in 1979. It's a short interview, but we're glad to have it.

Everett Lee was a fellow student, or contemporary, of yours at that time? He's a bit older than you.

EASTERLIN: Yes. I think at the 1953 graduation ceremonies at Penn, Ph.D. degrees were awarded to Everett, Charlie Westoff, and myself. [Everett Lee received his Ph.D. from Penn in 1952. Sidney Goldstein's was the third demography-relevant Ph.D. awarded at Penn in 1953.] Charlie and I stood next to each other in line. I had very little contact with Charlie, but I'd known him as somebody else in

the area of demography at the time. It's interesting that all three of us [thinking of himself, Westoff, and Everett Lee] graduated at exactly the same time. My degree was in economics and theirs were in sociology [as was Sidney Goldstein's]. There was no demography degree at the time. That program was really set up when John Durand came to Penn, because then we had at least two senior people in economics [Easterlin and Durand] who were interested in demography, along with a good group in sociology. Then we set up the Ph.D. program in demography.

VDT: You had sort of two divisions: the Population Studies Center and then, later, the Graduate Group in Demography. It was Vincent Whitney who applied to the Ford Foundation for funding for the Population Studies Center, wasn't it?

EASTERLIN: Right, he was director of it.

VDT: John and Pat Caldwell in their book on the Ford Foundation and population [Limiting Population Growth and the Ford Foundation Contribution, 1986] point out that Penn had some problems in getting their money because they thought you were U.S.-oriented and had been focused on migration, urbanization, and labor force.

EASTERLIN: Right, that was the Dorothy Thomas heritage.

VDT: Somehow fertility wasn't in there, which, of course, was the goal of the Ford Foundation--to bring that down in the Third World. But the monies eventually did come [see Caldwell book, pages 54-55, 65-77]. What were your connections with the Population Studies Center and the Graduate Group in Demography?

EASTERLIN: I was a co-member of the Graduate Group in Demography. I gave a course in the economics of population that was taken mostly by the demography students; there were a few economics students, but most were demography students. There were the basic methods courses that were taught by the real demographers and then my course was sort of an interpretative course, that had to do with the role of economic and other factors in affecting fertility, mortality, and migration and the effects of population change on the economy and other circumstances. And a number of my Ph.D. students were in the Ph.D. program in demography. There were some in economics and there were also some in a third Ph.D. program that I was involved in, in economic history.

VDT: Who were some of your leading students?

EASTERLIN: Oh, I don't know; I haven't tabulated that.

VDT: Some who come to the top of your head.

EASTERLIN: Well, I guess in demography, Gretchen Condran, who's now at Temple, was one. And Michael Haines, who started in economic history but ended up in demography, was one. He's just written a book with Sam Preston, based on the 1900 census. The title is something like "The Fatal Years" [Fatal Years: Child Mortality in Late Nineteenth Century America, to have been published in 1990].

A number of my students have been international. Shireen Jejeebhoy went through the demography program. She's now director of research in the family planning program in India and very active in Indian demography.

VDT: I think she's a candidate for the Council of IUSSP.

EASTERLIN: That's very likely, because she's sort of at that stage in her career now. And Wongboonsin, in Thailand; I guess he's now the associate director of the population program at Chulalongkorn University.

VDT: That name sounds familiar; haven't you coauthored articles with him?

EASTERLIN: Yes. Jack Chang, who's the director of the family planning institute in Taiwan, was a Ph.D. student of mine at Penn; Lin Chen Chang is his real name. Those are some of the more prominent people, I guess, in demography.

Then in more recent years, two students in economics who have gone into demography are Morton Shapiro, who's at Williams College, and Dennis Ahlburg, who's at the University of Minnesota. They were both Penn people in economics.

One of my students here at the University of Southern California], who is just due to get her degree, is Diane Macunovich. She is going to Williams College.

VDT: She's the one you coauthored your recent PAA paper with [Easterlin and Macunovich, "Economic Status and Household Living Arrangements of Children," presented at the 1989 PAA meeting].

Now I'd like to talk about your two streams of research: the Easterlin hypothesis, which began with the U.S. experience, and your research on less developed countries.

How did you come to devise the Easterlin hypothesis? And perhaps you'd give your standard, nutshell, description of it.

EASTERLIN: I think the Easterlin hypothesis is, in a nutshell, the conception that an individual's fortunes are affected by the size of the birth rate at the time they're born. Let me add that an individual's fortunes are affected by a lot of things. My argument is that in the post-World War II period in the United States, where the birth rate varied a great deal, this phenomenon has come to be especially important in affecting a lot of outcomes of people as they've grown up, and that includes social, psychological, economic, and even political aspects of their behavior.

Let's take somebody that comes from a small-birth-rate period. This tends to mean they typically are raised in small families, so there are fewer competitors for the resources of the family, including attention and love as well as economic resources, and that tends to result in the development of more positive attitudes and greater self-confidence as they grow up. In schools, in smaller cohorts the prospects of your being in smaller classes are greater and therefore having the benefit of a greater teacher-to-student ratio. The prospects of your succeeding in extracurricular activities--making the baseball team, being a cheerleader, being the editor of a yearbook, having a lead role in a play--things like that are all greater.

VDT: Did you have all those things, coming from the "Good Times" cohort [late 1920s, 1930s]?

EASTERLIN: More or less, yes. And when you get to the labor market, again you benefit from the fact that there are fewer competitors, so that it helps your wages and employment and you move up the occupational ladder through promotion more rapidly.

So, I think the Easterlin hypothesis is that there's this whole succession of effects, which go right from almost birth, in the family, educational institutions, labor market institutions, that tends to

be beneficial to members of a small cohort and less beneficial to members of a large cohort.

VDT: How did you come to devise it? Your first article, "The American Baby Boom in Historical Perspective"--obviously you'd been thinking about it before--came out in 1961 [American Review, December 1961]. Could you see this happening already in the mid-1950s?

EASTERLIN: I think one of the advantages one has in working in social science is that you do have the benefit of personal experience. It seemed to me that the kind of argument that I was led to through the data, having to do with the scarcity of young people, was confirmed by my own experience and the experience I observed among my contemporaries. So, although the idea didn't come directly from personal experience, it was confirmed more or less by personal experience.

VDT: By the mid-1950s you already had four children; you were adding to the baby boom?

EASTERLIN: I was working at it, but I hadn't had four by then--just two.

VDT: You're thinking more of your own social experience as . . .

EASTERLIN: Yes--or an economic sense, basically.

But I never had a nicely worked out model in advance, which I then tested against the data. I had inherited from Kuznets and Dorothy Thomas this approach where one has certain theoretical ideas and one looks at the data, learns from the data, and they reshape your theoretical notions. You work back and forth between the theory and the data, using the data to guide the development of your theoretical notions.

And it was really the data that eventually led me to the relative income view, because I became increasingly disillusioned with the usual hypothesis generated by economic theory, which was that as incomes go up people want more children. I didn't disagree with that, but the argument was always based on the idea that tastes are given. And the more I looked at the data and the more I learned about the nature of the world, the more I became convinced that people's tastes themselves are shaped by social processes and that you had to build a model that took account of that process of taste formation--along with what the economists were interested in, which was what they call technically the "budget restraint."

So it was in an effort to try and reconcile the data with economic theory that I was forced eventually--and, at first, quite a bit against my will--to develop a model that built taste formation in. Even today, many economists absolutely reject the idea of trying to look at taste formation, although there is now more marginal acceptance in the field of that type of work. But back in the 1960s, it was just about verboten. And I resisted it myself for a long time.

VDT: Was that about the time that the Chicago school of household economics . . . Gary Becker . . .

EASTERLIN: Oh, yes. Gary and I were friends, quite good friends actually. We were together for a year at the National Bureau of Economic Research right around the time that I was working on the baby boom paper and he was working on the paper that he gave that was one of the first statements of the economic theory of fertility, in, I guess it was the 1960 conference of the Universities-National Bureau of Economic Research. The volume was called Demographic and Economic Change in Developed Countries.

So Gary and I used to talk a lot, and we agreed a lot. I think that we still agree a lot, because he really has now incorporated in his own work a view that, although he doesn't call it taste formation, it's

endowments, and it comes often to much the same type of conceptual view: that people in their upbringing and in their origins are shaped or molded in various ways and their ultimate behavior is an outcome of that socialization experience and the environment that they're in at a given time. But a lot of Gary's students take a much more mechanical view, in my opinion.

VDT: You mean the mechanistic, economic view that only behavior counts?

EASTERLIN: Well, it's that tastes don't change, and that seems to me to be questionable.

VDT: You're one of the economists in the field who certainly brought together the sociologists and the economists, which you and Eileen Crimmins have stressed in your Easterlin-Crimmins model. I want to ask about that in a bit, and the criticism of economists like Paul Schultz of that particular model.

It was interesting that you brought that into the Easterlin hypothesis, that your tastes are shaped by the climate, the environment, in which you grew up.

I presume that you realized that you were leading the way in one of the most popular demographic research conundrums of the postwar era--explaining the baby boom, which the demographers were rather late in admitting was taking place. Even in the late 1940s--you have a fine quote in Birth and Fortune--they were apparently saying, fertility will never rise above replacement level. And, of course, in the 1930s they hadn't foreseen that at all.

EASTERLIN: They're still saying that.

VDT: Right. What do you think about what's happening now? The U.S. total fertility rate had been stuck at 1.8, but now it's crept up to 1.9. The Population Reference Bureau in their 1989 World Population Data Sheet, which is coming out in a couple of weeks, have got the U.S. TFR at 1.9; it rounded off at 1.9.

EASTERLIN: Right, it's above 1,900 [per 1,000 women].

VDT: It's above! I thought it just rounded off to 1,900.

EASTERLIN: It's about 1,920, as I recall; they rounded it down. [2.0 in PRB 1990 Data Sheet.]

VDT: So you're being justified [in his "suggestion" in his 1978 PAA presidential address, "What Will 1984 Be Like?", that because of "amelioration in a wide variety of social, political, and economic conditions" for the smaller cohort born after 1960, compared to the preceding baby boom cohort, "there may be a substantial rise in fertility over the next decade"]. It [the fertility rate] is getting up there?

EASTERLIN: Not wholly. It's still confined to people who are 30 and over.

VDT: That's right--delayed childbirth. And those are the baby boomers, among whom, as you pointed out in the 1987 second edition of Birth and Fortune, in the extra chapter you added, all these adjustments have taken place--which you had projected already--deferred marriage, wives' increasing labor force participation, and reduced childbearing. It's all coming to pass. What do you feel accounts for, well, deferred childbearing among women over 30, who will probably not have more than two children, certainly?

EASTERLIN: Well, they may have more than two. The old business about two girls producing a third child will, I think, still operate to some extent.

But I'm still expecting fertility to turn up among people in their twenties. It's still a little early .

..

VDT: The age-specific rates? You're expecting that to happen among the baby busters, those born 1970 and later?

EASTERLIN: Right.

VDT: Well, you're standing by your hypothesis.

EASTERLIN: My expectation was that it would have started already [before 1989]. But there are two circumstances that I think have forestalled that. One is that we experienced in the early 1980s the worst postwar depression that we had.

VDT: Recession, it's always been called.

EASTERLIN: Right.

VDT: Here's an economist calling it a "depression."

EASTERLIN: Well, Frank Levy, who wrote the 1980 census monograph on income distribution [Dollars and Dreams, 1987], refers to the period since 1973 as the "quiet depression." The recovery from that depression [of the early 1980s] was really fairly slow. Even by 1985 and 86, we still had relatively high unemployment rates, and it's only been in 1987, 88, and 89 that we started to get down to unemployment rates in the 6 percent and less area. So part of it has been that aggregate demand has been slacker than I was anticipating.

The other is a point that my coworker, Diane Macunovich, has developed in a paper with Lee Lillard at Rand. The cohort size effect in the labor markets involves not just the absolute size of the cohort, but whether you're at the leading or the lagging edge of the cohort. The ones that come later in the baby boom, the ones that are on the lagging edge, suffer not only by virtue of the size of the cohort, but also from the fact that because so many have gone before the market has become saturated.

VDT: That's interesting. I thought that was part of your thesis, but in my re-readings of your publications lately, I didn't see that. Yes, that seems very obvious.

EASTERLIN: It does, I know.

VDT: I know, because I have them. I have one born in 1956, the year before the baby boom peak, and one in 1960 and one in 1962. But to tell you the truth, the one born in 1962 is doing best of all. But then it took a lot of education and drive to get him there.

EASTERLIN: That saturation effect, you see, changes the timing of the [fertility] upturn by about four to five years. On the size of cohorts coming into the labor market, you would have predicted the start of an upturn in fertility in the early 1980s. Now, that's mitigated in part by the aggregate demand condition I talked about, and also by this saturation effect, that it takes a while for the labor market to get cleaned out from the surplus cohorts.

VDT: So her [Diane Macunovich's] projection is that it will turn up when?

EASTERLIN: Right around now [1989].

VDT: Well, the one born in 1962 was married last June [1988]. I'm giving him about another year [first child born April 1991]. When were your children born? Do you have one at the peak?

EASTERLIN: Yes. My first was born in 1955; then 1957, 1960, and 1964.

VDT: You have them spaced out to touch all the possibilities. How are they doing?

EASTERLIN: They're all doing reasonably well. The one born in 1964 is coming back in the fall to finish his degree here at USC; he's been in England for several years. He's the only one that's still at the college level.

EASTERLIN: He's bided his time while the pipeline cleared out.

EASTERLIN: Yes. Then my second family [with Eileen Crimmins] is 1981 and 1985.

VDT: Well, they were supposed to be the start of this next baby boom, upturn, but it turns out that they're the tail end of the baby busters, I mean the trough in fertility.

EASTERLIN: That's right.

VDT: I hope we all can see what happens to them.

EASTERLIN: Me too.

VDT: Talking about these qualifications as to when the upturn in fertility would happen, I suppose that is one reason you did a second edition of Birth and Fortune in 1987, only seven years after the first. You felt that epilogue was needed? Or was it selling so well that the publishers said you needed a reprint and update?

EASTERLIN: I really hadn't planned on a second edition. What happened was that I had been asked to do a paper for a Hoover Institution conference that Kingsley Davis organized [on nonreplacement fertility in industrial societies, November 7-9, 1985, leading to the 1986 Population Council publication, Below-Replacement Fertility in Industrial Societies]. That was how I sort of got back to the subject. Then I had certain disagreements with Rita Ricardo-Campbell, who was the editor at the Hoover Institution and had rather strong but not, in my judgment, well-supported views that she wanted to see expressed in my paper. So I withdrew it from that conference. At that time I had been talking with the Chicago Press and we worked out a deal, because I was very unhappy with Basic Books [publisher of 1980 edition], for Chicago Press to buy the rights to Birth and Fortune and put out a second edition, that would then include the new material that I'd developed for the Hoover conference. So it was just a fortuitous combination of circumstances.

VDT: So it wasn't that a reader coming to it cold would say, "Ah, but things didn't work out in the early 1980s the way he had projected. Therefore, he had to write this additional chapter on the ex post,

relative, conditions, and how the baby boomers have made adjustments to overcome their relative income loss of status, and have ended up being slightly better off, actually, than the parent generation."

EASTERLIN: Right.

VDT: Because they deferred marriage, increased wives' labor force participation, and reduced childbearing.

EASTERLIN: Right.

VDT: How did you come to write Birth and Fortune in the first place? Did you feel that was a message the public was dying to get at and you wanted to put it out in a popular form?

EASTERLIN: Well, of course, it grew out of my PAA presidential address.

VDT: I thought it must have. There was a tremendous reaction to that address, "What Will 1984 Be Like?"

EASTERLIN: Yes. And, mainly I'd been working on fertility for most of my career and I came to realize that there were these ramifications that have affected behavior more generally than just fertility. It seemed like there was this whole syndrome of effects that stemmed from the same sort of circumstances. The basic idea was in my presidential address, but the reason for writing the book was to have the opportunity to develop it more fully--to look at some of the data more fully and some of the related literature on things like suicide that I hadn't gone into very much and try and put it together.

VDT: The first time you looked at these other things--political alienation, suicide, homicide, divorce--was in the PAA presidential address?

EASTERLIN: Right, it really was. Some things I'd done, had a bit of knowledge about. For example, Martin O'Connell, who's at the Census Bureau, had done a thesis on suicide; that may have been in the early 1970s. He'd been one of the ones who had attracted my attention to the idea that these ramifications were really broader. His thesis was really quite a good work. It included not just suicide but talked about how attitudes were affected and so on.

So there were progenitors of whom I acquired some knowledge. But other things like divorce I'd really not looked up particularly.

VDT: They had not been looked at in terms of the age structure effects, no. Talk about divorce, you may be right [in his 1978 prediction of "an eventual drop below trend levels in cohort divorce rates," with the "growing relative scarcity of young adults" after 1980]. It's leveled off.

EASTERLIN: So has suicide; so has crime; so has drug use.

VDT: Not where I come from--Washington, D.C.

EASTERLIN: I can't generalize that way, from personal experience. But homicide rates have really fallen, and crime rates generally have leveled off.

I think one of the complicating circumstances is the appearance of crack, which seems to be--unlike the other drugs--much more tied up with violence. So even though drug use has leveled off and

declined, the incursion of crack has produced a sort of mini-wave of violence and drug use that doesn't alter the overall trend but has produced a very noticeable public effect.

VDT: These are complicating external factors which, like a new recession when it comes--the economists are beginning to say when it comes, not if it comes--would affect the chances of the baby busters in the labor market?

EASTERLIN: I think a recession is not likely to be a major one. You always have to hesitate on these projections. The last recession [of the early 1980s] was so severe because . . . It was really instituted by the Federal Reserve as a way of stopping the very high rates of inflation, upwards of 13 percent a year, that we were experiencing and with an extremely conservative and ideologically-oriented monetary and fiscal policy. The whole setup of monetary versus fiscal policy is still crazy. Anyway, we paid a very heavy price to stop inflation, but they succeeded in stopping inflation through a depression, and that's essentially what produced the depth of that depression. We're not experiencing inflation rates of that magnitude now.

VDT: But we're still having to worry from week to week, day to day, whether Alan Greenspan is going to try to adjust things the way Paul Volcker did.

EASTERLIN: Yes. The Federal Reserve really had altered its monetary policy in a crazy way and adopted monetarists' goals. Interest rates went way up; the whole structure of monetary policy was quite foreign to what our historical experience had been. They abandoned that in the early 1980s, but they also produced a very severe recession.

We're now back to a more normal type of situation. It's still abnormal in the sense that we're still running a very large federal deficit. But I don't see a prospect of the kind of depression or recession that we produced back in 1981, 82, 83. So I would view that as a pretty temporary interruption.

The qualification to all this is what's going on internationally--the trade deficit, and the extent to which we really are able to pursue policies independently of our developed industrial partners, particularly Western Germany and Japan. If those countries continue to run extremely conservative expansion policies--and Germany is; Japan has become more moderate--if they continue to do that, it makes it harder for us to expand. So that's the qualification there; the extent to which you really get international cooperation.

VDT: So that will have to be watched, in your anticipation of increasing numbers of births?

EASTERLIN: Yes--the possibility of another major depression interrupting my baby bust fertility.

VDT: Do you feel it's important for demographers, social scientists, economists--academic researchers--to reach out to the public, as you did with Birth and Fortune and have done with a number of papers?

EASTERLIN: Well, I've always tried to write in a way that was not so technical that it was inaccessible to the public--or to scholars in other disciplines, because I feel, based on my own experience, that you want to be able to communicate to people in other disciplines, because I've benefited from their work. So I feel writing only for your own profession is not a highly desirable goal.

But, on the other hand, the way economics is these days, if you don't write essentially in the

highly technical jargon of economics, you're not likely to get published in economic journals.

VDT: There's nothing in the economics field equivalent to, say, Population and Development Review?

EASTERLIN: It's not even clear to me these days that there's anything in the demography field equivalent to Population and Development Review.

VDT: Well, we consider that Population and Development Review is for demographers and economists who talk about population.

EASTERLIN: It used to be that Population Studies was a journal that had articles that were written intelligibly and were of interest comparable to what PDR does, but Population Studies has become a highly technical journal. Demography is a highly technical journal.

VDT: Demography has been ever since Donald Bogue finished as editor [in 1968].

EASTERLIN: Right. They're very comparable in the field of demography to most of the journals in economics, that is, they put primary emphasis on technique rather than on substance, in my judgment. And ideas are a third order of criterion for an article in those journals. So PDR, I think, is an exceptional journal, for demography as well as economics.

VDT: Yes. I was going to mention that when I come to Paul Schultz's criticism [in Population and Development Review] of your Fertility Revolution.

How did your interest in less developed countries evolve? You made some trips to India around 1969-70.

EASTERLIN: I always had dual interests in demography and economic history. When Simon Kuznets left the University of Pennsylvania, which was around the time I got my degree, I started giving the graduate course in economic development that he gave there. That is a course not in traditional economic history, but in the historical process of development of the now developed countries and including also the fortunes of less developed areas of the world. I'd always had that perspective of looking at the long-term experience both of developed and developing countries as part of this heritage of Kuznets's training, and from giving this course.

In my research, initially, I focused on the U.S. But in a course that I gave in economics of population I encompassed the literature on developing countries as well as the literature on the developed countries.

The India trip was an opportunity to get some firsthand experience in a developing country. Actually, I'd been to India earlier at a conference. Again, Dorothy Thomas was my mentor. She was there at the UN population training institute in Bombay.

VDT: You went while she was there?

EASTERLIN: Right. I was only there about a week or so, but she acted as a tour guide, and introduced me to people. This was about 1965. I think there was a population conference in Delhi in 1965, and I'd gone there for that purpose.

Dorothy was a great person. We were in Bombay, the usual congested situation. I had the usual Westerner's reaction to that: So many people, how did they manage? And she said, "Well, you've got to talk to the people and understand how they feel. You'll find out that Indians if they're off by themselves often feel very lonely. They're so accustomed to having large numbers about that,

contrary to the way we're brought up, they feel quite normal and natural in a congested situation."

So Dorothy did a lot to open my eyes, I think, to a less Western-biased perception of people in developing countries, and really to an appreciation of India and Indian culture. Even though I can't say that I'm anywhere like an expert or authority on it, I have a very great admiration for India, and I think it traces back to her tutelage.

In 1969 this opportunity came along to go to India as a member of a family planning team to look at the problems of spreading fertility control techniques in India. It was put together by the United Nations and was a team of international experts. There were a couple of Americans, a couple of Swedes and others. The team was headed by a Yugoslav diplomat.

VDT: You say in The Fertility Revolution and in an article before then that your research was based in part on analysis of data from an Indian survey taken about 1970. Did you have something to do with setting up that survey at that time?

EASTERLIN: No, those data just happened along fortuitously. I've never really been involved in generating any primary survey data. In my course and in my theorizing, I had developed some of the ideas that appeared ultimately in the synthesis framework, and it was at about the time I went to India.

The benefit that came from that trip was that I became more acquainted with what the issues of family planning policy were and was able to look more at the relevance of fertility theory to family planning policy. One of the most recent articles I did, with Kua Wongboonsin of Thailand and another good student of mine, from USC, Mohammed Ahmed, who's gone back to Alexandria now, had to do with applying the synthesis framework to family planning policy. That appeared in Studies in Family Planning. We used data for both Egypt and Thailand, the idea being to develop a measure of the demand for family planning in Egypt and Thailand, along with estimates of service supply.

So the India trip was of value in expanding empirically my understanding of issues of family planning and helping to stimulate more theoretical analysis of fertility behavior in developing countries. This involved in part trying to reconcile the concept of natural fertility with the economist's assumption that all fertility behavior is determined by deliberate choice.

VDT: Certainly in your Easterlin-Crimmins synthesis framework you did that, trying to reconcile the sociological and the economic points of view in researching motivations for fertility decline. That approach was heavily criticized by Paul Schultz as it came out in your book, The Fertility Revolution, in his review of the book in Population and Development Review [March 1986].

On the side, I should say that Paul Demeny said [in his interview for this series] that one of his aims for Population and Development Review was that such criticisms could take place there. He felt that demographers have always been too polite about criticizing each other's research and that they take it personally, perhaps because you know each other too well. He said that never happens in the economics field--too big perhaps.

EASTERLIN: Schultz and I are both economists. [Laughter]

VDT: Okay! Perhaps you had already been brought up in that way, and had a forum where you could do it.

I'm not quite clear on Schultz's criticism; it was very technical. You said in your reply ["Economic Preconceptions and Demographic Research: A Comment," PDR, September 1986], logically, that demography needs sociologists and anthropologists and the like as well as economists. And you've been in the forefront in trying to bring together these two schools. I suppose only an economist could have done it. The sociologists haven't always been taken seriously by the economists.

EASTERLIN: And the anthropologists don't take seriously the sociologists or the economists.

Well, let me state very briefly what I see as a central issue, which really is an issue in economics and what is admissible evidence. Economics in the past 30, 35 years has become highly quantified, with the incursion of quantitative methods, formalized in econometric study. And this has been intimately tied up with a behaviorist conception of the study of human behavior.

The idea is that you observe what people do and draw inferences from that, and that's as far as you go with your evidence. You never pay any attention to what they have to say about why they do something, i.e., to subjective attitudes. It ties in partly with the notion of how you treat tastes. So economists had abjured evidence that has to do with statements about desired family size. They just say, "Well, you can't put any credence in that." Whereas what I tried to do in the synthesis framework was build a framework that made it possible to link such statements to economic theory.

Similarly, economists have been highly resistant to the concept of natural fertility. Again, in the synthesis framework, I tried to build a framework that made natural fertility behavior a rational outcome of certain circumstances that people would be in. Now my reason for crediting the conception of natural fertility behavior was, again, what I referred to earlier, that I learned from looking at the evidence that demographers have produced justifying the view that people practiced natural fertility behavior in developing countries and in pre-modern times. Similarly, I've felt that if you looked at the data, the kinds of results that you got in statements about desired family size seemed to make sense.

So I felt that you had to have a theory that took account of these phenomena. But the mainstream of economic thinking in the area of population, which was dominated by the Chicago school, rejected this type of evidence. And that view was not confined simply to the Chicago people. It's much more common in economics because of this behaviorist idea. You look at people's revealed preferences as revealed in their behavior; you don't ask them about their preferences.

The result is that an enormous amount of fascinating work, some of which I'm now involved in with Eileen, that is produced by the Survey Research Center of Michigan about people's attitudes is just out of hand omitted from any sort of economic inquiry.

That's the crux of the issue that lies in Schultz's criticism of The Fertility Revolution and my response to it. The Schultz criticism essentially says the kind of evidence that I'm using is not admissible. This evidence says people make choices about marriage and breastfeeding behavior that are not geared to what their fertility consequences are going to be; rather, they're geared to other concerns. This evidence comprises the kinds of things that people themselves have to say when they're asked about why they got married at such an age, why they breastfed their kid for 18 months, and so on. Since his theoretical conception says you can, in principle, control fertility deliberately through breastfeeding or marriage behavior, therefore, people do that.

So that, to my mind, is the crux of what's going on. He's defending an attitude that I think will eventually be rejected--this behaviorist view. It's already starting to crack in economics. But it's a very strong heritage. Its origins go back to an attempt to purge economics of Marxism and social reform and preconceptions of an ideological nature. The idea was that econometrics could make economics into a pure science, devoid of any preconceptions, and, just by studying the data, you could somehow construct a theory about what's going on in the world that explains how people behave.

The trouble with that point of view is that there's no limit to what is endogenous. At the end of my response to Schultz's critique, I asked at what point is he going to start saying that something is not endogenous to the model and is, in fact, exogenous to the model. What I say is that you draw the line between what variables are endogenously determined and what are exogenously determined by what we learn from the people themselves. His view is that you can somehow tell that from the data. And there's no way that that can really be done. The econometricians--not all of whom are of that

persuasion, but many of them are of this behaviorist persuasion--will never be able to construct a valid model as long as they insist on a purely behaviorist approach. So that's the issue.

VDT: Thank you for putting it that way. I think you have a lot to do with perhaps changing the views of the holdouts. Sam Preston, who happened to come out of economics but says he doesn't think of himself as an economic demographer, wrote in an article in Sociological Forum a couple of years ago ["The Social Sciences and the Population Forum," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987] that he always himself felt that demographers had to pay more attention to the softer sciences, in which he was including the sociologists and the anthropologists, and perhaps the Jack Caldwell approach, going out to villagers and living with them, supposedly, for a year and really observing what they think and do.

EASTERLIN: Indeed. I used some of Jack Caldwell's findings about the reasons why people are breastfeeding and the reasons why they get married in support of my argument that we're really talking about natural fertility behavior and not endogenous control.

VDT: It goes way back, in a sense, to Kingsley Davis's and Judith Blake's framework of intermediate variables. They have felt that in recent years their framework has stimulated the collection of data just on the proximate determinants, intermediate variables, of fertility.

EASTERLIN: Yes.

VDT: In the World Fertility Survey and also in the National Survey of Family Growth--perhaps it hasn't in the latest one, but it certainly did up through Cycle III. And not enough on collecting the background data, which works through the intermediate variables, breastfeeding and marriage.

Do you think your research could lead to policy to influence the threshold of fertility regulation--in other words, to lower family size desires before full modernization? Now, India has had, at least on the books, laws to delay marriage. With breastfeeding, of course, it's not very likely you can do anything through policy.

EASTERLIN: Well, my view is that the essential pre-conditions for a successful family planning policy are to establish a real demand for family planning, and that the measures most conducive to that are social measures not economic measures: public health measures that lower infant and child mortality, and universal education. Most of the evidence suggests that these social dimensions of modernization are the ones that push families most rapidly into the situation where they're likely to produce more surviving children than they want. And once they're in that situation, then family planning measures can work.

VDT: Something like the state of Kerala in India?

EASTERLIN: Right, that's a good example of a place where social programs have affected fertility behavior.

VDT: It has stressed public health and education, and it still has a very low GNP per capita, but it has lower fertility than other states of India.

EASTERLIN: Right.

VDT: Talking about family planning success or lack of success, what do you think of the current world population growth trend? The growth rate seems to be stuck. Quoting again the Population Reference Bureau World Population Data Sheet--though you're not supposed to use them as a time trends series--they have the world growth rate inched up to 1.8 percent this year; it was 1.7 percent in their 1988 Data Sheet. And, of course, you know that the United Nations in their 1988 assessments jacked up their medium variant and all their projections for the year 2000 and 2020. In other words, we're not doing as well as we had hoped.

EASTERLIN: Well, I have just recently been involved in a conference on European population, sponsored by the Council on Europe and the European University Institute. Live-Bacci was one of the organizers.

VDT: Toward the end of last year; Ansley Coale was to go.

EASTERLIN: Ansley Coale and Paul Demeny were there, although there were probably only four or five Americans; mainly Europeans.

The Europeans, of course, are deadly afraid of lack of population growth. My general attitude is that the concerns at both ends of the spectrum are largely illusory, and that the problem in today's developing countries is they're the victims of worldwide economic breakdown that has partly been sown by the monetary and fiscal policies pursued in the developed countries. So, as their trade circumstances have been severely adversely affected, that's forced them to indulge in budgetary cutbacks and so on. They're less able to pursue their social programs as well as their development programs. And one of the consequences is that the fertility transition is correspondingly slowed.

But I see this as not a persistent phenomenon. I don't see the conservative reaction that's dominated the world scene over the past ten years as more than transient.

VDT: You think there's going to be a turnaround in this conservative view and we'll get back to what all our Californians are worried about?

EASTERLIN: Clearly Bush is not the ideologue that Reagan was, and Thatcher's days are numbered. [Prescient!] It depends what your temporal horizons are. I think in terms of periods of five and ten years.

VDT: Five and ten years; you consider that long?

EASTERLIN: Yes, when I talk about change. I don't anticipate that the conservative ideology that's become popular is likely to persist with the strength it has through the 1990s.

VDT: Will that also turn around the low fertility of developed countries?

EASTERLIN: Well, I'm not concerned about it. But, of course, I don't project the low fertility that people do for the developed countries, so I don't have to worry about it.

In the paper I did for this Florence conference, I looked at the historical experience of ten European countries that are in the forefront of modern economic growth. When you look at long-term economic growth, per capita income growth, and population growth; when you look at future prospects of dependency, and so on--it's hard to see where such unusual circumstances are in prospect. That the older population is going to be such a pressing burden on the economy.

VDT: What about fertility, which has got to go up if that is to come about?

EASTERLIN: Well, the last time we had a fertility boom we were experiencing high economic growth. My expectation is that productivity growth is going to go up; that economic growth rates will go up; that aggregate demand will be more fully implemented through monetary and fiscal policy; that baby bust cohorts will be coming on the labor markets; and that we will be able to enjoy the benefits of higher fertility--of a transient nature--and of a larger old-age population, which will have a larger number of workers to support it, by virtue of the baby boom in prospect.

VDT: The fertility will have gone up previously?

EASTERLIN: Right.

VDT: Well, you may be right. Again in the latest PRB Data Sheet, the latest total fertility rate of Sweden--well, in the proofs it was rounded up to 2.0, but at the last moment, Carl Haub decided--he said, "Don't tell Easterlin that"--it was going down to 1.9, but still higher than it has been. Now the low one is Italy, 1.3; it's lagged behind northern Europe.

EASTERLIN: If you look at completed fertility rates in these countries, and people's expectations about completed fertility rates, it's clear that the very low TFRs, like Italy's, tend to be an exaggeration of what their completed fertility is likely to be. Calot, I think it is, had a paper in Population Studies that showed that the completed fertility rates would be around two; I think this was just for France. He also had a little piece in PDR.

As I judge it, among most of the European experts, the tendency is not to assume that completed fertility is really going down anywhere as low--even for these low-fertility age groups that we're now observing--as the TFR implies. It's part of this problem with the period rate giving a distorted impression; completed rates tend to swing to a much more moderate degree than the period rates.

VDT: What are you doing now? You had your interesting paper at the PAA meeting this year on children in poverty ["Economic Status and Household Living Arrangements of Children"], which made the front page of the second section of the Wall Street Journal, in Alan Otten's column. He was the one journalist that PAA brought in officially this year. I've been sort of involved through the years with the effort to get more PAA papers into the press, but they're usually not official. Reporters come on their own; Randy Schmid of Associated Press was there this year, on his own, though I didn't see any results from that. But you made it, and you were saying that child poverty isn't as bad as Don Hernandez, for instance, who followed you in that session, makes out--in part because families have adjusted, even single-parent families have adjusted, by moving in with other adults and so on. Is this some of the research you're doing now?

EASTERLIN: Yes. This argument about children and another article that I published in PDR on age structure and poverty ["The New Age Structure of Poverty in America," Population and Development Review, June 1987] are really connected to that last chapter in Birth and Fortune. They all form the genesis of a more general inquiry into the relationship between demographic behavior and income distribution in the United States. The question basically is one of how people's life-cycle demographic decisions alter their economic status--decisions about forming or breaking up unions, having children, about spouses working, about establishing independent households, retirement--this succession of decisions affects the economic status of people.

What this paper shows, like the chapter in Birth and Fortune, is that people have made these decisions often in a way that helps to maintain and preserve their economic status. If you look, for example, at how well off the baby boomers now are, they really have improved their circumstances compared to their parents at the same age. And they've done it very largely through these demographic devices, because the labor market alone would have put them in a much worse situation. But they've compensated, because they have very strong aspirations for goods. They've foresworn a lot of much more traditional family types of circumstances in order to achieve more goods for themselves.

VDT: And you think that the results are going to be psychological stress, because they've given up . . .

EASTERLIN: They do have psychological stress, yes. Most of the baby boomers you talk to--at least the ones I talk to, but maybe I get a selected sample--all subscribe subjectively to my argument.

VDT: That things are tougher for them?

EASTERLIN: Yes.

VDT: They've had to put off marriage and having children, or as many children as they wanted. Well, perhaps you're right.

EASTERLIN: Anyway, the idea of that project is to try and look at the distribution of income in the United States over about a 25-year period in a way that looks at the different age groups and looks at the way their demographic decisions have affected their situation and the distribution of income.

VDT: You're doing this in this year that you're having a sabbatical? You did come to the University of Southern California in 1982; seven years later you get a sabbatical?

EASTERLIN: Yes.

VDT: There's just this little core group of you at the University of Southern California--David Heer, yourself . . .

EASTERLIN: Van Arsdol, Judy Treas has been here; she's leaving now, and Eileen. Marcus Felson--he's in criminology, but he has a demographic interest.

VDT: Is it just because you're here that the University of Southern California has a demographic kernel?

EASTERLIN: Don Van Arsdol established the Population Research Lab here and has always been instrumental in trying to engender a program of some magnitude.

VDT: What is your connection to the Research Lab?

EASTERLIN: I'll participate in their seminar intermittently and then usually I end up with a student or two whose thesis or research work I set up.

VDT: But your primary appointment is with the Department of Economics?

EASTERLIN: Right. But that was true at Penn. It's just that they had a separate graduate program in demography there and they don't have a separate one here; they have a program in sociology. So the students here who are interested in demography end up doing a Ph.D. in economics or in sociology, but they tend to have similar types of training in demography. So my Ph.D. students in economics, like Diane Macunovich or Mohammed Ahmed, the Egyptian I mentioned, have taken courses with David Heer and others in population as part of their economics training.

VDT: What accomplishments in your career have given you the most satisfaction? I think the answer is pretty obvious. And that ties up with, which of your publications do you consider most important and why? Now, of course, you've had your two streams of research and publishing.

EASTERLIN: I guess the principal accomplishments are represented by Birth and Fortune and The Fertility Revolution. Each of them represents what I consider to be a step forward in theory, in the interpretation of demographic behavior, that is tested in some degree against the empirical evidence--that it grows out of the empirical evidence and is consistent with the empirical evidence. I think I would consider those to be the principal ideas . . .

VDT: The principal ideas, and accomplishments, of your career? That seems very logical.

INTERRUPTION

VDT: You say you have an allergy. Is that from the smog?

EASTERLIN: No. I guess it's somewhat better out here. In the East it was ragweed and dust and all those things. Out here it's because they irrigate so much. There's so much pollen in the air and things are pollinating so frequently that the allergy situation is not much different from the East. In the East when the ragweed season came, I was always in a terrible state, and when the heat came on in the winter, that was terrible. But out here, I'm sort of at a level plateau, more or less.

VDT: Did you come West in part because of change of climate?

EASTERLIN: Well, yes, because of the change of climate, not because of the allergy dimensions. Yes, it's just much easier to live out here, and especially to raise young children--a lot easier.

VDT: Who approached whom?

EASTERLIN: USC.

VDT: They came after you? You decided it was time for a change after a lifetime in the East?

EASTERLIN: Right.

VDT: Now I'd like to get onto your recollections of PAA. You've already said that you went to your first meeting because of Dorothy Thomas's inspiration. Do you remember when that was?

EASTERLIN: It was in Providence; I don't remember what year it was.

VDT: Providence wasn't until 1959. You gave me the impression she got at you sooner than that.

EASTERLIN: She probably did, but that's the one I remember most.

VDT: What do you remember about it?

EASTERLIN: I just remember being there, that's all.

VDT: Can you remember the luminaries at the early meetings you attended?

EASTERLIN: The people I remember that were the leading figures were people like Irene Taeuber, Frank Notestein, P.K. Whelpton, whom I remember seeing at an early meeting of the American Economic Association, John Durand, Kingsley Davis, Ronald Freedman, Ansley Coale. I guess those would be the people that I remember most, and whose work I knew most.

VDT: Can you remember when you gave your first paper at PAA?

EASTERLIN: No, not really. It might have been at Providence. [The first Easterlin paper listed in meeting programs in the PAA archives, which lack the program for 1961, was the 1964 paper noted below.]

VDT: Already that first year?

EASTERLIN: Well, we started the population redistribution project at Penn in 1953, 1954, and we gave papers, I think, at various meetings thereafter.

VDT: We, meaning Dorothy Thomas . . .

EASTERLIN: It was usually Everett, Ann Miller, Carol Brainerd, and myself; we would be doing papers of some sort. That was my early involvement in professional meetings and in Population Association meetings.

The early PAA meeting that stands out most in my mind, where I can remember giving a paper, was in San Francisco, when I gave an early version of "The American Baby Boom in Historical Perspective" ["Interrelations Between Swings in Demographic and Economic Growth"].

VDT: That was in 1964. One question I forgot to ask was about your experience at the National Bureau of Economic Research. Where is that?

EASTERLIN: At the time it was in New York. I was there for a year, but most of the time I was connected with the National Bureau [1955-66], I was in Philadelphia. But I had an assistant working in New York, and I would go in about once a week to meet with her.

VDT: Tell me about that. Is that a think tank for economists, or is it a policy group?

EASTERLIN: The National Bureau of Economic Research was the first major non-profit organization in economic research. It was founded back in the early 1920s right after World War I by several of the great scholars in the field who believed in the importance of empirical inquiry as opposed to the primarily theoretical emphasis in economics. So it established a tradition of its own research program in empirical work. The business-cycle program was the best known, and is embodied today in the government's economic indicators.

When I went there, although initially I was involved in sort of the aftermath of the Kuznets-Thomas population redistribution project, it was to follow up more the question of long-term fluctuations in economic growth and its relation to population. It was while I was connected with the Bureau that I really did the work that came out in "The American Baby Boom in Historical Perspective."

VDT: Here's my copy from my student days at Georgetown in 1968-70; it's written all over the place. [Laughter] It was certainly a dynamic article.

So you went one day a week to New York. You were teaching at Penn and commuting back and forth?

EASTERLIN: Right, but one of the advantages of the National Bureau of Economic Research affiliation was that they paid half my salary, so it cut my teaching load in half. It helped me pursue a research career much more fully than if I'd been on a full-time teaching appointment at Penn. So that was a strong benefit, plus the people that were there.

Now the National Bureau of Economic Research is under the directorship of Martin Feldstein, former adviser to President Reagan. It's become more mainstream, and it tends to work now more on policy issues. It's changed its character somewhat, but it's still probably the premier research organization in economics, outside of universities.

VDT: Let's go back to PAA. Your first meeting, probably, was at Providence in 1959, and just 19 years later, you had risen up the ranks to become president. Were there any issues along the way to 1978 that you were particularly struck by? For instance, were you caught up in the Concerned Demographers issues of the late 1960s and early 1970s--what some people felt were efforts to politicize the Association, to get it to speak out on policy issues--and the Women's Caucus?

EASTERLIN: I wasn't particularly caught up in them. I guess I didn't hold views as strongly as Otis Dudley Duncan. And it had been Dorothy Thomas's view that the organization was primarily an organization of scholars, whose concern was research, and should not be involved in the promotion of any particular policy view.

But Dorothy's attitude was a product very largely of the early days of the PAA, when there was a struggle about whether it was going to be an organization to promote family planning or an organization devoted to scholarly research. She'd always been dedicated to fundamental research, and that's why she and Kuznets were connected, also. They were both students of Wesley Mitchell, who was the one that started the National Bureau of Economic Research, so it went back to Columbia University, naturally. I was part of that heritage and although I didn't become very active in the debates in the Population Association, my sympathies continued--continued then and continue now--to lie in the view that this should be an organization devoted to scholarship and it's not primarily promoting any particular policy position.

VDT: Well, those debates seem to have abated.

Now 1978. You've pointed out that your address brought together many of your ideas on the Easterlin hypothesis, and it led to Birth and Fortune. I've got down here that you stuck your neck out with your predictions and, of course, everyone has been waiting to catch you out on that, but we've discussed that. It was a very provocative speech. You realized that it probably would be?

EASTERLIN: Yes. Well, I thought it was a breakthrough of some sort for me, in bringing together a lot of things that had been lying around loosely and not put together coherently in my own mind. So I felt real good about that one.

VDT: And you felt that was a forum for such a speech? People seem to have differing views on what PAA presidential addresses should be. In many cases, it is indeed sort of a culmination of a lifetime of research, bringing it all together--at least, lifetime up to that point. In other cases, some of my interviewees have felt that they should speak out on policy issues. For instance, Ansley Coale's address, ten years earlier, was on "Should the United States Start a Campaign for Fewer Births?" That was just at the height of the Paul Ehrlich population-explosion furor. The gist of his speech was that, no, because the birth rate had already started down without any policy. That talk was quite aside from his regular research.

What was your view of that? Obviously, you opted to give a research-culmination kind of address.

EASTERLIN: In my view, it's sort of laissez faire--people should do what they feel moved to do.

VDT: In your case, of course, it struck policy too--an issue that Americans are very aware of. American Demographics, for instance, that's sort of based its whole existence on the existence of the baby boom, was taking off about then.

EASTERLIN: Right. But my own interests have never been that much in policy applications. Again, it's this heritage of Kuznets and Thomas. I believe before you can design appropriate policy, you've got to answer a lot more fundamental questions about what determines behavior, and I prefer to work on those. But that's not to preclude others from working on policy.

VDT: Do you regret the changes in PAA meetings, which have now exploded to 84 sessions, in the last New Orleans [1988] and Baltimore [1989] meetings, with eight overlapping, and going on through Saturday afternoon? The attendance was close to 1,200 [1,193] in Baltimore this year. And the splitoffs--there were the economic demographers who split off, well, you can't say that--accompanying workshops.

EASTERLIN: The economic demographers part I can quickly respond to. I had been an opponent of separate meetings, because I felt--for the reasons I expressed early on--that economists ought to be intimately involved in the work of the PAA. They didn't have a separate workshop this year.

VDT: Yes, why was that?

EASTERLIN: I don't know, because I don't really concur with the conception of a separate workshop. I've never attended those sessions, because I think that economists ought to work through the regular PAA sessions, and that they ought to be in direct contact with real demographers, rather than maintaining what to some must appear to be an elitist club, talking among themselves.

So I'm in favor of PAA trying to assimilate different disciplines within its framework, as I think it has to a remarkable degree.

And I'm in favor of maximizing participation of the membership, as I think PAA has done much better than the other associations that I'm familiar with--the Economic History Association and the American Economic Association. When I was president of the Economic History Association [1979-80], I tried to use some of the techniques that are employed by the Population Association to involve members more fully in the determination of the program and in participation in the sessions.

VDT: What techniques, for instance?

EASTERLIN: Well, the PAA runs this solicitation of what session topics there should be and then divides it up, announces it, and people then submit abstracts or papers for consideration and the chairman of the session typically tries to make a serious judgment on which papers should be included.

VDT: That's not done by the Economic Association, for instance?

EASTERLIN: It may seem obvious to you that this is the way to run it, but it wasn't. The way these organizations typically have been run is that the president-elect or somebody would say, "These are the topics I'd like to see talked about, and let's have so-and-so be the chairman," and the chairman in turn calls up two or three friends and says, "How about writing papers on this topic." That's putting it a bit in the extreme, but it's not much of a difference. Whereas, the PAA program is more or less a reasonable representation of what the membership is working on. That's not at all true of the way the Economic History Association does programs. Actually, after I was president of the Economic History Association, they continued some of the devices I brought in from the PAA, so I'm pleased about that.

Another one is that the PAA has limited the frequency that a person can be on the program, as a way of making it possible for more people to be on the program.

VDT: Other organizations don't do that?

EASTERLIN: No. There are all these techniques that the PAA has worked out that make it much more responsive to the membership, I think.

VDT: Why is that? I hadn't realized how unique the PAA is. Well, we all think of it as a special organization, but I happen not to belong to other big ones.

EASTERLIN: I'm not that closely or intimately aware of what the sociologists do, and it may be that the PAA's format is more like the American Sociological Association. I think the sociologists tend to be somewhat more democratic, less elitist, because they're much more aware professionally of such issues. Economists don't have any theoretical conception of elite, although obviously they have a practical conception; there's a whole status hierarchy in economics, but there's no theoretical notion. Whereas the sociologists are driven more to purge themselves of elitist tendencies, I think. It's possible that the PAA because of the sociological origins of most of its membership has been more democratic in the way it's gone about its business.

VDT: But you just suggested that they also would be conscious of an elite. Which you could perhaps say is still true of PAA, which after all only has about 2,600 members [2,679 at end 1989; 2,752, end 1990]. It's fluctuated about that number since the mid-1970s, which is very small compared to other professional organizations.

EASTERLIN: Not compared with the Economic History Association, which is maybe 1,200-1,500.

VDT: Okay. You feel, however, PAA has been quite democratic; it's encouraged participation by anyone?

EASTERLIN: Yes. And democratic in its interdisciplinary attitude. It's been good in this respect. I have some reservations about its greater involvement in the Washington scene.

VDT: You mean the Population Affairs Committee?

EASTERLIN: Right. I have doubts about how much it should move in that way. I think working in concert with the American Economic Association and the Sociological Association for the promotion of scholarly research is appropriate. But . . . I really don't follow much what goes on in Washington. Maybe it's one of the benefits--or costs--of being in Los Angeles.

By and large, I consider the PAA to be much more a role model of the way a professional organization should operate than has been true of the ones I'm familiar with.

VDT: That's a great observation! Even with the 84 sessions at meetings, eight overlapping, you still don't . . .

EASTERLIN: That doesn't bother me. The Association is there for the dissemination of scholarly knowledge and if people are willing to go and attend sessions, that's fine. Indeed, in my view, given the rate structure of the airlines now, since Saturday night is a good night to stay over, there's no reason they shouldn't run sessions that take advantage of that.

VDT: Through Saturday afternoon. Although I don't think they're too well attended; people have gone out on their sightseeing.

EASTERLIN: Yes, but that's because they cut them off so early. I think it's conceivable, if anything, they might start later and continue through the weekend. Start on Thursday afternoon, or something like that. But I don't have strong views.

VDT: A couple of final questions. What do you see as the outlook for demography in this country? Is there still room for basic research demographers, as you have been, or are the jobs now all for applied demographers?

EASTERLIN: I think the outlook is good. I think most of the appointments will continue to be of people who are in sociology and, to some extent now, in economics. I mean the people who conduct research.

VDT: Will have to have degrees in one or the other of those?

EASTERLIN: Right. And I think that's appropriate. I think they ought to have training in the parent disciplines, so to speak.

I think the advances in the field, the development of the computer and of microdata sets, set in motion a whole new wave of opportunity to be mined. Even with the waning of the population problem worldwide, I don't anticipate that this is going to mean any serious lack of funds for basic research in demography.

VDT: And, of course, it takes funds to make research happen.

EASTERLIN: Right. But I think demographers won't be competing at NSF [National Science Foundation] and at NIH [National Institutes of Health]. NIH [funding] has been a windfall, to some extent, for demographers.

VDT: NICHD [National Institute of Child Health and Human Development] you mean?

EASTERLIN: Right. And to some extent NIA [National Institute on Aging] now. For a long time, it was Jerry Combs [former Chief, Behavioral Sciences Branch, Center for Population Research, NICHD]. I think demographers have done well.

VDT: Although the percentage of NICHD's approved proposals that they can fund is now down to about 16 percent.

EASTERLIN: But it's still a source of funding that is not available to non-demographers.

VDT: It's much better for demographers than it is for others?

EASTERLIN: Economists have only gotten belatedly into the act. But demographers had this source of funding at the National Institutes, along with the opportunity for going in under the sociology program in the National Science Foundation. So, although the funds may not be as great over the longer term--who's to say? They may be better.

VDT: I think that's a good place to stop. Your plans for the future? Take advantage of some of these research funds? You've already said you're into a lot of research still; you're still working flat out. And there's still your new family of latent baby-busters, two children, to raise.

EASTERLIN: Right.

CHARLES B. NAM

PAA President in 1979 (No. 42). Interview with Jean van der Tak during the PAA annual meeting, Hyatt Regency Hotel, New Orleans, April 22, 1988.

CAREER HIGHLIGHTS: Charles Nam was born in Lynbrook, N.Y. He received a B.A. in applied statistics in 1950 from New York University and the M.A. and Ph.D. in sociology in 1957 and 1959 from the University of North Carolina at Chapel Hill. He worked as a statistician with the Census Bureau from 1950 to 1953 and with the Manpower Research Branch of the Air Force in Montgomery, Alabama, in 1953-54. From 1957 to 1964, he was at the Census Bureau as Chief of the Education Statistics Sector and later of the Education and Social Stratification Branch of the Population Division. Since 1964, he has been at Florida State University in Tallahassee, where he has been Professor of Sociology, Chairman of the Department of Sociology (1967-81), and Director (1967-81) and then Research Associate of the Center for the Study of Population, which he founded. Among other activities, he has served on the Census Bureau's Population Advisory Committee (1978-81), as a consultant for UNESCO, the East-West Population Institute, and the Government of Indonesia, and is a frequent guest lecturer. He was editor of Demography from 1972 to 1975. His broad-ranging publications in education, social stratification, general demography, migration, mortality, and population socialization include the 1960 census monograph, Education of the American Population (with John Folger, 1967), Population and Society: A Textbook of Readings (1968), The Dynamics of Population Change (1976) and Population: A Basic Orientation (1984) with Susan Gustavus Philliber, The Socioeconomic Approach to Status Measurement (with Mary Powers, 1983), and Our Population: The Changing Face of America (1988).

VDT: What led to your interest in demography?

NAM: I guess in one sense you could say it started when I was born, because my parents were immigrants as children, and you know the early U.S. demographers were really interested in immigration. My parents came as children from Latvia, outside of Riga. Their families were there when the Tsar was in power and East European Jews were eager to get out of that area. They came to New York by way of Liverpool and Boston. They were not well educated. My father only went to the eighth grade; my mother went to the third grade. There was no educational tradition in my family. For me to go on and get a Ph.D. was incredible.

VDT: Indeed, in one generation!

NAM: So I didn't have the kind of background that a lot of people have. My parents always strived for me, but they didn't assume I would ever get into college; they just hoped I'd be able to earn a living. But I went into the army after high school, came out, and on the GI bill got to go to college.

The first time I knew there was anything like demography as a field, I was taking a course when I was a major in applied statistics at New York University in the school of business with a professor named Ernest Kurnow; he's retired now. He gave us various kinds of statistical exercises and one of them, toward the end of the term, was on population projections. I found it fascinating; I didn't even know people did things like that. This was well toward the end of my B.A. before I knew there was such a thing. It whetted my interest and I started looking up some things about it and attended an American Statistical Association meeting where there was a session on population projections; P.K. Whelpton and Henry Shryock were presenters in that session. But even at that point, I didn't foresee a

career in demography. That came a little later on when I finished my bachelor's degree.

A friend who had been in the applied statistics program at New York University with me and I decided we'd apply for government positions. We got on the junior professional assistant register, which at that time was the way you got appointed to a beginning professional level position in the federal government. It was a general register--I think it still exists in a similar form--it was the very first GS5 professional level. We had to take an exam and we both got calls at the same time from the Census Bureau.

VDT: Did you mark the Census Bureau as one of the agencies you wanted to work with?

NAM: No, you didn't have any indication of where you wanted to work; just that you'd be a candidate for a position of a statistical nature. We were not knowledgeable about what the opportunities were. We weren't even sure we'd ever get called and were looking at other jobs, anywhere from market research to working at local New York statistical agencies.

Both of us got called by the Census Bureau and asked if we'd like to work on the 1950 census. This was 1950. The census had already been taken, but the processing of it was going on, the preparation of publications. We both accepted and went to Washington together. He got placed working for Eli Marks in the Statistical Division and I was asked to go to the Population Division, to work with Paul Glick.

That's sort of how things got started--just pure chance. It was not by any design that I started that career. I guess I can say even at that point there was no indication that I was going to continue in demography. It was a job; I was earning a living. I was interested in the subject, but I had just a bachelor's degree. So becoming a professional demographer started there, but . . .

They hired a lot of junior professional people to work on the 1950 census. They didn't hire till late. A few were hired just before the census; I was hired after the census was taken, but within months after. Every division, including the Population Division, had maybe 20 junior professionals. At that time, we were like glorified clerks. As a matter of fact, they started us all off by putting us in the coding sections with clerks to get a feel for what the coding of census schedules was like.

VDT: Hands on!

NAM: Right. We had to do it for two weeks, and the people who were full-time clerks didn't know I was hired as a junior professional. I didn't tell them, because I thought they'd be a bit bothered by my being there and thinking maybe I was a spy from upstairs. So I did actual coding of the 1950 schedules for two weeks before they called me back upstairs to start on more professional kinds of things.

We didn't know how long we would be able to work for Census, because we knew the end of the census period would come within a couple of years. I worked for Paul Glick and Henry Shryock. It was about a year and a half later that they started to lay off people, one by one.

VDT: I've heard that this was a Reduction in Force, during the Eisenhower administration.

NAM: Exactly. It was like "Ten Little Indians"; one would get picked off and we'd look to see who was next.

VDT: I've heard from Jack Kantner that you were so good that they kept hiding you, moving you around. They wanted to keep you as long as possible.

NAM: I don't know what the motivation was, but I was one of about three of the original 20 in the Population Division that was still around. Stan Greene, who worked in labor force, was another. Yes, Eisenhower was cutting back on the budget tremendously, so on top of the usual phaseout after the census, we had severe budget cuts.

Paul Glick and Shryock tried to hold on to me and they just couldn't do it; there was no way they could justify it. But there was this one "out" that Jack Kantner mentioned to you. Parker Mauldin was then running the International Population Statistics shop; Jack Kantner was there, Paul Myers, Jerry Combs, Art Campbell, and others. They tried to squirrel me away; they were working on Soviet censuses and other semi-secret kinds of things. I had to go through a secret clearance first, which I did, and they put me there.

But they shouldn't have done that; I didn't belong. And the question was how long I could remain there before they realized I wasn't a Russian specialist. I didn't have Russian language skills; I was just a plain old junior demographer. I stayed there a year or so, and then they discovered me and I was laid off finally.

This was in 1953. I was actually unemployed for about two months, looking for work in Washington. It was difficult because every agency was laying off on account of the Reduction in Force and the budget cutback.

Then an angel came along, and his name was C.A. McMahan. He was a sociologist/demographer who'd been a student of T. Lynn Smith at Vanderbilt, years before. C.A. McMahan was then down in Montgomery, Alabama, heading up a Manpower Research Branch for the Air Force--a mixture of civilian employees and people from the Air Force. Abbott Ferriss and Tom Ford were working there; it's amazing the number of people we now know in the profession who were down there. John Folger was working there and he had some link to Parker Mauldin's shop as a consultant. Parker mentioned to him that they had to lay me off and if they were looking for somebody I might be a good person to bring on. Tom Ford and C.A. McMahan had to come up to Washington on business, McMahan had a vacancy, and he and Tom Ford interviewed me in a hotel in downtown Washington. Two weeks later I was in Montgomery, Alabama; unattached, living in a rooming-house. I spent a year working as a civilian employee for the Air Force.

It was a crucial year, because that opportunity was really what brought me fuller into the demography profession. It came about this way. I did have an association with McMahan, Ford, Folger and Ferriss and the other people there, but it was a fairly routine job, working on demographic studies of Air Force personnel. But McMahan had an advisory committee for his division, consisting of T. Lynn Smith, Rupert Vance, and a man named Frank Dickinson, who was the chief statistician for the American Medical Association. They came down and spent two weeks while I was there and I got to meet Rupert Vance. At the end of the two weeks he asked if I had thought about going back to graduate school. Actually, I had taken some courses while I was at the Census Bureau with Frank Lorimer at American University, but not toward a degree, just two or three courses. Vance said, "Well, why don't you think about coming to Chapel Hill?" I said I couldn't afford it; I was very naive about graduate school. He said, "We could give you an assistantship." So at the end of one year in Montgomery, Alabama, I headed for Chapel Hill to work toward a master's and doctorate in demography under Rupert Vance.

All these chance events, starting with the chance that I got to the Census Bureau on the basis of a statistical register; the chance that C.A. McMahan had a job that John Folger could communicate to Parker Mauldin got me down to Montgomery, Alabama; then the chance that I happened to meet Rupert Vance who was a consultant there and brought me to Chapel Hill to study demography.

VDT: But you obviously took enormous advantage of those chances, because you raced through in just four years--1955 to 1959--to your Ph.D.

NAM: Well, they weren't easy years, for several reasons. One was I really wasn't fully prepared for graduate school. When I got out with a bachelor's degree I worked for the Census Bureau and I really didn't know anything about how to be a graduate student. I found as models the ones who were hard workers and I kind of trailed after them to see what they did.

VDT: Do you remember who some of them were?

NAM: There was one in particular--he isn't a demographer; he's a sociologist now at Texas A & M University--Al Schaeffer. Al was one of those people who worked literally half the night, so I arranged to get my desk placed in the same area where he was and became very close friends with him. I was prepared to work hard, but I needed that role model. He was a year ahead of me. I just worked very hard. I was a research assistant to Rupert Vance for part of the time.

Finishing the master's wasn't that much of a problem, and finishing all the courses toward the doctorate wasn't that much of a problem. Reuben Hill was a professor there at the time. Dan Price was one of my professors; I took population statistics with Dan. There were a lot of linkages there. Rupert Vance was very brilliant, another former PAA president.

VDT: I want to ask you about him, because he comes up often in your work, your dedication to him.

NAM: Yes. When I got through my comprehensive exams, I knew I had to start thinking about leaving the university and getting a job. I had met my wife, who was a graduate student also in sociology--sociology and anthropology; she was really more interested in anthropology. We got married New Year's Day, 1956, while we were both still graduate students. In fact, it was five years before we found time to have our honeymoon--literally--and we came here to New Orleans for our honeymoon! In late 1957, I had just got my master's degree and had the prospectus for my dissertation. The Census Bureau was then building up for the 1960 census. I got a call from Paul Glick and he said, "You were fired before, but we need to hire people and we have an opening in my branch to head up one of the sections. Would you be interested?" I said, "Certainly I'd be interested, but I want to finish my Ph.D." And he said, "Well, the problem is, it's almost now or never."

I had a real dilemma: Could I finish? It was a matter of starting almost from scratch on my dissertation. I had a prospectus that was approved, but I hadn't even assembled my data yet, much less analyzed it. In those days, there were no computers; people used Monroe and Marchant and Frieden hand calculators, and I had lots of statistical computations that I had to do and write up the results. I said, taking a full-time job, I'll probably never get it done. My wife and I had some long talks about it. We said probably I was going to have to get a job sometime, and she said, "Is this the kind of job you'd want?" I said I had enjoyed working for the Census Bureau, so she said, "Why don't you take it and we'll just see if you can't get that dissertation done." So I went off ABD [All But Dissertation] back to Washington. This was late 1957. I started collecting my data, found a lot in the Library of Congress.

VDT: What was your topic?

NAM: This goes back to my parents. I was interested in immigration and the adaptation of immigrants to U.S. life, particularly socioeconomic mobility. I was reflecting on my own status relative to my parents and got curious about, Is this a general phenomenon? I couldn't be the only one; I knew for a fact there were a lot of second-generation people that have succeeded where their parents didn't have much in the way of resources. We were not well off; my parents ran a small shop. So I got interested in that as a research problem.

My prospectus was to take census data, mainly the 1950 data because the 1960 census hadn't been taken yet; there's very good information on socioeconomic characteristics of nationality groups by generation. I must have hand-calculated about 150 statistical tables and each one had nationality groups, looking at their educational and occupational distributions. Talk about multiple standardization as a technique in demography! My proposal was to control for the effects of age, urban/rural residence, and sex, and I did multiple standardization, nine categories, on each distribution--several hundred of them. I knew I couldn't do it by hand. I needed a calculator and I couldn't afford a calculator. So I went down to the Marchant calculator company in Washington, D.C., said I was working in the government but was a graduate student as well, working on a dissertation: Could they let me rent a calculator at some reduced rate? Apparently they were impressed with my sincerity and let me have it at no charge for up to three months.

VDT: Wonderful story!

NAM: I took the calculator to our apartment and for three months--nights when I got home from work and on weekends--I did nothing but punch out multiple standardizations for distributions. Then I returned the calculator and set about doing the analysis. It took me a long time to write it up and I had to mail chapters back and forth to Vance. It took me a year and a quarter from the time I got to Washington till I got the dissertation done and then went back to defend it. [Nationality Groups and Social Stratification, Arno Press, 1980. Also published as an article, "Nationality Groups and Social Stratification in America," Social Forces, 1959.]

VDT: I think that still must be a record, for data collection through to the end. Was your wife working then?

NAM: She was teaching sociology and anthropology at Prince George's Community College. She's still a community college teacher today, in anthropology.

VDT: Let's talk about Rupert Vance, who was obviously such an influence on your career. I never met him.

NAM: He was a very interesting man. He got polio as a child, but he lived well into his seventies [died in 1975]. According to the authorities on polio, he was the longest-living child polio case ever in the United States. His legs were completely deformed, but the upper half of his body was powerful because, until his very late years, he would never go in a wheelchair; he would go on crutches. In the alumni building at Chapel Hill where the sociology department was located, you had to go upstairs to the offices. He would never let anybody carry him or help him; he insisted on going through doors, climbing up the stairs, with his crutches. He was a dwarf because of his leg problem, but he had a tremendous upper torso, strong arms and chest. People admired him greatly for that. He had a brilliant mind; very imaginative. And a good sense of humor; always a twinkle in his eye. He was a Southerner, born in rural Arkansas. His mother was a school teacher and she encouraged him to go on. He came to Chapel Hill and studied under Howard Odum, got his Ph.D., became a professor.

I first met him when I was at Maxwell Field, Montgomery, Alabama, but I had contact with his writings before then, because when I was working with Paul Glick and Henry Shryock, they kept a small library in the office at the Census Bureau, and one of the books I picked up and read was All These People [1945], which Vance had written about the population of the South--one of the first descriptive and analytical studies of the Southern population. I was fascinated because it was more than the humdrum census statistics; he put life into them. It was real "social demography," as we

sometimes say today. I found it more attractive than the kind of census publications that come out. So when I met him down at Montgomery, Alabama, and he talked about coming to Chapel Hill, I already knew a lot about his writings, and I was eager.

He drove a car, despite the fact that he had non-functioning legs. He had a steering wheel with hand brakes and so forth until he got more feeble in later years. He was a great inspiration for a lot of people. He had a lot of students who went on and did very well in the sociological profession, several in demography. Dan Price was one of his students; Margaret Hagood, another former PAA president, was a student of his.

He inspired me a lot. One funny story. Before I got started on the writing of my dissertation in Washington, I had all those calculations to do, and I spent about a year when I was working on Census Bureau reports, and you know how dull and boilerplate they can be. Apparently, it dulled my writing style, whatever writing style I had before. When I sent the first two analytical chapters for my dissertation to Vance, he sent them back with a note saying, "What's happened to you? Your writing has gotten terrible." It floored me, and I thought, "I'm really going to learn how to write." It was a challenge; I hadn't been a good writer before, but I was worse at that point. Ever since, right up to today, I've been very conscious about my writing, and I think I'm a pretty decent writer.

VDT: You are. I recently read Population: A Basic Orientation [with Susan Philliber, 1984]. It's a wonderful book, full of cartoons, fascinating.

NAM: That's the Rupert Vance influence. Actually, the first book I did was a reader called Population and Society [1968].

VDT: I was weaned on that in my demography at Georgetown.

NAM: People probably know me for that more than anything I've done because it was before there were very many books on population. It wasn't meant to be a textbook, but a lot of people used it as a textbook. It was a pretty good collection of things, and then I wrote all those introductions to chapters, and I dedicated it to Rupert Vance.

VDT: Yes, you did; I have it on my shelf.

NAM: I did the dedication for two reasons. One is that he did stimulate me a lot and really launched me into the population field, and the second thing is no one had ever dedicated anything to him. Most distinguished people have had dedications, recognitions, of various kinds. Everybody respected him, but nobody actually awarded him in any way. I didn't do it out of pity, but rather out of respect and gratitude.

VDT: He must have been very proud of you, because that was less than ten years after you'd done your doctorate and here you were producing a book.

NAM: Yes. His wife, Rheba Vance, who's still living in Chapel Hill, remains a good friend. I talked to her about it first, telling her I was doing this book and how did she think he would feel about my dedicating it to him; I said, "Don't mention it to him." She said she thought he'd be extremely happy about it.

VDT: A lovely thought. And you're still writing about him, because you have that article coming up ["Rupert B. Vance on Population," Sociological Inquiry, Spring 1988].

NAM: There's a long story behind that too. It deals with Vance as a demographer and ecologist. About a year after Vance died [1975], I wrote Mrs. Vance and said, "How would you feel about my pulling together some of his published works, some articles, excerpts from some of his books, into a small volume?" I thought the University of North Carolina might be interested in it; he'd been there so long. She said, "I think it's a wonderful idea." However, a fellow named John Reed, then in the history department at Chapel Hill and later a sociologist at Chapel Hill, just prior to Vance's death had talked with him about doing a volume on him, because Vance was not only a demographer, he was one of the last of the Renaissance people: he wrote for history, for economics, he wrote in the humanities. He was a great admirer and personal friend of H.L. Mencken; he wrote some articles for Mencken's magazine out of Baltimore. And he did a lot on the South as a region. Reed and Daniel Singal wanted to put together particularly Vance's works on the South. When they started, they thought about including demography but there was just too much stuff, so they talked to the University of North Carolina Press, which was going to publish this work, and they said, "Why don't you just drop out the demography." They kept in one article, on Virginia's population, but it was more a regional than a population concern. They eventually published it as a sort of tribute to him and his work [Regionalism in the South: Selected Papers of Rupert Vance, 1982]. It didn't include any population.

John Reed wrote me, apparently Mrs. Vance told him about my interest, and talked about the possibility of doing a second volume along these lines. It never materialized, so that's what led me to do this article. I thought the least I could do was to pull some of it together in an article. His publications even on population are spread out among all kinds of journals, and the next generation of students knew very little about him.

He had been president not only of the Population Association and of the Southern Sociological Society but also of the American Sociological Association, the youngest president the American Sociological Association ever had. There's a real distinction there. I was afraid he was going to be forgotten too soon and there were a lot of lessons to be learned from his works, particularly for the young demography students who know very little about the history of the profession or of the earlier demographers. There've been articles about other demographers, and I thought the least I could do was pull some of that together.

VDT: I hope we'll get that for the PAA archives file on Vance [Nam duly sent a reprint of the article]. You must have taken his influence tremendously to heart, because your interests and publications in demography have covered such a range. I want to get to that, why you have interests in so many facets of demography.

But let's go back to your second time round at the Census Bureau, working on the 1960 census. You ended up doing a 1960 census monograph.

NAM: With John Folger [Education of the American Population, 1967], who was the one who helped me find a job when I got fired the first time.

VDT: You must have been one of the youngest authors for such monographs. You're usually a senior statesman in the field before you're asked to do that.

NAM: When Paul Glick asked me to come back to the Census in 1957, the opening he had in mind was for somebody to head up the educational statistics work. So I didn't get into very basic demographic material; I was working with social statistics, first education statistics, then into other things, but all in the socioeconomic sphere. When they set up the census monograph series for 1960, actually they first asked John Folger, who had been doing work with the Southern Regional Education Board and other educational organizations and had been trained in demography, if he would do the

education monograph. He agreed on condition that I would be a coauthor. We worked it out that we would each do half of the book; we divided up the chapters. This is a curious thing, but at that time he had left the Southern Regional Education Board in Atlanta and was dean of the graduate school at Florida State University. I had no idea I was ever going to go there. He was in Tallahassee and I was in Washington; every once in a while he'd come up and we'd talk about our progress. Henry Shryock and Paul Glick were very good to relieve me of some of my duties to help me do my chapters. There were several people working on monographs, including Irene and Con Taeuber, who were doing the People of the United States in the 20th Century volume [1971]. Irene, who was then editor of Population Index and used to shuttle between Washington and Princeton, was given an office in the Census Bureau.

VDT: By that time it was the Census Bureau? At first it was the Library of Congress.

NAM: She worked also part-time in the Library of Congress, but to do the monograph they gave her a place to hide out in the Census Bureau, away from the demographers' side. When they decided they'd let me have one day a week to work on the monograph, I said, "It's no sense my doing it in my office." So they said, "Okay, why don't you go hide out in the same place where Irene is?" They checked with her and she said, "Fine," so I shared an office with Irene Taeuber while she was working on People of the United States in the 20th Century and I was working on the education monograph. So I've had contacts with lots of past presidents of the Association.

VDT: It's an interesting network. I'm finding more and more you're all connected with each other.

NAM: Right. Well, partly it's the nature of the profession. It's still not terribly large. You can tell at the Population Association meetings it's a very intimate group. It's maintained that intimacy.

VDT: By previous PAA standards, it seems enormous. But 2,600 members and a meeting of 1,100 is still small for a professional organization. Someone mentioned yesterday that the Statistical Association is 15,000 members and the Sociological Association is 11,000.

NAM: That's right. So most of my early publications are related to education statistics; I did a lot of that, starting in the 1950s. Then I realized that while a lot of that was interesting, that wasn't what I wanted to spend the rest of my life on. I finally went to Paul Glick and said, "I think I need to leave here and go some place else and I'd really like to go to a university and teach." Paul said, "You don't want to do that; we'd like to keep you here." I said, "I appreciate that, but I'm sort of in a rut with my work and I'd like to get into demography more broadly."

Through John Folger--again!--I heard about this vacancy for a demographer at Florida State University. They had practically no program then; they were trying to build up a population center. Meyer Nimkoff, the very well known sociologist--"Ogburn and Nimkoff" used to be a leading sociology textbook--invited me to come down to Tallahassee and interview for the job, and of course I got it.

It was really curious about Folger's connection. We were still not finished with the monograph, but right at the time I went to Tallahassee to take that job, in 1964, Folger gave up his position as graduate dean of Florida State and took a position for two years in Washington with the Commission on Higher Education. We almost literally exchanged places, because when we moved down to Tallahassee, I gave him my snow shovel and he gave me his lawn mower.

VDT: Fair enough! I still think you did a tremendous job with the two of you in different places. It

seems to me it's taking everybody a lot longer getting out the 1980 census monographs. The Suzanne Bianchi-Daphne Spain book on women [American Women in Transition] was the first out, and that was only in 1986. Jay Siegel just handed in his first manuscript for his aging monograph. You people must have worked faster.

NAM: Possibly, but we were probably facilitated more than the people are now. I think there was more of a commitment to the monograph program in 1960.

VDT: Was that the first time there were monographs?

NAM: The first series was based on the 1920 census, but there were just a few, done during the 1920s and 1930s; some were done late. Then there were no more for a while [1950].

VDT: Who inspired those monographs--the Taeubers?

NAM: It was the Social Science Research Council [in 1950 and 1960]. Conrad Taeuber was involved with the Census end, certainly. I think the monograph series was a stimulus to research generally, using census data; it had to be based at least partly on census data. Herman Miller was the first one to finish, on the income of the American population [Income Distribution in the United States]. The Taeubers' was the last one [of five] to come out, because it was the most voluminous.

VDT: I love to tell how it was an excellent thing to sit on at my typing table; made me just the right height.

NAM: There were some monographs that were supposed to come out and didn't, but I won't mention any names.

VDT: Now you found yourself in Florida, where you did have a chance to branch out into many other branches of demography. Since then your teaching and your publications have covered an enormous range: education, social stratification, general demography, migration, mortality, population stratification.

NAM: Yes, I guess some people would say being a generalist like that, I know a little about a lot of things but not a lot about anything. Most people in professions stick in one fairly narrow area. I got started off with that socioeconomic material from the census and the reason I left the Bureau was that I wanted to become more of what I would call a general demographer, and I had to build up that program in population at Florida State.

There was no population program whatsoever, only one undergraduate and one graduate course at the time. I was just hired in the sociology department until I could see what I could do about building it up. First thing I found out was that the federal government through the National Defense Education Act was giving fellowships for new programs. So even before I arrived at Tallahassee I worked with Meyer Nimkoff to apply for some fellowships, particularly for population, and we got four. When I arrived in Tallahassee I could immediately hire four graduate students to start building up the program. Introduced new graduate courses. Now our center is built up; we have about 12 different courses in population. Sometime or other I've taught all of them. In fact, I was the first to teach every one of the courses offered, although now with a bigger staff we have four people teaching.

We started from scratch. There were some resources. Those were the days when money, federal money in particular, was still plentiful and you could do something, in contrast to today. This

was the fall of 1964 when I got there.

VDT: Did you ever get money from the foundations--Ford, Rockefeller?

NAM: Not early on. I got some grant money--well, I got any kind of money I could get. Not only the fellowship money, but Basil Zimmer and Mary Powers and I had an idea for doing some analysis of census data and we got some money from NIH, part of which came with me to Florida State University.

Mary Powers had been an assistant of mine at the Census Bureau and just before her, Ed Stockwell had been my assistant. There were a lot of people who are well established demographers today who came through that Population Division at the Census Bureau: Harry Rosenberg, Bob Parke, John Beresford, David Heer, Patience Lauriat. We all worked together under Paul Glick. There's an old photograph of us and we're all junior people [photo of Social Statistics Branch, 1960, donated to PAA archives by Paul Glick and displayed at the 1991 60th anniversary meeting in Washington, D.C.]. It's amazing how many of them really stuck in the profession.

So I did get some grant money, and at the time I was hired at Florida State, three other people were hired simultaneously.

Then in 1964, 65, James Coleman's famous study of equal opportunities in education, the original Coleman, study was being fielded. Most people don't realize that in addition to the surveys that Coleman did of the school system, there were several associated studies. Coleman wanted to include one basically demographic study, so he gave money to me and I asked two of my colleagues at Florida State, Lewis Rhodes and Robert Herriott, to join me in doing a study based on the Current Population Survey of school dropouts, comparing blacks and whites. That got published as one of the chapters [Chapter 6, "Nonenrollment"] of the famous Coleman report [Coleman et al, Equality of Educational Opportunity, 1966]. Not very well known, but if you look in the fine print at the front of the book you'll see it there.

So we attracted some students; we managed to get a program going. It took a long while, because by the time I was really moving on the graduate program, money started to dry up; it was harder to get new positions at the university. So it was from 1964 to maybe 1970 before I could really get a population center [Center for the Study of Population]. We hired some people before that, but either they didn't stay or they didn't work out. In 1970 David Sly and Bob Weller came along. That was sort of a turning point, a threshold, and the center really started.

VDT: You said in the session yesterday on the "Graduate Training of Demographers" that some students go to the wrong centers because they don't pay attention to what different centers offer. What did you say the place of Florida was?

NAM: My notion of a center was that it should be very broad. I think if you look even today at all the population centers, you'll find different philosophies of graduate training in demography. Some intentionally are narrow; they specialize in certain areas and all the faculty they hire are people who are interested in those areas. The notion is, as Ron Freedman mentioned yesterday ["The Michigan Model"], you don't have many courses and you have internships where people sit before gurus and take everything in and they're all role models. That's one way you can train people.

I thought a better way was to give more training through a lot of courses in which they become familiar with all the theory and methodology and substantive areas so that they could choose as to which they wanted to pursue in their own careers. If they're only exposed to narrow areas, they'd never be even familiar with some other things. My approach was much more eclectic and broad. That's why I introduced a lot of courses; we did have more of a course approach. Students did work as research

assistants as well. But my philosophy was one of, "Let the people know what the scope of the field is."

VDT: You certainly did that, and in your writings too.

NAM: You realize that the Population and Society volume [1968], which was my first, came just within a few years after I'd gotten to Florida State University. Through Meyer Nimkoff I got in touch with the Houghton Mifflin people. I'd published a few articles, but nothing substantial before. I said this was what I had in mind to do and they said, "Okay, go ahead," and gave me a contract and, of course, I whipped the thing out as fast as I could. It defined in a way my notion of what demography was. It was very much tied in with social sciences, called Population and Society. It was not just what we call formal demography today. I was trying to show both the causes and consequences of demography.

VDT: You carried out that theme beautifully in your 1979 PAA presidential address.

NAM: When you get to be president, I'm sure every one of them, like Ren Farley today, had to decide what he was going to do for a presidential address. You don't know whether it's going to mark you in the sense that people will always associate you with a presidential address. I looked back at all the previous presidential addresses and realized that some people, probably most people, talked about what they were best known for in their careers. Well, one of my problems was that I wasn't best known for anything, because I was very broad in my coverage.

VDT: Which made it ideal. The title of your address was "The Progress of Demography as a Scientific Discipline" [published in Demography, November 1979].

NAM: There were two things I was concerned about. I thought about it and this was a good solution to it. I felt--this was another influence of Rupert Vance--that there were a lot of things happening in population but they weren't tied together very well. When Rupert Vance was president years before--in fact, it was the first PAA meeting I ever attended, in 1952, while I was a junior professional at the Census Bureau, so I saw him from a distance at that time.

VDT: It was in Princeton.

NAM: Yes, at the Princeton Inn, and Walter Willcox was a guest speaker and Rupert Vance gave his presidential address. It's indelibly marked in my mind, because at that time PAA--and I hadn't become a member until that time--was so small that everybody could go into the Princeton Inn; they had an annual banquet and everybody fitted in one dining room. There were probably 100 people in attendance.

Vance's address was called, "Is Theory for Demographers?" He was doing in a sense what I did later on, saying, What sort of integration is there in population from a theoretical perspective? The demographic transition idea had just become popular, and he was asking, Is it enough? He had some nice lines in there about the need for what he called a "binder" to bring together all the ideas in population. He speculated about what sociologists call middle-range theories, instead of this macroscopic kind of transition idea. It was very stimulating; nobody had written anything like that in population before. And then people started to talk about population theory. So I started thinking that 27 years had passed, 1952 to 1979, and nobody had a presidential address in the meantime which was anything like trying to tie things together. So I thought it was time for the profession to say, "Let's take stock of where we are." That was one motivation.

The other one was there was a debate going on, which probably still goes on today, as to whether demography is a discipline in itself or just an interest within the social sciences--sociology, economics, and so forth. I was one who felt it was a discipline, and sort of a focus of my paper was to indicate why I thought it was a discipline, which required defining what a discipline is and then showing how demography fitted the characteristics of a discipline.

You had to separate it from what was typically a department in the university. We don't have many departments of demography; even Ph.D. programs in demography, there are just a few now. But it doesn't mean it isn't a discipline. You find departments of demography in Europe and Australia. It's just not in the university tradition in the United States. Areas like demography that are interstitial between other traditional departments, like molecular biology--you don't find a department of molecular biology, because it's between physics and biology and chemistry. But that's where the real cutting edge in the natural sciences is; the most successful program in my university is in molecular biology. I see demography the same way; it's an interstitial area between sociology and economics and geography and other things. And it's got something important to say.

I tried to make the point that what made any subject area a discipline was the central subject matter, not the methods or the theory, because we borrow from each other as far as the theory and methods go. Sociologists, economists, and geographers look at population, even psychologists. But for none of those disciplines is population central. What made demography a discipline is that the central focus of demography is population, how it changes, what affects it, and what the consequences of it are. Being a central concern, that made demography a discipline.

VDT: You said that brilliantly; it was a very fine presidential address. Recently in my interviews in this oral history series, people have said perhaps more presidential speeches should take the opportunity to speak on such broad things, though perhaps not every year.

NAM: I heard very few comments about my presidential address. I got favorable comments at the time.

VDT: You've covered a lot of what I wanted to ask about what accounts for your great overarching view of demography. How have you managed to work it all in? You have a tremendous record of teaching, publishing, serving in many organizations; you've been active in a number of professional organizations besides PAA.

NAM: I'm a fairly well organized person, as you've commented before, and I haven't really learned how to say no to offers to do things. People who are reasonably well organized are often asked to undertake professional association tasks. So I've been involved in PAA, the American Sociological Association, American Statistical Association, AAAS, and so forth. I've enjoyed all the experiences, and I'm still very active in a lot of these things. I guess I'm kind of an organization man, for one thing.

VDT: Besides Rupert Vance, who have been some leading influences in your career?

NAM: My bosses at the Census Bureau, Paul Glick and Henry Shryock, in particular, and I had a lot of association with Con Taeuber. And some of my other colleagues at the Census Bureau. Probably Paul Glick was the strongest influence.

When it came time to give my presidential address, you know presidents always have the option of asking somebody to introduce them. In some associations, the vice-president automatically does it, but the tradition at PAA is for the president to pick somebody he wants to introduce him, so Ren Farley had Les Kish today. I asked Paul Glick to introduce me. With the sense of humor Paul

has, it was a delightful introduction.

VDT: Do you have a copy?

NAM: No.

VDT: I bet Paul has; he's good at keeping things [but had not, alas, kept this item]. Tell me about working with Paul. He's well known, and he's here today; he's always at the PAA meetings.

NAM: We've been good friends over the years. There's an interesting thing about the Census Bureau, which is one of the reasons I didn't stay. People talk about the "1940 cohort" of Census employees: Glick and Shryock, Henry Sheldon, Dave Kaplan, and so forth. The Census Bureau wasn't much of an agency and the 1940 census was the first one where they really built up a professional staff. So all the young recent Ph.D.s who were coming out trained in the social sciences were recruited to the Census Bureau at the same time. By the time I got there, they were mostly 15 to 20 years older than I was. They weren't anywhere near retirement yet and they were occupying all the key positions. Where was I to go after I got . . .

VDT: Sounds a bit like the baby boomers.

NAM: Exactly. It's the same phenomenon. They were the baby boom cohort for the Census staff, and I was in the small cohort that came along [in 1950], most of whom got fired, some managed to come back, but the opportunities weren't there. It was the same kind of story as the baby boom, because when they retired, they all retired in a short period of time. And since the next cohort or two, including people like myself, weren't there anymore, the people who replaced them in the key positions were two or three generations later, who lacked the history, the experience, and they were doomed to make all the mistakes that everybody had made before, before they learned how the whole operation should work.

VDT: That's interesting. I hadn't heard that.

NAM: I think when Census people get together you hear it; it isn't something that gets discussed publicly very much. The people in key positions now, the kind of positions that Shryock and Jay Siegel were in, don't go back very far. There's a real discontinuity there.

I can point to one right now. I was talking to Gordon De Jong earlier today at the session on the 1990 census plans, since I'd worked with education data a lot, this year they changed the question on education in the census. They changed it in two ways; it's the first time the question has been changed in maybe three or four censuses. One change is very good, I'd been pushing for for years and couldn't get done, that is to start asking for degrees instead of just years of schooling: Did you get a bachelor's, master's, Ph.D., and so forth? The other thing, however, is very unfortunate. They may not be aware of it but they've gone back to the question we had on education in 1940, the first time we ever asked a question, which was changed in 1950 and subsequently because of very poor reporting on the question in 1940. That is, people were asked how many years of school they completed. We realized that people were upgrading their education; if they'd started a year and not finished it, they were reporting having completed the year they started, not the year they completed. So in 1950 and thereafter, the question was broken up in two parts: What is the highest grade you attended? and then, Did you complete it or not? And the reporting was improved vastly, according to post-enumeration studies. So in 1960, 1970, and 1980, that two-part question was retained. Now somebody in their wisdom in the Census Bureau is saying, "Why do we have two questions? Why don't we just ask the

single question?" And they've gone back to the 1940 question, except for the additional information on degrees. It's going to destroy comparability with the previous censuses. I think this is an example of that discontinuity; people lack the history, the experience. It's all there in the files, but they probably haven't gone back.

VDT: Well, just the reason for this oral history project is that PAA lacks files. Perhaps some of it's in papers, but they found that just talking about it too helps a lot.

NAM: I'd like to add something when you talk about the breadth of my interests. I took a real turn in my major interests about a dozen years ago, and you noticed that I've been doing a lot of mortality research in recent years. I think that bears some explanation.

Again there's a long history about it. I always had an interest in mortality analysis, but never had a chance to practice it. Working at the Census Bureau, you wouldn't think I had an opportunity to do anything in mortality. But when I returned there in 1957, I met Lillian Guralnick, then with the National Office of Vital Statistics, the predecessor to the National Center for Health Statistics. She had been doing occupational mortality analysis and published a number of things with Iwao Moriyama. She said, "We need to get better data on occupational mortality, because the occupational reports on the death certificates are terrible." So we started talking and came up with an idea; I don't know whose idea it was but we worked together. That was to take death certificates and match them to the census records of the same people. You find from the death certificates all the people who died, let's say, in the four months following the 1960 census, who would have been enumerated in the 1960 census, though you don't know they're going to die, and you go back and find their records in the 1960 census. In effect what you're doing is identifying a subgroup of people enumerated in the 1960 census who were about to die. What that enables you to do is take all the information on their death certificates and link it to their whole census record, not only their personal record but the family record, the area they lived in, and it would be a tremendous storehouse of knowledge. Not only would you have good occupational data from the census, you'd have everything else from the census.

VDT: And you were allowed to do that?

NAM: No. That's the interesting story. Lillian and I said, "Let's test it out," and we got permission from our superiors to do that in connection with the pretest of the 1960 census, which was in Memphis, Tennessee. Lillian and I went and worked with the vital statistics people in Memphis and got the death certificates of people who died right after the pretest and then tried to see if the matching operation worked. It did work, and we published an article with Con Taeuber's assistance in the Milbank Memorial Fund Quarterly ["Census-NOVS Study of Death Certificates Matched to Census Records," MMFQ, April 1959]. That was one of my first articles.

Then we said, "Okay, it's feasible to do this; let's go ahead and do it." I guess we were both naive. We tried to get the health agencies to support it and they said, "You're both employees of the government; we can't give you money." We said, "That's unfortunate, because we think this would be a good study." They said, "If you can identify a university to whom we can give the money, we can give a grant to a university, not to the Census Bureau or Vital Statistics. If Congress in their wisdom had decided you should be doing it, they would have given it to you in the first place."

So Lillian contacted Phil Hauser and Evelyn Kitagawa at the University of Chicago and that was the beginning of the famous Kitagawa-Hauser study of social and economic differentials in mortality, which was a monograph in the American Public Health series of Harvard University Press [Differential Mortality in the United States: A Study in Epidemiology, 1973]. Lillian and I were the ones who conceived the study, but Chicago sent it in as a research proposal. It got the money from

NIH. Evelyn, as project director, got Lillian and me involved. Lillian handled all the death certificates at the Vital Statistics end; I handled the Census work, including the matching of records at the Census end, because it had to be done confidentially. We created the blended records and then turned it all over on computer tapes, without identification, to Evelyn, who did the monograph and worked long and hard on that study. There was another case, like the Coleman study, where I had a substantial input, and Lillian Guralnick, but we appear as a footnote, or an acknowledgement at the front of the book.

VDT: But you really inspired the whole thing!

NAM: Yes. Of course, we got that Milbank article in 1959, which is evidence that we pretested the whole idea, but it was on occupational mortality initially.

So I got my interest whetted in mortality analysis, but didn't have an opportunity to follow up at the Census Bureau, because they have no mortality data, except for the matching study. When I got to Florida State, I was so involved in building up the program that I didn't do much in the mortality area. I always had it in mind. Then along the way I found a niche and for the last ten to 15 years I've been trying to make that my concentration. You can see from my publications list, I've done quite a lot in mortality, and I feel I'm just really getting going.

VDT: You've managed to fit in other things as well. You have joint books on internal and international migration [International Handbooks on Internal Migration and on International Migration, with David Sly, William Serow, Robert Weller, and others].

NAM: Yes, the internal one, with colleagues. It's a book 25 people have contributed country chapters to from all over the world. The first volume is finished; the second is almost finished. But that's a sideline, it's something that needs doing. I didn't volunteer to do that; the Greenwood Press wrote and asked if I'd be interested.

VDT: What about Our Population: The Changing Face of America [1988]?

NAM: That's another interesting story. I've always been interested in population socialization.

VDT: Population education, isn't it?

NAM: Not really; that's a component of it. I have an article that provides the sort of framework for all of this, from a lecture I gave in Cairo some years ago ["Education, Learning, and Population Change," The Egyptian and Family Planning Review, June 1982]. The idea is that people learn about population in both formal and informal ways. Most of what we learn about population behavior is very informal, through our parents, peers, observing life around us, and so forth: desired family size or the pros and cons of moving your residence, leading a life style or taking risks that affect your life expectancy, and so on. The population socialization idea is that we get socialized into the norms of demographic behavior. I published a few articles on that.

Population education, which is trying to teach people about the causes and consequences of population, is one component within the broader context. It relates to my publishing of textbooks, but it also goes back to the fact that when I really learned how to write, I felt that something the field needed was to communicate better, that demographers needed to be speaking to the public. There were very few people in the profession who have done that. Kingsley Davis was sort of the model for it. He was berated by many of his colleagues in the profession for writing articles, even in The New York

Times. He wrote one on population and they said, "That's not a professional journal; you're belittling yourself." People had a very purist view of demography. My notion was that he was entirely right. I had some long talks with Kingsley on that. And I said, "I'm going to try to popularize demography as much as I can."

One of the ways--getting back to this book that just came out [Our Population: The Changing Face of America]-is through population education. I've been writing materials for secondary schools. This latest book is a general population book for middle and secondary school students. It's in a new social studies series by Walker Publishing Company, an educational publishing company in New York. It's got ten short studies. Leon Bouvier did one on immigration and Isaac Asimov has one on science and society, so I'm in excellent company. It's got very innovative titles. It's not like the old social studies; there's one on terrorism written by one of the hostages in Iran. I didn't think I wanted to do it, but they talked me into it when they told me who else was involved. So that book is one on population written at the language level of beginning secondary school students. We're anxious to see if schools pick it up.

VDT: Do you have any students who've developed such broad interests as you?

NAM: Not who kept broad interests, but who followed up on some of these areas. One is Susan Gustavus Philliber, who went to Columbia University School of Public Health, one of my prize students. I published some with Susan, including the textbook [Population: The Dynamics of Demographic Change, 1976, and Population: A Basic Orientation, 1984]. After she got to Columbia, she published a lot on teenage childbearing. She's had a pretty good career; I'm kind of proud of her.

There's an interesting story about her. She was an undergraduate at Florida State when I first got there. She had just come as a junior transfer from what was then Florida Presbyterian College, now Eckerd College; the Eckerd Drugs people bought it out and changed the name. She had a very good record and I had a grant I brought with me and needed a student assistant to do clerical work with population studies. Somebody said we have a student who needs some money and sent her to me. I told her about my research project and asked if she'd like to be my assistant; she was just a junior, sociology major. She said, "Yes, I need the money, but I'll tell you right now"--I've always reminded her about this--"I'm not interested in population whatsoever. If we just make it a business arrangement, I'll do the work for you, but don't try to interest me in population." Well, the short story is, she worked for me for two years and went on and got a master's and doctorate and got established in demography.

VDT: You must also have communicated your excitement about the field.

NAM: It's very fortunate that she came along. But we haven't had a lot of people who became real stars. Of course, we're still a young program. One of my former students is Russell Thornton, who was one of the few people with Native American background, a Cherokee Indian, who got his Ph.D. in sociology at Florida State, and demography was one of his areas. He's been at the University of Minnesota for a number of years. He's had grants studying American Indian population. I just heard from him that he got an appointment to Berkeley, so we're very proud of him.

VDT: Talk about being proud, what accomplishments in your career have given you the most satisfaction? I think you've had a lot of accomplishments.

NAM: I don't feel that way myself. I was talking with one of my sociological colleagues a few weeks ago, who'd come from Johns Hopkins so I think very highly of him, and he said something like that to

me. I know I've done a number of things. In terms of my own personal standards, I don't feel I've really achieved the real accomplishments I would have liked to achieve. There aren't any great ideas associated with my name.

VDT: Would you have liked that?

NAM: Well, I don't think my career's at an end yet. I just turned 62. I'm doing as many things now as I've ever done; sort of have a second wind. One of my younger colleagues and I are working on some infant mortality research which we're very excited about. Got some new ideas there.

VDT: In the U.S.?

NAM: It's actually data for the state of Florida; matched three different kinds of records together and are trying to integrate a fourth record. I'm using some of my old experience on matching records from the old Kitagawa-Hauser-Guralnick stuff. Today the methodology is much more advanced; conceptually we're much more advance. So I think some exciting things will come out of that study. I don't think that's the end of it; I think there are still other things I have ideas about that I'd like to develop.

I've also gotten interested in international areas. Most of my career has been concerned with research on the U.S. At Florida State there are a lot of international students, the largest groups being from Kenya and Indonesia, and a smattering of other countries. I spent a little time in Kenya a few years back, and there's a possibility I may be going to Indonesia very soon to do some work there, in Jakarta. [He did spend 1988-89 and another two months in summer 1990, at least, in Jakarta as adviser to the Ministry of Population and Environment.] A lot of our most successful students have been foreign students, who've gone back and done very well.

VDT: Do you feel there's still a great need for the American population centers in training Third World students?

NAM: Yes. But, you know, most of them do have connections with overseas places. Without being critical of Third World countries, I think after these schools have had connections with [Western] countries for a number of years, you don't find the countries are any better able to conduct their own population research or deal with their own population problems without the outside consultation. My idea is to go in there and really build up the capability of the people in the countries themselves to do what they need to do--create an indigenous set of demographers. Not that they would not be associated with anybody in the U.S. or other places. If you look at a lot of these countries, they're still highly dependent on the U.S. and other consultants. I don't know if it's done intentionally, but the fact is they haven't developed their own capabilities. So if I go to Indonesia, what I would really try to do is build up Indonesian ability to be their own demographers.

VDT: That's always been the goal, and, of course, there are some independent centers, certainly in India.

NAM: That's true. But I think they use an analogy with the family which is not perhaps a good one--parent-child relationship. Of course, we do take a paternalistic attitude toward a lot of these people in foreign countries. We feel they're still dependent on us; we don't let them shake loose from the apron-strings.

VDT: Has Florida trained many people who've gone into the applied field? We hear so much now of demographers going into the applied field.

NAM: We were one of the first programs that did a lot in that direction.

VDT: People going into state and local government?

NAM: I suspect we have as many people in state and local demography as any population center in the U.S. The state demographer in Oklahoma is one of our graduates, and the state demographer in Oregon. The person who was the state demographer in Washington, far from Tallahassee, is one of ours; he's now the demographer for the city of San Diego. The person who for a while was the state demographer in Florida is one of our graduates. We have people in various state and local government agencies, a number of people in federal government.

VDT: And also in business?

NAM: One of our graduates, Louis Pol, now at the University of Nebraska [later Rollins College, Florida], wrote the first textbook on business demography [Business Demography: A Guide for Business Planners and Marketers, 1987]. We're very proud of him. We have three people who are now in schools of business: in Tennessee, Pol in Nebraska, and one other. They are in schools of business but they're seen as demographers. We were doing it long before it was fashionable.

That's another thing that some centers look down on. I think the purists among us say, "Demography is formal demography; other people do the applications." But our center's view--and I guess it was my idea originally--is that you train people broadly in both the basic scientific approaches and the applied area, so you give people an opportunity to decide where they feel most comfortable in spending their careers. And, of course, the opportunity structure has changed so that there aren't many positions as demographer in the universities and there are a lot of them in the applied field. We have people all over the place.

VDT: Do you feel that's where U.S. demography is going?

NAM: Well, I think that's one of the areas we have to prepare people better for, as pointed out in that session yesterday on the graduate training of demographers. Gordon De Jong talked about that ["Academic and Applied Demography Training: The Same or Different?"]. We have to give them the basic science, but we also have to give them a notion of what's involved in applying it. I don't find I'm in tune with, say, Peter Morrison, who was talking about developing client relationships in a sort of consulting framework ["What Tomorrow's Demographers Will Be Called Upon to Do"]. What we're talking about is more what Gordon was talking about, training people to behave as demographers but in applied settings. They have full-time jobs, not just consulting with clients. Bowling Green has built up a center, the only center that's been built up in the last ten years strictly on applied demography. We had it quite a while back.

All we have is a lot of different kinds of students. We have our international students. We have a certain group of U.S. students who are still oriented to academic careers. And then we have a group that are interested when they first come in going into applied settings.

VDT: You say we can have a bit more time. You have a student prepared to set up the table for Florida State at what's going to be the first Alumni Night Party at a PAA meeting. Instead of all sequestering ourselves away in hotel rooms and everybody wanders from one party to another, it's going to be one big mass in a ballroom.

NAM: I hope it works out.

VDT: I was amused at Ren Farley ending his presidential address with his words of French--"Here we are supposedly in the heart of Cajun country." We're going to have a band of six Cajuns.

Let's turn to your PAA connections. You've answered my first question: Can you remember the first meeting you attended? You said that was in 1952 at Princeton. Rupert Vance was the president. Who did you say was the guest speaker at the banquet at the Princeton Inn?

NAM: Walter Willcox. What was interesting about Willcox--of course, he had written lots of things about population--but he lived to a very ripe old age; I think he died at age 105 [103]. And up until the age of about 102 or 103, he still testified before Congress; he was interested in legislation on apportionment; he was considered an expert on that.

VDT: At those early meetings, they had a guest speaker at the banquet as well as the president's address?

NAM: Well, this was a special occasion for Willcox. It was a birthday of his, ninety something. He stood up and gave a half-hour extemporaneous talk, mostly about his career.

VDT: Can you remember other leading lights in the early meetings you attended?

NAM: In 1952 most of the early demographers, the early presidents, were still around. The earliest one that I had actual associations with was Frank Lorimer.

VDT: You said you took courses with him.

NAM: Yes, I took two courses with Frank Lorimer, so he had some influence on my life. Conrad Taeuber, Phil Hauser whom I knew through the mortality study and other things, Harold Dorn, Warren Thompson--all these people, except for the first five or so PAA presidents whom I didn't know, were people that were still alive and active in the Population Association when I first joined. It was a real link to the past, the origins of the Association. Henry Pratt Fairchild [first PAA president, 1931-35], for example, was at that 1952 meeting. There were so few there that everybody could stand up and introduce themselves at the banquet.

VDT: I've also heard from Jack Kantner that he went to bed that night in the Princeton Inn and when he got up next morning he had an unexpected roommate.

NAM: I've heard that story--Dorothy Swaine Thomas.

VDT: No, it was you!

NAM: Yeah, but that's not the whole story. Dorothy Swaine Thomas signed in at the Inn, I think, as just "D.S. Thomas," and since the Inn was small and even though there weren't a lot of us, they encouraged people to double up. If you'd arranged in advance to share a room with somebody that was fine, but if not, they paired people off. So they paired Kantner off with D.S. Thomas! When he got up to the room and found out what was going on, it wasn't quite right. That's how I became Kantner's roommate. They assigned her to somebody else's room and when I came in, they assigned me to the room with Jack Kantner. [Kantner later told Nam that he also recalled that "D.S. Thomas" was first

assigned to the room of a male PAA member, but it was not his.] Kantner and I didn't know each other before that time. He was right out of Michigan, being a protege of Amos Hawley. We met, became good friends, and a year or so later, I got to work with him at the Census Bureau in International Population.

VDT: So you knew all those people well. You said you actually shared an office with Irene Taeuber.

NAM: Yes, for four months while we were both working on 1960 census monographs. That was an experience. She was a very interesting person. The only thing that bothered me about her was that she was a chain-smoker. I've never smoked in my life and I don't like smoking and I had to put up with it. I didn't want to be rude, but she just smoked incessantly, as did Dorothy Swaine Thomas.

But just the opportunity to talk with Irene was interesting. Every once in a while when she was working and came across something, she'd throw an idea at me and sometimes I'd do the same. We had two desks; it was a fairly large room in another part of the building from where the Population Division was. Nobody even knew we were there, except a few people like Con Taeuber and Shryock, so we were not disturbed. We weren't always there at the same time, but frequently we were. We just tended to our own business, writing or calculating. I had no assistant; Meyer Zitter's wife helped Irene.

VDT: You were typing when you wrote?

NAM: I've usually written everything in longhand.

VDT: Do you use a word processor now?

NAM: I've got one at home. The secondary school book on population I just did, I did that in about two-and-a-half months on a word processor at home.

VDT: Tables and all?

NAM: There aren't any tables in there. This was done for middle school and secondary school students and the idea is to communicate ideas in words. There are numbers there, but no statistical tables. I had to think about new ways of taking ideas and technical terms of demographers and telling it to middle school students in simple language. It was a real challenge.

VDT: What about Phil Hauser?

NAM: When I first arrived at the Census Bureau [1950], Phil had recently left and returned to Chicago. He had been deputy director of the Bureau when J.S. Capt was director and Capt died suddenly and Phil got pushed into being acting director for a while. The Census Bureau director's position is a political one and the Democrats at the time appointed Roy Peel as director, as a political patronage. Hauser returned to Chicago to start up the Population Training Center there.

So I did not meet him then, but he was chairman of the Census Advisory Committee, which included people like Don Bogue, Dudley Duncan, P.K. Whelpton, and Dorothy Swaine Thomas--an illustrious group. I was assigned to be secretary to take notes at the committee meetings. Otherwise, I wouldn't be allowed into that room. I got to hear all the debates they had about census planning, and then I had to write up the minutes, and send them to Henry Shryock for his approval, and then to Con Taeuber. So I got to meet a lot of the great minds that way.

VDT: Did you ever know Dudley Duncan?

NAM: Oh, yes. That was my first occasion of meeting Dudley. Later on, people who work in the social stratification area know that we became competitors in a sense. I got an idea for taking occupations, which were nominal categories, and trying to develop a statistical index of them according to their socioeconomic status. Unbeknownst to me, Dudley Duncan was trying something very similar but using a very different approach. Where I was trying to base them on other census indicators, like the income and education of incumbents of occupations, he was taking studies that had been done, surveys of the prestige of occupations, using that as a basis for giving them a score. When we found we were working on similar but competitive ideas, we got together and talked about it a lot. We never could agree whose approach was better.

VDT: His famous book about occupational/social mobility between generations [The American Occupational Structure, with Peter Blau, 1967] sounds a bit like your dissertation.

NAM: This was before that. He and Peter Blau were getting started on that original occupational changes in a generation study. It was done through the Current Population Survey and I was involved a bit at the Census end. Albert Reiss was putting together this book of NORC studies on occupations [Occupations and Social Status, 1961]. Dudley Duncan took this set of occupational scores and wrote up a chapter for that book ["A Socio-economic Index for All Occupations"].

At the same time, I was trying to convince Con Taeuber and others at the Census Bureau that this idea of mine of scoring occupations according to their socioeconomic characteristics should be something Census did as a Census project. That was more of an academic operation, it was not just tabulating census results, and they weren't convinced that it was an appropriate thing for the Census. They wouldn't adopt it. I was a bit miffed by that. But I said, "Would you permit me to keep working on it?" and they said, "Okay." So I kept working on it in my spare time.

Eventually, Dudley Duncan came to the Bureau to try to get the Current Population Survey to generate some data that he would use in this social mobility/occupational mobility study. By that time, several of my bosses at the Census had become intrigued with what I was doing: Shryock, Howard Brunsman, Glick, and Con Taeuber. So they gave me the go-ahead to do some work on it, officially. Duncan came, he was asked to comment on the work. He didn't particularly like what I was doing, for certain reasons, and I didn't like what he was doing, for certain reasons. There was one meeting when he came, seven or eight of us in Henry Shryock's office, where we discussed in a friendly manner the pros and cons of each approach. End result was we couldn't agree, but all the Census people stayed on my side, because my approach involved nothing but Census data, whereas Duncan's approach involved using data external to the Census as well as Census data.

They allowed me to put out a working paper, Working Paper Number 15, "Census Bureau Occupational Socioeconomic Scores." I've done a lot with that; published a book and a number of articles. I'm still involved in it. And Duncan published his stuff quite independently. When the Reiss book with his chapter came out, he sent me a copy. You know the shorthand for socioeconomic status is SES. On the inside of the front cover he'd written, "To Charles Nam, from one SESer to another." I treasure that a great deal. So I have links with Dudley Duncan.

VDT: Do you remember any outstanding meetings and the outstanding issues in PAA over the years? As you worked up to the presidency, you were editor of Demography.

NAM: The Population Association for me has always been the model of an association meeting. It's not just a matter of its size. The Southern Sociological Society is exactly the same size as PAA; I've

been president of that organization [1981-82]. There's no comparison in terms of how the organizations are organized, how they conduct their meetings. PAA is always a more intimate group. The meetings are structured in terms of time so that you can really participate. There's more of a "we" feeling; people associate with each other; they go to the sessions. Look at the presidential session today; we always have a large crowd at the presidential address session. At some of the others, that's a time for people to go out and do the town. So there's something special about PAA. It's not just the subject matter to which I'm attracted, it's just been an ideal organization over the years. I found that at the first meeting in 1952 and ever since.

But I think some of the things I remember most are some debates. Back in the 1950s, we used to run one session at a time. It wasn't until the late 1950s or 1960 that we even ran two sessions concurrently and you had to choose which one you went to. [Double sessions began in 1956.] So everybody went to every session. There were two big debates that took place in the early years. One had to do with population projections, and the big debate was always between Phil Hauser and an economist named Joseph Davis from Stanford. Davis didn't believe in population projections; he was always making critical remarks and Phil Hauser was always defending them. Every year you could always guarantee that at some point in the meeting the two of them were going to get up and argue. That's one debate that went on for a number of years.

The other one was Kingsley Davis and Frank Lorimer. Their approach to studying fertility was quite different. Kingsley taking a very sociological point of view and the article with Judith Blake on "Social Structure and Fertility: An Analytical Framework" [1956] . . .

VDT: Intermediate variables.

NAM: Yes, intermediate variables, which we now sometimes call something else.

VDT: I was delighted to find that in your Population and Society when I took it out the other night, because I wasn't sure I had a copy of it.

NAM: It's in there. I have a lot of Kingsley Davis in there. I was very much influenced by Kingsley's work; got to know him better in later years.

He was arguing the social structural point of view and intermediate variables. Frank Lorimer had gotten very involved in the more anthropological approach to fertility, spending a lot of time in Africa and focusing on cultural factors of fertility. So when there was a fertility session, inevitably it was Kingsley Davis doing the social structural approach and Frank Lorimer arguing for the cultural approach to understanding fertility. Today as you look back, they were probably closer together than they seemed to be at the time. But everybody looked forward to the session on fertility, which there always was, and knowing that at some point the two of them were going to get up and start arguing.

We don't have that sort of thing anymore and I miss it; getting people up and really having a debate about the issues. Everybody expected it, and the cast would change from time to time. Maybe it's not possible to do that anymore, but I think we could structure it so that we'd have more debates instead of just papers being presented. We do have panels, but the panels end up to be no more than the equivalent of paper sessions. I'd like to see some sessions where we really take an issue that's debatable in the profession and get some people on both sides of it and get them arguing.

VDT: That's the enormous change that's taken place. This time [1988 meeting in New Orleans] there are 84 sessions, eight simultaneously.

NAM: That's right. So my fondest memories are back in those earlier days when the program wasn't

so elaborate, but more importantly, we had these open debates. People enjoyed them greatly.

VDT: I guess nowadays the debates have to take place in the corridors. Do you still feel it's intimate? Well, you have your friends, colleagues, students.

NAM: But the issues don't get discussed in the same way. Somebody might make a speech, like Paul Demeny's presidential address, which had a particular point of view about the "Invisible Hand" ["Population and the Invisible Hand," 1986], and the way people come back with an alternative point of view is in another session in the next year's meeting. But there never was a real dialogue about it. And that's what I'd like to see. If you have an issue like that, get together people with different points of view.

I have an idea for a session along those lines for next year, in an area that I've been concerned about. This is where Ansley Coale and I touch base a little. It's this phenomenon of mortality crossover, which is still a very hot issue.

VDT: Longer life expectancy of older blacks?

NAM: It applies to many different pairs of population, but the one that's cited most often is black and white mortality trends by age in the United States. Starting at infancy, the black death rates are much higher, and when you get to older ages, they start converging and eventually the curves cross over, and at the very oldest ages the death rates are higher for whites than blacks.

Ansley Coale and a lot of his students, proteges, argue that it's an artifact of bad data at the older ages, which is differential for blacks and whites, and there isn't a crossover. Russell Thornton and I wrote an article on that in the late 1960s ["The Lower Mortality Rates of Nonwhites at the Older Ages: An Enigma in Demographic Analysis," FSU Research Reports in Social Science, February 1968]. I demonstrated that it wasn't a function of the data. We took hypothetical data from the Coale and Siegel estimates of errors in the population by age and sex. We applied it to the existing mortality rates and adjusted all of them. We said, "What if the data were corrected in a direction that we know the errors are?" We plotted the curves for blacks and whites and we showed that the crossover still existed, but the age of crossover changed somewhat. Sure, data errors are a problem, but they don't change the underlying phenomena; there is a real crossover, which is a function of selectivity in the populations dying. In the black population, the ones that are weaker and less healthy will die at earlier ages. The ones that survive to older ages are a hardier group than the whites who survive to older ages, who are kept alive by socioeconomic advantages. So when you get to the older ages, the selectivity process is at work.

Later on, Kenneth Manton, another brilliant mind, developed a mathematical-biological model involving heterogeneity of characteristics which supported our position. Ken's become the leading advocate of the crossover as real. Ansley Coale is still sticking to the guns of the crossover as not real, and there are lots of articles coming out these days arguing on either side. I'd like to see a session where all the people who are writing about it could just bang it out.

VDT: What a brilliant idea! Are you going to suggest that to Harriet Presser for next year?

NAM: Yes, I think I might.

VDT: If we could get some sessions where people are expected to talk.

NAM: Not all sessions like that, but if there was one every day like that, we could draw a tremendous

crowd. Maybe we want to make it a night session so it doesn't compete with other sessions. When you look at the program, I think we've lost some of that element of real talking about issues. Everybody presents their own research, there's a little bit of formal or informal discussion about it, it kind of nips around the edges. No real criticism is going on in the field, except through publications. I'd like to see more of it.

VDT: That's probably just a function of the numbers and size. With the Psychosocial Workshop, which I always attend, the 16th this year . . .

NAM: You get some of that.

VDT: We do, because we all attend the same session. We're all on the same level; nobody's sitting up on a platform.

NAM: There are some areas where that doesn't get done. The Psychosocial Workshop is a bit special in that respect. We're developing new special interest groups and maybe we'll see more of that. I'd like to have it not in the interest group context but in the program context so that people who are not even particularly interested in that area could hear a good debate.

VDT: A bit like the luncheon roundtables, but not all talking at once.

NAM: That's right.

VDT: And they also should be in smaller rooms.

NAM: And no presentation of papers. Somebody who's a moderator who presents the issue and the basic ideas on both sides and then let them go at it.

VDT: You're quite right that the panels have also degenerated into reading of papers. Well, I think that's a great idea.

What about Demography? You became editor in 1972-75, four years after Demography had had its sea change, after Donald Bogue was eased out of the editorship because he'd gotten too family planning oriented.

NAM: That special issue ["Progress and Problems of Fertility Control Around the World," second issue of Demography, 1968]. But, of course, we owe a debt of gratitude to Don for getting the journal started. Norm Ryder was the only other person who ever believed in a demography journal. But Don was the one who really went out and did it; have to give him credit for it. Then Beverly Duncan was editor [1968-71] and there was a short-term editorial stint.

This was a kind of calculated risk on my part, getting involved with the journal, because here I was at Florida State trying to build up the program. One good thing, I mentioned that about 1970 there was a turning point, we were beginning to build up a force: some of the people who are still around now, like Bob Weller and David Sly, and I had hired Tom Espenshade and he was there at the time. I said, "Okay, now what we need in addition to our grants, fellowships, and so forth is something to get better established in the profession." And Norm Ryder, I never knew why, suggested me as an editor.

VDT: He was president in 1972-73.

NAM: Okay, he was president and we needed a new editor for Demography. Basil Zimmer had done it on an interim basis; the promise was that he would only do it for one year. So Norm asked if I'd be interested. My first reaction was, "We don't want to do that. That's too big a job." I talked to my colleagues and we said we'd have to sacrifice a lot to do it. We'd get some money from the Association, but the university would have to kick in something. Talked to the university people, the dean and so forth, and they said if you want to do it, we'll give you some help, time off, not much but a little, and an assistantship or something. So we decided that would be one way our center would get known better.

VDT: Did you regret it? It must have been a lot of work.

NAM: No, that was one of the best things we ever did. We all think it was a good move for us, because it's one way that our center really did get its reputation. I think we did a decent job, editorially. That was the first time that Florida State became known as a place where population was going on. Also there were certain innovations we introduced, some of which continue.

VDT: Name one of your innovations.

NAM: One thing that's been dropped, which I think is unfortunate, something close to what I suggested for the program. That is, in some issues we took an issue and had people write on both sides of it. You go back to some of the issues during my editorship. There were Judith Blake and Larry Bumpass arguing about desired family size ["Comments on J. Blake's 'Can We Believe Recent Data on Birth Expectations in the United States' (Demography, February 1974), by Larry Bumpass and David L. Kruegel, Demography, February 1975]. Sometimes it was sort of a book review forum. Sometimes it was just done in terms of an article that one of them was publishing and you had others commenting on it. It's the same kind of exchange that was published . . .

VDT: In Population and Development Review.

NAM: Yes. So we had some innovations, but more important, I think, we kept Demography going. And for us it was an education; we got to see what everybody was doing in the way of research and writing in the field. So it was good for us in terms of our own learning experience and also our visibility in the field.

VDT: Did you attend the session yesterday with Jay Teachman's content analysis of Demography [Teachman and Kathleen Paasch, "Twenty-five Years of Demography: A Content Analysis," in Kingsley Davis session on "Two Centuries after Malthus: The History of Demography"]?

NAM: I missed that.

VDT: He did a content analysis of the 25 years of Demography, or a graduate student took nine months to do it. It's very interesting. Well, it was a bit distorted because the treatment of family planning, of course, had gone down, because it started off with the Donald Bogue years. He had some rather revealing things to say. Demography seems to have changed in some respects and remains unchanged in others. Not very many women authors out of the total, still now. One explanation is that women are perhaps writing on topics like fertility and family planning that can better be published elsewhere.

NAM: We were very conscious about articles by women. That was getting to be an issue at that time. I think we had a better record also in the meetings, the percentage of sessions organized and chaired by women. It was much higher the first year that I was program chairman than it had been before. [Charles Nam chaired the program committee for the 1976 meeting as well as the committee for the 1979 meeting when he was president. Responsibility for the meeting program switched from the first vice-president to the president with the presidential tenure of Sidney Goldstein in 1975-76. Since Goldstein, as first vice-president, had organized the program for the 1974 meeting just two years before, he asked Nam to chair the 1976 program in his place.]

VDT: You made a conscious effort?

NAM: Oh, absolutely. PAA has a tradition almost back to the beginning of women being in central roles. The three awards are all dedicated to women [Irene Taeuber, Dorothy Thomas, Mindel Sheps, with Robert Lapham Award added in 1989]. And the presidents we had. In how many associations can you go back that far and find people like Irene Taeuber, Margaret Hagood, Dorothy Thomas?

VDT: True, there were three women presidents almost in a row. Irene Taeuber was first [1953-54] and she was president Number 17, followed by Margaret Hagood [1954-55], and then a three-year gap to Dorothy Thomas [1958-59]. Then there was no woman president till Evelyn Kitagawa in 1977, 18 years.

NAM: That was a cohort effect.

VDT: What do you mean by that?

NAM: In the early group of PAA people, there were a fair number of women compared to most social science organizations, and they were established people and moved right into officer slots. But somehow women didn't get recruited into the graduate training programs for a long while.

VDT: Were they all home raising the baby boom?

NAM: I don't know what the explanation is. But I don't see that any time in our history, even when there weren't any women presidents, that we ever discriminated against women to the extent that--I'm sure there was some--that other social science associations did. If you look at, say, the American Sociological Association, their first woman president wasn't until the modern era. And I think it would be even more true for the American Economic Association, Political Science Association. So we were ahead of other associations.

VDT: Of course, it was Margaret Sanger, with Henry Pratt Fairchild, who called together the first meeting [May 7, 1931].

NAM: Absolutely. Maybe it's the subject matter that gave more opportunity for women.

VDT: But there haven't been many. Harriet Presser will be only be female president Number 7, and that will be in nearly 60 years. Evelyn Kitagawa was 1977, then there's a jump to 1981, Judith Blake, who was Number 5, Jane Menken in 1985 was Number 6, and Harriet coming up next year, 1989, Number 7.

NAM: If you look at the Board of Directors as well, you can see that we tend to have something closer to a 50/50 split in that, which very few associations have.

VDT: At our Women's Caucus meeting yesterday, I think it was a man who said that whenever a women is a candidate, she always gets in. There was a notorious year a few years back when there were no women among the candidates for the Board. We rose up in wrath and fortunately there was a ruling by which members could present candidates--the nominating committee didn't have the last word--and proposed four women and I think they all got in--maybe three.

NAM: There's one person who never believed in having a movement to get women involved.

VDT: Who's that?

NAM: Dudley Duncan's wife Beverly, who died just a short while ago. Beverly was a very active person, as Dudley was, in the Population Association and the year a Women's Caucus was set up in PAA, she resigned her membership. I remember the occasion. There was a big debate in the membership meeting, called the business meeting at the time. The Women's Caucus was mainly women graduate students. It was a period of activism. [The Women's Caucus was formed at the 1970 meeting in Atlanta. The "big debate" that Nam recalls was probably that which occurred at the business meeting during the 1971 meeting in Washington when three resolutions proposed by the Women's Caucus having to do with discrimination against women were discussed and the first resolution, seeking to eliminate "underrepresentation" of women "in graduate programs and professional fields related to population fields" was adopted by vote of the members after adding discrimination on the basis of race as well as of sex. See Harriet Presser on "The Women's Caucus of PAA," Vignettes of PAA History, PAA Affairs, Winter 1981, and Harriet Presser interview below.]

VDT: Joan Lingner and Harriet Presser were among the early members.

NAM: Before Joan Lingner and Harriet Presser, there was a group of students whom you might call radical.

VDT: Concerned Demographers?

NAM: Concerned Demographers had its start and so forth. Beverly thought that was wrong, because their agenda was not a scientific one. She was a purist. She thought that one thing that was great about the Population Association was that we were scientists; we weren't do-gooders and we weren't activists, like some associations were, and we should not get caught up in that. So she objected strongly. I remember her storming out of that business meeting. I went out later on and talked to her and she expressed all these feelings, and she never became active again in the Association. She was a very good demographer. Dudley remained active for a while until they went out to Arizona [1973]. But she was at least one woman--I don't know how many others there were--who didn't like the idea of the activist aspect of women trying to present themselves in the Association.

VDT: Now, at the Women's Caucus meetings, like yesterday, we mainly meet and talk about, "Why are we meeting?"

NAM: I think in all fairness, of course, there's been discrimination about women to some extent in PAA, as in other organizations. But I think PAA's record is quite good compared to most

organizations. If you look at the relative numbers of men and women in the Association relative to the number of offices filled by women, I think we have a much higher percentage than most organizations.

But we have one of the worst records of any association on blacks, and part of it is because of the nature of the subject matter and the fact that we're a very quantitative social science.

VDT: Tell me what you mean by that, because I've heard a lot of people talk about the lack of blacks in demography.

NAM: Well, there was a period when we did recruit a number of blacks to graduate schools.

VDT: Chicago took in a number of blacks [in sociology].

NAM: Yes, we had some black students. But it's just very difficult. I think there's more of a tendency for blacks who got interested in social science to go into areas that were more applied and certainly more qualitative than quantitative. You don't find many black statisticians. I don't know about mathematics, probably the same.

VDT: But you said "more applied."

NAM: I think where they could deal with the social agendas, like poverty and unemployment, but do it more from the policy point of view.

VDT: Julius Wilson?

NAM: Yes, he's a sociologist from Chicago. There aren't many people like that around; in sociology there's more. But how many of them in sociology do you find with quantitative orientations? Practically none. You might be hard-pressed to find one. The people who write about issues of concern to blacks, whether it's crime or poverty, things like Wilson and Blackwell and others do. There wasn't much of a quantitative tradition in the black community, I guess. So it's been very hard to recruit blacks to demography, particularly as demography got more and more quantitative, more and more methods-oriented.

VDT: That's U.S. blacks, of course. Now we have more from the Third World.

NAM: Oh, yes. We have a number of African students in our program. I'm sure every population center has done this at one time or another, but we used to have fellowships ticketed for black students and the most lucrative ones we couldn't interest anybody in. So I don't know what kinds of incentives and motivations we can use to bring more U.S. blacks into the Association. I think we need to do it because the people who study race issues, like Ren Farley, are all white, so you don't get any of the black perspective on issues of demography that are relevant to blacks.

VDT: There was one who came and spoke today at the session on whether or not the 1990 census should be adjusted for undercount. That was Robert Hill, who's not a demographer.

NAM: He's not a demographer. He's been on the Census Advisory Committee. Interested in population because of black interest groups.

VDT: That has been an issue through the years. They talk about the few times there were blacks and

PAA met in the South and there were problems with having blacks stay in the same hotel as white members. It happened rarely, because there simply weren't any black members.

NAM: Black college enrollments are going down these days more than white enrollments are, so the pool of eligibles is shrinking even more. There are more opportunities for blacks in other areas and the road to a career in demography is a lot slower and less lucrative than going into business or going into areas where blacks are being very well received these days.

VDT: You've given me a lot of wonderful time. Have you any final thoughts about your career?

NAM: Just that it's kind of like a dream. I never had any great ambitions, even when I started off in demography. I never thought I'd accede to the presidency, remotest thing from my mind. In fact, I remember the day I had a telephone call from Charlie Westoff, who was chair of the nominating committee, asking me if I'd run for president-elect, or maybe it was first vice-president, whatever it was it was an automatic succession. I remember saying to him, "Oh, no. I'm not ready for that yet." And he said, "The committee thinks you are." I said, "I just haven't achieved that much yet relative to the people who've been in that. I really don't think I want to do it now." And he insisted. Of course, it was just running, being nominated. I wasn't being picked for president. And I figured, "Well, okay. I'm going to get beaten easily anyway, so I'll be a stalking horse for somebody else." And I won!

VDT: That's right. You had a perfect line of progression, which doesn't often happen. You were on the committee on publications, 1968-71; then became editor of Demography, 1972-75, second vice-president in 1976, first vice-president 1977, president-elect 1978, president 1979.

NAM: I'm expressing some false humility here, but I really felt that the people who preceded me as president were much more accomplished than I. I still feel that way today. So I'm just tickled to death with the kind of career I've had so far. I'm not ready for it to end. I think the best years are ahead of me.

VDT: That's a marvelous note to end on: The best years lie ahead. Now I think we better go to the Alumni Party.

CONTINUED

VDT: We're talking about Dudley Duncan. I now understand he's been at the University of California, Santa Barbara, and that Beverly Duncan died a few months ago. Charlie was just saying that he was a very private person, at meetings. Even when he was PAA president [1968-69], he associated more with the students when he came to PAA meetings?

NAM: That was certainly my impression. He was well known and good friends with other people in the Association, but I think he tended to stay to himself or with his students and not interact with other people in the profession.

VDT: His wife and he always worked together? She was a demographer too.

NAM: She was originally a student of his, I believe, at Chicago [research collaborator at Chicago; student earlier at Penn State]. He was a young professor there. Her name was Beverly Davis, the name on the first article she published. I didn't know them very well, but I know it was a very strong relationship between the two of them. When they came to meetings, they always stayed together and

really enjoyed each other's presence.

[Discussion of Charles Nam's innovative invitation, when he was PAA president in 1979, to all living past presidents to attend the presidential address session, resulting in a famous gathering on the platform at that meeting in Philadelphia.]

VDT: There were all these famous names like Frank Notestein and so on.

NAM: It seemed to me that all the living past presidents were people that the younger demographers didn't know, had never seen; they were just names they'd read in books and articles. And I was always kind of humbled by their presence and I said, "If I'm going to be president, I'm going to have all of them come up and sit on the stage." So I wrote letters several months in advance to all the living past presidents, "If you're coming, prepare to come up on the stage at the presidential address session. If you're not planning to come, think about coming."

There were several of them who came particularly because of my invitation. Not all of them came. As you remember, Andy Lunde was the master of ceremonies and we had a big semi-circle of chairs which had a tremendous number of past presidents. That was the most exciting thing, much more exciting than my presidential address. Andy went around introducing all of them. I was just thrilled.

VDT: But we didn't get a picture!

NAM: You might want to put a note sometime in PAA Affairs--you've done things like this before--asking if anybody has a photograph they took. It's possible there is one.

VDT: Andy and I have asked for photographs through PAA Affairs and haven't got much response.

NAM: It was a tremendous group. There were a few people who weren't able to make it; Joe Spengler didn't make it.

VDT: Somebody made it the night before and wasn't there at the actual time. I think Phil Hauser.

NAM: Phil came in right after it. Don Bogue, I think, had left the night before. Who was the general from World War II, past president? Frederick Osborn.

VDT: He was a general! I know he was such a gentleman.

NAM: He was a general in charge . . . some relation to Los Alamos, atomic energy stuff [Major General in charge of the Army's Special Services Division; after World War II, U.S. Deputy Representative to the UN Atomic Energy Commission]. He was still alive at the time of that Philadelphia meeting, living in upstate New York; he'd long since retired. But his health was not good enough to get him to the meeting. There would have been a fascinating guy to have interviewed. If you look back at his life, it was just tremendous.

VDT: That was a memorable occasion.

NAM: Nobody's done it since, and I don't feel like I'm one to say that.

VDT: I think Reynolds Farley tried a bit this time. For instance, he specifically asked Richard Easterlin, Amos Hawley, and Judith Blake, who hasn't been at the last few meetings, to come and lead roundtable discussions. It probably didn't occur to him to have them on display.

NAM: I think myself I'd be embarrassed. But I think sometime it would be good. Next year it will be the ten-year mark since my presidency. It would be nice to invite all the living past presidents, because it gives some continuity as well as opportunity for the younger people.

VDT: Why don't you suggest it to Harriet? [JvdT did, but Harriet Presser felt the 1989 presidential address session was to be too full, with the addition of the new Robert Lapham award to be presented in that session.]

Andy Lunde thought of having an annual photo made of the Board of Directors, and I was the one who got them started putting it in PAA Affairs.

NAM: That's been done for a long time. In my office at Tallahassee, I have all of them when I was on the Board. That's quite a few, because I was on the Board before I was an officer.

VDT: They've been taken every year since 1967, but they weren't publicized in PAA Affairs. The first one in Affairs [1985 Board] ran as one of my "vignettes of PAA history." Now they regularly go in so the members get to know what people look like.

NAM: The first one I got, all the men are wearing black suits. When you get to some of the later ones, people are coming in Hawaiian shirts.

VDT: Why is it that the business [membership] meetings are not better attended, just a sprinkling of people?

NAM: It wasn't that way in the early years.

VDT: Everybody came to business meetings too? They cared about the organization also?

NAM: Sure. You were planning the development of the Association. I think part of the reason is that PAA is in such good hands. They have confidence that it's going to run fine. I think you find more participation in those associations where people are in disagreement and they want to fight about issues. PAA runs so well that who has to go to the business meetings?

VDT: That's another good ending. We'd better go to that party.

JACOB S. SIEGEL

PAA President in 1980 (No. 43). Interview with Jean van der Tak at the Department of Demography, Georgetown University, Washington, D.C., June 21, 1988.

CAREER HIGHLIGHTS: Jacob Siegel was born and brought up in Philadelphia. He received the B.A. and M.A. in sociology from the University of Pennsylvania. He was with Census Bureau for almost 40 years, from 1943 to 1982, moving up to the position of Senior Statistician for Demographic Research and Analysis. In 1982 he became Professorial Lecturer and Senior Research Associate at the Department of Demography of Georgetown University, where he had already been teaching a famous Saturday morning class on demographic techniques. He has also taught demographic techniques and population statistics in the Graduate School of the U.S. Department of Agriculture (1948 to 1968), in Cuba, at CELADE in Santiago, Chile, at the University of Southern California, the University of Connecticut, Cornell, Howard University, and other institutions. He received the Gold Medal from the Department of Commerce and is a Fellow of the American Statistical Association and of the Gerontological Society. He is particularly well known for his research and writing on population estimates and projections, the census undercount, aging, and, most recently, applied demography. His long list of publications includes the famous two-volume monograph, The Methods and Materials of Demography (with Henry Shryock and Associates, 1971), Projections of the Population of the United States by Age, Sex and Color to 1990 (1967), and a 1980 census monograph on the U.S. elderly population (forthcoming).

VDT: We are speaking on a very hot first day of summer. It's nice and cool in here, but the airconditioning in Jay's car isn't working and he's arrived in a sweat. He recently returned from a semester as visiting professor at Cornell. I am lucky to capture him, because he was very busy before he left for Cornell and now he's busy again, plunged into his summer teaching at Georgetown.

To begin at the beginning, Jay, how did you first become interested in demography?

SIEGEL: In my early academic training, I had no particular interest in demography. I found it quite difficult to select an area of professional and occupational interest, and having spent more time in courses in foreign languages, I early decided to become a specialist in Indo-European linguistics. I pursued courses even as a high school student and as freshman, sophomore, and even junior in college with the intention of becoming a specialist in Indo-European linguistics. Through those years, I got to have a reading fluency in French, Latin, and Greek, and even studied some Arabic and Hebrew.

VDT: You're famous for your Spanish. Did that come later?

SIEGEL: My Spanish is wholly self-taught, learned as a reaction to the fact that none of the languages which I tried to learn had become fully operative in a conversational context. So I started anew, learning Spanish almost wholly for conversational purposes, and then grabbed every opportunity I could to use it, even imposing my broken Spanish on the many foreign participants in the Census Bureau international training program.

VDT: It's on record that you've been to CELADE and to Cuba, and you taught in Spanish. So you also used it with demographers from Latin America when they came to the Census Bureau?

SIEGEL: At that time, I was merely meeting with them at lunch to get to know them better, but also

as a practical thing to try out my Spanish. That didn't work very well, not only because my Spanish was very defective but also they had no interest in practicing Spanish; they wanted to practice English.

Going back to your earlier question. As I came near my senior year, I had to grab for a formal major, and I began to realize that there was very little opportunity to get employment in the area of Latin and Greek, because I sat in a few classes in Latin and Greek where I was the only student or one of two.

VDT: At the University of Pennsylvania?

SIEGEL: Yes. This may be embarrassing to the university, but it's probably common. Professors want on record the fact that certain courses are taught and, even though few students may be registered, they continue to sponsor the class.

So, looking back at the record of my courses and noting that I had taken a lot of philosophy and sociology, I realized that I could register myself as a major in sociology and graduate on time--which is what I did.

Then, not knowing what to do with myself in the following year, I applied for an endowed fellowship in sociology to the graduate school, which I did win, and a principal course which I took was in population, taught by Donald Young, who later became executive director of the Social Science Research Council. He didn't tell me anything much about the methodology of population research but a lot about world and U.S. trends in population.

Then I had the task of finding a job and I saw or heard of an announcement by the Census Bureau. They were employing people to finish off the 1940 census. And so, with scarcely a few hours' notice to my boss at my Sunday school where I was teaching, I announced that I couldn't be present the following week because I had gotten a job in Washington. So in December of 1942, I arrived in Washington to become a clerk in the Business Division of the Census Bureau. Not knowing anything about administrative protocol, I insisted on having an interview with the chief of the personnel division at the Bureau, who happily granted me that interview. She told me that they had no vacancies so far as she knew in the area in which I was trained, but that if I wanted to discuss the matter with one of the staff I could do that, and she mentioned a Henry Shryock. I went to Henry Shryock and he offered me a job, virtually on the spot.

VDT: And you didn't part company for many years after that!

SIEGEL: I early, then, met that little coterie of demographers, consisting of Henry Sheldon, Paul Glick, John Durand, and Henry Shryock.

VDT: And Con Taeuber?

SIEGEL: No, Con Taeuber was not at the Census Bureau at that time.

VDT: That was what has been referred to as the "Class of 1940"?

SIEGEL: Not at all. Not one of them is in the Class of 1940. That's a different concept. The Class of 1940 is that group of recent graduates of the City College of New York or other New York colleges who in the year 1939, or maybe 1940, graduated and came virtually en masse to the Census Bureau to work in various junior professional and sometimes clerical positions. They included people like David Kaplan, Joe Steinberg, Norman Lawrence, maybe Joe Waksberg, Ed Goldfield. You cannot be a member of the Class of 1940 without having originated from New York and without having just

emerged from college and arrived at the Census Bureau about 1940.

VDT: With a Ph.D., or a lower degree?

SIEGEL: No, none of these people had PhDs. It was characteristic at that time for people with only bachelor's or master's degrees to come to the Census Bureau or another federal agency, partly because there were jobs available but also because then, as now, the Ph.D. was a rigid requirement for full acceptance into the fraternity/sorority, shall I say, of academics. I haven't given all of the names in the two groups that I've mentioned. I could add to the first group people whom I met shortly after, like Hope Eldridge.

VDT: Did that first group, the little coterie of demographers, have some informal or formal name?

SIEGEL: I don't know. But note the characteristics of the two groups. Every member of the first group had a Ph.D. in sociology or economics: Sheldon, Glick, Shryock, Durand, Eldridge. Three of them came from the same university and were friends originally: Glick, Sheldon, and Shryock--all Ph.D.s from Wisconsin. The second characteristic is that they typically obtained their PhDs somewhat earlier and arrived earlier at the Census Bureau, and hence typically were in positions of authority over the Class of 1940-type people. Not always, because sometimes the subject matters did not coincide. Steinberg and Waksberg and some others like them were mathematical statisticians and actually had degrees in statistics or mathematics. Perhaps that was true of all of the members of the Class of 1940.

It should be clear, then, that I am neither a member of the first group nor of the second. I'm a johnny-come-lately to both; I don't have credentials for either. In many ways, you'll find that my history is anomalous, especially my tremendous involvement with academic life without having had a doctorate.

VDT: Where did Con Taeuber fit in?

SIEGEL: I met Irene Taeuber first, although I knew of their work even before I arrived at the Census Bureau. I must say that population work and study fascinated me even before I came here. As an illustration, I recall that in trying to learn to read German, I began with a textbook in population. That was my first book in German--first thing before I hit the grammar--called Bevolkerungsentwicklung, Population Development. And so I was able to . . .

VDT: Plunge in.

SIEGEL: Yes. There's a great similarity, at least phonetically, between German and one of my familial languages, which was Yiddish.

VDT: You knew Yiddish as a boy?

SIEGEL: I could understand a good bit of Yiddish, though I never really spoke it. My parents spoke English, Yiddish, and Russian.

VDT: Were your parents born in America?

SIEGEL: No, they were born in Riga, Latvia. They were married here.

VDT: Where were you born?

SIEGEL: I was born in Philadelphia.

VDT: Of immigrants, that's marvelous! We were talking of your anomalous position between these two groups and you were telling me where the Taeubers fit.

SIEGEL: And I was saying that I was familiar with the work of the Taeubers. One of the leading "texts," in quotes--I say quotes, because I'm not sure whether it ever was intended for that use; I had it assigned to me--was a National Resources Committee publication called something like, "America's Changing Population" [National Resources Committee, Committee on Population Problems, The Problems of a Changing Population, 1938]. I got to know the names of the technical credits in that report: Frank Lorimer, Clyde Kiser, Conrad Taeuber, and so forth. So these were people whom I early had as sort of intellectual heroes, whom I had never met. I early met Irene Taeuber. In a way of speaking, she was one of my bosses, because as a consultant to the then Department of War, she was asked to prepare population projections for the prefectures of Japan. And the people who were on the production line of that job were Hope Eldridge and Jay Siegel at the Census Bureau.

VDT: That means you were in on Irene Taeuber's famous The Population of Japan [1958]?

SIEGEL: No, I didn't work on that book in any sense, but on a job being prepared as a demographic contribution to the war effort. That was one of my principal assignments, to assist in the work under her direction. She was not a Census employee at the time, although there was a period [1941-44] during which she actually was assigned to a project called the Census Library Project, where she did a bibliographic job on the censuses of the Americas, working at the Library of Congress.

VDT: She worked at the Library of Congress in part on her work on Population Index.

SIEGEL: I have the book here. It was a bibliographic project relating to the censuses of the Americas and I believe in doing that she was a Census Bureau employee assigned to the Library of Congress. So I met Irene fairly early.

VDT: Was that the first job you did when you arrived in 1943--doing the projections for Japan?

SIEGEL: One of the early jobs. I worked for Wilson Grabill at first, who was an excellent mathematician, from whom I developed a sort of early working knowledge of formal demography. Here he was, quite unlike many of the other Census employees, developing these, what might be called, refined measure of fertility in connection with doing census work. He produced a thin little report which stands today as one of the remarkable analytic reports of the Census Bureau, relating to the censuses of 1940 and 1910, an analysis of fertility changes over that 30-year period, based on the census question on children ever born in 1910 and 1940. He computed measures like intrinsic rates of natural increase, intrinsic birth rates and death rates--all of these things which I had to hit the books about, read Lotka and Kuczynski, to catch up on what it was all about.

He and I spent many hours in the lunch line and in his office communicating with one another, he to me by writing on sheets of paper or the backs of envelopes or napkins, and I to him by writing or signaling in sign language, which I learned--only haltingly, but enough to communicate with him, without a pencil and paper.

VDT: You mean Wilson Grabill was deaf?

SIEGEL: Yes.

VDT: I'd heard that you'd learned sign language to communicate with a deaf person at the Census Bureau, but I never realized that was the Wilson Grabill!

SIEGEL: Wilson, like some other very able deaf people who work to overcome their handicap, learned to speak a bit. He spoke in a kind of irregularly sounding speech which could not easily be heard in a crowd because it was high-pitched and thin.

VDT: Had he been deaf from birth?

SIEGEL: As a child he had some infectious illness that destroyed his hearing. He could speak a bit, but he could read lips very little; that was not something he tried to do. So our way of communicating was as I explained. We'd stand in the lunch line and, if he didn't have any paper, he'd pick up a napkin, although he often carried little cards or notepaper, and he'd write something to me. I could not receive the sign language, but I could signal back to him. I could make the alphabet signs quickly enough, so we could communicate. And we were talking about problems like adjusting the 1910 census for underenumeration; we were talking about what the next steps would be in our calculations. In those days, everything was done by hand, in the sense that it had to be laid out on worksheets by hand, and we worked with a crew, a section of statistical clerks who had a supervisor. So typically he would give me some general instructions; I would draw up a worksheet and then deliver it to the supervisor of the clerical section, who in the earliest days supervised a group of people, some of whom had Comptometers.

VDT: Is that a calculator? That's not a word I know.

SIEGEL: Yes. I'll give you the historical succession of calculating instruments. There's the abacus, still used by some. Going beyond that, in college, for example, when I wanted to do calculations for my statistics class, I pulled out a slide rule, which, as you may know, is an instrument which contains a logarithmically scaled section which slides in a groove in the bed of the logarithmically scaled rule. The Comptometer you might think of as an elaborate manual tool. It's a mechanical device, non-electrical; in other words, you had to grind it so many times to do a multiplication, and you were a real expert if you could do division on a Comptometer. Then came the earliest Marchands; that was the earliest company, I think, that produced electric calculators--noisy, slow.

VDT: I've heard about those; in a roomful of them you practically lost your hearing.

SIEGEL: Yes, when the Marchands were going, it was a noisy affair. Of course, the noise continued until very recently, actually, because the more elaborate calculating machines continued to rattle away; they just moved faster and a little quieter. With each new version, they could do the calculations more quickly and more quietly. But still, a roomful of Monroes or Marchands was noisy.

VDT: How did you get so interested in statistics? You came out of languages and, technically, you were a sociology major.

SIEGEL: Well, lots of sociology majors are quantitatively oriented.

VDT: Were you--as a sociology major?

SIEGEL: I was a sociology major, but the sociology department was physically housed in the Wharton School of Business at the University of Pennsylvania. I don't recall whether statistics was required, but I felt I had to learn it to do any kind of solid thinking in sociology. Except for the graduate year in sociology and miscellaneous courses, I don't have intensive training in any of these fields in a formal sense.

VDT: Yet you became famous for them--techniques, projections, undercounts, and so on.

SIEGEL: I never took courses in advanced statistics, that is, beyond the first year. I should qualify that; I'm overstating the case. Immediately when I arrived here, I thought I ought to get an education relating to the work I was doing. So I enrolled in a course in correlation analysis with Margaret Hagood, also in courses on the mathematical basis of statistics and on interpolation.

VDT: She was at the Department of Agriculture.

SIEGEL: Right. But I had already met Dr. Hagood, because she was a close friend of Hope Eldridge. And in a way of speaking, though she didn't come to the Census Bureau, she was a member of that first group--a recent arrival, a Ph.D. in sociology, and a friend of Eldridge. You see, you would relate Eldridge and Hagood and--now I'm embarrassed that I've forgotten the name of Hagood's principal assistant who later became president of the Social Science Research Council. Oh, yes: Dr. Eleanor Sheldon. You would associate those three and then the other three names that I gave you: Henry Sheldon, Shryock, and Glick, who were friends. Three from the South and three from the North.

VDT: Hope Eldridge was a Southerner too?

SIEGEL: Sure, from Alabama, and Margaret Hagood from North Carolina. I'm not sure I can separate the place of schooling from the place of upbringing, but these were Southerners. They all became good friends. In fact, after a bit, a sociological discussion group was organized in Washington, with the Wisconsonites and Marny Hagood as founding members.

VDT: The D.C. Sociological Society?

SIEGEL: No, it was a kind of para-D.C. Sociological Society, meeting in private homes. And in this group were Abe Jaffe, Dudley Kirk, Cal Beale, Margaret Hagood, Henry Shryock, Paul Glick, Henry Sheldon, and others. It began in the late 1940s and went into the early 1950s.

VDT: You met in each other's homes, regularly?

SIEGEL: Yes, once a month.

VDT: Did you call yourselves something?

SIEGEL: The Sociological Discussion Group.

VDT: Did you have set themes, or you just met and let the conversation flow?

SIEGEL: No, there was a theme for each meeting, selected on the basis of the interest of the next chairperson of the meeting, who may have been host also of that meeting. Usually the discussion revolved around some book in sociology which the group leader selected on the basis of his or her interest. So the discussion was not confined to, but usually focused on, the particular topic.

VDT: You said you took courses from Margaret Hagood.

SIEGEL: Yes, I took a course in correlation analysis with her, so I got to know her further that way.

VDT: Was that at the Department of Agriculture?

SIEGEL: Yes. The Department of Agriculture in a way of speaking runs the government general college. It's called the Graduate School.

VDT: It was in existence already then?

SIEGEL: Oh, yes. I took this course back in the early 1940s; it was in existence. The classes were held either in the Department of Agriculture or in nearby federal buildings. It wasn't long after that that I became a faculty member; that's where I began teaching. In 1948 I became a faculty member of the Graduate School and taught there for about 20 years.

VDT: So that's where your teaching career began; that's a very important feature of your career.

SIEGEL: I began about 1948, teaching a course in population statistics. This was the beginning of my teaching in that area. Con Taeuber was on the departmental committee; the department of mathematics and statistics, it was called--my department in the Graduate School. Each department had a committee to oversee the selection of courses and faculty.

I owe a lot in my own career to Norman Lawrence, who died many years ago. He was one of my first bosses--a very tough, but very fair, boss. He was in the Population Division. I worked for him for a brief time, if at all, and then he went off to the military, and I was assigned to him when he came back. Hope Eldridge, not too long after the war ended, went off to be one of the pioneering employees of the United Nations, as did John Durand. So, at first I worked for Grabill, then went on to work for Eldridge, and then when Eldridge and John Durand left and Norman Lawrence returned, I began working for him, because he was put in charge of population estimates. Through all of these years, beginning with Eldridge after I left Grabill--the short period of six to eight months with Grabill--I worked on population estimates and projections.

We found very early that we had to develop and create the methodology that we needed to do what we had to do. We often turned to chapters in Hagood's book, the well-known book at the time called Statistics for Sociologists. It was a rare book, because it had several chapters on demography, statistics in demography.

VDT: At least, the first edition. I understand that the second edition . . .

SIEGEL: It was removed from the second, yes. But it was a fat one that I, again, sort of grew up on; I read and re-read that book. In addition, about the same time I enrolled in a course with a demographer whose name I knew, Frank Lorimer, at American University--a course called "population methods," or whatever. This was before I started teaching in 1948. This was quite an experience for me. As a teacher, Frank was not always very effective.

VDT: Not effective?

SIEGEL: Haven't you heard this?

VDT: No, tell me more. I know he was quite a character.

SIEGEL: He was a tremendous scholar, I must say. He pioneered work in a number of areas in his writings. He did one of the first studies on the demography of the Soviet Union [The Population of the Soviet Union: History and Prospects, 1946]. He wrote a book that explored cultural factors in fertility [Culture and Human Fertility, with others, 1954]. But in the classroom presentation of algebraic materials, he often got kind of mixed up; I'm not sure how to say it. And he had his peculiar ways of speaking. I remember his habit of starting sentences and never finishing them. Frank had a way that could drive you absolutely crazy; you weren't quite sure what he was saying. His writing was beautifully done, but in his speech, if you recorded it, you wondered, is any sentence here ever finished?

VDT: In other words, it was difficult to make notes from him?

SIEGEL: Extremely difficult.

VDT: Was that just a one-semester course?

SIEGEL: Yes. I was flattered because, when he came around to talking about population estimates, he asked me to talk to the class. He said, "Young fellow, you work in this area, you must know something about it. Why don't you talk to the class?" So I found myself getting some notes together, back there in the mid-1940s--could have been 1944, 1945--explaining linear extrapolation and curvilinear interpolation and component methods of estimation and stuff like that. So my experience in the teaching area grew as I grabbed the opportunity that I had. There was another course that Frank taught relating to population trends, problems, and policy, and he called on me to come in and pinch-hit sometimes.

VDT: That's, of course, a tradition at American University in general. One reason it was established in Washington was to draw upon the expertise in the federal government, and there they were doing it.

SIEGEL: I later taught such a course in the Graduate School at the Department of Agriculture. At one point, I thought I wasn't covering enough in one semester and thought I should extend it to a two-semester course. I asked Henry Shryock whether he would like to join me over the year, so we alternated in teaching the classes of the same course. That was a course in population statistics.

As you see, I got into teaching rather early, in the late 1940s, and more or less continued teaching at the Graduate School to 1968. There were years or terms when I didn't teach, either because there weren't enough students or I was away. I followed the policy--I'm not sure if it was a conscious personal policy or just seemed to be--of leaving the Bureau periodically to work somewhere else, while keeping my home base. I did that with the blessing of the Census Bureau, as when I went off to CELADE, the Latin American Demographic Research and Training Center of the United Nations, even though it was clear that I was an active agent in arranging the leave of absence, because I applied for the job. I had to apply for this job of teaching and research adviser at CELADE.

VDT: When did you go to CELADE?

SIEGEL: 1962--for a little more than a year.

VDT: The Census Bureau gave you leave of absence?

SIEGEL: At that time, once you had been selected by the United Nations, they would petition the State Department for your release, and it was either a matter of tradition or regulation that the department you worked for was expected to release you for international assignments. So on the surface it all seemed to be agreeable, but in fact when I got back, I had some problem because of a technical detail, that is, I didn't return exactly as approved in 365 days. There was a one-day or so difference in the two periods and that had some impact on my accumulation of months of service in the federal government. It took several months before this administrative legal quarrel was finally straightened out.

VDT: You said you made this a regular practice, to take leave from the Census Bureau.

SIEGEL: It looks this way in retrospect. When the Census Bureau received a request in the late 1960s for someone to teach a summer course in population statistics at the University of Southern California, I offered to go. So, I'm saying, in retrospect you can see a history of someone who, while maintaining a base, went off from time to time. Now, some of these excursions were sponsored by the Census Bureau. Some of the early trips were very short. The trip to Cuba in 1959 . . .

VDT: The very year of the revolution!

SIEGEL: Exactly. I was the first new foreign consultant to Cuba under Castro. It came about this way. Our man in Cuba was Ben Gura.

VDT: What do you mean by "our man."

SIEGEL: The Census Bureau then, as now, had statistical consultants around the world in its training and consultation program. Ben Gura had spent many years in Latin America and his post was in Cuba, under Batista at the time. I knew Ben quite well; he came into Washington from time to time; quite fluent in Spanish. He was an important and close lieutenant to Cal Dedrick. Dedrick was assistant director for international programs at the Census Bureau, but in the historical archives of America, he may be better known as the man who superintended the statistical operations relating to the internment of the Japanese. That was his wartime assignment.

Gura in his role as field rep for the Census Bureau said they wanted in Cuba someone to give a course to government employees in population statistics. Ben on one of his trips here talked to me about doing it and I said, "Well, you know my Spanish is not very good." I was reading a lot of articles in Estadística and talking to friends in Spanish whenever I could, but . . . Anyway, I got the job.

Now, the contract had been signed with Batista's government in the month of November 1958. In November and December, Castro's forces were marching into Havana. The question was, would any of these previous arrangements be continued? And it turned out that Castro approved my coming.

VDT: Personally?

SIEGEL: I can't tell you how personally, but I arrived there in late January of 1959, the month of

Castro's entry into Havana. Every day there was an intense experience. I learned that this government was going out of its way to make Americans think that we were all friends.

VDT: You mean that the Cuban revolutionaries and Americans were friends?

SIEGEL: Exactly. You may recall that Castro's first cabinet seemed acceptable, reasonable, to the American government. They were not radical. Castro was doing everything he could not to antagonize the United States.

VDT: So then what happened?

SIEGEL: Exactly. After I'd been there for several weeks, into March, a new newspaper was established which was called La Revolucion, and in the newspaper they began calling Castro by a name that seemed awfully foreboding to me: el lider maximo de la revolucion. That translates, Top Dog in the Revolution; "lider" is just an adaptation of an English word. That language was suspiciously like the language of authoritarianism, either Fascism or Communism. There was an announcement that terrorized, or let's say, deeply troubled my landlady, who seemed to be a nice lady--that there would be rent control, that all of her apartment rents would have to go down. The middle class--you could almost sense it--began worrying what was going to happen. There was another event; Castro fired four members of the cabinet, one of whom was the mother of one of my students. She was Secretary of Health, Education and Welfare. She had been appointed by Castro originally but he fired her, for no obvious reason, except later you could infer it was because she was not following the party line.

VDT: You were in Cuba until when?

SIEGEL: I was in Cuba for just a few months, but by the time I left, the signs were clear that the publicly stated position of the government was changing. And by July, they were throwing out the U.S. embassy staff.

VDT: So the friendship was very fleeting?

SIEGEL: It was disguised, I would say, at first and then openly changed when Castro came to this country and spoke to the United Nations. I knew that they were trying to cater to Americans, because one day my family . . . My wife and daughter were with me. My daughter was two years old or so.

VDT: You must have been nervous.

SIEGEL: No, it wasn't like that. You got used to the barbudos--bearded soldiers--going around with their sawed-off machine guns. You recognized the military of Castro and they carried their little machine guns and rifles around with them. Just as you see a policeman here patrolling the streets, you got used to it. But you could see that they were anxious in the early days to cater to Americans because they gave the three of us a private tour of the capital.

VDT: Did your wife and daughter accompany you on these various other times overseas, as at CELADE?

SIEGEL: At CELADE, sure. My daughter was in first grade then. Her name is Lorise. She went to

first grade in Santiago, in a bilingual British-Chilean school, and in the course of the time we were there, she became fluent in Spanish. That was an experience which taught me how invalid are all the arguments supporting official bilinguality in this country.

VDT: Why?

SIEGEL: Because children have no problem in learning a foreign language in the context of its actual use. The burden is on you to prove to me that a five-year-old child can't learn geography and history and whatever else in the course of a few months, i.e., be able to understand the instructions in that subject, in the native language, with total immersion in the native language and some bilingual support.

VDT: So you think that immigrant children should learn English, the language of this country?

SIEGEL: I'm simply saying that I begin with the proposition that the burden is on you--and it's a heavy burden on you, in my mind--to prove that there should be official support of bilinguality after a short period. I didn't say anything opposed to private support. After all, you see how interested I am in learning other languages.

Now, where should we pick up?

VDT: The things that you did while at the Census Bureau.

SIEGEL: I spent most of my years at the Census Bureau in the area of population estimates and projections. Then there were the trips away. We mentioned Cuba, a summer at the University of Southern California to teach population statistics, CELADE. There were many occasions when I went off just for a day or a few days to lecture. I've lectured at many, many American universities.

VDT: They would come to you; by that time you were getting well known?

SIEGEL: They would invite me to come. Even after I came here [to Georgetown, in 1982], I was getting calls to come lecture at one university or another. In 1976 one of my friends at the University of Connecticut in effect arranged for me to come there. I had thought I was going there for a day to lecture about something or other, but when she called she told me that they had a visiting professor's program, she had thrown my name into the pot, and I had won the prize. So could I come for the semester? I said, no, I couldn't do that, I had made no arrangements for that, but if we could work a compromise, I would come for half a semester. So I went off for two and a half to three months in the fall of 1976 or 1977 to the University of Connecticut and taught a course in population statistics and did some other things, that is, I had to lecture to some six or seven departments.

VDT: Was that the longest stretch you had at a university, longer than the one at Southern California?

SIEGEL: Southern California was a summer course; it ran for a month or so. All along, as I said, I was teaching from 1948 to 1968 at the Graduate School. Now, by 1970, Methods and Materials of Demography had been finished; it was published in 1971.

VDT: That enormous work, that you and Henry Shryock had been doing over many years.

SIEGEL: No, not really. In a way yes and in a way no. Margaret Hagood and Henry and I had a contract with John Wiley and Sons in the late 1950s to write a book on techniques of demography, and

the thing foundered. The three of us worked at entirely different rates in rather different ways.

VDT: What's your way?

SIEGEL: Well, it's intense and relative to what Margaret Hagood was doing, it's slow and detailed. Henry probably showed the best balance of timing and detail. Margaret Hagood was writing rather general material that needed a lot of filling in, and my rate of production was not very good. You know we all had full-time jobs.

VDT: You each had a part to do?

SIEGEL: Different chapters. But we were not producing chapters fast enough to feel that we ought to hold John Wiley to this pace, or at least put ourselves in the position of having the editor frequently check up on us. There's a price you pay and that is that they keep calling you and asking, "Where are you now?" So that foundered. But we had these pieces of chapters around.

VDT: Didn't Margaret Hagood die in the late 1950s [1963]? Wasn't that part of the reason the project was never finished?

SIEGEL: Partly, but the project really, as I recall, foundered before she died.

VDT: Why did you take it up again in the late 1960s?

SIEGEL: Right--in 1967 or so. Well, for my part I recall conversations with people at AID who said they were interested in having a textbook for their consultative work and training programs around the world. I was dealing with people who may once have been Census Bureau people. There was Milton Lieberman, who had worked in the international branch at Census for a long time, and Joe Cavanaugh was involved.

Anyway, AID called me and/or Henry and said, "Would you like to do it?" If they called me I probably reminded them of this earlier work and said, "Maybe Henry and I could work on it together." We got official approval to make it a regular project, with a grant of some funds from AID. Henry was free for one year to work on this entirely; 1967 to 1970 were the years we were involved in part or wholly. I had to continue my regular work, but I could find time to work on it. I had some good people, among them, Sylvia Quick. Sylvia is today a high-ranking person in international statistics at the Census Bureau. And, of course, since it was official, in principal I had access to all the people in the Population Division to check or look at things.

But more than that, we had the group of people outside who submitted drafts and who were paid, though modestly. Some of these several associate authors wrote very good material, others wrote material that needed very heavy editing. They were not paid very handsomely. One author wrote a chapter that was just handwritten on long yellow sheets of paper. He was the last one. We kept pressing him and he came through with a pack of yellow sheets of paper and the writing was beautiful. The writing was beautiful then as it is today. Paul Demeny.

VDT: Ah, ha! You know, he's jumped into the word processor now. I interviewed him in New York a couple of weeks ago and he's got a word processor. He explained that he could never type, but somehow he was able to jump from longhand, legal-sized paper, and he now uses the processor.

SIEGEL: I don't suppose it's affected his ability to compose, because it is to me a wondrous thing that

someone born abroad can handle the English language better than most native sons and daughters that I ever met.

VDT: You're absolutely right--elegant style.

SIEGEL: In that case, my work consisted primarily of adapting what he wrote to textbook format and that meant reducing his material so that there were specific examples in the text with numbers that could be tagged to the tables. I had to do that kind of editing.

VDT: You and Henry were editing and writing?

SIEGEL: We were writing several chapters and we were responsible for certain chapters. All the chapters were assigned either to Henry or to me, by us. We then were the primary editors of certain chapters if we had other authors submit manuscripts for them. Sometimes our principal role was to extend the chapter because the author very frankly, and maybe properly, said, "Well, that's not my thing." There was an example of that with Francisco Bayo; he's the deputy chief actuary of the Social Security Administration. I knew Frank well from work contacts and Frank is an excellent mathematician/actuary. But I had to edit what he did for the purposes of a textbook. Frank said to me, "You asked me to write also about the uses of the life table in demography and that's not exactly my thing." I wanted him to explain how you calculate and apply survival rates of various kinds; how you average and combine survival rates; how the life table is applied by demographers in connection with the idea of model populations.

Furthermore, we wanted this not to read like there were 12 or 13 authors, but like one person wrote every chapter, since one person would be reading from chapter to chapter. I think we achieved that pretty much. It's very troublesome to a reader when he/she reads ten articles in different detail, overlapping, and in different styles. We didn't try to eliminate all the repetition, but I think we achieved a coherent, internally consistent document.

VDT: I've forgotten whether these associate authors are given credit at the front.

SIEGEL: Yes, they're named on the second title page as associate authors.

VDT: And it says somewhere who wrote which chapters?

SIEGEL: Yes, in the preface.

VDT: But in some cases they were heavily edited or added to by you two senior authors.

SIEGEL: There were some that had to be heavily edited and rewritten. There was one chapter for which there was no precedent and the management of the chapter bounced around between Henry and me and other people in the office. That is the one on marriage and divorce. There was no precedent for a textbook chapter on that subject at the time.

VDT: Really? But there've been whole books on it.

SIEGEL: You couldn't find a book on the measurement of marriage and divorce, or even a summary chapter on it. You couldn't find any place that said, Here's what a crude marriage rate is, in combination with a range of related measures.

VDT: What about Paul Glick? He didn't have measurements then? Who finally wrote that chapter?

SIEGEL: I started out. I outlined the chapter for a new employee who had just turned up in my office, whose background included a doctorate in sociology. His name is Charles Kindermann, and he is given credit for the chapter. However, I don't think Charles was very interested in that subject. It wasn't long after that that he went off to the Bureau of Justice Statistics. The chapter then was worked on by a number of people in successive stages.

VDT: I remember that the Georgetown people at your Saturday morning classes said you tried out the techniques of the book with them.

SIEGEL: The first time the book was tested as a text was at the University of Southern California in the summer of 1969. The students had xerox copies of drafts of the chapters. I started in 1971 at Georgetown, at Henry's invitation. I don't know if I've said that.

VDT: Not yet, no.

SIEGEL: Well, I'm saying it now. By that point, I didn't have printed copies of the book and I remember giving out galley proofs. I had several copies of the galley proofs for certain chapters and I was unloading these galleys on the students.

VDT: Talk about giving out something hot off the press!

SIEGEL: "Here, I have the galley proofs for this chapter and that. Great, I want to get rid of these anyway." It had the errors corrected on different pages.

So we finished the work over the course of three years, with the assistance of a number of people, and I must say, one of the persons who contributed heavily to this was Sylvia Quick. The reason I mention her particularly is that it was her work which essentially made the selection of countries and examples for many of the problems. She would say, "How about Iran for this?", and it was often a very wise choice that made the point. She's the one who did all the library and bibliographic hunting and extraction for many of the chapters in the book. Also at the Bureau, particularly for a chapter that I worked on, Wilson Grabill drafted the heart of the material on interpolation methods, a subject which he had been teaching me in part 20 years earlier when I first got to the Bureau.

VDT: He was still at the Census Bureau at the time you were working on Methods and Materials?

SIEGEL: Yes, he was there in the late 1960s.

VDT: Another thing I've heard about you and your work--and I know--is that you are meticulous about your work and writing and expression. I'll be asking you who have been the leading influences in your career--you've mentioned many of them. But first I want to tell you that Art Campbell called you the Number one influence in his work, from the brief time he was at the Census Bureau, working with you [1952-56, in the Foreign Manpower Research Office of the Bureau]. He said in part you were such an excellent teacher. You were meticulous about writing and you expressed yourself so well.

SIEGEL: Well, in this record, I should thank Art for his generous remarks and mention how fine a scholar I thought he was and is.

VDT: Have you always made a conscious effort to be careful about your writing and expression? You mentioned Frank Lorimer not being able to finish sentences; obviously, you must have been aware of that kind of thing.

SIEGEL: Well, that was a rather extreme thing, but I'm also aware, as I said, that Frank wrote beautifully. I have no question about that.

I do have a real interest in seeing the English language used correctly. So I do kind of balk at many of the popular tendencies to change the spirit of the language. There are some popular tendencies which are gross, like making every word into an adverb by sticking "wise" on the end of it. There are also a couple of others which I balk at, because I feel they so change the spirit of the language; for example, the tendency to make nouns into verbs, like, "The population will peak at 422 million," rather than say, "will reach a peak." It isn't simply that I object because it's an innovation, but one has to be very careful about changing basic usage, particularly, for example, intransitive into transitive verbs, like, "this will impact that." Or freely resort to jargon, such as, "The bottom line is."

But the commonest abuse of the English language is to give it the character of German. That is what the grammarian calls the "nominalization error," that is, crowding several nouns together in a single phrase, using them as adjectives.

VDT: I'm not quite sure what you mean. Have you got a favorite bad example?

SIEGEL: I'll make one up. Here's one that in a jargonish context might be acceptable because everybody would know what you're talking about, but if you do this to a general audience it would be extremely burdensome, as when you say, "the undercount improvement extension program." Now that one you know, because you happen to know the way I'm using the words "undercount" and "improvement" and all that. But English is constructed more like Spanish and French than it is like German and we should learn--perhaps it's a little awkward otherwise--to use prepositional phrases. I can give you examples from an article I gave to my class this summer in which the English is unfit for students to read. It was published, I think, by OMB.

VDT: Bureaucrat reports are often anonymous. Still, you cared.

SIEGEL: Sure. But on the other hand, I did often have to tell editors at the Census Bureau that they should not be so hidebound and restrictive in what they did as not to allow us to invent a new technical word. I know I've invented one word in the English language, if not more.

VDT: What's that--the word you know of?

SIEGEL: The word I know of, that I announced in a public forum and then began seeing used in articles by the people who were present, is "gerontic."

VDT: That's your word? Everybody knows that word!

SIEGEL: I could tell you the year, the public forum, and so forth.

VDT: We'll talk about your work on aging, but first on the undercount. That must have been an offshoot of your work on population estimates and projections.

SIEGEL: When I felt that I had gone about as far as I could in estimates and projections, the field I was assigned to--I don't mean that the field was completely developed; obviously, lots of things have been developed since--I thought I had better work in a different area instead, or in addition. So I then went on to explore the whole undercount issue. When I left estimates and projections per se, the next general area that I saw as an area for the Census Bureau to do research in was demographic measurement of the undercount.

VDT: What census did you work on first?

SIEGEL: First I worked with Don Akers, who did some work on the 1960 census. I think my first publication had to do mainly with the 1960 or 1970 census.

VDT: Were you the first to draw attention to the undercount, which has become such an issue now?

SIEGEL: No, no. That was noted by George Washington!

VDT: Oh, yes! That's true.

SIEGEL: That is to say, the probability of being omitted from the census; many census directors have commented on that. I believe there was a study by Warren Thompson relating to the 1920 census, or was it the 1930 census, which specifically discussed the undercount question, considering the consistency of the counts in successive censuses, and when I came on the scene, already it was clear from the publications of the National Resources Planning Board and all the work of Thompson and Whelpton in projections. They were showing two estimates of children under five: the estimated true number and the census consistent estimate.

VDT: Do you think we'll ever get an undercount really built into the census--the 1990 census seems it might come close to that--so we'll have the official "true" number?

SIEGEL: Are you saying that you think the Census Bureau will officially adjust the numbers?

VDT: Yes.

SIEGEL: Well, you're asking me more of a prediction.

First, instead of answering your question directly, you understand that one of the key dilemmas that people working in this now must face has to do with the fact that in 1980 the important coverage improvement procedures added millions of persons to the count, but many of the additions might be described as spurious, because they were duplications. The improvement procedures, a few of them, included many duplications or persons erroneously included, that is, people were added who had already been enumerated or shouldn't have been. And while the count was closer to what may have been the true number, it should be troublesome that you got to that by a procedure which raises serious concerns as to its acceptability.

So, if you are familiar with the position now taken by Barbara Bailer, she is saying that we need to take a counter-intuitive position--my words, maybe her idea--that is, to sharply restrict the coverage improvement procedures of the 1990 census. That is, to keep incorporating into the census coverage improvement procedures which would tend to make it appear to approximate the truth, tended in fact to add people spuriously and not contribute to the diminution of the difference between

the coverage of blacks and whites or various racial and minority groups. So one of the purposes of the coverage improvement procedures is not achieved, and, indeed, is defeated by that effort. So she's now taking a position that the census counts should be adjusted, instead of our spending as much money as formerly on coverage improvement--a logical, but counter-intuitive, position.

Now, do I think the census will be adjusted? No--not if you mean the entire census in its use for apportionment, allocation of funds, and for its general-purpose uses. I can more easily imagine the possibility of there being an official count, however achieved, without an adjustment, but maybe with coverage improvement procedures built in, used for the apportionment of the Congress, as in 1980. I can even believe that there will be a minor adjustment excluding illegal aliens for the apportionment.

VDT: You consider that minor!?

SIEGEL: Well, I say minor in the sense that numerically it wouldn't represent a major modification in the count, but it might affect the representation in some states, certainly.

I'm almost saying that what I think will happen may be something which I think would be okay to happen. That is, that the census count, combined with some coverage improvement procedures incorporated into the count, would be used for the apportionment of the Congress, subject possibly to the exclusion of illegal aliens--a demand being made by FAIR [Federation for American Immigration Reform]. And that the census data could be adjusted in specific cases where there was an allocation of funds by Congress, as required by specific legislation. In other words, if an act calls for the allocation of funds, the law could either incorporate the requirement for adjustment or be amended to require an adjustment for this particular allocation. That seems reasonable to me. Now I would not want the Congress in that case to specify the arithmetic of the adjustment, but simply that the Census Bureau should use its expertise to make the adjustment in certifying the numbers for that allocation. So I'm saying that I don't think there will be a general adjustment of the numbers of the census, and yet, under certain circumstances, adjustment could be called for.

You were asking about my shifting from subject to subject.

VDT: Yes. Now into aging.

SIEGEL: Art Campbell was, in an important sense, instrumental in my getting into the aging area. Along about 1972 or 1973, he called me from NICHD and invited me to give a paper at a meeting being held at our nearby retreat, the one in ElkrIDGE, Maryland, on what is called the "epidemiology of aging." There were a few sociologists there, but just a few in a large group of people who were mainly specialists in one or another area of medical research, whether neuro-muscular or neurological or digestive or whatever. I prepared a paper on the demography of aging, which was incorporated into the proceedings of that meeting. I subsequently decided to elaborate the paper into a Census Bureau report, because I'd done the work on Census Bureau time, more or less. So the first of the P-23 reports on the demography of aging appeared shortly after that [Bureau of the Census, "Demographic Aspects of Aging and the Older Population in the United States," by Jacob S. Siegel, Current Population Reports, Special Studies, Series P-23, No. 59, May 1976].

VDT: That became another bible of the trade, like Methods and Materials.

SIEGEL: That's right. That was the first time that the Census Bureau was in the business of the analysis of aging. In fact, if you'd asked who was in charge of "age," you would have a problem. Age wasn't thought of as a subject of analysis the way marital status or internal migration or education or income or fertility were thought of. "Everybody owns/knows age"--right?

VDT: You certainly became known as "Mr. Aging." You are known as the leading authority in that field now.

SIEGEL: Then in the years that followed, there was a second NIH conference on the "epidemiology of aging" and I gave an update of that paper in that conference. I updated the P-23 report. Two more have come out in that series [Jacob S. Siegel, "Prospective Trends in the Size and Structure of the Elderly Population: Impact of Mortality Trends and Some Implications," Current Population Reports, Series P-23, No. 78, January 1979; and Jacob S. Siegel and Maria Davidson, "Demographic and Socioeconomic Aspects of Aging in the United States," Current Population Reports, Series P-23, No. 138, August 1984]. Each accumulates and elaborates the earlier material. You don't need to refer back to the earlier reports.

VDT: Now you're working on the 1980 census monograph on the elderly population. Are you doing that on your own or with Cynthia Taeuber?

SIEGEL: No, she bowed out. It's on my own.

VDT: But you have done a few things with Cynthia Taeuber on aging?

SIEGEL: Yes. We wrote two papers, mainly on data sources and limitations of data on aging in the censuses and a general summary of the demography of aging ["Demographic Perspectives on the Long-Lived Society," Daedalus, Winter 1986]. And she is now the nominal expert at the Census Bureau. Now there is an office. When I left the Census Bureau [in 1982], aging became a subject. It never was a subject while I was there officially. In other words, there was no one who had a title related to aging. My title was whatever it was at the time [Senior Statistician for Demographic Research and Analysis]. But now the aging population is a subject, like internal migration. Cynthia Taeuber is the expert [later shifted to the homeless population and followed by Arnold Goldstein as the "expert" on the aging population]. And the international branch at Census has received lots of money from NIA [National Institute on Aging] to develop a data bank on aging and the two divisions are also working together.

VDT: And all of that originated with your input?

SIEGEL: First it was really under the table. There was no appropriation for that work; I just worked on it at the office, at home, wherever I could get it done. We're supposed to work on assignments in government agencies; we're not supposed to just sit there and dream up we would love to work on.

VDT: And that was something you thought you'd love to work on?

SIEGEL: Well, Art asked to write the paper and I simply agreed. Strictly, in a bureaucratic context, you've got to go to your boss and say, "Can I devote time to this?" Whether I did that or not I'm not sure. I can only tell you that in the first several years, there was no money for it, and then finally NIA gave me an annual grant. By the late 1970s, I was receiving a small amount of money from NIA for the work that I was already doing.

VDT: Did you also continue to work consciously on this because you realized it would become an issue of U.S. society--an aging society?

SIEGEL: That certainly was true. I kept saying, "Where could I go now to deal with the up-front subject?" In making population projections in the days that I worked on them, the big issue was fertility. You had to think of yourself as a fertility expert in the 1940s, 1950s, and 1960s. In turning to aging, you had to become a mortality expert, and I was saying, "That's really 'where it is' now. There have been fluctuations in mortality that have been missed and these need attention."

VDT: You mean the time the mortality decline got stalled, in the 1960s to the 1970s?

SIEGEL: It certainly did; it stalled for 14 years, 1954 to 1968. Life expectancy for men went slightly up and for women slightly down.

VDT: Were you the first to discover that--the famous plateau?

SIEGEL: I can't say that. I only know that I wrote all about it in that first paper and have since grouped my data specifically in 14-year blocks. In that way, you get the death rate declines sharply outlined.

VDT: In addition to the mortality decline patterns, all demographers knew that the American population was aging. The median age is going up. It will be sort of on a level for some time, but then it will jump up because of the baby boom's aging.

SIEGEL: And there were other things that we focused on, that is, that the elderly population itself was aging, that the 85-and-over population was growing most rapidly. But the other thing we chose to point out was that the aging wouldn't continue. When the baby boom group entered the elderly population, it would get younger. And also that the new declines in mortality, from 1968, are really spectacular. These declines were to be described either as more evenly spaced over the age span or as including sharp declines at the older ages from causes that were believed previously to be very resistant to any control or progress. We're talking about a one-third decline in mortality from heart disease between 1968 and 1985.

VDT: Which you've written about a lot, whether it is from life style changes or better medical treatment.

SIEGEL: The basis is not clear, but at least we're getting more people to recognize that these changes are not mechanically attributed to "medical developments." In fact, in the broad history of medicine, medical developments don't play a major role in the declines. You have to look to socioeconomic developments, that is, public health in a sense, and more recently, perhaps to the changes in life style, including responses to mass efforts to get people to check their blood pressure and to exercise and change their diet. And that is not a medical development. The medical development has already occurred, that is, say, establishing the relation between blood pressure and cerebrovascular disease and control of blood pressure by medication.

VDT: Well, you've gotten famous for how you treated what some might consider a deadly subject.

SIEGEL: I have gotten to be very--not active in, because I'm not seeking office--but I have gotten to be very much a part of the Gerontological Society, the professional society of gerontologists. I attend the meetings regularly. I was made a Fellow this spring. So I have some credentials.

VDT: Indeed. Then there was your PAA presidential address of 1980--"On the Demography of Aging". That was one of the most memorable that has ever been done, in part for what you left unprinted--I won't say unsaid--the part that was left out of the printed version that appeared in Demography of November 1980, but which was picked up by American Demographics in the September 1980 issue, and which we were all talking about on the plane on the way back from the meeting in Denver. That was your recommendations or suggestions for what to do about the tremendous excess of women at older ages. You were really saying, in effect, "Cherish the men." You suggested free health examinations for men only, higher deductions for medical expenses for men on income tax returns, bumping rights--this is the one I really remember--that men should be allowed to bump women in getting medical appointments, then, polygamy too! Now, that was lot's of fun. Didn't you enjoy that? And why did you leave it out of the printed version?

SIEGEL: I want to back up for a moment to say that I was striving merely for survival in 1980.

VDT: You mean survival as PAA president?

SIEGEL: Survival in the total sense of social, intellectual, and physical survival, looking to 1980 as having to oversee the meeting program of the Population Association, prepare a presidential address, and carry out my responsibilities in connection with the 1980 census, including the court cases. Add the summer of 1980 in Detroit; I was a witness in the Detroit suit against the Census Bureau.

VDT: Was Detroit the first to sue the Census Bureau?

SIEGEL: No, there was a suit that I was a witness in after the 1970 census. That is the case that nobody remembers: East Chicago versus the Census Bureau. That was the first big case. East Chicago, which is a suburb of Chicago in Indiana, sued the Census Bureau on the ground that the count would be inadequate because of the heavy concentration of blacks in East Chicago. I can't remember whether they were petitioning for an adjustment or not. They must have petitioned for something, since the law would suggest that if you want to win a suit you not only must show injury but you must also show that the defendant is responsible for the injury and has it in its power to do something about the injury which you consider a suitable remedy.

VDT: You were a witness for the Census Bureau?

SIEGEL: No, I was an adverse witness; I was subpoenaed by the city of East Chicago to testify about the extent of undercount in censuses. They subpoenaed me, and the government tends to honor such subpoenas of its employees. Then after I testified for them, I actually changed where I sat in the court to become a consultant to the Census Bureau.

So 1980 was the year that all this happened, including a grand conference that we had around Labor Day, a brainstorming session, to discuss all the issues relating to the undercount. In any case, I wanted to say that I had great difficulty in deciding what the topic for a presidential address would be. I asked myself: Would I talk about some aspect of population estimates, the undercount, or aging?

VDT: The three leading issues of your life.

SIEGEL: Yes. Of course, I have a fourth one now. Now I'm into applied demography.

VDT: That's right. I was going to ask about that at the very end of the interview, but now we're still

on aging.

SIEGEL: And I couldn't really decide, so I started to write something about aging and barely finished by the time I had to stand up in front of the audience. I spent that morning in my hotel room writing and throwing out paragraphs or stuff that had been written; I just hadn't had enough time.

The job of preparing the program had been a massive one, but I was ably assisted by three ladies in my office, who wanted me to remember them and who gave me a T-shirt with their pictures on it, which says, "Siegel's Angels." There was Mary Kisner, my secretary, her assistant, and another staff assistant. They're the ones who did all the checking in of the papers and most of the telephone calling that is required.

VDT: The PAA president assumed responsibility for the program in 1976 [with Sidney Goldstein's tenure]. Prior to that it was the first vice-president. It was then that they switched to the president having to do everything.

SIEGEL: Nowadays, mostly, they appoint a large committee. I appointed a few people, one of whom did some work and the others of whom did nothing.

VDT: So really you and your three angels handled the program for that year?

SIEGEL: I must say that I and the three angels did nearly all of it.

VDT: You must be the first who ever did it without a large committee. Plus you weren't at a university.

SIEGEL: Somehow I find myself trying to do many things alone or reinventing the wheel myself, without consulting.

VDT: So you're really saying all these things came together for you in 1980, so you can't be held responsible for what you said that day?

SIEGEL: This was by way of historical background. I believed in what I said. First of all, I do want to say very seriously that I consider the inequality in the numbers of the sexes at the older ages one of the leading demographic facts of our time. And one of the serious problems of our time is the inequality in the level of mortality of the two sexes. This difference in mortality or survival has many consequences of social import. They're not fully measured but easy to speculate about generally, including a lot of orphanhood, of course a lot of widowhood, and at least up to the present and to some extent in the near future, a lot of poverty. Certainly, a lot of solitary living among women. And while we can say that women manage to do well alone, I think that statement is very much in the spirit of saying that a person can do without classical music because they're enjoying rock music well enough: leave them alone. Okay, if you want to adjust to a limited life. But from my experience--and I speak not from research--I'll say that, while some studies may show that women adjust adequately to solitary living, my sense of it is that in truth most solitary elderly women are unhappy and lonely. That is the gutsy fact of the thing; that what we have here is merely an adequate adjustment. You can be adequately adjusted and still have a lonely life.

Anyway, my point there was simply to underline the facts, to emphasize the point that I saw this as a serious problem, and then simply to elicit the audience's interest by suggesting a number of alternatives, some of which were extreme for our society at this time, but some of which could be

taken as serious suggestions. I have invented a word now--I recently invented it while at Cornell--and I simply say that in this I am a "masculinist" as opposed to a "feminist."

VDT: That's great!

SIEGEL: I'm not a male chauvinist. I'm simply trying to say that we should look at this problem more as a problem of prevention rather than treatment. If you talk of old age as being mainly the problem of women, what you're simply implying is that most old people are women. Of course, this is true. But I'm saying no to this as a way to see the problem constructively. The problems of old age are problems arising from the fact that men die prematurely, to a large extent. Of course, there are some problems relating to old age that are not so intrinsically related to sex. And that extra effort needs to be devoted to try to diminish this difference to change the picture. We're talking about men dying in their forties, fifties, and sixties.

VDT: Is that why you go out and get your exercise, playing tennis last Sunday, even though it was too hot to be doing that?

SIEGEL: You would think that was a foolish thing. But I do try to maintain a life style that will keep me physically fit.

VDT: We've talked about several things I had on my list. I was going to ask about your fluency in Spanish; you've explained that. Both Andy Lunde and Con Taeuber had told me you learned sign language to communicate with a deaf person at the Census Bureau, but neither told me it was Wilson Grabill.

SIEGEL: Wilson Grabill and another staff member. He had an associate named Rex Loman, who became a professor at Gallaudet. But it was mainly Wilson; I didn't work much with Rex.

VDT: Who have been leading influences in your career? You've mentioned Wilson Grabill and Norman Lawrence.

SIEGEL: Well, of course, Henry Shryock. Henry's influence began as a kind of "tough lov" influence, somewhat like Norman Lawrence's and Hope Eldridge's in the sense that if you're a kind of self-effacing person that I tend to be, you're not comfortable with bosses perceived as severe. But you also profit a great deal from them, especially if you come to believe that they're fair in their judgments and they recognize when you do something okay. Self-effacing people tend to need supportive statements, like, "You're doing this or that okay." They react excessively badly if they're criticized, but over the long run it comes out well for them if they feel their boss is basically supportive of them. And Henry Shryock has been very supportive. It was Henry who said, "Do you want to come teach techniques at Georgetown?" He didn't have to do that. Of course, if there's a spot open, you need a teacher, but you're not going to offer it to somebody that you think can't handle it.

VDT: You mean the Saturday morning class?

SIEGEL: I said that the only way I could do it would be on Saturday, because I could not so disrupt my ordinary day, living where I do, to come downtown. I'm a kind of nut about the fact that you ought to live a kind of regular day. I'm doing something now which I describe as uncivilized; I'm teaching my class from 6 to 8 p.m.--an uncivilized time. It spoils your dinner time.

VDT: But weren't the Department of Agriculture classes at uncivilized times too? What hours were they?

SIEGEL: Uncivilized.

VDT: And you did it for 20 years?

SIEGEL: Right. Perhaps as a younger guy I was more flexible. I had a pattern which adapted to the situation. I worked to 5 o'clock. At 5 o'clock each Tuesday, I would put out the lights in my office, take off my glasses, maybe even kick off my shoes, and put my head on the desk for 15 minutes.

VDT: And take a nap?

SIEGEL: Whatever. I might even have locked the door. Then somewhere about 5:30, I would pull out a sandwich dinner that I had brought. I couldn't survive otherwise. There was no time to go to a restaurant before a 6 o'clock class and my metabolism then--and even more so now--would not permit me comfortably to talk for two hours without having dinner. That was the routine: that day I packed a sandwich dinner and sat at my desk from 5:30 to 10 of 6 and ate my sandwich. Everything was timed to the minute. Then I ran out to the car and by 6:20 or so I was at the class. Then I could have a second late dinner at home.

At Georgetown [regarding Saturday morning], people were not opposed, but doubtful. And, of course, history has proved that it was a very judicious choice.

VDT: That's right; it became a famous class.

SIEGEL: The students were fresher. I was in a good state of mind. I had no traffic problems getting there; no parking problems. The students didn't feel they had to sneak a bite out of a chocolate bar. And everybody began to realize that Saturday morning usually gets wasted anyway; this was productive use of Saturday morning.

VDT: And Con Taeuber said in his introduction to your PAA presidential address that you also led the class jogging down the corridor, halfway through. Are you a jogger?

SIEGEL: Well, not in the formal sense. I know people who jog in marathons, some of them in their fifties and sixties. It's only that, from way back, I found that exercise had a rather exhilarating effect on me, if it also--I'm pragmatic about it--helped me get something done. So that while I would beef often about mowing the lawn, seeing it as a job that got you nowhere--when you were finished, the lawn was mowed but that darned grass was going to come right back--after I'd mowed the lawn--and I nearly always have mowed the lawn myself for all these 30 years--I was pleasantly tired. I simply was in a different mental state after; I could concentrate and do my work. I have experienced the mental cloudiness--I don't have a better word for it--that comes over you from persistently trying to work. So I have made a point of regular exercise. Not as a fanatic, but I try three times a week to exhaust myself physically in one way or another. These methods include dancing. I'm an addict at rock dancing, meaning, you can't put me in a room with rock music and expect me to stay still; you can't expect me to go to a musical and sit still while they're playing that kind of music on the stage--it's unfair treatment. And I'm a fair expert in ballroom dancing, as dancers go.

VDT: Let's go back. Henry Shryock was your boss as well as coauthor?

SIEGEL: He was my immediate boss a good part of the time from, I guess, the late 1940s or early 1950s on through the middle 1960s. But, of course, I was associated with him at the Census Bureau from 1943 to 1970--all the years we worked in common at the Census Bureau.

VDT: And you worked well together. Any other influences?

SIEGEL: There were some people whose gentle nature I appreciated, like Don Akers, Henry Sheldon, and Dick Irwin. Dick Irwin was a Census Bureau employee who worked on state and local estimates with me for several years.

There were people outside the Bureau whom I have had tremendous admiration for in the work that they were doing. They set a goal for me, not always one I felt I could reach but one which inspired me. Among those, certainly, were Nathan Keyfitz and Ansley Coale.

VDT: You knew them best through their writings?

SIEGEL: Yes, but not solely. I had work associations with them, in the sense of talking about substantive matters with them over the years.

VDT: They are both mathematical demographers, which, of course, you became. You never felt the lack, obviously, of--well, frankly--a Ph.D.?

SIEGEL: Yes, I have. All these years I've felt I might go back and get a Ph.D.; I felt it's necessary. In the meantime, I had to go ahead and work, so I just did what PhDs do: I wrote books and papers and I taught college. But the fact of your not having it is something which you are quite aware of because of the way the system works. It is not that I disagree with the system. I do believe that you have to deal with certificates and formal methods of stating who is qualified. I don't have any objections to the system; it's only that it is quite influential in how you think or are treated or the alternatives that are available to you.

However, as was said, in practice it hasn't resulted in any tremendous restriction on me, because when I wanted to teach I was able to teach. Of course, in this process I had to have a home base and often had to go away to teach--to the University of Connecticut at Storrs, to California at Berkeley, and just a few weeks ago I came back from Cornell at Ithaca. These were only vitising professorships, but, you see, I was able to do what I wanted.

VDT: Indeed, you did. But you do stand out, somewhat, as a leader in the field who did not have a Ph.D.

SIEGEL: I had two models of an earlier period to inspire me; neither had PhDs. They didn't know that they were models for me, but I was acutely aware of the fact that Pat Whelpton didn't have a Ph.D., and I worked with him, and the other was Fred Stephan.

VDT: Fred Stephan? I don't know that name.

SIEGEL: Just talk to the older generation. He was a professor of social statistics at Princeton. I was quite aware of that in my junior days--that in academia, you could get around the lack of a Ph.D. It was known that you could get around it if you had some other professional degree. For example, if

you had a law degree, you wouldn't have any problem in the area of business administration, say. But in the more academic fields, it seemed like that the Ph.D. was the necessary thing, except that there were exceptions. The exceptions are made possible by the fact that you write the text that the others use in their classrooms or you write documents or papers--instead of a formal dissertation that was reviewed by a committee in a specific field--documents of a kind that they find themselves drawn to use in their classrooms beyond all the competing possible articles that they could use for the purpose.

Of course, in this it's all-important to write for so-called peer-review magazines, not just to get the stuff published, though that's all right for some universities. For the prestigious universities, you have to have a record of writing for journals requiring peer review. Or have other credentials which identify you as being professionally competent. So I felt quite pleased--more than ordinarily--for being chosen as a Fellow of the Gerontological Society, because I don't have the usual academic credentials.

If you saw the list of people who were made Fellows this year [1988], you would see that we're into a new academic era. When I was a college student, it was an uncommon thing in my circle to have a degree.

VDT: Any degree?

SIEGEL: Yes. It was not common in 1941 for someone to be a college graduate, and you were academically outstanding if you had an M.A. By the 1960s, to be academically distinctive, you had to have a Ph.D.; an M.A. alone was no longer good. Today, you're not academically distinctive merely with a Ph.D.

VDT [looking at list of persons appointed Fellows of the Gerontological Society in 1988]: The list includes people who aren't just plain old PhDs. What are those other letters?

SIEGEL: They are medical people. I often now meet or read of people who are PhD./M.D.s. A Ph.D. is not not enough to be different. You were in front of the crowd in 1940 if you had an M.A. Then you had to have a Ph.D.; today it's run-of-the-mill. In the circle of applicants as graduate college instructors or people that are distinguished in the academic crowd, I now see lots of people with these double degrees. Look at all of these.

VDT: Ph.D./M.D., Ph.D./LL.B.

SIEGEL: Yes. For example, this is what you do if you want to become a specialist in legal medicine or bioethics.

VDT: Well, how many of these have got the publications you have?

Let me throw out this big question: What accomplishments in your career have given you the most satisfaction? Now, you've got so many it might be hard to single them out, but--off the top of your head.

SIEGEL: You notice that I'm speechless. I don't know that I have any way of singling them out. I'm certainly tremendously pleased and flattered by all of the attention that The Methods and Materials of Demography has given me. I think perhaps I've gotten an undue share of it, because I am now easily the most visible part of the machinery that produced it. Henry is less visible [being retired]. I still participate generally in all the professional activities. The associate authors may never have been given their just share of attention and are easily forgotten. I don't think our third author, our editorial

associate Elizabeth Larmon, was even named in the condensed version [prepared by Edward Stockwell, published by Academic Press, 1980]. And I don't think that Ed Stockwell has gotten his fair share of attention. I'm the most visible person, I would say.

VDT: Methods and Materials goes on being reprinted?

SIEGEL: No, the original is no longer available. The two-volume book was printed four times and is now out of print and will never be printed again, barring a miracle. The Census Bureau was simply unwilling to put up what may have been merely a thousand dollars to assure the fifth printing, although I understand from having talked to people in the Data Users Services Division only yesterday that they get requests every week for copies of the original two-volume version. The condensed version is a commercial operation and I imagine that the publisher has reprinted that as needed. It's never been re-edited, so they reprint as often as they wish.

VDT: And you say that in the sense of being visible, you are identified as the author of Methods and Materials, which is the bible in demographic techniques.

SIEGEL? Well, I'm not the first-named author. I'll tell you why I could be most visible today in another sense and that is that it's probable that the use to which the book is most commonly put involves chapters with which my name is most commonly associated. I am not particularly associated with how censuses are taken or with the collection of vital statistics or the concepts in the census, but these topics are not usually included in courses in formal demography. But I was responsible for the chapters on the life table, the analysis of death and birth statistics, the mathematical material, estimates and projections, and the measurement of international migration. I did not do the ones on internal migration or reproductivity. So I think that may be another reason.

VDT: So that has given you a lot of satisfaction. Any other of your accomplishments which you can pinpoint? You've obviously had great satisfaction from your teaching.

SIEGEL: Well, I think I share the feeling of most people who teach over long years that they enjoy the lecturing and the classroom situation, including the sharing of knowledge, but they hate to mark papers. I find grading papers, unhappily, sometimes a depressing experience. You're troubled that you have to give a low grade to someone you think may be a nice person or a good student or who needs a good grade. You're troubled because students sometimes give you back so little of what you thought you had given them.

But, of course, I am tremendously pleased by what I can almost now say are the hordes of successful students that I have had. As I sometimes say, if all of the students I have ever had got together, we could conquer the demographic world!

VDT: Lovely! You have one right here at Georgetown, Betsy Stephen, who is now on the faculty of Georgetown. Have you got some other outstanding ones?

SIEGEL: Some have gone to high places. Judy Seltzer is an official at AID.

VDT: She did her Ph.D. at Hopkins.

SIEGEL: And Catherine Pierce is second in command or something like that at UNFPA. And there are many, many others who have gone on to doctorates. We have several people now or previously at

NCHS who have come through Georgetown, who were my students: Bill Mosher, Barbara Wilson, and Deborah Dawson. One is a professor at UNC in sociology: Margo Koss. Come to think of it, many of them are ladies. There are just tremendous numbers of them and they work in all fields of demography. People who've gone abroad for years--people who work for international agencies; foreign students who are officials in their own countries. And some work for state and local governments; I had Bob Scardamalia, the state demographer of New York. And what of all the established technicians who came by for the techniques course--Sylvia Quick, Cynthia Taeuber, Charles Jone, Signe Wetrogan, etc. They're all around.

VDT: Now we must touch on PAA--which, of course, is what this oral history project started with--your connections with it. Do you remember the first meeting you attended?

SIEGEL: It would be back in the 1940s. It would be at the Princeton Inn, back in that little inn, where I first met those notable people like Frank Notestein and the rest of them, all of these names that I had read about. Everybody that attended could stay in the Princeton Inn. Maybe the attendance was a few hundred.

VDT: Oh, less than that. We don't have a record, but some people think it was no more than a hundred.

SIEGEL: Could be--actually attending--sure. So those were unusual days because the thing about the PAA that we mention, among the first things about it, was all the more true then, and that is that you could get to know everybody in the PAA if you attended the meetings. It certainly was true then, and it was how I felt from the earliest days. I could meet "all" the demographers in the country.

VDT: And that was a great advantage of being in that field?

SIEGEL: It certainly was--that somehow or other it had a certain exclusiveness. At the same time, it would never become the grand organization of the American Statistical Association or American Sociological Association.

VDT: Grand in the sense of numbers?

SIEGEL: Grand in the sense of numbers, yes--that you belong to a massive organization that was commonly recognized. But, of course, in recent years, demography has been recognized, if only the word itself, including the unfortunate word "demographics."

VDT: You felt it was an intimate association; you could know everybody; and it wasn't grand in the sense that maybe ASA has more clout in the public eye?

SIEGEL: Well, I felt, of course, and still do, that somehow because of the limited recognition of the field of demography and the size of the organization, it does have less clout. But certainly it's more intimate; certainly it's more exclusive.

VDT: That was the word I heard. Do you think PAA is still elitist, exclusive? What do you mean by that?

SIEGEL: Exclusive in the sense that it includes as members a rather limited segment of the people

who are involved with population work. We know that, because there are many more courses taught in population than would be reflected by the membership in PAA.

VDT: You mean that the people who are teaching those courses are not PAA members?

SIEGEL: I did a study on that once for a paper I presented at PAA--the relation between population courses and PAA membership at that time, back in the 1950s ["The Teaching of Demography," presented in a session on "Methodology and Teaching," 1951 PAA meeting]. The majority of people who teach population courses are not members of PAA. A member of a sociology department, responding to the needs of the department, has to teach a course in population because that's what's coming up in the rotation of courses. The typical situation is that that person accepts the assignment, takes a textbook, and simply follows the book. I imagine it's true in most fields. The number of people who are researchers and creative practitioners or involved with the advances in the field as such are relatively few as compared to those who merely practice it or even just teach it.

VDT: So you think that PAA was confined to those who were the researchers?

SIEGEL: Yes--at least in the early years. And I imagine it's still true. The bulk of the people who do some kind of population work or teach a course in population are not in the PAA, partly because population is still thought of, unfortunately, as inter-disciplinary rather than a discipline. I have again and again said that there are no necessary inter-disciplinary fields or, alternatively, no detached disciplines, meaning that the way the pie of academic knowledge is sliced is arbitrary. But as it stands, demography is thought of as inter-disciplinary and, as such, is not a field in itself. So you're an economist or an historian or a sociologist; you're only incidentally a demographer. And that can mean that there's somewhat less prestige and recognition of the field.

VDT: Do you think demographers brought that on themselves? Most American demographers traditionally came out of sociology, like yourself. You mentioned the discussion group of sociologists that you had. You thought of yourselves as sociologists and perhaps demographers second?

SIEGEL: Can't say. Of course, all of them were in demography as well.

VDT: You define yourself as what?

SIEGEL: I have to define myself as demographer; I have limited credentials as a sociologist. Unlike some other people, I don't consider the fact that I'm a member of the American Sociological Association as giving me credentials. I don't know exactly what credentials are needed. Well, I do have an M.A. in sociology, but I have done very little writing in that area. Most sociologists would say what I write is not sociology; it's applied demography. Some of them have contempt for that, my even hinting that it's sociology. So I find myself most comfortable in saying that I'm a demographer, because I have written in that area, taught in that area, and have some organizational accomplishment in that area. Similarly in statistics. I'm a Fellow of the American Statistical Association, so I may say they have recognized me as a statistician. In gerontology, again, I have taught courses specifically in aging and I have this particular--call it award--as a Fellow.

Let's put it this way. I taught in departments of sociology; of course, you have to have a locus of these courses and demography is not usually a separate department. But the kinds of things I do, the associations I have, are not primarily with sociologists. And so I don't have a comfortable feeling of thinking of myself as a sociologist. Maybe some day.

VDT: Let's go back to PAA. You mentioned the thrill of being able to see the great names come to life, like Frank Notestein. Who were the leading personalities that particularly struck you at the early meetings of PAA that you attended? Here are some of the PAA presidents back in the 1940s. Phil Hauser [president 1950-51]; did you ever work with him?

SIEGEL: Sure. He was assistant director of the Census Bureau [1942-46; deputy director, 1946-47; acting director, 1949-50] when I was there, so I worked with him indirectly for a while.

I joined PAA somewhere around the mid-1940s. I guess by that point I knew Frank Lorimer [president 1946-47]. From almost 1945 I worked with Pat Whelpton [1941-42]. Incidentally, later I worked for T.J. Woofter [1940-41] indirectly, when he was director of research for the Central Intelligence Agency [1949-58]. During the 1950s, I was in the international population analysis branch of the Census Bureau, working directly under Parker Mauldin, and we were doing studies of Soviet bloc countries, so I wrote a monograph on Hungary ["The Population of Hungary," International Population Statistics Reports, Series P-20, No. 9, 1958]. T.J. Woofter was general director of this project in a sense, as director of research for CIA. Earlier, he was director of research for the Federal Emergency Relief Administration, the predecessor of the Departments of Health, Education and Welfare and of Health and Human Services, and director of research of the Federal Security Agency.

[Looking at list of PAA presidents.] I can't dissociate the role of outstanding president from the role as outstanding scholar and it does put me on the spot. Partly I would make preferences in my personal associations because of their association with fields that I myself was interested in. I'm not going to think about outstanding presidents, but just use these names as a kind of suggestion list. I really felt considerable admiration for many of these people, partly because I appreciated my association with them, partly because of the fields they worked in, partly because of the particular contributions and innovations that they made. I don't want to slight anyone here; some were good friends almost. Some I also admired tremendously as persons, like Everett Lee.

Okay, I'll single out some names. There was Frank Lorimer. I had tremendous admiration for Joe Spengler as a scholar. If you ever read anything he's written, you must be impressed. I found Harold Dorn personally a very charming and scholarly man, and Dorothy Thomas, of course, a very intense scholar.

VDT: Did you have her at Penn?

SIEGEL: No, she came after I left. I knew her only sort of in the Washington scene, the short time that she was here.

Then there is that admirable couple with the complementary styles--Irene and Con Taeuber. John Durand, another very intense, scholarly person. Tremendous admiration for the innovative thinking of Kingsley Davis; very conservative, but I'm impressed always by the kinds of things that he can continue to deal with. I'm tremendously impressed by just the grand production of Don Bogue. It's of good quality as well, and he's still a powerhouse. Ron Freedman and Norman Ryder I just know in a personal way; I don't follow their stuff. I have just tremendous admiration for the technical virtuosity of Ansley Coale and Nathan Keyfitz.

The next person whom I would single out and who is clearly going to be--if he's not already there--one of the really grand contributors and scholars is Sam Preston. He's still got a way to go and he's got all the makings of a Keyfitz and a Coale.

VDT: You once remarked to me that getting to be president of PAA is not always a sign that the best

man or woman wins.

SIEGEL: Yes. What I was saying is this: Consider the process by which elections occur. It is unreasonable to believe that, in an election in which two candidates of "equal" stature are pitted against one another and one wins while the other loses, that the one who loses is not the next best, if not the best. And yet that person may disappear from organizational history.

VDT: I think there's been a tendency for that to happen, yes.

SIEGEL: Some people are brought back, but do you remember the many efforts to make Christopher Tietze president?

VDT: Ah, he was the one I was thinking about.

SIEGEL: And he never sort of made it. Are you going to imply that Christopher Tietze was not on a par in his field with many of the people on this list?

VDT: You're so right. He was a bit of a mentor of mine; wrote the foreword to the only book I ever wrote on my own [Abortion, Fertility, and Changing Legislation: An International Review, 1974].

SIEGEL: It's partly a popularity contest, as is the presidency of any organization. If you write Methods and Materials of Demography and a lot of copies are used in classes, you get many of the students voting for you, because you tend to vote for whom you know. If two names are on a ballot and you know the credentials of neither but one is a name which has been frequently mentioned, you'll vote for that person--name recognition. Alternatively, if there are two names and you don't know anything whatever about them, you will tend to vote for the first name.

VDT: Or if you're a woman, you'll vote for the woman.

SIEGEL: Ah, you have said it! We recognize the situation, you and I. We just had an election [for PAA president of 1989]: Harriet Presser versus Joe Stycos. There's no principle except the arbitrariness of personal contact and sex polarization in voting that makes Harriet--whom I admire--win, and Joe--whom I also admire--lose. If you ask some of these older characters, they would tend to say, "You have to get Joe in now, because if he doesn't win now, he'll never win; you're not going to put him up again." And they would say, "They both have been tremendous scholars, but Harriet would have a chance again [being younger], but not he."

There you have the idea. In political contests, you have at least policy differences.

So it's partly a popularity contest, subject to knowledge of the names. And now we have the gender business and it's kind of unfortunate. I don't believe that in an earlier era voting for the officers involved as much of an issue of sex discrimination or preference.

VDT: You mean in the days of Irene Taeuber and Dorothy Thomas, they were on a par with the men?

SIEGEL: What I mean is that if a woman was up as a candidate, I don't think that the men tended to vote against a woman and that the women tended to vote for the woman. I think in that sense the elections were more just.

VDT: Many people have said that the early women stars in PAA were indeed accepted just for their

scholarliness and personalities. And we've now become much more gender-oriented.

SIEGEL: Sure. I would have no question about Irene Taeuber. I mean, I wouldn't think I've got to vote for her or against her because she's a woman. It's ridiculous. This whole sex business is new to me personally; it got imposed on my thinking.

VDT: Can you date the time?

SIEGEL: Within the last few decades, simultaneous with the sex revolution. I'm not saying that my vocabulary was not sex-oriented. Sure, all my sentences when referring generally to both sexes would say "he" and "his." But I'm trying to say that at least in my upbringing and practice, it had no clear impact. One of my early bosses was a woman. I have hired many women, and I have virtually a preference for working with women. But now the sex factor has introduced its ugly head, perhaps in ways that didn't affect me earlier.

VDT: I had a question on what changes have you seen in PAA over the years and have the changes been for the better or worse. You've just put your finger on one of them: We've become more gender-oriented. That's not for the better, you're suggesting.

How about other issues? What about the size of the meetings, which have gone from the small, intimate, one-session meetings at Princeton to the New Orleans meeting we've just been to [1988], with over 1,100 people there [1,115], eight simultaneous sessions?

SIEGEL: Like all organizations, the PAA is suffering from the problem of members' need to present papers as such. So, with the practical rule of the Board to keep the days of the meeting to two and a half, the crowding is just tremendous. The overlap of sessions which could be of interest to the same person is just very difficult and troublesome to deal with. In fact, given the varied interests of many of the members, every time you attend a session, you are making a decision not to attend another session that you would like to attend. And I don't know the solution now, except to extend the number of days and that seems to be a taboo with the Board.

VDT: Would you mind that--going to one more day?

SIEGEL: No, I think I would support that, although we must remember that any extension on the last day of the meeting tends to bring about a rather poor attendance at these late-hour sessions--the Saturday afternoon sessions. Maybe the trick is that you have to introduce the extra hours at the beginning, not at the end. The last day is always the last day. People will fly away in the morning and therefore sessions on the afternoon of the last day, as well as most of the morning, are not well attended.

VDT: What do you see as the outlook for demography in the U.S.? This brings up the question of the shift to applied demography which you in your own career have made. Do you think the world is open to applied demographers?

SIEGEL: Well, the acceptance of applied demography as a sub-field by the Association certainly is aiding in the recognition of demography as a field. On the other hand, I think that most of the demographers in the Association as well as academicians outside still tend to think of applied demography as--not quackery--but a kind of dollars-and-cents business, a mechanical business, which is separate from serious demographic research and scholarship. In fact, that's reflected in the fact that

most jobs in applied demography call for an M.A. degree and most jobs in demography call for a Ph.D.

So it is important for applied demographers to be sure to extend the range of their efforts to incorporate elements that one could call research demography and not simply be thought of as people who grind out numbers on short notice for arbitrarily defined areas for business uses. Rather, they need to show that they can bring to bear the full scholarship and research tools of demography to the problems, which would then mean, perhaps, tightened requirements for this sub-field as for any other--that is, a doctorate.

VDT: So we get some more degree inflation.

SIEGEL: More generally, I think the evidence is showing loud and clear that demography has begun to blossom in ways--especially mathematical--that would have delighted Lotka and Kuczynski.

VDT: What are your own plans? Obviously, you're not about to retire. You're involved in teaching and writing that census monograph and the new field of applied demography.

SIEGEL: Well, I have tremendous admiration for people who retire from the field and then turn to other pursuits. I have heard that Otis Dudley Duncan, for example, has turned to computer production of music; I have great admiration for him. I knew a Census statistician who on so-called retirement became a thespian--an actor. So I have that as an option.

But in the meantime, I will continue to do writing and consulting. I have as possibilities the updating of The Methods and Materials of Demography, the condensed version, and the production of a textbook, since an acceptable one is not at hand, in applied demography. And continuation of teaching, but not likely on a term-to-term basis; perhaps on a kind of irregular, ad hoc basis.

VDT: Like Cornell?

SIEGEL: Yes, picking up on the invitations, but maybe less likely going as far afield from where I live, since that process proves even under the happiest of circumstances--as they were in going to Cornell--disruptive for your private life and otherwise.

VDT: Well, good luck with all those future plans. May Methods and Materials of Demography go on forever!

CONTINUATION

VDT: I forgot to ask about your work on the International Demographic Terminology Project. Who's doing that?

SIEGEL: That was originally supported by the Census Bureau and carried on by the IUSSP. INED finally produced the basic version in French. In some sense I've worked on a few international terminology/glossary projects which involved demographic or gerontological terminology. In the case of the gerontological project, I worked in English and Spanish, but in the case of the demographic terminology project, I worked in English, French, and Spanish.

VDT: Any other afterthoughts?

SIEGEL: I may need a copy of this interview so I can write my autobiography.

JUDITH BLAKE

PAA President in 1981 (No. 44). Interview with Jean van der Tak at the School of Public Health, University of California at Los Angeles, May 4, 1989.

CAREER HIGHLIGHTS: Judith Blake was born and grew up mostly in New York City. She attended Columbia University, where she obtained the B.A. in 1950 and the Ph.D. in 1961, both in sociology. From 1953 to 1957, she was associated with the Conservation Foundation of New York City while conducting a study of family structure and fertility in Jamaica. She and Kingsley Davis were married in 1954 and moved to Berkeley in 1955. (They were divorced in 1977.) From 1957 to 1959, Judith Blake lectured in the School of Nursing of the University of California at San Francisco. She was then at Berkeley from 1962 to 1976. At Berkeley, she was, variously, Professor of Demography, founder and Chair of the Group in Demography (1965-67) and of the succeeding Department of Demography (1967-72), the first department of demography in the U.S., and Professor in the Graduate School of Public Policy. Since 1976, she has been at the University of California at Los Angeles, where she is the Fred H. Bixby Professor of Population Policy in the School of Public Health and the Department of Sociology.

Judith Blake's many publications include the monographs, Western European Censuses (1960), Family Structure in Jamaica (1961), and Family Size and Achievement (1989), and a long list of provocative articles and book chapters on such topics as American attitudes toward abortion, childlessness, the status of women, and federal family planning programs targeted to poor women and teenagers, and on the only child and the impact of family size on the quality of children.

VDT: What led to your interest in demography?

BLAKE: I was thinking about that when I saw your letter [with questions to be asked in the interview]. I guess I was really looking for a sub-field of sociology that was empirical and very broad in its disciplinary scope, international, and that would have an influence on world affairs. I wasn't interested in voting and political participation, for example, which was the focus of most survey research at Columbia. This was Paul Lazarsfeld's focus, really his main interest. Although I was interested in the survey side, I wasn't interested in the voting side. And I wasn't a Marxist. There was a very strong, very distinguished, Marxist tradition at Columbia, with Robert Lynd and Herbert Marcuse and all of that. So that was a whole contingent that was kind of ruled out for me.

So for a while, I was really at sea. Then, strangely enough, I was walking along Amsterdam Avenue one day and met Alice Taylor, who became Alice Taylor Day, ultimately, but was then just plain Alice Day, and told her, "Alice, I'm just not quite focusing in this place and I don't know what to do." Alice was always a dear, sweet person and she said, "Oh, that's no problem; you ought to take a course in demography. I know you're going to be crazy about it; it's just for you."

VDT: You were in graduate school at Columbia?

BLAKE: Yes, I was.

VDT: You'd gone straight on to graduate school after finishing your undergraduate degree there [in 1950]?

BLAKE: Right. I was a New Yorker and I really didn't know there was anyplace else; it never

occurred to me to go elsewhere. Also I didn't have much money. And it wasn't so dumb, because I knew the territory there, and for me to have uprooted myself to go someplace else without any financial backing or anything would have been a little foolhardy. So I stayed.

One of the things that had happened was I was sort of burned out, because I had taken all these graduate courses for undergraduate credit and by the time I was a graduate student, there really wasn't much else to take. And I had kind of worked my way through a series of enthusiasms, which I then worked my way out of, and I was just beginning to feel, "What am I doing here?"

Alice said, "The course is just starting up." So I registered. The course was being taught by Hope Eldridge, who was a wonderful person, but I wouldn't say she was a drop-dead lecturer. In spite of that, I just had a feeling that I'd found it, that this was a very exciting field.

VDT: Was this the time that Eldridge was at the United Nations, before she was hounded out of the UN during the McCarthy era?

BLAKE: Yes.

VDT: I didn't know she also taught at Columbia.

BLAKE: It was Kingsley Davis's course and he went to Africa on very short notice, for Carnegie I guess, so she agreed to teach the course. She did a very good job, but, as I say, you would, I guess, have had to have a predisposition to go into demography to have been so utterly overwhelmed by this as a field.

I've asked myself this over time: What was it that just knocked me for a loop? I think partly it was there were so many data. Now this may not sound important to you, but at the time, generating information was not easy. Sociology had just survived the throes--and was really in the throes--of community studies, studies that were mainly asking how many social classes could you find in a community, and some people found six and some found eight and some found twelve. But fundamentally, generating data, information about whether things were true or not true and whatever, wasn't easy. It was really a tough proposition to process those data as well.

So here was a field where you were awash in data; the stuff was just pouring out. That was wonderful. In the course, you were set to work on lab problems where you were actually looking at census data and manipulating them and there they were for any country you could think of. It was just terrific! Also, you could do comparative analyses and I liked that. I liked the idea of looking at a lot of different countries and comparing.

And surveys also seemed to me to be very applicable. As I started thinking about demographic issues, I got beyond the data side very rapidly and started thinking about why things were happening. I started to realize that the training I'd had in surveys should be very applicable, because what people were asking were analytical questions and with censuses it was hard to zero in on those. By and large, censuses were legal documents and you were very limited as to what you could ask, and vital registration data were the same. So I moved rapidly in my own mind from thinking, "This is a field where you're awash in data," to thinking, "This is a field where you're awash in data, but none of them are quite right for what you're asking, what you're interested in." They were suggestive and you would do these analyses, and then you would be left with kind of an empty pot, because the real questions you wanted to ask you didn't have any control over. So at the time, I thought that doing surveys was a way of amalgamating training I had had with what I conceived to be the new demography. And I got really enthusiastic about that.

So I stayed in the course for the semester and was making very rapid strides and thinking about what I would be interested in doing. Then Kingsley came back from Africa and gave the second half

of the course. He was full of all of the international comparative stuff and what had been going on in Africa. That just blew me away, I mean the fact that you would be going to the Dark Continent and seeing all these things; it was a period of enormous change and colonial problems and so forth.

I got a job at the Bureau for Applied Social Research, which was run by Lazarsfeld and Kingsley and Robert Merton and that crowd. I got a job on the cities' project, which was being done for the Air Force. This project was looking at census data on cities, aggregate data. It didn't take me long to realize that I didn't like dealing solely with very aggregated data. Then I thought, "Oh dear, what am I doing in demography, because demography is mainly aggregated data?" And I was beginning to worry again that perhaps I was in the wrong spot.

Then this Jamaica project came along. The Conservation Foundation funded it and they were interested in the birth rate and family structure and so forth in Jamaica. I just flipped out; I thought this was a terrific idea. Actually, it ended up being the third survey of the topic in a developing country. The first had been Paul Hatt's in Puerto Rico and the second was the one Joe Stycos was doing in Puerto Rico; Joe Stycos had worked with Hatt. Joe was going to be the project leader and I was going to be his sidekick and we were going to go down and do this project in Jamaica. You have to realize we weren't very old; maybe Joe was 27 and I was 26, something like that.

VDT: And you were going to run the whole thing?

BLAKE: We were going to run the whole thing. After some negotiations with the Conservation Foundation and being met by all the people who wanted to know who this "Miss Doolittle" was, I got approval. Joe was still finishing up the second Puerto Rican study, the one he did with Kurt Back and Reuben Hill. This was after Paul Hatt's study; Paul had died by that time. I started working that summer on all the problems of how we were going to work on the multiple unions in Jamaica, and we all met in Puerto Rico in early fall, 1953, and started working on the interview schedule--Joe Stycos and Mary, who then was his wife, and I.

We went in a few weeks to Jamaica and found, I would say, a not overly hospitable environment, which was mainly dominated by anthropologists, who felt that doing a survey in Jamaica was about as loony as you could get. They thought this had to be unreal. We didn't pay too much attention to this. Joe is an incredible field person--just an inborn talent for this. And we had the 1953 census we could use as a sampling frame. It had been done a few months before and we could use that. So we went out and got ourselves a sample and did all the right things. We had our schedule ready and we went to the Census Bureau and other agencies to get trained interviewers, whom Joe trained some more, with great skill, and went into the field and did the survey.

At that point, Joe decamped to write up the Puerto Rican results and I decamped to write up the Jamaican results and they became my dissertation and, ultimately, the book [Family Structure in Jamaica: The Social Context of Reproduction, 1961].

Those surveys--the Hatt, the Stycos and Hill and Back, and the Jamaican studies--I think really laid the groundwork for people no longer thinking it was loony to do surveys in these countries. And I think this work made people recognize that you could do what later became KAP surveys, which were much less interested in the causes of fertility behavior, which was our focus, and much more sort of descriptive knowledge-attitudes-and practice (of birth control) types of instruments.

VDT: Your study in Jamaica and Joe Stycos's before in Puerto Rico were looking more at the socioeconomic background?

BLAKE: Yes, and the family structure background and incentives and disincentives--what we considered the big questions. The surveys were much less descriptive than what later became KAP

surveys. Also I think people came to realize that you could do surveys in developing countries. You could train interviewers; you could get results; and this was a viable form of research.

So I enjoyed that an enormous amount; I enjoyed the whole experience. But my life had changed by this time quite a bit. Kingsley and I had gotten married [1954] and we moved to California [1955] and I was in the process of having a baby and finishing the dissertation and one thing and another. So I was a little less free to be running off and doing surveys.

It changed my life a lot to move to Berkeley, because I got pulled out of my own environment, which had been a very urban one. I'd grown up in Manhattan and been at Columbia and went into a situation at Berkeley that was very alien for me. It was alien not only because it was sort of a suburb [of San Francisco], but the university itself was . . . Well, it was very chauvinist; there were practically no women on the faculty and one had the feeling that they didn't want any either.

VDT: That was in the mid-1950s, when Kingsley was Professor of Sociology at Berkeley and setting up International Population and Urban Research?

BLAKE: Yes.

VDT: You lived at Berkeley, but you for a while were teaching at the university in San Francisco?

BLAKE: Yes, they [University of California] had a campus in the city, and I felt it was to my advantage to take lectureships wherever I could so I could get teaching experience and say I had some teaching. I didn't want this long hiatus to develop in my career while I was doing nothing but writing my dissertation. I was very worried about that. I had to have faith that there was going to be a future, which took a lot of faith in the 1950s, because it was not the time when women were doing this sort of thing. I kept saying to myself, "Just take it a step at a time and if somebody offers you a lectureship or this or that, take it. Then take the next step." It wasn't one of these things where you felt, "There's a career out here for me somewhere." You just felt you were going to have to see how it worked out and hope that things would improve.

That's not such a bad philosophy, actually, because life is incremental and I took those jobs and got some experience. I'm sure I was dreadful to begin with and those poor nurses [at the School of Nursing, University of California at San Francisco] probably suffered through quite a few years of boring teaching. But I would have had to do that somewhere. They seemed very appreciative, though. So anytime I got a chance to do something like this I took it, and I gradually got to be, I think, probably a much better lecturer.

VDT: You said Berkeley was very chauvinistic; they were not ready to welcome you aboard the faculty there?

BLAKE: They were not ready to welcome any women much. The chairman of the political science department said that a woman would never get a job on his faculty as long as he was chair. People didn't mind saying things like that. And you felt that you were totally out of the scholarly community, because I was no longer a student and I certainly wasn't a student there, and I wasn't anything else; I was just sort of a wife. So I really never felt that I had any collegial relationships with people much at all.

Well, Jacob Yerushalmy in the School of Public Health was very interested in demography. He felt that something should be done at Berkeley in demography. So he talked to Kingsley about it and then they talked to me about it. I thought that was fantastic. Again, I figured you take it one step at a time. Yerushalmy gave me a job and it was to try to start a group, which was the vehicle that Berkeley

had at the time for establishing academic programs. The idea was that you would bring people together from all kinds of departments who had genuine jobs in those departments but who were interested in a sub-field like this one and they would form a group and put a curriculum together and get a degree program started. So that eventually was what I started working on.

VDT: Which was to become the Group in Demography?

BLAKE: Yes. We got that established [in 1965] and then it was clear that we didn't have any say over people's time or anything. I mean, we were accepting [graduate] students, but you get students and then all of a sudden you realize that when people want to go on sabbatical or something like that, they just go and you're not able to say, "Well, this is not a convenient time for us this year. We're accepting x number of students, something has to be done with them, there have to be courses." Nobody's asking.

VDT: Their primary loyalty is to their departments?

BLAKE: Yes, they're not going to put themselves out of joint necessarily for a group. So it wasn't long before it was clear that this was going to be an economically non-viable situation. And it was pretty scary, because we had students on deck but you couldn't see faculty on deck, at least on a continuous basis. So we went to Roger Heyns and he said, "Well, there's really nothing else you can do but form a department." He was the chancellor of the University of California; he's now head of the Hewlett Foundation.

So the violence escalated, you might say, simply as a consequence of administrative rigidity. The real thought was to get some funding into the Group, but that wasn't being done at that time. So we asked how you could get the funding to get some bodies here that you could rely on and you could have the courses taught and be sure you had somebody there besides me.

So we started working on the department [of demography]. That took a couple of years to get that through.

VDT: The Group in Demography was 1965 to 1967?

BLAKE: Yes, approximately, and there were a lot of people interested in it.

VDT: I interviewed Ron Lee at Berkeley last Friday. He, of course, was one of your first graduates-- M.A. [1967].

BLAKE: Yes. A lot of people were very interested in this idea of a program that was interdisciplinary but that you could do in demography.

So the department [of demography] got established [1967]. It was established with three faculty positions and never had more. I was sort of beating the bushes, raising money, all the time. Well, money wasn't so hard to get in those days and we could get money to get visitors and get all kinds of people; we could patch this thing together for some years. But it became increasingly hairy. I could keep it patched together on the windfall money, but it was wild, because we kept getting all these students, large numbers of students, and with the doctoral program we had to keep trying to gin up these courses for them to take, people to supervise their dissertations, and one thing and another.

Well, it was going pretty well and we started an undergraduate set of courses.

VDT: Oh, you did actually get them started? I knew they were on the schedule.

BLAKE: Oh, yes, they started. They were very popular. This was the time, 1965 to 70, of enormous concern for population and younger kids were all excited about this, so you'd have as many as 200 students in an undergraduate course. It was incredible. We even had an undergraduate major that we graduated some people in.

So it looked as if it was going to be a success--in part, I think, because of the great concern for population in the United States.

But, in time, there was an awful lot of political activity on the Berkeley campus, and by the 1970 cohort, I guess, we had many students who were not only interested in population but were very political. They were very concerned about Vietnam--rightly so, of course. We became--it was a very little, obviously not very strong department--we became a big focal point for this activity.

VDT: You think more so than other departments?

BLAKE: Well, I think that other departments were too, some of them, but they could sustain themselves a lot better than we could, because we were pretty small. And the students felt that we were very vulnerable. A lot of student activity was sort of an effort to take over small departments as communication hubs. They wanted to take over the xerox machines, the telephones, a lot of the typewriters, everything. There was an effort to do this sort of thing.

And part of my problem was that I really felt that all one should be concerned about was demography in the university--I mean, that we shouldn't be political. This itself, I think, probably was a very political thing, in the sense of saying, "Well, I don't think a department should go political." But people who thought it should be political felt that I was being very difficult about this.

VDT: I've heard that side of the story a bit before, from Sam Preston, for instance, who was there [on the faculty] then.

BLAKE: Right.

VDT: I think he felt he was torn, that students were approaching faculty members, asking, "Are you on our side or not.?"

BLAKE: Right.

VDT: He gave the impression that it was a difficult time, indeed.

BLAKE: It was a very difficult time. And I think especially, probably, for a younger person like that, because he was younger than many of them. So much of this was generational; I mean, if you were a younger person you had to be on their side, in a sense. Some people were really very rabid about this.

It sort of erupted and, I think, really was the thing that created the biggest problems for the department. Roger Heyns, who had been very supportive of us, left and Bowker came in as the new chancellor and he was importuned very strongly by the students. A whole coterie of students was not this way, but that didn't count so much. So Bowker's feeling was, "Well, here's a set of faculty positions that I can get hold of and I have no commitment to demography; this wasn't my idea to create a department of demography. This was an achievement that Roger Heyns saw as his, but it's nothing in my life." He just felt that there were all these problems and why continue a department where there were all those problems.

Meanwhile, Nathan Keyfitz and Sam and Etienne van de Walle, who was a visiting professor,

really were not very supportive, I have to say. So Keyfitz went to Harvard and Sam left and went to the University of Washington and, of course, Etienne didn't have a job there; he just went back to his Princeton job, I guess.

But the fact that people left, left things things up to me, and really made it possible . . .

VDT: Left?

BLAKE: Left the faculty positions open and really made it possible to say, "Well, there's nobody here." So Bowker closed up the department. And Kingsley and I were left with a whole big bunch of students to see through dissertations and one thing and another through this sort of a nightmare of having all these people and nobody there to do the work.

So I went through a period--and Kingsley went through a period--of getting these people through their dissertations and out into the world. Which was a very strenuous period, and was, I felt, a very difficult way of going about things--let's put it that way.

Well, in any event, I was bid for by the Graduate School of Public Policy and went over there, which turned into sort of a five-year period which was a very happy one. There were lots of people I had known before and it was a very friendly environment and it was my first really friendly, supportive environment in academic life. I'd been going from one set of worries and crises to another. It really was a nice situation. I taught courses in population over there and helped them with their program in policy analysis and just had a period that was productive--a very pleasant period in my life.

Then around 1976, the School of Public Health here at UCLA got an endowment fund in population which was for a chair and they approached me. And it really didn't occur to me--I think I gave them six people's names, or something, and said these people would be good and that was it. I never thought about it.

VDT: It was a chair for population studies?

BLAKE: Yes, and I didn't even ask what that meant; it just never occurred to me to come down here.

Sometime in that period, Leo Reeder, who was a professor in the School of Public Health, called me. He subsequently was killed in that plane accident between San Diego and LA some years ago that killed everybody on board. He called me and said, "Why are you not interested in this job." And I said, "Gee, I don't know." I never felt I had to account for why I wasn't interested in the job; it never occurred to me to leave Berkeley. I said, "I'm very happy where I am now." I just hadn't thought of moving. So he said, "It's a very advantaged situation. The faculty position is paid for out of state funds and the chair is just available to you as a yearly income to do research if you wish. This is a nice situation here. You should at least look into it. Why are you so rigid about this?"

So I began to say to myself, "Why am I so rigid about this? It has a lot of advantages, being in the same system, because you're in the same retirement system, and it isn't that big a move." So I came down here and a great many of my northern California prejudices were dissipated quite rapidly. I liked it down here.

VDT: You mean the way of life or the university itself?

BLAKE: Well, the university I was very impressed by as being in some ways in social and biological sciences a much more dynamic kind of place, a more interesting place to me, than Berkeley. And I felt less political, which was very appealing to me.

And I found the Los Angeles area very attractive. Partly, I was a New Yorker and the proximity to this very interesting city, which was culturally sort of just bursting out all over, and the proximity to the open spaces, like the beach--40 miles of beach. I had had this mental image of its being wall-to-wall people and that's never true; it can be 100 degrees here and the beaches are never

wall-to-wall people. I just had a sense of its being an interesting place, being both urban and very cosmopolitan and culturally very interesting and a place where you could get to wide open spaces very rapidly, which is, strangely enough, true.

So I began to see it in a different light and we began negotiations. Part of what I think appealed to me is that they were interested in population, whereas where I was, although they were interested in me and were interested a little bit in population, it was really sort of a concession to having me in the School of Public Policy; population had never been one of their big things that they were interested in. And I felt here that they were.

So one thing led to another and, by that time, Kingsley and I were getting a divorce and it just seemed like a good idea to start a new existence someplace where you hadn't been all those years. So we negotiated a job here and I got a very substantial promotion and the chair and one thing and another.

I guess I realized that Berkeley since the closing of the department, however wonderful it was to be in Public Policy, was still a place that had had a lot of pain for me and that it was kind of nice to get away from that business. I'd been through an awful lot there, and some of it I had felt was very unfair and that I hadn't been treated very well. So I really wasn't sorry to get away from there. I realized after I left that it was a place that held painful memories for me and that when I got away I was feeling a lot better. [Laughter] I guess I hadn't realized that until I left, but it was almost as if something had stopped drilling and I suddenly realized, "There are other things and other places."

So it has been a very happy situation for me down here. We had our housing problems to begin with and, of course, I remarried.

VDT: Really! I didn't know that.

BLAKE: You didn't know that? Ages ago--yes.

VDT: To whom?

BLAKE: I'm married to a man called Leroy Gramer, who is the Director of Humanities, Sciences, and Social Sciences over in University Extension. We've always been on the same campus, fortunately.

VDT: You met here?

BLAKE: No, we had met at Berkeley. Then as I was negotiating this job and peregrinating up and down on Pacific Southwest Airlines, we remet, and by this time, Leroy was thinking of a new job. The man with whom Leroy had worked at Berkeley had left and gone to the Russell Sage Foundation and Leroy felt it was time for a change for him, so he was looking for jobs up and down the coast. We remet in that situation. That influenced the job he eventually took, of course, but that's another story.

We hit a pretty hairy housing situation, but we ended up doing very well in this, so that wasn't a catastrophic thing. People had said you'll never manage here and we had felt that to begin with, but then things worked out very well.

So I've always been happy here. I've always enjoyed being here and it's been a happy situation for me. I certainly never regretted leaving. This environment has been a congenial one. Again, I have colleagues with whom I've very close and happy.

VDT: There is a demography kernel here or are you it?

BLAKE: Oh, heavens no. There's a demography kernel in sociology and I have a joint appointment

with the department of sociology. There is one person in geography, the chair of the department, who is very interested in demography and does a lot of work. There are people in economics who have an interest.

VDT: Who are some of your colleagues here?

BLAKE: Well, Valerie Oppenheimer . . .

VDT: Of course! [Judith Blake's sister.]

BLAKE: And Ken Bailey and Georges Sabagh and David McFarland, so there was quite a nucleus over there, in the department of sociology. And there has been increasingly a methodological nucleus, which spills over; we attracted Dick Berk who had worked with Dudley Duncan at Santa Barbara and who came here a couple of years ago. And people at Rand [Corporation], a very large component at Rand [in Santa Monica]--Linda Waite, Peter Morrison, Kevin McCarthy, and Julie DaVanzo.

VDT: Do any of them ever teach here?

BLAKE: From time to time, yes. And then there's the University of Southern California group. We have a group that meets for dinner once a month and there's a lot of interaction.

So there hasn't been a feeling of isolation at all about this area. I'm chairman of the academic advisory board of the Rand Graduate School, so I have Rand connections. When you take it all together, there are really a large number of demographers down here, with fairly different interests, because the Rand group has been, with Peter, heavily into migration, and Kevin McCarthy has been involved in international migration, and Linda's been involved in fertility and female labor force participation; Julie DaVanzo has been involved in the economic analysis of fertility.

So there are just a lot of interesting people here. There is not a sense of, "There's nobody here to talk to." If we had the time to get together more, we'd have plenty to discuss. Then Dick Easterlin and Eileen Crimmins came to USC. The group at USC had quite a few people in it already--David Heer and Van Arsdol and Judy Treas. I've never felt I was out somewhere where nobody had even heard the word demography; it's been a very pleasant situation.

So that takes care of that saga. Let's see . . .

VDT: Could we go back a bit? You're marvelous; I just push a button and off you went--wonderful! You always could speak so well on your feet and you've got all your points there.

I'd just like to ask a bit about the department of demography--I know it was a painful time. You wrote an excellent overview of what such a department could be in the first issue of Demography, in 1964. I gather that was the protocol that you'd hoped for, the ideal program in demography: It doesn't need to be in the department of sociology, but it also needs biology, economics, geography, history, mathematics, and statistics. As Ron Lee said, having gotten his M.A. in it, it was a wonderful smorgasbord of things focused on demography. Do you think that it's really necessary to train demographers that way? Obviously, some others don't, because it hasn't happened.

BLAKE: It's happened more than you might think, because over the years, there have been these groups and they have incorporated maybe not that many different disciplines but usually about three. There's a group degree at Penn. And there has been an awful lot more interest in demography and really vital participation on the part of people in economics. So over the years, it has gotten much more taken for granted that that kind of training would be useful.

I think it would be a good idea to have that kind of fermentation, because the field I don't think has ever really seen its whole development, especially along biological lines and lines of that sort. It's been very heavily a social science field. I think it would be a very good thing for the field to have more input from, for example, people who I feel are very wrong in the way they think about things but nonetheless of great interest--the sociobiologists, who are out there doing their thing, which in some ways is very demographic. I went to a conference once that Alice Rossi had organized that had a heavy component of sociobiologists. I did a lot of reading in preparation for this and was fascinated at the overlap and realized that here was a whole group of people who really would benefit--and we would benefit--from interaction. Now, I don't agree with them, but that's irrelevant. What's relevant is that they're really thinking very demographically, and it's almost as if they don't know it.

I think the field of demography is very broad. I never agreed at all with Keyfitz's notion that it was just a kind of simple-minded mathematics or something--something that was pretty low-level. I have always felt that it was a field that impinged upon so many areas and that one could, if one could ever realize this in training, have people who brought a demographic perspective to a lot of other fields, which would make people realize what a vital and fascinating perspective this is. I think this may yet happen as time goes by, because we're getting a lot more demographers--a lot of them trained by Berkeley, I'll have to say. We trained an incredible number of people in the amount of time we were there.

VDT: Could you name some of them?

BLAKE: Well, there was Ron Lee and Harriet Presser, who got her actual degree in sociology, and Peter Uhlenberg and Bob Schoen and Don Hernandez, who's now at Census [Bureau], and Das Gupta, who's now at Census, and Jorge del Pinal, who's now at Census--who heads the ethnic division--and June Sklar, who died, and Geoff McNicoll, who's in Australia at Canberra, and Greg Spencer, who's now at Census. I don't know how many we have at Census; we must have about six or eight just there alone.

VDT: All of those were registered in the department of demography?

BLAKE: Yes--and a lot more. We had quite a few foreign students who were trained there. I never really counted, but we must have trained about 25 doctoral students there in that very brief period of time.

VDT: And these were demographers; the degree was in demography?

BLAKE: Yes. Now June, I think, and Harriet Presser got theirs in sociology, but the others were all demographers.

VDT: Richard Easterlin in our interview this morning said he felt that the primary degree of demographers should be in economics or sociology or one of these traditional things, because he felt you weren't taken seriously enough by--looking for an academic post, you had to be a sociologist or an economist or whatever and you would be able to teach population. What do you think of that?

BLAKE: I think as things stand right now that's true. How you would change that is the question I guess I would raise. I think one way is that you would start getting people who were from other disciplines taking these courses and training themselves, and then instituting courses in other departments, like psychology, biology, and so forth. This would really start a legitimization of thinking

of population as a field that belonged also in some of these other areas.

I think it's true right now that some of these social science departments have a lock on the field, but I think you could broaden the constituency very much and have people come at demography from these other vantage points. I just gave the example of sociobiology. That does mesh social science and biology. But unfortunately, in my opinion, the social science component of it is incredibly naive, so that they never really have gotten what they should in the social science and demographic training--they sort of glom onto ideas which are a little half-baked in these areas--and they've never had the basic training, thinking through some of the things that they should.

So what I would be interested in is trying to think of how one could bring to a lot of other perspectives what I think population training can do for them, and having this broaden out a great deal. That was always my interest. I never thought the status quo was any different from what they were saying; I was interested in how you changed that.

VDT: I think for applied demography this broad, across-the-fields training that you had in the department of demography would be the best--don't you agree?--applied demography such as in state and local government and in business, because that's perhaps providing a skill, not leading into research.

Even though you want demography to spread out, would you say that sociology is a good, if not the best, training for demographers? Of course, traditionally, American demographers have come out of sociology, although there are more and more economists. You wrote in your chapter called "Sociological Perspectives on Population Studies" in Schoen and Landman's 1982 book, Population: Theory and Policy, that training in sociology enables a population analyst "to make guesses or predictions that are more valid than might be the case in the absence of a sociological point of view."

BLAKE: I agree with this; my bias is very sociological. The only thing that I think you have to recognize to be realistic is that sociology as a field has so many components and the trend in sociology over the last few decades has been for it to fractionate more and more into many different things. It's very difficult from the institutional point of view to get any given sociology department to put a lot behind demography. What we're really talking about when we talk about whether you should train in a department of demography or in sociology or economics is not whether either of those disciplines is a good training as a basic training for demography. The fact of the matter is you're looking at departments that have so many claims on their faculty positions that it's very hard to get them to stabilize a component in this field. You can have periods when things are going very well and then, if you lose a few people, if someone isn't there, it's just very difficult for the younger people to keep that momentum going.

So I see whether a department is wanted or not wanted much more as almost a budgetary thing. It is much more saying, "Are you prepared; are you going to be able to keep this field going within this university, wherever it happens to be?" And if you don't have your own little bailiwick, you're competing for resources with all the new fads, whatever the latest fad is in those fields.

I see that as a major problem. It's not that I would ever argue that I don't think a social science is the best basic training; a social science and statistics and math training is, in my estimation, the best basic training. It's that it's very difficult to get sociology departments today to put a lot of importance on population. That's true here as well as a lot of other places. People just have so many different things that they're interested in in sociology--much more than there used to be. It's an incredible number of things that you probably haven't even thought of. Take something like ethno-methodology, which is a big thing here [at UCLA]. People want to keep a big component in ethno-methodology, so those faculty positions, if they open, you're competing with that. It's a very esoteric field, but it's got a foothold here. I'm not knocking it; I'm just saying that you've got all these people

that say, "No, we want that FTE for this."

VDT: FTE?

BLAKE: FTE is a faculty full-time position.

So I think this is the main problem, that you'll get one or two people, somebody like Dick Easterlin, who's in economics, and one other person here, somebody like me and a few people else, and that's going to be it. And if you want to talk of demography as a field, you have half of their time, because everybody's teaching something else as well. They're doubling up; they're teaching courses that aren't in their field in order to have coverage; doing undergraduate courses and graduate courses; they're really running. Because you can't get enough people in the field.

I never would argue with any of the academic reasons for putting a heavy component of social science training--that would always be my bias. The real problem that I see is that we're just not getting resources for population in universities. And if it isn't perceived as something separate, with people ready to fight for it, you've never going to get those resources. You're always going to be just about where we are now.

VDT: Do you feel that some of the training in demography, and the research that is going on, is getting a bit too mathematical--in economic approaches, very quantitative-oriented, and computer modeling and so on?

BLAKE: I don't know that that's the problem as much as that a lot of the people who have gone into it have come from a very quantitative background and are really not that much interested in the substantive problems of demography. Now, I think the substantive problems of demography are amenable to as quantitative an approach as one can make work in the field--as one can deal with in terms of measurement levels that we have and this sort of thing. It's not that I see the quantification as the problem; I see sort of playing games with the field.

VDT: Lacking the overall framework? You and Kingsley in your all-important 1956 article, "Social Structure and Fertility: An Analytic Framework" [Economic Development and Cultural Change, April 1956], which has influenced fertility research forever after, set out that framework, with intermediate variables. But you have criticized the World Fertility Survey as emphasizing too much the collection of data on the intermediate variables that were in that framework--marriage, breastfeeding, contraceptive use, and abortion [in review of World Fertility Survey Conference 1980: Record of Proceedings, Population and Development Review, March 1983). The same thing has happened somewhat with the National Survey of Family Growth. Of course, there was quite a bit in the National Survey of Family Growth on labor force participation, but not enough was being looked at as the social and cultural background, which determines the motivation for fertility and works through the intermediate variables to influence fertility. You said that your 1956 article had meant to stress that--getting the broad framework, the explanation.

BLAKE: Right.

VDT: And now you feel that in some cases demography is moving away from looking at the broad picture?

BLAKE: I think what's happened is that whenever you give people a framework and you're going to mount something like the WFS effort in which they're trying to standardize the data gathered among

huge numbers of countries and you're having to deal across a very large number of people, many of whom are fairly naive from a social science point of view, you are inevitably going to find that you get down to the least common denominator of gathering data on variables that may not be important. We said, "Here they are." They said, "That's fantastic; here are these variables, so you can just go out and crank this out this way with this template."

I think it's been fine that there have been data on those variables. But fundamentally what we ended up with with WFS was not really attention to--and it would have been impossible; I'm not saying that they could have, given the format--attention to what the social and cultural influences on those variables are. And that was the whole point. The whole point of the [1956] article was to say, "If you want to look at sociocultural influences on fertility, then you have to be aware of what they operate through and that these are the variables they operate through." I think nobody would ever have accused us of thinking that what we wanted to do was to just look at those variables.

So it ends up that something like that provides people with a very mechanical way of looking at things. It's unfortunate that that's happened. And yet a non-mechanical way, for all those countries with all that's going on, would not have been possible. So it's a valid criticism, but I'm not saying it's anything they could have done something about. I don't think they could have done it differently if they wanted to do something at that level. But it's a problem, because I don't think that survey has led to eye-popping theoretical results; we just have an enormous amount of data.

VDT: It generated the Contraceptive Prevalence Surveys and now the Demographic and Health Surveys, and that's a quick and dirty way to get data on fertility and contraceptive use.

BLAKE: Right.

VDT: How did you get the idea of using the Gallup Poll for collecting data on American attitudes relevant to fertility--the questions you added to those polls, beginning--when did you begin adding questions?

BLAKE: I think mid- to late 1960s.

VDT: Do you still use it?

BLAKE: No.

VDT: It went on for about 20 years, didn't it?

BLAKE: Not that long. It went on until I left Berkeley [1976]; I did some of it down here for a couple of years. It became incredibly expensive to do it. In the beginning, it had been a very economical way of gathering data and keeping a trend going on a lot of different variables, so it was really terrific for a while. They got all the background data and it was possible to ask these questions very inexpensively and get the advantage of all those background data. But as time went on, the questions became expensive to insert.

VDT: That was in a sense going back to your early work in Jamaica, asking attitude questions that you can't ask in a census or a Census Bureau Current Population Survey or whatever.

BLAKE: Yes, and it used a very big data set that was available and available for a long time span, and that's what I thought was so great about it.

VDT: Fascinating; every year on the edge of our seats, what would you come up with next?

BLAKE: Yes.

VDT: In your 1983 review of the World Fertility Survey in Population and Development Review, you laid out three things which I think must be your philosophy for research and writing. You criticized the WFS work and publications to that time as lacking what an "elitist voice" craves: a theoretical relevance and simplicity of conceptual framework, and that an elitist voice responds to lucid presentation of investigative problems, and admires papers honed to a state of deceptive artlessness. I presume that's been your philosophy?

BLAKE: Not a philosophy; it's been a goal--not always realized, by a long shot.

I've always been interested in what I felt to be major problems and not really that interested in just descriptive stuff. So it didn't thrill me with WFS that people were taking data for 20 countries on age at marriage and cranking them out and on this and on that; I don't find that a very interesting way to live. I guess it's a fairly elitist thing to say, but looking at some of those papers, I felt that a lot of young people's talents were not being used to the maximum in having to do that. And I wasn't even sure, although I don't know those data, that they were being used to their full advantage.

I felt always, in my own work, that I really wanted to deal with what I felt were some critical problems in the field. I don't know if I did, but I was interested in doing that. And that I wanted to present them clearly if I could, and in an interesting and very lively way, so that people would have access to them.

Also I always had as a goal that people should, I guess, have as much enthusiasm about population as a field as I did. So I wanted to almost shake them and say, "You've got to realize that this can be very interesting and exciting and you should realize this deals with things that are big things and big problems, not just little mathematical sums or something. I want you to see that this is something that can be interesting and that you can get steamed up about." So part of what I had as a goal myself always was that I would sort of try to reach out and grab people and have them suddenly realize, "This is a very interesting thing." And when you read it, you say, "Gee, I didn't realize this, that it related to this or that. I suddenly see this a little differently from the way I did, and it's not that dull after all." I felt so stunned by the notion that this was a very dull field, that people would keep saying, "Oh my Gawd, Dullsville--this is it!" So I always had this sense that what you did should reach out and try to make them realize that this isn't true and that they should get stimulated to be interested in it. This was always a sort of crusade, that I felt they should understand that this was just fascinating.

VDT: I think you have achieved that, because your writing is stimulating.

And you have worked on your own. You also said in your article on the WFS that "like so much demographic investigation of the past decade, it has been research by committee." And you, I have the impression, have avoided research by committee.

BLAKE: Yes.

VDT: In other words, you like to be a lone operator?

BLAKE: Well, I've worked with other people. I worked with Das Gupta for a while. It was a very good collaboration, because he was so mathematical.

VDT: I mean not at least one or two, but not any group project?

BLAKE: I've always found that maybe a little tiring. I've worked with doctoral students. But to work on collaborative projects, I don't know why I've felt that that way one got involved with people who might have standard definitions of situations that I wanted to break out of, and I would have found it very difficult to do that if I had hitched on to other people that much.

Part of what I found interesting about what I was doing was always to try to see things somewhat differently, and it's not necessarily so easy to drag other people along with that type of thing; you can spend a lot of energy on that. So I think that's been part of it, that I felt you should see things in a different cast if you're working on them.

VDT: Well, you certainly have.

BLAKE: Well, I don't know.

VDT: And you've written mainly articles. Was that deliberate--because they can get out faster and address current salient issues?

BLAKE: I have a book that just came out.

VDT: Oh, I didn't know that! It's Family Size and Achievement [University of California Press, 1989]. It's right on the topic of your PAA presidential address ["Family Size and the Quality of Children," published in Demography, November 1981]. Summing it all up. Good. So that's an important book.

That leads to my next question: Which of your publications do you consider most important and why?

BLAKE: I don't know. I think that's for other people to say.

VDT: Well, of course, "Social Structure and Fertility"--the framework. But what would you like to feel?

BLAKE: I guess some of the things that I enjoyed the most doing. I certainly enjoyed the most recent book a lot. And I guess I enjoyed the article I did for the Population Commission, which was "Coercive Pronatalism and American Population Policy" [in Charles F. Westoff and Robert Parke, Jr., eds., Commission on Population Growth and the American Future, Research Reports, Vol. 1, 1972]. And I enjoyed the consumer durables article ["Are Babies Consumer Durables?", Population Studies, 1968], which was the critique of Gary Becker.

Let's put it this way. When I'm working on them I'm always right in the thick of it and I think that there's nothing more important in life. And once they're finished, the fact of the matter is I rarely read them over again; it's over and done. So it's kind of hard for me to ever even think of them anymore as mine--I'm into what I'm doing right at the moment. It comes as a surprise to me sometimes--if I assign them to a class, I have to read them over--I say, "Did I say that?" So I don't dwell on them very much.

VDT: Even when . . . For instance, in your article on "The Supreme Court's Abortion Decisions and Public Opinion in the U.S.," that appeared in a 1977 Population and Development Review, you pointed out that in response to a question you'd put on the Gallup Poll in 1976, 46 percent of respondents then

avored a constitutional amendment to outlaw abortion, to revoke Roe v. Wade, which had been passed three years previously, and you foresaw a battle to implement Roe v. Wade. Well, that certainly happened. You must think back on that--though 12 years ago.

BLAKE: I have to admit I think back on that. And I more than anything think back on my rationale for that. I really had big fights with the Planned Parenthood people about that. My feeling was that they should quit doing things that made it worthwhile for the pro-life movement to really get under way. It's a little bit like, you know, OPEC has always tried to gear the price of oil to a level that would make it uneconomical for us to develop other fuels. They figured this out very carefully; they have never wanted us to feel that that was worth our while, so they always come in right under that figure, whatever that figure is.

I was really trying to say, "Look, you guys, you've won, and the basic thing is in place. Now don't do things that make it worthwhile to have a pro-life movement. Don't be so inflammatory. Don't insist that government money be used for abortion. Don't do the things that are going to create a really hard-core pro-life movement, because you're going to lose the ball game. Don't think that just because you won now, you're going to stay this way." And I would argue with Fred Jaffe [of the Alan Guttmacher Institute] about this, Jeannie Rosoff, with everybody, and try to say to them, "This is going to be a disaster, because you are giving these people a reason to have a strong movement. What you want to do is come in short of a reason, so that it's hardly worth anybody's while to make this big a fuss. If all you're saying is people should have access to abortion, and you're not using taxpayers' money to have abortions, you're not being basically offensive to anybody in the population, because you're not taking money from Catholics or fundamentalist Protestants for abortions; you're just saying people should have access to it. And you solve the money problem another way, somehow or other, but don't push it so hard." And they just couldn't see it that way.

VDT: Well, they should have listened to you! [Laughter]

BLAKE: I suppose so. I quit working on it, because I saw such disaster ahead and I didn't want to predict it anymore. I didn't want to be the person who was saying disaster is coming down the pike, because my pro-abortion predilection was obvious. I felt so overwhelmed by what I saw coming.

VDT: But you did see it, in that case. Here's another one I wanted to catch you up on. It's not quite fair, but anyway. Back in the 1965 book of Mindel Sheps and Jeanne Ridley [Public Health and Population Change], you wrote in your chapter ["Demographic Science and the Redirection of Public Policy"] that because of deep-seated familistic norms in Europe--and you'd already claimed that there were these norms in the U.S. too--"it seems unlikely that Europeans are on the road to permanent adjustment of family size at approximately two children." Now, what about that?

I have to admit that the latest [1989] Population Reference Bureau World Population Data Sheet originally had Sweden's fertility rate up to 2.0, but Carl Haub told me at the last moment, "Don't tell anybody out on the West coast, but I think I'm going to lower it to 1.9." Anyway, it's gone up. [Sweden's total fertility rate was 2.0 in the published 1990 Data Sheet.] West Germany has gone up slightly to 1.4. As you know, Italy is now lowest at 1.3. Do you still feel Europeans will not settle at two--in other words, you were assuming [in 1965] they'd go higher?

BLAKE: Mainly on the basis of the way social trends were at that time, which was not for there to be very, very high levels of female labor force participation. A lot of this was based on the notion that family structure would stay the way it was, and I'm not apologizing for having some clouded crystal balls.

VDT: Okay. Of course, it went up to nearly half of all couples are cohabiting.

BLAKE: Right. And not only that, but a very high level of female labor force participation. Very tight labor market there, so it became a viable solution for them, in addition to all the immigration and everything else. It was really more based on the notion that if they're not going to have these women go into the labor force, they're not going to shake up this family structure in some way, then this is what one sees for them. But, indeed, they did.

VDT: That also leads to my next question, about U.S. women and their labor force participation. In your September 1974 Scientific American article ["The Changing Status of Women in Developed Countries"], that great issue ["The Human Population"], you doubted that more than a small minority of American women would seek to be high achievers in the work world, because then you still felt that there were these underlying pronatalist constraints--I remember thinking, "She's right," when I read that article. High-achieving women who would seek a status not derived from husbands would risk not marrying and having children, which your research showed most women wanted. Have you changed your views on that?

BLAKE: Well, you know, not that high a proportion of women are high achievers even today. I think in this respect, we haven't really seen the whole picture yet. In a way, I've always felt that people sort of tinker with their fertility behavior and that they look to see what a previous generation has done and in a sense try to learn from that. And women are watching each other very carefully. I think young women are watching how contemporary women are doing things and asking whether this is the route they want to go. And I'm not sure myself that we're not just going to see some reaction against this extremely low fertility. That there's not going to be some generational reaction which in a sense says, "I've seen how those guys did it and I'm not sure that having that few children or one child or risking having no children, even"--which in a sense is what is happening with young women--"is the way I want to go."

So I'm not myself quite sure that things have changed that much for the majority of women, that young women are going to see razzle-dazzle careers for most women as an alternative to the family, because I don't think that's what one sees. I'm just not sure that young women who are really watching the situation could not opt to have two. I don't think we'll have a baby boom situation. But I think this period we're in could be something of an anomaly--not a huge anomaly, but something of an anomaly. Because you talk to a lot of younger women and in part what is influencing them is that the conditions for having children are so difficult. It's not that they don't want them, it's that they're saying, "My God, you can't get a house." When you start having children is when the sex roles start getting very rigid. You can share the dishes and the housework and the vacuuming, but once you have children somehow you get shunted into a situation where you're really kind of back to the 1950s. No matter what anybody says they're going to do, they're not going to do it, because they're going to be the chief wage-earner, by God, and somebody has got to make the money in the family. So the logic all goes in the direction of your getting into a much more rigid female role. And this is one of the things that really bothers them. We've moved close to an anti-natalist environment now, in which it's very penalizing to young women to think of themselves as getting involved with more than one kid or a couple of kids.

So I'm not sure that the score is in yet on this. As I say, I wouldn't predict a baby boom, but I'm wondering whether we're not going to see . . .

VDT: Well, the U.S. TFR now rounds off to 1.9, which is more than it's been in the last few years.

That's more than it's been since 1973 or 74. It will be interesting to see what happens.

You've written quite often of the only child, another issue, and the issues facing high-achieving professional women. I suppose that reflects your own experience--you have an only child?

BLAKE: Let me say this: This interest in the influence of family size on achievement was actually motivated much more by an interest in the effects of high fertility. And other people have just been so interested in the only child that this is all that I can ever somehow get anybody to ask the questions about. I did have one child, but in a sense I brought two children up.

VDT: Yes, your stepson [Kingsley Davis's son by previous marriage].

BLAKE: Yes. He lived with us during virtually all of my daughter's childhood and young adulthood, so I never thought of her really as an only child, because he was a very material brother to her and it wasn't the same as her really being an only child.

But my interest in this, which I think is important, is much more in trying to say, "Can one look on a disaggregated level, on an individual level, at the effects of high fertility?" Because if one always looks at the aggregated level--the sort of Julian-Simon level--one can say almost anything. And I really felt it was very important to gather data on what at the individual level high fertility does or does not do. I found in this book [Family Size and Achievement], using many more data sets than were in the Demography article [1981 PAA presidential address], that it really has very deleterious consequences--for IQ, for education, for a lot of things. It's very hard to find any good consequences of high fertility.

Then when somebody is saying, "Oh, rapid population growth is just terrific," you have to say, "Terrific for what?" If it's terrific for individuals, we don't find that. It has to be super-terrific at a macro level to compensate people for this suffering at an individual level.

VDT: You extend the field to less developed countries in the book?

BLAKE: No, only U.S. data. But it's on kids--a lot more on kids--and a lot more studies developed. And they are really sort of unbelievable in the sense that you never find good things coming from high fertility. You never find people from very large families doing well or better than people from the next size down or something; it's always bad news.

So what I was really interested in was saying, how can we start gathering data that are going to raise a question with this Julian-Simon-like argument, which is to say, "Well, if it isn't good for people, for individuals, what kind of an argument can you make that it's so terrific for society as a whole?"

VDT: Great! That's important to know. I'll be interested to see what you think of that recent Atlantic Monthly article I mentioned in which the author [Bernstein: "IQ and Falling Birthrates"] cites a 1984 article by James Flynn which spoke of a massive gain in the mean IQ of Americans from 1932 to 1978. This author, Bernstein, claims that IQs have actually been dropping and that it could be because high-status American women are leaving reproduction to the low-status, lower-IQ women.

What accomplishments in your career have given you the most satisfaction?

BLAKE: Oh, gosh, I don't know. I've gotten a lot of satisfaction out of teaching. I've greatly enjoyed teaching.

VDT: Everybody I know who's studied with you thinks you're a dynamo. That was one expression that John Weeks used when we were talking about you at the recent PAA meeting. Alex von Cube, a

good friend of mine at the Population Reference Bureau, said your classes were so stimulating, that occasionally there was controversy, lots of controversy. He said you could be hard on people, and sometimes you were a bit conservative, some of your students felt. You say you have gotten a lot of satisfaction out of teaching?

BLAKE: I've never liked teaching huge amounts, because I like to prepare a lot, and I like to use teaching as a way of picking up on issues that I wouldn't ordinarily be starting to write about or do research on and thinking them through. So I've always used it as that kind of a vehicle, for getting into something that I was just starting to think about. It gave me an opportunity to do a lot of reading, a lot of thinking. It has fed into research--not on a day-to-day basis but more to say, "This is a way of beginning to think about something and get input from people and have them criticize you and get a lot of stuff going on something." It's been very creative for me for this reason, and I've enjoyed it enormously.

Interestingly enough, over the years I've seemed far less conservative to people, partly because they have gotten more conservative, I think. So that is rarely mentioned at this point in time, because people have changed their own views about a lot of things. That helped--not that I changed them, but the world has changed.

VDT: Indeed. Have you found it an advantage to be a woman in your career? I think probably your enthusiasm--well, frankly, most women teachers I've known have been more enthusiastic than the men teachers.

BLAKE: I wouldn't say that when I was coming along it was an advantage. It was a big disadvantage, I think; there's no question about that. Nobody thought of you as being anybody and nobody ever thought of you for anything. When I looked and saw how young men were sort of mentored and coddled and taken under people's wings and pushed and so forth, I didn't feel that during my youth it was an advantage to be a woman at all, in academic life.

On the other hand, I have tried, I think, to make it an advantage for other women. I've felt very strongly about this. I'm not a rabid feminist in the sense that I've joined groups and things of this sort. But I have been very instrumental in pushing a lot of women along. And increasingly so, as opportunities are arising, I think I have helped a lot of people to get good jobs.

VDT: Among your graduate students?

BLAKE: Yes. And helped them over life-cycle events, like pregnancy, for example, which I think many males have always felt was, "She got pregnant; that's the end of that." I have been very supportive of people who have been in this situation and made them feel that there's no reason at all why they can't continue, through their pregnancy, getting their dissertation done.

VDT: It might have come out of your own experience. You had your daughter, presumably, finishing up your Ph.D. dissertation?

BLAKE: Yes. Well, being pregnant, they just sort of relegated you to the boondocks in those days. People's attitudes toward all of these things were very rigid.

No, I can't say that it was an advantage. I think really in my youth, it was a big disadvantage and it would have been a lot easier not to be a woman. But, on the other hand, it's a good character-builder. [Laughter]

VDT [talking about Richard Easterlin, interviewed that morning]: Easterlin comes into his office [at

USC] only Tuesdays and Thursdays.

BLAKE: He may just know how to live!

VDT: Well, this year he and Eileen are both on sabbatical. They have the two small children that have to be gotten to two different places each day, one to school, second grade, and one to a day care center. That makes life more complicated.

BLAKE: It really does.

VDT: You and I are past that stage!

What about some of the questions that I asked you in advance? What have you got there?

BLAKE: When I got your letter, I jotted these things down, quite a while ago. You asked me what I felt were some leading issues in U.S. demography. I think there would be a consensus that one is how we're going to handle the volume of immigration that we're experiencing, both legal and illegal, and the resulting fertility of these immigrants. And in a sense, I guess I would regard as a very high priority how we're going to upgrade them and be sure that we're not taking on a permanent underclass in the society, which I feel strongly about. It would be distressing to me--although I won't see it in my lifetime--to feel that we are moving in the direction of taking in people who are going to be low-level workers generation after generation and that we're not going to be able somehow to move them in the direction of education and better jobs.

The fact that they're not doing super-well right now compared to native Americans is not so significant. They're certainly doing a lot better, obviously, than they would be in their own countries, so you can justify that as a plus. But I wouldn't see that as the criterion of relevance for very long, and I would find it disturbing to think that what we're really doing in taking a mammoth number of people in--I guess we don't have too much choice--is that we're creating a group of people who are just going to be in ethnic ghettos, not speaking English and not able to move--and not have their kids able--to move ahead or have opportunities in the social structure. I see this as a major problem that we're faced with in this country and don't see the end in sight very soon.

VDT: Have you done any work on that?

BLAKE: No, I haven't; I probably never will. But I certainly feel strongly about it. I feel it's a baleful development, if what we're doing is not going to be able to offer them the opportunity that other immigrants have had in this society.

I'm also very concerned about the effect they're having on black opportunity. It's not only the immigrants' fault; there's a whole restructuring of the economy that is affecting black opportunities. But I think we're in a period about which we should be very concerned in terms of the legitimacy of occupational stratification in this country and how things are moving. As I say, I don't get too upset because people who come to this country from very poor countries are now not suddenly doing super. That's never happened; that's an unreasonable expectation and you can say, "Well, they're probably doing pretty super compared to where they came from." But I wouldn't consider that an answer for the second generation, and I would find that a very disturbing outcome if that is going to happen. So I think this is really a big problem.

I think we also have a problem with how to manage the enormous urban densities that we're accumulating and which I think we're going to continue to accumulate if, as Larry Long and others tell us, the metro-nonmetro turnaround has turned around and we're back to metro growth again.

Somehow, unless we can find mechanisms for dealing with the environmental problems, we're going to have some pretty crappy cities to worry about. And living here hasn't changed my views.

VDT: Absolutely--the smog and, alas, the freeways.

BLAKE: Right. I think we also have a problem of positioning ourselves with respect to the rest of the world, certainly the developing world, and having them perceive the high fertility as their problem and sort of not our mandate. I think this is a very important thing.

VDT: Not our . . . ?

BLAKE: That if they have an average of eight children, that somehow we are the ones who see this as a problem. I think we somehow have to position ourselves so that becomes their internalized problem about which they're interested in doing something. When you see what's happening in Africa and in some North African societies, it's a very distressing thing. And the fact that they're not taking hold vigorously in many cases is still a distressing problem which hasn't gone away.

We certainly have the issue of how to deal successfully with an aging population. I myself--this is my own personal predilection--don't think people should be retiring this soon. I think they're very, very ill-advised to do so--economically, psychologically, and all kinds of different ways.

VDT: At 65?

BLAKE: Yes--or earlier. We keep saying if you make retirement at age 70, you're never going to get people to retire. That's ridiculous; they're retiring earlier all the time, so it doesn't make any difference how high you set the retirement age. I feel this is a major problem that we have to concern ourselves with.

We've got plenty to worry about in demography; I don't see these problems disappearing. I think we have a lot of interesting things to do research on and a lot of interesting young people who are coming along to do that work.

VDT: That was going to be one of my final questions, what you see as the outlook for demographers in the U.S. You say there are young people coming along and there are still places for them in research and training. What I really wanted to ask was: Do you think all the jobs now are in applied demography--business and state and local government?

BLAKE: Heavens, no! If you want an academic department to recruit somebody, you have to fight to get them. And I think this is going to be true more so in the 1990s, when the demography of organizations is such that there's going to be a whole cohort of people, myself included, who will be retiring as the decade wears on. They're almost all going to drop off a cliff like lemmings, because they all got hired in this same period of the 1960s, when they came off the Ph.D. treadmill. All universities are just scared to death that they're going to hit the 1990s and they'll have a terrible time recruiting. Universities have a terrible time recruiting now, because--fortunately, in my opinion--there are so many other alternatives for young people. I'm just delighted. I thought it was terrible when all you could do as a demographer was to get an academic job, or a couple of jobs in places in Washington that hired people. I'm delighted that this is a brisk market today.

Now, one of the things that gives you a hard time recruiting, of course, is the dual-career problem. Every time we get a woman who is all set to come, we can't find a job for her husband, and vice versa. So this is the other feature of it.

But I would say there's going to be a terrific market. But there are some terrific young people coming along. My feeling is you look at this new crop of younger people and sort of mid-level people and it is a very encouraging group of people. I always am thrilled to see the work that's being done. There are just some really interesting people. So I think we're coming right along. But then, I'm always the optimist.

VDT: It's wonderful that you are. Let's turn to PAA. Do you remember the first meeting you attended?

BLAKE: I certainly do.

VDT: What was that?

BLAKE: It was down in Virginia, in . . .

VDT: University of Virginia, Charlottesville. That was 1954.

BLAKE: Yes.

VDT: It was the last time they took a photo of all the participants at the meeting, lined up, maybe 150, 200 people. Dan Price gave the archives a copy of that photo. It was on the campus. That must have been a nice one.

BLAKE: Oh, it was lovely; it was wonderful; I thought it was heaven!

VDT: Do you remember the early meetings you attended, the atmosphere, the luminaries? It must have been wonderful, because there were Irene Taeuber, Margaret Hagood . . .

BLAKE: Oh, I know.

VDT: And Dorothy Thomas, of course, and all the people who were great names then and became even more so.

BLAKE: I used to go very regularly. And the one at Princeton [1955] was the one where we read our initial "Social Structure and Fertility" paper. [Kingsley Davis and Judith Blake, "Recent research and the theory of fertility in underdeveloped areas," presented in a session on "Foreign Studies."]

VDT: That was the last one at Princeton.

BLAKE: Yes, after that it was not big enough.

VDT: That must have been exciting. Was there a reaction to your paper--aware that they were hearing something historic?

BLAKE: There was a big reaction all right. Some of it was very controversial, as usual. But when the paper finally came out, it caught on very rapidly, that whole idea, because it solved a very basic problem of how to look at things. It wasn't controversial after that, certainly.

I remember an awful lot of these meetings, looking at the list, because they've all had

something special about them. But for me, it was a great thing to be able to go to the meetings--I was still so goggle-eyed--and listen to the greats, just be around. It was a very satisfying experience.

VDT: Everyone has said that it was very easy to approach everyone; they were convivial and the high-and-mighties talked to the not-yet-so-high-and-mighties.

BLAKE: Yes, and they were very welcoming. You would be introduced to people that, you know, compared to them you were a kid, nobody, and you were always welcomed. They were always very happy that you were in the field. Now, from the vantage point of many years later, I can understand this, because it didn't have many recruits, and every time they saw somebody, they were so happy that at least there were some young people who were coming into demography. At that time, you just felt very much as if this was a good place to be and that these were not people who were unapproachable, as you say. It was a very pleasant experience always; I really used to love that.

VDT: You were only the fifth woman president in PAA's history.

BLAKE: Is that true?

VDT: After 50 years.

BLAKE: Awesome! I've never counted up.

VDT: Here's the list. There you are: No. 5 woman, 1981. There was a long gap of nearly 20 years between Dorothy Thomas in 1958-59 and Evelyn Kitagawa in 1977. What do you think accounts for that? There were the early women superstars. You probably read Anne Lee's nice vignette in PAA Affairs last fall [Fall 1988] on "Early Women Superstars in PAA"--Dorothy Thomas, Irene Taeuber, Margaret Hagood, and Hope Eldridge. You mentioned having had your first demography course ever from Hope Eldridge.

What are women's positions in the PAA--and in demography? You've already said it was a great disadvantage being a woman--not in PAA.

BLAKE: No, I felt that actually it was a very welcoming situation in PAA. I think what happened, perhaps, was that there was just a big gap in the population at risk, you might say. We went through the baby boom and one thing and another and there just weren't that many women who were the Irene Taeubers and Evelyn Kitagawas. So I think that if you were to look at the profession, you wouldn't have seen that many very distinguished women who were available to be a president. This might have been part of the reason for the gap.

VDT: That sounds plausible. Were you involved or interested in the issues raised in the early 1970s by the Women's Caucus--more equal rights for women in the profession, at the universities? That was, of course, the time of turmoil at Berkeley and at other universities. But the women's institutions in particular?

BLAKE: Well, as I mentioned, I have never been very political, in part because I thought when you got involved with groups that were very political, you never knew which way they were going. You never saw eye-to-eye with them completely, and when they were very radical groups, you weren't sure you were going to feel comfortable for very long in that situation. It's one thing to be involved with a big political party and to say, "Well, it doesn't represent everything I believe in, but I can live with it,

because it's never going to do anything much one way or the other." But when you get in with very radical groups who have very strong feelings about a spectrum of things that you may not agree with at all, I think you feel somewhat uncomfortable--at least I always did--throwing your lot in, because you never know which way the cat's going to jump and you see you can't control that very well.

And you have to realize, I was rearing a family and running a household and had a very, very busy husband, and I was just barely hanging in there. So I wasn't exactly a candidate for an awful lot of extra activity. I didn't have very much time. **There** were years and years when I had no relaxation at all. Life was pretty hairy; I wasn't exactly looking for places to sink my time. That was part of it.

VDT: There was one set of issues in PAA that you were involved in, in the mid-1960s, when you were asked to be on the Committee on Organizational Management . . .

BLAKE: Yes.

VDT: Which was originally set up by Paul Glick [PAA president, 1966-67] and Andy Lunde [secretary-treasurer, 1965-68] because the membership had tripled with Don Bogue's recruitment through Demography and there was just too much work for a non-paid secretary. You were supposed to decide whether or not there should be a paid business manager. But then the committee came up with a report that recommended several other things, which many people took issue with: that there should be an active membership recruitment drive and two classes of membership, formulating professional standards--somewhat like the American Sociological Association has done recently, apparently to everybody's horror--and a small grants program to fund small research projects. Those ideas were shot down by Ansley Coale, who was the incoming president and said he wouldn't put them into effect, and by the membership. Do you recall what your feelings were at the time?

BLAKE: I just sort of felt that the field should be more aggressive about recruitment and about its own position, and should have a little opportunity to do something like funding people out of the organization. I felt very much, as I grew older in the Association, that some of the charter members, or post-charter members, were very proprietary about it. I felt that Ansley was pretty proprietary and had a pretty rigid notion of what the Association should be: it would be what it had always been, in his experience. And I didn't see that that was necessarily true. I thought it should respond to its current problems and try to do something about them.

I didn't feel super strongly about all of this. It was an interesting committee; it brought together people who really didn't agree much on a lot of other things and they agreed on this. Parker Mauldin was on it. [Chair: Forrest Linder; members: Judith Blake, Parker Mauldin, J. Mayone Stycos, Conrad Taeuber; ex officio members: Paul Glick and Anders Lunde.] There was a feeling on the part of a lot of these people that we needed to change some things. I don't think anybody saw it as revolutionary. But I think that Ansley felt that anything that changed the status quo couldn't help but not be in his interest, or in the interest of OPR [Office of Population Research, Princeton], and that things were going so swimmingly for those guys that anything you tried to change was ipso facto going to be somewhat suspect. That was usually his position, that, "I like things the way they are." And he liked them the way they were because they were very much in his interest. I appreciated that; I saw what the situation was. But on the other hand, I felt some other people's interests should be taken into account more.

I think this was a big crisis for PAA, but it weathered it very successfully. It moved from being a kind of sandbox for a very limited group of people, who were benevolent--I'd always felt that they were very benevolent and meant well--but they came to feel, as the field burgeoned out, that things were getting a little out of hand and they lost control. And they did--in a way. But it wasn't a

revolution; it was just sort of gradual slippage.

VDT: I'm so glad you said that, because that is a little different from any angle that anyone's put on it. I've talked to maybe too many of the old guard, or they weren't involved at that time--Sam Preston, Jane Menken--they were not interested. But you're just the right group to approach. Thank you for that.

BLAKE: And I don't say that with any resentment.

VDT: No, it's just a slightly different view of it, which I hadn't thought about myself.

BLAKE: Well, you know how you feel when you start seeing change in things that you're comfy with. You just looked at it always as a place you understood and there were going to be no unpleasant surprises, or even pleasant ones; everything was going to be just itself. I think that's the way a cohort of those people experienced it, that it was just a nice comfy spot to land. And to have all those people come along and start making changes--I think that was really tough. If you give in on this one, what's going to happen next year?

VDT: Good point. Let's skip to the 1981 PAA meeting, which was the important 50th anniversary meeting, when you were president. How did you come to choose the topic of your address: "Family Size and the Quality of Children"? It's true it was growing out of your research, did you see it as a sort of summing up of a trend of research? No, you said you were embarking on . . .

BLAKE: Yes, it was really the beginning. I had been thinking about this for a long time. Some people gave presidential addresses which were very appealing and fun. But I felt that if it was the 50th anniversary, it shouldn't be something that was just topical or light-hearted. I felt it should be something that was serious, or was research, or whatever. And I had been thinking about this for a long time, in part because it stemmed from all the arguments--with the Julian Simon argument--about population growth. I had been thinking how one could zero in on this in a way that would be more definitive.

I started working on this material and realized that it could be quite interesting. I thought, "Well, I am not going to stop this now in mid-stream and start on something else, and what would the something else be?" I didn't want to talk about abortion, because I didn't want it to be controversial that way; I didn't want it to be inflammatory. So this was the logical thing. It was also practical: I was working on it and I just couldn't switch horses at that point.

I said, "Well, why not? I'll start in and do my first article on this. It's not going to offend people, I don't think." And it didn't, I don't think. I didn't want a presidential address that was going to be inflammatory or offensive to people; I felt I had an obligation to have it just be a scholarly paper. People might disagree, but they weren't going to feel out of joint or anything.

VDT: It was a very solid talk. As you say, some presidents have felt they should choose something topical. Ansley Coale did in 1968; he spoke on "Should the United States Start a Campaign for Fewer Births?"

BLAKE: Right.

VDT: Of course, he pointed out that birth rates were going down anyway and no campaign was needed. But he had decided he should choose a topical issue rather than his research, though others

have indeed done their research. Three years before you, in 1978, there was Dick Easterlin's address for which he became famous, "What Will 1984 Be Like?", on the Easterlin hypothesis. Which led to his book. And yours led to a book too!

BLAKE: Did that lead to his book?

VDT: Yes--Birth and Fortune. He said he had put things in the address, such as the impact of cohort size on homicides, suicides, SAT scores, and so on, that he hadn't done before, and he realized that should become a book. And yours did too--but you were heading toward that book, Family Size and Achievement.

BLAKE: I really didn't know that then. As I say, it was something I was working on at the time and I didn't have time to just gin up something else for PAA.

VDT: In it there was a nice piece on the only child, again--as you said, that's what people always want. Was that what you were being interviewed on by Cable News Network on that Saturday afternoon after the meeting?

BLAKE: I don't remember.

VDT: We both can't help recall that. Cynthia Green and I, who were running this press office for the PAA meeting, got you on Cable News Network on that Saturday afternoon, and then less than 48 hours later, just within feet of where we'd been sitting [in the Washington Hilton Hotel], there was the assassination attempt on Reagan.

BLAKE: Yes, it was really kind of scary, wasn't it?

VDT: Among other special things at that meeting, there was the "PAA at Age 50" session, which Andy Lunde and Con Taeuber organized, with papers by Lunde and Frank Notestein and Frank Lorimer--I'm sure it was the last time they spoke at a PAA meeting--and Clyde Kiser's paper, which was read by Dudley Kirk. I always regret so much we did not get a photo of that panel. Con Taeuber did do a tape and we've got that in the archives, but we don't have a photo. It should have been there.

Then there was the dance, with the piano music by Joe Stycos. Did you get him--old friend and research partner?

BLAKE: Yes.

VDT: And Lee Bouvier had his trumpet. They'd played together before. And that was the first and only time we've had a press office at a PAA meeting.

BLAKE: Not since?

VDT: Not a press office like that [but there was one, run by Mary Kent, at the 1991 meeting]. This year [1988] at Baltimore there was a session, chaired by Wendy Baldwin, on should demography reach out more to the press ["Research, Policy, and the Press: A Case Study"], at which Alan Otten of The Wall Street Journal was the press representative. He did write up several of the meeting papers. It appeared in his regular column on the front page of the second section of the Wall Street Journal--one of them was Richard Easterlin--but he never mentioned where he'd heard those papers.

BLAKE: Oh, really--just somewhere?

VDT: Right. Were you responsible for the press office idea?

BLAKE: I can't remember. I have to tell you I had an incredible committee here, who worked like dogs on that meeting. Peter Morrison, as usual, who has for years done very good things for the PAA, worked very hard. We had about six or eight people who worked very hard on putting that meeting together with me and were just incredible.

VDT: Some presidents have ended up almost doing it themselves.

BLAKE: People were so helpful. I think Bill Butz was involved and Peter and Julie DaVanzo--a lot of the Rand people--and David McFarland was wonderful. We had sociology people, Rand people, and I think we may have had some USC people, and Jorge del Pinal from Public Health, and me. We had a great group of people who took hold on that, because we were doing an awful lot of things that required fine-tuned efforts, like these special programs for some of the older people.

VDT: Dudley Duncan said he was there. Did you ask him to chair a roundtable then?

BLAKE: I think so.

VDT: The last meeting he ever attended, and he hadn't attended many for years till then.

BLAKE: We wanted to have this be a real 50th anniversary, with people who younger people might not see again in a long time for a variety of reasons. So we felt that there should be very special things, and things that made some of the older members of the Association feel very special. It was sort of our way of saying thank you to them and making them feel that we appreciated all the things that they've done over the years. So there was a lot of devotion involved in it, a lot of sense of people who we wanted to bring out and have appear and have younger members see them and be sure that they had their day still. There was a lot of very overt thinking on this, that we wanted to be sure that this was that kind of a meeting, that it wasn't only a scientific meeting. It was kind of a recognition of our forebears in the field. There was a lot of enthusiasm here for doing it. It brought people here together and the committee was full of beans about this and very helpful.

But it's an awful job, because, fundamentally, as you get down to the wire, you get to be the central nervous system for it. You can never delegate completely; ultimately, it's all got to come together. And as you get down to that point, it's murder--it really is.

VDT: It also must not have been easy being on the West coast, with the meeting in Washington.

BLAKE: No.

VDT: Well, it was a spectacular success.

BLAKE: Well, it was a fun thing, yes.

VDT: Do you regret the changes there have been in PAA over the years? The meetings are now up to 84 sessions, eight overlapping [90 in 1990, 91 in 1991, nine overlapping], sessions on Saturday afternoon, close to 1,200 people at Baltimore this year, which is the record [1,399 at 1991 meeting in

Washington], and many splitoff workshops--business, economic demographers, although the economists didn't meet separately this year--there were several workshops on China this year. We even had workshops Wednesday afternoon and evening, before the beer party.

BLAKE: No, of course not. That was never my philosophy. I'm just delighted that they're interested in having all these things go on. I feel that they should feel they can change if they want to, that nobody's saying PAA's got to look like it did 50 years ago. It's their association now, the young people's association, and I think they should be doing what they think it's worthwhile to do. I would never want to be a person who said, "I don't like the way this is going." Because I think they should try those things and have the freedom to do that, and not have people in the older generation sitting on top of them saying, "I don't like the way this went this year," or, "I don't think it's convenient to have it that way."

I think it's great that there is enough good stuff to be in eight overlapping sessions. Not everything's great, but there's a lot of good stuff coming out of this field today, a lot of good research, and I'm feeling very positive toward that. And if they don't change things the way I'd like them, I won't feel I'm losing something. [Laughter] I think that they're taking things in hand and doing them and feeling that they can. I'm very encouraged by that.

VDT: Well, you're optimistic.

BLAKE: Oh, really?

VDT: Quite a lot of the oldtimers regret the proliferation in sessions and numbers of people.

A final question, about your sister Valerie [Oppenheimer]. How come the two of you chose demography? And, incidentally, I see that she was born in England; how did that come about?

BLAKE: Well, of course, I'm older.

BLAKE: Yes, you were born in New York, but you were both raised there, weren't you?

BLAKE: Yes. Well, I spent quite a bit of time in England as a child--when she was born in England. I don't know how she came to choose it, really. I think she was surrounded by it, you might say, and found it very interesting.

VDT: You were in the field by the time she graduated?

BLAKE: Oh, yes. I'm six and a half years older than she is, so I was sort of well along by that time. And this was what everybody was talking about, so she kind of got into it too.

VDT: Ron Lee said that she was at Vassar and she babysat him--although he wasn't into demography at that time!

BLAKE: It was really funny. His mother was an anthropologist, a very distinguished anthropologist--Dorothy Lee.

VDT: He said both his parents were professors, but I didn't realize who she was. And his father was a mathematician?

BLAKE: I think that's what he was. His mother had been a widow for quite a while, and she reared all those kids as well. I'm not sure, but my impression was that she had quite a few children; she was quite a woman. It was a very, very smart family.

Valerie was born in England. We lived in England for four or five years and my sister was born there.

VDT: Isn't it interesting that you both did turn up in the same field, and now, same university, same department?

BLAKE: I know. One reason I'm not more active in the department of sociology is that I have felt that that is basically her department, and I feel somewhat reluctant to get very involved. I don't get very involved on something unless she asks me to. Occasionally, I will do something that's neutral, like being on a recruitment committee or something, that the chair asks me to do. And I do help her students to do things. And we're personally close. I think part of this has to do with not making somebody feel crowded, as I think she would justifiably feel if I were in there with both feet all the time. So I've tried to be sensitive to that, because I do have a place down here [at the School of Public Health].

VDT: Well, it's an interesting combination; it probably exists in few other fields.

BLAKE: I know--unique. At least my child didn't do that.

VDT: I understand your daughter is a physician.

BLAKE: No, she's a cell biologist. She got her doctorate at Rockefeller University and she's now got a post-doc at Whitehead, at MIT.

VDT: John Weeks thought she had graduated in medicine.

BLAKE: No, Kingsley's son graduated in medicine. He's a psychiatrist.

VDT: I guess that was it.

BLAKE: But Laura isn't. And she's definitely not a demographer.

VDT: Well, she's obviously a super-achiever, like her mother. She's had a splendid role model--high-achieving, and yet a very womanly woman. Can I take a picture of you? You look great.

BLAKE: Oh, thank you.

JOHN F. KANTNER

PAA President in 1982 (No. 45). Interview with Jean van der Tak at Dr. Kantner's home in Bedford, Pennsylvania, March 22, 1988. Dr. Kantner revised the interview transcript extensively in April 1990, writing that, "I removed large sections and enlarged others where it seemed to me I hadn't said what I meant to."

CAREER HIGHLIGHTS: John Kantner was born and grew up near Bedford, Pennsylvania. He received all three of his degrees in sociology: the A.B. in 1942 from Franklin and Marshall University, and the M.A. in 1947 and Ph.D. in 1953 from the University of Michigan. He taught sociology, including population, at the College of William and Mary from 1950 to 1953. From 1953 to 1960, he was with the Foreign Manpower Research Office of the Bureau of the Census. He was with the Population Council from 1960 to 1965, including two years as the Council's representative in Pakistan, based in Lahore. He then spent three years at the University of Western Ontario in London, Ontario, where he was instrumental in introducing training and research in demography. In 1968 he went to Johns Hopkins University, where he eventually became Chairman of the Department of Population Dynamics within the School of Public Health and Director of the Hopkins Population Center. Since 1984, as a member of the Joint Career Corps of the U.S. Agency for International Development, he has served as adviser to AID on their population work in India.

John Kantner is particularly well known for his research and publications on fertility, beginning in the 1950s with his contributions to analysis of the Indianapolis Fertility Study. In the 1970s, he and Melvin Zelnik conducted three ground-breaking surveys of fertility-related behavior among U.S. teenagers, which were reported in a series of articles in Family Planning Perspectives and the monograph, Sex and Pregnancy in Adolescence (1982). While with the Census Bureau, he also published extensively on the population of the Soviet Union.

VDT: We are speaking in Jack and Jane Kantner's beautiful restored home from the late 1800s in the historic area of Bedford, Pennsylvania, surrounded by mountains. The sun is shining gloriously the day after some snow.

How did you become interested in demography?

KANTNER: When I went to the University of Michigan for graduate studies [after serving in the military during World War II], I was interested in social theory. There I met Amos Hawley, who though not billed as a social theorist, captured the theoretical imaginations of students who were looking for a framework for viewing questions of social structure and social change. Human ecology as presented by Hawley cut across economics, geography, anthropology, and those areas of sociology that were not deeded to social psychology. It was exciting stuff and it took some of us a good many beer sessions to get it bedded down in our thinking. Amos, at that time, taught what limited offerings Michigan had in population. It was not until Ron Freedman arrived [in 1946] that these were expanded.

Ron, even then, had a good nose for research opportunities, which meant not only research ideas but the wherewithal--data and funds--to get something going. I have only the sketchiest idea of how it came about, but not long after Ron came to Michigan he let it be known that for those who might be interested, the data from the Indianapolis Study, a pioneering fertility survey conducted just prior to the war, might be available for analysis. Furthermore, it appeared likely that the Milbank Memorial Fund would put up some fellowship money for those who became involved. That was a fairly irresistible combination, even in the days of the GI Bill.

Some of us made a trip, a hair-raising trip as I recall, with Ron at the wheel--he borrowed his brother-in-law's car--and Ron drove with part of his mind on some more elevated plane, so it was quite a trip. We went down from Ann Arbor to the Scripps Foundation [for Research in Population Problems] in Oxford, Ohio, to talk with Pat Whelpton and, to a lesser extent, Warren Thompson about the possibilities. Scripps in those days was an important center of demographic research. Between them, Scripps and the Office of Population Research at Princeton just about blanketed the field of U.S. demography. Throw in the Bureau of the Census, which in those days had an impressive stable of analysts, and that was most of it. The Milbank Fund, first with Frank Notestein and later with Clyde Kiser, was, in a way, the yeast for this mix. I leave out my own institution, Johns Hopkins University, for although Alfred Lotka spent some time there and Raymond Pearl held forth from there and Lowell Reed graduated from life tables to become a much admired president of Hopkins, by the late 1940s there wasn't much doing at Hopkins--although Reed and his biostatistical laboratory were the custodians of the Indianapolis data.

VDT: So it was Ron Freedman who put the bee in the bonnet about Michigan getting involved in analysis of the Indianapolis study?

KANTNER: Ron was certainly the midwife. Later on, Dorothy Thomas at the University of Pennsylvania generated some interest in students there, most notably Charlie Westoff, in the Indianapolis data.

VDT: And Jeanne Clare Ridley was in there too?

KANTNER: Very much so. At this time, Jeanne was working at the Milbank Fund in New York; later on, she went to Michigan for her Ph.D.

In the beginning, the original committee that designed the Indianapolis study was concerned about maintaining control of the data. It was provided, for example, that one of the committee should appear as co-author on any articles that were published. And initially, we didn't have the data on hand, physically. The procedure, which must appear quaint if not unacceptable to a graduate student today, was to design the punch cards needed for your analysis and, after Whelpton or Kiser had a look at your layout and any constructed variables, send it on to Hopkins, where someone in Lowell Reed's establishment would enter the data. At first we sent our tabulation specifications to Clyde Kiser at Milbank and the tabulations would eventually arrive in the mail. Jeanne was a central figure in this operation, as was her colleague at the time, Nathalie Schacter. Some of the tabulations were produced at Scripps, beautifully copied on blue- and red-lined tabulation paper--in ink, at Whelpton's insistence! Subsequently, each analyst got his own set of punch cards, and except for the limitations of the old IBM record equipment, research proceeded much as today.

VDT: I'm not quite sure what the Scripps connection was with the Indianapolis study. I know that Milbank funded it.

KANTNER: Other than to say it was close, perhaps even parental, I can't say either. Thompson and Whelpton, two important members of the study committee, were at Scripps. One of the other members was a faculty member of Miami University in Oxford. Lowell Kelly was from the University of Michigan, just up the road, and Indianapolis itself was not far away. The other members, Notestein, Reed, and Kiser, were based at eastern institutions but were obvious collaborators, especially since two of them represented the distal end of the Scripps-Milbank axis.

VDT: That answers my first question on how you became interested in demography. My second question was how you picked fertility as your focal interest. Obviously, it had something to do with your work with Ron Freedman and the Indianapolis study.

KANTNER: It is interesting to recall how professionally avant-garde fertility studies that inquired into contraceptive behavior were in those days. There was Kinsey and a few other studies around, but there was also a sense that we were breaking new ground.

VDT: There was some done before the war, but perhaps it wasn't publicized.

KANTNER: There wasn't much. There were studies of fertility differentials between particular populations. There were studies based on clinic records that provided some data on contraception. Interview studies of contraceptive behavior based on household surveys were a rarity.

VDT: The Indianapolis study [field work done in 1941] was planned at a time of low fertility. There was Enid Charles's book, The Twilight of Parenthood, and I think you referred in your 1982 PAA presidential address ["Population, Policy, and Political Atavism," Demography, November 1982] to that period before World War II seeming to be the "twilight of fertility." ["Parenthood was thought to be descending into a twilight zone."] What a shock the baby boom was!

KANTNER: A phrase purloined in modified form, as you have recognized, from Enid Charles. The Indianapolis study was premised on the notion that the transition of American fertility was coming to a close--one more frontier closing down. The future could be glimpsed, it was supposed, in the fertility of white, married, middle-class, middle-American, urban, Protestant, women such as those that made up the Indianapolis sample. This homogeneity, along with the crude measurement of some of the crucial variables in the study, eliminated much of the variance in the sample and thus some quite plausible hypotheses received scant confirmation. But that realization came later. At the time, this appeared to be a sensible way to discover the "social and psychological factors" that determined the fertility and contraceptive behavior of women who exhibited the characteristics toward which American society was converging.

In one sense, the Indianapolis study quickly became an anachronism, but it did give birth to a series of landmark studies over the following two decades which were able to track the baby boom. These studies, the first GAF study [Growth of American Families, 1955] and the Princeton Fertility Study, were designed, in part, to address some of the perceived deficiencies--theoretical and methodological--of the Indianapolis study. They were also concerned, since demographic trends were not behaving as predicted and population projections were turning out to be embarrassingly wide of the mark even on short trajectories, to see whether fertility intentions might be a dependable substitute for demographers' intuitions. The failure of the psychological variables in the Indianapolis study to explain much of such variance as there was to be explained was particularly galling to some social scientists. A major aim of the Princeton study was to do a better job of conceptualization and measurement in this domain. The results, again, were disappointing.

VDT: A notorious case.

KANTNER: Right. So that, in a roundabout fashion, is how I got involved in fertility research.

VDT: You did your Ph.D. dissertation with Ron Freedman, based on the Indianapolis data. What was that on?

KANTNER: The study was an attempt to look at social mobility in relation to practice of contraception, desired family size, and so on.

VDT: The difference between generations of the respondents and their parents?

KANTNER: Yes, essentially that.

VDT: Jumping ahead, since we're talking about fertility, what led to the famous studies you did with Melvin Zelnik in the 1970s of teenage sexual activity, contraceptive use, and pregnancy? I'll ask you later how you got to Johns Hopkins, but now I would like to know more about those studies, based on surveys in 1971, 1976, and 1979.

KANTNER: I was still in Canada at the University of Western Ontario when Mel Zelnik and I began talking about undertaking a fertility survey. Mel had an interest in the fertility of U.S. blacks, a population that had been rather slighted in national surveys up until that time. Another group that had been insufficiently studied was the unmarried, especially young never-married females. Our thought was to find support for a national sample survey that would provide adequate representation of both of these relatively neglected groups which on the basis of fragmentary evidence, largely attitudinal rather than behavioral, were certainly not marching to the same drummer as the good ladies in Indianapolis. After some preliminary negotiation, during which time we learned that Charlie Westoff and Norman Ryder were planning another round [1970] of the National Fertility Study, this time with an over-sampling of blacks, we applied to NICHD for funds for a study of 15-to-19-year-old females, with no restriction as to marital status.

This was to be a longitudinal study and, indeed, funding was forthcoming for such a study. However, the longitudinal design had to be abandoned after one year for lack of funding, the preference at NICHD being, on advice of their study section, for a series of cross-sectional studies which, it was argued, would do the job just as well. It was an unfortunate decision which, I believe, even NICHD came to regret after a while. We put a lot of time and thought into designing the follow-up procedures and were able to locate a high proportion of our respondents one year after the initial interview. Admittedly, persons of this age with changes in residence and marital status, which then, more than now, meant a change in name, present special difficulties for a longitudinal investigation, but I believe we could have carried it off. At least the bile would not have risen so high when in later days well-intentioned critics would sometimes suggest that we could have learned more about adolescent fertility had we thought to employ a longitudinal design. As it turned out, we were able to carry out three independent surveys between 1971 and 1979 [1971, 1976, 1979].

VDT: Those surveys were extremely influential. I think they had much to do with the emphasis on teenage pregnancy. There has been nothing like them since, except the National Survey of Family Growth, which ultimately included unmarried as well as married teenaged women. Do you think there should be continuing, longitudinal studies of teenagers?

KANTNER: I am not sure that I would give that high priority. I think by this time, we probably know enough about that area for most practical purposes. There are more interesting topics about. In any case, the subject should be treated in the context of studies of changes in major social structures, such as the family and the work place. It is commonplace now, but I recall that Mel and I were as impressed by the family history data that came out of the studies--the large proportion of young women, particularly blacks, who had not lived in a stable family for major proportions of their life--as we were by the sexual and contraceptive behavior of our subjects.

VDT: You never published a book about the studies, just a series of articles?

KANTNER: We published a very good book which provided an overview of the findings from all three surveys. Unfortunately, the publisher did a poor job of promotion. Couldn't even get a copy on display at the PAA.

VDT: What's it called?

KANTNER: Sex and Pregnancy in Adolescence, by Zelnik, Kantner, and Kathleen Ford. Published by Sage in 1982.

VDT: I'm glad to know you had a book, because that bothered me. I think it's too bad when it's not all pulled together, and I'm sorry to know it wasn't promoted.

Let's go back. What did you do at the Census Bureau? You were there from 1953 to 1960.

KANTNER: That's right--seven years, but for one year when I was on leave to work in Indonesia with a Ford-financed project run out of the University of California.

Well, after three years professing sociology at the College of William and Mary, I joined the Foreign Manpower Research Office, then headed by Parker Mauldin. That office was concerned with making population estimates and projections for the countries of the Soviet bloc which, as you remember, published relatively little demographic information during those early postwar years--lots of information on FYP [five-year-plan] goals and "fulfillment" of Plan objectives, but little else. Stalin's purges in the 1930s had made population counts politically inflammable and the enormous war losses, had they been fully revealed, would have shown a nation much more badly maimed than most experts realized at the time. The U.S. as well as its allies "needed to know"--in intelligence parlance--more about the size, composition, and distribution of these populations than the countries themselves were willing to reveal. There were fragments of information around--old censuses, the odd number or distribution released in a technical publication, a newspaper, or even a radio broadcast. The Bureau had a staff of translators dredging and sifting this kind of material, as did the Library of Congress and other government agencies. Frank Lorimer had made what he could out of information from the last two prewar Soviet censuses and was free to say, in that burly manner of his, that those of us were attempting to estimate current and future trends on the basis of a few scraps of information and assumptions about demographic interrelations were "chasing our tails." He may have been right. When the first postwar Soviet statistical handbook containing demographic data finally came out in 1959, it appeared as if we had underestimated the demographic costs of the war by a substantial margin; we had overestimated their postwar population by about 20 million. Perhaps we should have received the Nobel Peace Prize! Overestimating Soviet manpower resources is tantamount to ceding them a goodly number of divisions.

VDT: Did you read Russian?

KANTNER: I worked at it. I was okay on reading statistical material, notes to tables, that sort of thing, and painfully slow on text of any complexity. I probably had the most overworked dictionary around the place.

VDT: Your interest in the demography of countries other than the U.S. began with your work on the Soviet Union, but the Population Council, your next stop, must have widened your horizons further.

KANTNER: I think that's fair to say, although the year spent in Indonesia, 1957, was also a turning point. Working at the Population Council with experienced, insightful, senior colleagues like Frank Notestein, Dudley Kirk, and Marshall Balfour was a marvelous internship.

VDT: And what did you do at the Population Council?

KANTNER: To answer that I should say a word about the Council in those days, which was a much different institution than it is today. Although the Council came into being [in 1952] because of concern in certain philanthropic circles with what were perceived to be the likely negative consequences of rapid population growth in backward areas, it was not eager to jump immediately into family planning. The watchword was caution, and the program emphasized research--demographic, social science, and biological research--and training, especially the training of foreign nationals in American universities.

Frank Notestein [second president of the Population Council, 1959-68, following Frederick Osborn] was extremely sensitive to any suggestion of imperialism. I recall him warning Warren Nelson and Shelly Segal of the Council's biomedical division that no trials of new contraceptive methods should be conducted using foreign cases unless and until tests had been carried out involving U.S. subjects. Or in the realm of economic analysis, Frank was supportive of work such as that of Coale and Hoover [Population Growth and Economic Development in Low-Income Countries, 1958], but when a few years later Stephen Enke produced his cost/benefit calculations to demonstrate the economic payoff of birth control [roughly, \$1 or \$5 spent on family planning is worth as much as \$100 spent on development aid], Frank was furious, expecting--correctly, as it turned out--to catch it from professional anti-imperialists like Mahalanobis and others. He would immediately demand to know whether "any of our money went into that." And, as most everyone knew at the time, Chris Tietze, though a highly respected colleague, was housed discreetly off the premises because of his enthusiasm for abortion.

To get to your question, I helped to process grants and for a while looked after the demographic fellowship program. Dudley Kirk covered Latin America and the Caribbean; Parker Mauldin and Marshall Balfour took on Asia and the Middle East, where they both spent time. Each of them spent a fair amount of time traveling in their regions to scout out the general interest in population and opportunities for research and training. Lord knows how many short-notice lectures they gave on the demographic transition. I generally stayed at home with various kinds of paperwork, commiserating with Dorothy Nortman about the lack of glamour in our professional lives. Dorothy later broke out of her role as statistical handmaiden and developed a well-deserved reputation of her own.

One of my tasks was to work up the language that was needed for the Council's annual subvention to the UN's two demographic research and training centers at Chembur, India, and Santiago, Chile. There could not have been a job description, except of the broadest sort, to cover what any of us did. One did what came to hand unless there was someone more competent around, with time, who would not be demeaned by doing it.

It was a fascinating place to work and a good time to be there. Perhaps the most important activity that the Council ever undertook is its fellowship program. Eugenia Gale and I developed procedures for administering the program and thought up reasons to prevent its being handed over, in the name of cost-effectiveness, for administration by the Institute for International Education.

VDT: You went to Pakistan while you were with the Council. How did that happen and what did you do there?

KANTNER: Again, some background. Around 1959, General Ayub Khan, who a year or so earlier had taken over Pakistan from its foundering civilian government, signaled his interest in doing something to slow his country's rate of population growth. Marshall Balfour, an old subcontinent hand, and Paul Harper, a pediatrician and head of Hopkins' department of maternal and child health, went to Pakistan to have a look. They returned with a plan, part of which was the establishment of two research projects, one in West Pakistan, one in the East, that would test the readiness of Pakistanis to use contraception. The Johns Hopkins School of Hygiene and Public Health and the University of California School of Public Health were given the task of designing and organizing appropriate projects. It was also proposed that there should be some training in population. The Punjab University in Lahore was selected as the most promising site for such a training program. My assignment was to set up the training program and, since I would be in Lahore, to make myself useful to the Hopkins project, which was located there also. I was there two years.

The "Diploma Program" at Punjab University was, at last inquiry, still in operation. The Hopkins and Berkeley projects ended around 1972, each learning much but accomplishing little with respect to finding ways to generate demand for contraception under conditions then prevailing there. John Gordon and John Wyon and later Carl Taylor were to have a similar experience in neighboring Indian Punjab. Our friends in East Pakistan, with less of a public health and more of a social science orientation, fared little better. Was it not the right time or had we somehow botched the effort? Some of each, I fear. There was no lack of commitment at the highest levels of authority, but Pakistan, despite Ayub's plan for encouraging grassroots initiative through village level "basic democracies," failed to penetrate the local community the way successful programs in Indonesia and Thailand subsequently managed to do.

VDT: The World Fertility Survey showed just 6 percent contraceptive prevalence for Pakistan in the 1970s. The Contraceptive Prevalence Survey showed 8 percent around the mid-1980s. Pakistan seems to be a tough nut to crack.

Let's jump ahead to your more recent experience in Asia. You recently spent two years in India and, I believe, you are still involved there.

KANTNER: Just about the time I stepped down as chairman of the Department of Population Dynamics at Hopkins, in 1984, the Agency for International Development announced a new program for attracting academics to spend time working with their foreign missions in a variety of fields. AID had been searching for a long time without much success for ways to improve its interaction with U.S. universities. Their new idea was an adaptation of the time-honored Interagency Personnel Agreement [IPA], which provides a mechanism for university faculty to spend time with government agencies working on projects of mutual interest. University demographers have used this device, for example, to work on special projects at the Bureau of the Census. The only thing new in AID's case is that the basic IPA legislation is being used to recruit academics to work in their missions overseas, with very loosely defined responsibilities. Collectively this group of academic sojourners is known as the Joint Career Corps. It's a sound idea, it seems to me, and is meant to be reciprocal, in that it is supposed to work in reverse, with AID personnel spending time at the Joint Career Corps' university. This side of the scheme has not been notably successful.

So with AID paying my salary to Hopkins, we left for India. After looking over the situation and discussing with AID officials their priorities, I settled on three major activities. One was to assist the Registrar General in getting U.S. funding for staff training and for modernizing his data-processing capacity. In doing this, AID was able to arrange for help from our Census Bureau and from the East-West Population Institute. My role, at most, was that of a go-between.

A second activity was to find ways of providing funds for India's population research centers,

most of which lack many of the basic facilities and resources that one would expect to find in such places. The proposal that I drafted has passed through many hands and has still not been approved by all parties at the same time. All parties, both on the Indian and the U.S. side, have approved it at one time or another, but there is such a game of musical chairs among our bureaucrats and theirs that this proposal, like many others, has become something of an administrative greased pig. After I left, Warren Robinson, my successor in the Joint Career Corps, tried to wrap things up and even invited me back to revise the proposal. Warren too has now gone and still nothing has been finally settled. Nothing out of the ordinary in this, but it does take a love of foreign living to assuage the frustration.

Finally, I was invited to write a piece for the AID mission, essentially to describe the demographic situation in India and the measures that were being and had been undertaken to slow the rate of growth. I produced a small report and am still at work on an expanded and updated revision with an Indian colleague.

VDT: Let's skip back to why you left the Population Council and why you went to the University of Western Ontario [in 1965], which seems to have been a sea change.

KANTNER: Foundation life is not for everyone, nor is it what some of us wanted for the long haul. Dave Yaukey, a veteran of the Pakistan campaign, had an office next to mine at the Council. We fed each other's sense of being displaced persons from academia. Dave left first, which only increased my determination to do likewise. Nothing strange about this. Dudley Kirk left a few years later, then Tom Burch, Allan Rosenfield, others. What is strange, perhaps, is that I went to Canada [as did Tom Burch]. But before I get into that, you should understand that some of us felt we were on the wrong side of the table. The scholars who came to the Council seeking funds for research--in those days, the Council was an important funding source--were those who were having the fun. We got our kicks, to the extent we did, vicariously--the satisfaction of having had enough sense to recognize a good idea.

When I left William and Mary for the Census Bureau, the idea that I would eventually return to academic life was merely postponed, never abandoned. I went to look at the opening at Western Ontario because the people there were both persistent and persuasive. I remember not feeling perfectly clean about the trip, because the possibility of accepting a position there, if one were offered, seemed so remote. Obviously, I liked what I saw: a strong economics department that wanted an equally strong program in population, there being no such program in population at the time in Canada except at the Universite de Montreal. The commitment from the university president on down appeared solid and the virtually carte-blanche opportunity had great appeal.

When I told Parker Mauldin I was leaving the Population Council and going to this place he said, "Oh, come on. Let's go see Frank," so we went to see Frank Notestein. Frank at that time was trying to give up smoking and he was nervous and fidgety and this was one more problem he didn't need. He looked out his office window up Park Avenue and said, "I hate see you leave, but I really hate to see you leave for a secondary place." I think he thought that would deter me, but I liked what I saw in Canada. We stayed there three years and would no doubt have stayed longer but for personal matters that were developing "South of the border"--as you Canadians sometimes refer to the U.S. And, of course, the opportunity to join some of my old friends in expanding the population program at Hopkins was an inducement.

VDT: Coming out of the cold Canadian winter . . .

KANTNER: Ah, yes. I went to look at the Hopkins job in the spring when the azaleas were in bloom, the boxwood getting pungent, and birds bouncing on the campus lawn. Back in London, Ontario, it was still slate gray--perhaps a few wild onions in the front yard.

VDT: Before we leave Canada, could you tell me more about the early development of the population program at Western. Being Canadian, I'm proud of what they've built up there, and you feel you had a hand in that?

KANTNER: I think I did. I was first chairman of their department of sociology. We started off as part of the department of economics while plans were going forward for a department of sociology, which was to be the primary home for teaching and research in population. Richard Osborn, now at the University of Toronto, was the first on the scene. Within a short time, there were four of us whose primary interests were in population: Osborn from Brown, T.R. Balakrishnan, a Keralian with a Ph.D. from Michigan, Jack Allingham from Australian National University, and myself from here and there. Of that group, only Bala remains at Western. Allingham, who was priming himself to be a second Chris Tietze, went off to McMaster for a medical degree and was claimed by medicine--a real loss to the field.

The build-up continued apace: recent Ph.D.s and a few ABDS [all-but-dissertation] from U.S. universities: Carl Greinstadt and Peter Morrison from Brown, George Jarvis from Michigan, Charles Nobbe from the University of Washington, Ian Pool from New Zealand, and Ed Pryor from Brown. It was a sellers' market then; difficult to hire and to hang onto good people. We were extremely fortunate to be able to get such fine young--as they mostly were then--scholars. After a year, we lost Peter to Penn, which shortly lost him to Rand; Pool was lured off to Cornell; and, eventually, Nobbe and Pryor went off to Ottawa for jobs with CIDA and Statistics Canada. But the commitment and the opportunity were there and growth continued. We were at the same time trying to build a program in sociology so that the intake of new population faculty had to be balanced with broader requirements.

We received great support in our efforts from the head of the department of economics, who was a master at academic politics, and from that grand institutional impresario, Albert Trueman, who came to Western as dean from the Canada Council. After I had decamped for Hopkins [in 1968], the department continued to expand. Canada was going through a silly period of nationalism in the early 1970s, which made it difficult to hire American scholars trained at U.S. universities. I recall protesting this policy vigorously to the Canadian Anthropological and Sociological Association. I sent them what I regarded as a masterful letter, pointing out how American universities had been strengthened by European imports and European models. In return they sent me a dues notice! Nevertheless, Western was able to bring in some excellent people: Tom Burch from the Population Council, Kevin McQuillan and Rod Beaujot, authors of Growth and Dualism: Canada's Population [1982]. I can't take a lot of credit for the ultimate achievement, but I was there at the creation. Perhaps the greatest personal satisfaction came some years later when two Western graduate students received the Dorothy Thomas award at the PAA meetings in Washington [1981].

VDT: As I told you, I take a sort of maternal pride in it too. Rod Beaujot wrote the Population Bulletin on "Canada's Population: Growth and Dualism" [April 1978] when I was Bulletin editor at the Population Reference Bureau and then went on to become sort of "Mr. Canada Population" and wrote that book of almost the same title with Kevin McQuillan.

KANTNER: I should say a word about population research at Western. Since we were offering a master's degree, we needed to have some research going on around the place. Incidentally, as a Canadian you will remember the Robarts fellowships which the Ontario government provided on a very liberal basis for master's level training. Though a Ph.D. in demography is now offered by the department of sociology at Western, in the early years when there were just a few of us, we did not aspire that high. The strategy was to send our graduate students off to institutions south of the border

with a solid grounding in the fundamentals of demography.

In any case, Allingham, Balakrishnan, and I sought and got funding for a fertility survey which was carried out in Toronto. As we note in the introduction to the book that came out of the study [Fertility and Family Planning in a Canadian Metropolis, 1975], the accustomed mode of demographic study in Canada had been the analysis of secondary data from official statistical agencies. Such statistics steer clear of the subject of contraception and neglect relevant dimensions of fertility such as birth spacing, desired family size, and the social and psychological context. Perhaps because Canadian fertility was falling in the 1960s and French Canada was declaring its determination to win "the battle of the cradle," the Canada Council bought the case we put up to them and awarded us \$67,000 to get on with it. When this grant was announced along with other Canada Council awards at a board meeting of the Canadian Anthropology and Sociology Association at which I was present, there were murmurs of surprise and whistles of incredulity. Small as that amount might seem today or even by comparison with what was being spent even then in the U.S. on fertility surveys, it was then an unprecedentedly large amount for the Council to put out for a social science investigation. The eventual goal we had in mind was a survey of national scope, but as you know that took some years to arrange. It eventually did come about [in the mid-1980s] and Balakrishnan is one of the principal investigators.

VDT: Now let's go on to Hopkins, where you went in 1968. You say that grew out of the contacts you had made with the Hopkins project in Pakistan.

KANTNER: That was part of it. Paul Harper was chairman of a fairly new department of Population and Family Health, part of the Hopkins School of Hygiene and Public Health. It is not easy to sell someone accustomed to the sheltered existence of a liberal arts faculty on the rough-and-tumble of a school of public health. Paul had been unsuccessful in his approaches to several well-known demographers to head up the demographic part of his program. Mel Zelnik was there handling the basic courses in population. His efforts were augmented by Margaret Bright, by Peter Newman who came over from the economics department to lecture on population economics, and most interestingly by outside lecturers, notably Frank Notestein and Irene Taeuber. In addition, some of the biostatisticians in Reed's old department considered one of their primary interests to be vital statistics. There was a good bit of basic demography in the introductory course in biostatistics, which virtually all of our students took. Population ecology was taught in the Department of Pathobiology and the Department of International Health dealt with the practical side of health delivery, including family planning. It was an interesting smorgasbord, but lacked the coherence and continuity needed for a serious graduate program.

After he got things reasonably bedded down in terms of staff and funding, Paul Harper stepped down as chairman. Henry Mosley, also a physician, was recruited to succeed Harper. Mosley stayed only a short time, being wooed away to take over as director of the Cholera Research Lab--now the International Centre for Diarrhoeal Disease--in Bangladesh. The search for a new chair dragged on for almost two years while I served as acting chairman, spending part of my time in Bud [Oscar] Harkavy's office at the Ford Foundation in New York. I was seriously considering joining the Ford Foundation's India program until a new, no-nonsense dean arrived at the School of Public Health, found our search committee in a state of stymie, and, in effect, gave me a battlefield commission as chairman of the Department of Population Dynamics; that was around 1976 or 77. There is obviously a lot more to any such story, but that will suffice. It was around this time also that we started the Population Center.

VDT: How is that distinct from the Population Dynamics Department?

KANTNER: That has never been easy to explain. There was certainly a great deal of overlap, but essentially the Center was useful as a device for melding the varied population interests found around the university: in sociology (Lois Verbrugge and, later on, Andrew Cherlin and Portes), in economics (Newman), in behavioral sciences (Bright), and in ob/gyn and urology, where there were researchers working in collaboration with the reproductive biologists in population dynamics. As I was told, it was Jim Coleman's idea to give all of this dispersed activity some greater collective concreteness through a university-wide Committee on Population--the forerunner of the Population Center. The Hopkins Population Center was one of the original group of NICHD-sponsored centers. All of the population centers established at schools of public health have had teething problems, some of melodramatic proportions. Ours were mild by comparison, but we were confusing to the outside world. When I finally left Hopkins for India, Jim McCarthy took over as director of the Population Center and Henry Mosley returned as chair of the Department of Population Dynamics. That perhaps clarified the situation somewhat, as compared to the same person--me--with two hats.

Somewhat earlier, a potentially greater confusion was headed off when AID proposed setting up a "Population Center" to house Phyllis Piotrow's highly successful Population Information Program and the related Population Communication Services program. AID has a penchant for structures that mirror its own program categories, but these are often ill-suited to the way universities are organized and function. Most AID people know universities only from having been students; few have worked in them as faculty.

VDT: Before we get away from Hopkins, what about Raymond Pearl? He was a powerhouse at Hopkins and in the field back in the 1930s. What legacy did he leave at Hopkins?

KANTNER: So far as the current work in population is concerned, it's as if he had never been there. His measure of contraceptive prevalence is presented in courses dealing with the evaluation of family planning programs only to be dismissed for its deficiencies. During his time at Hopkins, he stirred up a lot of resentment, according to Elizabeth Fee, his biographer, for the way he used his prestige and reputation to leverage the allocation of university resources in his favor. Quite the opposite of his collaborator Lowell Reed, who is remembered with great fondness and respect, except perhaps by the students unfortunate enough to live in Reed Hall.

As a scientist, Pearl was out of the same general mold as his contemporaries, men such as Corrado Gini, Karl Pearson, and Thomas Huxley, who were captivated by biologicistic theories of behavior. This was--and in some quarters still is--a congenial scientific paradigm at a place like Hopkins where the laboratory sciences are the jewels in the crown. One shouldn't make too much of the Hopkins connection, however. Biological and evolutionary interpretations were in the "air" that the scientific community was then breathing. Some years ago when I was introduced to the then chairman of the Department of Biostatistics at Hopkins, one of Reed's successors, he wore a white lab smock. While it undoubtedly spared the elbows of the mufti much as the rest of us wore, I believe there is a less functional interpretation. I suspect there was not only the scientific appeal of biological mechanisms, but also a certain mystique about them that appealed deeply to those born-again Darwinians. Pearl's logistic curve not only described the growth of confined populations of *Drosophila* [fruit flies], but suggested a natural law to which--quite fortuitously, it turned out--human populations seemed to conform. The logistic curve looked pretty good as a predictor of the U.S. population during Pearl's lifetime.

VDT: May I ask about the leading influences in your career? Obviously there was Ron Freedman, and you also mentioned Amos Hawley and Frank Notestein.

KANTNER: There are, of course, known and unknown influences. I've often wondered about the unknown influences that might have operated either positively or negatively.

But with respect to influences of which I am aware, Ron Freedman was certainly an important one. Ron was a role model back before there was that unfortunate term for someone that was worthy of emulation. I remember relatively little of what he taught me in a formal sense, but I have always admired his approach to things--his equanimity, his broad perspective, and overall judgment and good sense. At many junctures in my career, these qualities which I found in Ron have served as a touchstone in choosing a course of action or a response to a difficult situation.

Amos Hawley gave me an abiding idea of what the social sciences should attempt to be. Many others also appreciated Amos for his steady view of social organization and the proper business of the science of mankind, as witness his becoming president of the American Sociological Association [for 1978] on a write-in nomination.

Frank Notestein represented a bulwark against hucksterism in the application of knowledge to societal problems. International population assistance as offered to the underdeveloped countries by this country would have had a less stormy time of it, certainly, and might well have been more influential had some of Frank's circumspection been observed.

Strange as it might seem, since in some ways his views were the antipode of Frank's, I have found Kingsley Davis a pivotal figure. I don't have reference to his work on India [The Population of India and Pakistan, 1951], although that has been of lasting value, but to the fact that he has been consistently sociological in his viewpoint. Even when he has been mistaken, as in my view he was in some of his conclusions regarding abortion and contraception, he works with a logico-deductive method reminiscent of the Durkheimian tradition. It is good to be reminded occasionally of the utility of sociological theory.

VDT: And yet, as you said earlier, it was not sociological theory or even intellectual challenge that led you to study teenage pregnancy.

KANTNER: It needed to be studied for a host of practical reasons. It is the kind of research that is a good fit to the mission orientation of a school of public health. Where Zelnik and I ran into difficulty was in trying to limit our involvement to the facts of the matter and not to become involved in advocacy or prescription. As we became known for having specialized knowledge of the sexual and contraceptive behavior of young American females, we were frequently expected to make policy recommendations or prescribe a program of action. We were, Zelnik and I, old-fashioned in our notions about the proper division between science and advocacy. I recall one conference we attended when our unwillingness to make the leap from our data to advocacy of abortion on demand for young women was decried as rank defection by some of our respected friends at the Alan Guttmacher Institute. Fred Jaffe, one of the best in the advocacy business, was, I believe, stunned by such public pussyfooting.

VDT: That's a perennial question at PAA meetings--the extent to which demographers should become involved in policymaking. There is more and more pressure in that direction.

KANTNER: As much as they please--is my answer. Only be clear about where the science leaves off and opinion takes over.

VDT: In your PAA presidential address of 1982 ["Population, Policy, and Political Atavism"], you were pessimistic about the status and support of federal demographic statistics-gathering in the U.S. at that time. That was early in the Reagan administration and there was a lot of budget-tightening. You

also criticized the UNFPA for downgrading the gathering of statistics in less developed countries among its priorities. And you were critical of the revisionist view of population growth and economic development, à la Julian Simon, and this was before the 1984 Mexico City population conference, when the U.S., pushed by anti-abortion forces, turned around in population policy, declaring that population growth is a neutral factor in economic development, free-market strategies were most important, and eventually the U.S. cut off funding of the IPPF and UNFPA. What do you think about all that now?

KANTNER: On the whole, the Reagan and now the Bush administrations have not appeared as champions of the federal statistical system. One might suspect them of harboring the view that what you don't know can't hurt you. The intrusion of politics into government statistical policy appears to be every bit as bad as I had feared. A particularly telling case was the decision to chuck out the work that the Bureau of the Census had done on undercount adjustment in favor of a heads-in-the-sand policy. Democratic mayors of big cities versus Republican interests of the suburbs? Crude but credible.

On the other hand, we've gotten better organized on the other side. The PAA can take pride in the leading role it took in the founding of COPAFS, the Council of Professional Associations on Federal Statistics. Under the able direction of Katherine Wollman, that organization keeps a weather eye out for torpedos launched by OMB and other Luddite organizations. Awareness and protest help ward off some of the most flagrant attacks on the integrity of the system, but the ultimate solution has to be political, since federal statistics are part of the power game. It's not all undercount and ethnic representation in the U.S. either. You must remember AID's attempts to cook the data on the birth rates in some of the less developed countries where they had made large investments in family planning. Eternal vigilance is necessary, for the barbarians are forever at the gates.

The criticism of Simon's work [The Ultimate Resource, 1981] in my presidential address was unfortunate. It was too salient at the time to ignore on such an occasion, especially as the asserted "neutrality" of population in human affairs was being outrageously exploited by political opponents of international population assistance like Senator Buckley and certain political appointees in the State Department. To be silent on the matter went too much against my nature, but I could have handled it better. The shabby treatment of Richard Benedick later at the time of the Mexico City meetings didn't help my equanimity. [Benedick, Coordinator of Population Affairs in the State Department at the time, a leader in preparations for the Mexico City conference, and "an outspoken advocate of family planning programs and the need for the U.S. government to continue its leadership role," was not named head of the U.S. delegation as widely expected. Former Senator James Buckley was named instead, "largely on the basis of his personal (anti-abortion) views on the matter and his acceptability to right-to-life groups." From Jason L. Finkle and Barbara B. Crane, "Ideology and Politics at Mexico City: The United States at the 1984 International Conference on Population," Population and Development Review, March 1985.]

The argument is a silly one, in any case. Given the little that is known for certain about the interrelationships between population growth and social and economic change under the myriad conditions where this drama is played out, there are few fixed stars to set sail by. An open mind at Mexico City would have done credit to our delegation. Instead, when the astonishment passed, we became a laughing stock.

VDT: What do you think of the 1986 National Academy of Sciences study [Population Growth and Economic Development: Policy Questions], which came out very neutral?

KANTNER: I would regard it as balanced rather than "neutral." The word "neutral" is politically

loaded. The point is not whether this issue can be settled in general or even whether it makes sense to try to resolve it in global terms--I think it does not. The real issue is one of political doctrine. Under Ray Ravenholt we had a doctrinaire position at AID [pushing U.S. family planning assistance to less developed countries] that General Custer might have admired; at Mexico City our position was again doctrinaire, but in the other direction. Banners need not be unfurled in setting policy for international assistance. An informed pragmatism will do to give it direction; there is sufficient decency, compassion, and altruism in the basic American value system to give it commitment. We should be done with false doctrines.

VDT: Yet the issue of "revisionism" stays with us and has overshadowed PAA meetings almost ever since it came up in the early 1980s. The NAS study was criticized at the time for leaning toward Simon.

KANTNER: Well, I have given my view on the merits of this issue. In my reading of scientific advance, issues of a general paradigmatic nature are rarely resolved; scientific interest shifts elsewhere.

If I were teaching students these days, I might assign them an article I read the other day [in 1988] by Hans Binswanger and McIntyre--this was in Economic Development and Cultural Change--that traces out in hypothetical fashion the intricate, non-recursive pathways between population growth and economic structure. I would then ask on my pop quiz whether, in view of what they had read, population growth should be regarded as negative, neutral, or positive--I would assign Ester Boserup too. I would give an A for laughter--extra points for a Bronx cheer--and would consider, from the less expressive, the equivalent of, "Surely you jest, sir!" In any case, all is forgiven since Julian Simon asked me, "How many supply-siders are necessary to change a light bulb?" The answer, which you've probably anticipated, is, "None. The market takes care of it."

VDT: Before we get onto PAA, let me ask what accomplishments in your career have given you the most satisfaction? You mentioned your pride in being in on the ground floor in the development of the population program at the University of Western Ontario. What else?

KANTNER: Well, I would have to mention my association with Mel Zelnik on the teenage studies. The topic as such was not one with great intellectual interest for me, but the challenge of getting dependable data on a problem of social importance had its own rewards. I was fortunate to have a colleague with the high standards of craftsmanship that Mel brought to the collaboration. Because of its social salience, it's an area that attracts what my Indian friends might call "premature-conclusion wallas." We were lucky in having Dick Lincoln as editor of Family Planning Perspectives to act as a "fence" for the material we produced, so that it got out quickly to those who wanted it. There were some tense moments over matters of language, slant, and emphasis, but overall it was a productive relationship--with respect to both sides of the science-advocacy line.

VDT: What other satisfactions?

KANTNER: I think any academic to whom you put that question would be bound to mention the occasional student who turned out well, whether or not you can claim credit for it. Some good ones came my way. The better they were, the less I had to do with how they turned out.

VDT: Now on PAA. Do you remember when you first joined and your first meeting?

KANTNER: I joined as a student member while still at Michigan. My first meeting probably was at

Princeton--1950? The Association was small enough in those days to meet at a place like the Princeton Inn and to meet only in plenary session.

VDT: Everyone speaks lovingly about the Princeton Inn. Did you both stay there and have meetings there? [Meeting participants all stayed at the Princeton Inn, but the sessions were held elsewhere on the Princeton campus--McCormick Hall in 1950.]

KANTNER: We certainly stayed there. I remember luxuriating as the sole occupant of a double room and waking in the morning to find an unexpected roommate--one Charles Nam. Later we were to work together in the same office at the Bureau of the Census. When Charlie got caught in the dndndraft of the RIF [reduction in force] instituted by the Eisenhower administration, I was one of several who recognized his ability and suggested he go off and get some more initials to put after his name. The rest is--a subject of one of your future interviews, no doubt.

The next meeting was, I believe, at the University of North Carolina [1951], which may have been one of the last of the meetings when we all met together. [Double sessions first appeared at the 1956 meeting at the University of Michigan, Ann Arbor.] Phil Hauser was president that year. I remember his address, which he arranged to have given by his esteemed Chicago colleague, William Ogburn [on "Population and Social Change"]. Not that Phil didn't use the opportunity to speak his piece also. It may have been the only PAA presidential address with any poetry in it. Quoting W.H. Auden, Phil cautioned that, "Thou shalt in no wise associate with advertising guys, nor commit a social science." Not only was that a favorite line of mine also, but I was to remember it many times in subsequent years in connection with our work on teenage fertility and in coming into contact with the information-education-communication "specialists that abound abroad. For some, the word "specialist" partakes of professional legitimacy, as in medicine. I am old enough to remember Chick Sale.

VDT: What else comes to you in remembering some of those early meetings?

KANTNER: Racial discrimination at the Carolina meeting. One of our black colleagues was refused admission to the Carolina Inn.

More favorable recollections are of the debates, which seemed almost titanic to us notivates, with Frank Notestein, Frank Lorimer, and Kingsley Davis rising to argue on fundamental conceptual matters, with all registered members in attendance--not today's proliferation of simultaneous sessions strung out along the carpeted corridors of some look-alike hotel. There was Frank Lorimer, with his booming, burlap voice, which must have served him well in the pulpits of his earlier days, blazing away at someone's intellectual waywardness. There was the other Frank calling someone to task, but with such finesse and such sighs of regret and suggested extenuations that the victim may not have recognized immediately that he had been deprived of his trousers. Some great theater!

Remember, the theory of the demographic transition was still relatively new and intact and we still hadn't figured out how to look at the determinants of fertility or even how to decompose a fertility trend. But the analytical frameworks were being put in place, and that was heady: the demographic transition; Davis and Blake's intermediate variables framework, in 1956--I thought that was dynamite; and the Coale-Hoover study, which was another landmark in the late 1950s. The patterns were being laid that were followed up on subsequently. I don't see anything of that quality around these days. Maybe they're there and I'm not aware of them.

VDT: You think all the major frameworks have been put in place? What about migration? That hasn't been your field, but we have Everett Lee's . . .

KANTNER: That, as Everett acknowledged in presenting it ["A Theory of Migration," Demography, 1966], was a reworking and systematization of Ravenstein's propositions. I found it useful in teaching, but it didn't touch off a train of research as did Sam Stouffer's theory of intervening opportunities, perhaps because Stouffer's theory came earlier when there were graduate students around looking for something testable in a field that had been largely descriptive. Perhaps a more adventurous attempt to fashion migration theory was Zelinski's formulation of a migration transition, linking patterns of migration to states and stages of social development, an idea that could be seen as an extension of some of R.D. McKenzie's early ideas about how populations come to terms with their environment, given the technology and organization available to them. Certainly, I don't believe any field arrives at the state where all that is left is filling in the blank spaces of earlier frameworks.

It would be interesting to identify the major frameworks in our field and trace their development--and attempt to see ahead. I would expect new directions to rise from the interstices between the fraying edges of established disciplines and out of efforts at historical reconstruction, such as the Princeton European Fertility Project. A parallel account of the major methodological developments that have made us better able to grasp our subject--Coale and Brass's work in the 1960s, for example--would be valuable. I am thinking of something that would be more particularistic and on a less grand scale than Dudley Duncan's admirable book [Notes on Social Measurement: Historical and Critical, 1984]. Something to do in retirement, eh?--as you Canadians say.

VDT: What do you see as the outlook for demography in the U.S.? In a sense you've answered that in saying that you expect fresh perspectives to come not from mainline demography but along the lines of contact between disciplines and out of the work in historical demography.

KANTNER: On the substantive side, yes. But in the area of application and methodological innovation there seems to be an open field. One of the fastest growing areas in demography, I would suspect, is the application of demographic knowledge and technique to business planning and public administration. We have a new term in the last several years, "demographics," which is the commercial world's jargon for the essential demographic information it needs to go venturing. Demographic information makes good copy not just for those who may want to risk a buck but also for the serious reading public that wants to know more about the reeling world they live in. The success of American Demographics is an indication of this interest. In Canada where free-market orthodoxy is less canonized than it is here, Statistics Canada puts out a publication based on their enormous data base that would not look out of place on any magazine rack; none of your "Queen's Printer" drabness. I can't resist noting that this publishing venture in Ottawa was the brainchild of Ed Pryor, who followed me as head of the department at Western Ontario.

Coming back home, as we return to a more responsible position regarding the need for government statistics, the market for demographers in government service should be bullish. The receptivity accorded the briefings organized by the PAA [through the Population Resource Center] for leaders in government and business is an indication of the demand for the kind of information that demographers are trained to dispense.

On another front, it seems obvious that the decennial census is inadequate to the needs of a complex, fast-evolving nation. The long-denied quinquennial [mid-decade] census may not be feasible, but the need will have to be met. Imagine taking pictures of your grandchildren only at ten-year intervals!

In higher education, there are opportunities for some important academic missionizing. Any social science department that doesn't offer its students solid work in substantive and technical demography is shortchanging them. The limits and potentials of social organization are set by population parameters, not deterministically but in terms of a matrix of possibilities. I've been away

from the textbook market for a while, but the last time I looked at it there was little to choose from that would do the job that needs to be done. Some do a reasonable job of bringing social and economic factors to bear on population, but do less well in tracing out relations in the other field. Too much demographic theorizing in recent times has assumed a timeless, placeless setting, with no limits to demographic response except in terms of estimated elasticities.

Population education at the secondary level of education is important for an informed electorate. What I've seen of population education tends to put my teeth on edge. It is mostly concerned with conditioning students to recognize the individual and collective need for family planning while not saying so directly. Perhaps PAA should take a close look at what needs to be done in this area.

VDT: Since you're working on India, what hope is there for India really getting down to replacement fertility and population stabilization? Do you believe, for instance, the World Bank's latest projections that show China still in first place in 2100? Their earlier projections showed India surpassing China by 2025. What do you think?

KANTNER: They will undoubtedly run neck-and-neck in the next century. But as Frank Notestein might have said, sucking on the temples of his glasses and sighing impatiently, "Does it--the difference--really matter?" Their rates of growth are important, of course. But which comes out on top for the dubious honor of being the largest will merely be fodder for the journalists of the day.

On the question of India's rate of growth, there are parts of India--Kerala, areas in Karnataka and Tamil Nadu, Punjab--where fertility has declined impressively. The problem stems essentially from the failure of the northern states that make up the Hindi belt to follow suit. An increase in demand for contraception, already evident in urban areas and in parts of the south, can be expected to develop elsewhere in India, perhaps more rapidly than many now would expect. When and if that does happen, it will be essential to have a good family planning program in place. In spite of years of effort, I am afraid it must be acknowledged that the efforts of the government in this regard have been enormously disappointing. There is no shortage of trenchant diagnoses of what is wrong with the government's program, but in this area, as in many other areas in which the government is engaged, they seem not to know how to deal with their own people. One could say that about the Chinese also, except there the leadership is not as restrained by the norms of democratic society.

VDT: A final question. You're living in this beautiful house, built in the 1870s. Tell me why you moved to Bedford, Pennsylvania.

KANTNER: Chiefly because we didn't want to live in a large urban area. Jane and I feel at home here. We both grew up nearby and we have had a summer place in the mountains near here that is the psychological epicenter for our widely scattered family. So much as we have a repository for shared memories, this is it. In addition, I enjoy the sense of closure that a small town like Bedford has. As someone observed, in a place like this when someone you meet on the street asks how you are, they wait for your answer.

And I'm quite busy, professionally as well in other ways. I've just been named president of this year's [1988] Bedford Springs Music Festival; that happens every summer.

VDT: You mentioned that you have the musical son, the flutist, with the Grand Rapids, Michigan, orchestra. Are you yourself

musical?

KANTNER: I used to be a jazz musician years ago--the trumpet. I still get it out occasionally.

Though I am sometimes chagrined by not having my own fax number and some of the other stigmata of professional engagement, serve as my own secretary, pay my own way to professional meetings, drive a couple of hours to the nearest good library, have a book budget that must compete with the grocery list--je suis content.

GEORGE J. STOLNITZ

PAA President in 1983 (No. 46). Interview with Jean van der Tak at the Population Reference Bureau, Washington, D.C., January 20, 1988.

CAREER HIGHLIGHTS: George Stolnitz was born and grew up in the Bronx, New York City. He received his B.A. in economics from the City College of New York in 1939, worked a year with the New York City Department of Health, another year with the Census Bureau, and then went to Princeton as the third Milbank Memorial Fund Fellow (following John Durand and Ansley Coale) in the Office of Population Research, where he received his M.A. in economics and demography in 1942. Returning to Princeton in 1946 following wartime service in the Air Force, he was a research associate at OPR, received his Ph.D. in economics and demography in 1952, and taught economics until 1956. Since 1956, he has been at Indiana University at Bloomington, where he has been Professor of Economics, Director of the International Development Research Institute, and continues as first Director (since 1986) of the Population Institute for Research and Training. As a demographer and development economist, he has been a consultant to many agencies and institutions, including the Agency for International Development, the Departments of Health and Human Services, of Energy, and of Defense, NICHD, and other U.S. government agencies, the Ford and Rockefeller Foundations, the United Nations, and UNFPA. He has served on the Technical Advisory Committee of the Census Bureau and on the committee for the 1960 census monograph series. He has been a senior research associate with and later consultant to the population activities unit of the Economic Commission for Europe in Geneva and a principal officer for population and development at the UN Population Division in New York. He has also been a visiting scholar at Resources for the Future and at the Population Reference Bureau in Washington, D.C. In his research and writing, he is particularly well known for his perceptive work in the area of mortality and he has also worked and published on economic-demographic interrelations, migration, and world population trends.

VDT: We are speaking at the Population Reference Bureau in Washington, D.C., where George is the Andrew Mellon Visiting Scholar for 1987-88. George, what led to your interest in demography? You started out in economics. You must have been one of the early breed of economists who were interested in demography. Traditionally, many of the earliest demographers in PAA came out of sociology.

STOLNITZ: An interesting exception to that is Frank Notestein. He came out of economics at Cornell [Ph.D. in social statistics, 1927] and studied under a leading economist of that period, H.J. Davenport.

How did I get into population? It was in a sense by happenstance. I took no population course as an undergraduate at the City College of New York; I don't think any such course was offered, as a matter of fact. The day after I graduated, in the late spring of 1939, a bad time to get a job, I was by chance able to land a job with the New York City Department of Health as a sort of population statistician. Basically, my work was with vital statistics--mortality, morbidity, and so on--by health district: how many births, how many deaths. It wasn't profound demography and the job was for one year until the person I was replacing returned.

Then I got a civil service job with the U.S. Bureau of the Census, working on the 1940 census. This is what really started me in demography. It brought me to Washington, where my first boss, the one who had the biggest influence on me--Abe Jaffe--introduced me to historical and contemporary demographic research analysis on the 1940 census and historical censuses.

After a year, I decided to go back to graduate school. This was in 1941 and I was accepted in the Harvard economics department. On a Thursday when I was all set to go to Harvard the following Monday, Abe Jaffe asked if I'd be interested in going to Princeton; they had an immediate opening for a Milbank Fund fellowship. That very Friday I went to Princeton, Frank Notestein and I met, and by Friday evening I was a Milbank Fellow. Monday I showed up at Princeton rather than Harvard. That's really how I got into population seriously.

VDT: You had come to the seat of population training at that time.

STOLNITZ: That's right. Those were war years and there weren't many people around in 1941 and 1942. I managed to earn my M.A. in economics in 1942, went away to serve in the Air Force, and came back to the Office of Population Research in 1946. I resumed working on classes, taking more courses at OPR, no longer as a Milbank Fellow but one supported by government grants to the military.

Then I became a research associate at OPR, where I did my thesis on a mortality subject--how to derive a life table from a single census only, namely, assuming no information that you could use on deaths by age. This was then, and still is, the situation in quite a few Third World countries. If you want to get an approximate life table under such circumstances, how would you do it? That was the thesis I did under Frank Notestein and a number of economists in the department, with outside evaluation by Mortimer Spiegelman of the Metropolitan Life Insurance Company.

I received my Ph.D. in 1952, taught at Princeton for three years in the department of economics, and then went to Indiana University, where I've been teaching economics of various kinds, mainly development economics, population courses, and graduate economic statistics. In the late 1960s and early 1970s, I headed up the International Development Research Center at Indiana for about six years. In 1986, I became director of the Population Institute for Research and Training; the acronym is PIRT, which may be the best contribution I've made so far for the Institute. Happily, we received both inside--Indiana University--and outside--Hewlett Foundation--support at an early stage; the future of the Institute appears promising.

Beyond this, I have consulted widely on population with a variety of agencies and institutions. In the case of government, it's been the State Department, AID in particular, for which I did four missions in India.

VDT: What kind of missions?

STOLNITZ: In India, the mission essentially was to see about the status of family planning programs, the state of population knowledge there, what kind of policy prescriptions seemed to be working and which were not working. I went there in 1967 and 1968 and again in 1969 and 1970--about four years running.

I was also on the Technical Advisory Committee for the Bureau of the Census in the late 1960s, early 1970s, for about five years; served on the 1960 Census Monograph Committee for the Social Science Research Council; have consulted with the Ford Foundation, the Rockefeller Foundation; also for the Department of Health and Human Services; and have been for years on review panels for the National Institute for Child Health and Human Development. In other economic connections, I've long consulted with other U.S. government departments, such as Energy, Defense, and so on.

VDT: What did you do for them?

STOLNITZ: I've consulted with them on population policy, on economic-demographic interrelations

programming, served on review panels for them, consulted with respect to the kinds of steps they should take in the teaching of economics and population, or, rather, the teaching of population with adequate attention to economics, which is the area I'm most specialized in.

In more recent years, I've done a number of things for the UN, both in New York and Geneva. I was a principal officer for population and development at the Population Division in New York from 1976 to 1978, where I authored its 1977 Concise Report on the World Population Situation: New Beginnings and Uncertain Ends. In 1974 I was a senior research associate with the population activities unit of the Economic Commission for Europe in Geneva, where I wrote the first half of the supplement to its 1974 annual survey, dealing with Europe's population outlook to the year 2000. Since that time, I've consulted with that unit on aging and population change with respect to health care, household approaches to aging analysis, labor market-aging research needs, "echo" effects on current aging tendencies in developed regions, and several other things.

Still other activities? At Resources for the Future where I was a visiting scholar, I developed a system for analyzing migration movements among regions and industries and combinations of the two by using multi-year Social Security source materials to derive longitudinal conclusions, that is, tracing individuals over successive years--data which I believe I pioneered in. This was especially useful and uniquely indicative in that I could trace people longitudinally, with respect to incomes, doing so on a scale never before traceable for such large numbers and categories of population.

VDT: You're talking about U.S. data?

STOLNITZ: Yes, the data that the U.S. Social Security Administration keeps confidentially for individuals by their true Social Security number. They have the confidentiality problem and I wanted to trace individuals from annual tapes which required identification numbers so I could say, "This is what individual A was doing in 1960 and also what he or she was doing in 1968." Well, the Social Security Administration had to be--and became--persuaded to work out a system whereby they used garbled numbers in place of the social security identification numbers. Each individual A with a true social security number X was given a consistent garbled number Y. This enabled me to trace about a million people in the 1960s by industry, counties, and combinations of the two with respect to economic determinants and migration consequences. So far as I know, no one else has done that on a comparable scale. I still think--I'm stubborn about these things--I would want to do another such study, updated, before I'm through researching.

VDT: Could you do this by industry and much else?

STOLNITZ: I could trace individuals by industry down to four digit levels by counties and in ways that couldn't be shown from any other source--by economic earnings, cross-classified by whether they moved or didn't move, stayed or didn't stay in the same industry or region; I could do so by race, age, and sex subgroupings of the population.

VDT: All this was attached to the garbled number you had?

STOLNITZ: Yes. An individual with a garbled social security number in 1960, say, was of such and such race, sex, age, lived in such and such county, worked in an industry of four-digit level--which is very detailed--earned so much through wages or self employment, and this information was available at all successive dates under study. Moreover, this could be identified for combinations of counties in ways that no other source could provide--by regional groupings, of course, but also functional groupings like metropolitan and nonmetropolitan areas, urban/rural areas, central city and suburban

counties, and so on, and similarly for industries and income levels. This provided insights into migration movements of the labor force--as distinct from population--that you couldn't get in any other way.

VDT: You said in your PAA presidential address of 1983 ["Three to Five Main Challenges to Demographic Research," published in Demography, November 1983] that internal migration was one of the neglected areas of research, in part because one does not have the data. This was obviously an ingenious way of getting at it.

That leads me back to another question: How did you get into your specialties of mortality and migration? You mentioned that your Ph.D. dissertation already was on life expectancy derived from a single census. How did that happen?

STOLNITZ: Serendipity. One day at OPR I was thinking about some problem about Brazilian data. I needed national mortality data, but Brazil at that time had no reliable mortality data for the country as a whole. I knew Brazil had censuses going way back and was well acquainted with the work of Giorgio Mortara, the Italian demographer, who had reconstructed demographic times series for Brazil going back to the 19th century, based on censuses.

VDT: You knew him already as a graduate student?

STOLNITZ: I knew of him. In fact, I may have been the first American demographer, student or otherwise, who read his original articles in Portuguese. It's a semi-obstacle; with demography and just a bit of Portuguese you can do all right. What I did was ask myself: What could I do from a single census--which was basically all I had--to say something about mortality?

VDT: What census were you using?

STOLNITZ: At that time, I was using only the 1940 census for Brazil, and then the 1950 census, similarly. But I didn't want to use the inter-censal method, because it was too unreliable if you shifted from one to the other, since there was a lot of migration. Therefore, I just used the 1940 census to work with. I asked myself: What can I make out of this in terms of reconstruction for a life table? That turned out to be my thesis, and that's how I got into mortality.

At that point I got into mortality quite a lot with respect to international comparisons, as in one of the early issues of Population Index, in those short articles they have at the beginning. Then I looked at Third World life expectancy and found out that things were happening that had never happened before in history. In doing the Brazilian pieces, I had absorbed enough background in historical life tables that I had a sense of what had happened in terms of quantitative movements. I kept finding trends I'd never found before.

VDT: Now you're talking about your Ph.D. thesis, which was in the early 1950s?

STOLNITZ: Right. In that period I had had occasion to work on historical populations, because in the late 1940s and early 1950s, all you had was prewar data and going back. I couldn't use anything from Brazil that I knew of and I had the Brazilian census, so what to do? In a Mortara article, I found he did all kinds of wondrous things from single censuses, and I asked myself: How could I reconstruct a life table? I worked out a system and that became the thesis. Since I had to use comparison life tables at my disposal, I looked at data from Sri Lanka (then Ceylon), Jamaica, Trinidad and Tobago--about five to ten Third World countries, which was all that you had where you could believe the data. And I asked myself: How come what's happening there is so different, so remarkably revolutionary, as

compared to what the West went through? That's how I got started after the thesis and put out a series of articles on international comparisons of life expectancy.

VDT: You apparently became one of the first to point out that the postwar mortality decline which was happening so fast in developing countries was probably due to public health technology, which was imported, and not to socioeconomic development, which is what the demographic transition had traditionally said.

STOLNITZ: There is another especially interesting aspect there. In doing this, I came up with the notion that demographic transition formulations, including theory, really didn't have much explanatory power with respect to mortality and, moreover, either didn't have any predictive power or very limited predictive power beyond that. Mortality was going to go down; that wasn't any large mystery. But if you asked, when would it start, how fast would it go, where would it go next, transition theory just wasn't getting us very far. If you go back to an article of mine in 1955, I think you'll find one of the early critiques of demographic transition theory.

VDT: Is that the one in the Milbank Memorial Fund Quarterly ["Comparisons Between Some Recent Mortality Trends in Underdeveloped Areas and Historical Trends in the West," MMFQ, 1956]?

STOLNITZ: Yes, you'll find it there, but one that comes particularly to mind is in the Population Studies of July 1955 and then there's a follow-up in July 1956 [together titled: "A Century of International Mortality Trends"]. I think it's in 1955, about a half page where I really get at it. Frank Notestein, I must say, wasn't too happy.

VDT: I would think so. There he was: your boss, your professor, one of the prime originators of the demographic transition theory, and you were criticizing the mortality side. Everyone was a little uncertain about the fertility--but the mortality!

That brings me to your impressions of Frank Notestein, one of the greats both in PAA and in the beginnings of demography in the U.S. What was he like to work with and be a student of?

STOLNITZ: An excellent teacher. That I was a student was almost not a real situation; it was basically just Frank and myself. It was a war year when I took the courses and a lot of men were off to war--Princeton at that time was male only. So it was one on one, but we went through a rigorous workout, basically on fertility. Frank Notestein--I picture him still--I pictured him then as a research or academic statesman, less concerned really with technical details and not really very much up on it, because in his day one didn't have to have advanced mathematics, calculus, to be a leading demographer. Leading demographers of that time--Irene Taeuber, Frank Lorimer, and so on--didn't work at the calculus level of mathematics or advanced statistics. But he was very keen on sizing up political situations, interrelations, and some of his work on the factors making for demographic transition, I think, still stand up as among the very best in the field.

VDT: Some of the factors? You came around to criticizing his demographic transition theory.

STOLNITZ: It involved what I regarded and still regard as "theory," something explanatory of quantitatively specifiable relations. One of my criticisms was that transition doctrines didn't have theory in terms of a quantitative or modeling formulation and not much of theory in a predictive sense, except directionally. Presumably fertility would come down rather than go up and stay up, and ditto for mortality. But where it would start, why it would start, how fast it would go, and what the longer-

run prospects were--for none of these did you really have a theory or body of theories. What you had was a set of descriptions, which came down to saying that before modernization birth and death rates were high, and that with modernization in adequate amounts, birth and death rates became low--that's directional--but in terms of when and even why was not deeply clarified. That was my sense of demographic theory at the time and I don't think it's much more advanced now. I wrote an article on this for a book Ron Freedman edited, World Population: The Vital Revolution [1964].

Since that time, the Princeton European Fertility Project, which was directed by another Notestein protege, Ansley Coale, has come out much more than I did in terms of stating that the theory doesn't seem to work well in terms of the socioeconomic variables that transition analysts stressed. If you looked at smaller areas, say provinces, you found areas that moved faster than other areas but you did not find that this correlated well with socioeconomic levels or trends. There's a famous case involving Spain, where a student working on its demographic transition came up with a cross-sectional pattern which didn't have satisfactory ascertainable socioeconomic correlations. He happened to go to a linguist who said, "What you have here is a linguistic separation between those that moved transitionally and those that didn't change or changed less." So all of a sudden cultural aspects became prominent, which were not prominent in the early days of transition descriptions, even in Frank Lorimer's early writings or in Warren Thompson's, the latter being the earliest transition identifier, really before Frank Notestein. After Notestein, it began to come out largely with his proteges, although it's gone far beyond that since. The field has become explosive in terms of numbers.

That's one point I want to stress to you: the difference in research scales between then and now. At one time, almost everybody who was anybody in population came by OPR at Princeton; even despite the war, pretty much they all came by. So you knew them by name and, as a matter of fact, knew them personally. Now it's become enormously larger, so there are lots of people who are very prominent in the field who don't personally know lots of other people who are also very prominent in their own bailiwick. Or they know each other by name only, if that. As a youngster starting out today, you'd know almost nobody in the field personally. That was not the case in those days. I knew Lorimer, Irene Taeuber, Frank Notestein, and soon met very nearly all the then "greats."

VDT: May I ask you about some of those "greats" that you knew personally? Frank Lorimer, for instance; he died in New Zealand just a year or so ago at over 90, still writing.

STOLNITZ: I knew Frank Lorimer in 1940 when I worked here at the Bureau of the Census for a year, before I went to graduate school. In that year, I took a course at American University given by Frank Lorimer, so that's when I first got to know him. Then, of course, I came to know him much better through OPR, where he was working on his book on the Russian population [The Population of the Soviet Union: History and Prospects, 1946]. Irene Taeuber, of course, worked directly with the OPR.

VDT: She lived in Washington; did she travel up regularly?

STOLNITZ: Yes. She was involved with [co-editor of] the Index and often wrote introductory articles on timely topics for the Population Index. She was working on Japan at that time. I remember distinctly many talks we had on what was happening in Japan. Was Japan really a breadbasket case which would never develop because of limited land, less than in California, with umpteen tens of millions of population; how could it possibly survive? This was around 1950. Times have changed!

I got to know Henry Pratt Fairchild during my OPR days, and Clyde Kiser, whose wife Louise worked there; Louise was there all the time. We had Milbank contacts; we knew all the Milbank people through the Milbank Memorial Fund annual meetings. There was almost nobody you didn't

know. Also, the annual PAA meetings were much smaller.

VDT: What kind of person was Frank Lorimer?

STOLNITZ: He was a very exciting individual. I saw him as an enormous idealist, a liberal in the best sense, old-fashioned in some ways, yet in the best sense, always. Remarkably young for his age, even though I'm going back now to about 1940, a very long time ago. Frank was then middle-aged [born in 1894], but he was like a youngster in terms of interest in people, in events, in size-up of things. Frank was remarkable in that way and as far as I know remained that way. He was also a very humane person, who understood what made people tick and who was always willing--in my experience with him--to think the best of a person. Not that he was lacking in critical faculties, but if he could give you the benefit of the doubt, he would. That wasn't something Frank and I were involved with personally, but I saw it time and time again. We'd be talking about who's good in an area or who's promising, or about leading figures when they criticized each other. Frank was always tolerant and humane in all these instances.

Irene Taueber was an enormous personality, just tireless in work, full of enthusiasm. If she ever came through a door to where you were working, she was into the latest discovery she'd made almost before the door closed. There was a constant interchange of ideas. Dudley Kirk, younger than Irene but a relative senior OPR citizen, moved on eventually to the State Department [in 1947].

Also at that time, right after the war, we had a rather remarkable group of young people assembled at the OPR. Harvey Leibenstein was one such case. George Barclay, who left population after about a decade but did some very important work on Taiwan, was another. His book on techniques [Techniques of Population Analysis, 1958] is still in some ways one of two or three of the most widely used books around. Norm Ryder was a third. There were people of that sort around. And you also had visitors coming in who really livened up the place.

VDT: Do you remember people coming from developing countries after the war?

STOLNITZ: No, I don't remember them. I do remember Hannus Hyrenius from Sweden, a well-known demographer, and John Hajnal from England, who remains very well-known. But if you ask if I remember anyone from India or Latin America, no. There was an Argentinian whose name I don't remember. Argentina at that time was dubious as to where it belonged--LDC or DC? The UN at that time was calling it developed. Since that time it's become developing; it's had severe, self-made economic setbacks. They didn't yet at the time have programs whereby, say, the Ford or Rockefeller Foundation would send students over here to study in some numbers. One exception was Agarwala of India.

VDT: After OPR you went to Indiana, where you eventually developed the Population Institute for Research and Training. Does that bring people from developing countries?

STOLNITZ: Well, the Institute is relatively new. We just became constituted formally as of 1986. Still, we already have a great deal of representation from Third World areas. Indiana University happens to be very strong with respect to African and Latin American area studies. So we're having a number of their students come by in our courses and/or for our recently approved Ph.D. minor in population studies. In my courses in population techniques, problems, and economic interrelationships, I've had a lot of representation from all over the world--Orientals, Europeans, Asiatics, Africans, you name it. And we'll have more so as PIRT develops its master's and Ph.D. minor programs.

VDT: You said you'd been at the UN Population Division in the early days, in the 1940s. Can you tell us something about the early UN involvement in population studies?

STOLNITZ: I was at the UN in the summer of 1947 as a graduate student, when Frank Notestein had taken over as temporary director of the then newly-formed population unit of the UN secretariat. Because the Population Commission served by the unit was in heavy debate--the French in effect against the Russians, with the Americans squeezed in between, about what is meant by an "optimum population"--Frank had me do a summer's worth of work on its possible interpretations. Nowadays, there's a sort of standard critique of optimum population theory, its applicability to practical policy, and reasons to criticize it heavily or even discard it for practical purposes. At that time, it was heavily espoused by classical economists, in a limited way, and by the French. The Russians were against it--part of the old Marxian dogma that population isn't a problem since people are wealth. The more people the more wealth, diminishing returns was a capitalist plot and Malthus was ineffable, preferably kept out of sight from the viewpoint of rational analysis--all obvious grounds for argument between the Russians and the French. The Americans--Frank Notestein and myself, based on training as a graduate student--couldn't buy any of this. But the question was: How do you formulate for the Population Commission an approach which says, "If you mean something about optimum population, what's the most you can say for it?" I went into the various problems about formulation and ambiguities. John Durand, another Princeton product, inherited the problem. John was also running what became the Population Division of the UN during Frank Notestein's tenure. [Durand was assistant director of the population unit/division under the half-time directorship of Notestein until 1948, followed by P.K. Whelpton, and became full director in 1953--until 1965.] At the time, the going was rough. There was a lot of concern as to whether the population unit would remain as a separate branch--a lot of opposition.

VDT: This was the late 1940s, at the beginning?

STOLNITZ: It extended into the 1950s. The feeling of Irene Taueber was that John was heroically defending the role of population in the United Nations. And I would say from what I know--not long ago I had occasion to review 40 years of research in the Population Division--that John performed an excellent piece of service. It was, and today surely is, largely unknown, unsung, inadequately evaluated, and an excellent, enormously important job of furthering demographic research, not only in the Population Division but in the field at large, through the research and statistical programs he introduced at a very early stage in the 1950s, when there wasn't very much to work with, and, as I say, a good deal of internal opposition about what role concern with population should have in an international body. I think the profession owes John a considerable debt, which some people know about, but not many and not enough.

VDT: You also went on to say that a chief source of our demographic knowledge, our data, in a way the fountainhead of research, has been the UN Population Division.

STOLNITZ: Right. And Durand built it up at a time when it could easily have been discontinued. Perhaps it didn't seem like much, because there wasn't much by way of data to be had at the start. Still, John pioneered in many ways in getting surveys started. It's not well enough known that the Population Division did pioneering work in getting missions started in Third World countries that were based on surveys. Nowadays, it's commonplace, but if you go back to the late 1950s and early 1960s, it was pioneering.

VDT: You mean the KAP [knowledge, attitudes, practice of family planning] surveys or something like that?

STOLNITZ: No, before that time. I'm going back to the days of the Mysore study [begun approximately 1950, conducted by C. Chandrasekaran, then head of the UN Delhi office]. That's when it was parlous to consider whether demography would play a role in UN considerations other than in technical or statistical aspects of the field. John, I think, was a very central figure in making it work and branch out, despite considerable conservative opposition.

VDT: That's good to know. Did you ever work with Phil Hauser?

STOLNITZ: No, but I have known Phil since the days when he was already one of the great figures and I was a mere fledgling in the field. I knew him first from my 1940 census work when I was at Census; he was assistant director at the time [assistant chief statistician of the Population Division, becoming assistant director of the Census Bureau in 1942]. But I knew him after a while at OPR, when I got to know him well. Phil was another very important figure, but that's another story.

VDT: Going back to the UN, was John Durand the first director of the Population Division?

STOLNITZ: Technically, Frank Notestein was the first director. Notestein went back to Princeton, still consulted, but John Durand took over. Frank had me in there because he was still consulting or may have been director still as of the summer of 1947 [called "consultant-director," 1946-48].

Another big issue at that time was who was going to put out the Demographic Yearbook. Should it be the Population Division or the Statistical Commission? Eventually, as we all know, the Statistical Commission won out, though the Population Division has provided much informational input to it.

The Division was just getting focused on what was going on in the world of population. As far as the Third World was concerned, it was just beginning to collect data. I think I knew about as much as anybody--I say this quite modestly--about what life tables were around, what they were showing. But, of course, the number of life tables in Third World areas that you could find--let alone the number you could use--was very limited. I don't think you could go through your fingers and toes and find that many with adequate reliability for establishing trends, differentials, or, in many cases, even one-time situations. The very first life table based on a survey that I remember using was for the Belgian Congo, now Zaire, the first such entry, I believe, found in the Demographic Yearbook. And this was based on a 4 percent sample only, clearly not enough for many of the requisite age-specific rates for completing a life table exercise. You also had the Indian life tables, though here again there were major questions of reliability, in this case about time trends, infant mortality, and sex differentials. You did have, fortunately, Ceylon--now Sri Lanka--Trinidad and Tobago, Jamaica, and that about ended the list of life tables you could use, since there was no indirect estimation, practically no surveys, and too soon, often, for series of censuses. It's completely different now. If you look at the 1948 Demographic Yearbook, you can see everything the UN knew of and could publish going back to 1900 for all countries in the world. If you look for anything outside of Europe, North America, Australia and New Zealand, you'll find very little. That's what I and others were working on in those days.

What's happened since as a result of a flood of censuses, new vital registration systems, and innumerable surveys is that we have today, in one book, comprehensive biennial assessments of world, regional, and national population estimates and projections, with uniformly organized information relating to size, age structure, vital rate characteristics, and urban-rural composition. You can get all

this on a computer and analyze in one day basic data that I couldn't handle in years of research in population back in the latter 1940s when I started. Certainly not in mortality, not in fertility, and really not in any topic on comparative international population change or characteristics. The UN about 1950 developed a code on the quality of the vital statistics, which has appeared in the Demographic Yearbook ever since. I happened to review the first Yearbook with such information for the Milbank Memorial Fund Quarterly, where I paid tribute to this important innovation.

As to surveys, we now have these in the many hundreds. In the U.S., the Bureau of the Census was just starting its survey series in the 1940s. If you want to see the difference between then and now, you need only go back to the P-25 series, the P-20 series of the 1940s, to realize how primitive we were then compared to what we are now in terms of data availability and access.

Both of these aspects have been critical for research advance. Censuses were available for individuals and households, but you couldn't sit there and transcribe by the many thousands.

To illustrate this point, let me cite a counter case. When I worked for the Bureau of the Census in 1940, Abe Jaffe decided to look into fertility among the Mormons in Utah in 1850. At this point, confidentiality was no longer involved, so Abe could assemble a team to do case-by-case transcriptions from the original census forms. I headed up the copying team. Since this was not exactly a priority project, the women used were basically elderly ladies who had been given special access to jobs for the U.S. Census Bureau because they were widows of military service people. I'll never forget a little lady, 60 or so, who introduced herself by announcing that her name was Pope and quickly added, "but I'm no angel." I remember struggling over understanding an item about location until I discovered that the double "s" in Massachusetts was written in a very different way before the Civil War. The point is that we actually did a written equivalent of a fertility survey by meticulously copying data on number of children, associated, as best we could identify, by father or by mother, among Mormon families. And, although a minor affair, this went on for months. Today any schoolchild, undergraduate student, or graduate enrollee taking a first population course would immediately want to get on the phone and ask, "Where's the tape on this?"

VDT: You obviously have moved along with the changing times. This talk about the explosion in technology and numbers of people in the field leads into my next question: What do you see as the main issues in demography over the years you've been in the business? How have those issues changed since World War II?

STOLNITZ: Well, there've been lots of changes. Let me again emphasize initially the importance of data availability and their creative contributions to what one can deduce. It's not just that you have something for today, but you can evaluate it better because you also have something for yesterday. When I started, there was nothing on Africa. You looked into Kuczynski's book on Africa [Demographic Survey of the British Colonial Empire, volumes 1 and 2 on parts of sub-Saharan Africa, 1948-53], a work which was anecdotal by today's statistical standards, even for Africa, where data conditions are still far from satisfactory. So the availability of data, the ability to use it, has been one major facet that has transformed the profession.

A second major difference, I would say, derives from this. Whereas at one time you had to deal with nations or large units like states or provinces as the only available units of observation, today we can get down to individual and household levels. What used to be a macro-oriented field almost exclusively is now micro more than macro because of this revolution in availability of micro data and computational abilities to analyze them.

VDT: Perhaps micro and macro work side by side?

STOLNITZ: It's easy to infer that as a possibility. But the case remains that it's only recently, in the

fertility area, that researchers have given serious thought to how they can incorporate both micro--household--levels of data and what are called externality or contextual variables. As I discussed in my PAA presidential address, this issue still hasn't been straightened out even conceptually, in my opinion, as to what's wanted and what's the best way to go about it. The World Fertility Survey, almost by happenstance, has some contextual variables along with its household data core, but everyone so far as I know agrees that much remained to be demonstrated and executed to achieve improved analyses. We're not at the beginning stage of this issue, but we're also not far into an intermediate stage which would identify a best approach. That's another major area of focus compared to the early post-World War II decades.

VDT: Micro-level household data have become so available, particularly on fertility?

STOLNITZ: Also increasingly on mortality. You now have on tape data that I had to work months on with a cracker-jack clerk on a calculator, who worked endless hours without complaint, accuracy was perfect. That was a person by the name of Erna Harm at the Office of Population Research. It's worth having her name in the annals somewhere; she's done lot of good work for the profession.

VDT: I know the name. That's how you pronounce it, with the umlaut over the "a"?

STOLNITZ: Yes, umlaut over the "a". Her work involved a minor fraction of what we now have available and it took us months to complete--a factor of ten to a hundredfold in terms of amount of time to see the results. Today, it's overnight, if that.

Another very major change has been the recognition that population policy is something legitimate, much less something analyzable. This is really a product of the 1950s and into the 1960s, largely the result of pioneering UN work, lobbying in fact, by Milos Macura, a former head of the Population Division. At first, the UN could not, without damage to itself, talk about getting involved at policy levels in family planning programs or other policy areas. Before 1965, it was even dangerous for a UN agency or individual to get into policy. Now policy is very much to the fore as a special concern of a great many people in the population field. There are a great many policymakers, leaders but also analysts, what you'd call applied demographers as well as policy analysts proper with a professional specialty in demography. That's a very important postwar aspect.

Family planning programs, of course, basically date from about 1965, the mid-1960s, and in many countries more like since 1970. This is an enormous research field now, as witness entire publication series on family planning.

VDT: Is this legitimately part of the field of demography?

STOLNITZ: Well, if demography is what demographers do, certainly that's legitimate. I would say it's legitimate methodologically too. If you ask about theory in demography or, if you like, about abstract, objective, non-pragmatic research--where it's going, what it looks for--we find a great amount of this is generated by looking at the fact of policy needs. Take, for example, evaluation of family planning programs. In my opinion--not every demographer would agree with this by a long shot--if you want to know really what happens in early fertility transition in micro-level detail, not just statistically speaking but in terms of how the process actually works, a large part of what we think we know now and a large part of what's been documented has come out of servicing family planning program needs. What does it mean to say that somebody is exercising family planning decision making or thinking or behavior of any kind? If you look at the early transition theoretical or descriptive formulations, you'll find them miles behind what we've found because of such things as a family planning person asking: What do I call a family planning user or non-user; how do I define this?

This opened up the whole area of acceptors and how do you measure them. What do I mean by a family planning program? What do I mean by population policy? You weren't defining the latter as of the mid-1960s and we were still debating this as of the 1970s. And we still argue--much as we should--about the distinction between policy which works in response to population change or as a determinant of population change. Political and management disciplines are other aspects of the family planning area. Psychology as a significant source of explanatory variables has by now a place in population analysis, though not nearly enough. I think there's a return now to economics in increasing degree.

VDT: Return? You said before we started this interview that demography began with economics.

STOLNITZ: Well, the first social scientists, so to speak, were economists, since societal analysis talked about population even before Malthus and these were economists, primarily, in addition to social philosophers. Even Plato talked about the optimum size of a city state--about 5,000, if I recall. The first great demographic figure is obviously Malthus. If you look at who was great in the 19th century, you'll find some statisticians like William Farr in England, but the important work that commands most attention is John Stuart Mill, for example, an economist, and there were others, notably Wicksell, the Swedish economist at the turn of the century. Optimum population theory was clearly an economist formulation, miscast, but nevertheless much at the center of population-economic international analysis up to at least World War II. Increasingly since then, sociologists and population statisticians have become paramount numerically.

Today, I'd say, because of central interrelations between population change and economic development, population change in relation to development planning specifically has come to the fore, with enormous programs of research and application under way at this very moment. I was consultant, by the way, for the United Nations Fund for Population Activities, for whom I gave a major paper in its Mexico City forum on population policy in 1987 on the subject of population growth effects on development planning. Today, in order to incorporate population units in development planning units, the ones you need to anticipate dealing with mainly are economists. If you're going to train people to work there, you've got to train them in economics to a considerable extent, in addition to training them in demography proper. Indeed, that kind of retransformation of focus has occurred in the UNFPA's program within the last few years. I happen to know this pretty well, because I've been a consultant with UNFPA and indeed gave a series of lectures on the subject at the UN regional training program in Trivandrum, the capital of Kerala state in India.

VDT: Do you think that the growing emphasis on population and development planning may be one of the reasons that demography has become so quantitative in recent years? Do they go hand in hand? Demography always was quantitative, of course, but you mentioned that some of the earlier leading demographers--Notestein, Irene Taeuber, and others--tended to be transition theorists, not statistical modelers.

STOLNITZ: Irene was a sociologist. So was Lorimer. Notestein happened to come out of economics but it didn't carry him very far in his demographic research.

But I think you're onto something important when you ask about methodology. It happened that the economists had pioneered since the 1940s in econometrics, the use of formal statistical models for explaining economic behavior over time or cross-sectionally. It's the sociologists who caught up later on, when they began to branch out in terms of path analysis and other regression-type methodologies, in which, in fact, sociologists have led the way.

The original work on basic regression-type statistical models, examining the relationships

between causes and effects in the case of fertility, for example, followed along lines laid down by economists. What's happened since is that sociologists and demographers increasingly use modeling procedures for which they still have to go often to econometric sources but also to sociometric sources to formulate demographic models. This now runs across fertility issues, as in use of WFS data, mortality increasingly, and migration where, by the way, economists have perhaps been more prominent than sociologists in formulating theories of why people move, since it's often an economic issue, though one with sociological aspects. Now with recent policy concerns with aging and its very important economic consequences for the low-mortality, low-fertility world, you'll find still more economists stepping in. Aging ties in with labor markets, with fiscal policy, with rapidly rising costs of meeting health care needs, hence with medical economics.

VDT: So you have sort of been in the vanguard since the beginning?

STOLNITZ: Well, around, anyway.

VDT: I'd like your reminiscences about PAA and your connections with it. Can you remember when you first became associated with PAA, your first meeting, perhaps?

STOLNITZ: I think the first time was when I first came to OPR; this goes way back to 1941.

VDT: The 1941 meeting was in Princeton [May 16-17, 1941].

STOLNITZ: Right. I was half guessing, but right. Then the big names were Fairchild, Lorimer, Notestein, the two Taeubers, and so on, so I met them. That was the very first one. It was a small meeting; it was wartime also.

VDT: The next one, in Atlantic City in 1942, was the last one and then they closed up till 1946. Were you in Atlantic City?

STOLNITZ: No. In [May] 1942, I was cramming for my Ph.D. field exams, just before leaving for military service. Then I was at the PAA meetings in 1946 and 1947 [in Princeton]. But much more than those I remember 1941, because I was the awkward student gaping at the big names. It was sort of a roundtable. Of course, it was almost laughable by today's standards of PAA meetings, where you have five to seven concurrent sessions [nine in 1990 and 1991], which range in terms of size, availability of seats, from 50 to 75--which would have encompassed all of the 1941 meeting easily--to several hundred, often only with standing room or sitting on the floors.

And, again, you knew everybody, or Frank Notestein, who was very good at this, would introduce you. He had a great belief that personal contacts would have effects both ways: I to people who were already in the field and the people looking at who the newcomers might be. Another very basic figure then, whom I knew at the time, was Fred Osborn, co-author of a Lorimer-Osborn demographic text which still has useful material [Dynamics of Population, 1934], and who was important in terms of providing support for OPR, both monetary and institutional, as well as being a key player in encouraging foundation support for population generally, for example from the Milbank Fund.

VDT: What offices have you held in PAA?

STOLNITZ: Well, I was president in 1983, first vice-president in 1979, and then there's a long hiatus

back to when I was second vice-president [1961-62]. And there were other offices besides; when you become president, you automatically serve for a while as a PAA Board member. I have also served on various committees: for example, the nominating committee and one which decided who should be funded to go to a quadrennial IUSSP meeting abroad.

VDT: Can you remember some of the best meetings you've attended?

STOLNITZ: I've always come away fired up: "It's high time to get into this, get into that." Or, why don't I do this in fertility or in policy, or why don't I rewrite a paper or read one I first heard about at the meeting. It's always been an exciting occasion in that respect.

However, it gets harder and harder to keep up with at a PAA meeting today, since it really has proliferated in terms of people, concurrent sessions, and papers. There's an enormous competition now for getting papers accepted. The year I was running the program, 1983, I found that so many people were trying to get a paper accepted for presentation compared to what could be accepted that many such applicants had to be disappointed. Even so, I'm always refreshed in terms of what I learn is going on actively and would like to get into somewhere. I'm always learning of things I should know. Some people, I'm sure, do far better than I do in terms of keeping up, but the meetings enhance the urge to try.

VDT: You are one of the rare demographers who has been sort of working on your own elsewhere for a long time, as a demographer-economist.

STOLNITZ: That's right. But I think in general even those who know a lot more about the field than I do don't know all about it. Today, if you want to know who's doing what that's useful in the field, you've got to rely on at least several phone calls, or maybe more than several, in just one area. The big stuff you can find out easily--a call to Washington or to somebody who's heading a committee, say a National Academy committee--and find out these things very fast. But if you want to know who are the good graduate students coming up, what's a new program that's being launched, who's making a trip for, say, a field study of immediate or potential interest, it's kind of hard to keep up. You're dealing with hundreds of people today, whereas in my early career you were dealing with maybe two or three dozen. It's no longer the case that the U.S. or Western Europe has to educate all Third World demographers. There's still lots of such educating to be done, but there are a great many well-trained, competent people out there already in the field.

One of the things I did, by the way, when I worked for AID on missions to India was to consult with the UN Indian regional population center in the Bombay metropolitan area. If you were talking about who's going to do what, what Third World demographers know, how much data could they give me, how much did they need, what would they do when they went back--in those days it basically came from international foundations and AID and, more recently, UNFPA. For a decade or two after World War II, there was almost no developing area conducting research of world-class stature.

That's now changed. There are now world-class people in all three developing continents and in places like Latin America, there's a lot more than that. The Colegio de Mexico and now CELADE have top-flight demographers, both seasoned and training students competently. And to know all that is now practically impossible unless you make field trips. There are people like Ron Freedman who gets around in Asia, but he won't know Africa or Latin America. So you're getting specializations now.

VDT: Do you regret that--not being able to keep up with everything?

STOLNITZ: No. Although it was certainly psychologically pleasing, in a way, to feel you knew all of what was going on, it was only at the margins that you were really doing so. Today you are simply excluded even from the margins. You quickly learn that all the behavioral disciplines are this way-- sociology, economics, and so on. Population after all is so small compared to these, but in much the same fix by now.

VDT: That's right. You must know from being an economist, the American Economic Association is a multiple of PAA.

STOLNITZ: That's right. That raises another point. Look at the publications now in the demographic field. At one time we had Population Studies in English and Population in French; that was all. Now look at the proliferation of journals that come out. In my days at OPR, I remember distinctly that when a book on population came out, we all knew of it, we all could read it within a month; we could stay reasonably well on top of the field. Nowadays, just to read the titles of what comes out, let alone read the books, or just to be able to read a preface and size up summaries and conclusions has become impossible. It's more than a full-time job. What's inevitably happened, of course, has been a very heavy trend toward specialization and sub-specialization.

You ask me if I regret it, and of course I do. I'd like to know an awful lot that I know I'll never have time to really encompass even at a superficial level, much less in depth.

I remember once that Frank Lorimer, in the latter 1950s, acting on the thought that biologists and demographers ought to get together, helped organize a Cold Spring Harbor symposium, from which a book came out, with contributions from world-class representatives from both disciplines. Cold Spring Harbor was a leading center of biological research and Frank Lorimer's feeling was that the two fields didn't meet, despite their enormous overlaps.

VDT: What was the general topic?

STOLNITZ: Interrelations between demographic behavior and biological and genetic determinants, primarily. I don't think the organizing idea took hold; it didn't evolve into an ongoing thing. Geneticists and biologists went their way, as did the demographers. Only in recent years have they started to come together, as people like John Bongaarts, Jane Menken, Mindel Sheps, and so on have done useful work on biological and biostatistical aspects close to demography concerns. Also some biologists have gotten involved with demographic areas of behavior of special interest to biologists. Obviously, each has a lot to teach the other.

VDT: What are you doing now [1987-88] as visiting scholar at the Population Reference Bureau?

STOLNITZ: What I'm doing this year at PRB is two projects. In a sense, I've come back to mortality more than ever. I've always promised myself in the last ten years that although I've continued to write on mortality questions in all these years, really digging in for a full year's uninterrupted work on mortality on two levels would be of primary interest to me.

A first project, which is almost done now, is how mortality affects aging. How do you analyze it in ways which link them up most directly from explanatory, descriptive, and policy viewpoints? We all know there's a lot of aging going on, also much mortality change, but a system for analyzing how specific mortality changes lead to changes either in numbers or in proportions of population by age, that system hasn't been worked out satisfactorily, so far as I know. I've worked out a framework that I think does the job for any population, LDC or DC. But more than that, and what I'm working on particularly for the U.S. and want to do next for other low-mortality nations, is to focus on the fact that there has been what I call a new stage of demographic transition, involving a radical change in the age

function of survival rate percentage changes. Whereas the main such declines used to be in the infant and young ages, this can no longer be the case with declines of any magnitude that occur in the future. Why? Because under age 50, there's not much room between actual survival rates by age and their upper bounds of unity, i.e., the rate when nobody dies between successive ages, such as 0 to 1, 1 to 5, 5 to 10, and so on.

In the United States, and it's true of 15 or 20 other countries, if no woman dies under age 50, life expectancy would go up by about only two years. That's a small fraction of what's happened in the past. For example, life expectancy since 1900 in the U.S. has risen from something like 50 years to close to 80. This is a gain of 30 years. But now if anything big is going to happen in U.S. female life expectancy, by which I don't mean a 30-year increase but, say, even three- or four- or five-year increases, it will have to happen mainly because of survival rate changes at the upper ages of life, that is, over 50 years--unlike what used to be the case until not so long ago, say for a decade or two after World War II, when the main concentration by far of survival rate percentage changes was in the young ages of life.

VDT: Which is happening now in developing countries.

STOLNITZ: Yes. The developing region patterns today are those of the developed regions historically until about a quarter century ago, but are fast disappearing or already gone in the developed regions. There the age curve of survival chances and their effects on age proportions have changed radically. What happened before World War II in the West was that the long-run mortality decline transitions we all talk about reduced, not raised, the average age of populations, by raising the fractions under 15, raising the fractions over age 60 or 65 by lesser amounts, and maybe depressing the 15-to-65 fraction to some extent--in all three instances by not much. If you had more people surviving under age 15, they eventually moved up into the older ages, so the effects balanced out to an extent over time.

But today if you have most people surviving only after reaching 50, there's practically no compensating effect from added numbers in the younger ages. There are not many more young people than there would be whether older people died at 50, 60, 65, or survive beyond these ages. What's happening, therefore, is no longer just more surviving at all ages in greater numbers but not much in altered age proportions, but a piling-up tendency at the elderly ages only, uncompensated at the lower ages. So what is happening--no surprise to me, though it seems to surprise projection-makers--is that the most rapidly increasing age group in the United States is the 85 and over sector.

VDT: In actual numbers?

STOLNITZ: No, in percentage rates of change. Not in absolute numbers, because there are so relatively few aged 85 plus that a 1 percent increase under 15 can swamp a 10 percent increase over 85 in terms of absolute numbers. Of course, once the post-World War II baby boom cohorts move into the elderly ages, the absolute number increase is also going to be exceptionally large--I think far beyond what anybody is anticipating, including our social security projections people who are exploring whether social security funds will be enough. Although present plans are to accumulate trillions of dollars for the 2020s, the dollars needed when the baby boom people retire will be greater than that. Much greater, in my opinion, because there are going to be many more people in the retirement ages, and especially the late retirement ages, than we anticipate.

Not only will the number of baby boom survivors to old age be much larger than that of any of their predecessor old age cohorts--this is anticipated, from the relative numbers of births in these cohorts. But further, and not sufficiently anticipated, is what is going to happen to the age function of percentage survival rate changes. Where we used to have reversed-J age functions, they have become

J-shaped as the probability of surviving, say from 75 to 85, has suddenly jumped after decades or maybe a half century of stagnation. The reasons involve medical advances now and clearly in prospect. Through medicine, there's in effect conventional technological sources of longevity gains, but there are also dietary changes, less smoking, more exercise, less alcohol consumption, as tomorrow's elderly cohorts implement their own chase after ever greater longevity. Behaviorally, we already can begin to see this to an extent: today's 70-year-old is like a 55-year-old of 40 years ago.

My own guess, and I won't be around to either prove or disprove it, is that the 90-year-old of, say, the year 2025 is going to be like the 70-year-old or 60-year-old of today. Why? Because of an entire range of health-related changes. Now, whether that's good or bad . . .

VDT: That was going to be my next question.

STOLNITZ: It certainly raises problems.

VDT: It does, indeed.

STOLNITZ: The problems raised are being taken up by a two-volume set of studies I'm editing and contributing to for the UN Economic Commission for Europe in Geneva. However, to deal with such problems, one has to consider first the demographic magnitudes and causes that may be involved, and this has been the focus of my work at PRB, based on what one sees for the U.S. for 1945-85, also on what is readily foreseeable for the next 40 years--barring major holocausts. I'm prepared now to say with no small confidence that the mortality-induced aging linkages now in prospect would make the 1945-85 linkages piddling. True, that's what I foresee, but is also what makes this work exciting.

VDT: Is this one of the important highly probable transitions that demographers can point out policy-wise--warn that it's coming?

STOLNITZ: Yes, particularly since the likely dimensions of this transition process are greatly reinforced by what is happening not just here in the U.S. but throughout the lowest-mortality developed regions.

Not so incidentally, by the way, analogous such developments may with surprising speed begin to emerge in the Third World, as diffusion of health-related technological advances moves way ahead of the rate of development advances. An excellent example is China. And this is by no means a single case: add, surely, the Pacific Rim "tigers" or "dragons"--South Korea, Taiwan, and others. When I wrote the UN's 1977 Concise Report almost 15 years ago, I was already surprised to see that the most rapidly increasing population age sector in the Third World also occurred well up among the elderly ages. Although the data are not all that reliable, they are probably indicative. Add to the Pacific Rim cases Sri Lanka, where perceptibly rapid aging is going to happen shortly, also Jamaica and Trinidad and Tobago--three standard examples of especially rapid mortality declines right after the war. Mexico also is going to get swamped by elderly population issues, though this isn't well enough realized yet.

I hope with the framework I'm developing, more countries will be motivated to start looking for the facts and possible policy responses. You will get a lot happening at the infant and early childhood ages, but you're also getting a lot happening at the elderly ages, to a degree that didn't happen in the West. Demographic transitionists have not really taken this up yet, since they haven't analyzed mortality-age composition linkages as they should have for the West when considering non-Western possibilities.

Finally, it may be a matter of interest that I have a computer system which basically anybody

can use and, within a matter of hours or certainly in a day or two, work out what might happen under alternative scenarios of how age composition is likely to be altered by likely mortality transitions.

The other project has to do with the fact that I still think--I've been heavily criticized about this, which goes as far back as my 1950s articles--that there's enormous room for mortality decline in high-mortality countries faced with low, stagnating, and even retarded development, provided they compensate for this by what I call health-related technological inputs. I'm not talking about hospitals; the Third World can't afford hospitals. I'm talking mainly about public health and sanitation programs, cheap public health mass programs--ORT [oral rehydration therapy] for children, for example. If you exert efforts to teach people how to protect the lives of their young, you can do a lot to knock down infant and child mortality, even beyond where it's gone down, as has often been the case since, say, 1950. What I want to do is work out an analytic system for identifying possible tradeoffs revealed by experience between developmental change patterns and health-related improvements or inputs that can lead to three, four, or five years of added life expectancy under LDC circumstances. That's the second project I've started at the Population Reference Bureau.

VDT: That's sort of going back to what you'd already identified, that after World War II, it was public health inputs that had the largest impact on LDC mortality trends. Even in those countries that aren't developing much, or at all, because of rapid population growth and/or non-demographic reasons, perhaps if you just introduce more ORT, etc., mortality will go down?

STOLNITZ: The main issue here is not dollars, though obviously dollars count and money or affluence can help. The issue here is political will and organization, because feasible technologies for major results, if implemented, exist already. Even within severe budgetary confines, they can do a great amount. Needless to say, I'm not talking about civil war, hence I'm not talking about Ethiopia and its periodic, largely war-induced or ethnically-derivative famines. Even here, I'd note, recoveries have been remarkably fast, largely owing to powerful, yet cheap, technological antidotes to lasting injuries. Even in such worst cases--add the Sahel--infant survival and longevity are normally well beyond what they used to be, so that where we talked about life expectancies of 30 to 35, it's more like 40 to 50 in lots of very, very poor countries. China is another outstanding example, very poor if you really average its regional income levels, but it has life expectancies of between 65 and 70. They say more like 70; I'll settle for 65. I remember when 65 was high right here in part of the Western world, as I guess you also remember.

VDT: Yes, indeed. So all this means you've found a lot of satisfaction and excitement in your career.

STOLNITZ: There's been much of this.

VDT: And you're obviously not looking ahead very soon to real retirement.

STOLNITZ: I'll continue to direct the Population Institute for Research and Training--PIRT--for a while longer. I have a history it happens, very personal and not demographic, in that the last two sabbaticals I had I did work for others. I directed a center way back in the 1960s and early 1970s in which I edited books of others to get their publications out. My stay at the Population Reference Bureau has been the first sabbatical when I could say I'm on my own, able to answer to my four walls--a good, good feeling. No hostile interruption. There are things you can get in Washington which you don't get in Indiana.

On the other hand, we now have in Indiana for the first time since I've been there some very good demographers, as well as historians who are interested in population. We have, it happens, one

of the active groups of historical demographers in the country and are also especially active in mortality-morbidity areas. There's an economic demographer, a historical demographer, one in sociology, and an anthropologist with unparalleled long-term direct observation of mortality and associated social transitions in an African village area.

VDT: Who are they?

STOLNITZ: Core people with me at PIRT include George Alter out of Michigan and Pennsylvania, who has been publishing in depth on Belgian historical demography and getting into U.S. aging historical questions, for example, with the use of insurance records to improve what we know about upper-age mortality in the U.S. early in this century. True, we have the U.S. life tables, but you can't trust them for the advanced ages, for which the death rates come from extrapolations to an assumed end of life. As to trends rather than levels, you especially don't know what you've got. George Alter had the idea of going to insurance records, medical and health insurance records, which are reliable because they're administratively run and affect dollar returns; also to go to insurance companies and companies that have health programs. You can learn how many stayed away from work because they were sick, how many died, and so on.

An historian, James Riley, is doing analogous work based on British records. Laurel Cornell works on Japanese historical demographic patterns and family sociology. Jerry McKibben is the state's demographer for providing estimates and projections, plus recently providing attempts to enhance the accuracy of the state's 1990 census. Phil Cutright has turned his long-practiced skills for unearthing previously undetected or inadequately explored relationships to the question of child homicide phenomena. John Kennedy directs a program of ongoing social surveys with the state. Elyce Rotella, an economist, is interested in historical demography, women's roles in the labor force and other women's studies.

VDT: And you have your historical interests. You worked with Abe Jaffe on the historical censuses, and the Mormons, who have their magnificent records going back to when the first Mormons came.

STOLNITZ: That's right. You're looking at the eyes that have looked at the 1850 census, which later appeared as an article by Abe.

But that's nothing like getting today to micro sources, as I stressed earlier with respect to this project and my own work on earlier life tables. Apart from computational and accuracy factors of prime importance, we have much richer--I didn't mention this but I should--theoretical frameworks now to work with. There are many more hypotheses, both tested and untested.

VDT: You said the demographic transition theory was little or no theory. What theoretical frameworks are you referring to here?

STOLNITZ: There are many middle theories as to what's the role of this and that variable affecting population, let's say at the household level. There is much more sophistication about which variables are involved and how to treat them statistically than we had before.

As to demographic transition theory, I certainly didn't see it as the be-all and end-all in theoretical terms, and I don't now, but it has had a very major role in enhancing empirical descriptions of what actually happened. It also was the first indication of possible main explanatory variables, which have led to theoretical formulations of much more limited compass. But if you mean, do we have a predictive modeling that enables us to put transition statistics to work, my answer is no, not yet.

VDT: Well, perhaps that's what will make demography continually interesting.

STOLNITZ: Yes, since we're discovering things about fertility that we didn't know 40 years ago, such as with the Princeton European Fertility Project.

In another connection, mortality, I'd say that I came up with things, at an early stage, with respect to mortality transition possibilities in backward areas, that you simply could not read into what the demographic transition people or other analysts were saying before, when they simply referred to public health plus medical technology and improved levels of living as main causal factors of such transitions. That wasn't good enough either for explanatory analysis or policy guidance purposes.

VDT: Well, it's wonderful that you're here in Washington, having such a refreshing year, and we'll expect to hear from you for decades to come.

SAMUEL H. PRESTON

PAA President in 1984 (No. 47). Interview with Jean van der Tak at the Population Studies Center, University of Pennsylvania, Philadelphia, June 14, 1988.

CAREER HIGHLIGHTS: Sam Preston was born and grew up in Yardley, Pennsylvania (across the Delaware River from Trenton, New Jersey). He received his B.A. in economics from Amherst College in 1965 and Ph.D. in economics from Princeton in 1968. From 1968 to 1972, he was Assistant Professor in the Department of Demography at the University of California at Berkeley. He was at the University of Washington in Seattle from 1972 to 1977 as Associate and then full Professor of Sociology and Director of the Center for Studies in Demography and Ecology. For two years, 1977-79, he was a section chief in the Population Division of the United Nations in New York. Since 1979, he has been at the University of Pennsylvania, where he has been Professor of Sociology, Chair of the Sociology Department (1985-88), Director of the Population Studies Center (1982-89), and Chair of the Graduate Group in Demography (1980-82 and again since 1989). Among his many other professional activities, he has served on the influential Committee on Population and the Committee on Population and Demography and other committees of the National Academy of Sciences and was a Council member of the IUSSP for two terms, 1982-89. His honors include election to the National Academy of Sciences in 1987 and receipt of PAA's Irene Taeuber Award for Excellence in Demographic Research in 1983.

Sam Preston is particularly well known for his research and writing on mortality and demographic measurement techniques and has also worked on urbanization, the economic consequences of population growth, and the implications of aging populations for the well-being of children and the elderly. In addition to a long list of articles and book chapters, his publications include such monographs as Causes of Death: Life Tables for National Populations (with Nathan Keyfitz and Robert Schoen, 1972), Mortality Patterns in National Populations (1976), Patterns of Urban and Rural Population Growth (1980), Vital Rates in India, 1961-1981 (with P.N. Mari Bhat and Tim Dyson, 1984), Population Growth and Economic Development: Policy Questions (with Ronald Lee and Geoffrey Greene, 1986), and Fatal Years: Child Mortality in Late Nineteenth Century America (with Michael Haines, 1990).

VDT: We are speaking at Penn's Population Studies Center on a hot June Tuesday morning, though it's nice and cool in here.

Sam, let's begin at the beginning: How did you become interested in demography? You are a prototype of the small but increasing breed of economic demographers. Do you think the first one was perhaps Joe Spengler? Although Notestein technically had some economics as a graduate student.

PRESTON: I'd say Malthus was the first one.

VDT: Okay! That's certainly beginning at the beginning! How did you become interested in demography?

PRESTON: Well, I went to graduate school in economics at Princeton.

VDT: How did you get from Amherst to Princeton?

PRESTON: Princeton had a good reputation as an economics department and I won a Woodrow

Wilson fellowship coming out of Amherst and had applied to Harvard, Yale, and Princeton. Princeton was closest to home and seemed most attractive.

VDT: Where's home?

PRESTON: Home is Yardley, Pennsylvania--Bucks County--just across the Delaware River from Trenton [New Jersey] and then Princeton.

I remember looking through the Princeton catalogue which listed the economics department and looking at the courses they offered and there was only one course that I was sure I was not interested in taking and that was demography. The only one I could cross out; everything else looked terrific.

Toward the end of my first year at Princeton, I was deciding I was heading into labor economics and somebody told me, "Well, if you're interested in labor economics, a useful secondary course would be demography." So I signed up to take Ansley Coale's two-semester course in demography, and literally after one lecture, I was sure that I wanted to be a demographer. That abrupt! If I'd gone to an undergraduate school that had sociology I would probably have majored in sociology, but Amherst did not have sociology. I think it might have had political science, although I'm not sure about that. The main social science there was economics. I'm not sorry that I majored in economics; I think it's a good rigorous undergraduate major. But as soon as I could get away from prices and interest rates to birth and death rates, I made the switch. It seemed much more concrete and interesting to me, involving sex and death and all those deeper emotions and processes.

My recruitment to demography was aided by the fact that the Office of Population Research was a congenial environment, which was separate really from the rest of the university and had its own library. I could go in and sit in that library and concentrate on demography readings, whereas all the other economics readings were in this horrible room in the basement of the Firestone Library, where you were competing with scores of other graduate students for access to the readings; it was just really quite unpleasant. So I think the environment probably had a little bit to do with my conversion as well.

VDT: So you were lured by OPR's library, in addition to everything else.

PRESTON: I was lured by Ansley Coale's clear expositional style, his obvious enthusiasm for the subject. And also I think it just struck a responsive chord with me as something that was more pertinent, more engaging probably, than economics, especially labor economics. As economics was taught then, at least at Princeton, it was kind of an old institutional style and didn't really have a lot of rigorous accompaniments; it was kind of throwing ideas together. But you get to demography and you have these very rigid accounting identities, expanding into the stable population model and you know these are truisms--these are not hypotheses. These are exact identities that define processes in human populations. I think the rigor of it attracted me as well as the fact that it was dealing with very interesting materials that economics seemed awfully dry in relation to.

So that's that; it was really a very quick conversion. And I did well in the courses because I was very interested in them. After passing my Ph.D. exams at the end of my second year [at Princeton], I went and talked to Ansley about promising dissertation topics and he said he'd been trying for several years to get somebody to do a dissertation on cigarette smoking and its impact on mortality patterns. That was a fascinating idea to me and I immediately went to work on it. I actually tried to go to work, as I recall, in the summer, probably a month after my exams, and Ansley said, "You really should take a longer time off after your exams; nobody should start a dissertation right after their exams." But I ignored this advice and started to work on a variety of things. It turned out to be an excellent dissertation topic.

VDT: And a book!

PRESTON: And a book, subsequently, by the University of California Press [Older Male Mortality and Cigarette Smoking, Population Monograph No. 7, Berkeley, 1970, and Greenwood Press, 1976].

VDT: You managed it in one calendar year, summer to summer?

PRESTON: Yes, summer to summer, basically. I left for Berkeley [in 1968] without having defended the dissertation; I had a bit more to do in one of the chapters. And I came back at Christmas and defended the dissertation and it was accepted.

VDT: Great. What were your data?

PRESTON: Well, I put the data set together. It was death rates for Western European countries and overseas European countries since the 1880s, when the data set began. I just really went to different libraries and put together these rates. It was an aggregate-level, every-five-years, data set.

VDT: Rates on mortality, but where did the cigarette smoking come in? Did you have something on emphysema, or what?

PRESTON: The Surgeon General's report had come out in 1964, I believe, three years earlier, so that was a good guide to some of the references. And there was suddenly a lot of interest in cigarette smoking as a health hazard. What I needed to do was put together the mortality data with data on cigarette consumption by country and trends therein, which involved some reconstructions--tobacco use converted into cigarettes per adult. Then it was a matter of going to the epidemiological literature and trying to figure out how much risk would be associated with a certain dosage of cigarettes and then converting the national consumption patterns into estimates of excess mortality. And then see at the end whether the excess that would be predicted from the epidemiologic studies was identifiable in the national mortality patterns. The way I tried to do that was to associate older male mortality with mortality of older females, who were not nearly as susceptible to cigarette smoking, and younger males, whose mortality would not have been very much affected by smoking.

So the idea was to create correlations and then see whether a correlation that typically existed would be disturbed by added cigarette smoking. And the conclusion of the dissertation was that they were obviously being disturbed. Older male mortality wasn't improving anywhere nearly as rapidly as would have been expected based on what was happening to females or younger males.

VDT: You were one of the first to point that out, which became a hot issue?

PRESTON: At the aggregate level, right. I think it probably is one of the only studies that has been on national-level mortality as it would be impacted by cigarette smoking.

VDT: Does that explain your continuing interest in mortality?

PRESTON: Yes, I think it certainly had a lot to do with the fact that I worked extensively in mortality subsequently. You build up capital in a particular area, sources and more studies. And the World Health Organization asked me to do several things after the smoking book came out and that opened up some new areas.

Then I had gone to teach at Berkeley and Nathan Keyfitz was there and he was interested in multiple decrement life tables. He came to me and asked if I had any data that I could give him so he could illustrate the use of multiple decrement life tables in a book he was doing. I said, "Sure, here's some data; there's a lot more where that came from." In fact, nobody had ever systematically investigated cause-of-death patterns in national populations. He and I decided sort of on the spot that that would be a worthwhile activity.

So I set about to acquire, assemble, process all the data I could find on age-sex cause-specific mortality, as far back in time as possible.

VDT: In Western European and North American countries?

PRESTON: Yes, and Japan and Latin America. Latin America started to produce some reasonably good data in the 1960s and 1970s. It's the kind of project that now you know how complicated it is to try to do, but at the time I was very naive and just thought, "I'll just go to some libraries and xerox some pages and then try to get the data into uniform categories," which turned out, of course, to be a very big job.

The end product I saw, more than the tables that would come out, was some kind of overall assessment of the mortality decline in terms of causes of death, the contribution of various causes to the decline, the role of various causes in sex mortality differentials. So this ultimately led to two volumes. One was a volume of tables that I did with Nathan Keyfitz and Bob Schoen, who wrote the computer program as a graduate student at Berkeley, and Verne Nelson, another graduate student at Berkeley, who did an awful lot of the detailed tabulation work. That came out as a 770-page book [Causes of Death: Life Tables for National Populations, 1972].

VDT: While you were still at Berkeley?

PRESTON: Yes. Then I took the data and started to do a series of analyses of the data and that came out ultimately, after I'd left Berkeley, as another book [Mortality Patterns in National Populations, 1976].

VDT: So there you were, not yet 30, and you had produced two books!

PRESTON: Yeah. In fact, I guess three, if you include the cigarette smoking book.

VDT: Of course, and the one with Nathan Keyfitz, and the Mortality Patterns in National Populations. That got on the map.

PRESTON: Yes. I think the mortality patterns book was a good thing for me to try to do at that stage of my career, because it involved me in having to read a very wide range of literature in trying to interpret these patterns that were emerging. For a while, at least, I felt as though I were on top of the mortality literature. But other things subsequently have come to the fore in research; mortality has kind of split recently.

VDT: Then what brought you to the University of Washington, Seattle? What was the job?

PRESTON: Berkeley was a great place.

VDT: Who else was there besides Nathan Keyfitz at that time?

PRESTON: Judith Blake was the chairman of the Graduate Group in Demography [Chair of the Group in Demography, 1965-67, and of the succeeding Department of Demography, 1967-72] and at least by my final year there [1971-72], we were the only three members of the department. We were the only three members of the department in my first year there. I think in the second and third year . . . [Etienne van de Walle and Paul Demeny were there for periods as visiting professors.]

VDT: You mean what is now Ronald Lee's group [Graduate Group in Demography] at Berkeley?

PRESTON: Right, but at that time it was actually a department.

VDT: That was the famous undergraduate department too, wasn't it?

PRESTON: We did do undergraduate teaching also, right. I think it was the only department of demography in the country, to date. [Georgetown, at least, now has such a department, established in the mid-1980s.]

It was slated to go to seven faculty members, as I recall, but we never got that high and as a very small department, it was quite vulnerable to personality conflicts, ideological conflicts. Students--this was the Vietnam period of radicalization of the student body and nowhere more than at Berkeley--and the students had certain expectations about what the faculty ought to be doing and those expectations were not universally shared, although they were partially shared, by the faculty.

VDT: They wanted you to be out demonstrating too?

PRESTON: Well, that's exactly right. They thought the faculty should be participating in this important activity, and I agreed with them, and so did Nathan.

VDT: Hmm--and that third member didn't. Okay.

PRESTON: So it was a very exciting time. We had some terrific students at Berkeley. I didn't know it at the time. You come out of graduate school and you expect everybody to be of the caliber that you knew in graduate school and Princeton had some awfully good students in economics. The students in demography at Berkeley were every bit as good.

VDT: Who were some? You mentioned two who worked on the book.

PRESTON: Bob Schoen. I think Geoff McNicoll is one of the best students I ever had.

VDT: He's the Australian?

PRESTON: Yes, just went back to Australia recently. Griffith Feeney, now at the East-West Center in Hawaii. Bob Gardner, at the East-West Center, was my first dissertation student.

VDT: I know Bob well. He was author of two of my Population Bulletins, on the Asian American population and immigration.

Well, it must have been an exciting time. Did you leave with regret?

PRESTON: Yes, I did. But I think the stresses of living in the Bay area probably were pivotal in my deciding to leave. It was just a wild time, it really was, and I think the California life style didn't fit me

all that well.

VDT: A product of Pennsylvania and Amherst and Princeton.

PRESTON: Yeah, well . . . Seattle seemed more stable. And I had my own center to run there.

VDT: Were you married by that time?

PRESTON: Yes. I actually married right out of undergraduate school, so I was married to start graduate school and had a child at age 23, while I was in my third year at graduate school. My wife was a nurse and was supporting me through the first two years of graduate school. We had another child in Berkeley and went to Seattle [in 1972] and had our third child.

VDT: Which is the one who is a musician?

PRESTON: Second child.

VDT: I think middle children often are. I told you about my musician son; he's the middle son.

PRESTON: Seattle was a lovely place to live. I grew very attached to the University of Washington as an institution--magnificent campus and there seemed to be a unique environment. The Center for Studies in Demography and Ecology occupies a suite of offices in the basement of an older building. I think my most vivid memory is that they never washed the windows in the basement offices, so you'd be staring up through these muddy windows at a gray sky, invariably spitting rain. The weather in Seattle got me; it got to me in a major way.

VDT: Except that you say it was a beautiful campus.

PRESTON: It was a beautiful campus and on nice days there's no prettier city in the country than Seattle. But there weren't many nice days.

VDT: The only time I've ever been there was during the [April] 1975 PAA meeting. You'd left by that time?

PRESTON: I was there.

VDT: And that was a beautiful week. We all said, "Where's the rain?"

PRESTON: It was spectacular.

My wife was very attached to Seattle, and we were certainly going to stay there for good. We'd made that decision, although I don't think I was particularly happy to be there.

VDT: You were quickly made a full professor [1975], by the time you were 30, 32--something like that? That was pretty phenomenal, wasn't it?

PRESTON: Thirty-one, I think. Yeah, it was pretty unusual.

VDT: And simultaneously made director of the center?

PRESTON: Well, I came to Washington at age 28 as director of the center.

VDT: At 28! I think I've heard of that--the youngest director ever of any population center.

PRESTON: I was advantaged by having gotten out of Princeton very quickly.

VDT: I should mention that Ansley Coale holds you up as a prototype--three years from the B.A. to the Ph.D. He doesn't think anyone should ever take longer. And he holds you up as an example--you and Alvaro Lopez, who did it even faster.

PRESTON: Right. Well, Princeton actually had an institutional commitment to trying to get people through in three years.

VDT: You mean from the B.A.?

PRESTON: Yes, not just in economics. At that time. I don't think they are attempting to do that anymore.

VDT: Oh, no. Recently I talked to one graduate student there who was telling me under her breath, "I've been here five years." You're not supposed to say that out loud.

PRESTON: Exactly. But also my career path was speeded by the Vietnam War, because if I dropped out of graduate school--which, like most graduate students, I seriously contemplated doing--I would have gone to Vietnam. And that prospect kept me with my nose in the books.

VDT: I see, good point. So you were offered the directorship of the center at Washington. They came to you at Berkeley?

PRESTON: Yes, that's right. At that time, Nathan was pretty much decided to leave for Harvard and Harvard was going to make me an offer too, for an associate professorship without tenure. And Michigan had talked to me about a job and I was very interested in going to Michigan, but they were not ready to make me a tenured offer. And Washington was--in addition to making me director of the center, which was . . . It was a mixed blessing. I don't know why people want to be directors of centers, at this point. It really is a lot of fundraising activity that's required. Now, I'm overstating the case, but being director of the center was not one of the attractions of going to Washington, whereas being a tenured faculty member was, you know, attractive.

VDT: By tenured faculty member you mean you're on a tenure track?

PRESTON: No, I went with tenure.

VDT: Had you been at Berkeley?

PRESTON: No. I think I would have been. They had started tenure proceedings and it was well along and I had been hearing noises from the committee that I was going to get tenure, but I didn't wait around to find out what the verdict was going to be.

Actually, those were the days when tenure was not all that important, it didn't seem to me. I

didn't realize. It's true that I went to Seattle with an offer of tenure, which was attractive, but it never occurred to me that that's what the goal was in an academic . . . for an assistant professor. Probably in part because there were no other assistant professor in the department at Berkeley and I just wasn't socialized to thinking about an academic career. I was just thinking about doing interesting research and teaching students the best I could.

But I got to Washington and there was an obsession at that point in the department, as I recall--which I thought was very anomalous, but it turns out to be almost universal in large departments around the country--an obsession with tenure issues. By that time I had tenure and didn't need to worry about it, but it concerned me that everyone around me was as intrinsically excited about those issues as they were.

But I can understand why now, because what we expect of our assistant professors is almost superhuman--to get tenure here [University of Pennsylvania]--and we've had three people do it in population since I've been here.

VDT: Three in nine years. Is that a lot or a little?

PRESTON: That's actually a lot for a major research university, in one sub-field. All three of the assistant professors we've hired have gotten tenure.

VDT: For tenure they have to compete for a certain number of slots with people in other departments?

PRESTON: Well, if you do very, very well, there's a job there for you no matter what, so you've not competing with other departments. But we're expecting, oh, a couple of books and maybe 15 to 20 articles and other signs of distinction in an assistant professor's career that lasts about six years.

VDT: In other words, what you'd done, almost.

PRESTON: Well, I don't know whether I would have gotten it myself these days, because, as I recall, when I left Berkeley I had six articles, a paltry number by current standards, and then two books, but one of the books was a book of tables, which we wouldn't count as much as we would a book of analysis, and the other one was a dissertation, and that's also discounted . . .

VDT: Even though it became a book? They're not all picked up to be published.

PRESTON: That's right, but still it's assumed that if you're in a distinguished place your dissertation is good enough to have been published.

So I'm glad that I don't have to start out today, because I don't know if I would have made it. But that was a time when there was a lot more demand for academic talent relative to the supply than is the case today, so it was an easier time. And, as I said, I was not too concerned with issues of tenure at that point--naively unconcerned. But it was an attraction to go to Washington with tenure and then I was promoted, I think after two years there, to full professor.

VDT: And you said that the weather got to you after a while. You were teaching at that time. Had you started on your research in urbanization?

PRESTON: No, I hadn't started that at all; I was still mostly doing mortality and some fertility. Actually, the courses I taught at Washington were mainly in fertility, policy-oriented courses, which is what the market demanded. We had a course that was basically taught to all graduate students in

sociology. These are very good graduate students at Washington. It was probably the best sociology program at the time on the West coast, certainly the most rigorous, quantitatively. And it would attract very good students who had strong quantitative interests. Tad [Hubert] Blalock was, and still is, there-leading social statistician. I think his name drew in lots of students.

VDT: We were all weaned on his statistics book [Social Statistics, 1960].

PRESTON: Exactly. And I think probably the best of those students in general gravitated toward demography. So we had some wonderful students in Seattle.

VDT: Can you name some of them?

PRESTON: Yes. I think the best student there was Alberto Palloni, who's now a professor at Wisconsin.

INTERRUPTION

VDT: We're still in Washington. A last question before we leave, out of the rain, and move on to the UN. Cal Schmid was there earlier, wasn't he?

PRESTON: He was the first director of that center.

VDT: He's one of the PAA presidents [1965-66] who is sort of an outlier. He was born in 1901; he's still alive. Henry Shryock went to see him a few years ago. He's retired on an island off the coast [later in a nursing home on the mainland]. Was he around when you were there?

PRESTON: He was a vague presence; his souvenirs in some ways were still around. He had this major graph-drawing operation. He had a full-time draftsman, maybe two. An old-style demographer ...

VDT: I've heard he was very graphics-oriented.

PRESTON: Yes, so his graphs were around and we would periodically go through them and pull out interesting examples of graphics. I can remember Pete Guest, a colleague in Seattle, picked out one and put it on his door. The title of the graph was: "Indecent Exposure Arrests in Seattle, 1940." It had clusters of indecent exposure arrests on a map. So they were doing an unusual variety of things in demography. But Cal was not active by the time I got there; he was coming in every once in a while.

VDT: He was known as a sort of isolated figure there in the far Northwest, who was an early demographer.

PRESTON: That's right, absolutely.

VDT: He had risen up there and stayed there?

PRESTON: Yes, I think he was a native of the Northwest. He had built the center up in a way that emphasized service to the state of Washington, so his graduate student support came from grants given to him by the state of Washington through a variety of applied demographic work, much of it graphics-oriented.

VDT: That sort of tradition has stayed on in Washington, as a state. It had one of the first state demographers, didn't it?

PRESTON: I think that's right. Yes, a very practical tradition. But between Cal Schmid and myself was Stan Lieberman. Stan had moved the center in a much more academic direction and, of course, he's a brilliant figure and when I arrived I felt very much in his shadow for a while, partly because of his personality, which is the most expansive, humorous personality our field perhaps has to offer. But he had gone to, I suppose, Arizona at that time and left Seattle behind.

VDT: Now, what took you to the UN?

PRESTON: Well, you probably won't believe this story. The UN called me up for a job. I had apparently impressed Bourgeois-Pichat. I organized a seminar while I was at Seattle for CICRED on the effect of infant and child mortality on fertility and Bourgeois-Pichat was the head of CICRED. He came out to Washington, spent several days, and we then had the seminar in Thailand. I'm later told that Bourgeois-Pichat had recommended that the UN hire me, after John Grauman had died, to be head of the section on population trends and structure [within the Population Division, New York], and they called me up and asked me about this job. The last thing in the world at that point that I wanted to do was to leave Seattle for New York City. And I said--this was in the summer or late spring--I said I couldn't possibly leave Seattle; I had all these academic commitments for the next academic year. They said, "Well, what about a year hence?" I hemmed and hawed and said I could consider that, but I couldn't possibly consider doing it in the next year.

So nothing happened for about eight months. Then a bureaucrat from the UN, Sankar Menon, called up and said, "Mr. Preston, we agreed to your proposal." I'll never forget his language. The proposal he was claiming I had made was that the job be made available to me "a year hence." And I was so intimidated by the UN as an organization and I felt I must have made some commitment to them that I didn't remember, so I gulped and said, "Well, I guess then I'm coming."

I went home and broke the news to my wife, who was in love with Seattle, and she was devastated. But this was only to be a leave of absence. I took a two-year leave of absence from the University of Washington and I decided the UN job was for two years and had every intention of going back. That's how it was palatable for me to leave and palatable for my wife. But we were East only about three months when we realized that this was where we wanted to be.

VDT: In New York?

PRESTON: New York was not where we wanted to be, but the East was back with both of our families and back on more familiar terrain. So I resigned my job in Seattle and started looking for another job in the East.

VDT: Even though you were in the UN job?

PRESTON: Yes. I decided that the UN was corrupt and I couldn't cast my lot with them for the rest of my life. By the time I left, I was regretful.

VDT: You began to like the work better?

PRESTON: I liked the work from the start--a very interesting set of research reports that had to be

written in my section. This work on urbanization . . .

VDT: Began there?

PRESTON: Began and ended there. I did a book while I was there called Patterns of Urban and Rural Population Growth [1980]. That was what I spent most of my time on while I was there. Then I was responsible for a couple of other projects as well in mortality--the new model life tables.

VDT: Oh, you did those?

PRESTON: I didn't do them, but I supervised the doing of them and hired people, John Hobcraft, initially, and then Larry Heligman, who finished it off.

But it seemed to me to be an organization where there was a very weak association between achievements and rewards, whereas in an academic setting that is a very finely tuned association. There were people in the UN who had been there a long time and who were not productive at all and who were getting very well rewarded. There seemed to be a big random element in everything that was going on; sections were fighting to preserve their own turf or standards. And the conversations at lunch and so on seemed to be largely focused on who was going to get promoted next. It's typical, I suppose, of bureaucracy, but I had just had no exposure to it. And it just didn't seem to me that it had a place that was devoted to science, and at that point that was the chief value in my hierarchy, I suppose. So in that sense it disappointed me, although I think I had my two most productive years while I was at the UN, without question. I had a good research staff working with me. You're sitting on top of a lot of things that other people don't have and you're in New York, the hub of communication.

I really enjoyed and profited from my years at the UN. By the time I left, I realized why it had to be like it was, why any bureaucracy had to take on some configurations that make it less than totally efficient.

But in any event, living in New York was a problem. I had to commute about an hour each way--we lived in Tarrytown--old rickety train ride. It just came down to a choice of where I was going to go.

I had an opportunity to work with the Population Council, and at Princeton and at Penn. I was juggling them. I started to think maybe I should work out a half-time appointment at the UN and a half-time appointment at Princeton, but the UN nixed that. At that time they didn't want any part-time staff members; I think they have changed their mind about that. Going to the Population Council required me to stay in New York, which I didn't want to do. So it came down to a choice between Princeton and Penn, and really the pivotal part of that choice was the teaching that I would be doing in the two places. Princeton really only taught two courses in demography and Ansley Coale taught them both--wonderfully well. There was relatively little for me to teach there. They talked about getting a course in medical economics taught at the Woodrow Wilson School, which would have been an interesting possibility. But here at Penn, they'd had retirements--John Durand and Vince Whitney and Ed Hutchinson had just left--and there was just a wide open agenda.

VDT: And you loved teaching?

PRESTON: Well, I think if you're going to be on a faculty, you have to be very concerned with teaching and with what you're teaching. I won't go so far as to say I like it. I teach a course a semester, basically, and do spend quite a bit of time on that course. And here I can teach demographic methods, which I taught at Berkeley, and a course in mortality--mortality is still my major area of interest--and basically anything else I want to teach. So it really provided an opportunity for me that

no other place did, and I decided to come to Penn [in 1979].

VDT: Did you come in as the director of the Population Studies Center?

PRESTON: No, I came in as chairman of the Graduate Group in Demography.

VDT: Then you and Etienne van de Walle switched, or what happened?

PRESTON: Well, there are two administrative positions here. One is the person in charge of curriculum and the graduate students and the other is the person in charge of the resources of the Population Studies Center. I think it's a very healthy system; it divides the administration up in ways that don't make it too burdensome for any single individual. So I took over the Graduate Group and then in 1982 took over the Center.

VDT: Is the Center above the Graduate Group?

PRESTON: No, it really sits next to it.

VDT: The Graduate Group is more concerned with the administration of teaching?

PRESTON: Right, that is its exclusive domain.

VDT: And the Population Center with research?

PRESTON: Right--and hiring people, including some people who service the Graduate Group in Demography. What we needed here at that time . . . We had a center grant [from NICHD/NIH], but it was the smallest of all the [population] center grants in the country, by far--I think by about two to three.

VDT: Why was that? In the Caldwells' book on the Ford Foundation contribution to population [Limiting Population Growth and the Ford Foundation Contribution, 1986] they said that this center made an application for a \$1.2 million grant back in 1960. It didn't get nearly that much--it was a lot less [five-year grant of \$200,000], because it wasn't committed to the Third World, wasn't that it?

PRESTON: I don't remember--I wasn't here at the time--but it really has made a difference. When Ford got out of the population field, in the mid-1970s, they didn't get entirely out but they said, "We're going to stop this huge commitment that we've been making, but we've got these universities feeding at the trough and we have to help them dig themselves out before we withdraw all support." So they gave tie-off grants of a million dollars apiece to Michigan, Princeton, and Brown. For some reason which I never understood, Penn didn't get a tie-off grant and we're still suffering as a result of that. We don't have the income from that discretionary money that these other places have; it makes a difference. But that was made up in part by a grant from the Rockefeller Foundation that Etienne van de Walle had pursued in African demography.

VDT: That was important. Penn had taken a while to have an attachment to one Third World area; that's what the Caldwells pointed out. Michigan had its ties with Taiwan. Penn had its students from developing countries that came here to study in a traditional program, but did not have the ties with any one area overseas. Then Etienne built that up during the 1970s.

PRESTON: Etienne built that up and it's been quite a success, I think. But it still provided mostly stipends and tuition and we didn't have the resources for the staff. So my first job as director was to try and get the center grant expanded. We put in a supplementary request to NIH and they awarded us a much larger amount in our center grant. This was in 1983. They subsequently expanded the amount in the next round of our five-year application. So we're now in quite good shape; we're average by center in terms of NIH's support. We can provide resources and facilities for the faculty and students that I think are quite respectable now.

VDT: And you are very LDC-oriented now. The Caldwells said in their book that at the time Vince Whitney applied in 1960 for Ford money to build up the center, Penn had "the oldest and best developed teaching program in population studies in the world" [p. 54]. You had Ed Hutchinson and Dorothy Thomas. However, it concentrated on migration, urbanization, and labor force and it was very U.S.-oriented at that time.

PRESTON: Yes, I think that's right.

VDT: How did it pick up its LDC interests, which you didn't then have and was perhaps one of the reasons Ford didn't give you so much money in the early 1960s? Now, the Caldwells said, you have more less developed country students than any other center.

PRESTON: I think that's right and I think it was largely the students, but also Etienne. And my coming. By that time I had done a lot of work in Third World demography.

VDT: That came about through your time at the UN?

PRESTON: The time at the UN and it actually started in Seattle. I should mention a significant influence here that I think has been a significant influence on a number of people which was the National Academy of Sciences Committee on Population, under Ansley Coale.

VDT: You were already involved in that in the late 1970s?

PRESTON: I was involved in that from the beginning, which I think was the year I went to the UN [1977]. What it did was to provide an opportunity for very rapid technical advance in indirect estimation. Indirect estimation is an area of demography that sort of was dominated by William Brass and Ansley Coale. Coale and Demeny did the UN manual [Methods of Estimating Basic Demographic Measures from Incomplete Data, 1967] and Brass developed a number of techniques. It was developing, but very slowly until this committee was formed. The committee brought together ten people, including Etienne and myself, who were very interested in developing country demography, especially in the formal aspect thereof--estimates of birth and death rates. This provided a forum for new techniques; UN Manual 10 was produced by that committee. And it did something like 22 monographs on different countries and I was put in charge of the Indian monograph and in India recruited to Penn a student who turned out to be the principal author of that monograph, Mari Bhat, who was a terrifically talented Indian technical demographer.

PRESTON: Being on that committee, does that mean that you did a lot of research on your own that went into committee reports?

PRESTON: Yes. It turns out now that that's not the way National Academy of Sciences committees are supposed to work. You're supposed to be synthesizing other people's work, but you don't undertake original work. However, this committee was much more ambitious than most, maybe more than any other Academy committee, in the number of projects it undertook.

VDT: It came out with the famous report, Population Growth and Economic Development: Policy Questions [by Samuel H. Preston, Ronald Lee, and Geoffrey Greene, 1986].

PRESTON: That was a later document from a different committee. After the technical demography committee stopped functioning, a new Committee on Population and Demography was formed. Gene Hammel was the chair of that committee. It had two major activities, one of which was the family planning review [Robert J. Lapham and George Simmons, eds., Organizing for Effective Family Planning Programs, 1987] and the other was the review of the economic consequences of population growth.

VDT: I understand that you didn't take time within your other job to do this particular work. Were you expected to do it after hours? Did you get paid for it?

PRESTON: Good question. The India volume was done in the summers when I was being paid to do something else. The Academy really didn't have any money to support that research, but it did expect a volume and we finally came up with a volume.

The report on economic development was a different matter. I was asked to chair that committee and I thought they should have a full-fledged economist chairman. I thought it would give the report more credibility. Furthermore, I was so far removed from economics that it would not make sense for me to be in charge of it.

PRESTON: You no longer consider yourself an economist?

PRESTON: Absolutely not. I'm a demographer and if I had to be hyphenated I would be a sociologist-demographer. I went to the department of sociology at the University of Washington, and my only colleagues at Berkeley were Keyfitz, Kingsley Davis, and Judith Blake, who were sociologists. So I really never was in an environment with economists.

Ron Lee and Gale Johnson agreed to chair the committee and I agreed to write a substantial amount of the first draft of the report. They put me on the staff of the Academy for a semester to do that, although I never physically located in Washington. I agreed to give up teaching for a semester. I sat here in this office and assembled everything I could find that seemed pertinent and read through it and wrote a draft. There had been a committee meeting where some initial papers were presented, but basically the first draft of that Academy report was written without benefit of the commissioned papers. As you know, they came out two years after the report and they simply were not available at the time the report was written, most of them.

VDT: They were called your research, background papers.

PRESTON: I know, I know. One of them was far enough along to make a difference. That was by David Lam, one of the sections on income distribution. The things that we relied upon the most were IUSSP's volumes. Ron Lee had been in charge of a committee on economic consequences of population growth for IUSSP and he had done a terrific job in that role and had sponsored several conferences, papers from which were vitally important for that Academy report.

But also I attempted to read pretty widely, especially in agricultural economics which it seemed to me was an area that had been neglected by demographer-economists. It had done careful empirical studies of land-use systems as they were influenced by population: household allocation of labor and capital as it's influenced by household size. So there was an important literature that pertained to rural areas in developing countries. But, of course, the majority of residents of developing countries are rural residents, so I think the report was effective in bringing that literature to light in the context of economic and demographic relationships.

VDT: I think people must feel that was so. I think of Julian Simon growing out of Ester Boserup, an agricultural economist, and of course that was what everybody was uptight about . . .

PRESTON: That's right. The response by agricultural economists to the report was quite good. The response by demographers was very mixed.

VDT: It was, indeed.

PRESTON: They were expecting something controversial.

VDT: That report is sort of an unread report, although it's extremely readable. I think people can read into it whatever they want to find.

PRESTON: That's absolutely right. Everybody has an opinion on the report. Most of them haven't read much of it. I can remember Lincoln Day getting up--we had a session on this report at the PAA meeting--getting up and denouncing the quality of scholarship in this report and I asked if he'd read it and he said, no, he hadn't had an opportunity to see it but what he'd heard about it indicated that it was very flawed scholarship. Oh my! Then it was denounced by the president of PAA in the next session, Paul Demeny's presidential address ["Population and the Invisible Hand," 1986], which I was very upset about.

VDT: Then there was the press conference in Washington when you introduced it; that wasn't an easy time. But I think there was someone who criticized more the press release, the headline . . . Oh, that was Julian Simon.

PRESTON: Julian Simon thought it dramatized too much the role of population growth. As I see the conclusion of that volume, we argued that population growth was in general--that is to say high fertility more specifically--a negative factor in economic development, but it was not an overriding factor in economic development. One would have to pay a lot more attention to alternative strategies for advancing the rate of economic growth in order to say that population growth limitation programs were the most cost-effective way of stimulating economic development. But we all believed that family planning programs were an important component of a general development strategy, if for no other reason than that it enabled people to better achieve their own personal goals, but also because there were likely to be externalities for other families.

Those externalities seemed to me the key to the whole issue of economic consequences and we didn't get very far in pinning them down, nor has anyone else subsequently done so. Ron Lee is now in the process of trying to do that, so I'm hopeful that he'll produce some more definitive results.

VDT: You actually started that report, or the committee started work, in 1983, before the 1984 Mexico City population conference. So there you were already with what became known as the

revisionist view before the shock of it at that conference.

PRESTON: I suppose so. That's interesting. I remember seeing the first draft--I don't know who sent it to us--of the administration's document for Mexico City and reacting in horror and, in fact, writing a letter and going down with a delegation from PAA, three or four people, to meet with people in the White House to complain about this document. I could in no way endorse some of the craziness that was in that document, particularly about abortion and family planning programs. We met with George Bush's chief of staff. We made him look at this press release with this preliminary statement and he looked at it and said, "This is nonsense."

I think in many ways it was an embarrassment that some of the things in the NAS report sounded similar to things that were being said by the administration. Reagan is probably the most unpopular president among academics in the 20th century and if you sound like Ronald Reagan, you're not doing yourself any favor. But we had to go where our noses led us.

VDT: You mean some things in the NAS report, you felt, sounded a bit like the U.S. statement and policy at Mexico City?

PRESTON: Right.

VDT: But it was merely coincidental?

PRESTON: Well, it certainly wasn't because the administration had declared it as American policy.

VDT: Was it a reaction, rising to the bait thrown out by Julian Simon? The Ultimate Resource came out in 1981.

pPRESTON: Which I wrote a review on which was quite negative.

VDT: That's right. I read it recently, in Population and Development Review [March 1982].

PRESTON: I think that Julian did stimulate interest in this whole set of issues and did professional service by calling attention to the absence of empirical backing for many of the relationships that demographers had taken for granted. Or maybe--I don't want to charge demographers with ignorance--the layman had taken for granted.

VDT: It had been conventional wisdom for a long time.

PRESTON: I don't think demographer-economists have really changed their tune all that much about these relationships. As David Horlacher pointed out, the report itself sounds very much like the UN Determinants and Consequences [of Population Trends] section on economic consequences of population growth.

VDT: The one of 1973?

PRESTON: The earlier one [of 1953], but also the 1973 one, I think. Simon Kuznets was kind of an intellectual leader in the UN for a number of years and opposition was Kuznetsian in some respects. But the report has not been criticized by mainstream economists; it's been taken as a fairly obvious set of conclusions by economists. It's been the people sort of on the periphery of the population

movement, who have seen population growth as a tremendous threat to human advance, that have reacted most violently to what appears to be a more guarded assessment of the role of population growth.

VDT: How do you feel about population growth now? The Population Reference Bureau's 1988 World Population Data Sheet shows that the trend is tending toward the UN high variant; the growth rate has been stuck at 1.7 percent for some years. I know we can't use Data Sheets as trend data, but it's been stuck. Of course, we all know that China's birth rate went up and any blip in China will stick us there. But world growth seems to be faster than we expected.

PRESTON: It worries me for Africa in particular. Most of Latin America is beyond the point where the pressure of population on resources is the key concern in economic processes. Africa is not beyond that point. Parts of Africa are very poor in natural resources. Natural resources in very poor countries play a bigger role in production and the ratio of population to resources is important. I see no evidence of declining birth rates in Africa. I think they will decline in the next 30 years, but they'd be better off, most countries, if they declined in the next five years.

VDT: Let's jump to your other controversial contribution to the public arena, which was your PAA presidential address of 1984, "Children and the Elderly: Divergent Paths for America's Dependents" [delivered May 2, 1984, published in Demography, November 1984], which appeared in Scientific American a very few months later ["Children and the Elderly in the U.S.," Scientific American, December 1984]. By the way, did Scientific American come to you or did you go to them with that article, which put it into the public arena?

PRESTON: Dick Lincoln [editor of Family Planning Perspectives] was very insistent after my talk that I do something of a more popular version of it in a wider circulation.

VDT: I remember the hushed silence when you gave that speech; I think we knew that we were sitting in on something historic. You were pointing out that children were being shortchanged with federal money vis-a-vis those 65 and over. Of course, we had begun to become aware of how much the elderly were costing this country and would continue to cost, especially when the baby boomers got there in 2015, because with an aging population, low fertility, it was inevitable. However, I don't think before that time people had really thought of the implications of what happened to children. You came by that idea all by yourself?

PRESTON: I did, actually. I was casting around for an idea for a PAA talk and was probably going to do something on mortality in developing countries and thought, well, that's fine and that's what I know best, but most of the audience is probably interested in American issues and if I were able to find an American issue that seemed interesting I would go in that direction. Then a number of things that I was reading--the kind of stuff that comes across your desk heading for the library--sort of formed a pattern and I saw child poverty in particular having deteriorated substantially in the last ten years, without apparently any publicity. I had not seen anybody comment on it.

VDT: You noticed that? Now, of course, it's in the news.

PRESTON: Yes, in the news every week. And at the same time, looking at mortality rates among the elderly, which had gone down rather sharply.

VDT: That was about the time it was realized that the plateau where mortality had been stuck had changed and the decline had resumed?

PRESTON: Right. So I decided that I'd look at a number of indicators of well-being and see if there was an age pattern of change. And in virtually everything I looked at, the situation for kids had been getting worse and the situation for older people had been getting better. I thought that was an interesting phenomenon and I decided to investigate it and see if I could figure out some factors that were involved in the changes.

I think I made some headway in that. People came away from that address with a message, just as I think you said, that my main claim was that children were getting shortchanged by the federal government and the elderly were arrogating to themselves the larger share. But in fact only a couple of pages of that 22- or 23-page report was about political factors or government budget.

VDT: That's what stuck in people's minds.

PRESTON: That's what stuck in people's minds, but the address was mostly about family change for children as an influence on their well-being.

VDT: It derives partly from your own personal situation? You have children--under 18 at that time?

PRESTON: Yes, absolutely. No, I don't think it does arise as much . . . I think the pressure. I did point to the tendency of a greater burden on individuals from the older end of the scale and a lesser burden from the younger end of the scale and work through some of the life-cycle implications of that. That may well have come out of personal circumstances.

VDT: I was talking to Jane Menken yesterday and in her PAA presidential address the following year ["Age and Fertility: How Late Can You Wait?"], she talked about the woman in the middle, the squeeze, with the young and elderly dependents simultaneously.

PRESTON: Exactly.

VDT: In your case, you felt you were pointing out the demographic implications of the changing family and the policy implications?

PRESTON: I also had a long section on industrial change; talking about how the education industry seems to have been failing and I trace that--I don't know how effective or accurate this argument is--to declining youth population, which has led to a reduced demand for schoolteachers, which in turn has led to a horrible reduction in the average quality of schoolteachers. And all of those links, I think, can be documented.

VDT: Well, that must have stuck in the public mind too. I just re-read your address the other day and didn't pick that up, but that's common currency also in the press now. That must have been picked up from your paper.

PRESTON: Well, yes. Albert Shanker, the head of one of the teachers' unions, only about a month later in his column in the New York Times wrote entirely about that section of my talk.

VDT: He picked that up from your PAA speech?

PRESTON: I don't know where he got it; I think it was from the PAA talk.

Anyway, Dick Lincoln argued that I should send this to a wider-circulation journal. I sent it to the New Yorker at his suggestion and the woman from the New Yorker said she liked it a lot but I would have to personalize it; I would have to de-statisticize it and try to put in some personal examples. And I didn't feel that I would be effective at doing that. So I looked for another outlet and sent it to Scientific American, who accepted it quickly. So it did come out.

VDT: Have you written a lot for the popular press?

PRESTON: No, I haven't. I have been asked to do an awful lot for the popular press following up on that and I've done some of it. I did an editorial--I guess this is not the popular press so much--for the Journal of Pediatrics, a long editorial. I've given congressional testimony two or three times. Senator Moynihan has asked me to testify several times. He actually cited that paper very widely; I've been very flattered by all the attention he's given me. Sent me probably a half dozen personal notes and he's been a supporter.

VDT: He has been a population supporter all along. And there's his book on Nation and the Family.

PRESTON: His book on Nation and the Family starts out, paragraph one, with my paper.

VDT: Wow! That is an accolade.

PRESTON: I will say one more thing about that. All the positive press it's gotten from advocates of children has been matched by negative press from advocates of the elderly.

VDT: Well, I should hope so. Nelly Gray and her Gray Panthers.

PRESTON: Yes, and AARP in particular has taken out after me several times and their journal, Modern Maturity, second largest circulation magazine in the country, called me "America's leading crusader against the elderly" a couple of years ago. Immediately within two days, I had calls both from my parents and my wife's parents.

VDT: Oh, oh! You have all four parents alive?

PRESTON: Yep. "What's going on?" Actually, it turned out that they had hired a new editorial writer who'd picked up a file and hadn't understood the dimensions of what this file represented. And did an editorial--it was his first editorial for Modern Maturity--and got some significant features of it wrong. I wrote to them and they retracted their editorial, basically; published my letter and said they regretted the errors.

VDT: You said your parents had read it in Modern Maturity?

PRESTON: But they heard it from their friends; this is a very widely read magazine. Subsequently, they attacked me again, but this time it was on solid ground. They had a professor of political science say my political demography was full of holes. But at least it wasn't just wild charges.

And just a few months ago I defended the article at the Gerontological Society meeting; sort of called on the carpet, put before a jury of my peers.

VDT: But that's fun, isn't it? Or is it? What do you think about that, when you get hauled into the policy arena?

PRESTON: I'm thin-skinned. I do not like to be criticized in anything; I guess most people don't. I would rather they like it than that they didn't like it, and I don't get any pleasure from the combat and I've basically stopped combatting.

VDT: You mean you won't attempt to write anything controversial?

PRESTON: No. I'm going to do whatever I think is correct, but I'm not going to defend anymore. I have done a couple of follow-up things: one on Japan, looking at the same relationships in Japan [with S. Kono, "The Changing Status of Children and the Elderly in Japan"]. That's coming out in a book by the Urban Institute. The Urban Institute held a whole series of seminars around this issue of children and the elderly. They've got a very good book coming out [The Vulnerable, 1988]. Tim Smeeding, John Palmer, and Barbara Torrey are editing it. That'll be out soon. They timed it because they thought it would influence the elections.

VDT: I've heard Barbara speak on it regarding several countries. Why did you pick Japan?

PRESTON: I had done a paper on non-European industrial countries for the Hoover Institution forum on fertility change--Kingsley Davis's [later published: "The Decline of Fertility in Non-European Industrialized Countries," in Kingsley Davis, Mikhail S. Bernstam, and Rita Ricardo-Campbell, eds., Below-Replacement Fertility in Industrial Societies, 1987]. Japan was one of the countries and I got interested in Japan and then Barbara Torrey read that paper and thought I would be an appropriate author for her volume on children and the elderly. So I acquired Kono as my co-author, whom I knew through the UN, Director-general of the Institute for Population Problems in Japan. It was fun.

VDT: Have you ever lived in a developing country or any other country?

PRESTON: No I haven't. I have visited something like 40 countries, about 25 of them developing, but I don't believe I've ever spent more than two weeks in any one of them. That will change, but for 20 years I've had children in school and I have another five and it's awkward to move them around for a lengthy period of time. But I can see that changing in the near future.

VDT: You mean apropos of what Jane Menken is now doing, research in developing countries?

PRESTON: Yes.

VDT: I told Jane the story of meeting Marvellous Mhloyi, the Penn graduate, at the University of Zimbabwe two years ago--I was there with a World Bank mission--and she said, "I feel so alone." She had far too many demands on her.

PRESTON: She sure does.

VDT: She had done what sounded like an excellent study of couples in two rural areas which showed that the contraceptive prevalence was probably much lower than the Contraceptive Prevalence Survey of 1984 had found, which has been touted as showing that Zimbabwe was doing best of sub-Saharan countries. Subsequently, she and Ethel Churchill and I had some correspondence about her getting that

written up and into Studies in Family Planning. But--no time.

PRESTON: She's become an international superstar.

VDT: That's right. It was very obvious then.

Until you got into policy, I had thought of you as a technical demographer. In your excellent article in Sociological Forum ["The Social Sciences and the Population Problem," Sociological Forum, Special Issue: Demography, Fall 1987], summing up the links between population and economic development, you listed the contributions of technical demographers, things like studying deficient data, momentum, natural fertility--Bongaarts and Louis Henry--biostatistical models, and the timing of births, which makes such a difference in population growth too: The Chinese can have two children if they just time them right. Is there anything more for technical demographers to say? That's one thing I wanted to ask you. Then you felt that beyond that, we now have to draw more on the softer social sciences, even anthropology, to explain some of our links.

PRESTON: That's a good question. Any time you try to take a snapshot of the field and you look at all the interesting things that have been accomplished in the past five years, you begin to think, I wonder really if there's anything left? Fields go through periods in which they are extremely active. I would say that technical demography has been extremely active in the past ten years. The development of indirect estimation techniques for developing countries, the generalization of stable population equations to Africa or to any population, has had a number of spinoffs. Biostatistical models of conception and birth have been extremely important in understanding the intervening variables of fertility. What is going to happen next?

I can't predict that. I can see instances in developed countries where demographic modeling is going to be very important. I think mortality is an area where we really need some modeling, something I'm working on now.

VDT: You're working on something now?

PRESTON: Yes, with Gretchen Condran. We have a project from the National Institute of Aging to study old-age mortality patterns in developed countries. At this point we're in the tedious data-gathering and evaluation stage. It's clear that we're going to need some kind of modeling activities to represent the age pattern of mortality at the oldest ages and to be able to make comparative statements from one country to another. The problem in the United States is that our data are so lousy that we really need a variety of demographer's skills to try and make sense out of it. Some of the techniques that have been used in developing countries to discipline data, to make better sense out of what they're saying, I think at this point we need to be using in the United States, because our demographic data are the poorest of any industrialized country. Twenty percent of young black males are missing from the census; where are they? People are overstating their ages at older ages or misstating their ages very badly. That means we don't have a good picture of old-age mortality. Marriage and divorce statistics don't cover the whole country yet. Illegal immigration means that the demographic accounting identities that are so dependable in many other places can't really be used here very well, because you've got this slippage between the lefthand side and the righthand side.

So, yes, I think there are lots of interesting questions that formal demography can address for the United States.

VDT: So there's still work there. But you also seemed to be implying that some work like, say, the Caldwells' participant observation in Indian villages and other softer approaches in the larger picture . .

PRESTON: Right, are very important. For example, they're important in understanding where black males are between ages 20 and 30. Not to say that I'm going to be doing it. I probably have no skills whatever to bring to bear to that kind of activity, but I admire those who can, and especially Caldwell. He's one of my heroes in the field, without any question.

VDT: Do you think there's still room in demography for basic research such as you do? Of course, more and more demographers are getting jobs in the applied field, in business and state and local government.

PRESTON: I hope so. I think, though, our future lies in our ability to bring demography to bear on directly important human issues and questions. And very often that means that we've got to be more than demographers; we've got to be social scientists in a broader sense. I think that the future of our field really depends on how well we not only tend to the core of our own discipline, formal demographic methodology and so forth, but also how conversant we remain with the other major social sciences, economics and sociology in particular.

VDT: Will there be jobs in the academic field?

PRESTON: There will be jobs in economics and sociology departments. There will not be jobs--there are not jobs now--in demography departments, because we don't have any such. So, in order to train students for those jobs, they've got to be acceptable to a department of sociology and that means that they've got to do something probably beyond the core of demography. It's okay if demographic methods is a component of what's done. But there are not going to be many places--there are not many places now--for whom that's sufficient. I think demography has to be connected up with other disciplines and it has to address itself to major social issues, both in the U.S. and developing countries. And I think if we can do that it will certainly remain a vital part of the scene.

It's not guaranteed that that will happen. There's a temptation for programs to get more and more involuted. It's been a temptation everywhere I've been to teach the students more and more demography, to the point where there's not much time that they have left to learn anything else. And I think that temptation has to be resisted.

VDT: What about those who get demographic skills and use them in business and state and local government.

PRESTON: Fine, sure. That's certainly happening, and it's all to the good. I think demography has a lot to offer everywhere, because it has a sort of concreteness that some of the other social sciences lack. It has these accounting identities that are a solid basis from which to spread out and examine the world. And I would hope that it would thrive both in the business community and in the academic community. I don't see any reason why it won't, but we in the universities have to be sure to keep students looking outward instead of inward.

VDT: Who have been some of the leading influences in your career?

PRESTON: There are two key influences, unquestionably. Ansley Coale, who drew me into the field. I was infected by his enthusiasm and his wonderful expositional style.

VDT: That was both in his teaching and . . .

PRESTON: And his personal life. He's a magnetic figure.

And then I had the great advantage of going to Berkeley after graduate school where Nathan Keyfitz was. Nathan was a very different kind of character, but totally committed to science and the application of demography to interesting issues and the development of formal demography.

And both of them have always been very supportive of me. I've profited a lot from their having dealt with me as a promising young demographer. I couldn't have found two better people to have affiliated with by the time I was 26. So those were unquestionably the biggest influences on my career.

[Preston later added another influence.] Kingsley Davis was a formative influence, as Ansley and Nathan certainly were. He was at Berkeley at the same time.

VDT: You didn't mention that.

PRESTON: It was in a more peripheral role. He ran his own research institute [International Population and Urban Research], which was not part of the department of demography. On the other hand, he was there as a presence and he's a brilliant, insightful man. I really came to appreciate what effect he had on me subsequently, just in his recognition of human societies and how they construct individuals' daily lives. That was something that an economist just never thought about. It took a while, the process of osmosis, in really reading some of his writings after I had left Berkeley, before I came to appreciate the importance of that. So he certainly deserves to be added to the pair that I listed as most influential.

VDT: He's had influence on just about everybody I've interviewed so far, whether or not they actually worked with him.

How about some leading, outstanding students. You've mentioned a few in passing.

PRESTON: I think at Washington, as I said, Alberto Palloni was a leading student and has had a wonderful career already himself in demography. Here at Penn, I think we trained some excellent technical demographers. They're not well known in this country, because they've gone back to the countries they came from. Mari Bhat I've mentioned.

VDT: Worked with you on the India book. You say you picked him up in India?

PRESTON: Yes, I met him at a National Academy of Sciences seminar in India. He's gone back to Kerala, where he's working for a UN institute in economic development.

Michel Garenne, he's an outstanding French demographer who is working in Senegal now with ORSTOM, managing the ongoing--equivalent to the Cholera Research Lab [of Bangladesh]--activities of field stations of Senegal. It's been in operation for 20 years and it's produced a great deal of informative data about what's happening in mortality and fertility. Those are the two, probably, that stand out the most here at Penn.

I think to the extent that I've been successful in training, it's clearly been in technical demographers rather than substantive demographers. I don't know why, but that seems to be the record.

VDT: What accomplishments in your career have given you the most satisfaction?

PRESTON: Professional accomplishments.

VDT: You also said you're very pleased that you got Jane Menken here from Princeton. A loss to Princeton. Do you feel guilty about that?

PRESTON: Yeah, I do. I think the loss to them is as great as the gain to us, but I hope the gain to Jane exceeds the loss to Jane so she regards it as a gain.

I think probably the piece of work I'm proudest of is one that doesn't have my name on it and that is the Patterns of Urban and Rural Population Growth of the UN [published without attribution as Population Studies No. 68, United Nations, New York, 1980]. I think it was a valuable summary and analysis of urbanization patterns, about which there was a lot of confusion about the sources of growth and what countries are experiencing the impact of rural to urban migration and so forth.

VDT: And the comparison with the growth of urban population in now developed countries in the late 19th century. That was an important part.

PRESTON: I think that was probably the most satisfying volume I've produced. I had a lot of time to spend on it and a good staff.

The PAA address certainly was gratifying and the response to it. I never anticipated as enthusiastic or as vigorous a response. So that was very gratifying. And I think the points in there were basically correct and were points that probably had some small effect on the national discussion.

VDT: Certainly did.

PRESTON: The National Academy of Sciences report I considered to be a contribution. I thought also that it brought some light on a subject about which there's been mostly heat. But I think the failure of that volume is that we didn't have a clear enough conclusion that people could walk away with a message. As you say, people were able to read into that volume most anything they wanted. And that partly reflects the fact that we agonized over the wording of the conclusion and it was a committee document; the committee spent more time looking at the conclusions than it did any other part of it.

VDT: Being the one most likely to be quoted.

PRESTON: Of course. But we assiduously avoided quotable phrases and paragraphs. And that strategy means people can impose their own views on the document, because they're not going to spend days and days of their lives reading the text.

VDT: I assume that study had something to do with your being elected a member of the National Academy of Sciences [in 1987]. Is that similar to being a member of the French Academy in France?

PRESTON: Yes.

VDT: The top scientific minds in the country, I gather. And is there a top limit in numbers?

PRESTON: Yes.

VDT: Someone has to die off before someone new is elected?

PRESTON: There is a limit to the number elected each year, that is, in the aggregate set by how many vacancies there are created by death. But I think there's some possibility of expansion, small, but expansion from year to year.

But to comment on your comment. That study didn't have anything to do with my being elected to the National Academy of Sciences. I specifically asked that question and service to the Academy in theory has no bearing on election to the Academy, which is by other members of the Academy.

Two social and political scientists are elected each year. Not including economics, which has its own category, and there are several economists elected each year. The year I was elected, the other person that was nominated by the section was Samuel Huntington, a political scientist at Harvard, against whom a vendetta was launched by a mathematician at Yale, Serge Lang, and he didn't get elected. So all the publicity that year went to this guy that didn't get elected.

VDT: Not in demographic circles! Were you the first demographer that's made it?

PRESTON: Oh, no. There are Ron Freedman, Ansley Coale, Bill Brass as a foreign associate, Nathan Keyfitz and also, I believe, Kingsley Davis [yes, plus Dudley Duncan]. So demographers are actually quite well represented. I guess I'm the first of the younger generation, but I won't be the last. [Jane Menken was elected later in 1988.]

VDT: But that does make you very special. Let's jump into PAA, where you are among those who have been president at a respectably young age--you're among the younger ones--and who have decades and decades to go thereafter--if Kingsley Davis is an example, as he is in many ways. Did you see the photo of him with his baby at the PAA New Orleans meeting [of 1988] in the latest issue of Population Today?

PRESTON: Yes.

VDT: That photo was by my former PRB colleague, Art Haupt, editor of PT. The caption said: "It was noted that the U.S. birth rate had gone up."

PRESTON: It was good to see him there.

VDT: Can you remember your first PAA meeting?

PRESTON: Sure, I attended in 1968, in Boston, and Ansley Coale gave the presidential address.

VDT: That was the first meeting for Jane Menken too. And you remember his address?

PRESTON: I do. It was a very well done address ["Should the United States Start a Campaign for Fewer Births?"]. In fact, I think he departed from his normal research at that time to talk about an American issue and I think that was kind of an influence on my own choice when I came around to having to give a presidential address.

VDT: That was the issue of did the U.S. need a campaign to bring births down. Of course, in proper demographic style, he discovered the birth rate was coming down anyway.

PRESTON: But his answer was no, we don't, and not just because the birth rate was coming down.

VDT: Then we had the Commission on Population Growth and the American Future which was set up in 1970 to address that high birth rate, which of course had collapsed by the time its report came out.

PRESTON: The year its report came out [1972] the birth rate fell below the replacement level.

VDT: Besides Ansley, can you remember some of the other leading lights at the time of your first meeting?

PRESTON: No, I can't. I don't remember much of anything about that meeting except that address. We went to Jimmie's Harborside for dinner, terrific restaurant. That's all I remember.

VDT: What issues do you think have been important in PAA over the years--outstanding issues?

PRESTON: Well, I think there's been a systematic attention to policy-related issues on the programs at the annual meeting. I think there have been a lot of very good sessions that have addressed the major concerns in developing countries in particular. The Association has become more developing country oriented, or at least it had become more developing country oriented by 1980. A majority of the sessions, I think, and a majority of the distinguished demographers were spending most of their time studying developing country issues, which is very unusual for a professional association. I think probably only the anthropology association would be equivalent to that. It's one of the things that I like about the field, that we are not as navel-gazing a discipline as many others.

VDT: Do you go to other professional association meetings?

PRESTON: Yes, I do. I go to sociology meetings, public health meetings, and then occasionally gerontological meetings; the statistical meeting I went to last summer. And PAA are the best meetings.

VDT: Why is that?

PRESTON: The sessions are the best attended. The average quality of the papers is highest, I think. And there's this great communal feeling. Demographers know one another; at least they know a significant number of the people who are at the meetings. And you often only see them once a year, so it's a reunion time and the dance card is filled up from breakfast to late-night drinks, year after year. It's a very exciting time.

It comes at a bad time of the academic calendar; that's my only problem with the meetings. Right at the end of the semester and exams are getting under way and so forth. I would rather it were in, say, early June, right about now. But I think it's such a tradition that I'd leave it where it is. It certainly works **very** well as a professional association.

VDT: You're young enough to be able to say that objectively. The oldtimers always hark back to the Princeton Inn days where there were 50 to 100 people at the meetings and everybody attended the same session--there was only one. And that's a big regret, that it's gotten too big. But you still feel there's a feeling of intimacy?

PRESTON: Oh, absolutely, compared to the other larger professional associations. Demographers are in part selected on personality grounds, I think. They tend to be level-headed and yet not nearly as

intense in general as economists are, so that there is a little bit more good feeling at these meetings than is typical of many other associations.

VDT: That is a marvelous statement. It's not untypical of what a lot of people have said. Of course, it could be a measure of what it takes to become a PAA president--among all those personalities. What does it take--to rise to the top pretty rapidly, as you did?

PRESTON: I was very, very surprised when Peter Morrison called me up and said that I had been nominated to be president, because I hadn't been first vice-president and I thought that was the normal route. I had run twice for second vice-president and lost the first time to Jane. Then a year hence, I think, we were both nominated to run for first vice-president and I decided that, well, there's no point in my running against Jane again, so I declined the nomination. And then we were both nominated to run for president against each other and she declined. That was in 1983. If she had run she would have won.

VDT: Yes, I think so. All the women who were on the slate were winning in those days.

PRESTON: Yes, but I think she was also a better-known figure. And she'd already beaten me once. So if she had decided to run, I would have been delayed in my presidency.

VDT: Was that an OPR-mafia decision or something of the sort?

PRESTON: No. I'd already declined to run against her once, so I guess in some ways she thought that it was her turn to decline.

VDT: Ansley Coale was very proud that he was asked to introduce you--three in a row. There was yourself in 1984, Jane in 1985, Paul Demeny in 1986.

PRESTON: Absolutely.

VDT: I asked if he had his speeches in introducing you, because a few such speeches for past presidents have been kept and they're usually very enlightening, usually someone who has known you that well, whom you ask to introduce you. He tells me he speaks from notes and didn't have anything for the record on you.

PRESTON: He had my vitae.

PRESTON: Can you remember what he said about you?

PRESTON: I remember he told a terrible joke.

VDT: He's famous for that, isn't he?

PRESTON: I had suggested that he start off with one of his jokes. So he told the joke and there was a mild smattering of laughter. Then I said over the podium, "Well, my only regret is that I asked Ansley to tell a joke."

VDT: And then you launched into your famous speech.

Do you see any ways that PAA could be changed for the better? You say it still has a fine

attendance at sessions in particular.

PRESTON: This past year we expanded the number of simultaneous sessions.

VDT: Eight.

PRESTON: Yeah, it was too many. I think we should go back, because the attendance was down this year, in the sessions--not overall.

VDT: Scattered too thin.

PRESTON: Yes.

VDT: You say fewer overlapping sessions; what are you going to do about all those papers that people want to present?

PRESTON: Well, we could go to participation which is more restricted. I have had two participations on the program every year for the last 15 and that's not necessary. And I think we do need to worry about opening up slots for younger people, people who are not quite as well known. We could restrict participation to one paper, period.

VDT: Whether you're a single or a co-author?

PRESTON: Right, I think that would make a difference. Attendance at New Orleans this year was quite good.

VDT: Over 1,100 [1,115], nearly a record. That's right up there with the Washington meeting in 1981 [1,167] and the New York meeting in 1974 [1,110], which was the first time they went over 1,100.

PRESTON: That could have something to do with there being more sessions, in fact. There is a causal relationship; people have more opportunity to present. So, who knows? Maybe we're in a new era. But I would say that we ought to give that experiment a little longer--look at it very hard.

VDT: I've heard that what's often most missed now by those who have been there a long time is more informal debate in the sessions, as in the past when Kingley Davis invariably took off after Frank Lorimer and so on. Which is, of course, impossible now. It's been suggested that the panel discussions, which are an attempt at debate, are degenerating into paper sessions too.

PRESTON: I think that's right. And I think it partly reflects the fact that our field has become more normal science--more people who know how to do a paper that is to be presented at the PAA. It is not as speculative as it used to be and it is not drawing people into it who have a more speculative bent of mind.

VDT: Good point. Hadn't heard that.

PRESTON: I think sociology, for example, is still drawing those people. But demography has become a bit more routinized. That may be reflected in the styles at the professional meetings and the style of presentation.

If you look at our younger people--I don't want to disparage our younger people, by any means, and I would approve myself of that group--we don't have the kind of broader background that Kingsley Davis or Frank Lorimer had. You're being recruited to the field in part because of our technical skills. And I think that what's happening in large part at the annual meeting. I would like to see an opportunity for people with somewhat broader concerns to be recruited into the field. I don't know how to stimulate that, because our cirricula have gotten more and more technical and more and more narrow. It's a concern that we need to address in the future. Maybe the people who are going to be the kind of intellectual leader you're talking about are going to be drawn from other fields and they're going to have gravitated into population studies--much as I think Caldwell has done, for example. I do see him as a current intellectual leader of the field. He's a bit younger than the generation of superstars that we're talking about.

VDT: He is a bit younger?

PRESTON: Yes, he's 61 or so and still very vigorous and he's still out there in the field and I would say over the past decade he's perhaps made his greatest contributions to population studies. I'm a great admirer of him.

VDT: Is he now just on the Council of IUSSP or he's now president elect? No, Livi Bacci will be the next president.

PRESTON: He is not on the Council because he elected to run for president and then he lost to Livi Bacci [at the 1985 IUSSP general meeting].

VDT: Of course, I remember that. Yet he had tremendous backing.

PRESTON: He won the American vote.

VDT: Indeed, and I thought the developing countries too.

PRESTON: Yes, I think that's right. But the meeting was held in Florence and there was a huge European vote and that went to Livi Bacci [as an Italian; versus Caldwell, the Australian].

VDT: That brings us to IUSSP. You are very prominent there, the only American on the Council at the moment, and you have been on several of their committees. I see you have a paper slated for the IUSSP African conference of 1989. What about IUSSP?

PRESTON: It's a very vigorous association also, and I think the most vigorous of the international associations in the social sciences, without any question. With a huge budget. I compared the annual budget of IUSSP to the American Sociological Association and IUSSP is bigger. Isn't that surprising?

VDT: Where's it coming from?

PRESTON: It's coming from donors, European donors, UNFPA, the U.S. to a small extent, but it's coming from governments mostly.

VDT: Quite a bit of that money, though, goes to bring LDC people, move them around to meetings?

PRESTON: Yes, some fraction of it; I would say 10 percent of it, perhaps. A lot of it is for committee meetings.

VDT: Do you find your membership in IUSSP worthwhile?

PRESTON: Yes, I do. I think it's been a very important association. As I mentioned, the seminars that were organized by Ron Lee I think were pivotal in the National Academy of Sciences report. I think the set of volumes from IUSSP over the past 15 years has been a very distinguished set of contributions to the professional literature.

VDT: You include in that the papers that are presented at the meetings as well as their separate volumes?

PRESTON: Yes. I think they stack up against the last ten years of, let's say, Population Studies or Demography. They've made as big a contribution. The problem has been circulation.

VDT: Definitely, people don't know about them.

PRESTON: We tried to solve that problem by going with another press, Oxford University Press.

VDT: Supposed to sell more. The committee reports, for instance, every once in a while I read them in the Bulletin of the IUSSP. They're fascinating material but where else do they appear?

PRESTON: Right. It's a problem that I don't think we've resolved because Oxford Press is not doing a good job of getting the committee proceedings out; they've been very slow.

The other issue there is that IUSSP doesn't have its own journal. Population Studies is sent to all members of IUSSP and we have approached Pop Studies to see whether they would be interested in our being cosponsors of the journal and they have not reacted enthusiastically to that.

VDT: If Grebenik's still around.

PRESTON: Well, he was more enthusiastic than the rest of the Population Investigation Committee. So I'm in charge of a committee which is at this point enacted to look for alternatives, to see whether IUSSP really should have its own journal.

VDT: Another one in the field?

PRESTON: Yes. It could happen now, because we're trying to put a little pressure on the Population Investigation Committee to get better terms for Population Studies, which is costing us an arm and a leg, whereas--as I have pointed out several times--Demography costs members of PAA nothing. That is to say that production and mailing expenses are almost exactly offset by the subscriptions from libraries. So it costs the average member of PAA nothing to have that journal; in other words, it's not being subsidized at all by the Population Association of America. So where there are issues about improving it, I have brought that item up to the Board of Directors several times, that they should not be very niggardly about expenses for Demography, because at this point it's pretty cheap.

VDT: I didn't know that. But not with Population Studies?

PRESTON: Pop Studies is charging \$35 a year I think at this point.

VDT: Which is a large part of your the IUSSP membership fee, which is very high now, and a large part of it is subsidizing Pop Studies.

PRESTON: Yes.

VDT: However, the IUSSP you feel is important in keeping contact with demographers around the world?

PRESTON: Oh, yes.

VDT: Some people get a little nervous about the Third World demographers, but they should be there. There's been a big push to increase their membership.

PRESTON: Well, they've improved their quality a lot as more and more of them have gotten training in the West, especially in the U.S. I have half seriously compared the quality of African demographers to the quality of European demographers outside of France and Belgium. Really there are not very many good European demographers.

VDT: What about the Dutch?

PRESTON: The Dutch have a couple of good ones. Eastern Europeans--it's a wasteland. England has certainly some good demographers.

VDT: And the Africans, many of them trained in the U.S.?

PRESTON: Yes, and they're trained in modern demography and they're quite adept. On the other hand, they go back to situations which are not supportive of their research and, as a result, they have real trouble getting things published that they've done when they go back. We've been talking here for a number of years about trying to get an African demography journal started at Penn that would be an outlet for them.

VDT: At Penn?

PRESTON: Well, it's not going to happen anywhere else, I don't think, and probably shouldn't happen at Penn in the best of all possible worlds, but we have to deal with the world we're living in. Etienne in particular would like to get that started, but I would too. We haven't found anybody that wants to give us the money to do it, yet.

VDT: One final big question--the world population outlook. Are you optimistic or pessimistic? You've said what we all know, that the U.S. is an aging society and the problems that we face there.

PRESTON: I would say a hundred years from now the population problem as we have known it in our generation will have passed from the scene. Meaning that we are not going to be concerned with excess fertility a hundred years from now. Fertility is falling quite rapidly in many developing countries--as demographers have expected, but you're never sure your expectations are right. Latin America, East Asia, certainly, have rapid fertility decline and a number of countries are now below the

replacement level. Africa is very far from that stage, but there's every reason to think that if development is successful there . . .

VDT: Huge if.

PRESTON: Yes, a big if--but that eventually, within a century, fertility will fall very sharply. So if that's being optimistic, I guess I'm optimistic.

On the other hand, we all know that population decline, or population aging, which is the counterpart of slowing rates of population growth, creates its own problems. I guess I'm optimistic there too that we will then be able to deal with those problems. But right now I continue to feel that we are spending too much of our national attention and policy emphasis on the aged.

VDT: Too much? Even though you've just finished saying that this is an aging society.

PRESTON: It's an aging society, but I think on a per capita basis the aged have done extremely well, continue to do extremely well, are going to do even better in the future. In the policy arena it seems to be very difficult for congressmen to stand up to the elderly lobby, to the range of interests represented by older Americans, and call a halt to the expansion, although I guess the Pepper bill that was voted down last week is some sign that they're not going to get everything that they want. On the other hand, they also passed the catastrophic health bill the same day, which is going to be quite expensive [and was repealed--under pressure from the elderly lobby--the following year]. So, in the short term, that's the thing that I think is the great imponderable in the American political arena--where benefits for the elderly are going to come to rest. I don't think they've come to rest yet, because if you look at the public opinion polls the program that Americans want to see expanded--the most popular program for expansion--is medical benefits for the elderly. I think we'll be able to come to grips with this problem, but it hasn't quite happened yet.

VDT: My very last question is a corny one, one of Ansley Coale's: What makes Sammy run? How do you get it all done? You have a tremendous program, so much on your platter.

PRESTON: I don't know. I work steadily and I work quickly. Probably there are 55-hour weeks, which is not that unusual for academics, but I'm pretty steady at it.

VDT: And I understand that you have a fine family life and you're even a supporter of baseball--Little League baseball at one time, at any rate.

PRESTON: That's right. I have a reasonably healthy life outside of the office, I hope. I find that the hours that are sort of demanded of academics at this point are the most problematic feature of my life, but if I can keep it to 50 to 55 hours a week, then things are tolerable. But, again, when I see assistant professors, the pressure that they're under on a daily, weekly, monthly basis, I'm not sure I could do it again if I were just starting out now.

VDT: Well, you've done it brilliantly so far. I hope there'll be many more decades of it. Thank you very much.

PRESTON: Thank you, Jean. It's been very agreeable.

CONTINUED

VDT: Sam has just told me another very interesting thing--that you're songwriting?

PRESTON: You mentioned that Dudley Duncan is now deeply into music and that a number of demographers are, so I added the fact that my current hobby is writing country and Western music.

VDT: For the guitar?

PRESTON: Yes, for guitar.

VDT: Where did this come from? Did you have any musical training?

PRESTON: My only musical training was on the tuba. I was actually a good tuba player, the first chair in the Pennsylvania state band, when I was in high school, and thought briefly about going to musical school and becoming a professional tuba player, but . . . So then there was nothing for about 20 years.

VDT: You could read music?

PRESTON: I could read music, yeah. I was on my way to China, actually, and read an article in I think it was Esquire about Harland Howard, the famous country songwriter. I had come to enjoy country and Western music and had also thought that if I ever had a second career what I'd really like to be is a songwriter. So I decided on the spot, on the airplane, I'd launch my second career. I wrote a song and I've written maybe ten at this point and have had three of them performed by amateur groups that I was able to assemble and I have a tape. One of these days I'm going to take the tape to Vanderbilt--sorry, Nashville--and maybe even myself to Vanderbilt--and see if I can find a publisher who's interested in them. I just sent a bunch out in the mail and they mostly came back unopened. You've got to go to Nashville, apparently, to sell them. But mostly it's just a hobby and I enjoy it.

VDT: Interesting. Does your son, the guitar player, play them?

PRESTON: My son plays them; he unfortunately can't sing.

VDT: Do you sing?

PRESTON: No, I don't sing at all. I don't sing and I can't play an instrument that helps me write songs. I just pick them out. I write the lyrics and music, but not the chords; somebody else has to come along and do that.

VDT: I interviewed Paul Demy in New York last week. He was a topnotch bassoonist--also at the high school level. In those days in Hungary, things were very troubled. He wasn't sure he'd get into university--his father had been a judge--so he concentrated on the bassoon for a while and the local orchestra offered him a job as a bassoonist. Then he was accepted into the university and put away the bassoon and hasn't taken it out since.

And I interviewed Jack Kantner recently in Bedford, Pennsylvania, and he has the trumpet in his background. In fact he's gone back to it a bit, like Joe Stycos, who is also a jazz musician [on the piano], as you know. Bedford has a music festival each summer and Kantner is the director of it this year.

PRESTON: You're kidding!

VDT: And he has a son who is a flutist and plays in the Grand Rapids, Michigan, orchestra.

PRESTON: You know who is the conductor of the Grand Rapids orchestra? Michael Aiken's wife. Michael Aiken is our provost--also trained as a demographer, by Ron Freedman. So it's a small world.

VDT: Small world! Then there is my friend Leon Bouvier, who was at the Population Reference Bureau, who started off as a jazz musician, the trumpet. You remember he and Joe Stycos performed at the 50th anniversary meeting in 1981.

PRESTON: I remember.

VDT: There seem to be many demographers who have music in their background.

JANE A. MENKEN

PAA President in 1985 (No. 48). Interview with Jean van der Tak at the Population Studies Center, University of Pennsylvania, Philadelphia, June 13, 1988.

CAREER HIGHLIGHTS: Jane Menken was born and grew up in Philadelphia. She received the B.A. in mathematics from the University of Pennsylvania in 1960, an M.S. in biostatistics from Harvard in 1962, and the Ph.D. in sociology and demography from Princeton in 1975. She was an assistant in biostatistics at the Harvard School of Public Health from 1962 to 1964, mathematical statistician at the National Institute of Mental Health in Bethesda, Maryland, in 1964-66, and research associate in biostatistics at Columbia's School of Public Health in 1966-69. From 1969 to 1988, she was at Princeton where, among other positions, she was Assistant and then Associate Director of the Office of Population Research and Professor of Sociology and Public Affairs. From 1988, she has been at the University of Pennsylvania as Professor of Sociology and Demography and Director of the Population Studies Center (since 1989). She has consulted widely and as of 1991 is consultant to the International Centre for Diarrhoeal Disease Research in Bangladesh, chair of the National Research Council Committee on AIDS Research, and adviser for the Rockefeller Foundation Population Program and for the Demographic and Health Surveys. She received the PAA Mindel Sheps Award in Mathematical Demography and Demographic Methodology in 1982, was elected to the National Academy of Sciences in 1989, and had been a Council member of the International Union for the Scientific Study of Population since 1989.

Jane Menken is well known for her research and publications on the biological determinants of fertility and in the general area of mathematical demography, as well as teenage childbearing in the U.S., population and health in less developed countries, and population policy, among other topics. She is coauthor or editor of the monographs, Mathematical Models of Conception and Birth (with Mindel Sheps, 1973), Natural Fertility (1979), Teenage Sexuality, Pregnancy, and Childbearing (1981), and World Population and U.S. Policy: The Choices Ahead (1986), and author or coauthor of some 80 articles and book chapters.

VDT: Jane is just now [June 1988] in the process of shifting from Princeton to the Population Studies Center here at the University of Pennsylvania. However, in three weeks she's going off to Bangladesh for six months and you must tell us what you'll be doing there.

But let's begin at the beginning: How did you become interested in demography? You're one of the rare demographers who, like Raymond Pearl and Lowell Reed, moved into demography from biostatistics.

MENKEN: An even more interesting coincidence is that Bob Reed, the son of Lowell Reed, was one of my advisers at Harvard in the biostatistics department. He's best known for his involvement with the Harvard Longitudinal Growth Study. He was one of the statisticians who worked very closely with that project for many years, but he was teaching at Harvard for a long time.

My own involvement began when I was an undergraduate here at Penn and realized as a math major that I was not interested in pure mathematics but rather wanted something that was more related to people. At that time, a friend was a Harvard medical school student and second-year medical students at that time did courses in biostatistics and his instructor was Mindel Sheps. He asked her what opportunities there were for people who were interested in applying mathematics in other kinds of ways, particularly to health. And Mindel, being the kind of person she was, not knowing me in any way, invited me to her home one weekend when I was in Boston and told me about the School of Public Health at Harvard. There I was, 20 years old and completely ignorant of any of these things.

She sent me off to the department of biostatistics. It became clear very rapidly that that was where I wanted to go to graduate school and I applied for admission to the master's program. That was my senior year at Penn.

I went to the School, which at that time was geared toward mid-career students, by and large. More than half of the student body came from outside the U.S. and most of them from the developing world. I was age 20 by that time and even with me, the mean age in our class was 37. It was a real education. There were people who had been working in developing countries. One of my classmates was a nun who was a physician and had worked in Latin America and Africa. Another had worked in family planning in Argentina and Chile for many years. Others were involved in trying to develop trachoma vaccine in Saudi Arabia. So it was an education in and of itself.

After I received my master's degree, Reed hired me to work in the biostatistics department, which I did for two years. One thing I became involved in was helping in the analysis of the Khanna study, which John Wyon and John Gordon and then Bob Potter were analyzing. This is the long-term study in the Punjab in India which introduced family planning and health programs in a number of villages. It was their work in population, combined with the work that the people who had been working in Latin America involved me in, that drew me into population research.

At the same time--before I got there, Mindel and Cecil Sheps had left Boston and gone to Pittsburgh--but I began reading the early work that she was doing and that she and Ed Perrin were doing on mathematical modeling of human reproduction and very clearly felt that I'd found something that I was both interested in and suited all my ideas about what I was most interested in doing.

The period at the National Institute of Mental Health [1964-66] was a diversion. For two years, my former husband was a clinical associate at the National Institute of Mental Health. It was an interesting experience.

VDT: You'd been married before you went to Washington?

MENKEN: Yes. I was married at 20.

VDT: You came as a married woman at age 20 to Harvard--child bride! It didn't seem to truncate your education.

MENKEN: No, not quite. I went to NIMH as a mathematical statistician. It was a fascinating experience educationally, because what they did was to hire people and put you in a room and say, "Think." I had nothing to think about at that stage of the game, without experience. But it made me realize how important research apprenticeship is for most of us. I was sitting there at my desk with pencil and paper; I even had access to a computer--I was a good computernik. I could have done other kinds of things. But I didn't have the ability to formulate the kinds of questions that were answerable or approachable by me, at that stage. I finally did get involved in other kinds of projects, but the first few months at least made it very clear to me that our educational strategy needs to be involving people in research projects and not assuming that people can automatically generate projects on their own. If you have somebody like that--the first few graduate students I was adviser for included Doug Massey, who even at that early stage was somebody for whom all you needed to do was step aside and let him do what he planned to do. But that's not the case with a lot of people.

VDT: Were you an apprentice at the time of your master's in biostatistics from Harvard? You said you became involved later in the Khanna study; did your master's thesis have something to do with that?

MENKEN: No. I was always involved in working on other people's projects, but mainly as a consultant on the statistical part rather than carrying through on a project. When I left, I didn't have access to the data that were there at that time.

VDT: But at NIMH you were given carte blanche at the beginning?

MENKEN: Right.

VDT: And you hadn't gotten your doctorate at that time.

MENKEN: It was a different era. We did get involved in a number of projects there and it was clear that although there were issues that I thought were important, they were not ones I was particularly interested in working on and that population studies were where my heart was.

Then at the time that we were leaving, I talked to people at Columbia University; I was interested at that point in going into a Ph.D. program in biostatistics. But the most important piece of information I got from the conversation was that Mindel was moving from Pittsburgh to Columbia. So I wrote and asked her whether or not she had positions available in her projects.

VDT: You were going to New York anyway?

MENKEN: We were exploring possibilities and that made New York infinitely more appealing. We're talking about a different era, in the mid-1960s. When people look for two jobs now, it's a very different situation.

VDT: It was so unusual then to have two people looking for two professional jobs?

MENKEN: We had no problem. It was clear that at most of the places we were interested in going it was possible for both of us to do that.

VDT: Your former husband was a medical researcher?

MENKEN: He's a neurologist. When we were ready to leave New York City after he'd completed his residency at the neurological institute, we went looking for university towns that needed neurologists and had good demography. The two that were at the top of our list were Chapel Hill and Princeton, and it was possible either place. As I said, it was a very different era.

It was a very different era also for women with children--in many ways simpler than today. What I was able to do from the time that our first child was born was to work part-time, to name my hours. It was unusual enough for professional women to be doing this and there was sufficient need for people to do those jobs that there were no problems in saying that one wanted to work part-time.

So it's an interesting shift that there were many more barriers for women, but once you were within the barrier, there was more flexibility in some ways than there is today, when I think it's much harder to have an academic or research position where you're working, as I originally did, three days a week, six hours a day. I did that for nine months.

VDT: This was at Columbia?

MENKEN: No, that was first at NIMH.

VDT: Already in the government in Washington, they would accommodate you?

MENKEN: Absolutely. At Columbia I worked four days a week, the same kinds of limited hours, and I did that at Princeton for the first two years there. It's an interesting commentary.

VDT: It is indeed.

MENKEN: That while women's opportunities to reach higher levels of professions have expanded, the ways in which one has to do it have been through what were accepted as the male model. Rather than changing work, what we've done is change women to accept that kind of work pace. I think it's now very much more difficult to juggle children and work.

VDT: I think you're absolutely right. Washington right now is having a notorious case of a woman lawyer who has been ordered back to work four months after her baby was born, full-time, or else.

MENKEN: It's a major issue. I know how much my own career has depended on that kind of flexibility.

VDT: I didn't have this until page 2 of my interview schedule, but I'm glad you brought it up. Now back to Columbia.

MENKEN: I was a research associate, which was again flexibility; research associate was a Ph.D. title.

VDT: Although you hadn't yet started on a Ph.D.

MENKEN: I hadn't gone on to a Ph.D. at Harvard primarily because Harvard exams in statistics--I would have switched to the statistics department--were only oral exams. And as many people in the field know, I had a long history of being the shyest person on earth and knew that I couldn't face doing oral exams at that point.

VDT: So that's the reason you didn't do the Ph.D. at Harvard! Did you actually take the coursework?

MENKEN: I was getting a master's degree in biostatistics and they would take this as equivalent to the beginning years of the Ph.D. program in statistics. So I could have gone on the next fall and taken the prelims at Harvard. It was not the coursework that kept me from doing that.

VDT: Has that now changed at Harvard or do they still have oral exams?

MENKEN: I don't know.

VDT: Isn't it rare in the U.S. to have oral exams? I know it's common in Europe.

MENKEN: It's still common in mathematics. And if you can't talk, you're in trouble.

At Columbia what we started doing was the work that led to the book that Mindel Sheps and I wrote on Mathematical Models of Conception and Birth [1973]. We were working quite closely with Jeanne Clare Ridley and Joan Lingner, who were in Pittsburgh; we had a long-distance collaboration on that project.

VDT: Were Jeanne Clare Ridley and Joan Lingner involved in that book? I don't remember seeing their names . . .

MENKEN: No, they weren't involved in the more mathematical part. But there was a whole series of papers that we wrote together, looking at various aspects of mortality and fertility change. Jeanne was much more involved in writing the simulation model REPSIM, which was the basis of much of the work that we were doing in collaboration, whereas Mindel and I did things that were different, using mathematical analysis rather than simulation, and that's what led to the book. But they're very closely linked.

When Mindel and Cecil Sheps left New York to go to Chapel Hill early in 1969 and we left a few months later and went to Princeton, I intended to work at home and complete the book and had gone to see Charlie Westoff to ask if I could have access to the OPR library. When he heard what I was working on, he said, "Why do that sitting at home? Come and work here and work on your project." That's how I first went to the Office of Population Research.

Two years later, we had completed the book, or most of it, and I finally had decided that it was time to go into a doctoral program. I thought very carefully about whether or not I wanted to do it in statistics and decided instead to go through the sociology department, because I was much more interested in learning something about the determinants of fertility and about social science. So I applied for admission to the department at Princeton.

An interesting sociological point about that: Norm Ryder and I were walking back from lunch one day and I told him I had applied and been accepted. He looked at me, and true sociologist that he is, said, "This will change our relationship!" He knew something about roles: colleague/colleague versus student/professor--a different relationship.

I went through the program at Princeton.

VDT: You switched to being a full-time student or were you still assisting in some project?

MENKEN: No. Princeton only has full-time students; you can't be a part-time student there. Or, if you're a full-time student there, you can't have a job at Princeton as well; they can't stop people from having jobs elsewhere, I suppose.

I resigned and went into the program full time, and at the end applied for a job. There were no guarantees that there would be positions, but if they would want me back, I would want to stay there. But I immediately went back [after finishing Ph.D.] to be a research demographer. That was in 1975.

Again, there was a great deal of flexibility in having a research position that made it possible to juggle different kinds of responsibilities. That made it far easier for me, so it was a flexible and good position for me to be in. My research interests continued in mathematical modeling.

VDT: You'd done your dissertation on Estimating Fecundability, one of the shortest dissertation titles I've ever seen.

MENKEN: Well, that's what it was on. What I was interested in doing was looking more closely at a piece of the reproductive process. Mindel and I had done a lot of work on models of conception rates and the time to conception in the book and I was concerned about looking at real data to try and see what we knew about fecundability from populations. I was interested in looking at both what kinds of data were available and what kinds of methods had been used and whether or not the methods yielded different kinds of results. So the first part of my thesis was an extensive review, an attempt to really understand what kinds of methods had been used on data from as many places as I could find. Then to apply the methods that were an extension of work we had done on methods to estimate it.

In looking for data, I was introduced to Henry Mosley, who was then--as he is again--at Johns Hopkins University. Henry had been at the Cholera Research Laboratory in Bangladesh, which is now the International Centre for Diarrhoeal Disease Research, and he and Lincoln Chen, who is now at Harvard, and several other people had done a study of birth interval dynamics that basically took the same kind of framework that we were working with mathematically and attempted to collect data by following women, visiting them every two weeks and asking them whether or not they were pregnant, breastfeeding, menstruating, getting all kinds of detailed information. They had data from a small study of just over 200 women, who had been followed for two years, from 1969 to 1971, when the war broke out in Bangladesh and the data were curtailed. Those data were available and they had done some analyses, but they made them available to me. That was my first contact with the work that was going on in Bangladesh.

VDT: Was that the Matlab project?

MENKEN: Part of it. From that point on I was involved with some of the planning for projects there and worked with a number of people who are actually in Bangladesh. But it wasn't until four years ago that I felt I could leave and go for a month; that was the summer of 1984. I spent a month there in conjunction with a project that was basically an outgrowth of these studies.

So my interest in Bangladesh and the reason I'm going there actually grew out of a very general interest in the reproductive process. It moved much more toward a continuation of that interest, but also an interest in developing societies in general. Some of the work that we'll be doing in Bangladesh I hope will lead to a project looking at women and development.

VDT: You've certainly chosen the country with about as many population problems as there could possibly be.

MENKEN: It's a far cry from looking at mathematical models to doing this particular research project.

That leaves me, really, at the beginning of returning to Princeton [on the faculty], where the research we did covered a wide range of issues. It was, and has been, a wonderful place to work in collaboration with a number of different people and very fine students.

VDT: When did you start teaching? You said you were taken on as a research associate in 1975 when you got your Ph.D.

MENKEN: Charlie Westoff and I taught an undergraduate course every other year; as a member of the research staff, one can teach. We began it in either 1975 or 1976.

VDT: He would take one fall and you would take the next?

MENKEN: No, we did it together. We were always there together; one of us taught and one of us sat.

VDT: How interesting; I've never heard of that.

MENKEN: Team teaching like that? They teach courses like that. James Trussell and I for five years taught a statistics course like that. This was the basic introductory graduate stat course for students in the Woodrow Wilson School--public and international affairs and sociology students. We traded off. Team teaching is interesting. It was interesting in the small seminar class that Charlie and I taught,

because when we came to a topic, he and I would have a dialogue as well as having one with the students, and they got quite different perspectives.

VDT: One would do the talking one week and the other the next week?

MENKEN: We talked on our own topics; we took what we were interested in or had more expertise in and traded that back and forth. James and I did that in teaching statistics, in part to have discussions which showed that statistics was not something so cut and dried that there were no opinions and judgments that went into it. Sometimes we argued fairly seriously, so much so that one Asian student asked the then director of graduate studies why they made people who hated each other teach together!

It also made for some funny moments, so much so that an economist who was teaching next to us one year complained bitterly about how could he teach macroeconomics when people were laughing so loudly next door. I think it was a very good way to teach and something that we enjoyed.

Etienne van de Walle and I have team-taught in a research seminar here at Penn and I think it's been very good.

VDT: You think that Princeton was innovative in this system, or where does it come from?

MENKEN: Oh, I think lots of people do it. East-West Center [Population Institute, Hawaii] does it all the time in their summer seminars, by having usually two people who are the coordinators for their workshops and then resource people who are part of the seminar. A number of years ago, I was a resource person along with Ron Rindfuss and Larry Bumpass; Griff Feeney and Jay Palmore were the two East-West Center people in charge of the seminar itself.

VDT: A high-powered group. Was that over a period of several weeks?

MENKEN: Yes.

VDT: Lucky students!

MENKEN: It was a mixture of students, as the usual East-West summer seminars are. It was a mixture of people from Asian countries, most of whom are practitioners, and graduate students, both Asian and American, so you would have people from a wide range of backgrounds. I think in many of those situations, it encourages people to participate if you are already having people who are having interchanges as part of normal operating procedure. That was quite good.

Where do we go from here?

VDT: I'd like to ask a bit about your work. Of course, you are most famous for mathematical models and the famous work that you did with Mindel Sheps. Being at Princeton broadened your horizon--not that it needed to be broadened; you have many other interests in demography. I was intrigued by a statement--I suppose you've said it in other ways--you had in your recent article, "Proximate Determinants of Fertility and Mortality: A Review of Recent Findings," in Sociological Forum [Special Issue: Demography as an Interdiscipline, Fall 1987]. You said: "Although gaps in the knowledge of proximate determinants remain and continuing periodic measurement is necessary to monitor their levels and change, the primary need now is to improve understanding of the causal chain that determines fertility." You said that at the very beginning, you were interested in determinants of fertility. I interpret from this line that you feel still more needs to be done on the socioeconomic background to fertility change and family planning programs. Do you feel that's the work that still

needs to be done, or do you feel there's still something to be said on proximate determinants?

MENKEN: I think there's always something more to be said on almost any research issue. But if you're talking about priorities, I think what we've done is come a very long way in understanding the proximate determinants. When we were working in the late 1960s on models of fertility, the common wisdom among physicians was that breastfeeding didn't matter, that it was an old wives' tale that there was any relation between breastfeeding and conception. So we were working in a very different kind of situation, even at that stage. By the time we were completing the book, my very strong feeling was that to go on and develop more and more elaborate models was certainly feasible but it was wasteful until we had some knowledge of what went on in the real world--that theoretical work guides the kinds of questions one must ask of the real world, and then you can go back and fill in your models again.

And I think that we've done that on proximate determinants.

VDT: Using, in part, the World Fertility Survey?

MENKEN: Parts of the World Fertility Survey, yes. I would say that the work in Bangladesh has been extremely informative because they were able to do much more detailed prospective studies. I think we can get a great deal from World Fertility Surveys; we're never going to get very good data on fetal losses, because people just don't report them.

VDT: Unless you have a prospective study like the work in Bangladesh, which went back every two months?

MENKEN: The first one went back every two weeks and did pregnancy tests every two weeks, so they had good data.

VDT: In the same issue of Sociological Forum, Rindfuss, Palmore, and Bumpass ["Analyzing Birth Intervals: Implications for Demographic Theory and Data Collection"] say that despite looking at the now classic proximate determinants which John Bongaarts pointed out--age at marriage, breastfeeding, contraception and abortion--there were still variations by women's education, so something had to be missing, and the something had to be coital frequency.

MENKEN: I think that is one of the major unknowns. There's still no satisfactory explanation for why such a high proportion of conceptions in Bangladesh occur in a very few months of the year--I can't remember the figure. It's a very strong seasonal variation. There are people who've explored the nutrition hypothesis and that doesn't offer much explanatory power. In my own thesis, I explored separation of spouses, because there's a pattern where men are either working in fields farming or they're fishermen who are away. Even if you adjust for the number of days away per month, that very strong seasonal pattern remains, although slightly attenuated. I can think of no explanation but that of differences in coital frequency that are related to life style changes through the course of the year. But, again, that's very difficult to obtain data on. I tried for a long time to push for good studies of that. I can't think of any other explanatory factor at this point.

VDT: Except for sexually transmitted disease, perhaps?

MENKEN: I think there's much more disease that people have really gotten; there's more variation in sterility and in age at sterility than is commonly accepted. I would have said that we knew enough about sterility until a graduate student, Ulla Larsen, who was at Princeton and is now a postdoctoral

student at Berkeley, worked on a procedure for estimating proportions sterile at each age that could be applied to survey data. She applied that to World Fertility Surveys from sub-Saharan Africa. And it seems to me that sterility in a number of populations is occurring at earlier ages and is more variable than I had thought in looking at data from Western countries.

So, I think, while I would stand by what I said--I would put higher priority on looking at what causes populations to change their fertility behavior, what determines the levels of fertility-related data--I still think that there are issues in proximate determinants that are worthy of study.

VDT: You yourself, of course, have obviously been interested in wider fields too, drawn in, for instance, by your work on family planning in the U.S. You've written a lot in Family Planning Perspectives, for instance, on teenage pregnancy.

MENKEN: You can't help that, being a demographer. A friend of mine who is a physician, who got tired and burnt out by years of practicing, became interested in legal issues in medicine. He's now, at age 50, a second-year law student. We were talking about what we were both doing and I told him about going to Bangladesh and about my interest in AIDS and in a number of different areas, and he said, "You know, after all these years, I finally know what being a demographer means. It's a license to go and do what you want to do." He said, "If I want to change my interest slightly, I have to go off to school for three years to become a lawyer."

I think that my interests do fit together in a weird kind of way. The interest in teenage childbearing came about really in interest in what the timing of fertility meant in more sociological terms. Contraceptive effectiveness, that's a natural outgrowth of my basic biostatistical interest in measurement and my demographic interest in measurement. I think we can't understand causes until we can measure what happens. I think one of the major problems with all of the discussion about AIDS today and about HIV prevalence comes down to a real need to get better measurement systems so we can talk about differences--measurement of AIDS cases and of the prevalence of the virus itself in different sub-groups of the population in different geographic areas. It seems to me we don't have any good idea of trends in incidence. Most of the information we have on sero-positivity comes from very non-random samples, to put it politely. They're convenience samples and it's very difficult to try and decide whether or not these are comparable measures and what it is that they actually do measure, if they're interpretable in any way.

VDT: Does AIDS come into the work you're doing in Bangladesh?

MENKEN: No.

VDT: I didn't think it could; I thought Bangladesh had the lowest prevalence of AIDS. But everybody else is interested.

MENKEN: Oh yes. I sit on a committee of the National Academy of Sciences that's looking at research needs in social and behavioral sciences with respect to AIDS. So I've spent a lot of this past year not doing my own research but reading a great deal and hearing a lot of presentations and evaluating a lot of studies. That's what led to the remarks I made at PAA [1988 meeting in New Orleans]. This year there was a panel on AIDS and I was trying to think through what demographers could contribute to research on AIDS.

VDT: You've certainly always been at the cutting edge of "hot" issues. There was your famous PAA presidential address ["Age and Fertility: How Late Can You Wait?"], delivered at the 1985 PAA

meeting in Boston, published in Demography, November 1985]. It was right on the button with "How late can women wait?" which is an increasing issue with U.S. women who are delaying childbearing to the ages when infecundity seems to set in. You set to rest some alarmist views, particularly of that French study that came out about that time--you become terribly sterile at about the age of 30 or 35.

MENKEN: Fall over the cliff.

VDT: Yes, something like the Haylick thing of longevity; at 85 everything falls off. Also the thing I particularly liked about that speech was the woman in the middle--women caught in the squeeze. You showed so ingeniously, demographically, the women who have children under 18 at the same time that they have parents 65 and over and how increasingly there are those women caught in that squeeze. I haven't heard too much on that since then; perhaps I'm not reading the right things.

MENKEN: Once again . . . What I was doing in that project and what Susan Watkins and John Bongaarts and I did in a later article that was in the American Sociological Review ["Demographic Foundations of Family Change," ASR, June 1987] was to do this based on models, because what we were saying was there are no data on families. There were data on the components--on marriage, divorce, mortality, fertility. That's what we put together to generate a picture of what would be happening on average. But we didn't have any data. There were no studies we could find that asked people of different ages, "Are your parents alive?" What we have done in this country was to collect data on households and not on families, and we felt very strongly that it was family ties that we were interested in.

The data collection by Larry Bumpass and Jim Sweet in the new [1987-88] National Survey of Families and Households is the first data set--becoming available this summer [1988], supposedly--that will contain sufficient information. The meeting I have after this is with some of my colleagues here; we're writing a proposal to begin to look at some of the issues of intergenerational responsibilities and to try and quantify who is in what kind of family situation. We plan to use that data source and a number of other kinds of things.

But, once again, there's a limit to how much talking one should do before one really can go and look at what is happening in an area.

VDT: Were you yourself in that situation? Did you have elderly parents as well as your children growing up?

MENKEN: Yes.

VDT: Like most people do. I think it takes a woman to feel that that's a demographically researchable topic. Your interest in the "woman in the middle" grew out of your own experience?

MENKEN: Very definitely. I think it was experience of just talking with people, and when people of a certain age gather together and hadn't seen one another in a while, being struck by the fact that we were as likely to be talking about our parents, or possibly more likely to be talking about our parents than we were to be talking about our children. That the experience of facing problems of aging was not just a problem of a person who was aging him or herself but really was a changed family situation.

And we began to ask the question, How much has this increased over time and how many people are in this situation? Is it that we're hearing much more about this because there are so many more people who are having the experience of caring for or being responsible to parents at ages where in the past they would be the senior generation? We have to be very careful--we try hard to say that it's

not just caring for, but that it's a changing social framework. We all know senior colleagues who show no signs of having fallen over a cliff at age 65, but who remain active and involved for many years after that.

VDT: They might get a little piled up. As you know, I've just been to Princeton to interview your former colleagues there. I'm interested that Ansley Coale, though technically retired, is still very much involved, still in his office.

MENKEN: Of course.

VDT: And Norman Ryder expects to stay in his office when he retires next year, because it has more wall space for his books. Things get a little piled up. Where are you going to put the next generation of professors?

MENKEN: I'm not worried about the offices. One reason Ansley retired before the mandated age was his own strong feeling that there should be turnover, that there should be room for people to move up. And he quite happily moved over to become professor emeritus, but with no diminution in his activity. Nathan Keyfitz is another example of someone in our field who has done more in retirement . . . I remember laughing that when Nathan retired from Harvard, he remained on the faculty half-time and at the same time he accepted a professorship at Ohio State which was two-thirds time; that was "retirement"! He has been returning to Indonesia, where he'd been 20 years earlier, and he's also in Vienna with IIASA [International Institute for Applied Systems Analysis].

VDT: Here at Penn you have Ann Miller, Ed Hutchinson, and Vincent Whitney, all still on the faculty as professors emeritus. They don't teach; do they occasionally come in?

MENKEN: Ann comes in all the time and Ed comes in on quite a regular basis; he's in his eighties and he's working on a book on vital statistics. [Dr. Hutchinson died in December 1990.]

VDT: Well, as I say, you're certainly into a hot topic there.

MENKEN: I think it's a very important one and I think it has both demographic and policy implications, many of them positive, some of them raising problems.

VDT: I'll be talking to Sam Preston tomorrow. I think he's putting into public view really for the first time the idea that the necessarily more attention to an older population means we're shortchanging our children. But that's not your topic . . . caught between the two.

MENKEN: I had a sabbatical for a semester when I was beginning the work that led to the PAA address and I was actually here at Penn a couple of days a week. Sam and I traded off. He spent a sabbatical at Princeton; I spent a sabbatical here. We were down the hall from one another working on both of these things at the same time and not knowing what the other was doing.

I think the two policy issues of major importance are what happens to people. The elderly population seems to be split into groups that are very well off and those who are poverty-stricken. Children seem to be more and more concentrated in a disadvantaged category, and unless we pay attention to that, we're risking the future, as the title [Children at Risk] of the National Academy of Sciences report on adolescent pregnancy stated; I think we're risking the future of our children.

VDT: Those are two policy issues in developed countries. You've also been involved in the policy

issues of developing countries, where there continues to be rapid population growth. You were director of the American Assembly symposium on it [and editor of the resulting publication, World Population and U.S. Policy: The Choices Ahead, 1986], which was a reaction, I presume, to the U.S. policy turnaround at the 1984 Mexico City population conference: Population growth has now become a neutral phenomenon to be solved with a free-market approach.

MENKEN: Yes.

VDT: What do you think about that issue now? Has it simmered down? Of course, it [the 1984 U.S. turnaround] was followed by the National Academy of Sciences study [Population Growth and Economic Development: Policy Questions, 1986], which didn't end up by saying population was a neutral phenomenon, but it did take a less alarmist stance.

MENKEN: I think the most thoughtful people always were of that view, that the population issue was blown up to far greater import than it merited. I believe it was Etienne van de Walle who responded to the comment in President Johnson's speech [1965] about dollars for development and population [roughly, \$5 spent on family planning is worth \$100 spent on economic development], which Etienne amended to what I think is a very good statement: that if there were \$20 to be spent on development programs, the program would be improved if one of those dollars went for population issues. That looking at population alone, unless there are other changes going on in society, you're not necessarily going to improve the lot of people; you're not going to improve the economic position simply by changing population growth without any other changes in society. It's always been appropriate, to my mind, to have this as a part of development effort, but not as the only effort.

VDT: You've answered my next question: What do you see as leading issues in demography over the years you've been involved? You've said that in this country, at least currently, it's the issue of distribution of the population by age, and in developing countries, you'd see it as . . .

MENKEN: Rapid population growth, still.

VDT: What are you going to do in Bangladesh this time? This will be the first time you've had six months there?

MENKEN: Yes, a long stretch there. In Bangladesh what you find is that, as in many other developing countries, there's a lot of data collection that goes on and much of the data sits unanalyzed. The attempt to increase expertise in this area has generally involved sending people for graduate studies either to institutions that are set up specifically, like the UN demographic centers in different regions, or to Western research institutions. So people go away for three or four years at a time and when they come back to their own institutions in their own countries, they have loaded on their shoulders all of the responsibilities of policymaking and administration and then attempt to do some research with facilities that are not up to the standard they were led to expect in the major demographic centers of the world. Our response to this was to ask the question whether or not we could do the training within countries, working with people on research projects--an extension of the apprenticeship-in-research kind of approach.

In the project that I'll be working on, there'll be two research workshops this time. There was one a year ago for two months at the International Centre for Diarrhoeal Disease Research in Dhaka, in which we worked with people on the staff there, not choosing the research project, saying the research project had to fit within the center's research plans and that we would work with people; that we were not coming there to get research done for ourselves, we weren't going to be writing papers, we were

going to be working with them on their research. Instead of normal classroom teaching--most of these people have some background in statistics or epidemiology--as questions came up, we would set up lectures on specific topics that seemed to be of general interest because they were appropriate for several of the projects at the same time.

But we began trying to work with people in actually carrying out research and in trying to set up collaboration. So, while I'm in Bangladesh this time, we'll be running one workshop this summer and another late in the fall. The one late in the fall will bring people from other research places in Bangladesh to the center. We do this as a pilot study for the possibility of doing a different kind of teaching and training.

VDT: Interesting! John and Pat Caldwell in their book on the Ford Foundation role in population [Limiting Population Growth and the Ford Foundation Contribution, 1986] mention that Penn formerly had less of a close tie than, say, Michigan did with a particular developing country, although you've had Etienne van de Walle in Africa in recent years, and that the relationship was mainly sending Third World students here. But now you're going there; work on research there.

MENKEN: Yes. I think it's very difficult to go there until there is an institution of some sort with which to work. There hasn't been that kind of thing in Africa yet.

VDT: I visited Marvellous Mhyloyi, who had studied at Penn, at the University of Zimbabwe in Harare. She was overwhelmed. Too many demands on her there; couldn't get on with her own research.

MENKEN: Absolutely.

VDT: Boy, she could have used some help from "home."

MENKEN: That's one of the things we hope we'll be able to do more here. What Penn has done is to bring people in from different African countries, and I think it's getting to the point where there's going to be a network of well-trained African demographers--unfortunately, not more than one or two in each country.

I've been on the Rockefeller Foundation Population Program Advisory Committee, along with David Bell and Ron Freedman. The three of us have said over and over again that we can't simply train people and send them back with no support. What happens is what's happening to Marvellous, who is incredibly competent, and she's overwhelmed. She was at a meeting at Hopkins several months ago. We had a marvelous conversation; I had not known her well before. And I think that that's in general true.

VDT: She said to me, "I feel so alone." She needs support.

MENKEN: That's right, and I think what we need to do is to have programs that will allow people like Marvellous time to come back for a few months so they can bring their research and do it, or programs that allow people to go there.

VDT: She had done a survey of 120 couples in two different regions of Zimbabwe which showed, probably, far lower contraceptive prevalence than had been found in the Contraceptive Prevalence Survey [of 1984] which was high for Africa, but she had no time to analyze it.

MENKEN: That happens over and over again and I do feel that we educators in the population field need to pay attention to that.

VDT: Who have been some of the leading influences in your career? Obviously, Mindel Sheps. Tell me something about her. She was a biostatistician . . .

MENKEN: And a physician. I don't find it easy to talk about Mindel. She had a very strong social conscience. Right after World War II, she and Cecil went back to their native Canada, to Saskatchewan, and were involved in writing the first socialized medicine act for a province in Canada. They had very strong feelings about the plight of the poor; were very much involved in civil rights. So she was a very strong influence in all aspects of life.

She was somebody who was a wonderful friend, besides having a sharp and incisive mind, and a willingness to work and to encourage work in many, many people. When she died, there was a memorial service in Chapel Hill, which turned out to be a gathering of her friends and relatives and I found out then how many lives she had influenced and how many people had benefited from her support. I mentioned earlier what she did for me when she didn't know me! Just simply by her interest in encouraging science and encouraging people in whatever way they could best use their talents.

VDT: She died in 1973, which was much too young.

MENKEN: Yes.

VDT: Did you have anything to do with setting up the PAA Mindel Sheps Award in Mathematical Demography, which you yourself won, naturally, in 1982? This has become such a heartwarming part of our meeting every other year.

MENKEN: It was her husband Cecil and her son Sam and his family who decided that they wanted to set up this award. Bernie Greenberg, who was then dean of the School of Public Health at Chapel Hill and who had been chairman of biostatistics and was a PAA member, did most of the negotiating with the Association to establish the award.

VDT: How about some of your other influences? Ansley Coale was your professor at Princeton.

MENKEN: Long before that, at Harvard, there was a microbiology professor, Roger Nichol's, who was working in Saudi Arabia on the vaccine trials that I mentioned--Harvard had these people who'd go out to do field work and come back to analyze data. He came looking for someone to help with the statistics and help design some of the vaccine trials. I was a newly hatched master's degree recipient. I was doing some of the consulting there and became very interested in the project, and he encouraged every step of the way, and remained for many years after he came back to Harvard somebody with whom I always talked about what I was doing, what I was hoping to do. He left Harvard a number of years ago, because his own interest was science education and he felt that science in this country really was not being taught well at early levels and became director of the Boston Museum of Science.

And the morning of my PAA speech, which was in Boston, he came over and we had breakfast together and talked about it, and he said, "You've come a long way, baby!"

VDT: Lovely.

MENKEN: He was a very strong influence in my life. Even harder, he died a couple of months ago, very suddenly.

Yes, Ansley--and Charlie Westoff--have been enormous influences, in quite different ways. Charlie has a great deal of concern for his younger colleagues and very frequently involves them in projects in ways that open up career opportunities for them. He was the one who came wandering into my office one day and said--it was during the days of the Commission on Population Growth and the American Future--he said, "I don't like what I see about teenage childbearing and health. Are you interested? Do you want to go find out about it?"

VDT: You were pretty early in your career when you wrote that important paper for the Commission report ["Teenage Childbearing: Its Medical Aspects and Implications for the United States Population," in Charles F. Westoff and Robert Parke, Jr., eds., Commission on Population Growth and the American Future Research Reports, Vol. 1, Demographic and Social Aspects of Population Growth, 1972].

MENKEN: I said, "Sure." He said, "You're a public health person; go find out about these things." So off I went to do that. And he has done that for a number of people and does it consistently.

Ansley, by his example, his enthusiasm all the time for his research. Everyone laughs about Ansley, running around with these little scraps of paper on which he has a graph, saying, "Look at this!" The joke around OPR was he was even known to be found talking to the cleaning lady when there was nobody else available to share his latest finding. I think the quality of his intellect is only one of the facets of the man that makes him such a great person.

VDT: He has great physical bounce. He said he'd be in for our interview at 9:30 on his bike and he arrived within minutes of 9:30, on his bike, and it was pouring rain.

MENKEN: His wife has finally persuaded him to wear a helmet, but Ansley, in all kinds of weather, rides his bike.

VDT: And plays tennis, every day.

MENKEN: Absolutely, both he and Charlie.

And Jim Trussell has been extremely important. We have shared a very good research partnership. One of the difficulties of the move I'm making [from Princeton to the University of Pennsylvania] is the distance that has been put on that kind of relationship.

VDT: Why are you moving?

MENKEN: I'm moving because I did not want to continue to live in a small town; I wanted to live in a city. And I faced incompatibility between my work life and life in a small town.

VDT: Protected environment?

MENKEN: Precious.

VDT: Ah, good way to put it. Ansley Coale simply explodes at the traffic in Princeton; it's horrendous compared to some 60 years ago when he was an undergraduate.

MENKEN: None up until four years ago. You could just watch the traffic around Princeton increasing month by month.

It was a decision that was a long time in coming and a very difficult one to make. I thought about it for a very long time and finally had to come to the decision that I had to move and try something else, so that's a very tough decision. However, given that I was moving, I'm delighted at where I've landed.

VDT: You'd worked already, of course, with people here, at least with Sam Preston--you've been coauthors of many studies and articles--and with Frank Furstenberg.

MENKEN: Yes, we've all worked together in various combinations. So there are people with whom I already work well and others with whom it's quite clear that there will be increased collaboration.

One of the special appeals for me of coming to Penn is the presence of a medical school and a greater health component to the university. Doug Ewbank has been teaching a course in clinical epidemiology in the medical school in conjunction with a program there and I've been teaching in that program and we plan to continue. There's a project being set up on AIDS in Philadelphia and we'll be involved in trying to estimate . . . I've already been working with a young medical student trying to design a study to look at newborns, where they take blood samples for testing for metabolic diseases.

VDT: Any other colleagues who've been outstanding influences?

MENKEN: Ron and Deborah Freedman, just in their general enthusiasm for things that they do. John Bongaarts and I have collaborated.

VDT: You've never been in the same university with the Freedmans.

MENKEN: Nor with John. And, until now, not with Sam or with Frank Furstenberg. I think that's one of the beauties of the field, that we can do things across universities.

VDT: It doesn't require living in the same place. It's a small enough field still?

MENKEN: I think so. John Knodel and I talk a lot about our shared interests in working in developing countries and that's been important. Henry Mosley, with his work on health and population, has been an enormous influence on my own interests and work. People at CDC, Centers for Disease Control, who are working overseas.

Those are really the major ones, I think, within the field. Nathan Keyfitz, who is part family. His daughter is married to my brother.

VDT: Interesting! How did that come about?

MENKEN: Not through anything we did; they met in New York. The funny story about that one is that my brother, Marty Golubitsky, wrote a paper on mathematical demography that appeared in Theoretical Population Biology when Nathan was one of the editors. One morning I had walked into my office, picked up my mail, and found a request from Nathan to review a paper. I looked at it and burst out laughing; it was my brother's paper. So I wrote back to Nathan, saying as soon as I saw the title page I knew it was a marvelous contribution but I thought he needed another referee, and signed it Jane Golubitsky Menken.

VDT: Now your students. You have a wonderful reputation for mothering your students. Who have been some of your students that you're proudest of so far?

MENKEN: I have wonderful students and I hate to . . . Okay, my first student was Jim McCarthy.

VDT: At Princeton--the first student whose dissertation you monitored?

MENKEN: Yes, that I supervised. The second student was Doug Massey. There was a little group at that point that was Jim McCarthy, Doug Massey, Susan Watkins.

VDT: Was she your student?

MENKEN: No, she and I had become friends when she first came to Princeton as a visitor, before she became a graduate student, and it would have been inappropriate for me to supervise her, so I stayed away from that. Those two, Jim and Doug, were incredibly self-starting and bright and interested; they were wonderful to work with.

But I've had a long series of extremely good students. Jill Grigsby, who is at Pomona College. They've gone off to do quite different kinds of things--some to work for foundations, some to work in developing countries, some to combine doing all of those. Most recently, Carolyn Makinson finished and she went to work for the Demographic and Health Surveys. She wrote a wonderful thesis on sex differences in mortality in Egypt. She spent a year living in Cairo and then in a village, living with families in both places, and locating data sources, collecting data, and then coming back and writing an excellent thesis.

VDT: This is an obvious question to ask any woman professor. Do you think that women really have an advantage in their relationship with their students, in part, the maternal instinct? Your predecessor here at Penn, Dorothy Thomas, had a reputation for being particularly a "mother" to her students. Of course, Sid Goldstein at Brown has a reputation for his "paternal" hand with his students. What do you think?

MENKEN: I really don't think that's true. I think there are differences in the way one handles relationships with students. I think of it as being much more personality rather than gender.

VDT: You're probably right.

MENKEN: I should add--when you ask about students--one of the things that was wonderful about Princeton is that the occasional undergraduate would find his or her way to the Office of Population Research--Princeton undergraduates all write a senior thesis--and we've had some wonderful ones. Michael Stoto, who is now at the National Academy of Sciences and had been at Harvard for a long time--got a Ph.D. in statistics there--was an undergraduate at Princeton and that's where his interest in population developed. Judith Seltzer, who's an assistant professor at Wisconsin, was an undergraduate in the program. There've been a number of people. Some of these go off and do other things, they don't all go into demography, but they've been wonderful to work with.

I think that one of the real rewards of being in a major research institution is the kinds of students one gets. One of the joys here at Penn is that by having a Ph.D. program in demography you can fit in many more people from developing countries, with many different kinds of backgrounds, than has been possible in the traditional programs that have been either through sociology or economics, where people may not have either the background or the interests to go through the standard programs.

I've been so impressed this year with the students here at the Population Studies Center, many of whom, like Marvellous, will go back and be swallowed up in their countries and we don't hear about

them as major figures in demography, but they are going to be major figures doing the work of beginning to get better, or continuing to improve, demographic data collection and analysis in their own countries. And they are plenty smart and devoted!

VDT: Let me ask you, again as a woman in the field. You've obviously never felt any discrimination because you were a woman. You followed in the wonderful tradition of Mindel Sheps in your own biostatistics field. And, of course, in PAA and demography, there were the early women leaders, Irene Taeuber, Dorothy Thomas, Margaret Hagood. As people always point out, the three awards in PAA are all named for women, although now there will be a fourth one, for Robert Lapham. So, as a woman, you have not felt any discrimination? Or, on the other hand, was it an advantage?

MENKEN: I'm very aware of what other women have felt they faced in demography and in other fields. I think early exposure in such a masculine field as mathematics must have immunized me--and the experience of working with outstanding women.

VDT: Meaning Mindel Sheps?

MENKEN: Mindel Sheps and Jane Worcester, who was one of my biostatistics professors at Harvard, who was at that time a woman university professor. And there was just no question in her mind that she was doing what she wanted to do.

I have never really felt that I was discriminated against. And I know I've been fortunate in that regard.

VDT: Do you think perhaps there was an advantage?

MENKEN: Yes. I think there was an advantage in the sense that men would not have been allowed to have the weird kind of career pattern I had; they would have been excluded from the academy long before. That women, perhaps because they weren't taken as seriously, were also given special options. That's possibly inappropriate, but it certainly was useful for me.

I really don't think about me as a woman in those situations, so that in many cases when I'm talking to women about the problems they face, I sometimes feel like I'm not a native of the same country. I think that I continue to behave as if I'm not going to be discriminated against. I think that there are men who have trouble accepting women as determined, aggressive, as all of us are who have chosen this kind of work. But by and large that's their problem, not mine.

VDT: Which accomplishments in your life, so far--because there are going to be still many more--have given you the most satisfaction?

MENKEN: I can't answer that. I don't think like that. I don't count my life in chalking up . . .

VDT: Okay, you've already talked about accomplishments.

One last thing before turning to PAA. In the PAA meeting just past [1988] in the Kingsley Davis session ["Two Centuries after Malthus: The History of Demography"], there was an interesting paper [by Jay Teachman and Kathleen Paasch] on a content analysis of Demography over 25 years. Most of your publications have appeared elsewhere and they pointed out that often women publish in fields, like family planning, that are better covered elsewhere. One thing they pointed out was that women seldom are single authors, and you have nearly always appeared as a coauthor. Is that because you're just very generous to others, or you genuinely do work as a joint author?

MENKEN: I prefer joint work and, therefore, joint authorship. I suspect that, if you count James Trussell's publications in the same way, his were equally coauthored. That's the way we work. I think that demography is a field in which many of us do coauthor. Phil Morgan, who just stuck his head in here, has just been promoted to a tenured position here at Penn and one of the issues that came up about Phil Morgan was how many of his publications were coauthored. The same issue came up with Ron Rindfuss a number of years ago--his publications were mostly coauthored. I think we are a coauthoring field.

Now, it may be disproportionate--I didn't hear that paper and I don't know the statistics on that--but I think that for every example of women who coauthor . . . I think our field is built with men who coauthor; we tend to work that way.

VDT: Now on PAA. Can you remember which was the first meeting you attended?

MENKEN: Sure, it was 1967 in Cincinnati.

VDT: What was so outstanding about that particular meeting that you remember it so quickly?

MENKEN: Birdwatching.

VDT: Most unusual answer I've gotten from any of my interviewees!

MENKEN: Putting faces to all these names. I felt I should be wandering around with binoculars, looking at nametags.

VDT: Where were you by that time?

MENKEN: I was at Columbia. It was the first year after I started working there. I started working in the fall of 1966; this was the spring of 1967.

VDT: By then you were really absorbed in . . .

MENKEN: In population, yes. And so to go off and begin to meet, or at least see and listen to, some of these people whose work I'd been reading, it was a wonderful experience.

The following year the meeting was in Boston and Ansley was president and gave a talk on should the U.S. begin a campaign for fewer births. That was what really made me think about going to Princeton. I think the best PAA presidential addresses have come when people have really tried to pull themselves out of their normal research focus and to try and think of broader applications of work. And Ansley was asking a question, "Does what we know mean that we should be beginning programs that would have specific focus?"

VDT: "Should the United States Start a Campaign for Fewer Births" [published in Population Index, October/December 1968]. That was, of course, about the baby boom, though fertility had started coming down by then.

MENKEN: That's what he was saying. It was 1968--not all the data were available; we're always several years behind--but he was saying that everything was indicating that a decline had already begun and there seemed to be no need for intervention to change motivation in the U.S. It was such a

reasoned approach to an important problem. And here was this person who was known for his esoteric mathematical kinds of work or his work on economic change and population change. I found it a memorable experience to sit there and listen to that one.

VDT: Can you remember other memorable PAA addresses or events over the years that stand out in your mind?

MENKEN: What I remember most after those early meetings is the combination that PAA has offered of meetings where people really go to sessions and listen to what's going on, that we view this as a real means of communication in the field, unlike meetings of other associations where you wander in and the meeting halls are empty and you're not getting that kind of intellectual stimulation or intellectual exchange that goes on at PAA meetings. So I find the meetings themselves good. They have always provided very useful ways of doing our work.

And then, they're just plain fun. Basically, I think demographers are interesting people to be around.

VDT: Though you weren't in on the early Princeton Inn days, that everybody regrets have passed, because there was one session at a time that everybody attended, "around the table"? But it's still the same flavor, you feel?

MENKEN: It's getting larger; I think it's getting more difficult to retain that kind of flavor. But I think it's there, more than with most other associations.

VDT: Did you, when you were president in 1985 and responsible for the program, make a concerted effort to see that some of that flavor was retained?

MENKEN: What we were focusing on mostly when planning the program was to cover the broad range of interests of people in the Association and to take into account some of the changes in interests, like state and local demography and business demography. I was, and am, a strong supporter of planning programs that meet the needs of people who are in those areas. Unlike economic demography or a field of substantive interest that fits within the traditional framework of fertility, mortality, migration, many of the issues that people in the applied fields face are not the subject matter of standard research papers. We needed to have different kinds of approaches that would allow them a forum. And I think the increase in the breakfast meetings, the introduction of Applied Demography, the newsletter, and the support now given by the Association to the extent of allowing, in the membership mailings, a checkoff for a subscription to Applied Demography are all good innovations.

VDT: Did that happen in your year?

MENKEN: It was being worked on. I can't remember whether it happened then or the next year, but it was certainly part of the discussion. I spent a lot of time with the committees on state and local demography and business demography.

VDT: Do you see that as the way that PAA will need to change? I suppose it always has to accommodate new interests and needs.

MENKEN: Of course, sure. It may mean that at some point there will be a need for a different organization. I hope that doesn't happen. I would much rather see the changing of applications or

interests accommodated within the framework of one association.

VDT: In other words, you wouldn't like to see what's happening to the American Psychological Association, now it's about to split up into several different groups? You think it's all right having those meetings that do occur at the breakfast meetings, as you say, and on the day before, on the Wednesday?

MENKEN: Absolutely. And one of the things we did start was the Thursday evening sessions, where people who wanted special topics--what we did was give them room--could organize something. I think the Association should be open and receptive to the changing interests of the membership. And I think that can be done without diluting standards of the organization.

VDT: That sort of answers my question on whether there are changes you'd like to see in the program structure. You have said, to accommodate these changing interests.

Some of my interviewees, going back to the early days at Princeton when it was possible to have informal debate in the sessions, have lamented the fact that there's now not time or seemingly the place to have more informal debate, that they have panel sessions now that have ended up being paper sessions. What about that?

MENKEN: I think that the roundtables were a good innovation, in that they allow for smaller discussion. I think that what we're stuck with is that the field has grown. If they wanted to stay with 50 people around a small table, then none of them should have had more than one student in their lifetime!

INTERRUPTION

VDT: Sam Preston has just come in for his mid-afternoon snack of M&Ms, sitting in a lovely glass jar on Jane's desk. That's a measure of the lovely atmosphere around here. Obviously, there's lots of informality and fun back and forth, certainly in a place like Penn's Population Studies Center.

One last big question. What do you see as the outlook for demography in the U.S.? You've just put your finger on one question I had: With applied demographers coming up, not just in state and local government but also in business, is there still room for the basic demographic researcher like yourself? Of course, there are still going to have to be professors, but you've just pointed out that all your professors emeritus are living on and on, spry and active.

MENKEN: I think the answer is yes and no. I think that funding for demographic research is going to be more difficult to obtain. I think we have benefited for many years by being a sexy topic and sexy topics have a way of declining in appeal, whether or not the problems go away or the importance of issues goes away. I was at an NICHD Advisory Council meeting just last week and for NIH grants from that institute, where most of our funding comes from, 19 percent of approved grant proposals will be funded this year.

VDT: That's even worse than I heard from Art Campbell a few months ago.

MENKEN: Yes. And I think that we are going to have to, in many cases, draw back. It's going to be much more difficult.

I don't see an unending population establishment. I do see that more universities, more colleges, will be increasing their interest in having a faculty member with demographic interests. I mentioned earlier Jill Grigsby, who teaches at Pomona College, a small, very good college. I think

that there will be opportunities for people who want to do undergraduate teaching. But I think, realistically, we face a situation in which demographers will have more difficulty in finding the traditional kinds of academic positions.

I do see that there's a greater interest in interdisciplinary kinds of endeavors, that there are more demographers in places like medical schools, and that may be a way for the future. I think we have to realize when we're training people that we're frequently training people who will be applied demographers in a variety of situations, whether they're working for government or for business or working in overseas agencies or a variety of different kinds of work situations.

VDT: For that will they need a Ph.D.? Will a master's do in some cases?

MENKEN: In some cases. But I think . . . I look at science policy and the way it's formulated, or people who are making decisions, and in many cases their knowledge of the area in which they're supposed to be making decisions is abysmal. I think that in the field of population, people who are in many of these other kinds of positions do need a Ph.D. I also think that, even if they move off into other kinds of areas, the knowledge they carry with them and the ability to ask questions and to work to find answers and the ability to ask questions of people who are trying to offer them solutions is sharpened by more years of academic training--twice the master's level training. I know that Bowling Green State now has an applied demography master's program and I think that there are places for that kind of technical training. I would say there's a place for more applied undergraduate training. But I still think there's a very strong place for a Ph.D.

VDT: Thank you very much, Jane. You must go to your meeting--and on to Bangladesh.

PAUL DEMENY

PAA President in 1986 (No. 49). Interview with Jean van der Tak in Dr. Demeny's 43rd-floor office at the Population Council, New York City, June 8, 1988.

CAREER HIGHLIGHTS: Paul Demeny was born and grew up in (hometown), Hungary, which makes him one of PAA's three foreign-born presidents, following Louis I. Dublin (president in 1935-36), who was born in Lithuania, and Alfred J. Lotka (1938-39), born in Poland. He received his first degree in statistics from the University of Budapest in 1955. As a staff member of Hungary's Central Statistical Office, he was at a meeting in Geneva in the spring of 1957, following the Soviet crushing of Hungary's anti-Communist uprising in 1956, and opted to remain outside Hungary. He then went to Princeton as a Population Council fellow and received an M.A. in economics in 1959 and Ph.D. in economics in 1961. From 1961 to 1966, he was Associate Professor of Economics and Research Associate of the Office of Population Research at Princeton. In 1966 he went to the University of Michigan, leaving in 1969 as Professor of Economics and Associate Director of the Population Studies Center. From 1969 to 1973, he was with the University of Hawaii as Professor of Economics and first Director of the East-West Population Institute, which he founded. Since 1973 he has been at the Population Council, where he has been, variously, Vice-President, Director of the Demographic Division and its successor, the Center for Policy Studies, Distinguished Scholar, and Editor of Population and Development Review, which he launched in 1975. He has published widely on the economic aspects of demographic change and population and development policy in less developed countries and is also coauthor (with Ansley Coale) of Regional Model Life Tables and Stable Populations (1966) and Methods of Estimating Basic Demographic Measures from Incomplete Data (1967).

VDT: How did you become interested in demography?

DEMENY: I am somewhat sheepish to claim that I knew more about birth and death rates when I was age 12 than I do now. That must sound absurdly improbable, but it is true. My father was a judge in the Court of Appeals in Hungary. For some reason, he had a special fondness for statistics and in his library there was an impressive number of statistical publications. I regularly raided that library and read into all kinds of unlikely things for a youngster. I got very interested in population matters in Hungary, which were much discussed in the 1930s, the time from which most of this literature, though, of course, I was doing this in the early postwar years from 1946 on. So I had a very early interest in statistics and population matters in particular. And it was natural that I picked this up at the university, where I specialized in statistics in the [faculty of economics?] at the University of Budapest. When I graduated, it gave me an opportunity to get a job.

The job I got upon graduation in 1955 was through a very dear, old friend to whom I owe a great many things in my later career, Louis Dhirring, who was head of the vital statistics division at the Central Statistical Office. He brought me in there and I worked under him as a budding analyst of fertility and mortality statistics and also did work related to the then planned next Hungarian census.

VDT: This was in 1955-56. What happened to you when Hungary erupted in 1956?

DEMENY: Before the big eruption in October 1956, I made some name for myself in my census work. I worked out a proposal aimed to simplify classifications used to analyze occupation statistics. The idea was to have an eclectic combination of statistics relating to occupation, branch of

employment, and status in the profession, this three-way classification in which each has its own tradition, and whittle it down to a small number of composite socioeconomic categories; for instance, to study differential mortality or differential fertility--in general to simplify description while still retaining essential features of these occupational categories.

Now, the Economic Commission for Europe has an annual conference of European statisticians and in that somewhat friendlier atmosphere after Stalin's death, Hungary tried to be more in evidence and cooperative in its official representation. But it didn't have much to offer and it so happened that in September 1956, I think it was, the little memorandum I worked out with this proposal of combined classifications was picked up by the then head of the Central Statistical Office and taken to Geneva and put in with other memoranda as suggestions for this conference to study. And--surprise, surprise!--the French delegate was apparently very interested in this and proposed that the conference study this question and submit a report to the next year's meeting. So the president came back with the message that the Office had had a certain success and they wanted to send an expert group to study this question and work out a report to be submitted to the next year's meeting. There was only one problem. There was no budget to provide translators for the working group and since the proposal came from the French side and the Frenchman who had proposed this was promptly appointed to chair the committee, they decided that the work would be performed only in French. There would be no translation; the so-called experts had to speak French.

My French at that time was about like my English is now; perhaps not as fluent as I am in my ungrammatical English, but I had an easy working knowledge of French at that time. Not having used French for 30 years, my spoken French is now miserable. Well, they told me that really I was too young to be on this working group but since (a) I was the one who knew this subject, having proposed it, and (b) spoke French, I would have to be there, although since I was a very junior person, I would be accompanied by a proper representative of Hungary and I would be there as a technical adviser.

A month or so later came the confrontation in Hungary. You know about those times, I'm sure, if only from secondary sources; it was a very sad time in Hungary. But, I must say, although the borders were totally open for about six weeks or two months after that and one could leave the country without great risk, it never occurred to me to leave Hungary. I felt that people should take life at home, no matter how difficult, and because of my family background, we had had a terrible time after 1948 when the regime changed. But anyway, I didn't want to leave Hungary.

So I was just doing my daily work when this proposal for an expert group suddenly re-emerged in the form of a letter from Geneva to the Central Statistical Office, saying that they scheduled this meeting for April and please send your representative.

VDT: This was by now April 1957?

DEMENY: It was perhaps February 1957; the meeting was scheduled for April 1957. Besides France, who was given the chair, and Hungary, who proposed the matter, the members included Yugoslavia, Belgium, I think, and either Spain or Italy. I was put in the very awkward position that they expected me to go to Geneva; awkward because the political situation in Hungary was so bad and I didn't want to leave. The notion of going to Geneva didn't bother me. What did bother me was the prospect of coming back from Geneva under these circumstances.

Well, I was in an awkward position with the man who was the formal Hungarian delegate; I was his technical adviser. Needless to say, when the Hungarians showed up at this international meeting at that time, they were constantly asked, "What is the situation in Hungary?"--it was still constantly in the headlines. Two days after we arrived, some prominent [journalists] were, for instance, after us. In these conversations, the head of this delegation was, of course, representing the official position of the Hungarian government. And since I was always around and would be brought

into these conversations, he tried to induce me to hold this kind of line and, of course, I refused. A whole series of episodes occurred in these conversations that upset this person exceedingly and it was obvious that I would be in very big trouble if I went back and he reported my bad behavior in Geneva. To make a long story short, at the end of the meeting I applied to the Swiss authorities for political asylum and within 24 hours after they interviewed me, I got it.

A week later, I found myself a student at the Institut des Hautes Etudes Internationales in Geneva. It was my good fortune that Professor Roepke, who was a prominent professor there and had visited Hungary in the past and was in particular fond of my hometown [?], took a liking to me and helped me a great deal to settle in Geneva. In fact, that summer he arranged for me to go to a university course in Nimes in southern France. This was basically an American-sponsored summer institute; people like Henry Commager were lecturers there. For the first time, I had some wide exposure to English. I read English pretty fluently, but it was totally book-learned. I never studied English, never took any grammar lessons, nobody ever taught me English; it was all picked up later. I had great trouble following the lectures in English, both at the Institute and at the summer university institute in Nimes, but it was also clear to me that I understood everything I saw without looking in a dictionary and I wouldn't have great trouble picking up English if I had to swim rather than sink.

As a result, I thought it would be more to my liking if I could get training not at the Geneva Institute, which was primarily international politics and economics, but something closer to the area that really interested me, namely, demographic and its relation to other things, of which there was little at the Institute or the University of Geneva. I contacted Louis Dhirring, my former boss in Budapest, and he wrote to Frank Lorimer, whom he knew through correspondence and perhaps one personal meeting, saying nice things about me.

VDT: You could still write out of Hungary?

DEMENY: Yes. Hungary was never like China or North Korea. Even at that time, for instance, I could have long-distance telephone conversations with my family. I would know that the conversations were recorded but I didn't think anything of picking up the telephone in Geneva and telephoning home.

VDT: Where was Frank Lorimer at this time?

DEMENY: At American University in Washington. What Frank did was to write Dudley Kirk at the Population Council and Dudley, knowing nothing about me, nevertheless promptly sent me a brief letter along with a blank application form for a Population Council fellowship. I filled this out with the help of a Porky Campbell, who was with one of the Smith College contingents regularly at the Hotel de Russie at that time and whom I lost trace of a long time ago. I wrote it out in French and she supervised the translation into English. And, probably in early September, I sent this application to the Population Council. It was a little late in the game and I didn't think much might come of it. But all of a sudden, lo and behold, I got a special delivery letter cable, locally posted, saying that Dudley Kirk is in the Hotel Beau Rivage and would like to see me that day to interview me. Beau Rivage is a lovely hotel right on the lake.

VDT: I know, it still is; costs a lot of money.

DEMENY: It has a certain Austro-Hungarian memory attached to it, because Queen Elizabeth, Franz Josef's wife, was murdered in front of the hotel and was brought into the hotel, where she died. There's still a little memorial room commemorating this event.

I showed up at this beautiful Beau Rivage and had lunch on the terrace with Dudley Kirk, a marvelous gentleman, as I felt right away. It was just my good fortune that he took a very strong interest in me, a certain liking it must have been, because he virtually on the spot told me that he would get me a Population Council fellowship and suggested that I go to the University of Chicago.

It was very impertinent and ungrateful on my part, but I confessed that I had virtually no knowledge of the American university system and very little about America. I knew pretty much the demographic and population scene in Western Europe, particularly on the continent, but had little notion about America, with the single exception, of course of Princeton. I knew about Princeton, because I knew works by Frank Notestein and Population Index, which at that time was the only American periodical in demography. I also knew that Princeton is located close to the Atlantic Ocean, whereas Chicago is very far from . .

VDT: I'm sure you thought of Chicago as a wasteland.

DEMENY: Well, I don't want to bore you with such detail.

VDT: Wonderful story!

DEMENY: Chicago at that time in Hungary was very much Al Capone. There was a district in Budapest with, I'm sure, the highest crime rate in the city, for which the popular name was "Chicago." I'm not suggesting that these were important considerations, but the positive one was that I knew Princeton was a marvelous place and had great professors. And I told Dudley, like somebody who had made a valid fore-judgement, that it would be great to go to America, but could I go to Princeton.

Of course, Dudley told me that he had been in Princeton for a number of years; I didn't know that originally. He had worked on the League of Nations European population studies during the war; he wrote a great book [Europe's Population in the Interwar Years, 1946]. Dudley was skeptical and said I would have much trouble there. I must say that much of this conversation was in French and my French was greatly superior to Dudley's. But he consented, with the proviso that he would, of course, have to find out if Princeton was willing to take me. Essentially, we parted with the notion that the Council would bring me to the United States and probably to Princeton, subject to their accepting me. Their acceptance was not for any formal course or degree work; it was to do one year's work on a Population Council fellowship, as a sort of visiting fellow at Princeton.

Now a word about my arrival in America, which was on October 3rd, 1957, and indeed I was told that I could go to Princeton. I arrived at Idlewild Airport, which was just being built and in a shambles at that time. Someone from the Population Council was waiting for me, put me in a taxi. We went to Pennsylvania Station, the old station, demolished a long time ago, and I was put on a train to Princeton. I was told that I would have to take the shuttle there and when I arrived at the end of the shuttle, there was Warren Robinson, who was a graduate student there, who took me over to OPR [Office of Population Research] to see Frank Notestein.

Notestein wasn't in, but while I was waiting a few minutes for him to arrive, another student came. Unlike Robinson who was a graduate student, this was obviously an undergraduate, with tennis racquet and shorts, and he greeted me. Well, it turned out that the undergraduate was Ansley Coale; he looked like a sophomore.

VDT: He'd been an undergraduate 20 years before that!

DEMENY: Right, but he still looked like a sophomore. That's how my career at Princeton started.

That very evening I was ensconced in the annex of the graduate college and had to eat my dinner in a black bachelor's gown in the chapel of the graduate college, a very peculiar place at that

time. Women were still not permitted to enter that holy of holies, except that we were served by young women dressed in white starched uniforms. Life at Princeton was very special.

VDT: And this was only a few months after you left Hungary; amazing how quickly things happened.

DEMENY: Indeed, it was like a dream, and life on the Princeton campus at the graduate college was just marvelous, although very difficult for me, because my English was in very poor shape.

At the same time, I was eager to take formal graduate courses and I was permitted to attend the courses, as an auditing student, of such luminaries as Professor Baumol, on economic theory, and Professor Jacob Viner, on international trade. I was also permitted to take examinations and by the end of the second semester, I had graded [accepts?] from five or six graduate courses. Well, I wasn't a straight A at this time; I was an A in [? 's} course, which was the most important course there. They turned me into a regular student by the end of the year and I got a second-year extension of my Population Council fellowship and stayed at Princeton and got an M.A. in 1959. Then I got a Milbank Memorial fellowship and got my Ph.D. in 1961.

Now, I was under the tutelage of--my boss was--Frank Notestein during the first year and a half. Then Frank left to be president of the Population Council and my Ph.D. was done under Ansley. Of course, I took courses from Ansley too, so Ansley became the professor who was my mentor there, although Frank always maintained a very kind and paternal interest in my progress and later career. I was just very fortunate to have great opportunities.

VDT: You had great opportunities but obviously you seized them too. You arrived in October 1957 and had everything finished for your Ph.D. by 1961; obviously you were strong enough to write a dissertation, for Princeton, in English. What was your topic? Were you beginning to work with Ansley Coale?

DEMENY: No, it was in economics; I think it was investment allocation of population growth. The work on life tables with Ansley was partly later. My main task there was computer programing. At that time, very few people knew computer programing and I taught myself a couple of computer languages. I started in computer programing in 1959.

But I wasted much time in getting to know the country; I wasn't totally ensconced in the library. I had a very happy and different life there.

VDT: The Population Council and Milbank Memorial Fund fellowships gave you enough to live on, in addition to tuition?

DEMENY: Well, it didn't make you rich, but it was a very generous fellowship, particularly since it was combined in the first two years with this housing in the graduate college, and that was just great. All you needed was a bicycle to bicycle from your graduate college residence to OPR at the other end of the campus. I didn't move much out of Princeton during those first few years. Yes, it was perfectly sufficient for a bachelor.

VDT: Then when you finished the Ph.D. in 1961, you stayed on.

DEMENY: I stayed on as an assistant professor of economics in the department. Besides population, I was teaching [complex?] economic systems and economic theory, which was what assistant professors did, teaching three subjects; small-group teaching was part of a large course.

VDT: And you were doing research. In those years, you and Ansley did your regional model life tables.

DEMENY: Yes, I dealt with various subjects.

VDT: The one I particularly like was "Early Fertility Decline in Austria-Hungary" [Daedalus, 1968]. That must have been a forerunner of the European Fertility Project.

DEMENY: Yes, it was an early result from that. I dropped out as an active member of the project. Unfortunately, I never finished up my book on Hungary, which I was supposed to write; an unfinished project. I don't know how to describe myself, but I'm a great one at writing a paper and then feeling that it shouldn't be in the public domain and filing it away and just letting it gather dust.

VDT: Then you were also involved in The Demography of Tropical Africa [1968] at Princeton. Was that sort of the beginning of your interest in the demography of developing countries? Where did that come in?

DEMENY: No, it wasn't; it was strictly demography. My main interest was economic aspects of demographic change, development economics. I had to do rigorous coursework in formal economics for my Ph.D. in economics and some of those courses were extremely stimulating and memorable. For instance, I took Jacob Viner's international trade course twice, I liked it so much. I took his history of economic thought course; again, a marvelous intellectual experience. I was interested in development economics.

You ask where it comes from. I always thought of Hungary as a developing country, in many ways, and even my experiences as a child and youth I felt were relevant to many things that were happening in the Third World. As a matter of fact, Princeton saw Hungary that way. One of the important works of this League of Nations series was a study by Wilbert Moore on The Economic Demography of Eastern and Southern Europe [1945], including Hungary. I did some research assistance work for Wilbert Moore, a very eminent person who at that time was still associated with Princeton's sociology department and with OPR.

So the development aspects of population change were very much in the center of my interests at Princeton, although it is true that a good deal of the work at the early stage of my career, perhaps primarily under Ansley's tutelage, was analytic demographic techniques, particularly how to make silk out of sow's ears, namely, how to do demographic estimates from limited bad data. One manual from the United Nations that I coauthored with Ansley was on this subject [Methods of Estimating Basic Demographic Measures from Incomplete Data, 1967].

But, eventually, I kind of grew tired of that work, which was finding out whether a country had a birth rate of 46 or 44 or 44.2. I thought it was fascinating but perhaps not something I would want to spend a lifetime on; I hope that doesn't sound bad. My interest was more in the direction of development aspects of population change and also population and development policy. Also, I always had a historical interest. I didn't finish my work on the Austro-Hungarian demographic change but I always spend a good deal of time reading ancient [demography].

VDT: That showed in your PAA presidential address, "Population and the Invisible Hand" [published in Demography, November 1986]. I was going to ask you about your good historic sense, but now you bring it up, with your Austro-Hungarian article as an example.

DEMENY: One thing I didn't want was to become tagged as an expert on Eastern Europe. I thought that I'll just be Hungarian; it's not something that should stamp me forever in professional activities.

So I never had ambitions to do much academic work in area studies that related to the area I was most familiar with, namely, Hungary.

My interest in other places of the Third World was perhaps manifested in a paper I did for the World Bank, a background paper presented at the World Population Conference in Belgrade in 1965, called "Demographic Aspects of Savings and Investment." I think that paper, more than anything else, had to do with Michigan approaching me and Ron Freedman routing me to Ann Arbor, which happened in 1966.

I went to the University of Michigan as an associate professor of economics and then research associate of the Population Studies Center, of which I became associate director a year later, in 1967. Ron Freedman was director and the founder of that institute, a very stimulating, active, and prominent place. I didn't stay long in Michigan, although I did achieve a full professorship in economics in 1969.

VDT: Very young!

DEMENY: Yes, but very soon afterward I left Michigan. In fact, in between I was away for a semester teaching at Berkeley as a visiting professor in that demography department, chaired by Judith Blake at that time, a very interesting place. Nathan Keyfitz was there and I met a number of young people who were taking courses I gave and I was fortunate enough to persuade some of them to come to Hawaii, where I moved in late 1969.

That was kind of an adventurous move and I must say that many of my friends, not only at Michigan but elsewhere, took a rather skeptical view of how much sense such a move made. Hawaii wasn't on the map as one of the leading lights of academic study in the United States and resigning a full professorship in economics and associate directorship in the population center at Ann Arbor seemed to most intelligent people a very foolish thing to do. Well, I was 37 years old then and I thought that having migrated from Hungary to Geneva to Princeton to Ann Arbor, I might just as well go as far West as I could without getting into Japan or China.

VDT: Hawaii approached you, did they?

DEMENY: Yes. What happened had to do with the East-West Center, which at that time was formally associated with the University of Hawaii but was nevertheless a governmental institution, established on the Hawaii campus by Lyndon Johnson as an incorporation between the federal government and the state of Hawaii. They got a large grant, for no good reason because they had nothing there whatsoever, to move into the population field and they looked around and approached me with an offer to be in charge of building up a program of population studies.

So I went to Honolulu, joining the University of Hawaii as a professor of economics, but mostly because of the prospect of setting up something at the East-West Center. That was a difficult battle, but I won it very quickly; namely, I persuaded the chancellor to make me the director of a formal institute called the East-West Population Institute, which, unlike the then programs of the East-West Center which were entirely service programs--arranging visits between East and West and arranging fellowships for people who then studied at the University of Hawaii--became a program to do research and analysis and was formed as an institute within the Center. I had a single secretary and a retired ambassador, Ambassador [Giftcock?]; he was an ambassador at the [?], who was the person who obtained the grant for the institute from the State Department, using his political connections. So I went there and stayed for four years.

VDT: Was the ambassador on the staff?

DEMENY: No, he quickly retired. The institute was my brainchild. As a matter of fact, subsequently the Center used it as the model for four more such institutes in other spheres.

VDT: That must have been unusual; you had no contacts with people in the Pacific Rim countries.

DEMENY: That's true and, of course, I had to compensate for that lacuna in my background by constant travel. I did very little teaching during those years.

VDT: Had you been involved with the Taiwan program when you were at Michigan?

DEMENY: No.

VDT: You were constantly traveling, setting up contacts?

DEMENY: Our son, our second child, was born there and my wife has exact records of how much I traveled. On the average, I spent half of each of those four years away from Hawaii in various countries in Asia, from Japan to Pakistan. It was very interesting and stimulating but I think a little hard on my dear wife--and hard on me too. But the institute thrived and when I left, there were 18 PhDs on the staff. And I am glad to say that ever since the institute has been well established, a center of research on the map; very satisfactory.

VDT: The students that are there, are they at the insitute or the university?

DEMENY: The students who studied had to study at the university. As I said, I was on the university faculty; I taught courses. But my main aim was to have a research center. Most of these 18 PhDs had a joint appointment between the university and the Population Institute but the institute had to get half-time, six months a year, of the researcher's time. So it was essentially a research program focused on the Pacific area, Asia all the way to Pakistan. South Asia was included, although most of the work was Southeast and East Asia. Very little on Mainland China, which at that time was kind of an impossible place to reach.

It reflected my interest in trying to institutionalize population research in ways that go beyond a research program subordinated to a teaching program or to the overlordship of a particular department. The group I got there is really eminent; many of them still there.

VDT: Was Jay Palmore there at that time?

DEMENY: Oh, yes. I lured Jay away from Michigan barely after I got there from Michigan. And I got people whom I taught at Berkeley, such as Geoffrey McNicoll, whom I subsequently lured to the Population Council. Bob Retherford and [Buckheimer?], also from Berkeley, Jim Fawcett from the Population Council, Lee-Jay Cho from Chicago and Michigan. And a number of very fine people who were in the departments of sociology, geography, and economics, who then got joint appointments at the institute. Griff Feeney from Berkeley, Fred Arnold from Michigan, a former student of mine there. And all these people stayed and thrived and became very well-known figures in the profession.

Why did I leave? Well, as I said, when I went to Hawaii it was an adventure, to get more in my life. I had taken big risks before and I thought here is another such opportunity. But I didn't really picture myself staying forever in the middle of the Pacific Ocean, much as I enjoyed it. I never got to the beach; I had to work day and night or travel.

One day came an approach from the Population Council. Barney Berelson, the president, gave

me an offer that was extremely tempting and eventually I found it irresistible, namely, to come to Population Council as a vice-president and director of the demographic division, with the mandate of building up a much broader-based social science program in the demographic division.

VDT: Dudley Kirk had left long before this?

DEMENY: Four or five years before [1967].

VDT: You were to build up the demographic division?

DEMENY: To be in charge. Perhaps buildup is not the right word, but to shift its direction in a way that I tried to do, was doing, at Michigan, of which Barney was well aware, namely, to have a more interdisciplinary group, focusing not so much on demography as such but what demography means in economics, sociology, ecological studies, and so on. That is, less demography than social science.

I decided to do this in Tokyo, of all places, where there was a Second Asian Population Conference where I was in the one and only formal diplomatic capacity in my life. I was deputy head of the U.S. delegation of which Philander Claxton was head. That was in November 1972. By mid-December, I said yes to the Council, having come to New York and discussed the arrangements. I came back to the East coast in 1973.

Well, ever since I have been at the Council and it was a hectic event for history. For the first two years as director of the demographic division, my main ambition was to expand the international outreach of the division, bringing in people who then got appointments as members of the demographic division serving overseas, including people like Fred Shorter, Ron Lesthaeghe, Hillary Page, Dennis Detray, Allan Hill, Frank Mott, Doug Ewbank, Shea Rutstein. These people were scattered from Peru to Nigeria to Kenya to Tanzania to Pakistan.

VDT: And you set up all the arrangements for them to go there?

DEMENY: Yes. Well, of course, there was a well-established tradition, but the aim was to broaden the interests beyond just demographic measurement. People like Dennis Detray, now with the World Bank, were taken as development analysts. They were not demographers but of broader social science interests.

But this came to a fairly early end, unfortunately, because of the resignation of Barney Berelson as president, which was followed by a longish period of uncertainty at the Council, eventually resolved by George Zeidenstein coming in as president in 1976. As a result of the new president's reorganization of the Council, the demographic division was succeeded by a Center for Policy Studies. I proposed both the name and the substance of the group. What I didn't propose was that the Center be limited to New York staff. The overseas staff of the Council was assigned to the unified management of the international program, of which I have no management responsibility. Thereby the Center was put in a somewhat isolated position on the 40th floor of 245 Park Avenue, first, and then on the 43rd floor when we moved to Dag Hammarskjold Plaza. But it was a very fine interdisciplinary group with many eminent people on the staff, most of them brought in by myself. I would mention primarily such fine people as Geoff McNicoll, who followed me when I moved from Hawaii, John Bongaarts, Michael Todaro, and, later, people like Odile Frank, Samuel Lieberman, Susan Greenhalgh--a distinguished group, indeed.

But after this isolation of mine, or, rather, removal of responsibility for overseas, there was a bit of a limited mandate. I tried very hard to change the situation, by trying to persuade George

Zeidenstein to give back to the social science group direct responsibility for overseas staffing. The overseas staffing went more and more into technical assistance and foreign assistance, AID-related work assignments. Social science work overseas, I believe, suffered greatly.

What I did in compensation, however, was something that I started already in the second year that I came to the Council, that is, before Zeidenstein came in, based on my old feeling that the field lacked a periodical that would bring into focus these broader social science and development-related issues of population change. In 1975 I launched Population and Development Review.

VDT: That was your invention? I was going to ask about that.

DEMENY: Not only my invention but it was a difficult birth, because all my good friends took a very skeptical view of this--not just friends, but senior advisers and mentors. People felt that the last thing the field needed was another periodical. Frank Notestein told me that anything written in this field would always get printed somewhere; we didn't need a periodical. Ansley said the same thing, and Ron Freedman and Parker Mauldin. But I had a pretty clear blank authorization from Barney that something that I really wanted to do, he would back me up. And while Barney himself was very skeptical about the journal, when I said I wanted a journal, he said, "Well, fine, go ahead." And so in 1975 this journal was born.

VDT: It was well timed; it came after the Bucharest population conference, where the slogan was, "Development is the best contraceptive."

DEMENY: I think the journal made a substantial contribution to the field.

VDT: Undoubtedly. It's the most readable thing in the field.

DEMENY: And it's not easy to sustain because this is not the normal mode in which population students write or work.

VDT: You try first to get readable articles?

DEMENY: Pretty readable and competent too; there's a lot of editorial push and pull when they start off. But mostly to open windows to neighboring areas more than existing journals, certainly Demography, even Population Studies or Population; namely, to see the field as broader social science, rather than a technical organ of a discipline that became, in my opinion, more and more narrowly focused and more and more demographers talking to each other. The field wanted to elevate itself to a respectable department-level discipline and that meant more and more specialization and a nose-to-the-grindstone attitude to focus on demographic analysis and sort of shun contacts with neighboring disciplines--economics and sociology, the nearest neighbors, not to speak of history and philosophy--to shun, if you will, policy studies, development economics. I feel there is a tendency in the profession that paralleled the positive aspects of specialization and scientific rigor, but at the expense of an excessively narrow focus and too much inward-looking and demographers talking to demographers. Both the Center for Policy Studies concept and, in particular, the journal tried to counteract this in ways that I hope were at least partly successful.

VDT: Oh, absolutely, I think you have. Although I'm inclined to think that some of these things have been tried in the past, certainly with the economists.

DEMENY: Yes, which is already a big breakthrough. But we tried hard to bring in anthropology, for instance, village studies, and the more fashionable aspects of modern demography, namely, survey research.

VDT: You haven't had too much on the World Fertility Survey; you've had lots of studies in family planning, of course.

DEMENY: Yes, not by accident, certainly by design.

VDT: Did you inaugurate the historical section in PDR? I realize now that you must have.

DEMENY: Oh yes, the archives . . .

VDT: The readings of the great predecessors.

DEMENY: It's 95 percent of them. I write those little introductions to them.

So, I got a good deal of enjoyment from the journal and was very gratified by the very good reception it got from the profession.

VDT: Absolutely. It must be tremendous work, though; each issue is so thick and you've got authors all over the world.

DEMENY: It's a lot of work. It's like putting out four books a year and a huge amount of work goes into it. I was fortunate to have very eminent colleagues--from the very beginning, Ethel Churchill as managing editor. In the first few years, the two of us were basically the journal. Then after a few years, things got formalized with an editorial committee, with Geoff McNicoll in particular, Michael Todaro, Mead Cain, and, later on, Susan Greenhalgh and Odile Frank. Odile Frank is now gone. The Council was a very hospitable home for the journal.

VDT: You talk about it in the past tense; it's not past tense, is it?

DEMENY: Well, I hope it remains. My status at the Council will change as of next January 1st [1989]. There will be no Center for Policy Studies; it will be replaced by a research division. And I retire from management responsibilities at the Council and will be so-called Distinguished Scholar here. I anticipate full freedom to do my own thing--write and, most of all, devote more time to editing the journal.

VDT: You might get it out a little faster; it's getting a little behind.

DEMENY: I must say, particularly under the impact of the internal turmoil in the Council for now quite some time, the history of the journal--it's badly behind and my first ambition is to bring it back to the normal schedule.

VDT: Well, it's eagerly anticipated. I grab my copy, read it immediately, and phone my former my former Population Reference Bureau colleagues and say, "Have you seen this in PDR?" and they say, "Has it come in?"

DEMENY: It's very kind of you, Jean, to say that. I must say I'm loathe to compromise on anything for time reasons. In the long stretch of history, I feel that if somebody three or four years from now--

which is a short time--picks up the journal, he will not say, "My God, this March issue came out really in May; how come?" But if he sees it's poorly edited . . .

VDT: We don't all have to emulate American Demographics, which comes out a few days before the month it's dated.

DEMENY: Well, look at me a year hence and my hope is that I can tell you that we are totally on time and maybe we appear the month before.

VDT: Here's a big question, you've answered it some along the way. Who have been some of the leading influences in your career? Now, obviously Dudley Kirk in a sense, Louis Dhirring already in Hungary, and Ansley Coale.

DEMENY: You have named a few of them. In Hungary, Louis Dhirring; now he has died. He came from a very distinguished family. His father was very well known in the International Statistical Institute, had an illustrious post there, and was a prominent scientist in statistics and economics in Hungary.

VDT: You kept in touch with him until his death?

DEMENY: Yes. I became an American citizen in 1966. By that time, there was a general amnesty in Hungary, so it was very easy to travel back, although I must say that when I first went back to Hungary, I was a little nervous. In 1966 I was in Vienna with Ansley Coale and Ansley had an invitation to Budapest and he said, "Come on, Paul, let's go to Budapest." And I said, "No, no." And he said, "Oh, come on now." And we rented a car in Vienna and drove into Budapest. That was my first visit since [I left in 1957?]

VDT: Did you have a visa?

DEMENY: Yes, you had to have a visa. Now you can even fly in without a visa and I have been back in Hungary about 60 times.

VDT: Three or four times a year?

DEMENY: That's right. I was there two weeks ago to see my mother, who was 80 years old, and in February, July, and November last year [1987].

VDT: You're a contemporary of Andras Klinger; were you in class with him?

DEMENY: Yes, I know Andras very well. In fact, he worked at the Central Statistical Office with me and he's a good friend.

VDT: One of the first tasks I had in one of my jobs was to tour him and Szabady around Washington. I have a photo I took of them in front of the Capitol.

DEMENY: Andras had the same boss as I at that time, Dhirring. I'm talking about back in the 1950s. Szabady was there but he was a kind of super boss, an elevated kind of bureaucrat.

[On my influences], I should single out Dudley Kirk; most of all Frank Notestein; Ansley Coale

very, very warmly; and Frank Lorimer.

VDT: Tell me about Frank Lorimer.

DEMENY: Frank was just a unique and terrific person. He helped me at the very beginning, as you know. Then when he came to Princeton, he looked me up and I looked him up and we were in touch. Then Frank went into the Africa studies phase of his and came back to Princeton to join the Africa project. He was part of that book we did [The Demography of Tropical Africa, 1968], by William Brass, Coale, Demeny, Don Heisel, Lorimer, Anatole Romaniuk, and Etienne van de Walle. So there was a period of perhaps one and a half years when we were in daily contact.

VDT: I thought he stayed in Washington, but he physically came to Princeton?

DEMENY: Yes, he came. He rented a house there and he was at the office. He was just a very remarkable fellow--in many ways, a cantankerous and difficult person, but extremely interesting company, with lots of ideas, sometimes strange ideas.

VDT: He lived on a commune at one point.

DEMENY: Yes. He hailed from a Protestant minister's family; he himself was an ordained minister at one point. Frank also liked my wife Lynn very much. Then he and his wife had a child, who was about the same age as my daughter.

VDT: With the nurse, whom he met in a bar in Nairobi.

DEMENY: Yes, Petra, a New Zealander. To be honest, I had more disagreements than agreements with Frank, but I had enormous respect for him. As I said, he was a very intellectually stimulating person to talk with, even though the stimulation was partly by eliciting your disagreement with some of his ideas and work and views.

Then the friendship and influence of Ron Freedman were very much appreciated. Also there were some of the younger scholars at Princeton: Charlie Westoff, Bob Potter, a very dear fellow, perhaps you know of.

VDT: Yes, of course, a mathematical genius.

DEMENY: And Wilbert Moore, as I mentioned. He was still there, but then his wife died and he left. And Barney Berelson was someone I looked up to.

VDT: Tell me about him. He was not a demographer, he came from another field.

DEMENY: Communications. He was a Chicago man.

VDT: How did he get into this field?

DEMENY: I think Barney was primarily Frank Notestein's discovery for population. Frank had dealings with Barney at the Ford Foundation. Frank was a Ford Foundation executive and he spotted Barney for his interests and ideas and lively mind. He lured him over to the Population Council as a vice-president to do communications. And the Council tried to dabble in odd things, contracting with

Walt Disney to do a population information film; that was back in the early 1960s. I think Frank Notestein and Barney were much on the same wave length. Frank liked his lively mind and colorful English and when Frank decided to step down from the presidency, Barney was the logical successor to him. Barney's collected papers in population have just appeared. I call your attention to it.

VDT: Berelson on Population, edited by John Ross and Parker Mauldin, published by Springer.

That's a nice picture of him too.

DEMENY: I should add to that list, in a different category--much more a coworker and collaborator--I tremendously enjoyed interaction with Geoff McNicoll, a brilliant fellow, whom I lured to the Council and who was at the Council until two months ago when he left, to my great chagrin, to join ANU's demography department. Geoff is an Australian, so he was going home in a sense, but I hope that one of these days we will be able to attract him back to the Council.

VDT: Australians, or people who are there, never seem to stay put; I think they are the most peripatetic demography department. Jack Caldwell doesn't often seem to be there; Lincoln Day is as often in Washington as anywhere else.

DEMENY: Another person whom I had much contact with and, in fact, was successful in persuading him to accept an appointment at the Population Council was Davis Glass.

VDT: Was David Glass ever at the Council?

DEMENY: No. When he was about to retire from the London School of Economics, I offered to meet him in London and then he was here and I persuaded him to come to the Council as senior scholar to do what he wanted, and he agreed to this. He was to come for eight months and be free for four months to do other things, consultant to go to India and back to London. He died before he could take up his position.

Another person I worked closely with and who was associated with the Center for Policy Studies here for a number of years, an outstanding scholar, was Harvey Leibenstein of Harvard, a Princeton Ph.D., a very dear fellow.

VDT: That's a long list of those who have been leading influences in your career.

What explains your elegant writing style? You claim you had only book-learned English 30 years ago, but you have superb English and your writing style is really admirable. Is that a classical education in Hungary?

DEMENY: You are very kind and I don't take that at face value.

VDT: I mean it; I'm an editor and I don't say that often.

DEMENY: My spoken English hasn't improved in the last 20 years or so, my accent.

VDT: You finish your sentences, which is more than most English-speaking people do.

DEMENY: I write very slowly, I must say; I don't write easily.

VDT: I see a Word Perfect book, do you have a word processor?

DEMENY: That's a new thing. Last October I bought a word processor for our son for his homework

and I very quickly discovered that that's indeed a very nice device and I have done a lot of work at home and I carry floppy disks back and forth.

VDT: Had you reviously typed or done everything longhand?

DEMENY: I never could do typing. I always worked longhand or dictation. I never dictated serious writing, just letters. But it's easy with a word processor, because whatever you don't like you can put into instant oblivion, demolishing it, whereas in a typewriter, it stares back at you. I like it.

VDT: You'll be interested to know that Ansley Coale is doing the same. He'd been doing a lot with Lotus, doing tables, but he hadn't actually been doing text with a word processor, but he said he was going to learn to do that this summer. You and Ansley actually look a bit alike--curly hair and square heads.

DEMENY: He's a marvelous fellow, admirable, ideal teacher and professor.

VDT: Did you study with Charles Westoff or was he just a colleague?

DEMENY: I never studied with him. I once took a course he taught, but it was never a close interest of mine; it was survey research.

VDT: Where did you meet your wife?

DEMENY: At a cocktail party three blocks from here.

VDT: Wow, you mean real genuine relationships come out of New York cocktail parties!

DEMENY: A few months later, I sent her a cable from Europe proposing marriage; I was traveling. She came over to Europe and we got married in Geneva.

VDT: Good heavens, just like that. Is she American?

DEMENY: Yes, from Massachusetts.

VDT: That's very romantic. Another big question and you've answered it also in a sense, certainly through your writings: What do you see as leading issues in demography over the years you've been involved? Obviously, you have been most involved in the issue of population trends in less developed countries and with their impact on economic development and so on. A lot of your writings have focused on the rapid population growth in developing countries.

DEMENY: As I noted, I had a very early liking for this field and I have a great respect for the scholarly production that is going into it. But I always felt that as a field of discipline, it's not among the glamorous spheres of study for its own sake. As in this little paper that I sent you [Social Science and Population Policy, Working Paper No. 138, Center for Policy Studies, The Population Council, May 1988], based on a lecture I gave at the Carolina Population Center earlier this year and a paper I gave at the New Orleans PAA meeting [1988, in the session, "Two Centuries after Malthus: The History of Demography"], I note that there are fields like Assyrian architecture or musicology that I can see being studied for their own sake. But for demographic study, I think the justification for it has to be that it leads to some improvement in the lot of mankind, that it's basically a science that should

lead to useful applications in particular matters of public policy; that is, demography cultivated for its own sake doesn't have tremendous appeal. God knows, the big population issue of the second part of the 20th century is this inordinately rapid expansion of the human population that is probably much faster than would be consistent with the best interests of mankind and its future. So I have always been interested in trying to see that what population studies does is something that can be effectively used, influential in shaping public development policy.

VDT: In this paper, you claim that most funding for current population research comes from what you term the "population industry" and that research must be directed toward justifying family planning programs, or "operations research," as you call it. You're saying now that demographic research is most valuable when it leads to population policy. But in this paper, the way I interpret it, you also are saying you don't think it should be devoted to operations research, program research. What kind of research do you think would be ideal?

DEMENY: I see the field as in many ways in bad shape, because the very richness and funding of population programs and their ownership, so to speak, by international agencies and foreign assistance programs resulted in these programs having overweening influence and a determining hand in what gets studied and even with what results, and how the results are used. Population research got institutionalized and in a very palpable way subordinated to the day-to-day needs of international population programs. And this is not how research should be done. In order to get useful knowledge, you have to have a more detached and independent base, to look at the world and try to figure it out, what is going on, with what results, how it could be changed, rather than simply kowtowing to existing population programs, which have to spend hundreds of millions of dollars, which means that they have to have very definite notions of what it is that they want to do. And they expect the research establishment to certify that what they are doing is just right and just what the doctor ordered.

So I see kind of an institutional design in the way of the financing and the use of population research. I think what would be needed is a much more conscious separation between operational programs and demographic analysis and analysis looking at what demographic trends do to society and what society can do about those trends, to the extent that it doesn't like them.

Now, you might say that don't universities do this? My answer would be that universities could do it and ought to do it, but I'm not sure that they are doing it, or certainly not doing it to the optimal degree.

VDT: Doing it with respect to the problem of the rapid growth in developing countries?

DEMENY: Well, indeed, I said that the rapid growth is the most manifest element in the second part of the 20th century. But the issue is broader than that. There is also spatial distribution, mortality trends. And, of course, there is the potential problem of unduly low fertility, which affects Western Europe and North America. I don't say that is necessarily a problem, but at least it should be the object of sustained attention and analysis by population scholars.

VDT: In addition to Ben Wattenberg [The Birth Dearth: What Happens When People in Free Countries Don't Have Enough Babies?, 1987].

DEMENY: Ben Wattenberg doesn't do it in a very scholarly fashion, but certainly he deserves commendation, in my opinion, for being alert to what is potentially an important issue. It's characteristic that he comes from a think tank and not a university, although universities are not so far behind. We ourselves in cooperation with another think tank, the Hoover Institution, had a conference

on this topic about the same time that Wattenberg's book was being written and we published a special issue of Population and Development Review.

VDT: Excellent publication: Below-Replacement Fertility in Industrial Countries [supplement to Vol. 12 of Population and Development Review, 1986].

DEMENY: But, again, this illustrates that the field is too fractioned, fractioned into many university-based centers. Some of them are very small in size and each of them, I think, especially and unnecessarily, is under the sway of their teaching programs and under considerations that have to do with department ownership and departmental organization. And a field like population studies, which is interdisciplinary and should be extending its reach in the direction of neighboring disciplines or attracting people like historians and economists and philosophers and biologists from other disciplines, it doesn't have a natural and comfortable home for it.

Now, universities organize their programs either under the ownership of a major department field, like sociology, or follow the inclination--understandable in a university setting--to make population studies, demography, an independent discipline. This further narrows the field of investigation and makes it more of an esoteric study than it ought to be.

I think the field cries out for some institutional base that has some minimum critical size and is not subordinated to the whims of AID or UNFPA or even foundations. Now foundations used to be--perhaps if you have read Notestein's PDR article of December 1982 about this field ["Demography in the United States: A Partial Account of the Development of the Field"], he points out what important influence foundations had in this field. He compares the innovative mass and the new ideas being injected, how much more effective foundations were in the past than universities. And he's a good judge of this. But I think the recent performance of foundations in this field is very mediocre and betrays very little independent thought or willingness to stake out positions.

VDT: And you had hoped that was what you could do at the Population Council? Were you promised that by Berelson?

DEMENY: Well, I did. I don't like to talk about the [politics] of this institution. But this is an institution that doesn't have financial independence, certainly not the kind of financial independence that can sustain a \$24 million annual budget, and this is our current annual budget. That means that we really have to be very close and on friendly terms and look for the direction from which the wind blows to be able to sustain our programs at this high level. So what AID or UNFPA wants is terribly important for us and we can ill afford to be independent from them. An alternative to this would be to live with a much smaller staff. If this were a much smaller institution, I think it would be in many ways more influential and more important. But it's simply not in the cards the way things are organized now.

In this paper [Social Science and Population Policy], I make a heuristic reference; I'm not saying that it is necessarily the model to follow. In a country so much smaller and by any standard poorer than the United States, they have an INED-type institution, an independent research institution, government supported, that is not at the beck and call of any administration, that has a mandate of formulating a scientific program and keeping an eye and fingers on the pulse of what is going on. [The paper refers to Canada's International Development Research Centre and the German Foundation for International Development in the then Federal Republic, p. 28.] INED [Institut Nationale des Etudes Demographiques] in France [deals] essentially only with France; they are interested in French national issues. Well, show me the institute that is comparable in scientific standing and institutional integrity that works in the field of, say, Third World population issues. There is simply no such thing.

There is the United Nations Population Division and they do fine work, but . . .

VDT: Number crunchers.

DEMENY: Well, partly that and partly, of course, they are hemmed in with the terrible personnel policies and constraints that characterize United Nations organizations. You have to watch to see, "Do we have too many Indians; shouldn't we hire a Pakistani; or why don't we have somebody from Brazil?"

VDT: They also don't have people from a broad array of disciplines. Does INED, for instance, have, as you mentioned, anthropologists, biologists--people from different disciplines?

DEMENY: Its emphasis is in demography, but they do have an impressively wide [staff/grasp?], including, for instance, genetics, human biology.

VDT: You mentioned in this paper the independent researchers like Gunnar Myrdal, his American Dilemma and Asian Drama, and INED, and I asked you over lunch if you would like to see innovative research like the Caldwells have done in South Indian villages and Nigeria.

DEMENY: Their innovation in Nigeria was under Population Council auspices. Jack Caldwell is a flower cultivated by the Population Council.

VDT: Meaning you?

DEMENY: No, before my time. We overlapped. He had been in the demographic division perhaps not quite a year when I came in, then he went back to Australia. Jack's launching as a field researcher was a Population Council product.

VDT: Do you consider what he did operations research?

DEMENY: No, not at all. Operations research should now be interpreted as what it predominantly is; you can measure it by dollars and cents, namely, what AID calls operations research. Not to object against the term, but the way it is interpreted by AID, it's really a very misconceived direction of work.

VDT: Don't you think it a bit ironic that AID is still pushing operations research, still pushing family planning programs? Yet, as you said in your 1986 PAA presidential address, "Population and the Invisible Hand" [published in Demography, November 1986], when we were all in the throes of reacting to the NAS report [Population Growth and Economic Development: Policy Issues, 1986], which in turn was a reaction to the U.S. turnaround in policy at the 1984 Mexico population conference, you made it very clear that we still needed, in your estimation, family planning programs; that one couldn't just say population is just one of the many issues that one takes into account in economic development. You were bearing quite hard on the downgrading of population in the NAS report.

DEMENY: There is no contradiction here, because I indeed take a very dim view of what is happening in the Third World, the rapid population growth. But I feel it is almost scandalous how little original thinking bubbles up in the countries that are most affected by this. This is something that should be of [grass roots] in the countries themselves and they should be helping themselves. It should

be a more autochthonous process than it is now. It is now more of an international business that exports readymade models of what countries should be doing. We are selling recipes to Africa that should be emerging in Africa, responding to African problems.

VDT: At one place in your paper [Social Science and Population Policy], you said that the minister in charge of family planning, rather than meeting a foreign scholar at 9 o'clock, should go out to a family planning clinic to check whether the staff is there and it's clean. I liked that.

DEMENY: That's right. They have a problem and they should tackle that problem. And to the extent that they have an operational problem, their business is to make that program work. I don't think the program would be so much better if the minister spent his time talking to demographers. The demographers should be talking to their counterparts, social scientists in those countries. And the influence should be not from AID to the minister, or social scientists in the U.S. to the minister, but it should be an influence that generates an intellectual ferment that then gets translated in the Third World into appropriate programs.

VDT: Do you think, in other words, that it's time for developed countries to back off from saying what family planning programs should be in LDCs?

DEMENY: Well, in many ways a much more standoffish attitude, which would make it absolutely clear that, "Look, either you have a problem and you have to deal with it or you don't. Well, then you have to bear the consequences." A much more relaxed and standoffish attitude on the part of the West, I think, would be conducive to effective action. It is too complex a problem to put in simple terms, but it is no accident that by far the most effective program dealing with population--not that I applaud every facet of it--came from China and was hatched at a time when there was no AID adviser there. These people realized that they had a problem and tried to find a solution for it.

I apologize for bringing this up in this facile manner, but I do feel that in this field it would be far more effective to put much more emphasis on making sure that intellectual [grant?] in the countries themselves is in much better shape than it is now. AID just doesn't understand this or positively rejects this by saying that would just lead to losing time, fuddy-duddying, and what have you. For instance, they believe it's not worth training Third World scholars in America in population studies, partly because it's too long-term, partly they don't find that cost-effective. I think it's a very misguided and shortsighted policy.

VDT: There are no AID-supported students in the U.S.?

DEMENY: The Council had the most important and influential program in this and AID withdrew its support from that program back in the early 1970s. They do very little in this field. They have some short-term technical training, but . . .

And there is very little institutional growth of research and analysis that is country-based. Well, these countries cooperate with international assistance because it brings them foreign exchange. Some of these governments are simply co-opted; they say, "Well, let's humor them." It should be a much more home-based and autochthonous process than it is now.

VDT: Well, all things in an ideal world. You certainly were a little skeptical of Bangladesh back in 1975, when they were just starting off, presumably full of zeal, with their population program, first five-year plan, and you didn't give them much credit at that time ["Observations on Population Policy and Population Program in Bangladesh," Population and Development Review, December 1975]. Of

course, you've been proved right. They're up to 100 million, is it? Then they were only 75 million. They didn't seem to have home-grown talent to manage their program.

DEMENY: And Africa too. If Africa turns out to be like Pakistan where there were vigorous family planning programs 30 years ago and nothing happened. Well, I would hate to think that the same thing might happen to Africa.

VDT: I visited Zimbabwe two years ago and their family planning program seemed to be going well. There was a Marvellous Mhyloy.

DEMENY: A Population Council fellow.

VDT: Was she? She had been at Penn. I went to see her at the University of Zimbabwe and she is so overwhelmed because she's almost alone, everybody making demands of her.

DEMENY: The one research demographer in the country.

VDT: That's right. Of course, she's needed to teach and build up the department there and she was going to write an article. She'd done some surveys that showed that contraceptive prevalence wasn't nearly so high as shown in the Contraceptive Prevalence Survey. In fact, Ethel Churchill and I had some correspondence with her about it and tried to encourage her to write that article for Studies in Family Planning. It didn't get written. She has no time; she's overwhelmed. So I think you can only go so fast in developing native talent.

DEMENY: But please don't misunderstand me. If one thinks of it as a half a billion dollar industry, nowadays, of the developed countries; the developing countries supposedly put in three times as much, although those are very fishy and elusive figures, it's hard to estimate it properly. I'm not saying that this is something that should be done away with--far from it. There's a very good case to expand these family planning programs.

What I'm bemoaning is the lack of parallel attention--the vastly smaller scale, financially speaking--to make this field a respectable area of inquiry that is not under the thumb of this operational program. They should be in different boxes; they should be separate. There are many institutional models of how this could be done. After all, there is NICHD, which has a certain way of distributing funding for research, and NIH or the National Science Foundation, for primarily domestic purposes. There should be something equivalent to that in the international field.

One way to do this on a sounder basis would be simply for AID to imitate [this] to the extent that they spend money for research--and they claim that they spend tens of millions of dollars for research. We are just applying for a \$16 million operations research contract here and there are many such offerings. To the extent that AID has a research program, they should say, "Okay, 5 percent of our money goes to research," and set up an institutional system that apportions those research funds properly. They now have total discretionary control over it; typically they do this by issuing requests for proposals or going to institutions. More and more, they shun universities totally, as a matter of fact, and just go to profit-making institutions, like Westinghouse, corporations. This is not a sound way to do it. They have a model like NSF or NIH. I don't think it's the best way to do it. I think it would be better to have some institutions of integrity, just as the U.S. government has an institute that studies meteorology in Boulder, Colorado, and has a proper physical plant for it and lots of scientists who look into what the weather is doing and what the weather does to us and what we can do about the weather. Well, they could have a similar institute that studies population, without trembling that next

year they might irritate the head of USAID's population office and they will discontinue their funding. This is not the way to run respectable scientific organizations.

And the peculiar and sad thing is that there is a big research establishment that grew up in demography, partly because population became a big problem. After all, the Population Association has 2500 members, and most of them are scattered, doing their own thing, and they are supremely uninterested in this institutional aspect of the profession. Universities are comfortable institutions; they somehow have lots of tenured posts. Their nontenured posts are filled with people who hope to get tenure by writing scientific papers that nobody understands except people with Ph.D.s in demography. They are certainly not interested in opening windows, approaching the broader public with these issues. So the universities happily draw themselves up from the map in international assistance. Very few of them are now recipients of AID funds. Some of them are proud that they are not recipients.

This is just an inadequate and too brief way of saying that I see the institutional structure of this field, particularly vis-a-vis Third World population issues, as in very, very poor shape. I don't quite see future progress as exceedingly rosy or promising, either.

VDT: You think that some of your colleagues in demography are out for their own thing and the money is not in research unless you are willing to go along with operations research.

Yet, somebody who stuck his neck out and you lit on him right away was Donald Bogue, who had made a number of optimistic world population projections, assuming that family planning is strongly implemented. I was, of course, very much involved in the controversy when you lit into his Population Bulletin of [October] 1978, "Declining World Fertility." Donald Bogue has always been in the thick of the family planning field. I don't think you'd call it operations research that he was doing--population projections.

You're saying that demographers should communicate more widely; you're implying that some are looking today more inward. Yet, here was a demographer that went out for a lot of publicity and you lit into him [On the End of the Population Explosion, Working Paper No. 39, Center for Policy Studies, The Population Council, March 1979].

DEMENY: I certainly regret to have hurt or irritated Don, if I did. But I did feel that that rosy set of projections did damage by putting the public into an excessively rosy frame of mind.

VDT: You wrote: "spreading complacency of opinion about prospective population growth." Of course, a few years later in the early 1980s, the NAS report put us into that frame of mind. Julian Simon too.

DEMENY: Don's projections had to do with demographic developments. He always assumed that rapid population growth is bad for you--correctly, in my opinion. But he declared that the problem was essentially solved. And (a) it was incorrect as far as the prediction was concerned--also inconsistent with some earlier predictions of his--and (b) it came packaged with a very simple-minded explanation of why these developments are taking place, namely, sort of uncritical credit being given to better contraceptives and family planning programs, which I think is a considerable trivialization of the issue.

Bogue is not alone in this. You read Lester Brown and you learn that if only there was twice as much money, or maybe three times as much money--not half a billion dollars but a billion and a half--this problem would be solved. Now, of course, this is ridiculous. Even in this imperfect world, surely if the American taxpayers or the government could be persuaded that for another dime per person you could solve this problem, I believe that money would be found. But it's not true that the population

problem is a problem because there are not enough people going around distributing pills.

So, I was arguing with the things that I think provably and genuinely were wrong and harmful for sound policy-making, and also lulling the public into an excessively rosy frame of mind.

VDT: I think they'll be jolted out of that now. The Population Reference Bureau's World Population Data Sheet this year [1988] got quite a bit of media coverage and the theme of the news release was that the world population growth rate has not declined as rapidly as expected. You know we're trending toward the UN high variant. We always assumed that we'd be at 6.1 billion and no more in 2000; 1998 is the date for the 6 billion, according to the UN 1984 medium variant assessments. But we're not on track with that now. The growth rate has been stuck for three years. China . . .

DEMENY: China is the culprit there. I think 6.1 billion for 2000 is still not too bad a figure. I think the bigger deviation will come beyond the year 2000.

VDT: Right. You've pointed that out in your writings. Well, we could go on and on about that. Let's make a sea change and talk about your connections with PAA, which is how this whole interview project started. You've been a leading figure in PAA for some years. Can you remember the first meeting you attended?

DEMENY: I'm not sure. Was 1959 in Princeton?

VDT: No, in Providence, Rhode Island.

DEMENY: 1958 was in Chicago. I was in Chicago; that was my first meeting. There was a big clash there between Phil Hauser and Bill Petersen.

VDT: Tell me about it; I love clashes for my historical records.

DEMENY: I would not try to reconstruct it, but it was some kind of cause celebre. Now, I didn't go to Providence [in 1959], but from then on I was at every PAA meeting, except Atlanta in 1978, when I was in China. I was in Chicago with my daughter in December last year and we were taking a taxi down to the University of Chicago; I was chair of the nominating committee. I was pointing out to my daughter the hotel where this 1958 meeting was held.

VDT: Tell me about the clash between Phil Hauser and Bill Petersen.

DEMENY: I had been in America for about six months at that point and it would be very unfair if I tried to reconstruct it on the basis of my English knowledge at that time. I really should track it down by asking some more senior people in the field. The ideal person would be Phil Hauser. Will you be talking to him?

VDT: Yes, I hope to interview him, Don Bogue, and Evelyn Kitagawa in Chicago this fall. Okay, I'll ask him [but forgot to, in the Hauser interview of November 12, 1988].

DEMENY: A last comment on Don. I had a great fondness and liking for Don and I certainly didn't mean to be beastly. But let me say this about this field. In this field that grew from such a small size that everybody fitted in the same room, there is a remarkably high degree of reluctance among population scientists, demographers, to criticize each other. Which is not criticizing each other because it sounds like a personal thing, X criticizing Y and Y criticizing Z. But just getting at issues

and dealing with them and disagreement and clashing interpretations of facts.

VDT: They are reluctant to clash?

DEMENY: Yes, it's almost nonexistent. In a field like economics, it's normal that a book comes out and that it's torn apart by a reviewer. Nobody thinks this is personal or that there's a personal animosity there. But in this field, say, Demography, which was a late creation--Don Bogue had much to do with it--never got up the gumption to launch a book review section. So in this field, it is possible to bring books out without any critical attention being given to them.

VDT: I never thought of that. So PDR's is really the first book review section, other than Population Studies and . . .

DEMENY: I was told from a very high source that PDR is known by many people in the profession as an abbreviation for "Periodical Destructive Reviews," because the field is so unaccustomed to clashing and critical examination of issues.

VDT: You had an excellent forum on the NAS study ["Review Symposium: National Research Council, Population Growth and Economic Development: Policy Questions," reviews by Allen C. Kelley, Julian L. Simon, Joseph E. Potter, and Herman E. Daly, Population and Development Review, September 1986].

DEMENY: My comments on the NAS study were immensely resented by some very eminent people. I never would have dreamt that I would have been accused of being personally beastly to them. I thought that I was discussing issues rather than people.

VDT: This is something I've thought with you. I'm married to a Dutchman and I know this is a European tradition.

DEMENY: I think so.

VDT: And not American. Americans take things too personally. You think that's it?

DEMENY: I think there is a problem here, yes. But I think it's worse in population than in most fields. Perhaps because of this clubby nature, that everybody knows everybody else, and people are perhaps too friendly with each other in personal terms and they're reluctant to be critical.

Now, I run the book review section of the PDR. You cannot imagine how difficult it is to squeeze out from well-placed professionals in this country demographers' book reviews. The typical answer is, "I never do book reviews; I make a principle of not doing book reviews."

VDT: Why?

DEMENY: There is no profit in it. It is seen as . . .

VDT: You can't put it on your resume and have it count for something?

DEMENY: Well, you should be able to put it on your resume if you write more than routine book reviews; not if you just summarize the table of contents and say, "This is a great book."

Now you mentioned Wattenberg's book and that he got many very critical reviews. I must say

that it comes easier to demographers--yours wasn't so critical [JvdT review of "The Birth Dearth" in Population Today, July/August, 1987]--because Wattenberg is not a member of the club. He's an outsider and people resented an outsider barging into the holy of holies. They suspect he can't calculate a net reproduction rate.

VDT: He did make a few mistakes which he perhaps wouldn't have made as a demographer, at least in the pre-publication galleys from which I did my review. At least one of these was corrected in the published book.

We have been saying that you tend to be a bit critical and I can read over your writings, which I have done recently, and I realize that you're saying some things that Americans would not say right out in print, or at a conference, such as you did in Bangladesh in 1975 [Bangladesh 1975 PDR article stemmed from remarks first made at a conference in that country]. That seems to be a European approach.

DEMENY: People were even startled with that brief commentary I made after the Mexico population conference. Have you seen that? This is a reprint of my speech in Bucharest, back in 1974.

VDT: I'd like that. I've got all of PDR from the beginning. It's the only journal I kept complete when I did some housecleaning a few years ago.

DEMENY: How kind of you.

VDT: Because every issue is valuable. This is PDR, June 1984 [Archives], "Population on the World Agenda, 1984: A View from Bucharest." This was a speech you made in Bucharest, in 1974?

DEMENY: Yes, not at the conference but at the so-called Bucharest Tribune, which ran parallel to the conference.

VDT: So you like to be provocative.

DEMENY: Well, I was there, and people were appalled by . . .

VDT: What you were saying?

DEMENY: Well, I was arguing for taking population seriously, not diverting attention by talking about other things. I said this is a conference about population and let's keep to the business at hand. Do read it, Jean. Perhaps you'll see that my contrary nature is not a [European] tradition.

VDT: Well, how did you become so much of the club? Because to become president of PAA you have to be a member of the club. Of course, you had the advantage of starting at Princeton.

DEMENY: I was surprised about my election. I don't know how to explain it. Well, I feel myself in rapport; I feel very kindly toward all my colleagues. But I tend to be perhaps . . .

VDT: Prickly?

DEMENY: I don't see it that way, but others do. Yes.

VDT: Can you remember any other outstanding PAA meetings over the years, in addition to the Hauser-Petersen faceoff, which you won't tell me about?

DEMENY: Among the presidential addresses that I've heard--and, as I said, I was present at every single meeting except Atlanta--I would give top mark to Sam Preston's address; it was exceptionally fine ["Children and the Elderly: Divergent Paths for America's Dependents," Minneapolis 1984, published in Demography, November 1984].

VDT: When he said that young people were now being shortchanged; the elderly were now getting all the government money and attention?

DEMENY: Yes. There was a good example of a piece of demographic analysis of very great population policy import. But most products in this field, I would ask the question: Does it really matter; Would this eventually filter down to the general public; Will it move people to arrange their affairs somewhat differently? That was a good example.

VDT: He did pick up rapidly on that. It appeared in Scientific American within months ["Children and the Elderly in the U.S.," Scientific American, December 1984]. And I think it probably has had an impact. I think the U.S. government is looking again at children.

DEMENY: Yes. Such things take time to have an impact, but they have a way of influencing the state of games: When madmen speak, they have an economist looming behind their backs. Ideas have a way to move the world.

VDT: What do you think have been some leadings issues of PAA itself that you remember?

DEMENY: I've noticed that PAA has become so much more quiet. Back in the late 1960s and early 1970s, the Concerned Demographers people, feminists, and all kinds of rebellious groups kept things at the boiling point and made things sometimes quite excitingly eventful. But that has largely disappeared. There used to be a time in PAA when the business meeting was an overflow audience; everybody wanted to go because things were happening there. Now, when I was at the last business meeting in New Orleans, I think there were maybe 30 people there.

VDT: Why was that? It's too bad.

DEMENY: They don't think what is being transacted is very important or worth attending.

VDT: And yet they flock in for the presidential address; everybody seems to make a point of attending that. They don't seem to care for PAA as an organization but they want to hear the person they voted into the presidency; make sure his or her speech is good. It's the one you're usually known for, isn't it?

DEMENY: I was chairman of the nominating committee this year and I note that the professionalization of the field as "demography defined as this membership in this club" bars some people who perhaps should be eminently considered for high posts in the Association, including president.

VDT: You mean people who are demographers . . .

DEMENY: This is a population association so it's really broader than demography. A good example here is Gary Becker. He is somebody the profession should hang on to, or attract in, and give an honor

to, but it doesn't happen. Harvey Leibenstein, now unfortunately in very bad health, such a person should be closer to the profession's bosom than he is now.

VDT: I think there's a certain kind of person who does not want to be particularly involved with the organization.

DEMENY: Well, the organization should go after them.

VDT: There are some people who have not been so active or president. I can think of Parker Mauldin; he's eminent in the field and he's been on the PAA board. And then there was Christopher Tietze, who was here at the Population Council.

DEMENY: Both Chris and Parker ran for president.

VDT: That's right, and they didn't win.

DEMENY: That's a sad fact; there are two candidates and only one can win. So it happens to people. I don't think people run twice for president; I think they are reluctant to do so.

VDT: They feel the decision has been made of them and that's that?

DEMENY: Some people have run more than twice.

VDT: I think of Joe Stycos, who ran against Harriet Presser. That was agonizing, deciding who to vote for.

DEMENY: Similarly Paul Schultz, a very eminent person. I think even his father should have been pursued by the Population Association. Most people in the Association would say that's absurd because they think of population as being population studies.

VDT: And not the Theodore Schultz approach?

DEMENY: In the past, great figures in the profession were recruited from social sciences at large. Malthus wasn't a demographer and John Stuart Mill wasn't a demographer and Walter Willcox wasn't strictly speaking a demographer.

VDT: Well, at that time they didn't have demographers, for that matter, but they certainly taught demographic studies. The discipline has done a lot to define itself as a discipline. Do you regret, for instance, that the PAA still has only 2600 members? [2,952 listed in the revised 1989 Directory of Members]. That's very small compared to the economics association, for instance.

DEMENY: I think that's too large a number.

VDT: What do you want? First you say you want to broaden the club and yet you don't want it to grow too big.

DEMENY: We'd like to broaden the club to bring in people in neighboring disciplines who are very interested in population.

VDT: Like the IUSSP, for instance. At the Florence meeting [1985], the secretary announced that they were going after economists and anthropologists and so on. Of course, that's a much smaller organization; just 1600 or so still in IUSSP.

Would you want to drop out some of the applied demographers?

DEMENY: Anybody who wants to be a PAA member should be welcome. No, I'm not complaining about the size. To begin with, for sheer economic reasons a larger membership is better than a smaller one--for journals, for instance.

VDT: It would get more people to subscribe to PDR, which I understand doesn't have a very large subscription list.

DEMENY: It has a very good subscription. We have a larger paid subscription than, for instance, Economic Development and Cultural Change, a periodical that is [also] not an association journal. Of course, if you are an association journal you have a captive audience because membership means subscription. If you're not an association journal, it's inordinantly difficult to maintain a very large subscription list. We have over 2000 subscribers in North America.

VDT: I think of Economic Development and Cultural Change only as the famous Davis-Blake intermediate variables article, but that continues to exist?

DEMENY: In fact, it's considered the leading development journal in the world. The great institution of the University of Chicago is behind it and I think the University of Chicago Press probably subsidizes it. It's a fine journal.

VDT: What do you think of People, which also tends to be both population and development?

DEMENY: It's a well edited journal and very lively and quick on the ball and with a good network of feeders. I think inevitably it has a point of view that is pre-ordained. They know where their heart is, no question of that. There is a lack of detachment and open-mindedness that I think would not be good for a scientific journal. But it's a magazine so conceived and within those terms of reference it's very, very nicely done.

VDT: Do you ever read anything like American Demographics?

DEMENY: I always go through it and I find it a little repetitive, coming back to the same themes. After all, there are just so many things that business is interested in. I wouldn't have thought that it would be able to sustain itself, but it's done very well.

VDT: Right. Of course, it now has big business behind it--Dow Jones.

What changes have you noted in PAA over the years and have they been for the better? You have already mentioned that in the late 1960s and early 1970s, there was all the excitement of the activist groups that packed the business meetings. At your first meeting in Chicago, that was one of the first that had double sessions and now we have eight simultaneous sessions, and so on. Do you still enjoy PAA meetings?

DEMENY: Yes, I do. It's a very efficient way of touching base with friends and meeting them and I go to the sessions and enjoy them. Obviously, when there are 200 papers you don't expect a very large

proportion of them to be outstanding or highly polished, but there's always a fair amount of interesting papers.

VDT: You must pick up some papers for use in PDR, like Dick Lincoln did for Family Planning Perspectives.

DEMENY: Too few are suitable.

VDT: They're too technical?

DEMENY: They tend to be too rough, half-baked, sort of researcher's progress.

VDT: You do go after great names. For instance, I know Ester Boserup quite well, my husband worked for her and her husband in Geneva, and I was delighted to see an article by her in PDR quite recently.

DEMENY: We've published her before; we published Mogens Boserup when he was still alive. In a way, Ester represents some difference from the core of demography and we're always pleased when we can attract such names.

VDT: And, of course, she was the first to say, like Julian Simon, that population growth stimulates innovation and technology.

DEMENY: She is not the first. I can show you 120 years before her some good expressions of that theme.

VDT: My final questions were things like your plans for the future. You've already said that: You're going to be a senior scholar at the Population Council and you say you have several articles on the go.

DEMENY: One thing on which I'll have to put much time. I'm chairman of the Committee on Population and Policy of IUSSP and that takes a good deal of work.

VDT: Do you have a conference coming up?

DEMENY: Several conferences coming up, the first one jointly with the UN Population Division, later this month here in New York, at the UN.

VDT: One last question. Are you pessimistic or optimistic about future population growth trends?

DEMENY: Much of what we have has already happened, so we are locked in a pattern. I do think that many countries have managed to get a much larger population than they should have done or would have liked. I think stabilization at 10 billion is, in my opinion, at the very low end of the spectrum. That's easy to say because there will be nobody around to say, "You were wrong."

VDT: PDR recently had the article by Ansley Coale that pointed out, "Don't forget the possibility of a nuclear holocaust" ["Nuclear War and Demographers' Projections," PDR, September 1985].

DEMENY: You know why we published that? It was given at a conference at the United Nations and

the United Nations didn't feel they could publish the article.

VDT: You mean it was left out of the proceedings?

VDT: Yes.

VDT: Well, good for PDR!

DEMENY: There was another case with IUSSP. John Aird gave a paper highly critical of the Chinese family planning program, something on which we had published many positive articles. IUSSP said, "We can't publish this or China will be offended." We published it ["Population Studies and Population Policy in China," PDR, June 1982].

VDT: Then PDR certainly has a role. By the way, you were very critical in your book review of Judith Banister's latest book on China [review by Susan Greenhalgh of China's Changing Population, PDR, December 1987].

DEMENY: Rightly so. We were on the other side of the horse there. These are very debatable issues and I think there should be airing for a wide spectrum of points of view. There is no the right thinking about it. I think these are genuinely unresolved and, in a way, nonscientific issues, opinion issues, on which facts can be brought to bear. I don't think we should say, "This is the right thing and we publish him or her but not somebody else on this."

You ask about People, I think they would have a party line on this and they wouldn't confuse their readers by putting in some things. We've published a number of items by Julian Simon, of course, and critical reviews of the World Bank's fine study of economic consequences of population growth by people like Easterlin and Ronald Lee ["Review Symposium: World Development Report 1984," reviews by Richard A. Easterlin, Colin Clark, and Ronald Lee; comments by Kenneth E. Boulding, Garrett Hardin, Harvey Leibenstein, and Jan Tinbergen, PDR, March 1985]. I think it's important to have a forum for this.

VDT: Absolutely, and I hope you go on providing that forum. I think we've talked enough. It's been wonderful; I thank you very much.

DEMENY: I must have bored you to death.

VDT: No, on the contrary.

DEMENY: You'll have these tapes with this Hungarian accent; it may drive you crazy.

VDT: I'm so amazed; as I said, you finish your sentences; many people do not. I just interviewed Norman Ryder, who is another person whom I think speaks and writes eloquently. I asked him what explains that. Of course, I have some national pride in Norman, being Canadian myself, and he made the answer I expected, which was "my fine Canadian education." But he went beyond that: both his parents were English and they expected him to do a lot of reading. In your case, you mentioned your father's library. You read in English at a young age?

DEMENY: Not at the time I mentioned was my first exposure to demography. I learned English reading only when I was in college.

VDT: And French?

DEMENY: I had a French teacher at home.

VDT: Was that because French was the language of the elite?

DEMENY: No, that was German, I'm afraid. When I was a little boy, we had a German fraulein and I spoke a child's German. My German is now in even more terrible shape than my French. Now, English would be totally the dominant language there. Of course, I also had to study Russian and that left me without a trace. But we had to study eight years of Latin. I remember my baccalaureate; I was given a text from Tacitus and I had to study the Latin and say it in Hungarian--sightread the Latin and say it in Hungarian. At that time, I was very good at this.

VDT: It's a bit like the Dutch. After all, who knows Dutch? Who knows Hungarian? They've got to learn all the other languages. You have a tremendous advantage.

DEMENY: It's easier for the Dutch because Dutch is so close to German or English, whereas Hungarian is related only to Finnish.

VDT: And who knows Finnish? Do you understand Finnish?

DEMENY: No, it isn't that close. It's like saying that English and Persian are closely related.

VDT: Have you ever lived for a time in any Third World country? You mentioned China; were you there for an extended period?

DEMENY: No. The only other country I've lived in for an extended period is Hungary. Otherwise, I'm afraid that's a lack in my education. If I add up the amount of time I've spent in India, it comes to a respectable number of months; it surely adds up to six or eight months.

VDT: Perhaps that's where you should go to set up your institute. Perhaps you'll feel that you have to set it up, to get it set up.

DEMENY: Yes, I agree with you, but I'd like to point out that in intellectual influences the [Third Worlders don't do that]. It comes naturally that for your knowledge, you go to Oxford or Cambridge or Princeton or Berkeley. Always in history, there were main seats of learning where the main ideas were cooked and then spread and filtered down. It should be at a strategic location and then it works. If I think of my own former country's history, they were always looking to Heidelberg and the Sorbonne and London and Oxford. Nobody resented that ever, I think, because there's no power attached to those ideas except the power of the idea. But, my God, if I think of a foreign assistance mission coming from Wilhelm in Germany to Hungary in 1880 saying that "You people have a birth rate of 44; why don't you have some of the German doctors to give you contraceptive tips and persuade your cousins to have a lower birth rate?" I think those Germans would have been put on the first train out of the country.

VDT: When you put it like that it indeed sounds impossible.

DEMENY: I think we fall into this pattern very often, in more or less subtle form, but basically this is

what is happening. Or it happened before. I think now America is somewhat more reticent and I think rightly so. But before, it was routine that American ambassadors were expected to cajole the political level, their Third World counterparts, that, "Look, fellows, do this, do that." I think it's not the most effective way of doing things.

Now, some of the contraceptive technology--I'm not famed for thinking that contraceptive technology is the main way to change demographic behavior, but it's a very proper export. Just as people take over Coca Cola, cosmetics, soap, toothbrushes, transistor radios, television sets, and what have you, technological devices, tactics, or what have you have a very natural way of spreading, of responding to demand. The notion that somehow these are inferior goods that Third Worlders couldn't possibly pay for, why can't they pay for it, if it is good for them? It [contraceptive technology] should be more attractive to them, under the right circumstances, than buying soft drinks or cosmetics or items of feminine hygiene or transistor radios. These things are all over the place without any foreign aid program or anybody pushing them or operations researchers coming out to find out what your unmet needs are in soft drinks. And they [contraceptives] would be be [all over the place], if the right policies were followed and there was a genuine demand on the part of the countries that need these devices.

VDT: You sound suspiciously like the American delegation to the Mexico conference touting a free market.

DEMENY: Well, it's worked elsewhere.

VDT: This is an unending topic and I think this is a good place to stop. [But interview resumes.]

Here we are back again. We're talking about Irene Taeuber. You knew her when she would come up to Princeton?

DEMENY: Why not add her to that list of influentials. I mentioned in a different context the very strong influence that Jacob Viner of Princeton had on me. Irene Taeuber was somebody to look up to at Princeton. She was a very frequent presence at 5 Ivy Lane, the marvelous old OPR building. Irene Taeuber was doing her voluminous writings and Population Index. She was a dear, dear person.

VDT: The students would all have a chance to talk to her when she came?

DEMENY: Oh, yes, and more. Once you engaged Irene in a conversation, or once she buttonholed you, it was a never-ending conversation. Irene was a fantastic conversationalist; she would just go on and on. She had this slightly [confrontational?] view of the world. She thought, properly, that population is a terribly important field. But if you talked to Irene, you always had the impression that one third of the KGB and one half of CIA's industry was population problems and population issues in the world. It was always great fun to talk to Irene. Did you know her?

VDT: Well, I had one memorable contact. I was to give my one and only paper at a professional meeting, on illegitimacy ratios in D.C., which are high because there are so many blacks. This was at an annual meeting of the D.C. Sociological Society, on a beautiful Saturday at Howard University. Irene Taeuber was the winner of the Stuart Rice Award that year, a year or two before her death [in 1974]. Everyone stayed to hear her deliver her Stuart Rice Award address and then everyone left en masse, because it was much too pretty an afternoon. So nobody to hear my paper, except Irene Taeuber, Con Taeuber, Paul Glick, and Murray Gendell all stayed; well, Murray was my coauthor. Needless to say, I did not deliver my paper, but I have a very soft spot in my heart for the Taeubers

after that. That was my sole contact with her.

DEMENY: One other great loss to demography was the early passing of John Durand, who was a very dear person and I had many dealings with him in his capacity as head of the Population Division at the UN and also I succeeded in luring him out to Hawaii to the Population Institute. In fact, when he stayed there, not only did he work in my institute but also he was a nextdoor neighbor. Our neighbor's house was up for rent, so we were close neighbors with the Durands. He had some sabbatical time off; he was doing his historical demography paper there. It was a history of world population growth. Later an offshoot of that work was published in Population and Development Review, back in 1977 ["Historical Estimates of World Population: An Evaluation," PDR, September 1977].

VDT: John Durand was retired at the time of his death, wasn't he, living in North Carolina?

DEMENY: Yes.

VDT: We have an interview with him. Abbott Ferriss had an interview with him for the PAA oral history project in 1979.

DEMENY: Another person whom I lured to Hawaii was Nathan Keyfitz, a very dear friend and a marvelous person.

VDT: Fellow Canadian. When he's not in Vienna, he's in Indonesia. He's having a conference in Vienna in the fall.

DEMENY: It's in Hungary.

VDT: Oh, it is? Are you going?

DEMENY: No, I have another engagement. We published his last Indonesian article, in which he looks at Indonesia 30 years ago, when he was first there, and then recently revisited that village ["An East Javanese Village in 1953 and 1985: Observations on Development," PDR, December 1985].

VDT: Yes, that was interesting, what happened in that one particular village.

DEMENY: A very positive assessment of development and family planning.

VDT: Yes, they finally had turned around and accepted it, generally. He seems to be a very far-ranging person.

DEMENY: Yes, we need many more Nathans.

VDT: I think of him as a mathematical demographer, but that's just one facet of his work.

DEMENY: That's right; a very lively mind.

VDT: If I don't get to interview him, I'll have to come back and interview you about him. [See Keyfitz "self-interview" of December 31, 1988, taped in Jakarta, above.]

[Interview continues.] Here's another point. Your wife did the illustration, map, for . . .

DEMENY: In OPR there was a tradition that this illustration was done in-house, so to speak, and Daphne Notestein, that marvelous lady who still drives at age 91 . . .

VDT: No, she has just given up her driver's licence, Ansley Coale told me. She could still drive, but if anything happened they would blame it on her age and she didn't want that.

DEMENY: Daphne Notestein was doing covers for Population Index and some maps and graphs for Princeton books. My wife inherited that post when the Notesteins were in New York and no longer associated with OPR. From 1962 to probably 1965 or 66, my wife did Population Index covers and all the maps and graphs for that book on Africa, The Demography of Tropical Africa.

VDT: She wasn't the only wife who worked on that; who was the other one?

DEMENY: Yes, I said there was this tradition and it started with Daphne Notestein and there was also Charlie Westoff's first wife, Joan Westoff.

VDT: Is your wife an artist?

DEMENY: She's a painter, yes, not practicing very actively these days, but she did this artwork for Princeton books. She enjoyed it.

Footnote on Demeny remark after the interview.

Paul said that he, like many demographers it seems, had been a musician. He played the bassoon as a young man, became quite professional, and was offered a job [with the orchestra?] in his hometown. He had taken up the bassoon and pursued it rigorously, because there was a possibility that he might not be accepted into the university in Budapest in the troubled times that Hungary was going through at the time he was ready for university. But he was admitted to the university and he did not carry on with the bassoon.

RONALD D. LEE

PAA President in 1987 (No. 50). Interview with Jean van der Tak at the Graduate Group in Demography, University of California at Berkeley, April 28, 1989.

CAREER HIGHLIGHTS: Ronald Lee was born and grew up in Poughkeepsie, New York. He received the B.A. in philosophy from Reed College in 1963, the M.A. in demography from the University of California at Berkeley in 1967, and the Ph.D. in economics from Harvard in 1971, interspersed by two years (1963-65) with the Peace Corps in Ethiopia. He spent 1971-72 at INED (L'Institut National d'Etudes Demographiques) in Paris and was Professor of Economics with the Population Studies Center at the University of Michigan from 1972 to 1979. Since 1979, he has been Professor of Demography and Economics with the Graduate Group in Demography at Berkeley.

He is well known in the demographic world for his research and writing on mathematical models of fertility and population growth, particularly applied to U.S. trends and historical trends in England and Europe. He won the PAA Mindel Sheps Award for Mathematical Demography in 1984. He has also done much work related to less developed countries. He chaired the working group on fertility determinants of the Committee on Population and Demography of the National Academy of Sciences and was co-editor, with Rudolfo Bulatao, of that group's report, Determinants of Fertility in Developing Countries (1983). He was co-chair of the NAS working group which produced the famous report, Population Growth and Economic Development: Policy Questions (1986), of which he was co-author with Samuel Preston and Geoffrey Greene. He chaired the IUSSP research committee which produced the monographs, of which he is a co-author, Economics of Changing Age Distributions in Developed Countries (1988) and Population, Food, and Rural Development (1988). He has also served on the grants review committee of the National Institutes of Health.

VDT: How and when did you become interested in demography?

LEE: Well, I should go back to my undergraduate career at Reed College. I started out as a major in physics and then switched in my sophomore year to a major in mathematics and then switched in my junior year to a major in philosophy. By my senior year, I was getting interested in social problems and actually had applied to and been accepted in a graduate city planning program. But I decided to go into the Peace Corps instead and spent two years in Ethiopia, teaching school from seventh grade through seniors in high school, in math and physics.

VDT: This was in the mid-1960s?

LEE: This was from 1963 to 1965, so it was in the years of the Kennedy euphoria--what you could do for your country and so on. The Peace Corps had just begun. It was also the time that the Vietnam War was heating up and more and more people were being drafted. The Peace Corps didn't provide any exemption from the draft afterwards. In 1965 it didn't even merit you a deferral, as people were being drafted out of the Peace Corps. I was very concerned about being drafted and didn't like the war or any part of it. That was the background when I was thinking what I was going to do after the Peace Corps.

I thought about continuing to teach school, which was a deferrable occupation at that time. But I really wanted to go to graduate school; however, I didn't know what I wanted to go to graduate school in. What I wanted to do was continue my very diffuse undergraduate school career because it was

interesting; it was fun taking courses in a lot of different things. But no graduate school that I knew of had a "graduate-student-at-large" program--you had to be in something. I did know that I wanted to go to Berkeley; I liked Berkeley.

VDT: Why did you know that?

LEE: Well, at Reed College people thought of Berkeley as a wonderful place to be. And I had been down here. I'd taken summer school courses in economics and I had a couple of sisters living in the Bay area and I'd come down and visited them, so I knew a bit about what it was like here.

In any event, a brother-in-law in Berkeley mailed me a letter to the little town in Ethiopia where I was, mentioning that there was a new program in demography and it was very interdisciplinary; you took courses in many different fields--social science, math, biology, and so on. I had no idea really what demography was, what the word meant; I had to go look it up in the dictionary. Having looked it up I thought, well, all right, why not? It didn't sound fascinating to me, but it sounded as interesting as anything else did, but I wasn't particularly interested in any subject.

So I applied to that program. And eventually I learned in Calcutta, on my way home and my mail caught up with me, that I'd been accepted. So I came to Berkeley. I think it was the first year the Group in Demography here was really under way. So I was in the first cohort.

VDT: In 1965?

LEE: It started in 1965, yes. I studied demography and it was indeed a very interdisciplinary program. I took courses in economics and biology and statistics and math and sociology and public health. It was a very demanding two years of courses for the master's degree. Then having gotten my master's, I decided I was tired of graduate school and left.

VDT: You didn't have a thesis, just coursework?

LEE: Just coursework and then an oral exam. I think you got to choose whether you wanted to write a thesis or do an oral exam.

My teachers here were Judith Blake, who was running the program, and Kingsley Davis, who was in the sociology department, and Nathan Keyfitz, who came as a visitor a couple of times and was very influential in the development of my career. Peter Mazur, who is now in a college in the state of Washington, Thomas Burch, Karol Krotki--these people were visitors. Eduardo Arriaga was around in a semi-student, semi-faculty status.

VDT: What a battery of famous names!

LEE: Yes. And William Petersen and Carlo Cipolla, who does Italian demographic history. There were people like this. It was an exciting place to be.

I remember the Friday afternoon seminars, extremely lively, and I think often uncomfortable for the speakers. Calvin Goldscheider was another name, also teaching.

VDT: Those seminars were by the professors on anything?

LEE: Sometimes a professor, sometimes a visiting speaker. I remember Ronald Freedman coming through, for example.

These were the days when there was a deep split between people with a public health, family

planning orientation toward population problems in the Third World--people who believed that making contraceptives available would lead just in itself to fertility decline--very much opposed to people with, say, a more sociological structural view of the problem, and certainly Judith Blake and Kingsley Davis were in this latter group. So when someone like Ronald Freedman came through, I think we students expected someone far less well informed from the sociological point of view than in fact we encountered. Ronald Freedman clearly understood that fertility was a matter of both institutions and norms and of access to contraceptives. But that was also a lively debate, a controversy, within the Group in Demography and then when Freedman came.

VDT: That was about the time when Kingsley was preparing his famous article, "Population Policy: Will Current Programs Succeed?" published in 1967 [Science, November 10, 1967], when he became skeptical of family planning programs because they were pushing the number of children that people desired and that was too high.

LEE: Yes, so that was all very much in the air. I don't remember hearing Kingsley talking about that article specifically. I do remember a lot of discussion of Kingsley's also very influential article on the demographic transition and multiphasic response.

VDT: That was his 1963 PAA presidential address, "The Theory of Change and Response in Modern Demographic History" [Population Index, October 1963].

LEE: I should mention also my fellow students at that time, many of whom are active now. Ruth Dixon and Harriet Presser, our current [1989] PAA president, were graduate students at the same time.

VDT: Also in that first cohort of the Group in Demography?

LEE: Ruth and Harriet were doing PhDs in the sociology department, but they were taking demography courses so I encountered them in classes. George Simmons was doing an economics Ph.D., but I knew him also through courses we took together, in particular one that Karol Krotki taught in population and economic development. The influential Coale and Hoover book [Population Growth and Economic Development in Low-Income Countries, 1958] had come out fairly recently and I remember that much of the discussion in that class was about that book. Cam Gibson, who for many years constructed projections at the Census Bureau, was in the class, and Roger Avery, very active in research at Cornell and now at Brown, and Peter Uhlenberg, now at North Carolina.

VDT: A tremendous battery of those who went on in the field. Did you realize you were sort of the beginning of a new era?

LEE: Not at all. There was also George Masnick, who was at Harvard Population [Sciences] for quite a number of years. Barbara Heyns, who's now at NYU, was an undergraduate but a research assistant for Judith Blake and also involved in the program. Shirley Hartley, who is now at Cal State, Hayward, Robert Retherford, who's at the East-West Center. There were probably others whom I'm not remembering, but there were a lot of people who are quite well known. And it was a small program, so that's a high proportion who were around in the two years I was there.

VDT: You were there because your brother-in-law sent you something. Do you think these others were drawn by Judith Blake and Kingsley Davis?

LEE: Most likely, yes. And I think most of them knew much better than I what demography was,

what it meant, what they were doing. They perhaps had some background in it before. Harriet Presser, in her presidential address [PAA meeting, Baltimore, April 1989], mentioned that she'd been at North Carolina. Maybe she was at Berkeley to do her dissertation, I'm not sure. [Did M.A. at North Carolina under Daniel Price; came to Berkeley specifically to do demography-focused Ph.D. in sociology.] Many of these would have been people who didn't come to Berkeley to do demography, but got interested in demography through coursework there and then continued it. Many were in economics and sociology, rather than demography per se.

VDT: I wanted to ask you later, when we come to your current time here at Berkeley, whether you think it's important to have a separate department of demography, which the Group in Demography went on to become for five years [1967-72] and then ended.

LEE: It was just becoming a department as I left. It was the Group in Demography for two years [1965-67] and became a department thereafter. It had some sort of existence before I came, but I think it did not actually admit its own students in demography and probably didn't teach courses in its own listing. I think that sort of change happened at the time I came in 1965. But you'll be able to confirm that with Judith Blake, who'll know better than anyone [see Judith Blake interview, above].

VDT: Why did you leave Berkeley if they were just setting up a new department of demography? Or why did you go to Harvard? Was it the push or the pull, or what?

LEE: It was a difficult time to be trying to do something serious, the mid- or late 1960s, at Berkeley--perhaps all over the country. This was the time when you didn't trust anyone over 30 and the prevailing morality was that responsibility within the system and any sort of routinized activity was equivalent to brain death and a sort of moral decay.

VDT: Already in 1967 that's the way students felt?

LEE: Absolutely, yes. It was very much the cult of young people; aging was the process of encrusting with responsibility.

VDT: You mean that's the way they viewed the professors--or anyone over 30?

LEE: Yeah, all of that. And the process of life itself. That is, the noble fight was to prevent the growth of these barnacles of, you know, professional life or anything of that sort occurring all over your mind and spirit.

VDT: So you should just come get a quick degree and out . . .

LEE: I sat in Golden Gate Park and listened to Timothy Leary say: "Tune in; turn on; drop out." And I dropped out.

VDT: Having first got your degree [M.A.].

LEE: That's true. But it was really in that spirit of sort of striking out into the world and looking for adventure. I thought I wanted to be a writer, perhaps. I wanted to see what else life had to offer besides this narrow routine of student life, academic life. So I didn't leave with anything else set up to do, really. I had no notion at that time of going to Harvard. I just left and traveled a bit. I went to

Mexico; I traveled a bit in the States. I tried my hand at writing for a while. I decided that I was not going to be a writer.

Then I had to decide what I was going to do and I still wasn't sure at all, but eventually I ended up in Cambridge, sometime late in the fall of 1967, five or six months after I'd gotten my master's. I thought about different things I might do--law school, medical school. Eventually I decided I'd look into graduate school in economics and I managed to talk Harvard into letting me enroll in mid-year in the economics program. So I started working on a Ph.D. in economics in January 1968.

VDT: Did you have a fellowship already?

LEE: I didn't have any fellowship; I had some savings. I had had a National Institutes of Health traineeship at the Group in Demography, the same source of funds that many domestic students studying with us get these days. But at Harvard I was living on savings that first semester and then I got fellowships.

VDT: You still weren't gripped by demography?

LEE: No. I felt I'd had enough of demography.

VDT: You went purely to the department of economics at Harvard?

LEE: Yes. I had no intention of ever getting involved in demography again. But as I started taking my economics courses and had term papers to write, I found myself being led back into demographic questions. But now they were economic demographic questions: How could I relate what I was learning in economics to what I had learned in demography? I was interested in that both at the formal level of mathematical modeling and also just trying to understand the substance of what I was studying. So from the very beginning, I started writing term papers on economic demography. Some of my first term papers were on economic history, which was a requirement in the Harvard program; I think it still is. I wrote papers on the role of population in English economic history and that work eventually led into my thesis.

VDT: Your thesis was "Econometric Studies of Topics in Demographic History." It all came together.

LEE: That's right. In other courses, I had econometric papers to write and I was trying to integrate the period and cohort mathematical models in demography or the interactions of age distribution changes in fertility and economic well-being in the United States, which I then discovered had been pretty thoroughly explored by Richard Easterlin. These are topics I began to work on as term papers and then developed into my dissertation and then into a lot of research afterward.

One interesting thing at that point was in working on these historical economic-demographic questions in England, I came across Tony Wrigley's now classic study of one parish there, Colyton. It was a reconstitution study, but he also showed the numbers of births, baptisms, deaths, and marriages over about three centuries in Colyton, from 1538 to the 1840s, something like that. I looked at the reconstitution results on changes in life expectancy and marital fertility and age at marriage and I looked at the numbers of baptisms and burials and I just didn't see how they could be consistent with one another; how the aggregate trends could be consistent with these estimates of vital rates. So I tried to formalize my dissatisfaction with what I saw and did it by working out a method I called inverse projection, which made it possible to take the birth, death, baptism, and burial series as an input and

then estimate what total fertility rates and life expectancy and population age distributions must have generated that series of baptisms and births. I wrote a program to do this, having learned programming in a course with Keyfitz at Berkeley that was ostensibly on sociological methods and was actually a course on FORTRAN programming. I ran the baptisms and burials through this and out came exactly the vital rates that Tony Wrigley had found by reconstitution.

So I was disappointed, but it turned out that that was the development of what has subsequently turned out to be a useful method in historical demography. Indeed, that was the method that Wrigley and Schofield used in their important book on the reconstruction of population history in England [E.A. Wrigley and R.S. Schofield, eds., The Population History of England 1541-1871, 1981].

VDT How did you convey what you'd done to them? You published?

LEE: Yes, but before I published I did what I now advise all my graduate students to do. As soon as I finished my dissertation, I had ten xerox copies made and sent them to all the people I thought would be potentially most interested.

VDT: You had the inverse projection method in your dissertation?

LEE: Yes, that was part of the dissertation. So I mailed one of these to Tony Wrigley and another to Ansley Coale, I'm sure. I don't remember where I sent the others, but some of them landed on fertile ground. Chuck [Charles] Tilly organized a three-week working group at the Princeton Institute for Advanced Studies the summer of 1972 and I think because I had sent this dissertation to a number of people, including Wrigley, I was invited to that group of about eight people and I met Wrigley and Schofield. That was the beginning of my subsequent collaboration with them.

VDT: Which ended up in several books, or at least there was Population Patterns in the Past [1977]--that was later.

LEE: That's right.

VDT: So that's how you got into the group of those who have looked at the past. You summed that up in a sense in your PAA presidential address in 1987, "Population Dynamics of Humans and Other Animals" [Demography, November 1987].

LEE: That's right.

VDT: Your interest in demographic history began at the time you were working on your dissertation. You started out with that economic history course, which fed into the demography that you did have.

LEE: Yes, but I was never interested in the history per se. I was interested in trying to find general laws, general principles, governing the movement of population over time, growth and decline and so on--a sort of Malthusian approach. In order to do that you need a long series of data and if you can't get data for the future, you're obliged to go back into the past. And to look at Malthusian kinds of principles, you pretty much have to look before the Industrial Revolution, so that puts it still farther back. But my first interest was in the demography--demography in the ecological sense of why was the population of the world whatever it was; why was it this in England; why was it bigger than it had been before; why did it decline in the 17th century. That sort of question. Then I just had to use what data were available, which turned out to be historical data.

I think that's somewhat the spirit in which demographers have generally tackled historical demography. There are few of us who are real historians, who really know something of the context of history. Most of us are demographers and were fascinated by demographic data and some of those data are historical data.

VDT: That must have led you into the Easterlin hypothesis.

LEE: Yes, indeed.

VDT: You said you found that he was the one working on age structure and the long swings in fertility.

LEE: Yes. It seems to me it's all part of the same idea, some sort of feedback or equilibrating process in that a population gets too large, encounters economic hardships, which lead to reduction in population growth rates, perhaps population decline, until some sort of equilibrium is established. That is the basic Malthusian idea, which did not have age distribution well incorporated, but as soon as you put age distribution into a model of that sort, then the possibility arises that current fertility is responding to current labor market conditions. By the time the current crop of babies comes of age, labor market conditions may have completely changed. So what may have looked like a good idea at the time the kids were born turns out to be a bad idea in the sense that conditions are much worse--or much better--than anticipated when they're grown up than the labor market was when they were kids. So you can have a kind of disequilibrating response, perhaps falling too strongly in the sense that fertility is cut back too much, so that you end up with a cohort that is in some sense too small 20 or 30 years, one generation, later. Then, responding to the fact that it's small and its economic conditions are therefore good, it has fertility that is in some sense too high and this leads to every-other-generation cycling.

VDT: You wrote an article for the symposium on the Easterlin hypothesis at the 1976 PAA meeting which was published later that year in Population and Development Review ["Demographic Forecasting and the Easterlin Hypothesis," PDR, September/December 1976]. You presented a forecasting method based on the Easterlin hypothesis and, according to that, you suggested that the current U.S. fertility--which was down to 1.7 in 1976, I believe, the lowest it ever got--was only temporarily low and there should be an upturn in "five or ten years."

LEE: Did I say five or ten years?

VDT: Yes, you did [p. 467 of that issue of PDR].

LEE: In what year?

VDT: 1976.

LEE: My recollection is mid- to late 1980s.

VDT: No, that's not what you said then. Easterlin, after all, talked about a fertility upturn by 1984 in his PAA presidential address of 1978 ["What Will 1984 Be Like?"]. You did say "five or ten years" in 1976.

And that hasn't really happened, although the current [1989] total fertility rate rounds off to 1.9;

it's crept up. And U.S. births in 1988 totaled almost 3.9 million, the highest annual number since 1964, largely due to rising births among women in their thirties. It's not the baby busters. It's the baby boomers who are having delayed children and it isn't the baby busters who are finding jobs and able to afford more children.

LEE: I would say there's no evidence now [1989] of the sort of upswing I was looking at, despite these little jiggles. It really isn't too late for something like that; it's the timing. I thought what I said then was this would be in the late 1980s. What is it?--we're almost out of the late 1980s.

VDT: You had the TFR shooting up in the 1990s. But, okay.

LEE: We will see. On the face of it, it just looks like it hasn't happened. At this stage in the 1980s, if the argument is right, the upturn should be really quite mild. But, as you say, in the 1990s, it should be very visible.

But I would say that on the whole, it's not looking good for the Easterlin hypothesis in the last few years. On the other hand, almost every other theory I know of would have predicted continued fairly rapid fertility decline through the 1980s.

VDT: You mean as Westoff thinks, after the social changes?

LEE: Yes, the social changes have continued, divorce going up and so on.

VDT: You mean below 1.7?

LEE: Oh, yes. Below 1.7 isn't so extraordinary. We've got quite a number of European countries below 1.7.

VDT: Except that, did you know that Sweden is almost going to round off at 2.0? I brought you the proofs of the Population Reference Bureau 1989 World Population Data Sheet. I have to admit that when I was in there the last day, Carl Haub said: "Scratch that 2.0; I'm going to put it down to 1.9." But nearly 2.0!--which is something for Sweden. Italy is the lowest now, somewhat delayed, at 1.3. And West Germany rounds off to 1.4.

LEE: In any event, it seems to me that most, if not all, of the theories are predicting continued decline until we don't know what, because most of these theories don't really have any natural stopping point until you get to zero. There's nothing particularly natural about 2.1 or 2.5 as the end point of the social and economic changes that make up the driving force of the demographic transition. So the fact that fertility has just stopped declining and pretty much been flat for the last 10 or 15 years is, I think, more consistent with the Easterlin theory than it is with most of the others. That is, Easterlin theory would have predicted this leveling off even back in the early 1960s when Easterlin was writing. I'm not sure if he did predict a leveling-off in the mid-1970s, 1980s.

VDT: There were going to be the Kuznets swings, which are much longer--40 years.

LEE: Oh, yes. I mean he predicted the baby boom, but it's also implicit in that approach and in the diagrams he drew that you would expect a leveling-off in the 1970s and early 1980s, preceding this upturn that's expected in the late 1980s. So from that point of view, I think it hasn't done so badly, although this prediction of an upturn I would say should have happened last year, the year before--

there isn't real evidence that that has happened.

VDT: Just delayed [prophetic! U.S. TFR in 1990 was 2.0].

Let's go back and talk about Harvard. Who did you do your dissertation with?

LEE: Formally, Harvey Leibenstein was chair of my dissertation committee, but in practice he had very little influence on it. He was out of the country most of the time I was working. The person who had most influence on it was not a demographer at all but an econometrician, Dale Jordanson. He'd been a professor of economics at Berkeley when I was here and I had taken a course in econometrics from him. Then I went to Harvard as a graduate student and a year later he came as a professor and I asked if he'd be on my dissertation committee and he said he'd be happy to. He had lots of time then, I think, wasn't surrounded with graduate students since he'd just gotten there. He said to me, "The way I supervise dissertations is we set up a time each week, half hour, an hour, and you come in and present to me in a little seminar what you've accomplished in the past week."

So that really got me hopping, kept me hopping. Fortunately, I'd done a lot of the work already in these terms papers and it was more a matter of putting a polish on that work, deepening it mathematically and working out the econometrics and so on. It was very helpful, this arrangement I had with Jordanson, and I learned a lot from him. So I think of him as my main adviser, main influence, when I was working on demography at Harvard.

Basically, it was very isolated. I don't think I knew another person who was interested in population. I can't think of a single graduate student; there really wasn't any community of graduate students.

VDT: Of course, Harvard didn't have a population center. It was once offered the Office of Population Research and turned it down. Fred Osborn went there first when he was setting it up.

VDT: Yes. The Harvard population program was really sort of a mess when I was there. And it's had a lot of problems since then; had a history of problems. A lot of brilliant people, but who aren't really interested in population.

VDT: Nathan Keyfitz came a couple of years later [in 1972].

LEE: Keyfitz is the great exception. He wasn't there when I was there.

VDT: I want to know more about your interest in mathematical demography. You mentioned that at one point in your undergraduate years you were majoring in math. But the math has always just been there, as a sort of native talent or interest?

LEE: Yes, a lot of it is native talent. I come from a large family of people for whom math seems to be a very natural mode of expression.

VDT: Your sisters included?

LEE: My sisters, my uncles, my aunts, parents. I have a sister who majored in math and a number of female cousins who majored in math; uncles who were math teachers. Math and philosophy seem to be a little family trade.

VDT: Isn't there a connection, two parts of the brain that work together or something?

LEE: Yes. Anyway, I always enjoyed math, enjoyed the puzzle-solving aspect of problem-solving. I always tried to be taking a math course of some sort or a statistics course through my college years, and when I was in Ethiopia in the Peace Corps, I took along math books and worked along in math on my own, evenings. When I was at graduate school in demography, I took a graduate math course in topology, of all things. So I sort of kept my hand in. It isn't that I have any deep knowledge of math, but I feel comfortable in it, have a pretty good mathematical intuition. So it's natural for me to formulate problems in mathematical terms.

VDT: Great. Now what about your year at INED? That was right after you got your Ph.D.

LEE: Yes. Well, I got married after I'd been at graduate school for about six or eight months, in 1968.

VDT: Did you meet your wife at Harvard?

LEE: Yes. She was a graduate student in Slavic languages and literatures, doing Serbo-Croatian and so forth, but she also had a strong interest in France; fluent in French, and had always dreamed of spending a year in Paris. And it sounded pretty nice to me, although I didn't know any French at all. So we started planning on spending a year in Paris after I finished my degree and while she was writing her dissertation. I applied for a Social Science Research Council postdoctoral fellowship. Nathan Keyfitz was somehow instrumental. He had a friend who worked for the SSRC and rather than having a sort of organized open system of applications for these, they seemed to work through contacts and one of the contacts was Keyfitz. He suggested that I apply and recommended me to the selection committee. At the same time, he recommended to Robert Retherford that he apply and he also got a postdoctoral fellowship of the SSRC and also applied to go to INED. So we both ended up spending a year at INED.

I busily tried to learn French that year before we went. I audited a class in intensive French for a semester and spent hours in a language lab listening to tapes and responding to questions in French, with the help of my wife. When we got to Paris, I hired four different French tutors. I'd spend an hour with one and then I'd go to the next for an hour. I did that for four hours a day for two weeks, just doing conversation.

VDT: Did you think you'd wear out one person if you went for four hours?

LEE: It seemed too strange to any of them to do more than an hour. After two weeks of that, I felt like I dared show my face at INED, so I did. I was officially under Louis Henry's jurisdiction.

VDT: Who must already have been aware of you. Did you send him a copy of your dissertation?

LEE: I believe I did, but I don't know whether he read it or not. I think he was unhappy to have me there. I had applied to Bourgeois-Pichat, the director, and I think he sort of imposed me on Louis Henry. Not that Henry had anything against me, but I think he didn't like the procedure or felt that it should have been his slot to award.

I had most of my contact not with Louis Henry but with people like Georges Tapinos, Herve Le Bras, and Henry Leridon, all of whom now are well-established senior researchers at INED, but who then were--like I was--just sort of starting out and trying to make their way.

VDT: This must explain your interest in and connection with IUSSP, because these are all in IUSSP,

and Tapinos, of course, is the current secretary-treasurer.

LEE: That's right. In fact, I've maintained fairly close contact with all three of those since then, particularly with Tapinos. We go back to France every once in a while and we see them at international meetings.

VDT: Did you set up your own research program in that year?

LEE: Yes. I really worked on extensions, polishing up, of material in my dissertation, primarily.

VDT: With more data that you could get at INED?

LEE: Yes, with more data. But mostly just with time to reflect and a very primitive computing system and an obscure new computing language to learn--PAFF, it was called, a paper tape system, not IBM cards. You punched holes in paper tape and that recorded your program and you ran it through. Eventually, I decided that computer was just too primitive and someone at INED arranged for me to have access to a computer at a large medical research facility outside Paris and in exchange I did a series of lectures on spectral analysis to the research staff there and it worked out well.

One new project I worked on there was the research on forecasting post-transitional population, using statistical times series methods. I didn't submit an article on that until a couple of years later. That turned out to be quite an important article and that whole approach has become quite important. In fact, now the U.S. Census Bureau is using essentially that kind of method to do their fertility projections. That was a 1974 article in the Journal of the American Statistical Association called "Stochastic Renewal of Serially Correlated Fertility: Forecasting Births in Post-transition Populations."

VDT: I know of that one; I can't say I've read it. It was a fruitful year?

LEE: It was a very fruitful year, although much of the fruit didn't ripen for a couple of years.

After INED I went to Michigan. I'd been hired there before I left Harvard, but asked for a leave of absence in my first year so I could go to Paris. I got to Michigan because a Michigan professor of economic history, Alex Eckstein, was spending a year at Harvard and he was aware of the fact that the Michigan economics department was looking for an economic demographer, because Paul Demeny, who had been there, had left for Hawaii to develop the East-West Population Institute. I believe Michigan had just gotten a training grant in economic demography from NIH, but they had no economic demographer. Of course, Eva Mueller and Deborah Freedman, who are indeed economic demographers, were there, but I think they didn't think of themselves as primarily economic demographers. Eckstein and I were attending an economic history seminar, quite a famous seminar run by Alexander Gerschenkron. At some point, Eckstein asked me to lunch and asked whether I might be interested in this position at Michigan and it sounded fine to me. That led to a job interview at Michigan and I got the job.

So I ended up at Michigan with my wife. She decided she really didn't want to be a Serbo-Croatian folklorist and went to law school instead at the University of Michigan and we spent the next seven years there.

VDT: Is your wife a practicing lawyer?

LEE: No, she teaches law at Hastings College.

VDT: I had a question here: Did Michigan arouse your interest in less developed countries? But that had come some time before.

LEE: Well, yes.

VDT: I assume you weren't involved in the Taiwan project.

LEE: No, but you couldn't be there without having a lot of that rub off on you and the other . . . I guess that was the major international research project. David Goldberg was working on Turkey and Mexico, and I'm sure other people were working on other countries. Not as much as now; there wasn't such a wide volume of international work. I suppose the World Fertility Survey changed that. The work was more narrowly focused on countries that happened to have data.

VDT: Let's skip to your return to Berkeley. Nathan Keyfitz, in the interview he taped for this series in Jakarta, said the Graduate Group in Demography is the spiritual successor to the Department of Demography and that you're doing very good work here in economic demography and anthropology, under Eugene Hammel. Is it really necessary to have a separate department of demography or does the Graduate Group serve the purpose sufficiently?

LEE: That's not quite the right question. I'd say that makes very little difference, between a department and a graduate group. You probably don't realize that the Graduate Group, which is what we are still, grants degrees. We admit students in demography per se and we award master's degrees and PhDs in demography per se. That's the real question: Should there be degree programs in demography or should it be done the way it is at Michigan and Wisconsin and most places, where you get a Ph.D. in sociology or economics, but you do a field in demography?

VDT: Is this graduate group the only one that gives these degrees in demography? Although my master's at Georgetown [1970] was in demography; the later ones were in sociology.

LEE: I thought Georgetown still gives degrees in demography.

VDT: Well, now they have a department of demography.

LEE: But they don't give PhDs. The Department of Population Sciences at Harvard gives PhDs [in population sciences]. There are very few who get PhDs, but Noreen Goldman came out of that program. It's a much more public health service type of program.

VDT: What question should I have asked?

LEE: What I said: Whether there should be degree-granting programs in demography, or whether this other model is better. I have mixed feelings. When I came here, I felt fairly strongly that it was not such a good idea to have degrees specifically in demography. So I had strong reservations about coming here, when that was the model. But now I'm not so sure. It's really a question of whether there is enough substance and theory to demography for it to be a field of its own, and I'm not sure one way or the other. I must say that teaching it now and seeing the development of our students, I feel more and more that it is sufficiently rich as a discipline to merit being a department or a degree-granting program--a field in its own right. But I'm still not quite sure. The issue is: Are all our theories really borrowed theories, from sociology and economics primarily, in which case I think there's a strong

argument for saying you should get your degrees in economics, sociology, and just learn some demography along the way. Or is there enough of a theoretical corpus in demography itself that is home-grown?

If you look at Caldwell's theory, for example, I think it's fair to say that's a demography theory. It's not really sociology or anthropology or economics, it's a demographic theory. And stable population theory is really not much of a theory--it's a fairly straightforward mathematical model--but it has been twisted and mixed with other ingredients in so many complicated ways that I think now you could say there is a body of mathematical theory surrounding stable population theory, or models of demographic renewal in general, blended with theories of economic growth, intergenerational transfers, or that kind of thing, so there really is some demographic theory in that core. Yes, I think that's true in a number of places where the economic demographic models by themselves don't really make that much sense and the sociological models by themselves are sort of wishy-washy and vague, but when it's sort of all put together in the demographic context, it becomes much more viable; it's synthesizing these different approaches. I think there's really something there that merits separate attention. And I think there's enough in demography that it's difficult to cover sort of with your left hand while you're getting a full-time Ph.D. in some completely different subject with all of its own demands.

VDT: How is your Graduate Group in Demography program different from what you got as a student some 20 years ago?

LEE: That's an interesting question.

VDT: Which was a multi-disciplinary program. You mentioned public health, biology, as well as economics and sociology.

LEE: Yes. In terms of the kinds of courses the students take, I wouldn't say they're fundamentally different. What they include is different because the field has changed in 20 years. But the kind of course that Judith Blake taught, for example--she taught fertility--and mortality was another course. Those courses and their general conception could have been taken as models for the sorts of courses that we teach here and I try to teach, in that they were a blending of social theory with demographic data, demographic measurement issues, and what fresh insight she brought. They were very lively and broadly-informed courses. And I think that's the kind of thing we try to do here.

Then there were also methods-oriented courses. We are considerably more model-oriented and mathematically-oriented than the subject was in those days.

VDT: Part of the development and in part because of you?

LEE: Yes. Partly because of the specialization of Kenneth Wachter and of myself and Hammel. So our admissions requirements also are much tougher in the quantitative direction. I imagine the graduate students with interests in social science are probably generally quantitatively stronger these days than they were in those days.

But the general spirit of the training, I don't think, is any different now. It was certainly not an insular program in those days; it was interdisciplinary, it was rigorous.

VDT: You have more or less an updated version of the original Group in Demography. Then there was the Department of Demography in between. Is it important to have a department with that actual name?

LEE: It's interesting that you ask, because Gene Hammel and Ken Wachter and I had lunch just four days ago with the dean of Literature and Science and discussed this question, among others. It seemed to be the leading question: Should we be a department?

I think the truth is that that makes very little difference. It may make some difference to the outside world, which is what I suggested to the dean, when it comes to recruiting students or whatever. It's a model that looks more solid if it's called a department. But in terms of the reality of the situation, it would hardly change anything. We have our own full-time tenured appointments in demography; we admit our own graduate students; we give our own degrees. What would be different if we were a department?

VDT: Is there something about the funding that makes a difference if you're a department of the university?

LEE: Well, it can, but in our case it doesn't. We have a quite good budget that is our own budget. Ordinarily, a graduate group would not have its own faculty appointments; we would all be borrowed faculty from other departments. But in our case, we do have our own FTEs. We just got another one.

VDT: What's that?

LEE: Full-Time Equivalent--that's the unit of currency for faculty positions at universities.

VDT: How is it that the graduate demographic group at Berkeley has all these special privileges that maybe some groups at other universities do not have?

LEE: Well, originally it was Judith Blake who persuaded the administration to set it up in that way. Of course, then it [the department of demography] was liquidated, in the 1970s [1972]. I think it was really the administrative genius of Gene Hammel who got the thing set up again and fought for resources.

VDT: When was that?

LEE: It was happening during the second half of the 1970s. I was first offered a job here maybe in 1976 or 1977; there must have been a good deal of work done before that. Hammel chaired a university-wide committee on demography that was in charge of setting it up.

VDT: But you came in as the first director; it really only started again when you came back?

LEE: I was offered the position of chair and declined, because I didn't want to take on the administrative burden. I wasn't sure I wanted to come into a program that was just starting up when I was so comfortably established at Michigan and I was very well funded and so on. So I turned down the first offer. Then a couple of years later, Hammel called me up again and asked whether I would accept an offer that was separated from the administrative work and come as a professor. So after a while, soul-searching, I decided I would. So I came in the fall of 1979, as professor of demography and economics, within the Graduate Group in Demography. Gene Hammel is the director.

VDT: And you still have to decide whether a separate department is necessary?

LEE: Well, it's an important and controversial question.

VDT: But obviously this group is flourishing.

Who have been some of the leading influences on your career? You've mentioned the people who were here at the time you were studying at Berkeley.

LEE: Yes. I would say Nathan Keyfitz, certainly. It was sort of a spirit of intellectual excitement. That was very important when I was a student and throughout earlier stages of my career.

Ansley Coale was also very important. I was never a student of Ansley Coale's; I was never at institutions where he was. I sent him a copy of my dissertation and he became familiar with it early on and liked it. I guess I corresponded with him. I'd see him at meetings and somehow he was a guiding light. Again, I think it was his enthusiasm, encouragement, appreciation, because that work I'd done at Harvard was pretty much done in a vacuum so far as any kind of demographic advice was concerned. So Keyfitz and Coale were very important.

Then once I'd gotten to Michigan, Ronald Freedman was very important, just as a source of advice and, again, encouragement.

VDT: Though the two of you don't do the same kind of demography.

LEE: No, that's true. There wasn't anybody there doing--what shall I say?--mathematical demography, the kind of modeling that I was interested in. But I found him an important influence.

VDT: What about Easterlin?

LEE: Yes, Easterlin too, certainly--particularly his writings. When I was writing my dissertation and came across his work, that was very important to me. It was also very disappointing, because I thought I'd invented something new and I realized that not only it was done, it was published, and it had been done 15 years before. But I enjoyed many discussions with Easterlin; found his work very interesting and still find his work very interesting. He was another participant at this seminar that Chuck Tilly organized at the institute at Princeton, so I got to know him that summer [1972] as well.

There are four people who were important.

VDT: And who have been some of your leading students?--so far, because you've got a long way to go. Keyfitz cited you as one of his leading students.

LEE: Well, I'm glad to hear that.

VDT: Students you're proud of.

LEE: Recently, Andy Foster, who just went off to the University of Pennsylvania, and David Lam, who's at Michigan and essentially replaced me at Michigan in economic demography.

VDT: He stepped right into your place, though he was just finishing?

LEE: Five years after I left. Also Mark Montgomery; he was at Princeton for five or six years. He's now abroad someplace and is going to Stonybrook. Andy Mason, at the East-West Center. Barbara Devaney--she was at Duke for a time; I'm not sure where she is now [Mathematica Policy Research, Princeton, New Jersey].

VDT: All these people did mathematically-oriented dissertations under your aegis?

LEE: Quite mathematically-oriented, yes.

VDT: They had to. Now let's talk about your work on the two NAS [National Academy of Sciences] panels. First, on fertility determinants. How did you get onto that panel? I wouldn't have thought of you as a fertility determinants person. There were your long projections?

LEE: Yes. I had worked on the problem of predicting fertility in developed countries. I'd worked on the Easterlin hypothesis; I'd written quite a number of papers on that besides the one in PDR. I've worked on fluctuations in marital fertility in relation to fluctuations in nuptiality and other short-run fluctuations generally and their relation to economic and climate changes--those kinds of things. But fertility . . . Well, actually I've done more substantive work on U.S. fertility as well. I did a paper on target fertility.

VDT: I just read that last night ["Aiming at a Moving Target: Period Fertility and Changing Reproductive Goals," Population Studies, 1980]. I couldn't put my latest Population Studies on the shelf and decided to throw out the whole of 1980, but first looked, and there was your paper.

LEE: "Aiming at a moving target"--that's one of my favorite papers. But there was a predecessor, published in Demography in 1977, on "Target Fertility, Contraception and Aggregate Rates" [1977]. So I've done quite a number of papers in the area of fertility, mostly the technical sort and mostly for developed countries.

How I got on the panel is difficult for me to say. My guess is that Ronald Freedman and Richard Easterlin, who were on the panel, suggested me, not because I had any special knowledge of fertility in developing countries but because they thought I'd just have generally good demographic skills, bringing another dimension--perhaps for economic demography. Well, Bob Willis was on the committee also. Also I taught economic demography for many years and in teaching you have to teach everything, which means you have to learn everything, so I'd done quite a bit of reading on fertility in developing countries.

In the end, I was made chair of the working group to summarize our state of knowledge of fertility and its changes in developing countries. I think that came about because at one stage we were all asked to write little summaries on some aspect of the problem. I was assigned to write up something on income distribution and fertility change. Many people sort of ignored this assignment, but being more junior than most of them, I took it very seriously. I think the powers in the committee [on Population and Demography], chaired by Parker Mauldin, must have read this little piece and decided it looked good and that that sort of synthetic and critical skill was just what they wanted for this project of summarizing the state of knowledge.

The working group was Randy Bulatao, John Bongaarts, Paula Hollerbach and myself. We met here at Berkeley and drew up an outline of what we thought were important areas of the field. At some early stage, there was a decision to use basically the Easterlin framework. [This is the Easterlin or Easterlin-Crimmins "synthesis framework of fertility determinants," first set out by Easterlin in "The Economics and Sociology of Fertility," in Charles Tilly, ed., Historical Studies of Changing Fertility, 1978, and described by Easterlin in Vol. 2, Chap. 15, of the NAS report, edited by Bulatao and Lee, Determinants of Fertility in Developing Countries, 1983.] I think it was a good decision and we probably would have done it in any event, but in fact, a separate working group of the committee had looked into the question of the theoretical framework and Ronald Freedman had his framework and

Richard Easterlin has his. They were both in this group to hammer out what framework we should use. Ronald Freedman started out by saying he didn't think it made much difference what framework we used and he was perfectly willing to have the committee use the Easterlin framework, which he thought was a convenient one. So that framework was adopted early in the work plan of the committee and it was natural for us as a working group to adopt it.

I think there was a lot of misunderstanding--and still is--about that framework, which I view as nothing more than a framework, that is, without theoretical content, without bringing in any assumptions. People continually believe that it's an economic theory of fertility, which it's not. They think the terms "supply and demand" are economic terms, which they're not in this context. In fact, any sociological or psychological or economic theory can drive that framework. It really categorizes influences into things that work through the culture and sociology and biology of reproduction on the supply side or influence how many children people want, on the demand side. But the framework doesn't say in what way the demand for children is determined.

VDT: That framework got a scathing review by Paul Schultz in Population and Development Review [PDR, March 1986, review of Richard Easterlin and Eileen Crimmins, The Fertility Revolution: A Supply-Demand Analysis, 1985, which Schultz describes as seeking "to bring empirical content to the Easterlin synthesis framework"].

LEE: My feeling is that that was a very small-minded review, that, in fact, Schultz and Rosenzweig had pretty much swallowed whole Easterlin's framework and adapted it to their own purposes and used it in a very nice article in American Economic Review, but essentially a derivative article from Easterlin's framework. So I view that review as nitpicking with details of the way it's done, but it's essentially accepting the broad outlines that Easterlin sketched.

VDT: Did your work on the two NAS working groups--fertility determinants and population growth and economic development--overlap?

LEE: No, we'd finished the fertility determinants work. I was off the working group and had nothing more to do with the National Academy of Sciences for a few months.

VDT: Then you were appointed to be co-chair [with Gale Johnson] of the working group for that report. Sam Preston said he felt an economist should head it, so he turned that down. And he said that the work you had been doing meanwhile on the IUSSP committee was very influential background to the report, Population Growth and Economic Development: Policy Questions [by Samuel Preston, Ronald Lee, and Geoffrey Greene, National Research Council, Working Group on Population Growth and Economic Development, Committee on Population of the National Academy of Sciences, 1986]. Almost none of the research, background papers, were ready at the time he--who did the first draft of the report--was writing, so the work of your IUSSP committee was most useful.

LEE: I was deeply involved with the IUSSP. I had been asked, around 1981 or 82, to chair what was essentially the economic demography research committee of the IUSSP, which I was very honored to to. We were supposed to look at macro consequences of population change, and I, together with Georges Tapinos, assembled a strong committee. There were Brian Arthur, Allen Kelley, T.N. Srinivasan, the development economist at Yale, Gerry Rodgers, English development economist working for the ILO, and a Brazilian.

We planned two conferences, which have now resulted in two volumes. One was Economics of Changing Age Distributions in Developed Countries [1988] and that wasn't really relevant to this topic. But the other one was on Population, Food, and Rural Development [1988] just explicitly on the

Third World and that was very relevant for the work of the NAS panel. Putting together these conferences and editing the volumes took a great deal of time. They have both now appeared in the IUSSP Oxford University Press series. And with the IUSSP approach, which I think is very nice, these are very international. You look for scholars from all over the world who are doing interesting work in these problems.

The IUSSP conference on population and development was held in December 1984 and it was ahead of the NAS schedule. Although the book actually came out a couple of years later [only in 1988], most of the papers were available at the time of the National Academy committee's work. The ones that were most useful--well, I'm not sure which ones Sam had in mind.

VDT: He said there was one on income distribution by David Lam that was finished.

LEE: That was one of the National Academy background papers.

VDT: Oh, yes--the only one finished in time for Sam's first draft of the report.

LEE: T.N. Srinivasan had a paper on food and population in the IUSSP conference and then we borrowed it for the NAS. Pingali and Biswanger were two interesting people; they also did a paper subsequently for NAS. Their paper was sort of testing and exploring empirically the Ester Boserup ideas about induced technological change, particularly in sub-Saharan African agriculture. Then there was a very interesting paper by Robert Evenson of Yale, again looking at Boserup-type outcomes of population change in India. We were fortunate to be able to sort of pick off research by Indian economists that had been going on for quite a number of years.

VDT: And all of these were available and sort of fed into your NAS report?

LEE: Yes.

VDT: Let's jump into the flap over the report. As I said to Sam, one can read what one wants to into that report. You concluded that: "On balance, we reach the qualitative conclusion that slower population growth would be beneficial to economic development for most developing countries." Julian Simon felt that you were defaulting to the other camp, those who believe that population growth is still important. In the symposium review of the report in Population and Development Review [September 1986], he criticized you in particular, saying you had been skeptical that rapid population growth impedes economic development, or at least per capita income growth, in your review [PDR, March 1985] of the World Bank's 1984 World Development Report and also in your IUSSP newsletter article of 1983. In other words, he felt that you really had great reservations that rapid population growth was all that important in economic development. That's the way I interpret what he was saying there about you and your past writings.

LEE: Yes, well, Julian Simon being upset doesn't make a big flap. The flap was more on the other side.

VDT: Right, okay. Well, what did you feel about your qualified conclusion, for which you have been criticized?

LEE: I think it's a very good report. I certainly stand by it. My impression of it at the time we were putting it together, writing it up, or as I read drafts of the policy conclusions was that it was very bland and guarded, middle-of-the-road views that we were espousing there. And I still think that's the case.

There are really dreadful sentences, like: "Investment in family planning is justified provided the benefits are greater than the costs." Sentences like that don't just happen; they're put in there because someone wants them.

VDT: A committee.

LEE: You have to get something through a committee. So I was very surprised and taken aback by the reaction of people like Paul Demeny to the report. I think basically they're wrong and arguing for a very parochial point of view, and perhaps have not read it carefully. Demeny complained about citations, such and such was not cited, but that report is really written to go with the background papers and many of the citations in it are to the background papers [published separately, in 1987]. The background papers are reviews of the literature. So I would say the citations in the report, although they were ample, were far from exhaustive and far from as many as there would have been had there not been this set of background papers.

I think demographers are a rather self-selected group of people who believe this is a terribly important problem. I think that is a good part of why some demographers were upset.

VDT: You had upset the orthodoxy. I guess you've read Dennis Hodgson's just-published article on orthodoxy being overthrown by revisionism ["Orthodoxy and Revisionism in American Demography," Population and Development Review, December 1988].

LEE: Yes. Well, it also seems to me that the National Academy report was not very different from the World Bank's World Development Report of 1984, the paper by Timothy King on population and development, the paper by Geoff McNicoll in PDR on population and development, or, most recently, the review of the issue by Allen Kelley in the Journal of Economic Literature. It seems to me that there is a very broad consensus on these issues among people who are studying population and development.

VDT: It just wasn't as alarmist as some of the old guard felt. And, of course, it came after the turnaround in U.S. policy at the 1984 Mexico City population conference, and a lot of people were alarmed by that.

LEE: When you release this sort of thing, you could write it with an eye to how it will be used. I'm thinking of stories like one that somebody, I think maybe George Zeidenstein [Population Council president], told me of having been in Kenya and having a discussion with the Minister of Planning, who pulled the National Academy report off the shelf and said, "See, we don't need to be worried about family planning here."

But I think when you write a report like that what you're supposed to do is evaluate the scientific evidence and describe what you think is known and what isn't known and not slant things in order to affect policy in one way or another. I think you have to just trust that the knowledge that you summarize as well as you can will be appropriately used. So I don't think it's right to say, "Well, if you write a report like that, then funding will be cut for family planning." That's some other sphere of activity. The point of a scientific report of a group like the National Academy is not advocacy.

VDT: But wasn't AID money behind it?

LEE: It was money from AID, from Hewlett, and from Rockefeller, I believe. But a point of the National Academy structure is to insulate the activities from the interests of the funders.

VDT: And you came out with something so different from the previous report of the National

Academy of Sciences on Rapid Population Growth [:Consequences and Policy Implications] of the early 1970s [1971]. Well, it was 15 years down the road.

Now you're working on externalities to childbearing, which you made a big point of in that report--more should be known about the externalities of childbearing.

LEE: That's right. That's one of the things I'm working on; I'm very interested in it.

It seems to me there are probably many consequences of population growth that simply don't get reflected in standard economic measures. Virtually all the environmental effects, for example, don't. They will be eventually, if the environment turns out to have a strong effect on agricultural yields, things of that sort. There may be quite a number of ways in which population change can affect our lives without affecting what is conventionally viewed as economic developmental type variables. So that's part of the interest here, to look beyond the kinds of things that were in the NAS report.

I was very frustrated as we were working on that report because so little seemed to be known about this subject. But at the same time many people thought it was centrally important and we commissioned a paper from Bob Willis on this topic for the volume and I think it was a very good paper, but it was a narrow, sort of mathematical, theoretical paper, and it seemed to me that there was quite a bit that could be said relatively easily, without needing a lot more theory than we already have.

VDT: You mean the social costs of childbearing?

LEE: Yes, something of that sort. I was asked by the United Nations and INED, who were planning a joint conference, to do a paper for them on my work on demographic history and I accepted. But a few months later I began to think, "I'm so bored with that topic and I'd like to try a paper on this externalities issue; maybe I'll call them up and see if I can change my topic," and they were agreeable. So I started working on this externalities paper. I presented it to the UN last August [1988].

VDT: The conference Paul Demeny was involved in?

LEE: Yes, he was at the conference also. I've presented it maybe six or eight times now, including at the PAA [meeting, spring 1989], and it always seems to generate quite a lot of lively discussion.

VDT: Indeed. You pointed out in your PAA paper ["Externalities in Childbearing in Developing Countries: The Case of India"] that some costs are just not there [thought of]. What was the one that cost the most--something like \$12,000?

LEE: That, in the case of India, was the fact that when you have another child, you're diluting everyone else's birthright, everyone else's per capita share of the value of publicly-owned mineral resources, which are dominated by coal in the case of India, and that is some number of thousands of dollars per head.

VDT: I hope you will go on with that. What are your recollections of Bob Lapham, who was staff director of both NAS projects you worked on? Alas, I didn't get to interview him [died February 20, 1988]; PAA secretary-treasurers are being interviewed for this series too. I'd like to get his daughter Susan's lovely accolade to her father that she read at the PAA meeting [1989] when they presented the first Robert Lapham award. He must have been a dynamo. He overlapped his work for the NAS Committee on Population and Demography with directorship of the Demographic and Health Surveys. And he was your PAA secretary-treasurer too [held office 1984-87].

LEE: That's right, so I had a lot of interaction with him. I was very saddened by his sickness and his

death. He was very serious, very hard-working, and had an enormous amount of integrity, in particular in the National Academy context, which is where I had the most contact with him. He was always particularly careful to insulate us from the views and feelings of our financial sponsors. He was always making sure that we didn't feel undue pressures--do this; do that--taking pains to let the work of the committee move forward according to its own internal logic. Very well organized; things always went very smoothly. A pleasure to work with.

VDT: Good. Now, which of your publications do you consider most important and why? You've had a lot; you've already mentioned several, but mention them again.

LEE: How many can I mention? I think, of all my work, probably the work that's closest to my heart is the historical work on England. And I think the first article I ever published, which was in 1973, is one of the ones I'm most pleased with, if not the most important. It was called "Population in Preindustrial England: An Econometric Investigation" [Quarterly Journal of Economics, 1973].

VDT: The one where you projected backwards?

LEE: No, that was another one. The 1973 paper was about the causes and consequences of population change in preindustrial England: the effects of population growth on wages, the effects of real wages on population growth rates, and the influence which I thought--and I still think it's very important--of long-run changes in mortality that were essentially independent of the economy but caused these population swings, which then had repercussions throughout the society and economy. It was a sort of empirical investigation of Malthusian kinds of ideas, with some additional theoretical ideas. I've written probably ten papers along that line, but I think that one was probably the most important.

The forecasting paper I mentioned earlier I think is quite important ["Forecasting Births in Post-transition Populations," Journal of the American Statistical Association, 1974]. Also the inverse projection paper, the methodological one; that was the method for inferring vital rates from baptisms and burials ["Estimating Series of Vital Rates and Age Structures from Baptisms and Burials," Population Studies, 1974]. There was a followup in 1981, but that's derivative; the more important one is the first one.

Then there was the "Aiming at a Moving Target" paper ["Aiming at a Moving Target: Period Fertility and Changing Reproductive Goals," Population Studies, 1980]. That I think is quite important; I'm about to do another version of that.

I think--of course, I can't be sure now--but I think this paper on externalities is going to be important. I like it a lot and want to do more on that.

Then I've done quite a bit of work now on intergenerational transfers. That's sort of an integration of stable population theory with some economic models of growth: how age groups that don't produce are able to consume, and so on. I've done four papers in that area and I think one of those, which is not yet published--it's under review now--is probably the most important of that bunch. It's work generally on demographic change in relation to economic well-being in certain developed countries. The most recent paper in that line of research is on mortality decline and aging--what it costs. I concluded costs as about 1 percent per year of our consumption for each additional year of life expectancy.

VDT: Are you talking about national averages?

LEE: Yes. The life expectancy of 76 rather than 75, or 71 instead of 70, means we'll all live longer,

we'll consume more in total over our life cycle, but we'll have to consume about 1 percent less each year in order to fund the increased year.

VDT: That's feeding right into what Kingsley Davis has been saying about the high costs of our non-productive elderlies.

LEE: Oh, absolutely. At this point, for each year we gain in life expectancy--we can think of that as a gain in person years lived--two thirds of that gain is gained after the age of 65 and the remaining one third is divided between our working ages and childhood. But as life expectancy increases, we move closer and closer to having 100 percent of the gain occurring in the retirement years, and that takes an ever-higher toll in terms of what our consumption has to be. We have to provide for those years either through savings or through pay-as-you-go pension systems or through transfers in the family. But it all comes to the same thing in the end. It doesn't make any difference in some sense which of those you do; it still costs what it costs, and what it costs is about 1 percent . . .

VDT: Who are you aiming that at, other than your fellow professional demographers? Do you think it important to speak to policymakers?

LEE: I think the real message--what I learned when I wrote that paper was that we should not complain about rising payroll taxes and such things, that in developed countries in a low-mortality setting essentially, it's just a matter of having to pay for our own retirement. We're really paying for ourselves at a later stage in our life cycle. It isn't that we're paying for a lot of other people's children or because other people aren't having children. It's just that we're living longer and that has to be provided for and that's the way you do it. We should be glad we're called on to do it, because if we weren't, it would be because we were dying younger.

VDT: A great message. Do you think it important to get such messages out to the general public?

LEE: Yes. I would like to do an op-ed piece or something like that at some point.

VDT: Have you done that?

LEE: No, I have never done that. But I think this is a simple idea, which I didn't really appreciate before. And it's in contrast to the situation in developing countries. In a high-mortality setting when mortality declines, the person years you gain are mostly in the productive ages and consumption can go up, from that point of view. But what also happens is that declining mortality means the population starts growing a lot faster, gets younger, and you've got more dependents. Although you've got more productive years in a life-cycle sense, at any instant there are more dependents, more children, in the population and you end up having to consume less. Now in that situation, you can feel aggrieved, you can feel, "I have to pay higher taxes just to take care of this growing number of young dependents." It's a quite different situation. You're not having to put by money to provide for your own retirement, which is completely minimal--with very high mortality there's only one person year of life expectancy after age 65. In that situation you're much more a victim of societal demographic trends. But in the developed low-mortality setting, you're basically just providing for yourself. I think if people realized that it would make the payroll tax a lot less painful, and the increases we're going to have going into the 21st century.

VDT: I hope you will write that op-ed piece, at least. That leads into my next question: What

accomplishments in your career so far have given you the most satisfaction? In a sense you've answered that with your publications, your development of your several influential models.

LEE: Well, yes. I think I've talked about what in my research has been most satisfying. In terms of recognition, the single most satisfying thing was the Mindel Sheps Award [for Mathematical Demography, PAA, 1984].

I'll tell you, I would not rank my work for the National Academy of Sciences high in terms of being satisfying. Extremely time-consuming, though it had its interesting points.

Of course, I find the reception by some demographers of the work on population growth and economic development discouraging--the NAS report. I put in a lot of work; I think it's a good report. I think these people are largely wrong and have not read it carefully or have not read the background papers. I don't know what its impact will turn out to have been. Of course, I will be unhappy if its impact is that it has led to reduced funding and so on. As I said, I don't think that should have been, and wasn't, our concern as we were writing. We shouldn't be writing with an eye to its uses. So I'm not thrilled with that.

I'm not thrilled with the results of the fertility work [Determinants of Fertility in Developing Countries, 1983]. Not because that was controversial or anything, but because I think it didn't come out with one crystalized, clear insight. It's sort of a review of an enormous amount of research and it doesn't try to tell you, "Here's truth," which means from an intellectual point of view, it's not terribly satisfying.

I must say that in the last few years I've come to appreciate the teaching more and more. I don't think there's any feeling quite like the feeling that can--when things are going well--develop in a classroom where there's sort of a shared understanding, a shared intellectual effort to reach the truth. I've seen students get excited and try to do their own work; seeing graduate students enter and then maybe four, five, or six years later leave as professionals, publishing interesting work. That sort of thing; that's a very good feeling. So, as opposed to my earlier days, I'd rank teaching right up there with everything else, I think, among the most satisfying of the activities. The teaching and research have been the most gratifying.

And of the--I don't know what to call the NAS work; it's not really research, it's not administration, it's something in between. Well, of course, it had its satisfactions. The IUSSP work had its satisfactions.

The other organization I've been quite involved with, aside from NAS and IUSSP and PAA, is the NIH [National Institutes of Health] and their grant review panel. I'm just now finishing four years--I'm now chair of the Social Science and Population Study Section. That's a major piece of work also.

VDT: Reviewing all the proposals?

LEE: Yes. We meet three times a year and each time it ends up taking two or three weeks of work.

VDT: You go to Washington for it?

LEE: Yes, I go to Washington, but the hard work you do before you go. You write up these long critiques, five or ten typewritten pages for each proposal you have to review and go to Washington and read them off.

VDT: And do you find it discouraging when so few of the approved ones get funded?

LEE: Yes and no. Every time I think of how expensive research is and how many households in the

United States are paying taxes to support some demographic research project, then I think we shouldn't give nearly as much; we want smaller projects and to fund only really the most crucial. At other times, I read a proposal and think this is marvelous and I'd like to see it funded and then it doesn't get funded and that's very discouraging. But all we can do is rank them and then which get funded and which don't, how far down that list they go, has nothing to do with what we say about them--that's a matter of how much money there is.

VDT: I understand that of those that are approved . . .

LEE: About 20 percent get funded.

VDT: I understand only about 16 percent are getting funded; it's way down.

LEE: Well, about 99 percent get approved.

VDT: Oh, I didn't realize that.

LEE: Almost all get approved. But then we give them a score, between 1 and 5, or 100 and 500--100 being the best. And based on that, a ranking is established, and based on how much money is available, they just go down that list until they run out of money. It's not just our list, but we are in with other lists.

VDT: What about your own center when it submits a proposal?

LEE: We're reviewing what are called ROIs primarily. Those are individual research project proposals, not center grants, not training grants. But when I submit a proposal that means they have to appoint a special study section to evaluate it; it's not evaluated by the same people at all.

VDT: So you don't find that work terribly satisfying?

LEE: It is satisfying, and I think it is very well done; I've been very impressed by those review procedures. But it's extremely time-consuming; it takes a big bite out of the time available for research. Research comes out of whatever is left after you do everything else you have to do, and something like that will make a big difference. And I know that my career, having been simultaneously involved with all four of these organizations at the same time, has been seriously affected.

VDT: NAS, IUSSP, NIH--what's the fourth?

LEE: PAA.

VDT: Good heavens, yes! Well, you have had a very full plate for a while.

My last question and then a little on PAA. This we've also touched on: What do you see as leading issues in demography over the years you've been involved? You got into it sort of by the back door--historical demography. You weren't so concerned with the baby boom and bust, perhaps, until you discovered that Richard Easterlin had already stolen your ideas.

LEE: Certainly not stolen my ideas, but working on it very nicely long before it had occurred to me.

VDT: What do you think have been the leading issues? I think of you as somewhat removed from the hot spots, other than that of population growth in less developed countries.

LEE: Yes, I think my work has always been on topics other than those most people were working on.

VDT: It's a nice position to be in?

LEE: Sometimes yes; sometimes no. It means that getting people to read my work is often difficult. Getting people to review my work for journals is sometimes difficult. I had an article that took a year and a half before it got accepted--that is, that long before I got any kind of response from the journal I sent it to.

VDT: Because they try to get a reviewer and they can't think of anyone?

LEE: Yes. They send it to someone who says, "I don't know what this is about; send it to somebody else," and so forth.

When you're working in a well-established school of thought you can cite people who've made this and that assumption and who've tackled the problem this way or that. So you don't have to justify every step in the same way, because much is taken for granted. Those first steps, the sort of assumptions that are made, are made by the first few articles in an area of research and then after that research goes on without questioning. So I think that has been a difficult aspect.

But on the whole, yes, I like being in that position of doing sort of off-beat, unconventional work. I think nothing pleases me more than something that feels really different. What was your question again?

VDT: What do you see as leading issues in demography over the years you've been involved; you were not so immersed in the leading issues.

LEE: Leading issues--certainly, the demographic transition and then as it is applied to developing countries and the fertility decline in developing countries and appropriate policies. That is one set of leading issues and through the National Academy work I was drawn into that. And the consequences of population growth, into which I was drawn both through the IUSSP and the National Academy, is another leading issue, but one in which surprisingly little research is done, compared to fertility, contraceptive behavior, and marriage, where a great deal of research is done.

I think now the issue of intergenerational transfers and tensions and aging population and so on is quite a hot topic. Again, there aren't that many people working on the macro issues, and that's something I've gotten quite interested in.

VDT: Do you mean by intergenerational transfers something on the "sandwich" generation too--the ones caught between their own parents and children?

LEE: Sure, absolutely. In any case, I think much of that problem is overblown, because so much of the greater costs of the aging population is offset by the lower costs of children.

VDT: That's interesting; some people don't remind you of that.

LEE: These days the costs of supporting the elderly population are primarily borne by the public sector and not only by the public sector, by the central government. The costs of children are borne

mainly by the private sector; within the public sector, they're borne at the state and local level--different sources of revenue, in any event. But primarily they're borne within the family, the private expenditures. So people are unlikely to think, "My payroll tax has gone up, but then I only have one child to put through college instead of three." But, in fact, there's a considerable offset. The numbers I gave earlier are what I would say are the costs of aging. That is, if we stay at zero population growth but have higher and higher life expectancy, each additional year of life expectancy costs us about 1 percent a year of consumption. What are we consuming now per person per year? Probably \$12,000, something like that. So a percent of that is about \$120 a year; it's not much.

VDT: Let's jump into PAA. Can you remember the first meeting you attended? Here's the wonderful list that Andy Lunde put together, started it. Not that you have to think that far back; some people have to be reminded of meetings 40 or 50 years back. Was it when you were at Berkeley the first time?

LEE: Heavens, no. I didn't go at Berkeley; I didn't go when I was a graduate student at Harvard.

VDT: Good heavens!

LEE: I didn't go when I was at INED. If I had stayed at Berkeley, at the master's level, I would have been drawn into PAA much earlier. As it was, I left Berkeley and went to Harvard where there was really no sort of socialization in population. I didn't even know what the PAA was.

VDT: It must be in part because you were on the West coast and most meetings were . . .

LEE: That's right, and then I went to Harvard where it never occurred to me. I didn't know any faculty who ever went to meetings like that. I remember when I went to Michigan for the job interview, in 1970, Beverly Duncan, who was then the editor of Demography, sort of chastising me for the fact that I didn't subscribe to it and probably didn't read it and I didn't belong to PAA.

Then the spring after I got to Michigan, I wasn't planning to go to the PAA, which was in Toronto [1972], and I noticed that as the week to the PAA was coming up, suddenly graduate students and faculty started disappearing from the Population Studies Center. I didn't know what was going on, but somehow I got the sense this was something I was supposed to do. So at the last minute, I found out whether my way would be paid and, yes, it would, and I got reservations. I couldn't get reservations at the hotel where the PAA was.

VDT: That's a good thing, because that was an embarrassing hotel; it turned PAA women away from the bar. That was an old Canadian custom; there was a ladies bar which you could go into with male escorts, but unescorted women couldn't go into the main bar. That led to the business of PAA resolving never again to hold a meeting in a hotel that had such discrimination.

LEE: We're about to go back to Toronto [1990 meeting].

VDT: We're going to the Royal York Hotel. The King Edward Hotel where the 1972 meeting was held was shabby during my growing-up days in Toronto and it was still shabby then; I was embarrassed. The Royal York was once touted as the largest in the British Empire and it's an old dowager, but I hear it's been fixed up.

LEE: I liked Toronto very much; it's a lovely place.

VDT: So you don't have memories of the barriers, okay. What do you remember about the meeting?

LEE: I got into this hotel that was about 14 blocks away; it was a long walk. The talks I remember best were Tom Espanshade's on the cost of children . . . In those days, I don't think this happens much anymore, but then the audience would often applaud, occasionally applaud, and sometimes stand up and applaud if they really liked the paper.

VDT: I never saw that!

LEE: Yes. They stood up and applauded Tom Espanshade's talk on the cost of children. The other talk I remember that was very well received was one by Janet Salaff.

VDT: On Singapore.

LEE: I think maybe it was. She was interested in Chinese demography but, of course, Chinese demography couldn't really be on China then, because there wasn't any data. So I was struck by that and also that she gave two papers at the same meeting and both of them were applauded.

And I remember Ansley Coale being awarded--I think it was at that meeting--the first Mindel Sheps Award [in Mathematical Demography], which was presented by Keyfitz, who was chair of the committee. So that established in my mind forever the value of the award, the magnitude of the honor. [The Mindel Sheps Award was first awarded--to Coale--at the 1974 meeting and presented by Jacob Siegel, chair of the first award committee.]

The presidential address, it must have been at a banquet.

VDT: In 1972 it was Amos Hawley.

LEE: I was a fan of Amos Hawley--I mean, I'd just read a couple of his articles, but those worked well. The truth is I can't remember a word of his address ["Population Density and the City"].

VDT: Norm Ryder was the next year [1973].

VDT: The Norm Ryder talk I remember.

VDT: He criticized his own National Fertility Study ["A Critique of the National Fertility Study"].

LEE: Yes--so I remember that. That year, I actually gave a talk at PAA. In fact, it was in the session organized by Julian Simon and it was on my historical work. That was in New Orleans, wasn't it?

VDT: Yes, 1973 was New Orleans.

LEE: Then since that, I haven't missed one. But that means I've only been to 18 or something like that.

VDT: How did you have such a meteoric rise from 1972 to become president just 15 years later, in 1987? I bet that's a record, practically.

LEE: You mean in terms of length of time of attending these meetings?

VET: In the sense of being a part of the PAA crowd, a member. You obviously moved up the ranks very rapidly. What do you suppose accounts for that?

LEE: I don't know. Ask somebody else. [Laughter] I don't know how these things work.

VDT: How do you think people become president?

LEE: Let me think about it a moment more seriously. I remember feeling, when I was at Michigan, that no one was paying any attention to me and no one ever read or cited my research, and I felt annoyed--for a number of years. I'd see other people going off and doing things on committees and so on. Then I do think I was beginning to get better known while I was at Michigan. But I had the most minimal involvement or no involvement in national committees and in PAA; I didn't do any of this stuff. Somehow it all happened at once after I had come to Berkeley.

I think the first. . . well, I got asked to be on the NAS fertility committee. Organizing these things you spend an incredible amount of time on the telephone, calling people up and asking them for papers and so on, so people get to know who you are. Then there was the IUSSP meeting in Manila in 1981. I had never had anything to do with the IUSSP. I was interested in the IUSSP. I'd have liked to be going to their meetings and be invited to seminars, but I never heard anything from them. Then suddenly for this meeting in Manila, I was asked to do a plenary address, of which there were only two. So I really worked hard on a paper and did a plenary address.

VDT: What was that on?

LEE: Well, they assigned a topic, which was: "From Rome to Manila: How Demography Has Changed in 30 years." [Rome was the site of the 1953 IUSSP meeting.] So I was cast in this role of an elder statesman or something, although of course I wasn't. As I say, I took this very seriously and did what I think was an interesting paper.

VDT: I must read that. Nobody that I'm close to went to Manila. It was so far away and it was just before Christmas.

LEE: Yes, they often are. After that, for many years my Christmas was messed up by IUSSP committee meetings; they always seemed to be just before Christmas.

VDT: When it's cool in less developed countries.

LEE: Yes. And that led also to my then being asked to chair this IUSSP committee, which again involved innumerable phone calls, trying to gather information and put together programs for this conference and so on--just an incredible amount of work.

The NAS and the IUSSP and all of these things gave me more visibility, I guess.

VDT: It must have, because it was indeed quite a rapid rise--in PAA. I guess you weren't too involved in PAA at the time the Concerned Demographers were active.

LEE: There was a bit of that going on when I first went to meetings. I was asked to chair the Mindel Sheps Award committee pretty early on. I did that for several cycles, so for a while every other year, I was up there presenting a Mindel Sheps Award to somebody.

VDT: You became known as Mr. Mathematical Demographer.

LEE: Those are the only things I can think of. It surprised me, and also, unlike most people, I never served as vice-president. I went directly from being a Board member to being nominated to the presidency.

VDT: Who did you run against?

LEE: Valerie Oppenheimer.

VDT: That's amazing. In recent years, it's usually the woman who wins if a man runs against a woman.

LEE: There's an interesting wrinkle also, because I grew up at Vassar College and Valerie was an undergraduate at Vassar and, in fact, she babysat for me.

VDT: Was your father a professor there?

LEE: Both my parents were professors at Vassar. [His mother was Dorothy Lee, the well-known anthropologist, who was widowed quite early--his father had been a professor of mathematics--and raised a large family on her own, according to Judith Blake in her interview, above.] So I had known her. I can't say I had a very clear recollection, but I knew that I knew her and I recognized the name, Valerie Kincade, as she then was. That night, I think I would have felt good if she had won and I felt good when I won.

VDT: What do you remember about the year you were president? We've already mentioned your address, homeostasis, which I still find difficult to follow ["Population Dynamics of Humans and Other Animals," published in Demography, November 1987]. I remember you'd been swimming just before, so you were full of beans. Did you make any special efforts in organizing that meeting in Chicago?

LEE: This is by now a very familiar refrain, but it is an incredible amount of work. And at the same time I was doing that, I was chairing the group here, Gene Hammel was on leave, and I was graduate adviser here because Ken Wachter was on leave, and I was trying to edit these books for the IUSSP and I was reviewing proposals for NIH and I was still involved in the NAS business. Then there was this PAA program to put together, which I took as a very serious obligation, and the presidential address to write. It was a very difficult, just extremely demanding year.

My view was, and is, that you pretty much have to design the program to be a forum for members to present their research. So what you put on the program should be driven by the kinds and the distribution of research that people are carrying out--of course, taking into account how good the research is. And that there should be a minimum of sort of cute, trendy topics, which look very interesting on paper but for which, in fact, there aren't good papers to be presented.

VDT: Give me an example of a trendy topic. Cohabitation?

LEE: Well, okay. If you do these things prematurely--cohabitation, AIDS, homelessness, immigrants, squatters--all of these things. These things can be, and now are, excellent sessions. But there's a time at which you can do them and you will get hardly any submissions and the submissions you do get will not be qualified.

VDT: The Population Today writeup of your year said it was a subdued meeting. "Econometrics" seemed to be the buzz word; maybe that was thanks to you. And you were speaker at the applied demography breakfast. How did that come about?

LEE: I was asked. I in fact have an interest in applied demography. I taught a course on demographic forecasting for social planning, so I had to learn more about some of these topics. In any event, my interest in forecasting, my recognition of this as an important area of demography . . . It's an area in which many of the jobs are.

VDT: I was going to ask: What do you see as the outlook for demography in the U.S.? Does the future lie with applied demography? Is that where the jobs are?

LEE: Yes, there are a lot of jobs there, but it's hard to say now whether there may not be an opening-up of job opportunities in academics as well.

VDT: There could be jobs in academia still?

LEE: Well, still, or again. There is a big change; everyone was talking about it at PAA this year and I think last year as well. That is, the demographics of the situation are that there's going to be a greatly increased demand for college/university faculty in the 1990s.

VDT: Those who will be retiring, is that it?

LEE: Those who will be retiring and an upturn in births--when? I guess in the 1970s. There has been an increase.

VDT: In numbers of births, right: 3.9 million last year [1988].

LEE: How far down were we in the early, mid-1970s? It was 3.1, 3.2 million, maybe. I'm not really sure.

VDT: Never below 3 million, but okay.

LEE: That's a substantial increase and it's working its way through.

VDT: And you feel that there will be jobs in demography, that there's increased interest in demography?

LEE: I have been surprised at how much interest there seems to be in our demography PhDs that we're turning out now--from sociology departments, for example, and I'm hoping that one of our new PhDs will be hired by an anthropology department. That makes me think that well-trained people in demography will be very much in demand. Social science generally is becoming more and more quantitative and demography is at the core of the techniques, the knowledge of data sources, and all these things that are important for quantitative social science. I think in economics it's a much tougher row to hoe, because economics is already so sophisticated quantitatively and statistically, econometrically, and mathematical modeling and so on. But in sociology, and I hope in anthropology, there'll be more demography.

VDT: You feel a quantitative demographer has a break there; they wouldn't just take a mathematician, for example, if they're looking for someone quantitative?

LEE: No, because you need a deep interest in substantive social problems. So just the techniques by themselves, I think, go very little way, either towards producing good research, interesting research, or towards being a good teacher. You have to have both the technique and training in math and statistics and modeling and a really substantial knowledge of theoretical and substantive issues of demography. You can't just have one or the other.

VDT: What do you think of the changes in PAA meetings over the years? You mentioned a lovely thing that nobody had mentioned before--that they applauded papers. Of course, you haven't been going that long and the changes haven't been so radical in your time, but we're now up to 84 sessions with eight overlapping, many side meetings--even on Wednesday afternoon and evening this last time, we had these side workshops.

LEE: That was going on when I was president and I added seven sessions. I added an early Saturday afternoon time slot.

VDT: Oh, were you the first to do that?

LEE: I ended up with a lot of good papers left over after the sessions I had originally decided to have were filled up. I thought that wasn't right and tried to find places for them, so I myself essentially organized another seven sessions and put in that official time slot, and that worked out. I don't recall how many sessions I had [80], but probably fewer than there were at this last meeting [84]. So it's gotten bigger and bigger. Economic demography has just come a million miles.

VDT: Did you have something to do with starting that particular group that now meets on Wednesday?

LEE: Yes, I did. If I remember correctly, Lee Edlefsen and I did the original organizing of that group. We had a breakfast meeting on a Thursday or Friday; I can't remember where we were or how long ago that was. That came after we had a West coast meeting of economic demographers, which Lee Edlefsen organized and I hosted here at Berkeley.

VDT: Was that under the aegis of PAA?

LEE: It was not, no. But it was a great success; we all loved it. And there was no money involved: nobody's transportation was paid; people paid out of their pockets to come from San Diego or Seattle or whatever. It was nicely attended by 20 or 30 people and we had a delightful time.

That was so nice that it was decided to try to have a special meeting before the PAA; at least that's the way I remember it coming about. And the next year, instead of repeating this West coast meeting, there was a special meeting before the PAA on Wednesday afternoon. That has continued ever since. Unfortunately for me, as I got more and more involved in PAA business, I was always at Board meetings on Wednesday and I haven't been able to go for many years now. This last PAA meeting was the first year I was able to go to Wednesday meetings and this year they didn't hold any. I don't know just why. I guess they will be held again next year, because I'll try to see that they are held again next year.

I think that's been nice. But at the same time, there's been a strong feeling that it's important that economic demography be in PAA and not off to the side.

VDT: You don't think people will split off; they will still like to stay within the umbrella?

LEE: I think so. Now it's my impression, however, that in the last two years economic demography has not been very well represented on the program. I'm hoping it will be in the future. I hope that maybe Larry Bumpass [1990 president] and other people will. I would like to see more economic demography on the program. But I think economic demographers, for whatever reason, do not do a good job of writing to the incoming president, filling in the suggestions for sessions, and so on. They all sort of wait passively until the call for papers goes out and by that time the sessions are what they are.

But there's been a great change. In the beginning, there was very little economic demography, and the economic demographers you did meet were regarded as very weird people. I remember it was the New Orleans meeting where the "new home economics" presence was first felt. There were papers by Willis and Jim Heckman and Gronau, and this was regarded as very strange. Rockefeller, or whoever had been funding this work at Chicago, was very concerned about whether it made sense or whether it was a waste, and so on.

VDT: You mean the Gary Becker school of thought?

LEE: Yes--Gary Becker and other people. But I would say that now the economic demographers, although they're not so numerous, are really at the core theoretically of what an awful lot of demographers, and sociologists as well, are doing. That the basic ideas have been largely absorbed into sociological theory about fertility, as well as the techniques--econometric modeling and so on. So there's just been an enormous change in that way.

VDT: And you like that, I'm sure.

LEE: Yes, I like that. I don't think it's become dominant in any way; it's just another flavor at the counter now.

VDT: So they will stay within PAA. Yet, do you think that PAA membership is still a bit elitist and clubbish? It's only been around 2,600--it's fluctuated at that number since the mid-1970s [2,655, end 1988; 2,752, end 1990]--compared to, well, I always hear about the American Economic Association, which is multiples of that.

LEE: Yes, but after all, there are a lot more economists than there are demographers. I think PAA is a delightful organization. I guess attendance in Baltimore [1989 meeting] was far bigger than it has been.

VDT: Yes, for the first time over 1,200 [1,193; record till 1991 meeting in Washington: 1,399].

LEE: I think the meetings are delightful; they are a very nice size. Of course, they have gotten bigger, but still I know most of the people there. One thing in my having been elected president relatively rapidly may have been I know so many . . . I mean, I know all the people from Berkeley when I was here and a lot of people at Michigan I know from when I was there. Of course, there are not many at Harvard.

VDT: One thing Jay Siegel pointed out about being elected president was, "Well, all your students vote for you and everybody who's ever read your textbook." There are only two names and if only one is recognizable . . .

LEE: That's right. I saw someone in an elevator in Baltimore with a nametag that said "Shryock" and I thought, "Ah ha!" [Shryock and Siegel are co-authors of The Methods and Materials of Demography, 1971.]

VDT: You've never met Henry?

LEE: No.

VDT: You had never met Henry Shryock! He's at all the meetings. I hope you introduced yourself.

LEE: Well, no, I didn't. Nor did he.

VDT: Well, he's not that outgoing, but oh, what a pity. You should have done that.

LEE: Next year I will.

VDT: Obviously, you've had a good time in your career and in your connections with PAA and the others--not so much NAS. Are you just going to go on this way--your career plans for the future?

LEE: Oh, I am very pleased to be getting out of all the organizational work.

VDT: You've got it all over with early in your career!

LEE: I think I don't have any obligations with PAA or IUSSP or NAS or NIH. So after this June, I think I'm going to have a lot more time for research. I'm really looking forward to it. I have enjoyed the last year a great deal because my administrative stuff was way down and I felt much more productive.

VDT: You don't think you'll ever become the chair of the Group?

LEE: Oh, I probably will sometime, but I haven't, and I think actually I'd rather not. Research and teaching and just sort of international coordinating, liaison work--I think on the whole I'd much rather be doing those kinds of things than doing local administrative work. I've not been involved at all in university politics or administration here and I just prefer to have my energies elsewhere.

VDT: It sounds to me like you've had a lovely career and you've managed it very well, even though you didn't know what it was going to be when you started out. Thank you very much.

LEE: Well, thank you. I've enjoyed it.

CONTINUED

VDT: Afterthought. I've just asked Ron if he works with his computer or his pencil and he's claimed

it's with the pencil.

LEE: Yes, I work mostly with pencil and paper, that is, when I'm actually doing research. And most of my so-called empirical work is on my calculator watch, which is a fairly serious calculator watch. Casio doesn't make these anymore, but this does inverse trigonometric functions and so on. But, of course, I do my writing on the computer and statistical and spreadsheet analysis.

VDT: But when you're really thinking up your new theory and models, it's pencil?

LEE: Yes, pencil and paper, pen and paper.

VDT: That's the classic way--really classic.

LEE: I wish I could tell you I used a quill, but I can't.

VDT: And you say your oldest daughter is traveling around the world, in Israel at the moment and has been in Italy?

LEE: Greece, France, Germany, Switzerland.

LEE: On her own?

LEE: She went with a friend, but at the moment she's on her own on a kibbutz and then she's coming to Paris in June to study French. I'm going to a conference there in the beginning of June, so we're going to hook up early in June.

VDT: That's great. She's what--19 or 20?

LEE: She'll be 19 in a week or two.

VDT: That's amazing. That also goes back to your interest in the Peace Corps, but I think that's quite precocious--the younger generation.

LEE: Not long before that, she bicycled across the United States--part of this fundraising business--so she bicycled 3,800 miles.

VDT: Wow! Of course, everybody knew about those, with the--what's it called?

LEE: I didn't know everyone knew about it; this is Overseas Development Network. There was some group doing the same thing in Europe as part of it. There were 150 kids in the United States. There was a group that started in Texas, one that started in San Francisco, one started in Seattle, and I think one started in San Diego. They met in Washington and bicycled to the United Nations. That was the final destination.

VDT: They were front-page news in our local paper, the Washington Post. Great!

REYNOLDS FARLEY

PAA President in 1988 (No. 51). Interview with Jean van der Tak at Jean's home, Washington, D.C., February 4, 1989.

CAREER HIGHLIGHTS: Reynolds Farley was born in Harrisburg, Pennsylvania, and grew up in Akron, Ohio. He received a B.A. in liberal arts in 1960 from Notre Dame and an M.A. and the Ph.D. in sociology in 1963 and 1964, respectively, from the University of Chicago. He taught sociology and demography at Duke University from 1964 to 1967. Since 1967, he has been at the University of Michigan, where he is Professor in the Department of Sociology and Research Scientist at the Population Studies Center. He was Assistant Director of the Population Studies Center from 1969 to 1979. He was a member of the Advisory Committee on Population Statistics of the Census Bureau in 1975-81 and chair of that committee in 1980-81. He has been a Visiting Scholar at the Census Bureau, most recently in 1989, and serves on the advisory committee for the 1980 census monograph series.

Reynolds Farley is perhaps the authority on the demography of blacks and black-white demographic differentials in the United States. He is the author of many articles on the black population and three monographs: The Growth of the Black Population (1970), Blacks and Whites: Narrowing the Gap? (1984), and the 1980 census monograph, coauthored with Walter Allen, The Color Line and Quality of Life in America (1987).

FARLEY [elaborating on biographical introduction]: My father took a job with B.F. Goodrich in Akron, Ohio, at the start of the World War II boom, so we moved from Harrisburg, Pennsylvania, to Akron when I was three years old and I lived in Akron until I went off to college. My mother lived there until the last year or so. I've lived in the Midwest all my life, except for the three years early in my life and the three years I was teaching at Duke University in Durham, North Carolina, 1964 to 1967.

VDT: Currently, Ren is Visiting Scholar at the Census Bureau, here in Washington. You've followed Omer Galle, haven't you?

FARLEY: Omer was Visiting Scholar at the Census Bureau a couple of years ago.

VDT: Interesting. I'll tell you later why that conjunction is interesting.

To begin at the beginning, how and when did you become interested in demography and what led to your special interest in the demography of blacks?

FARLEY: I became interested in demography certainly by my first year in college. When I was a freshman at Notre Dame, I took a course in population from Donald Barrett, who had studied at the University of Pennsylvania. The course was something about world population growth and with a great deal about U.S. population growth and differential fertility in the United States. At that time, it struck me that demography was pretty interesting.

You could ask me what kinds of things I did for relaxation or liked to read when I was in high school or even before. I can always recall reading a fair amount about history, particularly U.S. history. And I can remember as a child paying a great deal of attention to statistical compilations of trends in size of cities, when states got large, when states got small, relative size of countries--things like that. So I got some early interest in both sort of population size and U.S. historical trends. As I was in college, it struck me that demography would allow you to invoke the skills that historians would

use, as well as looking at what happened to population size. So that is when I started getting interested in population.

VDT: You certainly capitalized on those early interests, because your history of the blacks, for instance [The Growth of the Black Population, 1970], obviously was drawn from the history of the United States in general.

FARLEY: Yes, it does fit in. The second year I was in college, the Population Association was meeting in Chicago [May 1958] and that professor, Donald Barrett, encouraged me to go up to the meetings and I did. I remember I went up on a Saturday morning and came back on Saturday night; Notre Dame is only 90 miles from Chicago. That was the first PAA meeting I attended.

VDT: I was going to ask you that later.

FARLEY: 1958 was Chicago, then the next year was maybe Providence?

VDT: Right--1959. I have the list here. 1960 was Washington. 1961 was New York.

FARLEY: I didn't go to that one. Then 1962 was Madison; I was at the Madison meeting. 1963 was Philadelphia; I remember going.

VDT: Your memory for the dates is astonishing!

FARLEY: 1964 was San Francisco. I didn't get to the 1964 meetings, but I've been to all the meetings since 1965.

When were the meetings in Ann Arbor [University of Michigan]? It was back quite a while.

VDT: Yes, in 1956. Ann Arbor was just that one time.

FARLEY: Ron Freedman says that the party [cocktail before the meeting banquet] was given at his house and Amos Hawley's house. They lived next door to each other and the group was small enough then that they could have a party split between the two houses.

VDT: Right. Amos Hawley told that story in his interview. Did you take demography all through your undergraduate years?

FARLEY: I think I took only one course in demography as an undergraduate, that first year.

VDT: So why did you pick it up and what took you to Chicago?

FARLEY: When I got to be a senior in college, I knew that I wanted to go to graduate school and that I was interested in population studies. So I applied to study at Chicago and at Michigan. I think I also applied to Michigan State, because I had a faculty member who said it would be quite straightforward for me to get some financial support if I went to Michigan State, but there wasn't a great deal of demography going on at Michigan State. There was one demographer who did internal migration. I wanted to go to the University of Chicago or Michigan and there was a financial . . . I guess I got some support from both places, but it looked more attractive at the time to go to graduate school at the University of Chicago. I graduated in the spring of 1960 and started in at Chicago in the fall of 1960.

VDT: And were you immediately swept up in the Population Research and Training Center?

FARLEY: Yes I was, indeed. When I got to Chicago, I served an apprenticeship working with Dudley and Beverly Duncan. I was very pleased; I learned a great deal.

VDT: Were you working on a particular project with them?

FARLEY: It was several different kinds of projects and I worked with some of the more advanced graduate students, but it was on population distribution, particularly in the Chicago area.

VDT: That resulted in which of their books?

FARLEY: I think they had already published their "housing a metropolis" volume [Philip Hauser and Beverly Duncan, Housing a Metropolis--Chicago, 1960]. The Negro Population of Chicago [Otis Dudley Duncan and Beverly Duncan, 1957] was published even before that, wasn't it?

VDT: I think so. I've just transcribed my interview with Phil Hauser and all these books came up.

FARLEY: They were frequently doing things on population growth in Chicago and the Chicago suburban ring. When I got there, the 1960 census data had not yet become available but people were getting ready to analyze those data. We were still working on growth of Chicago's population back to World War II. I remember we were looking at neighborhoods which were close to the El lines, close to public transportation, growing more rapidly than other areas. And, of course, there was Dudley's interest in how people were distributed across the city--interest not only in black-white differences but also occupational groups, income groups, differentially distributed throughout the Chicago area. So I started working on those kinds of issues and enjoyed it very much.

VDT: And that must have led to your interest in blacks. There's a high proportion in Chicago, isn't there? What was your Ph.D. dissertation on?

FARLEY: My dissertation was entitled Negro Cohort Fertility. The stimulus there was a mix between Dudley Duncan, Beverly Duncan, and Don Bogue. Dudley and Beverly with their quite liberal viewpoint on social equity were very interested in the fact that in almost every indicator, blacks seemed to be worse off than white. Why was there so much segregation? Why was the occupational situation so much bleaker for blacks in Chicago than it was for whites? Beverly and Duncan would talk about that a great deal. And I recall at the time, Gary Becker, who's gone on to great fame as an economist, was propounding his ideas that the market economy is equitable and if blacks are not getting such good jobs or if the quality of black housing is inferior to that of whites, it must be because blacks don't have the abilities to get better jobs or don't have the money to buy better housing. And Beverly and Duncan would point out: Could there be systematic discrimination? Perhaps there were factors such as realtors would keep blacks out of neighborhoods. And Gary Becker would say, "That can't happen, because if one realtor keeps blacks out of an area, some other realtor will realize there's great profit to be made by introducing blacks, and so in the market economy, you can't really have discrimination." I remember those kinds of fascinating arguments and that certainly stimulated me.

Dudley Beverly had me, as an apprentice, work on things, showing where blacks lived, showing some of the characteristics of blacks, how they differed from whites. So I got into racial issues by looking at what was happening in Chicago. If that had been an era when there was a large

influx from Poland or Russia or something, I suspect Dudley and Beverly would have had students like myself looking at what was happening with European immigrants. But at the time, Chicago was receiving a very large influx of migrants from the South. Indeed, they were getting off the train at 63rd Street, just below the university, and the area around the university was becoming largely black. There was great controversy about the university wishing to maintain its presence in a safe area as a largely white area--a fear that blacks were going to overrun the area, make whites unwelcome, increase the crime rate and so on. Even before World War II, I believe, there were discussions about whether the university should move lock, stock, and barrel way out to the suburbs.

VDT: Was the university in the midst of the riots of 1917?

FARLEY: The riots were just north of the university. The university had a long history of involvement with studying racial and social change in Chicago. The definitive work on the Chicago riots was officially published with the author as the Commission on Race Relations in Chicago, I believe, but the book was actually written by Robert Parke and Charles Johnson, both of whom were at the University of Chicago. So the university's exposure to the risk of racial conflict was well known, I think, even before World War II. And many of the faculty members saw the university as having a kind of moral obligation not to run to the suburbs, and the university didn't run to the suburbs. But there are racial problems around the university today, which are related to the racial makeup of the area.

So one set of the interests that I had in blacks came from the Duncans, Phil Hauser, and other people on the staff who were pointing to the dramatic changes occurring in the demography of the city of Chicago and their implications.

The other stimulus I got was from working with Don Bogue, actually taking a course or two with Don Bogue. Don by this time had become very outspoken in advocating lower fertility rates around the world. Don, more than some professors, occasionally showed signs of being a bit of a missionary in the classroom.

VDT: "Missionary" is a word often used about him.

FARLEY: It was clear. Certainly, he would have been very respectful of any student who disagreed. But Don could get quite enthusiastic about efforts to reduce fertility around the world and in Chicago. I remember how Don, more or less as an aside, was talking about how blacks at one time in the United States had very low fertility. During the Depression decade, reproduction rates for blacks were close to unity and the black population was on the road to zero population growth. Then it became clear by the late 1950s and early 1960s that black fertility was very, very much higher than white fertility. And I remember Don in the classroom saying, "How can this be? We don't understand this." Here blacks had had low fertility rates and suddenly they have not just a baby boom after World War II but they go on to extremely high fertility rates.

I recognized when Don said that, or shortly thereafter, that that was a very interesting question, that would involve some historical work to determine why black fertility rates were low in the 1920s and 1930s and how come they were much higher in the 1960s than they were for whites. I guess I thought about it for a year, a year and a half, and decided that would be an interesting thing to do as a dissertation.

Don always spoke extremely highly of Pat Whelpton. Pat Whelpton unfortunately died [in 1964] before I could meet him. He had an article coming out in Demography, it must have been the first or second issue, and he died after it was submitted. I remember Don asking me to go over the galleys and proofread it and make sure it was appropriate. Pat Whelpton himself, I think, could be

somewht difficult in certain circumstances, but Whelpton's cohort fertility analysis was a major breakthrough, disaggregating the tempo of fertility. And Don Bogue said it ought to be done for blacks, people assumed to have a history of fertility going from high fertility to low fertility to very high fertility. He was the one that recommended that I do a cohort analysis of black fertility, and that's what I did in my dissertation.

By the time I got around to doing my dissertation, Dudley Duncan had left for Michigan. My M.A. thesis, which was a large project, entitled Suburban Persistence, was done under Dudley Duncan's guidance. What I was looking at there was that many suburbs seemed to get a kind of social class identity when they developed right after World War I. Some suburbs seemed to attract, in that period of five or ten years after World War I, a high-status population; others had a sort of blue-collar population; others a sort of middle-class population. Then after World War II when suburbs increased their population size dramatically, it seemed that they didn't change the kind of people who were there. Suburbs which after World War I were very high in status seemed to be very high in status as late as 1960. Why wasn't there more shifting of status?

VD: What an interesting topic for an M.A. thesis--very ambitious! You used census data, of course.

FARLEY: Yes, census data. Dudley was the one--with a lot of help from Beverly Duncan--who put me onto that project. Then Dudley went to the University of Michigan [in 1962], so I knew I couldn't work with him for a dissertation, so I worked with Don Bogue on cohort fertility, at that time Negro cohort fertility.

VD: Well, I should tell you that Phil Hauser--I always ask people whom they consider their leading students--had a list, many of whom he shared with Don, but he had a list of just 21 Americans, including Beverly Duncan at the top. Nathan Keyfitz was an afterthought, because Keyfitz came in middle life, just to add a Ph.D. to his already illustrious reputation. You are on that list, and you and Omer Galle are next to each other on that list. Just 21--those who've gone on to outstanding careers afterwards. So Phil considers you a leading student of his too, but you actually did your dissertation with Don Bogue.

I must say that Hauser said that Don Bogue liked to claim that any student who worked with him was his student and his student only. However, Hauser pointed out that you all got your degrees from the department of sociology and you did take courses in the two different centers with the professors who were there.

FARLEY: Yes. Well, Phil was very helpful when I was at the University of Chicago. He was a large presence and probably still is a large presence today. You knew that Phil was around. Phil would say things to you that even as a graduate student you didn't agree with and once in a while you were suspicious about the validity of some of the things he said. Phil could extemporize and exaggerate a bit. But he was a very important person and very supportive. What was unusual was that Phil was, I thought, extremely supportive of graduate students at all levels at a time when he didn't particularly need to be. I mean, he was an important figure in the University of Chicago's system; he was an important figure in national politics. When I was there, he was at one point very interested in becoming commissioner of the schools in the city of Chicago.

VD: He ran for office?

FARLEY: No, that was an appointive office. But he was quite interested in that, before Ben Wallace got the job. So Phil had a public presence that went beyond that of even most of the faculty at the University of Chicago, many of whom, like George Schultz, have been public presences all the time.

But Phil was very supportive of graduate students at all levels.

VDT: But you didn't have as close relations with him as you did with Don Bogue?

FARLEY: That's right.

VDT: Let's talk a bit more about Dudley and Beverly Duncan, who were, of course, enormous presences in U.S. demography. I hope to interview Dudley on my trip to California in April-May [see above]. He's now retired in Santa Barbara and Beverly died about a year ago.

FARLEY: Beverly and Duncan were extremely important for me. When I first got to Chicago, working as an apprentice, I worked more with Beverly than with Dudley. Beverly would lay out interesting things to do and have us do them. She was very supportive of me throughout. It was after I got to the University of Michigan that Beverly said that it was really very appropriate for me to specialize in the demography of American blacks.

VDT: It wasn't really until then, 1967?

FARLEY: I'd been doing it, but I never thought of myself as specialized, as she was suggesting I could be. And I found that very helpful, even after I'd been a professor for three or four years, for her to suggest that was really a good thing to do.

VDT: They were really a very close-knit team in every way; worked beautifully together as well as being married.

FARLEY: Yes.

VDT: Was there an age difference between them, because she was a student a little later than he? A student of Phil.

FARLEY: Her obituary didn't mention her age, but I thought she was about 61 when she died [early 1988]. I can recall her mentioning when she was born and I think it was 1927, 1928 [1929].

VDT: She died prematurely then.

FARLEY: That's right. And Dudley was born in 1918, was it? [1921] Yes, there was an age difference, but it wasn't great.

VDT: And Don Bogue, tell me what he was like as a professor. You said he was such a missionary in classes. In the mid-1960s, for instance, he was making his extraordinary projections of world population at 4.5 billion in 2000--if there was a crash family planning program. But he still was a good professor and obviously you worked well with him.

FARLEY: A very stimulating professor. He taught the demographic methods course at the University of Chicago, which was required of all demography students. He stimulated students to do very good work in that course and a lot of collaboration among students. We used to spend a lot of time trying to figure out exactly what was going on: what were these methods, these techniques. He was very stimulating in that regard.

On the other hand, even when I got to Chicago, he was getting money to more or less establish clinics in Chicago or to facilitate the delivery of birth control information and there was some faculty resistance to that. I don't suppose it was so much the purely mechanical side of it as it was the idea that, "We at the University of Chicago are research investigators. We do not go into the community, particularly we don't go into the community with condoms and encourage people to control their fertility." But Don was doing that already. Then by the time I left Chicago, Don was making a number of claims about the effectiveness of these programs he had initiated in Chicago and I think many of the faculty were skeptical--not only about sophisticated design, but you need a long-run period to show that your intervention program has actually lowered fertility. And I think very many other demographers were skeptical of Don, on the one hand because of his missionary zeal, but also because when Don got a program in place, other people thought Don very shortly thereafter claimed that the program was effective.

VDT: That was brought out particularly . . . The one time there was a book review in Demography was in 1967 [Vol.4, No. 1] and Phil Hauser reviewed Bernard Berelson's [editor] Family Planning and Population Programs [1965], which was the proceedings of a conference in Geneva in 1965, and he lit into Don Bogue's contribution to that and particularly the Chicago experiment, the fertility and low-income population experiment. He said he, Phil Hauser, had irrefutable independent proof that there had not been a drop in Negro fertility in Chicago in the 1960-65 period, which Boage was claiming there had been. He went on to say that people who set up a program should not be the evaluators of that program.

FARLEY: Yes. That kind of strain went on among the faculty at Chicago at that time. Some of it was interesting; it made it a more dynamic place to be. But Don was irrepressible. I've talked to his daughter recently and he is as irrepressible now as he was then.

VDT: I was supposed to interview him last Monday night. He was going to be in Washington one day, but it turned out he did his business by phone. He's very elusive. When I was in Chicago last November, he was in Ethiopia, China, and Mexico, drumming up business for his new Social Development Center. He says we'll get together at PAA in Baltimore in March. I'm taking that with a large grain of salt [but we did]. Where's his daughter?

FARLEY: His daughter is a social work student at the University of Michigan. She worked for a while and I think he would say that she's a kind of part-time dissertation student at the University of Michigan. She also works as a computer programmer. She told me Don has learned Portuguese and is bringing up 85 people from Brazil to learn how to analyze data at his center and then he's going off to Fiji or somewhere. It sounds as though Don is putting in the 80 hours a week that . . .

VDT: Everybody says he's the most prodigious worker that ever was in any field.

FARLEY: As I recollect, Chicago was very, very stimulating, but faculty members there, including some of the demographers, had big egos.

VDT: Including Dudley?

FARLEY: They sometimes insulted each other. You're telling me how Phil Hauser was saying that Don Bogue had no evidence that his program worked. Well, that kind of thing went on with some frequency.

No, Dudley is a very appropriate person; he would never insult someone else. But in Dudley's very careful exposition of his own analysis and work, it would become clear that his work was far superior in care and execution than that of some other people on the faculty. So there **was** that kind of competition.

Don Bogue, by the time I got there, was running the Community and Family Study Center. He used those auspices to get some of his activist programs under way. But that was a kind of demographic setup. It was different from the Population Research and Training Center, which housed Phil Hauser and Dudley Duncan. So there were different centers, sometimes competing a bit for students. Betty Bogue was the supervisor for employees at the Community and Family Study Center. I think she had the same kind of work habits that Don had and she expected other people to have those work habits also.

VDT: Driving themselves?

FARLEY: Yes.

VDT: Did you ever work with Evelyn Kitagawa?

FARLEY: I did work from time to time with Evelyn--less Evelyn than with the Duncans or with Don and Phil Hauser. But she was around at the Population Research and Training Center.

VDT: These professors all seemed to produce the most prodigious amount of work. Did they have graduate students doing a lot of that? For instance, Don Bogue took on the editorship of Demography, in addition to everything else he was doing at that time. The first issue listed a lot of people, his colleagues and graduate students, who helped. You're not there. That was 1964, so you must have been on the verge . . .

FARLEY: I was on the verge of leaving then, yes.

VDT: A lot of these projects had to have been done by graduate students. For instance, Phil Hauser published 32 books, chapters in 50-60 books, and over 500 articles. My husband said he must have written an article a week! A lot of this must have built on the work of graduate students.

FARLEY: Yes, I think graduate students had a role in it. But I think those were very hardworking individuals, who literally did turn out a tremendous amount of work by working many nights, evenings. And, of course, the 500 articles are not 500 distinct articles. Phil did write for a popular press, to some degree. They were very thoughtful articles. For a while after every census was taken--Phil had findings from the 1950 census, findings from the 1960 census, and I think he even went on and did some findings from the 1980 census--and those were kind of overview articles. Phil, of course, had Evelyn Kitagawa to help him with some things and Beverly Duncan helped him with some things. Dudley and Hauser collaborated on a number of books. But they were productive individuals.

VDT: They certainly were. Who were some of your fellow graduate students at that time?

FARLEY: Well, I certainly remember Omer Galle; I was very close to Omer Galle as a graduate student at Chicago. Bill Hodge was there at the time; Bill's teaching out at Southern California now. Paul Siegel, who's at the Census Bureau now, was a graduate student there. A fellow I worked with when I first got there, Robert Fenier--I don't know what happened to him; he was from Montreal.

Judah Matras was there, finishing up graduate studies. There were a wide variety of students. Those in population tended to see each other most frequently.

VDT: And then you went to Duke. What took you to Duke?

FARLEY: I was getting my degree in 1964 and I was interested in becoming a college faculty member and I hoped to go to a place where there would be some interest in population. Hal Winsborough had graduated from Chicago some years earlier and had worked with Dudley Duncan. I think Hal taught for a year maybe at Iowa and one year at Ohio State and then went down to Duke, maybe in 1960, 61. And he had created something of a population program at Duke. Kurt Back was there; Joel Smith was there [and Joseph Spengler]. So there were a few demographers at Duke. Hal was interested in doing more with demography and I was interested. It seemed like a good place to teach, so I went to Duke.

VDT: For three years only. Then the lure of Michigan . . .

FARLEY: Yes, Michigan had a stronger demographic program and Ron Freedman was the person who got in touch with me and eventually convinced me that I should move to the University of Michigan. So I moved up there in the fall of 1967.

VDT: And it must have been a great place to be--the Freedmans, both of them, and the two Duncans.

FARLEY: The two Duncans were there and David Goldberg was quite active at that time. Leslie Kish has always been interested in demographic issues, so it was a great place to be.

VDT: Tell me about Leslie Kish. I must admit that I didn't know much about him and you invited him to introduce you, your PAA presidential address last year ["After the Starting Line--Blacks and Women in an Uphill Race," PAA presidential address, New Orleans, April 22, 1988], and one always chooses the person, it seems, who has been the great influence in one's career to do that.

FARLEY: Well, I don't know that Leslie was a great influence in my career. He wasn't on my dissertation committee or anything of that nature. Leslie is just a perfect European gentleman, who has always been . . .

VDT: European? Where's he from?

FARLEY: He is of Hungarian ancestry; he was born in what's now Romania. Lots of Hungarians lived in what's now Romania and Leslie was born there and came to the United States sometime in the mid- to late 1930s.

Leslie is just a very supportive scholar at the University of Michigan and I've enjoyed having him as a colleague in the 22-23 years I've been at Michigan. There is not a great intellectual connection. Leslie's contribution to our field is in survey sampling; he's trained probably more survey samplers than anybody else. I haven't had a course in survey sampling. I haven't designed a sample. So it's not such a close intellectual feeling with Leslie. It's rather that I thought he was a very appropriate person to do that--introduce me.

VDT: And Ron Freedman? You never really worked with Ron, I presume, because his interest has been in Third World demography.

FARLEY: Yes. Ron and I have not worked on a paper or something like that, because our interests have been in different directions. Ron has been my colleague for 24 years and certainly we talk all the time and so forth, but we haven't written anything together.

VDT: Well, who have been the leading influences in your career? You have pretty well covered them already, presumably.

FARLEY: Yes, I have. Donald Barrett is the man who in some ways got me interested in population, but there was not a great deal of research going on where I was an undergraduate [Notre Dame]. At the University of Chicago, I think the stimulating influences were Beverly Duncan, Dudley Duncan, Don Bogue, Phil Hauser. Those are the individuals whom I remember most as people who helped me on the way.

Nathan Keyfitz came to teach at Chicago in my last year. I took one course from Nathan. Had he been there earlier, I think I would have liked to see if I could have done the kinds of work that Nathan has stimulated, namely, mathematical modeling. I found that very fascinating and interesting. But by the time I took a course with Nathan, I already had my dissertation under way. I knew there were many students who stuck around Chicago for ages and I didn't want to do that. I wanted to get out and not be a permanent graduate student. I remember talking with Nathan and Nathan saying that if I were interested in that kind of demography, he would encourage me to stay around. But I think it was a question of time. I was conscious of the fact that developing skills to become a mathematical demographer required at least a year or maybe two more years of graduate school and I found what I was doing very stimulating, so I didn't follow up on Nathan's suggestion, although he was certainly very helpful at that time.

VDT: You certainly got your Ph.D. very promptly, in those four years from the time you went to Chicago.

FARLEY: I think that came from working with the Duncans. The Duncans had an image of projects with a deadline and I think that's a very good model to have. There were other faculty members who didn't see deadlines and who, as the project went on, would find new data, new analytic techniques, and the project would expand. The Duncans on the other hand would say, "In the next 18 months we want to do such and so and we'll do it." And they were pretty faithful about meeting those deadlines.

VDT: Then their students were expected to write up their part in the projects; get it done?

FARLEY: Right.

VDT: Chicago didn't have the sort of implicit deadlines that Princeton had? I've heard that Princeton, the OPR [Office of Population Research], expected people to do their PhDs in three years from the B.A. I don't think that many people have done it, and in recent years, no. But Sam Preston did, and there's the phenomenon, Alvaro Lopez, from Latin America [Colombia], who did it in about 18 months.

FARLEY: Right. Lopez's dissertation was not a data dissertation. It was a modeling of . . . was it fertility and reproductive growth? [Problems in Stable Population Theory, 1961, an extension of Lotka's work.]

VDT: That's right. Well, sometimes models take much longer. I've got a son doing a modeling exercise for his economics dissertation for Stanford and it seems to be taking about ten years.

FARLEY: There were a lot of graduate students at Chicago who had spent five or ten years at graduate school. I didn't want to become like that.

VDT: At Chicago, even at that time?

FARLEY: Even at that time. Chicago didn't have a lot of undergraduate teaching opportunities; you couldn't be a teaching assistant for that long, in most cases. So these people would scrape together a living in certain kinds of ways and that looked pretty unattractive to me. I knew I was going to have had enough of Chicago at the end of four years, so I'm glad I finished up.

VDT: Good for you. Let's look at the other side of the picture. Who have been some of your leading students in the 25 years, now, that you've been teaching? Some of the students you're proudest of.

FARLEY: At Michigan we have a fairly large faculty in the sociology department and a fairly large faculty at the Population Studies Center, so maybe there's a little bit less direct mentoring or sponsorship that there was at the University of Chicago. By that I mean we certainly encourage students to work with a large variety of faculty at the Pop Studies Center. For some obvious reasons, in recent years a large number of students have chosen Ron Freedman or Al Hermalin as their dissertation directors.

Then we have students who have worked with faculty. Suzanne Bianchi comes to mind; she's now our secretary-treasurer of the Population Association. She worked as a teaching fellow in the Detroit Area Study, that project back in 1976. [Investigating racial residential segregation in Detroit. Farley was principal investigator of the project. See Suzanne Bianchi's interview, above.] Her dissertation was about changing household composition in the United States and its implication for poverty and women and children.

Diane Colisanto worked with that project, who is now at Auburn. She worked with me and I supervised her dissertation. Barry Edmonston worked on a dissertation having to do with metropolitan distribution, suburbanization of people and industries. Barry taught for a while at Cornell and will be going to the Urban Institute; he worked with me. Paul Voss, I supervised his dissertation. He's been very successful at the University of Wisconsin; he's a state and local demographer.

VDT: Now on the PAA Board, one of the first business demographers.

FARLEY: Business or applied demographers, on that board. One of the more recent students whose dissertation I supervised is Robert Wilger, who has moved over to Wisconsin, working in Paul Voss's shop.

A woman by the name of Patty Gwartney-Gibbs worked with me on how women and men get slotted into jobs--the different kinds of occupational achievements that women and men make. She's now teaching at the University of Oregon, so I was pleased to work with her as a student. Shirley Paget, who's working at the Institute for Social Research at the University of Michigan now, did her dissertation on racial attitudes in the Detroit area, using partially the Detroit Area Study data and other data gathered by the University of Michigan. I was pleased to be on her dissertation committee.

So there's a variety of students I've worked with over the years. That's one of the advantages of being at a place like Michigan. We now have a particularly talented group of students. It's a good opportunity for faculty members; it's a good opportunity for students too.

VDT: Indeed. It's a great place, and so is Chicago. You have been at the two top centers, just about.

BREAK

FARLEY: Jean, are you going to write this up?

VDT: What I plan to do--did you ever see the "proposal" I submitted to the PAA Board? I've now done 20 interviews and one from Nathan Keyfitz is on the way. He's done his own taped interview from a schedule I sent to him in Indonesia. I hope to do 35 or 37 interviews [actually did 41]. Andy Lunde in the 1970s did about a dozen. Some of those tapes are mechanically defective. For instance, the one with Henry Shryock goes dead after ten minutes. I've redone one with him. The one with Ron Freedman was pretty good but I'm going to do another one with Ron too. Similarly, I've done another one with Ansley Coale; he did one in the 1970s.

So I hope to interview all past presidents who are interviewable and reachable. I've eliminated two, Joseph Spengler and Calvin Schmid. Joe Spengler is still alive but has Alzheimer's disease [died in late 1990]. He was interviewed. Harry Rosenberg interviewed three people at once--Clyde Kiser, Joe Spengler, and Horace Hamilton. That didn't work out very well, but at least we have their voices on tape. Another one was done with Clyde Kiser and he's excellent. I won't repeat that one. I won't repeat one with Con Taeuber [but see "constructed interview" above].

FARLEY: Con is all right? I saw Con recently.

VDT: He always turns up at meetings, but he has Parkinson's disease. He's bright and alert. He's always sending me stuff for the archives.

FARLEY: I don't know about Clyde Kiser.

VDT: Clyde Kiser is fine, but there was an excellent interview with him in 1973.

FARLEY: How old is Clyde?

VDT: He's in his eighties [born 1904]. There was an excellent videotape interview with him in that series [done in 1985] on directors of national fertility surveys, instituted by Barbara Wilson and William Pratt, so I feel he's pretty well recorded. I went to Chapel Hill for interviewing, but Bessemer City where he lives is 200 miles away. Bessemer City is where he grew up; he went back to his native land when he retired from the Milbank Fund.

FARLEY: Is his wife all right?

VDT: I don't know [Louise Kiser, coeditor of Population Index, died in 1954], but Clyde was in fine shape three and a half years ago when he came for his interview for the fertility survey directors. I got pictures of him. There was a photo of him and Barbara Wilson in PAA Affairs from that time, when there was a little party for him at Barbara Wilson's. [See Barbara Foley Wilson, "Videotaped Interviews about American Fertility Surveys," Vignettes of PAA/U.S. Fertility History, PAA Affairs, Winter 1985.]

One other past president who is still alive whom I won't try to get to is Calvin Schmid. He's 89, lives on an island off the coast of Washington state [later in a nursing home on the mainland]. Henry Shryock visited him a few years ago. I gather he's really very alert [at time of Shryock's visit], but he's

a bit far removed.

And I've been trying to do all the secretary-treasurers too and one of those, Kurt Mayer, went back to Switzerland in the mid-1960s. I'm debating whether I'll write him and ask if he would do a tape as Nathan Keyfitz has done, whom, of course, we had to have in this series. [See above Mayer's contribution to the series, written in April 1990.]

I still have a group of people out in California and Arizona. I hope to get seven in one fell swoop in April-May. I will interview the Freedmans in Ann Arbor in June, en route to Canada, and there are a few other outliers, like Sid Goldstein in Providence [interviewed December 1990, also Alice Goldstein].

Then I will have all the tapes and transcripts and, of course, they are available to scholars to study. But I think they're not very accessible when buried in our archives at the Georgetown library--and our archives at the moment are sitting behind the papers of Harry Hopkins, so I'm not looking forward to going back through them. And I'm doing photographs; I will do a photo of you before you go. I hope to put out a very personal book, with excerpts from these interviews, along with the vignettes of PAA history we've had since 1981 in PAA Affairs and these lists that Andy did of the meetings, where they've been and how many were there, the officers and Board members and memberships totals from the beginning. And I'd like to get permission from Population and Development Review to reprint that last article of Frank Notestein's of December 1982 ["Demography in the United States: A Partial Account of the Development of the Field"], which is a pretty good history of U.S. demography up to the 1970s. Maybe some other bits and pieces [e.g., papers on PAA's beginnings given in the "PAA at Age 50" session at the 1981 50th anniversary meeting]. This is not by any means intended to be a formal history of U.S. demography or PAA, more a collection of materials, but I think it should be of interest. Interesting and fascinating [which is why VDT later decided to assemble a collection of the full, edited interview transcripts, the full flavor of which would be lost in mere excerpting for the "Selective History" described here]. Now I'm talking too much.

FARLEY: That could lead to a very complete history of U.S. demography. I'm thinking of Jean Convert's book on the history of survey research in the United States, University of California Press; unfortunately, it sells for something like \$59, so it's not easily purchased. It's very important for important for social scientists, survey research. She takes it up to 1960, very, very competently done, looking at it in somewhat the same perspective you are, namely, personalities, and how things get from one institution to another. Not so much through looking at the journals, but how did people contact and what kinds of feeder funds became available. If funds were not available to support research, why did a place like the social science group at Columbia have such a predominant lead in survey research around the time of World War II but then lose it after World War II? What were the intellectual traces? She lays that out very, very well, and I would like very much to read the same thing for demography, and you are in the position to do that.

VDT: Well, I agree, except I'm not going into such detail as that. For instance, I'm not probing for where the funding came from. Part of that is covered by the Caldwells' book on the Ford Foundation contribution [Limiting Population Growth and the Ford Foundation Contribution, 1986]. Of course, that was for university centers that dealt with Third World population, not for the others or programs on the U.S. side. I don't have such high pretensions.

Andy started the series of interviews to get a hold on PAA history because he, as secretary-treasurer in the late 1960s, found there was nothing on record. Of course, he had that excellent session at the 50th anniversary meeting in Washington in 1981, which covered very well the early history of PAA [papers by Lunde, Frank Notestein, Frank Lorimer, and Clyde Kiser]. This later history will have a bit on the later history of PAA; it's not all that exciting. But I think personalities are interesting. And

demography is still a small discipline, relatively, and all so interconnected. Those who were students of Phil Hauser, Keyfitz, for instance--Kingsley Davis comes up time and again--knew each other as students. And, of course, the select sample of those who've risen to the top in PAA obviously would be intertwined. But I hope this will be a contribution to someone who will do the definitive history of demography in the U.S.

Interestingly, a friend of mine who graduated from the University of Toronto two years before I did, Sylvia Wargon, who is at Statistics Canada, has just written to tell me that she has been officially detailed to write a history of the development of demography in Canada, so they have thought that that would be important there too. It's too bad someone at the Census Bureau doesn't sit someone in an office to do it for the U.S. It's a much bigger story.

FARLEY: It's a much bigger story. It strikes me it has to be done by someone who has got a lot of experience. It's harder for a younger person to do, probably, although you might find some historian who might do it, if someone wants to write a dissertation in history.

VDT: Well, for someone as a final project just before retirement--that's the way with Sylvia--it would be ideal. I had thought of this as a perfect retirement project. My husband forced me to retire early [to travel widely]; I loved my job at PRB [as editor of the Population Bulletin]. But this I knew I wanted to do [complete the interview series]. Andy asked me to carry on as PAA historian in 1981. I think Con Taeuber put him on to it, knew I had a historical interest; my undergraduate major was history. And I was in Washington; they needed someone to be close to the archives, which Andy set up, with the help of Con Taeuber and Tom Merrick, who was at Georgetown then.

FARLEY: Well, if you ever have any doubts about it, I think a 200-, 250-page history of demography in the United States would be a very important intellectual enterprise.

VDT: Well, it is. Ralph Thomlinson once said he was interested in that--I don't know why that should be--but he was going to have to get funding and be put someplace to do it.

FARLEY: It's very hard to get funding for that. As you say, this is a pre-retirement project. You need a time span to do it. A person who would not surprise me if he attempted it would be Bill Petersen.

VDT: True enough. He's enormously prolific and, of course, he's had the experience. His Biographies of Demographers [with Renee Petersen] are invaluable; it gives a sketchy background of each of you, at least to the early 1980s. And he did the Dictionary of Demography [1985, in two volumes, of which Biographies is Volume 2]. I also think he'd be a good one to do it. Maybe I should go see him when I'm out in California, just talk about this in general [did not]. I presume he knows what I'm doing.

FARLEY: He's a Dutchman, isn't he?

VDT: He might have been. I remember reading an article of his about Dutch fertility; he knew more about it than most people.

FARLEY: I thought he'd spent a couple of years in the Netherlands.

VDT: I spent a year in the Netherlands just after I was out of Georgetown and PRB approached me: Would I write an article on why Dutch fertility remained stubbornly high? And, you know, the year I

was there [1970-71], it collapsed!

FARLEY: I was going to say, it's not stubbornly high now!

VDT: I was the first staff member of Dirk van de Kaa's institute, the Netherlands Interuniversity Demographic Institute. I sat in an office all by myself and set it up. Then Dirk van de Kaa came; we overlapped for three months.

FARLEY: Italy's got about the lowest fertility rate in Western Europe now, except for Denmark.

VDT: Italy has the lowest [1.3] at this point. But the Dutch fertility plunged down in 1970-71. What happened was that suddenly abortion was available; it was not really legal but people could get it easily. And the pill suddenly became available too. And fertility just collapsed. So my story was pulled out from under me.

Actually, I collected some data on their internal migration trends. They were making an effort to deconcentrate the Randstad, as they call it--the heavy concentration in the west of the country--and some government offices were being moved out. PRB, though I didn't work for them then, asked me to collect data and I went to see Henk Heeren and ter Heide, who were working on that. I gave PRB these data and they sat on them. Years later, Gordon De Jong wanted to do some work on population redistribution policies in the Netherlands and he heard from the Dutch that Jean van der Tak had collected these data, and I got them from PRB for him. Well, that's me. I want to get back to you.

FARLEY: One other question. Are you interviewing the living editors of Demography?

VDT: No, I'm not; perhaps I should. Well, I think the Demography story ended with Don Bogue [first editor, 1964-68]. You know the famous story of his thick issues, ending up with the family planning issue ["Progress and Problems of Fertility Control Around the World," Volume 5, No. 2, 1968], with the inverted red Indian family planning triangle on the front and the slogan, "Stop at two or three," which caused such a flap. Then, of course, Demography collapsed into the skinny little thing it became under Beverly Duncan and has not changed very much since.

FARLEY: No, it hasn't changed too much. I always thought Don was the one who started Demography.

VDT: He did. Well, Norman Ryder had the idea.

FARLEY: Norm Ryder takes a lot of credit. I wouldn't want to get those two people in the same room, but I think Don's version and Norm's version don't overlap very much.

VDT: That's interesting; tell me.

FARLEY: My impression is that Norm thinks he started Demography.

VDT: He had the idea.

FARLEY: I may be wrong, but I think Norm believes he did much more than the idea. He was asked to be the first editor and was prepared to do so and something happened that was quite believable: Norm had some falling out and it looked as though there would be no Demography.

VDT: Falling out with whom?

FARLEY: I don't know with whom. It must have been with whoever was responsible in the PAA for initiating Demography. Only after Norm turned them down, whoever this committee was, crawling on their hands and knees, came to Don Bogue and Don . . .

VDT: Wow! That's not Don's version.

FARLEY: That was not the version I've heard from Don; that was not the University of Chicago version. But I get the impression . . .

VDT: I interviewed Norm and he said he had the idea of Demography but Wisconsin didn't have the money to support it, nor did Michigan; he went to Michigan. Then finally Don said he could get the money, through the Community and Family Study Center. Don wrote a vignette on the start of Demography in PAA Affairs ["How Demography was Born," PAA Affairs, Fall 1983]. The Ford Foundation was supporting his center and through them he had the secretarial help and he corralled all his students and colleagues to help. He didn't mention the National Science Foundation. After that article appeared, Paul Glick wrote and said, "He happens not to have mentioned that the National Science Foundation came in with \$30,000 for the next three years" [after the first edition of Demography].

FARLEY: That was a substantial grant then.

VDT: Indeed. Glick [PAA president, 1966-67] and Calvin Schmid, president before him, got the money. Then the family planning issue had cost so much money that Don . . . Don felt his thick issues were justified because he'd sell them to libraries, and indeed he did. In the end, the Community and Family Study Center absorbed all the costs of that 1968 family planning issue. But Ansley Coale said--he was president then--he got these irate phone calls from people saying, "What are we doing getting into the family planning business?" But it was a tremendous issue, invaluable. And that first volume, in 1964, that was a bible of demography.

What else is there in the history of Demography, that became sober, scholarly, skinny?

FARLEY: It became thin, and Population and Development Review took on . . . It's a different thing; the articles are not necessarily reviewed in the same fashion as Demography is. There's the very formal review of articles before they appear in Demography, but Paul Demeny, I think, has quite a degree of editorial freedom to publish or not publish what he wishes in PDR [but they go first to reviewers]. I guess many people look upon that as a more dynamic, lively journal than Demography.

VDT: For instance, Paul Demeny pointed out in his interview that he has his book review section, which is often controversial. And Demography, why did they never have a book review section? There was that one book review by Phil Hauser, in 1967, and as I mentioned he criticized Bogue heavily. I surmise that perhaps that's why no other Demography editor ever dared have a book review section. [A book review section appeared in the May 1991 issue of Demography, edited by Avery Guest as of 1991, announced as the first of a three-issue series of reviews of the 1980 census monographs.]

Paul Demeny says that the tradition in other disciplines, in economics, is that when a book comes out it's criticized. But demographers, he says, are very sensitive, know each other too well, perhaps, and seem to take criticism as a personal affront. Now what do you think of that?

FARLEY: I think the reason there's no book review section in Demography is a financial one.

Namely, every editor I've talked to or whom I've heard about when I've been on the Board or one of the officers of the Association, all those editors say, "We have many more good manuscripts than we can publish now." So the editors have been reluctant to sacrifice what they see as good manuscripts to devote six, eight, ten pages to book reviews. I think that's the reason. Nobody ever said, "We're going to add 50 pages to Demography next year so you can have book reviews." I think that's the primary reason.

VDT: It might be.

FARLEY: But the other reason you're suggesting is an interesting one. It [demographers] is a small group of people. There are some people--you've mentioned them already--who would write pretty critical reviews, and I think Kingsley Davis would be another name to add to that list. But there are very many other people who work so closely, collaborate so much, know their research grants are going to be evaluated by other demographers, that there would be implicitly, I think, perhaps some inclination to tone down otherwise hostile reviews.

VDT: I gathered that with Paul Demeny too. He said also he finds it difficult to find people who will agree to write reviews. He says people see no political future in it; in other words, you don't get that much credit for it, in your resume.

FARLEY: If it's a 300- or 400-page book, you might ordinarily scan it for your own purposes or read a chapter or two. But if it's to be a review, you've got to read every page of that book and think about it. So it's a lot of hard work sometimes.

VDT: The bulk of reviews can't make much difference in your own bibliography. They certainly do, I guess, in The Economist, which will take off on some book.

FARLEY: I don't think they do among sociologists. I don't think that when you look at a person's vitae, you pay much attention to what books they've reviewed. Certainly, one strategy would be for an editor of Demography to have a review symposium where a person instead of just reviewing one book could review or discuss the merits of two or three books. But that's even more work for the reviewer.

VDT: Or the other way round: two or three people discussing one book, which has happened often in PDR, say, with the Julian Simon book [review symposium on Julian Simon, The Ultimate Resource, reviewed by Peter Timmer, Ismail Sirageldin and John Kantner, and Samuel Preston, PDR, March 1982].

FARLEY: Among other things I wanted to initiate as PAA president last year was to get some serious discussion of books in our field. So I added those two "Authors-meet-critics" sessions.

VDT: I was going to ask you about that; that was very noticeable.

FARLEY: The Teitelbaum book [Michael Teitelbaum and Jay Winter, The Fear of Population Decline] and the other book about women, the 1980 census monograph [Suzanne Bianchi and Daphne Spain, American Women in Transition]. I don't know if those were successful sessions or not, but I wanted to stimulate . . .

VDT: Now we're having them this year [1989 PAA meeting]; They're on the program I got yesterday.

FARLEY: In certain cases, with people who're particularly controversial like Julian Simon, you could pair his book against a book presenting a very different view and use that as a theme for a stimulating session. If you can do it for the meetings, I don't know why you can't do it for Demography or something else.

VDT: Indeed. Well, I had that on my list for when we get to PAA. Now I want to talk a bit about your work in black demography. I'd like to ask a straightforward question that has always puzzled me: Why are there so few black demographers and so few blacks in PAA?

FARLEY: There are very few blacks. Last year I tried very hard to see that there was a representation of blacks on the program.

VDT: You did try last year? Was it last year that Robert Hill was there? He's a sociologist.

FARLEY: No, I don't think so. He may have been there two years ago. Last year we did have Don Deskins, Walter Allen . . .

VDT: He is a black, your coauthor? [on 1980 census monograph, The Color Line and the Quality of Life in America, 1987].

FARLEY: Yes. Why are there so few blacks in demography? Phew! Give me a speculation.

VDT: Well, I've heard, of course, that there was a concerted effort at the University of Chicago to bring in black sociologists, from Atlanta University and so on. I knew one, John Reid, who . . .

FARLEY: John Reid eventually ended up in Washington, didn't he?

VDT: Yes, he did, and he was author of one of my PRB Population Bulletins, on blacks ["Black America in the 1980s," December 1982].

FARLEY: With Bill O'Hare, I guess?

VDT: No, Lee Bouvier. Actually, Lee wrote a lot of it and so did I, but John Reid's name alone was on it, and he did a good job with the media coverage that came afterward. But as I understand it, they did make an effort to bring blacks from Atlanta University, for instance, out of the South to Chicago.

FARLEY: Yes.

VDT: And then someone has said, there was no money in demography. Blacks who were that highly trained could go into fields where they were more recognized or highly paid or whatever. Maybe that's wrong. There are more black sociologists, for instance, Julius Wilson.

FARLEY: That's true. There are more black sociologists, but there aren't very many blacks in any of the academic disciplines. Education and social work probably have the greatest representation. Economics, being a very large field, there are numerically more blacks there, but there aren't very many. There certainly aren't very many blacks in demography. I know at Michigan, certainly at Chicago, we tried to recruit blacks students from the United States. We have a moderately high failure rate from white students--people who don't just necessarily fail out but they don't complete their degrees. But we've been not so successful with black demographers, at Michigan or elsewhere. I was

co-chair on Tom Viest's dissertation committee. He is on a post-doc now. He was interested in black-white differences in infant mortality and did some good work on that topic. But I don't know if he's going to go into demography or something having to do with public health.

There are a few black demographers, but not many. Larry Carter at the University of Oregon had some interest in demography for a while. Claudette Bennett has a degree from Howard University, out at the Census Bureau; I would include her in that group. But it is a small pool of individuals.

VDT: Did you in a sense see an opportunity there? Of course, you said that the Duncans encouraged your seriously considering concentrating on black demography. Was there a lacuna?

FARLEY: Well, the Duncans certainly tried to recruit blacks into the field. One of my colleagues was Nate Hare, who got his degree in sociology and taught for a while on the West coast in the Berkeley area. But after teaching sociology with demography for a number of years, he decided to go back and get a degree in clinical psychology. I believe he got a second Ph.D. and, so far as I know, he's a clinical psychologist now.

So, yes, there are very few black demographers. Leroy Stone is one who comes to mind, a Jamaican and a Canadian.

VDT: Oh, he's originally Jamaican? I've just read the section in your Color Line that points out that the thesis that West Indians do much better is all balderdash!

FARLEY: Dubious, yes.

VDT: I've just finished the Color Line--fascinating, marvelously done book--but so somber. There is just no progress.

FARLEY: Yes.

VDT: And that's been true of the reviews of the book. I was so proud to see it reviewed on the front page of the Washington Post book review section a year ago. And also delighted to know it was out; you got it out so fast, in 1987, one of the first of the 1980 census monographs to appear. And the review of Blacks and Whites: Narrowing the Gap? [1984] in PDR [March 1986]. They all end up saying you are so somber; the outlook for blacks is depressing. Is that the way you feel?

You may be even more depressed now, arriving in the District of Columbia, where we've had a record number of homicides in this past year and in the past month, January 1989, the highest in any month ever--and mostly blacks, if not all. You had predicted that in your 1980 article ["Homicides in the United States," Demography, May 1980], where you pointed out that homicide rates for blacks were up and that the increasing availability of guns was going to make the situation worse. And that's absolutely true. We're a living example of it here in the District of Columbia.

FARLEY: Yes, it is. The situation is not a good one. There certainly is some growth of the black middle class, but the racial gap in the United States is very large. My own impression is that there is a reluctance to talk about this. It's a reluctance, perhaps, on the part of many whites. It's a sensitive topic. If you're a white politician, there's not a great deal to be gained from talking about the racial gap, the history of racial hostility. The blacks for the most part do not have a tremendous amount of social or economic power. A small segment of them may, but the resources for change in the black community are not great.

I don't know whether one should be pessimistic or optimistic. Certainly, there are some signs

of blacks moving into important positions. And I would speculate that if a black leader has a certain degree of talents or a certain array of signs, namely, college degrees, they can enter into a middle class, where they will face, I presume, no more than modest and maybe very little racial discrimination. But it seems to be a small proportion--20, 25, 30 percent--of the black population moving into that middle class.

We assumed on the basis of what was happening from the late 1950s through the mid-1970s that there would be a rapid expansion of black middle class population, that poverty rates would go down gradually or rapidly. But since 1974 on almost all economic indicators, there's been no contraction between blacks and whites, with the exception of black women who in terms of occupations and earnings are doing quite well compared to white women.

VDT: Well, that is a depressing outlook. And you talk about the few blacks in positions of prominence and you're in a city where unfortunately our black mayor [Marion Barry] is not doing very well [and this was before Barry was charged with drug use].

FARLEY: That's right. People have pointed out that as blacks became predominant in cities, they certainly would elect mayors. But very often they are cities with immense financial problems. Cleveland, Gary, Detroit, the first cities to have black mayors, are not the financial bulwarks they were 30 years ago. Industry has moved to the suburbs; the middle class population, black and white, has moved to the suburbs. The financial encumbrances in the cities are much greater than their abilities.

VDT: That's happened in D.C. too.

FARLEY: Yes. D.C. is really different in many ways from all other cities. In some ways, it's becoming a city of the rich and the poor.

VDT: Yes, it's a frightening city--the divide between the whites and the blacks and also there's an extreme divide between the rich and the poor. Well, it must be an interesting city for you to be in for six months!

FARLEY: Washington is always an interesting city to be in. Washington has a larger array of what used to be called "stable" neighborhoods. I don't mean necessarily whites. There are stable black middle class neighborhoods in Washington--out by Walter Reed Hospital, for example. Washington is a city of neighborhoods. You can find a few of these in Detroit, but proportionally they're less numerous in Detroit than they are in the city of Washington itself [District of Columbia]. There may be hope for Washington in some regards, if the drug problems are ever solved. No one seems to have a solution for that.

VDT: No. Well, that's an inexhaustible topic. But your books are excellent; I want to talk about them now. They've been praised for your "careful presentation of both sides of the argument." For instance, you say higher black fertility can be due either to the minority group hypothesis--minority groups want to produce more. I'm interested in that because one of my first papers was on French Canadian versus Anglo Canadian fertility and that was a favorite hypothesis at that time, that French Canadians produced because they wanted to keep up. And the same in the Netherlands, between the Catholics and the Calvinists.

FARLEY: Jean, the solution for high fertility is to get you to study it! You studied the Dutch right before their fertility went down; you studied the French Canadians and now the French Canadians have

very low fertility.

VDT: Absolutely. You looked at the minority group hypothesis versus the social characteristics hypothesis--that there's something about certain groups that want to have high fertility. You carefully did, indeed, present both those arguments. I'll ask you straightforward: Do you feel as a white demographer studying black demography that you have to be exceptionally careful about looking at all sides of each issue--indicating the data, which you do exhaustively?

FARLEY: I think the answer is yes. But if a black demographer were studying black fertility, I think he or she ought to be equally careful. I mean, in the social sciences there's such a strong tendency to look for one causal factor that I think all of us have to be very suspicious of that, whether we're explaining why people vote for one candidate or why women are now working in greater numbers than they used to. There is such a tendency to say: "This is the cause." In particular, if you're going to write a 500-word essay for the newspaper, you want something in the popular press, you can't go through all the various perspectives which you really ought to go through when you're looking at something like this. And when you come to the basic question that I've been trying to address for the last few years, namely, how the civil rights revolution occurred but left a very large gap between blacks and whites, particularly in economic status. There you get very many single-factor explanations. People saying: Well, it's because manufacturing jobs are no longer available to blacks, or it's because there's something deficient about black culture, or black families are unstable and that's the real reason, or white racism is still as persistent as it used to be. All of those single-factor explanations are unsettling when you read them, but people defend them.

And in this town [Washington], everyone thinks a little bit about policy and if it's policy, it's quite often going to be targeted toward one factor, and that may not be the real factor or the only factor or it may be a vast oversimplification to say that one factor . . .

VDT: Like Charles Murray on welfare.

FARLEY: Sure. Murray says blacks are taking advantage of welfare because welfare gives them as much income as work.

VDT: So now they say those on welfare should work.

FARLEY: That idea permeated the Reagan administration, at least for the first couple of years, with George Gilder, Murray, and goodness knows how many other people operating as if the availability of welfare was the only problem for the black community.

VDT: So you see a need to temper that?

FARLEY: Yes. I don't think it's particularly because I'm a white person studying blacks. I think if I were doing something else I would hope I would be just as . . .

VDT: A careful scientist.

FARLEY: Yes.

VDT: That also shows in your referencing of your books, which is exhaustive. You must have read everything ever written on blacks and the history of population in the U.S. I learnt a lot from your

books.

And you have been praised for having lovely, clear tables and graphs, and you obviously make an effort to have them appear close to the place in the text where they're being discussed. Do you make a conscious effort?

FARLEY: Yes, sure. I've been helped a lot in that book [Color Line] and in the Blacks and Whites book. There was a woman at the Population Studies Center who was our editor, Cathy Duke, and she was just extraordinarily helpful in cleaning up our prose and making sure the tables and charts were legible to her as well as to everybody else. She played an important role in doing that.

VDT: And you got out your 1980 census monograph, The Color Line, pretty rapidly. The 1980 census monographs are carrying on the splendid tradition of the 1950 and 1960 monographs, but they've been slow to appear. Suzanne's and Daphne Spain's [American Women in Tradition] was the first [1986]--and yours was second?

FARLEY: I think ours was third. The Politics of Numbers volume by William Alonso and Paul Starr came out maybe two or three months before ours [in 1987].

VDT: But you made a tremendous effort, obviously, and got it out.

FARLEY: Yes. I don't see any reason for putting census monographs off to the end of the decade.

VDT: Indeed, it seems very frustrating, although you had the post-1980 data. And you've been on the advisory committee for the monographs. Have you been working with some others on an actual monograph?

FARLEY: The advisory committee worked to select authors and give a bit of the reading of the manuscripts as they came in, but Charlie Westoff [committee chair] did most of that. That committee has not met since the Pittsburgh meetings of the PAA [1983].

VDT: Tell me about your work with the Census Bureau advisory committee [on population statistics, 1975-81]. For instance, you were on the committee around 1980; were you involved in the problems of worrying about the undercount?

FARLEY: Yes, we were somewhat concerned at that time. But the outside advisory committees, I would say, play an advisory role which involves more suggesting and reacting to Census Bureau proposals. The advisory committees are not given carte blanche to come in with new suggestions. The Census Bureau doesn't say: "We've got some troubles with undercount; please come up with a plan to solve the undercount problem." Rather, the Census advisory committees, it seems to me, are presented quite often with work in progress at the Census Bureau and suggestions are sought. It is not the kind of technical board that bureaucracy might go to and say: "We've got to do something. We don't know what to do. Please tell us what to do."

VDT: What were some of the issues at the time you were on the committee?

FARLEY: One set of issues had to do with what would be tabulated from the 1980 census and how things would be published--those kinds of fairly mundane but important issues. Of course, there was concern about the undercount as the 1980 census came out. We were presented with a tremendous

amount of information about the publicity campaign for the 1980 census. But, again, the advisory committee might make a few suggestions, but for the most part they were not advertising or promotion experts.

I think one of the more important ways that the advisory committee actually influences policy at the Census Bureau is that one or more members of the committee who are experts in an area will be alerted on some issue that is arising at the Census Bureau and those people will call the appropriate person at the Census Bureau with suggestions or reactions. So the advisory committee acts to stimulate this sort of interchange, but that is not always done formally. Sometimes calling up and saying: "Look, you have four ways to tabulate or you have six different measures you can give us on fertility, this is the one you really ought to give us." That kind of informal exchange.

VDT: Have you done that?

FARLEY: Oh, sure.

VDT: On what, for instance?

FARLEY: There was some of the informal work on retaining questions for the census of 1980. And on the 1970 census, I remember working with Wilson Grabill at one point . . .

VDT: He was still working then?

FARLEY: Yes, he worked through the 1970 census. I recall suggesting and encouraging him to put in some measures of variance of fertility, which I think he did put in one of the 1970 reports.

VDT: I guess you corresponded with him. He's the famous deaf person.

FARLEY: Yes, a Gallaudet graduate.

VDT: And have you had much to do with encouraging more work on the undercount? That always seems such an issue with blacks, particularly young black males.

FARLEY: I don't think the Census Bureau needs any encouragement. My impression is that they devote a great deal of resources to it. They approach it statistically, statistical modeling, and of course they went through the furor which involved Barbara Bailer of a possible resampling of the population and the actual adjustment of the census counts. At the same time, they now have a group working with ethnographers to describe who is it that's not going to be counted in the 1990 census. There are a tremendous array of efforts going on at the Census Bureau. I don't see why they are so criticized, because we are not a police state; you don't have to fill out the census form under the guidance of a policeman or something of that nature. There are a lot of people just being slovenly, being indifferent, or something else will lead them to not fill out the census forms. I don't know how you push those people to fill out the form. Plus the other real problems of: Are there housing units which are missed or people in households who are missed? The Census Bureau says it does a tremendous amount of research on this.

So I don't think they need any more prodding. I'm sure they would appreciate some secret way to make sure everybody is counted, but we don't have that.

VDT: What are you doing now, in these six months at the Census Bureau?

FARLEY: That period has to do with evaluating the quality of ancestry data in the United States. The 1980 and 1990 censuses have a question asking everybody their race and another asking if they are of Spanish origin or not, which really screens out the 7 percent of the population or so who are Spanish. Then a sample of the population in both 1980 and 1990 are asked a question: What is your ancestry or ethnicity? And people can write in as many as they want. The Census Bureau typically codes their first two ancestries.

The Census Bureau has not done a great deal to evaluate the quality of those data. For instance, are people consistent? If you ask them their ancestry at one point and come back six months later are they consistent? About 12 percent of the population refuses or does not answer the ancestry question. Is that because they don't know their ancestry? There's another 6 or 7 percent who write down "American," and there's a great debate among scholars . . .

VDT: What do you think about "African American," the latest way to call blacks?

FARLEY: Well, race is the race question, but blacks will also be asked the question about ancestry. I've proposed since I've been there that there be a separate code for African American. Those who write down African American or black or colored or Negro--that they all be given a separate code.

VDT: In the ancestry question? Some people will write black or Negro in the ancestry question, having already had the race question?

FARLEY: A few people answer white by race, but will recognize that they have some black ancestry. That's extremely rare.

VDT: The answer to the question is wide open; there are not eight categories or something?

FARLEY: Wide open. In the Census Population Survey, they show you a flash card with 30 groups and you have to pick one of 30. I think Number 30 is "other." But for ancestry in the census, they will have 600 and some codes for what people write in.

VDT: What did they write in 1980, for instance?

FARLEY: The most common response in 1980 was English and about 24-25 percent of people write English. In the 1980 census, about 49 million people said they were English; 49 million said they were German; about 49 million said they were Irish.

VDT: You could write in as many as you liked?

FARLEY: Right. They coded the first two only.

VDT: What is your ancestry? Are you Irish?

FARLEY: I would say Irish to that question. But if someone pushed, I'd say, "Yeah, there's some German ancestry, also maybe some French ancestry." So that's the problem you get into.

Frankly, I think that for people who are in racial minorities and people who are first or second generation immigrations, you get something on the order of consistent information from the race, Hispanic, and ancestry questions. For people, whites primarily, who are third and higher generation

Americans, it's not clear that you get much consistent reporting on ancestry. Some people know they're Dutch or Greek or whatever, but after three generations there's likely to be intermarriage and there's probably either some misinformation or lack of information about the country or nationality of origin of the people who came. And what do those answers mean? That's a subjective concept and some people will write religious groups, which the Census Bureau will not code. Others will write language groups. If someone writes French, we don't know if they're referring to a language group or to a country. Or a term like Moravian used to refer to Czechs at the turn of the century, but it's also commonly used to refer to one of the Protestant religions.

So there's a fuzziness about the ancestry concept and it may disappear from some future censuses. The question comes up, we will in the foreseeable future, anyway, continue to identify racial minorities and Hispanic groups, but for that very large other group of us, will we be asked to give something in the line of an ethnic or ancestral origin, or will we be just not-Hispanic, not-Filipino, not-black? I don't know how it will turn out.

That's what I'm working on now--the quality of the data. There was a 1986 pretest of the 1990 census in which about 61,000 people were asked questions about race, Hispanic origin, and ancestry. Then some 18,000 were reinterviewed about six months later and asked the same questions. So I'm looking at how consistently did people report ancestry, among other things. And I'm also finding, for instance, that a lot of those who don't report on race don't report on whether they're Hispanic and they don't report on ancestry. I don't know exactly what that means. A lot of people don't know their ancestry and another significant fraction of population say they're Americans, and maybe we ought to accept them now.

VDT: Now let's talk about PAA. You have identified Chicago in 1958 as the first meeting you attended, and you've gone to almost every meeting after that. Here's the list of meetings since the beginning which Andy Lunde compiled. What do you remember about that first meeting?

FARLEY: I remember Phil Hauser discussing a book. I don't remember which book; I do remember it was interesting. [Philip Hauser was discussant of a paper presented by William Petersen on "Critical Evaluation of the 'The Study of Population--An Inventory and Appraisal,'" the landmark review of demography as a discipline of which Philip Hauser and Otis Dudley Duncan were co-editors, published the following year, 1959.] The only person who stands out is Phil Hauser. I remember spending a Saturday, which was probably May 3rd, given that at time the meetings were on Saturday and Sunday [1958 meeting was May 3-4].

Then the next meeting I attended was 1962 in Madison.

VDT: That was the last meeting on a university campus [University of Wisconsin]. Norm Ryder is very proud of that meeting. He got it up to Madison and you met in a smallish conference center on the campus.

FARLEY: It was a lovely warm spring weekend. We had had in Chicago that year a rather horrendous winter, so I remember it was a very pleasant climate. It was May 4th and 5th. It looks like that would have been the last of the two-day meetings? I see some later; I thought we switched to three-day meetings quite a while ago, but maybe not. [1966 in New York was the last two-day meeting.]

VDT: Was that Friday-Saturday or Saturday-Sunday?

FARLEY: I think Madison was Friday-Saturday. [Dudley Kirk in his later interview said he

inaugurated Friday-Saturday meetings when he was president in 1960.]

VDT: You already had double sessions then. They started with one double session at the Ann Arbor meeting in 1956. The next year at the University of Pennsylvania, there were three double sessions and Anne Lee, who was running the local arrangements committee with Everett Lee, said people were furious with that. Now we're up to eight simultaneous sessions. There were 84 sessions at your meeting last year [1988] and will be 84 again this year. What stands out in your memory about the early meetings you attended?

FARLEY: I was very interested in the outspoken exchanges of views about demographic issues that I recall at the Madison meeting [1962], the earlier Chicago meeting [1958], and the Philadelphia meeting in 1963, which were the first three I attended. I found it very stimulating, as a graduate student, to hear people talk about various things. I can remember asking a question or two from the audience at the Philadelphia meeting.

VDT: You dared to stand up?

FARLEY: Yes, while I was still a graduate student. It was April of 1963.

VDT: And everybody knew everybody else?

FARLEY: To a modest degree, yes.

VDT: Who were some of the leading lights then? You mentioned Phil Hauser dominated the 1958 meeting, as far as you can recall.

FARLEY: I don't remember any great detail on individuals. I knew the people at the University of Chicago and Phil Hauser used to introduce us to a large array of other demographers at the meetings. I don't remember any particular papers given at the meetings in 1962, 1963, and so forth. 1965 was the first meeting I gave a paper. That was in Chicago. It was on my dissertation research, black fertility trends in the U.S.

VDT: Do you remember outstanding meetings and issues over the years, say, in the early 1970s? Everybody says now the business [membership] meetings are so boring, because there are no hot issues. In the early 1970s, there were the Concerned Demographers, who wanted to get PAA more involved in policy issues, and the women's issues.

FARLEY: I remember at the 1967 meeting in Cincinnati, at the business meeting there were very strong claims made by the quote/unquote Concerned Demographers about the need for the organization to become more an activist group. And I suspect it was at that meeting, although I can't remember certainly, that some people pointed to the eugenics movement in the early 1930s and how we dealt with the eugenics movement in the 1930s as not only distinguished from demography and would that bad precedent be somehow a harbinger of what would happen if PAA became more of an activist group, as some people were advocating at that time. I remember that one in particular. But the following year in Boston [1968], I don't remember discussions of that nature. Concerned Demographers eventually published a newsletter a few times, so there was that kind of an issue.

I don't remember a very activist what you might call Women's Caucus. There has been something of a Women's Caucus, but we've now had seven presidents of the Association who were

women; we've had women on the Board for a long time.

VDT: The Women's Caucus was formed in 1970 and became quite aggressive in their demands. PAA quickly went along with gathering more data on women in PAA and the profession in surveys that went out to members and that women should have more rights within the PAA and so on [see Harriet Presser on "The Women's Caucus in PAA," Vignette of PAA History, PAA Affairs, Winter 1981]. After that, Beverly Duncan walked out of the meeting and I think she even renounced her membership in PAA. Apparently, she did not go along with the position that women should be accorded affirmative action, say.

FARLEY: I can remember Beverly talking about that frequently at the University of Michigan--her strong opposition to either a real quota or something that looked like a quota for women. I think she felt very strongly that women could contribute on the basis of their own merits and didn't need something like a quota system to help them.

VDT: How has PAA changed over the years and do you feel the changes have been for the better? There is the obvious change of numbers.

FARLEY: There is the one of numbers. The scope of what is done by demographers has changed over the years. To be sure, there were some early indications of what would come later. But public health as a field which incorporates demography has expanded such that at the meetings now there are lots of session where something having to do with public health and demography are very prominent.

VDT: You mean mortality? We've always had that.

FARLEY: Not only mortality, but, for instance, lactation and its effects on fertility, or to some degree, diseases and their effect on fertility, and now AIDS--that kind of melding of interests of traditional public health and demography. It seems to me that's a very big development.

Economic demography is now something different from just economic development, so it seems to me that's another major development--something having to do with economic issues in demography.

There have been a couple of other attempts that have never gotten very far in PAA--psychology in demography. There are psychologists in PAA and there may even be a section in the American Psychological Association that deals with population.

VDT: There is: Division 34.

FARLEY: Okay, but that's not quite as prominent in the PAA. I thought at one time it would become . . .

VDT: Jim Fawcett and that group didn't take off. But we have our Psychosocial Workshop; that's where they are. That's like PAA in the past; it has single sessions, 70 people there. I go to all of them, because my former boss, Henry David [director of the Transnational Family Research Institute], is very active in that.

FARLEY: So that's one area that has not taken off.

The other thing, I think, is the increasing number of demographers who specialize in some foreign area. In the early days, there would be a few people who were specialists in Asia, a few who

knew a bit about some of the European countries. Now we have sessions specializing in a country. So we have that kind of international focus, much more detailed and specified than . . .

VDT: You mean more detailed than a general interest in Third World demography?

FARLEY: That's right. If you'd had a session on Africa 20 years ago, you might have had a hard time filling that. Now we could have a session on West Africa or AIDS in Central Africa or population trends in sub-Saharan Africa--all those sessions would fill up, I think, because of the increasing specialization.

VDT: Increasing specialization, of course, means proliferation in numbers. Do you feel that the numbers of sessions are getting a bit out of hand--84 sessions, eight overlapping? Or is it inevitable, to be accepted?

FARLEY: I think you want to encourage membership and you satisfy the members if you provide them with opportunities to be on the program every two or three years and in that regard it's good to have a lot of sessions. I think most of us would have a taste for a modest-sized organization, rather than a huge organization.

VDT: By modest-sized, you mean . . .well, let's talk about meetings. Modest-sized, you think would be 1,100? That was the number [actually 1,115] at your meeting [1988], which was the record number so far, except for the Washington meeting in 1981 [1,167].

FARLEY: I think 1,100 is a good number. I would guess 8,000 is a bad number, because you are very isolated. It's my impression that even the American Statistical Association meetings, which run in the neighborhood of 3,000, in some ways that's too large. But that's partially a matter of my taste.

I don't know if there's any law about organizations dividing when they get very large. I say that because the American Psychological Association has been going through its splits between theoreticians and practitioners. The anthropologists are very likely to split. Political science as a professional organization has gone through some difficulties because of the high degree of specialization--need for the various wings to have their own journals. So there may be something quite dysfunctional about a professional organization reaching the size of 15,000 or something of that nature. I don't know.

VDT: You think the PAA is safe with 2,600? It's fluctuated at that for a number of years; it can't seem to go up [2,679, end 1989; 2,752, end 1990].

FARLEY: No, it hasn't gone down, but it hasn't gone up.

VDT: Why do you think that is? People have often pointed out--Jay Siegel first in a paper at a meeting in the 1950s ["The Teaching of Demography," 1951]--that there are hundreds more people teaching courses in population than there are members of PAA. Is PAA still fairly exclusive--those who are really doing research in the field? Although we now have more and more applied demographers.

FARLEY: Yes, but they're doing research--those people who are estimating state populations or looking at health issues in local areas. I think PAA is a professional association for people who have a pretty serious commitment and spend a lot of their time either teaching demography or doing

demographic work. I imagine there are courses taught at small colleges or community colleges, but the one who teaches a demography course is probably teaching three or four or five other courses and is not a member of all those professional associations.

Certainly, there are a lot people who are doing demographic work in marketing. We have expanded there; I think we are increasingly tapping into that area. There are very many people who use some elements of demography in that kind of work. We also have had, I think, increasing contacts with and memberships from geographers who do things with population materials, so we have had some expansion there.

VDT: It's the European tradition that demography came out of geography, much closer than here--territorial distribution.

FARLEY: Yes. I think geographers were pretty rare in the PAA for a number of years and I don't think we've ever had a geographer as president. But I think now we're seeing more people whom we would identify as geographers coming in.

VDT: And a couple of sessions cosponsored by the Association of American Geographers.

Let's go back to the problem--if you want to call it that--of the 84 sessions, eight overlapping. I've asked people before what's the solution--another day? Of course, more and more is being added. This year [1989], for instance, there are even workshops Wednesday night before the beer party, and many more on Thursday night. Add another day, or . . . ?

FARLEY: Well, I suppose that's something the Board and the president will have to wrestle with in the future. Adding another day would impose extra costs on those who want to come and it's not obvious that it would increase the attendance if you add another day. Maybe there's merit in doing it. I don't have any strong feelings about it.

VDT: What about being very strict on the number of times people can appear on the program? Of course, it's been pointed out many times you have to give people the opportunity to give a paper; their way is paid if they give a paper. So that explains in part the proliferation of sessions. But what about being very strict: you can be only a chair, only one author, or coauthor, or whatever? Now some people get around that rule and appear two and three times on the program.

FARLEY: Well, they shouldn't be on more than twice, but they do get on twice. Maybe the solution would be to have a person appear only once or even once every other year. You could have such a rule. The question would be, would the quality of the papers and the overall quality of the meeting be compromised if we went in that direction. We don't know how to measure--at least we don't try to measure--the quality of these sessions. But I've thought there were some of those 84 sessions which were really so weak that they should have been wiped out, or if there's one or two good papers, they should have been put together with some other good papers.

None of these are easy decisions to make. Because maybe you've got an excellent paper on what's happening in Chile and another excellent paper on population redistribution in Edmonton and you can't put those two together in one session. So I don't know what the solution is. I think basically our meetings are pretty open to anyone who wants to get on. We might go to nine overlapping sessions [and did in 1990 and 1991]--who knows?

VDT: Let's talk about your plans last year. Many oldtimers, of course, deplore the lack of free exchange from the floor that was possible when the meetings were much smaller and everybody went

to the same sessions--as you said, you had the nerve as a graduate student to stand up and ask a question. With the meetings much larger, that's not possible. Did you . . .well, people have praised your author-meets-critics sessions of last year. That gave people a chance to participate. Plus the panel sessions are supposed to give people more chance, but people have also said those have degenerated into paper sessions too. So there's not so much exchange from the floor.

FARLEY: It's very easy for any session to degenerate into a paper session, in that someone thinks that they're going to have 15 minutes and they want to put on a good show in that 15 minutes, so they work very hard on a paper and don't like to be interrupted by somebody telling them that their ideas are loose or something of that nature. So, yes, I think we have a problem in having informal exchange. But we did have some panel sessions.

And I had one other session which I hope was somewhat innovative last year, where Cam Gibson from the Census Bureau and Gerry Hendershot from NCHS were supposed to do a kind of show-and-tell with graphics, Gerry doing things about data from the National Center for Health Statistics and Cam doing things from the Census Bureau ["Current Trends in United States Population: A Visual Presentation"]. I don't know if that was a success or not, but it was meant to be something other than the regular paper session.

VDT: Harriet Presser must have decided it was a success, because she's having something similar sounding this year [computer demonstration sessions].

FARLEY: I tried to encourage poster sessions, but I got a very small number of people. There are so many things in demography where a person with five good posters could illustrate their findings--trends in some disease, trends in fertility, in migration. If they were to post five or six good figures and then stand there for an hour to talk to whoever wants to come up to talk to them about it, it would be a useful exchange. I tried to initiate that and I think there were five or six poster sessions, meaning people who were there talking about their findings as illustrated in some set of charts or graphs. That's been very popular in other associations.

VDT: Harriet again is going to try that this year, I see. I frankly have never gone around to look at those. Did that come out of your concern about good graphics?

FARLEY: It came out of good graphics, and alternative ways. As you say, there is something a little formal, sometimes dry, about a man or woman getting up and reading a 25-minute paper. Poster sessions are an alternative. Particularly if you're in a specialized area or you're doing preliminary work, you can discuss it presumably on an informal basis with other people who're interested, maybe get together, go out for a drink or lunch with them, and presumably that would exchange information in something other than a formal session.

The roundtable luncheons do that too, not that they are all that successful.

VDT: No, it's a little noisy. I was going to ask you about the roundtable luncheons, because that was obviously a vehicle for you. You seemed to have made a conscious effort to draw in the oldtimers. Last year, you had six past PAA presidents among 25 who were in charge of those roundtables.

FARLEY: Yes.

VDT: For instance, Amos Hawley, whom I interviewed just before that. That was wonderful; he hadn't been there in years. Of course, Judith Blake had been, and you had some others like Ansley Coale, who is still a regular.

FARLEY: I was hoping to get Joe Spengler, but as you mentioned--I had asked George Myers if Joe could do it and he told me Joe had Alzheimers. I asked Don Bogue to do a table on the history of demography, but he was going to be somewhere South of the border and couldn't do it. I think I had two or three other past presidents.

VDT: You had Judith Blake, Ansley Coale, Richard Easterlin, Hawley, Con Taeuber, and Charlie Westoff.

FARLEY: I think I asked a couple more, besides Spengler and Bogue. At one point, I had Ron Freedman down for a luncheon table, but he then gave a paper . . .

VDT: An excellent paper on the University of Michigan population center. I was especially pleased with that. Then there was the Kingsley Davis session on "Two Centuries after Malthus: The History of Demography."

FARLEY: Yes, which drew a very good crowd. I frankly thought that would not draw a large crowd.

VDT: That's an interesting thing. Norman Ryder in his interview said that in this country there has not been a tradition of interest in the history of demography. There is in France--of developments since Malthus or even before. And he was hoping to do something on that. You talked earlier of your interest in my project; you feel there should be a history of demography in the U.S. How about you taking it on--20 years from now?

FARLEY: Maybe, I don't know.

VDT: Very good.

How did you organize your preparation of the program last year? Incidentally, do you know when they shifted from the first vice-president being in charge of the program to the president?

FARLEY: No. I heard that Jane Menken [1985 president] resisted--thought that it was too much for the president to be in charge of the program. But I think I do know. Because when Evelyn Kitagawa was going to be president in 1977, she was going to be obligated to organize the program twice.

VDT: You mean she came from being first vice-president to president?

FARLEY: Yes. [Kitagawa was first vice-president in 1974-75, responsible for 1975 meeting program.] And she asked me to organize the meeting, so I organized the 1977 meeting in St. Louis. [The meeting program became the incoming president's responsibility in 1976 with Sidney Goldstein's tenure, 1975-76. He too had been responsible for the program two years earlier, 1974, as first vice-president, and asked Charles Nam to organize the 1976 meeting.]

VDT: That was the year the terms of office were switched to the calendar year for officers [except secretary-treasurer]. So you were an old hand--you did it twice. Did you have a committee last year? You didn't try to do it all by yourself?

FARLEY: Oh sure, I did it mostly by myself.

VDT: Jay Siegel claims he did it by himself [in 1980], except three women in his office at the Census Bureau helped him. He said that's a stupid way to do it, but his committee members didn't contribute much. You did it mostly by yourself?

FARLEY: Yes. I found it difficult to use a committee because of the tremendous overlap. I thought maybe I could have a committee and have one person do fertility, another do mortality, one doing developing countries, and another doing the U.S., or make some set of categories like that. Then you're inundated with suggestions for papers which are the effects of infant mortality on fertility or mortaling in developing countries and you realize there is no way you could sort those papers out. So I ended up doing the program, because I thought I would be forever calling up one sub-organizer and telling him or her to call up some other sub-organizer because the paper overlapped.

VDT: The organizing consists of designating people to be the chairs and organizers of sessions and fitting papers into those sessions, or vice versa?

FARLEY: No, first you get the organizers of sessions and the title of their session. Now, people write in with suggestions and I--as I think most other program chairs do--you take the suggestions. Some people suggest themselves, others will suggest "anybody," in which case you find one, but that's the pool of candidates to be organizers. Then the list of that set of organizers is sent to all the members and members are told to send their papers to the appropriate organizers, but a lot of members send their papers to you or organizers send the papers to you. So there's a lot of sorting out of papers to be done as the program goes on.

VDT: But you can pretty well take the initiative in deciding what topics are going to be covered?

FARLEY: Oh yes, as long as you can find an organizer. Last year, for instance, I wanted to make sure that AIDS was on the program appropriately.

VDT: It was on it several times.

FARLEY: That was Wendy Baldwin. I called Wendy: "Who should I get to do AIDS?" I got some suggestions from people who volunteered themselves, like Andrei Rogers wanted to do something on training issues in demography, but in other cases you had to scout somebody out.

I tried very hard to get someone from the University of Montreal to do something on the French population of the United States, since we were meeting in New Orleans--the French heritage population, people like Acadians who were shipped out of Acadia by the British--and I did not. I thought there was somebody up there who would be working on the history of the French population of Canada who would also be going occasionally to Louisiana to look at the contacts or something, but I never got any positive response on that. I did get one session there on Cajun life, supposed to be Cajun demography.

VDT: And you also had the Cajun band at the alumni night. Did you start the alumni night party--having all the different universities join together?

FARLEY: Yes. The different universities used to have parties in hotel rooms separately, and sometimes the hotel would come around and shut parties off because people were too rambunctious. So we put that together and hired the band.

VDT: It worked well.

FARLEY: I thought it worked well.

VDT: The dance floor wasn't very good; we were dancing in a carpeted corridor. But otherwise, it was great fun.

FARLEY: I wasn't sure whether it was going to be a success or not, so I didn't want to reserve a big room for that. Also we knew it was going to be noisy, so it had to be away from other places in the hotel. So that wasn't quite ideal, but it wasn't a total disaster.

VDT: I thought that was a great idea. Other years, you'd go to your university party, in my case it was Georgetown, and you felt a little diffident about dropping in on others.

FARLEY: But you did. None of those parties could be exclusive. People didn't know how much liquor to bring. Some hotels tried to make you pay a fee if you brought liquor in, because of their local regulations. So to avoid that hassle, we had the one alumni night party.

VDT: And that's going to happen again this year. Another thing you have introduced is the fun run. Was the first one at Washington, in 1981, for the 50th anniversary meeting? I proudly show off my T-shirt from that one.

FARLEY: The first one was in Philadelphia in 1979, which was a lovely fun run through Fairmont Park, finishing on the steps of the art gallery. It was after Charlie Nam's presidential address on a Friday afternoon--a lovely, warm Friday afternoon--5.:30 or 6 o'clock and dusk was just gathering when we finished on the steps of the art gallery. Then we went around the capitol in Denver [1980] and we went up Rock Creek Park in Washington [1981]. San Diego [1982] was a very ugly site; the hotel there was ugly.

VDT: I agree with you. It was my first time in San Diego and it was a disappointment. We were isolated . . .

FARLEY: We were isolated. We did a fun run there. We had to go on some highway; there wasn't much traffic, but it was unpleasant.

VDT: And it was an overcast week; there was no sun. Were the fun runs after the first one in the morning--Saturday morning?

FARLEY: Yes, they were all in the morning.

VDT: And the T-shirts. I've got mine right here from 1981, green, with "PAA 50 years," and you turn it around and it says, "Running reduces downtime."

FARLEY: We had T-shirts for Denver also; that was the first time. There was one another year with a PAA fun run that Dudley Poston organized. I think we've had T-shirts for three runs.

VDT: You're obviously a jogger.

FARLEY: Yes [and a marathon runner].

VDT: And another outstanding thing about Ren is that he's always looked about half his age. Everybody thought you were too young to become PAA president.

FARLEY: But Sam Preston looked very young when he was giving his address [in 1984].

VDT: But he was young.

FARLEY: I think Jane Menken is younger than I am and she looks young.

VDT: You're just about a year apart.

FARLEY: Jane and I are very close; we've talked about that. Sam is younger than either of us, I think.

VDT: Yes, about four years younger than Jane. Now I want to talk about becoming president, how you work your way up the old-boy/old-girl network to become president. In a couple of interviews I've had, we've talked about the fact that two top people run against each other for the presidency: one wins, one loses. And the losers are outstanding people, like Chris Tietze. Last year, Joe Stycos lost to Harriet Presser. I think the gender issue was involved there somewhat. And you ran against Paul Schultz. Then these people sort of drop, which is unfortunate. Have you got some comments on that? How do you get, first, in the position of being nominated to be president?

FARLEY: I think almost everyone who has been nominated to be president has had a long involvement in demography and in the Association, through committee or other service. It seems to me these days the nominations committee selects two distinguished people, either of whom is very well qualified to be president. I think unlike some other associations, we don't put a loser up against a winner. I think in some elections for professional associations, there's kind of a designated victor and a person who is on the ballot for some other reason.

Now that's good that you have two strong candidates running. But it does mean that both of those people are likely to be quite invested in the thing and one of them will be disappointed.

VDT: It's unfortunate. Apparently, some people have run twice for the presidency, but I would think you decide, no, that's it. For instance, with Joe Stycos--he's much older than Harriet--some people felt it was a bit unfortunate, because he will not run again. Harriet could have run again.

FARLEY: Yes. Have there been many people who've run twice?

VDT: I don't know; I'll check that.

FARLEY: Have they been elected?

VDT: I don't know. That's a historical detail to check. [Paul Schultz ran again in 1989, for president-elect of 1990, and lost out to Ronald Rindfuss.]

You still enjoy the PAA meetings?

FARLEY: Yes, definitely.

VDT: Even though they've gotten very large and it's sometimes hard to see everybody one wants to?

FARLEY: We do have the beer party the night before, which I think is pretty functional in letting people get in contact with other people. And we certainly instructed Ed Bisgyer [former manager of PAA business affairs at the American Statistical Association] to get us hotels where there was good space for informal meetings. That did not happen in San Diego, but that was the extreme in lousy meeting space. Most other places have had good places for individuals to meet other individuals.

VDT: Although Norm Ryder complained that at the hotel in New Orleans, there was no place to sit down in the open public area.

FARLEY: He's partially right, but with the homelessness issue being what it is, I think there are going to be darn few hotels that have the traditional old lobbies with lovely places to sit. Anybody can come in off the streets and sit there.

VDT: That's not something I realized. You mean that's why hotels have not wanted to have seating . . .

FARLEY: Many hotels, if they have public areas, they're either small, they're unattractive, or they're on the third floor so that they can discourage someone from coming in.

VDT: Final questions. Obviously, you've had a wonderful career. You've enjoyed it, right?

FARLEY: Yes. But it's not quite over, Jean.

VDT: Right! You still have decades.

FARLEY: I want to do some traveling.

FARLEY: In connection with your career?

FARLEY: No, not in connection with my career. It would be fun to take off three months and travel sometime, which I've never done.

VDT: You have been very involved in U.S. demography, demography of blacks. That's unusual among demographers now; most have had international demography as part of their work too. And you've had Ron Freedman as a colleague, who is involved in the Third World. Is it just that you did not have time for that?

FARLEY: I think it was not only time but sort of deliberate choices on my part. When I came to Michigan, Ron asked me several times would I be interested in getting involved in the Taiwanese fertility project. I thought about it, but never pursued it because I found it very interesting to work on U.S. population trends, minority groups in the United States. So my work has been almost entirely on the United States.

I would perceive probably continuing to do that. But if another opportunity presented itself . . . I think increasingly I'm getting into not just blacks in the U.S. but sort of the assimilation or competition among ethnic groups, minority groups, the new immigrants coming into the United States. I can imagine working on what's happening to our country as increasing numbers come from Asia and

Latin America. I might get abroad sometime, but I have't done so thus far.

VDT: Your career has been basic research. Is there still room for that in demography? Now, increasingly, demographers are going into applied fields. Is that where the future lies, or is there still a chance for someone to have a career such as you have had and people like Ron Freedman and Phil Hauser?

FARLEY: Phil has been applied and I would say that Ron has had some applied elements in his research. Who knows what the future of demography holds? I would think, though, that as has been the case in the past, the problems that demographers address are often contemporary problems, be it fertility, mortality, immigration, assimilation questions, and that funding to do research is going to come from foundations or governmental agencies interested in that problem. So just as in the past, I would think that in the future a lot of the research will be addressed to contemporary issues.

On the other hand, we certainly do give accolades to people who work on different things. John Knodel's work on 17th century European villages is very different from what most applied demographers do. And I think we still do have an interest in those issues, demographic history and some of the mathematical modeling that goes on in demography. It's pretty abstract, something that economists or even mathematicians might do, rather than somebody else. On the other hand, if you were going to simulate the spread of aging in a society, I suppose you'd use some very sophisticated mathematical model. It seems to me demography has always had quite an applied element to it.

And I think that's why the future of demography is reasonably bright, that we're going to continue to have a whole series of issues about population. As we reach zero population growth, there's going to be one set of issues. If our population expands rapidly, that's another set of issues. If our growth is entirely attributable to immigration from Latin America and Asia, that's going to raise another set of issues. We're going to continue to have in the United States, it seems, lots of migration and shifting distribution. So we'll be having a lot of challenging things for demographers to do.

VDT: For basic research, in addition to applied . . .

FARLEY: You draw a line between basic research and applied. I guess I see those two things as pretty much the same. You've got a set of issues and it stimulates some scholarly activity and many people who do the research are going to abstract from that one problem, while they may solve that problem, they will write more generally on the topic. So I don't think there's quite that dichotomy.

VDT: That's interesting, because others, more traditional demographers, would make the dichotomy.

FARLEY: Yes.

VDT: Certainly of the applied demographers who work in state and local government and in business. But you don't?

FARLEY: No. I think some of our better graduates from the University of Michigan who have gone out into applied work . . .

VDT: You mentioned Paul Voss.

FARLEY: Sure. And others who've gone on to commercial marketing, for instance. I guess I don't see the huge distinction.

Now, there is a distinction in what's rewarded. If you're working for a marketing firm, I assume that writing scholarly books doesn't get rewarded the same way that if you're working for a university and start writing books.

VDT: Do you think that applied demographers should continue--at least some of them--to get doctorates, or is a master's degree program, sort of a journeyman apprenticeship training, enough? You have, for instance, your program of Robert Groves at Michigan. I've inquired, because I have a niece at the University of Western Ontario, where there's one of Canada's few population centers, in her last year of sociology and interested in going into business demography. There's also Paul Voss's program at Wisconsin [where the niece ended up].

FARLEY: Yes. I think for a lot of positions a master's degree is very appropriate. I suppose it's the unique set of skills that a person brings to the job. There have been some very distinguished demographers at the Census Bureau who didn't have doctorates.

VDT: Yes, Jay Siegel.

FARLEY: Jay Siegel, okay. A master's degree is very appropriate for certain circumstances. Again, it's the individual skills and interests and so forth. Who was our last president who didn't have a doctorate? Jay Siegel, I guess [president in 1980].

VDT: Jay Siegel has not--he's very conscious of it--and Art Campbell [president in 1973-74]. Perhaps some of the very early ones did not.

FARLEY: Clyde Kiser, Vance?

VDT: Yes, they all did. But Jay Siegel and Art Campbell did not and, funnily enough, Jay Siegel at least is very conscious of that, because he felt that being in the accepted group, you had to have a doctorate. But certainly he went on to write books and has taught in universities everywhere.

FARLEY: That's true. My guess would have been that there were more presidents who didn't have doctorates, but now that I look at this list, I realize that they all did. Whelpton [president, 1941-42] did not have a doctorate, I don't believe.

VDT: That's the other outstanding exception. Jay Siegel mentioned him.

FARLEY: What about Truesdell [president, 1939-40]?

VDT: Truesdell did. I've just transcribed Philip Hauser's interview and he was recruited by him, interviewed by him, for the job at the Census Bureau and he kept referring to him as Dr. Truesdell.

FARLEY: Truesdell died within the past five or six years. He lived to be quite old and I think he wrote poetry. [Notestein in his interview, above, confirmed that Truesdell was a poet and said he "didn't complete his doctorate until long after he was in the Census Bureau."]

VDT: Phil Hauser described him as a taciturn New Englander, not very forthcoming. Anything more on presidents? Joe Spengler--why were you after him last year?

FARLEY: I was very impressed with Joseph Spengler. When I went to Duke, he was the senior demographer there and he was among the most scholarly individuals I have ever met, tracking down obscure references which might bolster a point he wished to make. I was very impressed with Joe Spengler. He's also an Ohian. Came from Piqua, Ohio; they pronounce it "Pickway" in Ohio.

VDT: Did you ever know Rupert Vance?

FARLEY: No. And I did not know Margaret Hagood.

VDT: She died at a reasonably young age. There's a sad story about her. Henry Shryock attributes her death in part to her despair over the fate of her friend Hope Eldridge, who was a victim of the McCarthy era, hounded out of her position at the United Nations, because they claimed she had belonged to Communist organizations, which was quite untrue. Margaret Hagood also had a heart problem.

FARLEY: Con Taeuber's appointment at the Census Bureau was held up a year or so because of the McCarthy stuff. Con had written something about social change in Russia in the early 1930s, I was told--I don't know this for a fact. Maybe he even visited Russia in the 1930s. And somebody picked out this article he had written, probably 15, 25 years earlier, and that delayed his appointment at the Census Bureau [in 1951] during the McCarthy era. [Conrad Taeuber describes this affair in his "interview," above. The "article" Ren Farley recalls was a special issue of the American Sociological Review put out after the U.S. entered World War II "with information about our wartime ally, the USSR. The Soviet-American Friendship Society informed its members of the existence of this publication and that it could be ordered from Conrad Taeuber (managing editor of the Review) at the Department of Agriculture. In 1951 when I was to join the Census Bureau, my necessary clearance by the FBI took much longer than expected. From my FBI files, which I obtained years later under the Freedom of Information Act, it was clear that the special issue of the Review had been the focus of their extra-long investigation. Lowry Nelson in describing the McCarthy era in Washington in his autobiography wrote of being questioned in his office by an FBI agent on the loyalty of Conrad Taeuber to the U.S. Lowry had no doubts on that score. The FBI finally sustained that judgment.]

VDT: Oh, dear. Maybe this is why he is so conscious of this. He recently sent me some material he'd found: a sort of disclaimer, signed by Hope Eldridge, saying, "I have not had anything to do with the Communist party," and a newspaper article on the fact that Dag Hammarskjold had gone along with this business and some UN employees were dismissed because of the allegations from the U.S. government. The UN system was supposed to be above that.

FARLEY: Yes.

VDT: Did you know Irene Taeuber?

FARLEY: Oh, sure. Irene was a very nice person. I remember being interviewed by her at Michigan; she was doing some survey of some kind. I guess I've known most all the presidents, some of them not very well. I didn't know Harold Dorn. I've met Cal Schmid, but I didn't know him very well. John Durand I met. Going back toward the earlier ones, Con Taeuber, Frank Notestein, Frank Lorimer--I did not know Whelpton, and the early ones I did not know at all. But I think I've had a good bit of contact with all the more recent presidents of PAA.

I remember one story of Joe Spengler. He used to talk about going to the PAA meetings,

maybe in Princeton, and taking I guess it was the Seaboard Interline Railroad up from Raleigh. He used to go up and sit with Rupert Vance and he somehow used to try to get Rupert to order something in the dining car and he knew Rupert wasn't going to finish it all and then he could finish it--scallops or something. Joe Spengler used to have lots of stories like that; he was a very interesting person. Too bad you can't interview him.

VDT: He was a storyteller besides a scholar?

FARLEY: Oh, yes, he was a storyteller.

VDT: He was still writing and publishing in the early 1980s. He's considered one of the great overall theorists in the field, like Kingsley Davis.

FARLEY: Yes.

VDT: What do you think about Kingsley? He's having another session at the coming meeting and he's involved in a new book.

FARLEY: And he had the thing in the op-ed page of the New York Times.

VDT: Oh, yes! Retirees play golf and cost us so much ["Our Idle Retirees Drag Down the Economy," New York Times, October 18, 1988].

FARLEY: I know Kingsley; I don't know him intimately.

VDT: I think you're going to be another Kingsley, because you keep yourself so fit. Maybe you won't be producing children when you're 79. Do you have children?

FARLEY: No, don't have any children.

VDT: Where did you meet your wife?

FARLEY: I was teaching at Duke and she was a graduate study in psychology at Duke.

VDT: What is she doing in these six months in Washington?

FARLEY: She got a letter yesterday from the District government. She's a forensic psychologist. When she was here last time, she worked with the District's . . .

VDT: When were you here last time?

FARLEY: I was here for a sabbatical leave in 1981 and 82. The Reagan freeze had just come into effect. She worked with the agency then. It's the forensic division of the superior court of the District of Columbia. So this time, she finally got appointed to a job there, which barring cutbacks on the part of Marion Barry or something, she will start a week from Monday.

VDT: Despite our indefinite D.C. budget, she nevertheless got a job. Great. Thank you very much, Ren.

FARLEY: This was a lot of fun. I enjoyed it; it's interesting. You've got a vast amount of good information.

VDT: Absolutely. It's so interesting and revealing. I learn a bit from each of the interviews, for instance, your telling me that Norman Ryder's view of the establishment of Demography is not quite right.

FARLEY: I said it's not quite Don's view. I was a young man at the time, but Don and Norman, I think, have different views.

VDT: And the fact that I just finished transcribing Phil Hauser's interview and there you are on his list of 21 outstanding American students at Chicago. Of course, there are the foreigners too.

HARRIET B. PRESSER

PAA President in 1989 (No. 52). Interview with Jean van der Tak at the Department of Sociology and Center on Population, Gender, and Social Inequality, University of Maryland, November 15, 1989.

CAREER HIGHLIGHTS: Harriet Presser was born in New York City and grew up in Miami Beach, Florida. She received a B.A. in sociology from George Washington University in 1959, an M.A. in sociology with a minor in mathematics from the University of North Carolina at Chapel Hill in 1962, and the Ph.D. in sociology from the University of California at Berkeley in 1969. Between getting her degrees, she worked at the Census Bureau, at the Institute of Life Insurance and the Population Council in New York, taught demography at the University of Sussex in England, and spent a year in Puerto Rico doing research for her doctoral dissertation. From 1969 to 1976, she was Assistant and then Associate Professor of Sociomedical Sciences in the School of Public Health and the International Institute for the Study of Human Reproduction at Columbia University and also Associate Chief of the Demography Division (from 1973). She also taught sociology at Rutgers during that time. She has been at the University of Maryland since 1976 as Professor of Sociology and, since 1987, Director of the Center on Population, Gender, and Social Inequality, which she founded. In 1986-87, she was a fellow at Stanford's Center for Advanced Study in the Behavioral Sciences. She has been a consultant to many government agencies, survey organizations, and other universities and has provided testimony at several Congressional hearings. She is well known for her research and numerous publications on sterilization, the timing of first births and teenage pregnancy, work schedules, women in the labor force, and child care, among other topics.

VDT: To begin at the beginning, how and when did you first become interested in demography?

PRESSER: Maybe the beginning should be how I got to be interested in a profession, because that goes back a little and helps explain the demography. I had gotten my B.A. at a time that I also separated from my husband.

VDT: Were you separated before you went to university or during the time?

PRESSER: During the time. I had had a year and a half, or two years of college, got married. My husband was in the Navy. I joined him, we traveled around, and we got to the Pentagon and the Washington area, in Arlington, in 1958. I had just had a child.

VDT: Your daughter Sheryl?

PRESSER: My lovely daughter Sheryl. I was debating about going back to school, because it was something I had thought about. But I had grown up in the 1950s and the ideal at that time was to get married, have lots of kids, and have your husband support you. Going back to school when I did--in 1958--was actually associated with a shaky marriage. I thought it might improve my marriage by making me more active and happier--but it did not.

VDT: You went back to George Washington?

PRESSER: Yes, I went there because they had a night school and I could go at night and feel I was not abandoning my daughter to go back to school, which was the ideology at the time also.

VDT: You went at night--who looked after her then?

PRESSER: My husband did sometimes; he was working for the Navy and had a rotating shift. When he could not, we got a babysitter. Except for one course in French, which I could not take at night, all my courses were at night. GW had really great faculty at night. There was a course, I remember, on social research that was really quite good, in the department of sociology, and there were several other faculty there that had gotten their degrees at Chapel Hill.

I separated from my husband while I was going to school and thought I would stay in the area initially. After graduation, I went to work at the Census Bureau; I was sent there by the university's placement office. That was in 1959, before the census of 1960. In 1960 the census enumeration was still person-to-person rather than by mail. I went to work for Jack Silver in the field methods research branch, not knowing anything about what demography was. I was classified as a statistician, because I had had some statistics in the social sciences and I wanted this job. As a matter of fact, I didn't think I was qualified, but the GW placement office told me the Census Bureau would be flexible if they wanted to hire you by considering the content of all your courses and if there was some logic, mathematics, or statistics, they could classify you as a statistician.

So I met with Jack Silver and it sounded like a really interesting job, which it was. It was to go out in the field with all the packages they had, instructions they created . . . Do you want me to go through all this?

VDT: Absolutely. This is obviously where your first interest was roused in rotating shifts and who looks after the child when a lone parent has gone out to school or work. Please go on.

PRESSER: So I went out in the field, used the various manuals they had created for the average census-taker, crew leader, etc., for the taking of the 1960 census. They thought I was a good person to do this, since I came in fresh and didn't know the census terminology. They wanted to see if I could follow the instructions as a typical person out in the field that wasn't exposed to Census Bureau terminology, etc. I went out in the Greater Washington area, as a crew leader, saw if I could follow the maps, went out with instructions about being an enumerator. I got really interested in field methodology. They had issues about quality control, determining how many callbacks to make before quitting. I really didn't identify then as being a demographer; that was survey research.

This was in the fall of 1959. Unfortunately, I found I really couldn't afford to work. My husband was not making that much money; we were young and hadn't much assets. It was hard for him to help maintain two residences or to provide adequate support for both me and my daughter if I didn't work. And with my job and the cost of child care--which I was expected to pay--and the fact that I really did feel badly being away from my daughter, who was just a few months old, for the whole day, plus a long commute from Arlington to the Census Bureau, because there was no Beltway then; you had to go into Washington and out.

By the way, one of the most interesting aspects of the job was the commute, because various people from the Census Bureau were discussing the politics of the Bureau en route. That was far more fascinating than any tutorial would have been. We had carpools and the carpool had several interesting people in it that worked in various parts of the Census Bureau, much higher in status in every sense than I, as a lowly GS5.

Speaking of GS5, I had taken that job, rather than as a secretary which I had been in the past, at a lower salary than I could have gotten as a secretary, because I was quite a good secretary. But having gotten my B.A., I didn't want to go back to being a secretary.

I found that working was really difficult. I missed being with my daughter; I wasn't making much money; it was very hard to make ends meet. So I decided to go home, which was Miami Beach,

to my mother, who was separated or divorced at that time. And speaking of work schedules, my mother worked in the daytime and I worked as a hostess in my uncle's restaurant in the evening, so we could alternate child care and, therefore, with the little money I made, along with minimal child support, I could manage to both work and be with my daughter all day.

I found it was not a very satisfying life style--to be living at home with my mother, whom I loved dearly and got along with very well, but I had no friends down there who were in my situation. This was . . . my daughter was born in 1958 and I moved to Florida in 1959, after working briefly at the Census Bureau. Before I had graduated from college, I had been encouraged to go to graduate school by the faculty at GW.

VDT: Because you had done well in sociology at GW?

PRESSER: Right. Then I got a letter from Richard Stephens at GW, when I was down in Miami Beach and feeling that this was not the way to live the next few years of my life. He asked if I would be interested in being nominated for a Woodrow Wilson award; the notice had come across his desk. That's what got me thinking seriously about graduate school. "Gee, I'd really like to get out of here. If I go back to school, I wouldn't have to work a full day. If I can get support for school, then I would be able to combine taking care of a child and doing something worthwhile for myself."

I was thinking only about a master's degree; a Ph.D. was totally out of my mind then. I don't know if I'd even heard the word before. That's a joke; I just really didn't understand what a Ph.D. was and it seemed like a really long-term commitment that I probably wouldn't be able to make. So I went back for my master's. I had applied and said I was only seeking a master's. Actually, if I had said I was interested in a Ph.D. maybe they would have been more interested.

VDT: This was a Woodrow Wilson fellowship for GW?

PRESSER: No, I'm talking now about my application to the University of North Carolina. Woodrow Wilson fellowships, if you were awarded one, were offered for any school you chose. I only applied to Chapel Hill because Chapel Hill had a child care center.

VDT: I was going to ask what took you to Chapel Hill, and that was it!

PRESSER: My former professors at GW suggested it and they had a child care center. It probably was one of the first universities with a child care center on campus. What was interesting about it was I believe I was the only unmarried mother with a child that was in graduate school at that time. I had to live in family housing with a child; they wouldn't let you live in single housing because you had a child. I knew almost all the other people there and I was the only single mother. And at the child care center, I believe I was the only mother that was putting her kid into child care in order to go to school. The center was there for the students but typically it was that the mother was working so her husband could go to school and that was the main rationale for providing child care to students--male students who were fathers. They also used the center to do research on children and it was tied in with the university. It is now one of the leading child care centers at a university. I think it had been formed not too many years before I arrived in 1960.

Anyway, that was a critical consideration in choosing Chapel Hill. I received child support of \$40 a month and child care cost \$36 a month.

VDT: They gave you \$40 a month with your fellowship?

PRESSER: No. This was child support from my husband--with no alimony. As I said, we had no savings to speak of, either of us. On the other hand, by now he had a good job and it wasn't very generous.

So I saw graduate school--with the assistantship--as a way of managing my life. I went to Chapel Hill. I did not get the Woodrow Wilson award, but it had started me thinking along these lines, and I had gotten a very positive letter from the department saying, "We can't promise you an assistantship, but it is very likely that if you come up you will get one." I had maybe three or four hundred dollars in my bank account when I left for Chapel Hill.

When I arrived, I was not well received by the chair of the department. I don't mind saying his name--Charlie Bowerman; he was a family sociologist. He reacted very negatively to the fact that I had a child, was divorced, and wanted to be a graduate student. In those days, one was very explicit in expressing such views; it wasn't considered wrong to be prejudiced in this way. He told me that I was a bad risk, that the letter they had sent me was a form letter and I really shouldn't have interpreted it so positively. He didn't think there was any chance for support from the department.

VDT: Support in the department?

PRESSER: Financial support--any research or teaching assistantship. He sent me to apply for secretarial jobs on campus. That meant I could not get my tuition waived, which was extremely important to me because I was an out-of-state student and the amount was substantial for me.

I went around to various faculty members and Richard Simpson, who was then an assistant professor, now a leading sociologist, gave me work that I could do at home, coding occupations. I also managed to get a half-time teaching assistantship--which waived my tuition--for Harry Crockett, who was teaching marriage and the family. The job was just grading papers. It was only 10 hours a week rather than 20 and paid \$900 a year, rather than \$1,800, but it did waive my tuition. I did that on top of doing coding for Richard Simpson. I don't know if he had a grant then; who knows where the money came from.

VDT: Some of which you could do at home, when you were there with Sheryl?

PRESSER: Right. In those days also, even though the child care center was a full-time center, the ideology was such that it was generally considered a very negative thing to put your kid in a center so you, the mother, could go to school. It was better if you did it so that your husband could go to school. So I used to pick her up early and take her with me, feeling she was better that way.

VDT: Meaning before 5 o'clock, before closing hour?

PRESSER: Oh yes--much before. I'd take her to the library with me, tell her to be quiet, feeling that was somehow better than being in a well-organized child care center. I took her with me to the data lab. We had those old calculators where the numbers roll around and, on call, she would push the buttons; she would do the calculations for my research with me. Even though I had access to a full-time child care center, I felt very guilty about her being there--at ages two through four--and probably put her through more than she should have been put through, having her tag along with me at various places that were not fun for her. But we got through all that.

Then, while I was doing all this juggling with two jobs, I had a note on my desk to see Dan Price. I don't know if he said in the note or I had determined from others why he wanted to see me, but I learned he was looking for a research assistant and this would be full-time; it would be the full \$1,800. The money was very appealing, so I went around and asked what does he do. I knew of him

because his office was in the sociology building. He was a demographer but I really didn't know what demography was all about. I'd picked up the word at the Census Bureau, but it was really hard to know what it was. So I went to the dictionary--as I understand others have done; at least one other past PAA president--and looked up the word, and it still didn't tell me much. I wanted very much to say that I was interested in demography. I was interested in the money.

VDT: You were not interested in demography per se?

PRESSER: I didn't know if I was interested or not. And I teased Dan Price afterward, because he never did ask if I was interested in demography and I was so relieved. [Laughter] But he offered me the job and that was very significant for the rest of my career. So that's the answer to your question, how I got into demography.

VDT: Okay! And you did the master's in two years, at North Carolina. Did you have a thesis?

PRESSER: I had a master's thesis. What I did was work on a topic that became part of Dan Price's 1960 census monograph on blacks [Changing Characteristics of the Negro Population, 1969].

VDT: He was working on it already in 1961 or 62? How did he get the data so fast?

PRESSER: I don't know. My thesis covered the period 1920 to 1950 and he added the 1960 data later. I examined occupational trends for blacks and whites from 1920 to 1950, using census data. I learned then how you could make age cohorts over time with census data. I was working with published census volumes, not with tapes or anything modern like that, spending a lot of time in the library. That's when I took my daughter with me, going through the volumes there and copying lots of figures from them. A lot of it was manual copying--no xeroxing even!

VDT: And that became a chapter in his census monograph?

PRESSER: Yes. He updated the figures and he didn't use it verbatim. He used the data, but it was not a chapter that I had authored in the monograph. I mean, I didn't think of this possibility at the time, nor do I think he did. I probably could have done something to publish my thesis, but I wasn't thinking along the publication track at that time.

It was very interesting to work with him. He's a wonderful person to work with. He always had his door open; you could go in for any reason. I never realized how unusual that was.

Actually, graduate education at Chapel Hill was terrific that way; most faculty were very open and accessible. I never realized how fortunate I was in that regard until I went to Berkeley many years later. It was very nice for demography students working with Kingsley Davis, but students in general complained often about inaccessible faculty. If I had come to Berkeley never having been to Chapel Hill and had my first indoctrination into graduate school this way, I think it would have been a very cold first experience.

VDT: North Carolina had this caring approach?

PRESSER: That's right. The department [of sociology] was all in one building. There were receptions, various functions with students. We still called all our faculty by last names, Dr. So-and-so. Years after my Ph.D., I couldn't call Dan Price "Dan" without difficulty. There was no mixing between faculty and students at each other's homes or anything like that, which is more common now;

it certainly is here at Maryland, we have a lot of social mixing. But relative to Berkeley at that period, Chapel Hill was a very warm, nice place to be as a student.

And it was politically active. There was a lot going on then; there were the sit-ins in the South. When I was there, they started picketing the two movie theaters in the town, the only two movie theaters, because the owners would not allow blacks to come in.

VDT: And you were interested in that?

PRESSER: That was my first exposure to such issues, to political action of that kind--blacks. By the way, I mentioned that my master's thesis was on black occupational mobility as compared to whites. All my comparisons were black males relative to white males and black females relative white females. It never occurred to me then to study females in relation to males. Nor did it occur to anyone else at that time, I think, which is interesting. I had all that data on both sexes and we never thought about studying males versus females; it was always whites versus blacks within gender.

VDT: Well, perhaps it was a non-issue then. Black and white was enough to grapple with at that time.

Of course, this was before Chapel Hill got into population in a big way. The Carolina Population Center and so on came in the mid-1960s. But obviously, the seeds of demography were there, with people like Dan Price.

PRESSER: Yes. And they made it very appealing for me to stay. Although the beginning time was difficult--my first few weeks in getting established with an assistantship--when I said I wanted to leave after my master's, they made some very generous offers beyond what they do normally for students, offering me a research associate position at the Institute for Social Research, that would have paid more money than a student assistantship. Even Dr. Bowerman tried to get me to stay, thinking I was leaving because of how I was initially treated, which was not so--I would never have cut off my nose to spite my face. I think I thought I had come for my master's, I had gotten my master's. I was very young--only in my mid-twenties, with a child--and my high school friends were all in the big city and I wanted to go to New York.

VDT: So that's why you went to the Life Insurance Institute?

PRESSER: Dan Price, again being very instrumental, set up some interviews for me for jobs, with my master's degree. One was at the Russell Sage Foundation. It was really the job I wanted at that time. However, they were paying only \$6,000 a year and there was no way I could live there and pay child care costs, which were much greater in New York than in Chapel Hill, and my child support remained \$40 a month. The Institute of Life Insurance was \$7,500, I remember. It's interesting that I remember these figures; they were so important to me at the time.

VDT: Of course.

PRESSER: Survival. So I went to work at the Institute of Life Insurance. They had been doing summer workshops for teachers, educating them on family finance. The intent was that as they got more sophisticated with family finance, they'd get more sophisticated with insurance. This would carry over into the curriculum and, therefore, the Institute of Life Insurance, which is a trade association for all life insurance companies, would be having an impact in making both teachers and, more importantly, students more aware of the importance of life insurance. They'd been running this program for years, spending millions of dollars, and they had never evaluated it. So I was asked to do

a national evaluation--which was terrific for a person with just a master's degree--on the impact of the family finance workshops that they had in the summer. And who was my boss?--Al Hermalin!

VDT: Interesting!

PRESSER: Al Hermalin had only a master's degree then; he was a statistician. He was an associate director in the statistics division of the Institute of Life Insurance. He was a fantastic boss.

I had completed the field work for the survey and had worked there close to a year when the sociology meetings [American Sociological Association] were being held in Los Angeles. The Institute of Life Insurance was getting interested in funding some sociologists to do work on the family, seeing that as part of their agenda to develop social science research. Al may have had an important influence on that. Since I was going to the meeting, I was asked if I would talk to people like Reuben Hill, whom I met for the first time, and others whose research the Institute was funding, or thinking of funding.

So I went to Los Angeles and happened to go up after to San Francisco, for the first time in my life. Until this point I had only traveled on the East coast in the United States. I absolutely fell in love, not only with San Francisco but the Bay area. And I thought if I ever go back to school, which I had been thinking I might do at some point after my daughter was of school age, this is where I want to go. So I came back to New York after the meeting and wrote to Dan Price in Chapel Hill and asked, "Do you know anything about sociology at Berkeley?"

Well, Dan Price, unknown to me, was at the Center for Advanced Studies [in Behavioral Sciences] in Palo Alto, at Stanford. My letter got forwarded. He contacted Kingsley Davis and I either got a letter or a phone call offering me an opportunity to come right away. It was already September or October, so this meant for the January semester, the next semester. Judith Blake was at the School of Public Health at Berkeley when Dan contacted Kingsley Davis and apparently Judith and Kingsley had talked about Dan's call. It turned out that Dr. Yerushalmy--he was a very distinguished biostatistician in the School of Public Health; he's dead now--needed an assistant right away.

When I arrived in January, I had an assistantship in the School of Public Health even though I was a sociology student, because while I was at Chapel Hill I took a minor in mathematics. I did that because of the Census Bureau. When I worked there and they heard that I had never had calculus, my boss Jack Silver said, "You haven't been educated without calculus." So I took undergraduate calculus as a graduate student and decided to take a minor in mathematics at the graduate school. Mathematics at Chapel Hill was a very hard department to take a minor in, especially as a sociology major. I sweated that one out, but somehow made it. And when Yerushalmy saw I had a mathematics minor, he offered me this assistantship. I could be either a teaching assistant or a research assistant. I took the teaching assistantship because I thought the hours of work would be more clearly defined, which was important because of having a daughter and other responsibilities. I think it was the right choice. With a research assistantship, I know now, it's very hard to end work for the day. Yerushalmy was a wonderful man.

VDT: You went to Berkeley in January of 1964? You were just two years at the Institute of Life Insurance?

PRESSER: A year and a half, actually.

VDT: You took that job that was available right then?

PRESSER: Yes. I was really writing Dan about the following year, September 1964, but I went

earlier. My daughter was of kindergarten age, so child care to go to school meant kindergarten with an occasional babysitter after kindergarten, which was only half day. This way I could financially make it. This time I came with money being promised before I got there.

I worked for Yerushalmy one semester and then Kingsley Davis offered me . . .

VDT: You were teaching . . .

PRESSER: Biostatistics. I didn't have my own class; I was an assistant. I did the labs for Dr. Yerushalmy's class. At the end of that semester, Kingsley Davis offered me the departmental Population Council fellowship. This was a fellowship given to the departments which in turn decided who would receive it. You didn't have to apply to the Pop Council as a student, which was a great advantage. If you look at my CV, you'll see I had a Council fellowship for three years, which is generally not the case; you can only get a second year renewal. But the first year didn't count, as far as the Council was concerned.

An interesting thing about Pop Council fellowships . . . I did an evaluation of the fellowship program some years ago and noted, which people had forgotten, that when the fellowships were initially given in the 1960s, if you were a man and had a child or a wife, you could get additional support for them as a dependent. If you were a female, even if you were quote "head of household," you could not get extra money for a dependent child. I was happy to be told that they don't do that anymore.

VDT: Even though there were obviously some women like yourself who had a child?

PRESSER: A woman like myself was so happy to get a fellowship she would never complain about this. I felt it was unfair, but I would never write to complain because I'd fear losing the fellowship.

VDT: That's an interesting sidenote. I've interviewed Dudley Kirk for this years and he's so proud of the Pop Council fellowships, which he nurtured along. I thought very few of them, if any, went to Americans. I thought you had to be from an LDC or Paul Demeny, who was in a special situation. But you were an exception?

PRESSER: There were a few other Americans during the time I had the fellowship. I remember because we had an annual meeting in Washington and we had a picture taken and there were mostly developing country people but there were a few Americans in the picture. You'll be interviewing Larry Bumpass next year probably [PAA president in 1990]; he had a fellowship at the time I did.

VDT: Was he at Berkeley then?

PRESSER: No.

VDT: I was going to ask you about Larry later. Besides being successor presidents in PAA, you wrote some papers together.

PRESSER: That's a different track; that came much later.

VDT: What was the atmosphere like at Berkeley at that time? I've interviewed Ron Lee and he explained that you and Ruth Dixon and others were in the department of sociology but you were taking some courses in the Graduate Group in Demography, and of course the connection was there because

of Kingsley Davis.

PRESSER: It wasn't called the Graduate Group in Demography at the time.

VDT: That's true; I take that back. [Group in Demography.]

PRESSER: What it was, we took courses from Kingsley Davis and some students like Ron Lee were not in sociology. This is also related to my subsequently going to England. David Eversley, the British economic demographer, was visiting Berkeley one year and what he did, which was very good for all the students, was invite us regularly for gatherings at his home, which other faculty did not do. Berkeley was not a place for informal mixing between faculty and students in general, although demography was an exception. We had IPUR [International Population and Urban Research], our own research center, and that facilitated students getting to know each other pretty well. Kingsley let me have a desk there; even though I was on a Pop Council fellowship and not doing research for him, I was allowed to have a desk in the institute where all the demographers were. So demography was one of the few sub-areas in sociology where students got to know each other quite well. The student body was very large in sociology and it was hard to be in touch with faculty unless you were in a situation as we were where the faculty were in the same physical place. Faculty came to the office and shut their doors in the main sociology department, generally, so you typically didn't see faculty unless you were in their class.

When David Eversley came, he organized a regular seminar series and had all the people interested in demography, from whatever discipline, come to his home regularly for this series. And Sheila Johansson--her maiden name was Ryan; she wasn't married then--was going off to England to do her dissertation. At that time I was going off to Puerto Rico, but soon after, when I was in Puerto Rico, I got a letter from David Eversley, who had returned to England. I had earlier taken his course on economic demography, or something like that, taught in the economics department at Berkeley; it had a very historical perspective, a very different kind of course. I just audited it. I had also broken my leg skiing, so I had this big full leg cast on and David Eversley told me once he couldn't help but notice me every class, hobbling in; I had three months with a full leg cast. He knew me first from this class and he also knew me from the seminar that he ran at his home. He wrote me from England after he returned, saying that Kingsley Davis had indicated that I was close to getting my Ph.D. and would I be interested in a position at the University of Sussex. It was a new university and they had just created this position in demography, lecturer in demography, which is a tenured position; assistant lecturer is the lower level. This sounded interesting to me, although I indicated that I didn't want to make a permanent commitment, but I would be interested on a temporary basis. He urged me to take the permanent job and leave it after a year if I wanted to, because they did not want to create the job as a temporary job.

VDT: Let's backtrack a bit. You keep anticipating my questions. Going to Puerto Rico was, of course, at Kingsley's suggestion. Kingsley had been there in 1940, one of his first demographic excursions, he told me. What was the appeal of Puerto Rico?

PRESSER: I don't think it was appeal. I was scared, very scared. I had a young child; she was only six years old at the time and she'd been moving around a lot. I was very reluctant to do that. But on the appeal side, I had been doing class papers on Puerto Rico as a consequence of Kingsley Davis.

VDT: Kingsley had the connection still with Puerto Rico?

PRESSER: He had given me the name of Jose Janer, an older demographer, close to retirement. I was going down there to study what was happening to sterilization in Puerto Rico. We had no idea what data were available. Kingsley thought I might find a hospital that was doing sterilizations and do sort of an in-depth analysis of a particular hospital. The Pop Council funded me to go; they gave me extra money for this. However, when I got there, it was so expensive that I wrote Dudley Kirk telling him I had to put my daughter in private school because she couldn't go to the Spanish-speaking public school; the housing was far more expensive in San Juan than it was in Berkeley. I itemized my expenses, asking for \$50 a month more, and Dudley sent me \$100 a month. I never forgot that--it was the nicest gesture that had been made to me as a student, financially. It was really tough making ends meet.

At any rate, Jose Janer told me about this national island-wide survey that was recently completed, the 1965 round of the Master Sample Survey. They had been doing this survey regularly to measure health conditions on the island but they had asked about sterilization only in 1965, and he suggested I use that as a data set. That sounded great and I got access to the data tape. After the Department of Health closed for the day, they would let me use their sorting machines for the IBM cards. I started doing some analysis of the data this way and found that a third of women in Puerto Rico were sterilized. I couldn't believe it; it was really a surprise.

VDT: It turned out to be such an interesting dissertation, which was published in Kingsley's series of population monographs from IPUR, in 1973 [Sterilization and Fertility Decline in Puerto Rico, Population Monograph No. 13, Institute of International Studies, University of California, Berkeley, 1973]. He's very proud of those and they are still in every population library. Those orange monographs were very important and particularly yours. Was there any fuss made at the time about the fact that one-third of the women in Puerto Rico had been sterilized by the mid-1960s? Any media coverage, anything like that?

PRESSER: No, I never thought about publicizing it. I'm much more aware of the potential use of the media now than I was then. I was just doing my work. As a matter of fact, I was writing to Kingsley Davis and I was collecting all kinds of data because I didn't think what I had was enough, this Master Sample Survey. I was going to family planning clinics, collecting data on male vasectomies. I didn't know, as most graduate students don't know, when enough is enough for a dissertation. You don't know until you really get down to the nitty-gritty of writing it. Kingsley very appropriately told me to interview various people around the island, to make use of the fact that I was down there, so I interviewed hospital administrators all over the island about sterilization. One chapter of the book was on that.

But for the most part while I was down there, I didn't have a good sense of how significant the overall finding was. I wrote to Kingsley from Puerto Rico and the response was, like, "Keep up the good work," but I didn't know if he thought it was really good or not. When I came back, I remember complaining to him that I didn't get much feedback while I was down there and he said, "I thought you were doing great, so you didn't need it."

So I came back to Berkeley in the spring. By the way, the time I was at Berkeley--which we aren't talking about because it's not really relevant to demography per se--was the time that the free speech movement began.

VDT: That's right. Did you miss out on the student riots?

PRESSER: I missed out because I had broken my leg. When I broke my leg, that night I went into the university hospital. I had gone skiing for the first time, with skilled skiers; they really should not have let me go along with them on the slopes. I must say I was responsible, because I wanted to go

with them. I had rented skis, they weren't that good, and the ski didn't release when it should. I was taken back from Tahoe to Berkeley to the campus hospital and was in there when--I think the next day or the day after were the student riots on campus, the taking over of Sproul Hall. That was in November 1964, before Puerto Rico. I was in the hospital when all these students who were hurt by the police were coming in.

VDT: The real Berkeley riots, which really led off the student riots, the campus movement of the whole 1960s.

PRESSER: Right. It was a very exciting time to be there. Although, because of having so many constraints on my time--I either went to school or I had to take care of my daughter; I really couldn't afford to have babysitters and I didn't psychologically feel it was appropriate to use a babysitter so I could sit in the coffee shop and talk about the student movement. So there were constraints on my time. I think I would have been politically more active; I was certainly politically very interested in what was going on. That dominated student conversation and whatever; it was a very exciting time.

VDT: When you came back from Puerto Rico, there was still unrest but then you left immediately?

PRESSER: Yes. I came back in the spring, worked very hard on my dissertation, did as much as I could, but I didn't finish it then. I finished it actually when I came to the Pop Council [after Sussex]. I was teaching at Sussex full-time after Berkeley, so there was not that much time to work on it; I had hoped to finish it there.

VDT: In the book, Sterilization and Fertility Decline in Puerto Rico, you give credit to people who typed it and so on at Sussex, so obviously it was still on the go then.

PRESSER: Right, the writing.

VDT: You spent the year in Sussex [1966-67]. Why did you decide to leave? You went with the idea that it would be temporary?

PRESSER: I was a little lonely. If it had been London it would have been different, I think it would have been more exciting, I always liked big cities. That's one reason I left Chapel Hill, when I was a student; it wasn't a big city. I felt like an outsider as an American. I remember the assassination of Martin Luther King while I was there . . .

VDT: You mean they singled you out as an American because of that?

PRESSER: No, not because of that. But I felt like things were happening in the States; I was a foreigner in England. You feel marginal; I think everyone really does, most people who live in other countries, at least in the first few years. Also, they told me it was only an hour to London from Sussex, but they didn't tell me that the trains don't come back after ten o'clock at night, so if I ever went to London I'd have to stay overnight and I'd have to either take my daughter with me if I would spend an evening in London and get a sitter there, which was difficult, or hire someone to stay with her overnight in Sussex. It was not an easy place to be as a single person.

I lived in a very quaint village, called Piddinghoe; I think it's mentioned in a Kipling poem. It was really idyllic; I lived in Church Cottage, which was a renovated 18th century cottage. The owner liked to rent it to academics who would use it during the academic year and release it in the summer so they could get high rents from the summer tourists.

It was really a very good year and I felt that a second year wouldn't have the advantages that a first year had. I went there because I had never been abroad--California and Puerto Rico were my first big ventures--and had never been to Europe, and thought I would never have a chance again in my lifetime to go. That was my perspective at the time. And they would pay my fare and my daughter's fare and it was an exciting opportunity. But I never really thought of it as long-term. The question was should I stay two years or one; that was really the question.

When it got to be spring and I was thinking about coming back, I wrote to Dudley Kirk. He had interviewed me in Berkeley for my Population Council fellowship after the one-year fellowship I had from the department. He knew me from the interview and I later received a letter from him, also when I was in Puerto Rico, asking if I was interested in a job at the Population Council. By the way, I had also gotten an offer at the University of Texas at Austin, because that was where Dan Price and some of my fellow students from Chapel Hill now were--on the faculty at Texas. So there were choices for me. Even though the market in general for women was not that great, I think I had been thinking I had some good opportunities. Maybe the whole job market was better then than now, I don't know, but I didn't perceive getting my first job as a Ph.D. as difficult.

VDT: And no question of discrimination because you were a woman?

PRESSER: Well, there were little things. Like I was not interested in a job at Brown. However, Cal Goldscheider, who had come to Berkeley from Brown . . .

VDT: He was on your dissertation committee, I noticed.

PRESSER: Right. He came on late because he had come to Berkeley while I was in Puerto Rico, but I put him on my dissertation committee when I came back, since Bill Petersen had left. He said, "You really should apply to Brown because they are looking for someone." I said, "I don't know if I want to live in Providence." As I mentioned, I like big cities. But I applied anyway, really to satisfy him. Then I remember wondering why I never heard from them and he called them up and they said--he told me this; that was the first time I had heard this explicitly--he said he didn't understand why they felt this way and he was not sympathetic to this, but, he said, "They don't want to hire a single woman."

VDT: A single woman? You mean if you had been married that would have been okay?

PRESSER: Well, then I said, "You're damned if you do and damned if you don't." Because if you're married, they'll say you're more interested in your family than a career. Had I not had other opportunities, if I'd had to search more actively for a job, maybe I would have heard this more; I'm sure I would have. Even when I came back to the Pop Council or when I went to England, I never searched for alternatives.

I decided I was coming back to New York; I wanted to come back to New York. I asked the Pop Council if they were still interested. Parker Mauldin, whom I didn't know at the time, wrote back and said, "Yes, we are and this is what we can offer." And I took it, because I couldn't search for other jobs in New York while I was in Sussex, and I couldn't afford to come earlier and look things over. I took the job at the Population Council.

VDT: It was mainly to get yourself back to New York?

PRESSER: And it seemed like an interesting job.

VDT: And what was that job?

PRESSER: Well, accepting the job at a distance and without an interview, I wasn't sure what it was. I was the first Ph.D. female they'd had, at least in the New York City office; I don't know what their field operations were like. And Paul Williams was the first black, a black demographer with a Ph.D. from Chicago. We had very similar careers. We had worked professionally between our master's and Ph.D.s; we had some teaching experience, he at Vanderbilt just before he had come to the Council. We joked about the fact that he made \$500 a month more than I did. I learned an interesting lesson from this. Why did he make \$500 more? When he had gotten the job offer, he asked for a higher amount. It never occurred to me to ask for more than I was being offered. That was an interesting lesson that was very useful for the future. We had quite an experience at the Council, he and I; we compared notes often about whether being a black or a female Ph.D. there was worse, and the pros and cons of each. You have to remember--what year was this?

VDT: This was 1968-69, just before you went to Columbia.

PRESSER: Right. I came in September 1968, finished my dissertation on Puerto Rico within weeks of returning. I had the last chapter to do when I came back. I think most of the time completing it was spent typing; I was doing the final typing myself. We didn't have the word processor; there was lots of retyping and the tables. I had to get the manuscript into final form myself.

I also had an agreement with the Council that if I accepted their full-time research job I could also do part-time teaching elsewhere. Livingston College had just opened and they approached me.

VDT: I don't know that. That's in New York?

PRESSER: No, it's part of Rutgers University. It was a campus that was starting with a special interest in minorities, both Puerto Ricans and blacks. It was near New Brunswick [New Jersey, site of Rutgers]; it still exists. It was a very experimental school when it opened. They had very few regulations; they had girls living with their babies in dorms. It was quite an interesting time.

Irving Horowitz was the chair, a political sociologist, and I think he saw the advantage to having part-time faculty, which would not cost much and would mean having more faculty listed than they otherwise could afford as a brand-new department. So he made a nice offer which was more than was usually paid for part-time teaching and I was expected to be on campus one day a week. I mentioned that to Parker and he said, "Why go all the way out to Livingston? Columbia University is looking for someone to teach on a part-time basis." Well, I went to talk to them, at Columbia, and they made me an offer to come full-time. Parker later said he could have kicked himself, because he had told me about this opportunity.

So I accepted the Rutgers job part-time. I asked them at Columbia if this was acceptable to them and they said, fine. So I taught full-time at Columbia, which was mostly a research position, and part-time at Rutgers.

VDT: After the Pop Council, which only lasted a year, you decided you wanted to get back into the academic field?

PRESSER: Yes. It wasn't too pleasant as a female at the Council then. You were not allowed to travel as a female, when everybody else was traveling--the men. I was told it was not good for their international image, that women wouldn't be as well received as men. The head of the AID equivalent from Sweden would come to the Pop Council and she was a female, but that was what I was told.

There were a lot of disturbing things like that. I worked on a worldwide sterilization report ["Voluntary Sterilization: A World View," Report on Population/Family Planning, July 1970]. I could see why I was asked to do it, because I had done work on sterilization in Puerto Rico, but it was really reviewing and integrating a lot of literature. I was told by Joe Speidel and others that it was extremely important to their work. He regards it as my most important work, although I would never regard it as such, but it had some impact on changing attitudes more positively toward sterilization as a birth control method. Joe Speidel was then at AID and he found it useful. But I'm too much of a researcher to really want to do this type of work. I wanted to do my own research more. And it was clear that at the Council that was not something I would be allowed to do. And I wouldn't be allowed to travel.

Then one day, there was a memo in my in-box stating that there was a reorganization of the staff; titles would be changed. There were three women then on the professional staff: Emily Moore, who had a master's degree; Dorothy Nortman, who had a master's degree; and myself, who had a Ph.D. The three women were all made research associates and all the men were made staff associates. There was no explanation given for the difference in titles. There was Bill Seltzer, with his B.A. at the time, with far less experience than he obviously has now at the UN, he was changed from a research to a staff associate. There were other people--even Parker Mauldin has only a master's degree.

VDT: Has a master's only?

PRESSER: Parker has no Ph.D. It clearly wasn't the degree that was the reason. So I wanted to know why my title had been changed from staff associate to research associate.

VDT: Oh, you had been called a staff associate?

PRESSER: Yes, that was initially my title. But then the men who were called research associates were made staff associates and I as a female was made a research associate and the only ones who were left alone as research associates were Emily Moore and Dorothy Nortman.

I couldn't understand this and went around and talked with my colleagues, with whom I got along very well. There were people like Gavin Jones, who is now in Australia. There were lots of new people that were being hired around then. I liked my colleagues and there was no antagonism among us. I just say that the atmosphere at the Council was such that if you were a woman, certain policies were a little repressive. There was a whole fuss about wearing pantsuits. They tried to make a regulation that women were not allowed to wear pantsuits, which was the style then. To me at the time this was less serious than this issue about titles.

I had gotten this memo on titles just before the PAA meetings were to be held in Atlantic City [April 1969]. I couldn't believe the memo, so I went to see Parker and he was at the meetings already. I didn't want to bump into Parker at the meetings and bring this up because I knew that wasn't the place to bring up such an issue. So I decided to write a memo to Parker about this. I stated that given my education and years of experience relative to other professionals in the division, the criteria they had used to change the titles were unclear. All I could think of were two things: one, that they were dissatisfied with my performance and therefore were demoting me, in which case I resign, and, second, that it was sex discrimination, that they wanted all women to be research associates and men staff associates, in which case I resign. Then I said I hoped there was another reason that I was unaware of, and I was sorry that I couldn't speak to him--Parker--because he wasn't there. I left the memo on his desk and also copied it to David Sills, who was associate director.

VDT: Parker was your boss?

PRESSER: Yes, he was the director of the demography division. David Sills was the associate director. I had left a copy on his desk, not knowing that he had not left yet. He came into my office later and said, "I don't understand this either. Don't make any decision yet and we'll talk about it when you come back from the meetings." I was going to the PAA meetings the next day.

We had a staff meeting afterwards in which Parker said someone was unhappy about the title changes, not mentioning my name, but everybody knew it was me because I had talked to them all about this. A lot of people knew about the change in my title prior to the memo and said they hadn't told me because they knew I'd be upset. Well, I don't know what that resolved, but they didn't want to be the bearer of bad news. So when we had the staff meeting, I asked what the criteria were for the different job titles and nobody could come up with criteria that weren't gender-based. So they made everybody staff associates as a consequence.

I remember Dorothy Nortman thanking me afterwards. She felt held back. She had joined the Council prior to Parker. The title of staff associate was a way of upgrading her, rightly so, because she was certainly far more productive than some others with this title and had done a lot for the Pop Council.

Parker argued that the difference was that staff associates could travel; that was the only distinction he could make. But that was sex discrimination also, not permitting women to travel.

So for the time being, I decided to stay, and it was not until months later that I decided to go to Columbia University. But I think I saw my future as being very limited at the Council. I would never move up within the division. David Sills, after I was at Columbia, talked to me about coming back to the Council. I said I was happy in academia, thought I would stay, but if I ever went back to the Council it would be with the expectation that I could think about being an associate director. He ruled this out as a possibility, and that was the end of his recruitment effort.

VDT: It was ruled out even several years later?

PRESSER: This talk was about five or six years after I left the Council, after a strong research record and being a Board member of PAA. I said that if I were to come back I'd expect a higher status position than what I left. And he said, "Well, you don't want to do administration, do you?" And I said, "I think I would like administration, as well as research." This was not "working-out-the-details" talk, because I was not that keenly interested in returning. It was sort of "feeling-the-waters" talk; he had invited me over to talk about the possibility. It was nice that the Council wanted me to return. I did not leave the Council feeling I was pushed out. It was just that I didn't like the role I was given there; the future didn't look too promising.

VDT: Well, now you're at Columbia, teaching full-time, and part-time at Rutgers. Did you seek out the job at Livingston or did they come to you, because it sounded like the sort of place that you would be interested in, given your interest in discrimination, at least at that time?

PRESSER: I knew Irving Horowitz; I may have met him at Berkeley when he had been a visiting professor there. Later in New York, perhaps at a meeting, I may have mentioned I wanted to do some teaching. I don't remember actually pursuing the Livingston possibility; it somehow came my way.

By the way, I didn't feel that Livingston was the right place for me on a permanent basis. It was mostly undergraduate teaching and the department was a very radical sociology department, which meant anti-empiricism. Nevertheless, I felt I played an important role teaching population at the undergraduate level. I also was a key informant teaching family planning. The questions that students raised--many of whom were minority women, and they or their friends were having babies while they were in school--were often about controlling fertility.

Given the way the department was going, I decided to stop teaching there. Again, the money was important, because my daughter went to private school in New York City and so this helped to pay for private school. So when Matilda Riley heard I was going to stop teaching at Livingston and asked me to teach at Rutgers' New Brunswick campus, I agreed. I did this for two years, one day a week. It worked out very well. I only taught one course a year at Columbia, so my total teaching load was not heavy.

VDT: Let's talk about Columbia. How did you get the idea for the interesting survey of women who had had first births in 1970, 71 and 72 and who were interviewed in 1973, 74 and 76?

PRESSER: You've done your research.

VDT: Well, you published so many papers on it [timing of first births and teenage pregnancy] and I went back and read some of them now.

PRESSER: The idea of doing a New York City study was initiated by people at Columbia. Jeanne Ridley was director of the demography division at that time; Mindel Sheps had just left. It was the International Institute for the Study of Human Reproduction, funded largely by Ford. What made it very appealing to go there and have a, quote, "academic-type" position was that they provided a full-time secretary and a full-time research assistant, thanks to the Ford Foundation money. So it was a very good, supportive start for doing research.

The International Institute had a biomedical side and a demography/family planning side and Sam Wishik was the head of the family planning part. Mindel Sheps had been head of the demography part. She had left for Chapel Hill. Jane Menken had been there; Jane had left. Jeanne Ridley was still there and it was essentially Jeanne Ridley, myself, and Moni Nag. Moni was initially on the main campus most of the time and later moved up to the medical campus, where the Institute was located. So the demography division was very small.

This was the time when there was a lot of feeling around Columbia University that Columbia was doing all this international work and wasn't doing much for the local community. So someone at the Institute thought it would be a good idea to do a New York City study. They said it would be nice to do something on fertility; take it from there. I think they were thinking about a study focused on ethnicity, but I had just done this work on sterilization, both for Puerto Rico and for the Pop Council, the worldwide study.

VDT: The Commission on Population Growth and the American Future--you and Larry Bumpass?

PRESSER: No, that was after I had gotten to Columbia. I had done this worldwide study of sterilization for the Pop Council, as well as my Puerto Rican research. I felt that sterilization was important as a birth control method; it doesn't have many of the side effects that some other methods do and it clearly should be a voluntary option. But in terms of the status of women, it was controlling fertility at the end of women's reproductive careers, by definition. And to me the more important issue for the status of women--not necessarily for demography per se--was the timing of the first birth. I thought it would be more interesting to look at the consequences of the timing of the first birth and consider ethnic differences in this context. Now given how the field has moved on, it's hard to believe that no one was particularly interested in this issue at the time.

VDT: That's right, you certainly were ahead of the game.

PRESSER: I wasn't thinking of the timing of births strictly in terms of adolescent fertility, which shortly after became a major issue.

VDT: I was going to ask if it grew out of the concerns at that time about adolescent fertility.

PRESSER: That was part of it, but I really saw this as a more general issue. Demographers were focusing so much on family size and the number of births women had, but the more critical factor for women might be the timing of births and particularly the first birth, because in many ways, the first birth is the unplanned child more than the later children. So that must have some effect on women's status.

VDT: Well, of course, it grew out of your own experience.

PRESSER: Actually, my own experience shows that early first births don't necessarily hold you back.

VDT: Exactly, even if problematic.

PRESSER: Right, and an early first birth requires a lot more energy and motivation to compensate for than if you didn't have a child.

VDT: So you conceived the idea of focusing this study on that?

PRESSER: Yes, focusing on the timing of first birth and using New York City as a test population. The city provided a very diverse population to demonstrate that the timing of the first birth may have important consequences for women. The study also considered possible determinants.

I spent a lot of time developing the proposal and submitted it to NICHD, both as a contract and as a grant, because the contracts were general enough for most fertility studies and you could dually apply. The contracts were not as targeted as they are now to very specific subjects. You could submit your proposal for a contract as well as a grant review. The proposal was accepted by both, but the grant they offered was only for one year, because I was a new investigator and they didn't know me and wanted to see how I did in the first year before funding the second. The contract offered me two years.

I only mention this because I took the contract, thinking it was the better offer. Then all federal contracts to Columbia University got held up because they were not providing the federal government the requested information on affirmative action issues, such as the salaries of men and women they employed. There was a lot of tension between Columbia University and the government. Columbia was being stubborn in not wanting to comply with the federal regulations about affirmative action.

VDT: Which had gone into effect when--in the late 1960s?

PRESSER: Yes. And the federal government could respond to non-compliance by holding back all federal contracts, whereas they couldn't do this with grants because that type of money is given up front. So it turned out to be a mistake to take the contract, because it held up the research.

That was when I had an opportunity to collaborate with Larry Bumpass [e.g., "Demographic and Social Aspects of Contraceptive Sterilization in the United States: 1965-1970," in report of the Presidential Commission on Population Growth and the American Future, 1972]. Charlie Westoff called me from the Commission on Population Growth and the American Future and wanted to know if I'd be interested in analyzing the U.S. data on sterilization.

VDT: You put in your contract and you didn't get it at all?

PRESSER: We got it, but it got held up.

VDT: You must have done the report for the Population Commission in 1970 or 71, because they only existed from 70 to 72 and then the report came out.

PRESSER: Right. It took a while to write the NICHD proposal. I spent a full year writing the proposal, along with doing other research, and then it took time for review and funding. The award had been made when this problem with the university emerged, but it was being held up.

So when Charlie called about doing work on the U.S., I agreed to do it, never dreaming that we'd find such high figures. I believe it was the major method then, in 1970, for couples who wanted no more children. Now it's the major method for couples--period. The 1970 finding turned out to be a surprise. Larry Bumpass called me, because he knew about the data and that Charlie had asked me to analyze it and asked if he could collaborate with me.

VDT: The data of the National Fertility Study?

PRESSER: Yes. Larry had the tapes up and running at Wisconsin, and that was the primary reason I agreed.

VDT: He had it from Princeton?

PRESSER: Right. He had been at Princeton, moved to Wisconsin, and then he called me and asked if he could collaborate, which I agreed to. So that's how the collaboration happened. It was specifically to do that study.

VDT: And the abortion was sort of thrown in as an extra because abortion was such an issue then?

PRESSER: My research with Larry on abortion came about when Ortho Pharmaceuticals ran a conference to highlight the findings of the Population Commission. Charlie was organizing this and asked me to present a paper on sterilization and abortion. I asked Larry if he wanted to collaborate with me on this and he did [Bumpass and Presser, "The Increasing Acceptance of Sterilization and Abortion in the U.S.," in C.F. Westoff et al, eds., Toward the End of Growth: Population in America, 1972]. Charlie also asked me to present a paper on the consequences of perfect contraception.

VDT: "Perfect Fertility Control: Consequences for Women and the Family." That was how it came out, the chapter in Toward the End of Growth.

PRESSER: Right, that was the title they had given; I never really liked the title.

VDT: That conference was sort of simultaneous with the Population Commission?

PRESSER: It was held after the Commission was finished.

VDT: After the report came out and was almost totally ignored.

PRESSER: Yes. I didn't do any research on abortion for the Commission, but they wanted a paper on abortion for the conference.

Speaking of abortion, when I was at the Pop Council and Emily Moore was there--Emily was very instrumental in the early abortion movement. I believe she was one of the key people who started NARAL, the National Abortion Rights League; I don't know if it was called that then. I had just come back from England and was working at the Population Council. Here was this woman at the Council who was very much involved in the abortion issue, on her own time, and was forming an umbrella group of organizations that were interested in abortion. I remember being very impressed by what she was doing and wanting to learn more, but feeling I had to finish my dissertation first and then I could get more involved. And I did.

New York City was then considering reforming their abortion laws, which were very restrictive, and they were holding hearings down in city hall but they had no women testifying and no women on the Board of Health at the hearings. The Board agreed, under a lot of pressure, to add a woman to the group listening to the testimony and they put a Catholic nun on. So Emily Moore organized a group of women to go down to city hall to demonstrate against this. Several of us were from the Council. That was direct political activity. Berkeley was politically active, but I was watching on the sidelines. This was the start, for me, of getting into the action.

I remember they all said at the Council be clear to others that this was being done on your own time; it had nothing to do with the Council. They were very concerned about that. But we went down. It was quite interesting. Emily was extremely impressive talking with the press. We staged a walkout, which got a lot of media attention.

VDT: A walkout?

PRESSER: We picketed in front and then went inside to the hearings and then we staged a walkout. Which at first was a little distasteful to me, but I came to realize after that this was the only way that the issue would get attention. And it did. We were characterized on television as being in our teens, kid-like. But the interesting thing was how hard it was for Emily to get a group of women to do this; we were only ten or twelve, at the most. We were not all Pop Council people. Ruth Dixon was in town; she joined us.

VDT: Yours and Ruth's activism in women's issues started then?

PRESSER: Yes--at least that's true for me. Emily was also very involved in the formation of the National Organization for Women, which started in New York City. So that was my exposure then to the feminist movement, as a movement, having just come back from England. And it was a very good start. It was very fortunate that I had been exposed to someone like Emily at that time.

VDT: Back to your New York study. You plumbed it beautifully, because it just fed into the interest, as you yourself mentioned, the increasing interest in the early 1970s in teenage pregnancy. You then realized, of course, that this was a good vehicle for studying that too.

Then in 1976 you came to Maryland. What prompted that?

PRESSER: Well, one thing, my daughter graduated from high school, so the timing was right for a move. I had decided when I was in New York after all the moves I had put my daughter through that when she was going through high school she was going to stay in one place, so I was committed to staying in New York until she had graduated from high school.

I never really actively looked for jobs. At that time, by the way, Brown was interested in me

coming to Providence, which was an interesting thing--many years later wanting me to come.

VDT: Woman and all?

PRESSER: Yes. I think they were under some kind of legal problem also for not recruiting enough women, the university as a whole. I remember being sent some legal papers to sign off later on something about being in their pool of potential candidates, which was related to a class action suit against the university. But I think at that time they were genuinely interested in considering me for a position, and they did hire a woman; I don't know whether it was Fran Goldscheider or Barbara Anderson. The Brown story I was talking about earlier was the 1960s, but by the mid-1970s they were making a more concerted effort to hire women. I don't know if a woman's marital status in the 1970s was as relevant as it was when I was first considered.

But I had an offer at Maryland and I really loved Washington. I liked New York too. Again, a major job criterion was that it be in a large city, or very near one, like Berkeley being near San Francisco. And I had a lot of friends who either had moved here or I came to know by being on various committees that met in Washington and coming down often. I had come to an NICHD meeting in Washington that Ken Kammeyer also attended. He was the new chair of the department of sociology here at Maryland and wanted to build it up. He had just recruited Manny Rosenberg, who is a distinguished social psychologist. He also recruited David Segal from Michigan. Many full professors were being hired. I was just an associate professor for three years at Columbia and did not have full tenure. I had the equivalent of "tenure of title." This means that unless there was external support, which for me came from the Ford Foundation and my grants, there was no assurance that you would be paid by the university.

So the full professorship with tenure I was offered at Maryland was appealing. But tenure wasn't the reason I left. Allan Rosenfield, whom I liked a lot, had just come to Columbia to direct population activities at the Institute. He was very supportive of the work I was doing. Non-tenured senior positions are very characteristic of medical schools; they don't offer tenure to all their faculty. A lot of the physicians are also in private practice and don't really care. So often it's the chair of the department who has full tenure and maybe a few others. Allan came and they told him that they would create a tenured slot for him. He didn't have it the first year he came. When he heard I was leaving, he said he was willing to give up his slot for me. I say tenure wasn't the reason I left, because I remember explicitly saying to him, no, thank you very much, it was a lovely thing to have offered, but it wasn't the full reason. I was ready to move.

I had gotten the offer of a full professorship here--the first woman full professor ever in this department--and I loved the Washington area. There's a lot of demography going on here. So it wasn't that important to me that there wasn't much demography here at Maryland; Ken was offering an undergraduate course. I had decided that as long as there was departmental interest in demography, that was sufficient.

VDT: There were enough demographers in the Washington area?

PRESSER: Right, and that it is a really good demographic community in many ways. It's excellent, as you know.

VDT: Yes. Well, you established Maryland's credentials in demography, as a complement to Georgetown, and all the people in government, etc., in the Washington area. Is that what you consciously set out to do, to establish Maryland's credentials in demography?

PRESSER: I wasn't thinking when I came of building a major program, because I don't regard myself--or didn't then--as someone who would be interested in building a demography center. It wasn't that important to me to establish demography credentials for Maryland. I don't think Maryland was that interested in expanding in this area. Soon after I arrived, Ken Kammeyer stepped down as head of the department and Jerry Hage, who subsequently became chair, was not interested in developing this area.

VDT: Ken was the only other demographer?

PRESSER: He regards himself as a family sociologist interested in demography. So I didn't feel any special mandate at all from the department. The faculty liked the fact that I was generating research money and overhead and my release time from teaching was giving the department discretionary money for other activities. So that was the basis for any positive feelings about demography, because research money is generally more available in demography than some of the other sub-areas of sociology. That was viewed as positive, but there was not that much interest in the department or among the department chairs in particular for expansion of this area.

However, the year I was leaving to go out to the Center for Advanced Study at Stanford, we had hired a new chair, Bill Falk. I had been on the search committee. Bill is a rural sociologist and when we talked with him, he was very supportive of demography. I was away for the year at the Center [1986-87], with plenty of time to think, and was thinking about what I wanted to do the rest of my career. Did I want to be at a research center, if so, what kind of a research center? I realized that the ideal place that I wanted to be at didn't exist. That would be a place that was focused on what we now call our center, the Center on Population, Gender, and Social Inequality.

I was thinking that if the department wasn't interested in establishing this, I would do so, as a separate entity outside the university, or go somewhere else that was interested in creating it. I anticipated that Bill might have lost his enthusiasm for demography, that when other demands were made for departmental resources he'd forget his interest in demography. Well, it wasn't that way, fortunately. He was very keen on my building the program, and also setting aside new lines for hiring. We began by hiring Joan Kahn.

VDT: When you came back from Stanford [1987], you right away established this new center you'd conceived?

PRESSER: Yes. While I was there, I was talking to Bill about it and we agreed that we would hire a person while I was gone who would start the year I came back, which was Joan Kahn, who was a junior appointment. And that when I came back, we would hire a senior person, who was Jay Teachman. Now [end 1989] resources are very limited, but he's agreed to hire another assistant professor in demography, because he agrees we need someone who does research on developing countries, someone who combines population, gender, and development issues.

We were able to get a Hewlett grant for the Center--I wrote that proposal as soon as I came back--to give us money for graduate students and for a part-time programmer. That was important money, because it told the university that people outside think this is a good idea. This in turn helped us get money from the university. With our additional funding for research projects, things are going very well.

VDT: I wanted to ask what its role is.

PRESSER: We've had these monthly seminars that you've come to.

VDT: Religiously--except when I'm away. Obviously, the Center grew out of the research that you had been doing through the 1970s and 1980s, for which you've become well known. So let's talk a bit about that--your work on work schedules. It's all summarized in a way in your PAA presidential address this year, "Can We Make Time for Children? The Economy, Work Schedules, and Child Care" [Demography, November 1989]. You looked at shift work among dual-earner couples. Were you the first to look at that? How did you conceive of this way of looking at women in the labor force and child care?

PRESSER: I think it's a combination of interests. I think all these topics, like the timing of first births, work schedules, and child care, are combinations of personal, in that sense political, and intellectual interests. I was interested in demography and I got involved in fertility. I thought demography was interesting at Chapel Hill, but I really loved it when I got to Berkeley and focused on fertility because it had so much to do with women. It was very easy to read; I read not only because it was assigned but because it was intrinsically interesting. And it obviously had something to do with important personal issues as well--although the jargon is often artificial.

For example, in demography we talk a lot about fertility and births, but we don't often use the word children in our papers; we mean children but we don't talk about children and child care, even though the rearing of children has something to do with fertility. And the work place--we talk about labor force status but we don't talk that much about work-place issues.

My feeling is that to really understand demographic behavior, you have to conceptually extend some of these very crude, census-type variables into more meaningful dimensions. And now that we're conducting national surveys on such a broad scale, and writing our own questions and not having to rely as much on secondary analysis of census and vital registration data, it's a real opportunity to broaden our perspective of demographic behavior and get a better understanding of its causes and consequences.

VDT: It grew in part out of your own experience, as a woman and as a lone mother juggling work and child care. There weren't too many data. You said now you can make up your own surveys. One of your articles I re-read was the one you wrote with Wendy Baldwin back in 1980 ["Child Care as a Constraint on Employment," American Journal of Sociology, March 1980], using CPS [Current Population Survey] data. Have you managed to get some questions on the CPS?

PRESSER: Yes. The data on child care were for June 1977. I was on a committee formed by the National Center for Health Statistics on data needs to implement the Population Commission report recommendations. One of the Commission's recommendations, which I had helped formulate, had to do with child care. It was mentioned that demographers had to look more at its relation to fertility. So this was on my mind when the NCHS committee was formed. The committee was chaired by Mary Powers and Maurice Moore was a member. Maurice held the position at the Census Bureau that Martin O'Connell now holds, working with the June CPS fertility supplement. I mentioned to him that it was really important to look at the experience of raising the first child to understand fertility expectations after the first child is born and that it was too bad we didn't have such data. He indicated that the fertility supplement on the CPS was such that there was about two inches of space on the bottom of the page to add a few more questions.

So we went to Jerry Combs, who was then head of the demography branch of the Center for Population Research of NICHD, to see if he would be interested in funding the Census Bureau to do this. Maurice was more than willing to add some questions about child care to the June supplement, which asks fertility and fertility expectations so that the interrelationships can be explored. Jerry

Combs was a little reluctant. He particularly didn't like our question that had to do with whether or not child care was a constraint on employment. What would we do if we find that a lot of women say that child care is holding back their employment? It was viewed as a political question, measuring whether women felt child care was constraining their employment activities. Which in fact it was. Wendy Baldwin was very supportive of this and also she wanted to work on it. That's how Wendy got involved in the project.

VDT: Child care vis-a-vis employment issues?

PRESSER: Right. I believe Wendy played a very instrumental role in getting Jerry Combs, whom she worked for, to support this project. Actually, I drafted the proposal that the Census Bureau submitted to NICHD. I must say that Maurice Moore met with some resistance at the Census Bureau; some were not happy about doing child care research.

VDT: Even though they would collect the data?

PRESSER: They didn't think it was a legitimate Census Bureau topic. It was very hard to get the Census to go ahead and submit the proposal to NICHD, but finally they did. It's very interesting, because after the report came out, Martin O'Connell, who worked in Maurice's division and became head of the division when Maurice left, did a very nice descriptive report on child care in the United States.

VDT: From that 1977 CPS data?

PRESSER: Yes, and we had no other child care data of this kind. What Wendy and I did--and Mary Powers also did a paper based on those data--was look at child care in relation to employment and fertility in a more analytic way. But the Census Bureau did a descriptive report on the prevalence of child care. And they received tremendous positive feedback, so they then became interested and approached NICHD some years later to repeat the data collection in 1982.

Some child care professionals have complained about the limitations of these data, that it was collected in June, which isn't the best month, and all that stuff, and I remind them it wasn't meant to be a child care study. It was a fertility supplement with some child care questions. It's very telling that we had no other current data in the United States, that this became very important as a source of basic parameters on use of child care.

VDT: You mean the child care people would have liked it in March, with more labor force data?

PRESSER: Yes, when kids are clearly all in school; June is not the greatest time. And then we only ask this of women children under age five and they wanted it asked about older children. They just don't realize that, number one, it wasn't a child care study; number two, we had just a few inches across the bottom line of an interview schedule, which was very limiting.

This was a learning experience for me: that you can use this small amount of space for questions that would help to define problems and generate interest in more research. In other words, one could take advantage of piggybacking on other surveys, get some questions added, and they may not be all the questions you want, but people are not going to do all you want all at once. With a few questions, you could show that child care is a problem and that it is of interest to pursue further.

VDT: Then you did have something to do with the 1980, was it, data on the shift workers?

PRESSER: This interest came from analyzing the 1977 data and looking at the care of children when mothers were employed. I was doing a preliminary analysis for a paper and saw that 15 percent of employed women with children, regardless of marital status, were saying that the fathers were the primary caregivers when they were employed. I thought, 15 percent of employed fathers are taking care of children when mothers are employed? Who are these fathers, and who are these mothers?

I looked at several variables and when I examined detailed occupation, I saw that it was women nurses, women waitresses, telephone operators who disproportionately reported care by fathers. To do this, it was very clear that these dual-earner couples had to be staggering their work schedules, not necessarily purposely, but had different work schedules and that's how 15 percent of fathers could take care of kids. If you looked at particular occupations, like nurses, you had 30 percent or more of all nurses in the United States with preschool-aged children reporting that the father was the primary caregiver of the child. And if you consider that a lot of nurses work rotating shifts, there are probably many more fathers who are secondary caregivers.

So then I asked, "Gee, what do we know about work schedules in the United States?" I'd never thought about this before. I then learned that the Bureau of Labor Statistics had collected data from 1973 to 1980 on work schedules; it was actually the May supplement to the CPS on dual-jobholding; that's what they referred to it as.

VDT: Dual jobs?

PRESSER: Trying to learn more about people who had two or more jobs; that was the prime motive of their May supplement. They also happened in that supplement to ask the hours work begins and ends on the principal job. These findings were reported in a three- or four-page circular from the BLS, which indicated how many people work in the evenings or night, by selected variables. Until 1980 they didn't ask about shift rotation, which is important.

VDT: What does rotation mean?

PRESSER: That your hours change regularly. A nurse often works the morning shift first, then the evening shift, then the night shift--changing every few days or every week, whatever the schedule is. It's important to determine this because at the time of the interview, you can get someone who's working days but is a rotator and overestimate the number of people that have a regular day schedule because they're on the day shift the week that you're interviewing them. There's a lot of shift rotation in the United States. So you really underestimate the prevalence of shift worker or non-day employment that way. But in 1980 they corrected this and asked about rotation, and they included information about children in the household for the first time in the May 1980 CPS tape, including the age of the children.

Virginia Cain was a student of mine here at the time and was looking for an independent study project. I said I didn't have funding for a shift work project but I'd love for her to work with me on it, do a lot of the computing work, and perhaps coauthor a paper after we see what we find.

The May CPS data that the Bureau of Labor Statistics reported was all on individuals; the data tape wasn't set up to study families as a unit of analysis. So we had to use the ID numbers of all individuals and consider the relationship of everyone in the household to each other, whether the children were related to the "household head," etc., and create families. Virginia worked very hard and diligently with me to do this. We found that a third of dual-earner couples in the United States with preschool-aged children had very different work schedules, where only one of them was working regularly in the daytime.

VDT: This is fascinating. As I've said, you've always been right on top of issues, often at the leading edge. Were you aware that you were in this issue and, well, all the way back to sterilization and also in your timing of first births? Your research has been focused on things that have sort of anticipated what would become a policy concern?

PRESSER: I don't think of it that way. I just like to get into my data and think about things while I'm working that are not necessarily what other people are thinking of. It's fun to explore.

VDT: But you have also talked to policymakers. You've served on many panels. You're right now finishing up on the National Academy of Sciences Panel on Child Care Policy. Do you think it important for demographers to speak to policymakers?

PRESSER: I guess, yes, is the answer to that. I think what we're doing is policy-relevant. If we don't talk to them, someone else will, who will be less educated or informed on the subject, and this is our field. I felt this way being on the Academy panel. It's composed primarily of people in child development, and child care has a lot to do with the development of children; that's clearly an issue. But also there's a society out there and there are macro issues to address. For example, there's concern about women's employment that child development people are not especially interested in, how child care relates to that. I think there's really a need for more people to pick up on that and provide that perspective to policymakers.

VDT: More demographers?

PRESSER: Right. Demographers are in the right field for that because they have a national, macro perspective on issues that goes beyond what's happening within the family.

VDT: Toward the end of your PAA address you said there are people who are concerned about whether fathers should take more part in child care and you said demographers can give another perspective on that.

PRESSER: Right. We can be looking at what's happening in the society that is in fact structurally enabling fathers to do that. They are not necessarily taking odd-hour jobs because they can provide child care. Odd-hour jobs may be the only jobs they can get because of the changing economy. The growing diversity of work schedules is permitting the sharing of child care between husband and wife and between mother and grandmother, when all are employed. The workplace and child care are important societal issues. There's no doubt about that; they affect everybody.

VDT: And you're being sought out because you have looked at them, with this macro view.

And you've also felt it's important to talk to the press. You got quite a bit of press coverage on your dual-worker shifts.

PRESSER: Yes, there is continued interest. When the Science article came out, the first one on shift work [Presser and Cain, "Shift Work Among Dual-Earner Couples with Children," Science, February 18, 1983]--Science regularly distributes their table of contents to the press prior to publication and the press is eagerly waiting, so when each issue appears there is potential for tremendous coverage. Our findings appeared in The New York Times, the Wall Street Journal, Washington Post. What's interesting to me is the recycling of it. It got into the New York Times twice, into the Wall Street

Journal at least twice, the Post at least twice, and the Chicago Tribune several times, as well as in the Los Angeles Times. It usually is written as a short piece first in the main part of the paper and then someone does an elaboration in "Style," where they'll discuss their own interviews with shift work couples or whatever. It seems to be something that's not a one-time coverage item.

I find it has a lot of ramifications, that first you can focus on work schedules, then you can focus on child care. An issue that I want to pursue that came up as I was analyzing my data for the PAA talk was the fact that there's a lot of women with children who say the main reason that they're working irregular hours is to care for other family members, not their children, and many more may be doing so for secondary reasons. Care for the elderly, for example, which is a major interest now, may be facilitated by people working nonstandard hours.

Now, because I write about it a lot doesn't mean that I necessarily think it's all 100 percent great.

VDT: Your research or . . . ?

PRESSER: I mean it's fun writing about work schedules, but it doesn't mean that I think working nights is the greatest thing. It's part of the present situation, and there may be negative consequences. It's going on whether we like it or not and we better take a good look if we're interested in the family and the consequences of work schedules. On the physical side, there's growing evidence that it is not a good thing to work at night, particularly a rotating schedule.

VDT: You feel you haven't necessarily reached out to the press but that's come as a result of your research on these very interesting topics?

PRESSER: And the same journalists come back to me, like Carol Kleiman, who does the syndicated column for the Chicago Tribune. She does a sort of Ellen Goodman column, in the business pages. She covered this while I was out at Stanford, so it was several years ago. Now she just did it again last week from a different perspective. She focused more on the work schedule issue the first time and more on child care this time. It's the same person, that's what I find interesting,--it isn't just the same newspaper--the same person wanting to stay with it and repeating it in a different way.

VDT: I was very interested, having worked a lot with the press when I was at the Population Reference Bureau, which is one of the reasons for PRB's existence, in the fact that you had the session in the last PAA meeting [1989] on the press, called "Research, Policy and the Press." The only press person you actually had on the panel was Alan Otten from the Wall Street Journal, whom I knew. Unfortunately I missed it; I was interviewing Don Bogue; of course, he got press attention in the past. Art Haupt from PRB was there and said it was very good but very few were there. That's too bad. We have talked from time to time about PAA encouraging demographers to get their research into the media. How do you feel about that?

PRESSER: I feel very strongly about it. I think Gordon De Jong is doing a marvelous job right now with Demography.

VDT: Right, he's doing some press releases.

PRESSER: Right, he's taking each issue and getting press releases out [on individual articles].

VDT: You think he did one on the article in the latest issue, on cohabitation, by Larry Bumpass and Jim Sweet ["National Estimates of Cohabitation," Demography, November 1989]? They're getting so

much out of that marvelous National Survey of Families and Households. Their last article [Teresa Castro Martin and Larry L. Buypass, "Recent Trends in Marital Disruption," Demography 1989] reporting that two-thirds of [recent first] marriages will end in separation or divorce got a lot of coverage [data were from June 1985 CPS].

PRESSER: Right. Everybody's interested in that. As I say, we are the demographers; we should be saying this.

VDT: And write the press releases first. And Gordon De Jong is really doing it.

PRESSER: The other thing that's relevant right now for PAA, I think, is related to this. When we had the Board meeting in Baltimore last April [prior to main meeting], there was discussion, I think initiated by me, of shouldn't we be doing not only more press releases but thinking more of what we can do as an association to promote our discipline. In part we didn't get enough press interest in the meetings because we have no standard procedure that goes on about press releases of the meetings themselves. That's the first test.

VDT: Cynthia Green and I manned the one press release office we ever had, which was for the 1981 50th anniversary meeting.

PRESSER: I asked Jean Smith whether we should have a press office for that and she said PAA members don't provide press releases. Of course, they don't. The thing is that you need someone who can be in charge of it.

And that relates to the more general issue that we need people who can be in charge of a lot of issues. The PAA president does the meeting program, partly as president-elect, the presidential address, and there's a lot of other responsibilities. He or she really can't think about doing more than what has to be done. And the secretary-treasurer is loaded down with work. The officers' positions are not subsidized to provide release time at their institutions; we're all doing our teaching and our other responsibilities as well. What we really need, in my mind, is someone who can play the role of an executive director, who cares about the PAA and has the kind of interest that can provide continuity over the years.

VDT: That's an issue we'll get onto when we talk about PAA. You've established that, yes, you feel demographers need to get their research out into the press.

PRESSER: Not all demographers' research is policy-relevant, nor should it be. So I don't think every demographer has that responsibility.

VDT: But if you have something interesting, you should put it out, like Larry Bumpass and Jim Sweet getting out of their survey things that are of such interest, like cohabitation.

On some of the big questions I warned you I'd be asking. Who have been the leading influences in your career? You have mentioned Dan Price, whom you invited to be your introducer at the last meeting as president of PAA; it's become a convention that somebody who has been important in your career is asked to do that. And you have mentioned Kingsley Davis.

PRESSER: He gave me that first fellowship; he's certainly been supportive and influential. The reason that I think of Dan Price more is that came at an earlier and more critical point in my career, when I wasn't yet identifying myself as a demographer or professional. As I said, I had this 1950s

mentality; going back to school was a way of surviving at the time, it wasn't primarily to be a professional. And Dan got me the first job in the Institute of Life Insurance. He was the person who first nominated me to go to the Center for Advanced Studies.

VDT: Oh, he was--even in the 1980s?

PRESSER: He had nominated me earlier, in the mid-1970s. I was at first told it was highly likely I'd be invited, since I passed preliminary review and it was going to the board, but then the board didn't approve it. Few women had been Center fellows at that time. But an invitation re-emerged later. I don't know why I didn't get the invitation earlier, but Dan had nominated me earlier, so that was key. And he has always been supportive.

I got a beautiful letter from Kingsley Davis after the PAA address this year, saying that he thought the whole program and the address were among the best ever. And that was very important to me.

VDT: So he has been in on-going contact with you. I'm sure you're one of his leading students. Of course, I've asked that of him. I can't remember if you're on the list, but then he didn't have a list prepared for the interview as some people have had, like Phil Hauser. [In answer to the specific interview question on his "leading students," Kingsley Davis replied, "It's hard to say one is more outstanding than another; they do different things." But on editing the transcript, he added six names, including Harriet Presser's, to those of other students mentioned during the interview.]

You mentioned in passing, well, people at the Census Bureau. Who jumps out in your mind, like Dan Price and Kingsley Davis--influences on you, as a demographer?

PRESSER: There's a big gap between them and all the others, so it would be hard to spell out the others. Kingsley Davis was the demographer at Berkeley; Dan Price was the demographer at Chapel Hill.

VDT: What about Judith Blake, who was on your dissertation committee too?

PRESSER: I had just that one semester at the School of Public Health, where she was. I didn't take any of her courses; I audited some, but I never was a student of hers. This was before she had established her graduate program in demography. Before I left for Puerto Rico, she called and asked if I wanted to join that group, it was just starting, and I had already had all my sociology requirements then and there was no point in switching to another program. I thought being a sociologist and a demographer was better than being in the demography program and possibly having trouble with my credentials as a sociologist.

I would say that Kingsley Davis and Cal Goldscheider really were the ones that worked on my dissertation when I came back from Puerto Rico. But I had little time with Goldscheider, because it was only a few months, coming back and working on it. And Goldscheider and Kingsley Davis had some friction at that time between the two of them and Goldscheider left Berkeley.

VDT: Was that just a personality conflict?

PRESSER: I wasn't there at the time because I'd been in Puerto Rico most of the year and also, as I mentioned, I think most of us students were so far removed from what was going on with the faculty that I didn't know what was going on. Even if I had been there longer I probably wouldn't have known.

But after I had finished my dissertation and was at the Pop Council and revising it for a book, Goldscheider wrote me a letter with some comments that he'd had on my dissertation that he thought he wouldn't introduce earlier in terms of approval, because of the difficulty between him and Kingsley. He hadn't wanted to suggest these revisions earlier, as they would not necessarily be what Kingsley wanted for my dissertation. But he thought it might be helpful on the revision since it was going to be a published monograph. I thought that was considerate, because I'd have been torn if I had two different positions to consider and had to get my dissertation finished. And they were very constructive comments.

I would say, as a student, it really was Dan Price and Kingsley Davis--oh, and Bill Petersen, excuse me. He was at Berkeley. I took courses in the family from him, but of course he's a demographer and taught the family from a demographic perspective. He was extremely supportive of me as a student also. Dan Price was helpful in different ways at critical times, but in terms of just getting positive feedback, that you're doing well, that they really think well of you and keep it up, which is very important at graduate school, I was getting that very much from Bill Petersen.

VDT: This is the student-mentor relationship. Are there any of your colleagues who've been influential?

PRESSER: Well, you have to remember that I've not been in a population center with many demography colleagues. Jeanne Ridley at Columbia University was not helpful to my career. As for graduate students, I haven't been in a major place where there's been a high concentration of demography students.

VDT: I also note too--I was going to ask this when we got to your publications--that you usually publish on your own. Remember that content analysis of Demography a couple of years ago, over 25 years?

PRESSER: Yes, I think Jay Teachman did that.

VDT: Of course, he did [Jay D. Teachman and Kathleen Paasch, "The Sociology of Demography: A Content Analysis of the Journal," paper presented at the 1988 PAA meeting in the session on "Two Centuries after Malthus: The History of Demography"]. He found that very few women were single authors of articles in Demography, but you're an exception. Almost all your articles are yours alone. You have coauthored with Virginia Cain, Wendy Baldwin, Larry Bumpass, a few times, but generally you're the sole author. Is that explained by the fact that you worked as almost the only demographer in the places you've been?

PRESSER: Yes. And I think the research has been motivated by me, so that when it's been coauthored others have joined my project. Even with Larry Bumpass; I think at one time we exchanged authorship or we rotated it [senior author]. But essentially the projects have been mine in which people joined.

Now we just put in a project to do some research on Puerto Rico again, and that's the advantage now that Joan Kahn and Jay Teachman are here. We submitted a big joint project to the National Science Foundation. That will be different if we get funded and go on with it, because we're starting together and that's very different. That's a function of having colleagues at your institution. I think co-investigation is different when you're physically both in the same place. You can bounce things back and forth; it's better than when you're in different places. My minimal coauthoring is partly structurally determined by being the lone demographer in an institution.

VDT: Interesting point. Who have been some of your leading students?

PRESSER: I haven't had that many "leading" students. Several have taken non-academic paths; Virginia's one. Lilian Floge worked with me. She was a Columbia University student; she's now at Bowdoin. She does research on child care issues and I don't think is known so much as a demographer, although she identifies as a demographer. There were various students at Columbia University, but I don't think they would be known to demographers that much. Helen Ginn, one of my first students at Maryland, did a text on population with Ken Kammeyer. I had a student, Konia Kohelan, who is Liberian and is teaching at Eastern Shore and trying to get some demography going in a predominantly black school, he himself being black. I could list names, but I don't think . . .

VDT: Not yet that well known in demography?

PRESSER: Right. Again, I think that's a function of not being at a population center.

VDT: What do you consider your leading publications and why?

PRESSER: I was hoping you wouldn't ask me that one. I think you've highlighted the areas that I've focused my research on, but that's different from leading publications. My book that elaborated on the finding that a third of the women in Puerto Rico were sterilized [Sterilization and Fertility Decline in Puerto Rico, 1973]; my MMFQ article that argued that the timing of the first birth matters for women ["The Timing of the First Birth, Female Roles and Black Fertility," Milbank Memorial Fund Quarterly, July 1971]; the Science article that revealed that a third of American dual-earner couples with children work different shifts [Presser and Cain, "Shift Work Among Dual-Earner Couples with Children," Science, February 18, 1983]. These are discoveries that I am proud of.

VDT: And you have treated all those topics in several different articles.

PRESSER: And with different data sets. We discussed my role in getting the CPS questions on child care included in June 1977 and that led to a repeat in June 1982, with additional questions on work schedules. This played a significant role in having such questions included in the National Longitudinal Survey of Youth.

The limited but interesting findings at each stage generated a desire to know more. I see my role as legitimating these issues as demographic concerns and getting relevant questions added to data sets. The Wisconsin study, I played a role in having work schedule questions asked and I hope to analyze the data.

VDT: You got work schedules onto the National Survey of Families and Households?

PRESSER: The Wisconsin group clearly were responsible for agreeing to it, in doing it. I don't take full responsibility for it, but I certainly played a role in arguing for it and they agreed, so they get the credit.

VDT: You were on the committee for the questionnaire of that survey, I noticed. And you've done that also for the National Survey of Family Growth. Have you put some specific questions on that?

PRESSER: Well, for the most part they have not taken the advice I offered; they have other agendas. They did a bit on child care; they did very little on women's occupation. Their agendas are defined differently than mine, so I wasn't very successful.

I'm now on the Board of Overseers for the Panel Survey of Income Dynamics and just came back from Ann Arbor a couple of days ago. The Board had commissioned papers on the future of that survey because the current funding ends in 1991 and the question was where should they be going and what topical areas should they be adding.

VDT: They're still following the original 5000 families?

PRESSER: There are these families, but they're also doing all their offshoots. I believe they've done special studies on the parents and parents-in-law of the original respondents or family heads. It's a wonderful, complicated data set for sociologists interested in the family network--the giving of time as well as money between family members and issues like that. Sandy Hofferth's presentation on possible new areas in sociology and demography included the need for work schedule data, which I know she's sensitive to, doing work on child care, but the economists came out for this also. That was very pleasing; they really wanted more data on work schedules. I thought, "Okay, we've made it!"

VDT: That in a sense answers the next question: What accomplishments in your career to date have given you the most satisfaction? You've just said, perhaps, rousing the interest of people who can do something about it and these issues you . . .

PRESSER: It was very satisfying after the PAA presidential address ["Can We Make Time for Children?: The Economy, Work Schedules and Child Care," delivered March 31 at the 1989 PAA meeting in Baltimore]. You know, people will offer congratulations, etc., and that's satisfying. But what was especially pleasing to me was that several women whom I didn't know came up and said, "I never thought we'd have a presidential address on child care. Thank you so much." It was very touching. This really legitimated the area for them and, hopefully, other demographers.

VDT: They were into it themselves. Like Suzanne Bianchi, our current secretary-treasurer, whom I told you I just interviewed [then pregnant with her third child].

PRESSER: I told her I'd be most interested in what she had to say. [Laughter]

VDT: Well, she's going through it right now. By the way, where is your daughter Sheryl? We left her last . . .

PRESSER: In Puerto Rico.

VDT: No, we got her . . . you could leave New York because she'd finished high school. What did she do after that?

PRESSER: She's now a lawyer. She finished high school, went to college, graduated from Penn, in economics, then went to work. She writes very well and had thought initially of being a journalist and was, I think rightly, advised by some of my friends that she shouldn't major in English, she should major in a substantive area and would be more competitive as a journalist. She graduated from Penn and then worked for Facts-on-File as an associate editor. That is a major weekly economic news service in New York. It publishes a book of demographic facts, by the way. Then she went back to law school, graduated, worked for the Commission on Human Rights in New York. She's interested in public interest law, which is on the decline now. And she is now working for the city of New York in personnel, as an assistant general counsel.

VDT: She had a fabulous role model, and obviously nothing was neglected in her upbringing.

PRESSER: I think that's an overstatement. Speaking of role models, someone I haven't mentioned but who was very important to me is my own mother. I had a mother who thought the best thing for me would be to get married and have lots of children; that was the ideology in the 1950s. She had a very hard life and worked a lot herself and wanted my life to be easier. She was a very capable, competent woman. She was divorced with three children and had to fully support the three of us when we were young, although she subsequently remarried.

VDT: That's why you left New York [when Harriet was in the third grade] for Miami Beach?

PRESSER: No. My parents moved together to Miami Beach and then soon after, my mother and father were divorced and my father left the state and my mother had sole financial responsibility for three children. I never remember her ever saying anything negative about how tough her life was. She started with nothing and she ended up owning a hotel. She leased the snack bar behind that hotel for many years and learned the hotel business that way, initially. She was very competent and despite adverse conditions really made it, in a non-complaining, positive way. So I would say I had a great role model.

VDT: It's carried on through three generations--great! We're going to get on to PAA in a moment.

PRESSER: And discuss the Women's Caucus, which I really want to talk about.

VDT: Absolutely. My final question on your career and views on demography: What do you see as leading issues in U.S. demography over the years you've been involved? We've talked about those you worked on. For the future, what do you think the outlook is? You've talked about women and work. Low fertility, an aging society? An interesting point you made, you found many women who were choosing non-traditional work shifts had to look after other members of their family, not just children.

PRESSER: The care of the aged, I think, is a big issue that merits the attention of demographers. I think another important issue is why women aren't having the children they want; in developed countries that's the big issue. We used to talk about unwanted fertility, why women were having births they didn't want. Now I think the issue is why women aren't having the births they want. And that's timing on the other side. Instead of adolescent fertility, it's postponed childbearing as a problem. There's a large cohort of women who are not married or not having children but want them. I think that's a critically important issue that is going to lead to concerns about, quote, "deficit fertility."

VDT: Interesting. You haven't worked so much in Third World demography, though you mentioned that you now have a proposal in to go back to Puerto Rico.

PRESSER: And we're recruiting now at the Center for someone interested in Third World countries and I hope, again, that will lead to more collaborative work. I'm working with three students now on papers. One is occupational segregation in Puerto Rico. That's a separate issue; I'll be giving a paper on that at the next [1990] PAA meeting [Presser and Sunita Kishor, "Economic Development and Occupational Sex Segregation in Puerto Rico: 1950-1980"]. It taps into a general interest I have that I'd love to work on more and that is what happens to the status of women during the process of economic development. People studying women in development are asking the question, but there

aren't that many demographers. Ruth Dixon may be one of the few, and Karen Mason. I think it's a very important issue. You're getting this tremendous increase in "female-headed" households. By the way, I have to put household head in quotes. I played some role in getting the head of household concept off the census. I think that in developing countries the rise in father-absent families is a very important issue.

VDT: Let's talk about the outlook for demographers and demography. Do you think there's still room in demography in the U.S. for research as you have done or are the applied demographers on the rise now and that's where the jobs are?

PRESSER: What do you mean by that? I think of myself as an applied demographer. [Laughter]

VDT: Well, it's a matter of degree. By applied demographers, I mean in state and local government and business. Do you think there's still room in demography for academic, like yourself, researchers or are the majority of jobs going to be in applied, meaning business, demography in the future?

PRESSER: As a person in the Association, thinking of it as a discipline as opposed to a job market, I would say there's plenty of room to go all ways. My own research has sought to expand the definition of what are the relevant variables. I don't see a problem intellectually or in terms of the profession of defining demography to include a lot of what state and local people do, and business demography, and expanding in all areas.

Your question is, I think, what will the job market be like? I don't know. One of the reasons I'd like PAA to get an executive director is that the profession, demography, is so much bigger than the number of PAA members would suggest. Relative to other disciplines--people are always shocked that we only have 2,700 members [2,679 at end 1989; 2,752, end 1990].

VDT: That's not many. Should we reach out?

PRESSER: Not only just reach out but increase public awareness of our field. People think that PAA is a huge organization because demography is such a huge discipline in many ways--huge in terms of its impact on the issues of interest. We don't make the most of it because we don't have--I'm going back to the need for professionals in our home office. We can't charge enough dues to generate a large staff because we're just a small group of people. Relative to what other associations or disciplines have, we're not representing ourselves, in my mind, well enough in a public sense.

VDT: Now we're on to PAA and that's, of course, one of the main things I wanted to ask you about; you want to talk about the Women's Caucus. Let's jump into the PAA part of this, which is the reason this series of interviews got started. However, these careers are fascinating and how everybody networks with everybody else.

What was the first meeting you attended. Here's a list, the famous list of meetings that Andy Lunde put together.

PRESSER: It was in Philadelphia, I think in 1963.

VDT: Philadelphia was . . . 1963.

PRESSER: The reason I know was at that time I was doing this national survey of teachers who went to family finance workshops of the Institute of Life Insurance. I was going down to Philadelphia for

the Institute and Al Hermalin said, "The PAA is meeting in Philadelphia. While you're there, why don't you go to the meeting?" I had just finished my master's degree and I went to the meeting. I had other business there, so I didn't spend much time at the meeting. I remember this wonderful reception that we had at the museum [of the University of Pennsylvania] in Philadelphia. Were you there?

VDT: No, but it's been mentioned in several of these interviews.

PRESSER: That was my first; it was wonderful. I got to meet people that I had known about as a student and was awed by. I had the sense that I was one of very few females; I don't know how true that was, but I was very conscious of that. And I remember a panel with Charlie Westoff and Judith Blake on it and I remember thinking they were great. I don't remember what it was about, but I thought this was really something; it was terrific. At that time I hadn't yet decided to go to Berkeley, so it was just an impression.

VDT: What about other early PAA meetings you attended? San Francisco was 1964.

PRESSER: Right, I was out there then. Berkeley was the host, so we were in charge of the registration, all that stuff. I remember volunteering to be behind a desk and handing people their pre-registration packets. So then I put the names to people's faces and I loved that; that was really nice, participating in the meeting in that context.

I enjoyed all the meetings. I've gone to almost every meeting, except when I was in England that one year, and Puerto Rico, and there was one meeting when I was on the East coast and it was in San Diego [1982] and for various reasons, there was a period of family illnesses and deaths and I didn't go that year.

VDT: You've played a leading role in PAA for many years. It's interesting that you say you were aware of being one of the few women at your first meeting in 1963 and yet you must have been very much into women's issues. Let's now get onto the Women's Caucus, which, of course, you were very much into.

PRESSER: Yes, I was.

VDT: You wrote the first vignette in the PAA Affairs history series and it was on the Women's Caucus ["Vignettes of PAA History: The Women's Caucus," PAA Affairs, Winter 1981]. When Andy Lunde started the series, obviously it must have been a leading issue, because the first topic he thought of was the Women's Caucus and went to you to write that story.

PRESSER: It was good to do because I went through the files and put things together that might have gotten lost otherwise. I just re-read my writeup last night and thought, it was really a very interesting time. Someone noted in the introduction that I found it sort of frustrating that I couldn't communicate in this writeup the level of excitement and how radical it was to bring up women's issues in the PAA. [From the introduction to the vignette: "Harriet has indicated that this format has made it difficult to reveal the true level of excitement and frustration in trying to move the PAA forward on women's issues or in the extent to which the movement was regarded as radical."]

One of the first things we wanted to know was how many women were in PAA. I remember going to a session--I was not on the Board then, although I did go on the Board soon after [1972-75]. This was when the Women's Caucus was just formed [1970] and we wanted to know who the women in PAA were, how many we were, and whatever. We wanted some kind of mailing to the membership

to survey the sex of members. We had discussed it first at a Women's Caucus meeting to which many non-members came, to watch the crazy types--it was really radical to do these things. We knew as feminists what radical feminism was and we were not radical feminists. Radical feminism in the beginning of the women's movement was very hostile toward men and a lot of that ideology wasn't part of our ideology. We were interested in the status of women and as good demographers we really wanted data.

I remember someone got up at the session where this was discussed later, a distinguished demographer, I forget who he was, who said that we could not ask the sex--and ethnicity, which we also thought would be useful--of our people because it was too sensitive for a questionnaire. I remember that because my reaction was--I'd just gotten out of a session, abortion was still illegal in those days, the Supreme Court decision [Roe v. Wade, 1973] had not occurred, and we were having these sessions in PAA about asking women about illegal abortion, how to improve reporting of abortions they'd had. And these same people, who were saying we couldn't ask the gender and ethnicity of our members because it was too sensitive, had no qualms about using interviewers disguised as nurses and other devious means to get women to confess to their illegal abortions. [Laughter] It just struck me as bizarre that demographers were saying it's too sensitive to ask the sex and ethnicity of our own members.

VDT: You wanted a questionnaire, which you did have a few years later [sent to all PAA members in May 1973].

PRESSER: Right, asking those kinds of questions.

VDT: You wrote a nice story about how the first Women's Caucus meeting was at the Atlanta meetings in 1970 and you drew up three resolutions, which were not [allowed to be] presented to the membership until the next year [following review by the Board]. The first had to do with underrepresentation of women in graduate programs and in the profession [related to population studies]. The second resolution was no woman should be impelled to bear a child against her will; that came out in "free access of every women to contraception, sterilization, and abortion services." And the third resolution was that PAA resolved to support the development of non-familial roles for women, by encouraging research on and removal of "impediments to the full emancipation of women."

All three of these resolutions were shot down by the Board in 1971 [at the meetings in Washington, D.C.]. The first one was passed by the members at the business meeting [after rejection by the Board at its meeting the day before], by adding in discrimination on the basis of race as well as sex [and calling for a committee to study the extent of sex and race discrimination in the population field].

And in 1971, for the first time, you had a session on equality of women, "Equal Opportunity for Women: A Concern for Demographers," organized by Ruth Dixon, with one lone man on the panel.

PRESSER: That was Moni Nag, who agreed to be the discussant--reluctantly. He received a lot of negative feedback for participating in that session. You have to remember that when I went to my first PAA meeting and for many years subsequently, it was not common to see women on panels at all. And if they were on, the panels were never predominantly composed of women.

VDT: I wonder how that happened, because everyone stresses the fact that in the early history of PAA we had such outstanding women as presidents--Irene Taeuber, Margaret Hagood, Dorothy Thomas.

PRESSER: That was in the 1950s.

VDT: And then there was a gap, to Evelyn Kitagawa in 1977. Evelyn Kitagawa explains the fact of that gap--I haven't been able to interview her but somehow I've heard that she feels that the big gap from when those three were president [Taeuber, 1953-54; Hagood, 1954-55; Thomas, 1958-59] to herself in 1977 was because all the women had dropped out of higher education, graduate school; they were home raising the baby boom. What do you think it was?

PRESSER: Well, there still were women in PAA, if you look at the membership, and they were not prominent. So I don't think you can explain it that way. I think the old boy network operated and expectations of what women should be doing kept women down. I don't think it was conducive for women to be that visible, even if they'd wanted to.

Take Dorothy Nortman--that's a very interesting history at the Population Council; we don't have time for it--but I think there was a case of a woman who had done a lot in the field who never really got the recognition that she should have because she was a female during the times when it was her role to provide--and she will say that--support to all the others in the Council, to the other male members of the Council. She helped a lot of people write their papers, often not getting authorship on the paper. I asked her why she didn't insist and she said you didn't do that at the time and she thought that was how her job was defined. I think it was defined that way for me when I came, but I wouldn't have it that way.

VDT: And you think that was the case in PAA--during the 1960s and early 1970s, women were to be seen but not heard?

PRESSER: If you think about it, they weren't nominated--by an appointed committee--to office, so they're not going to get elected. Whoever organized sessions was not asking women to present papers. Think, why would there have been such a reaction to having this panel which were all women except one speaker? There had never been a panel with all women speakers [paper presenters].

VDT: This first one, in 1971?

PRESSER: Yes, it was considered really a radical thing. Moni Nag opened up his discussion by saying, "Despite what you might think, these are all very scholarly presentations." We got at him afterwards, why did he have to start off by saying that? And he said, "I think everyone thought it was going to be radical sloganeering or something and it wasn't going to be scholarly." Papers about women by women don't have to be non-scientific; they can be honest, academic research papers about women's issues. There were people who told him his career could be hurt by being a speaker.

VDT: Dudley Duncan in my interview with him talked about his and Beverly Duncan's positions at that time. She was very upset that women were rising up and saying PAA had discriminated against women; she didn't feel that--and the fact that PAA was taking what they considered political stands. They left PAA at that time, because of that.

PRESSER: I don't recall that in particular, but I remember some women speaking up at the time and saying this. If you look at the women who were speaking up and didn't identify with this, they were all associated with distinguished male demographers and had an asset that other women didn't have.

VDT: Had a what?

PRESSER: An asset. Do we all have to marry distinguished male demographers not to be discriminated against?

And if you think about it, where was Irene Taeuber? [Associated with Princeton's Office of Population Research but working out of her home and the Library of Congress in Washington.] You look at Judith Blake, she's in a school of public health. You look at distinguished women and where they've been placed; they haven't had major traditional disciplinary appointments. They've been on the fringe in their employment pattern, in the past. It's not true now.

Now, they may not have minded and it may have been what they wanted. What makes me happy about myself professionally is that I've exceeded my expectations of what I had earlier thought possible. Even if I were discriminated against in the blatant old-fashioned way--I don't feel I am now--but if I were discriminated against, the fact remains that I exceeded my expectations and society's expectations for a woman, given my cohort. These women may have been comparing themselves to their expectations and they went so far beyond what society and everybody wanted that they felt good about their careers as women.

But that may not be the way other women feel about it. Some delayed their careers because of their families. Deborah Freedman had Ron Freedman; she came to the field late. It was undoubtedly a great advantage to have his support and she does wonderful work. She would have done wonderful work on her own anyway, but she very well may not have been given the opportunity. I don't say she's one of that group, because I think she's been very supportive of women's issues in PAA. Judith Blake had Kingsley Davis. Take all those women. Who were the women that didn't have a distinguished husband. Who were the presidents?

VDT: Evelyn Kitagawa [and Margaret Hagood]. They were rare.

PRESSER: Right. So that's my view. And the other thing is I think there's a socialization, a consciousness-raising about what discrimination is and how it plays a role. It's very subtle and people may not be aware of the subtlety unless they've felt they've experienced it themselves.

VDT: You pointed out in your vignette the discrimination that was perhaps unintended in the first version of PAA's first "Careers in Demography" pamphlet [published in 1974]. The two examples were both male. When you did get a female in there after protesting, it turned out to be a consultant with a glamorous job that was rare for demographers, male or female.

Then there was the notorious case of women being kept out of the bar at the hotel in Toronto [1972 meeting]. That's my hometown and I've always been embarrassed about that; it was my first meeting. I wasn't aware that that happened, but I could well imagine it because Toronto was very backward [then].

PRESSER: Yes. Then we got the assurance that it wouldn't happen in New Orleans the next year, but it did. We called the press in on that. It was a men's grill. They had promised they would serve women before PAA agreed to meet at that hotel. We had heard that women did not get served in the grill when the sociology people [American Sociological Association] met there. So we anticipated we would not get served. We went to the grill as a group and sat at the table, we had a reservation, they did not serve us. The TV people came and we got media coverage on this. The hotel then changed its policy.

VDT: The policy of the Montelone Hotel in New Orleans--great!

PRESSER: It was the media attention, not the principle, that led to that.

VDT: And you also changed policy on the "head of household." You were one of the feminist group--you called them feminists then--who formed the Social Scientists in Population Research.

PRESSER: Right. I came down here in the fall of 1976. I loved Washington because I knew a lot of people here, including several good friends, and I realized that most of these people that I liked a lot did not know each other. I formed this group initially just so a lot of us who were doing work in population could meet and talk informally. We created our name after we got together.

At our first meeting at my house, we decided since we were very task-oriented professional women--government and academic--that we would focus our meetings on specific issues. Barbara Bergmann, whom I had recently met as a colleague here at the University of Maryland in the economics department, was then on the Census Advisory Committee of the American Economic Association and had asked me how demographers use the "head of household" concept as designated by the Census Bureau. There were women's groups that were protesting its use, NOW and other feminist groups. I said, "Gee, I never use 'head of household.' I'll go look it up." I looked it up and it defined head of household as whoever designated him/herself as such--no precise definition. I don't remember the exact words, but it was very ambiguous and the respondent was expected to know what was meant by "the head." However, if you were a wife, no matter what you thought, the Census classified the husband automatically as the head.

This is problematic strictly from a technical side. Take, for example, a two-generational household: a father, mother, and adult daughter are living there. If the mother says she's the head, it will be the father who's coded as head. If the daughter says she's the head, she could be coded head because she's not married. Which is correct? Why should it be the daughter or the father? In other words, where you have multi-generations there's no clear answer to that. And for married couples, what does it mean when the federal government insists only the husband can be the household head?

So our group called a special meeting to discuss this. NICHD was having an advisory committee meeting which Jane Menken and some other women were attending and they joined our group. We invited Paul Glick and Arthur Norton from the Census Bureau to come and talk on this issue. Norton elected not to come; Glick came. His interpretation as I've heard him tell it--he must have talked to you about it [He did; see Glick's interview above]--is a very different interpretation than we would have. I think when he came to meet with us--at Mary Grace Kovar's house--he was shocked at how many people were there, and we were all women. We basically wanted to know why the Census needed to designate a household head. There was really resistance to changing it because of wanting to keep things the same [comparable data] over the years, etc.

Actually, initially I didn't care that much about that issue. I was always defined as head of household, being the only adult member of my household for many years. I wasn't sensitive to the issue, but I became concerned when I looked more closely at the ambiguous nature of the concept. Technically, I didn't know what it meant. So we had thought we would meet with people from the Census Bureau before we made a bigger issue of it, so that we'd make sure as demographers that we weren't getting rid of something that was really important.

Essentially, what it came down to when we met with Paul was that they wanted a measure of who was "top dog" and I recall that phrase was used. So it was valued as a measure of authority structure. This explicit justification by Paul then mobilized those at the meeting to feel that if the Census wanted to measure who had the most authority, let them ask an authority question. And if it's not authority, ask what it is that you want to know--who owns or rents the house, who's the primary wage-earner, whatever. Be clear about what you mean when you say "head" so that we can be clear about what you get as a response. And that was really it.

Some of the people attending the meeting, Sandra Tangri for example, worked for the

Commission on Human Rights. Afterwards, the chair of the Commission on Human Rights, Arthur Fleming, wrote a very strong letter to the acting director of the Census Bureau, Robert Hagan, expressing concern about the concept as a human rights issue. The women in our group were members of different professional associations and mobilized a letter-writing campaign to the Census Bureau and Congresswoman Pat Schroeder. At the same time, Barbara Bergmann was on this Census Advisory Committee and she had raised the issue. She claims she was accused by Dan Levine at the Census Bureau of trying to destroy the 1980 census by pushing for this change. Several in our group met with Congresswoman Pat Schroeder, who was then chair of the congressional subcommittee that funded the Census Bureau. She got very interested in this issue.

At first the Census Bureau said they weren't going to change, but then they decided to drop the concept of head of household and to pretest alternative ways. Lots of other censuses in the world do not refer to a head of household--Canada, for example. We looked into this because we wanted to know did it make a difference, would it be wrong technically to change?

It's very interesting that the Census and BLS [Bureau of Labor Statistics] resisted dropping the concept, but having done so, nobody's yelling about how they can't do what they used to be able to do.

VDT: They were not clear on what they needed to know. That's great!

PRESSER: The other policy side of it, which I think is interesting, is that once we knew how to interact with Pat Schroeder's office, we were able to move on to other issues and get her support. She was very interested in family issues--still is--and interested in getting needed data. The major national data on child support that helped to make the case for the Child Support Enforcement Act was collected by the Census Bureau--for the first time--as a consequence of our group's efforts as well. Barbara Bergmann was the key person again. She was very much interested in child support and got our group to push for a supplement on the CPS on child support. We succeeded, although we didn't have a say over the specific questions. We lobbied Pat Schroeder to get this child support supplement, which she very much wanted also.

This was in 1979 [supplement to the April 1979 CPS]. It showed that a high percentage of unmarried women with children weren't even decreed child support, and of those who were decreed child support, a very low percentage were getting it. That's again an example of the significance of getting some basic parameters as a start, which, in turn, gets people interested in learning more. That's what findings from the supplement did; it generated a whole lot of interest in the topic. Now you look at the PAA program, child support's there, which was never a demographic issue.

I think that our group really had an impact. We disbanded over the years, after we started to rotate organizers. It was a very time-consuming activity for otherwise busy professionals.

VDT: What about the Women's Caucus, is it going to disband? In 1981 you wrote, "We still cannot take for granted that women's issues are recognized by demographers as an integral part of the study of population. Nor is sexism in the profession dead. The need for an active Women's Caucus continues." What do you feel about it now? Is gender a non-issue in PAA now?

PRESSER: It may be less of an issue, but I don't think it's ever a non-issue. I felt good about the PAA program this year [1989 meeting, arranged by Presser as 1988 president-elect and 1989 president]. Almost 40 percent of session organizers were women. I was very keen to have at least the same percentage as of women in the association. I think we got close to 40 percent, which is more than we are in the Association. The current percentage of women is somewhere in the 30s; I don't recall exactly what it is.

Session organizers play a major role. This is why I think women had gotten left out in the past,

because organizers select the papers and when they're almost all males, it makes a difference. It isn't just the topics selected that is relevant. In planning the program, you're constrained if you want to represent all of demography, but I think we had more papers on gender issues at the last [1989] meeting than we've had before. And when women present papers that do not focus on gender issues, gender nevertheless often enters into discussion of these issues. Every topic, even mortality, is gender relevant.

VDT: Even mortality? Especially mortality!

PRESSER: So I think one had to continue monitoring this. Knowing who's coming up as president, Larry Bumpass, I think that the representation of women won't be a controversial issue [at 1990 meeting]. Maybe, hopefully, it will never again be a controversial issue.

VDT: In Jay Siegel's interview for this series, we talked quite a bit about gender. I asked him how you get to be a PAA president. He said that in recent years it's helped to be a woman, that when you have a man and woman running against each other the woman wins, because all the women are going to vote for her outright. That's his claim.

PRESSER: But women are not the majority of PAA, so I don't understand.

VDT: You've just pointed out that they're only 30 some percent. Well, frankly, he used you running against Joe Stycos for an example; you were the two nominees for president in your year [1988 for 1989 president-elect]. He felt that Joe Stycos, who's older than you and been in the field longer, had lost out in his one chance to be PAA president, in a sense, because he ran against, well, a very popular demographer, but also you were a woman. He felt you were voted in, perhaps, because you were a woman. What do you think about that?

PRESSER: Well, it's hard to talk about my own election. Was Joe Stycos ever vice-president of PAA? If not, would Jay attribute this to Joe not being a woman? [Stycos has not been a PAA vice-president. He was a Board member for the term 1968-71.] I don't like the argument generally that women get elected because they're women, because you could say then that all men have gotten elected because they're men. And we don't say this, that the reason we had all these male presidents in the past is because they were male. Is Jay Siegel making a big thing about that?

VDT: Well, that's not quite how they were picked. He suggested [regarding presidents in general] name recognition, reputation, all their students will vote for them.

PRESSER: You could argue also that I have not been in a population center; I don't have all these students.

I really thought I was going to lose for exactly the reason that Jay said, that people would be saying that Joe Stycos might not have a chance to run again and I would. I have a high regard for Joe Stycos and I like him. So I didn't like running against him because you don't like to run against someone you like.

There are women who did not win in a recent election. And the other thing is--I don't mean this in my context but I would say this is how I used to think in the past, maybe less so now when things have opened up--when it is difficult for women to achieve, then the women who make it have to be better than average. If those same women are going to run against men, it isn't an asset to be a woman. They have to be distinctive in some way to have gotten where they were, more so than men on average

have to be. It's actually a put-down to say women win primarily because they're women, that merit doesn't play an important part. I don't feel I won because I'm a woman. I feel I won despite the fact that I am a woman.

VDT: Great. That takes care of that.

[**Editorial note:** Suzanne Bianchi, PAA secretary-treasurer 1987-90, reported (November 28, 1989, personal communication) that the count in this case was: 656 votes for Presser; 309 for Stycos. This was a greater margin than in three other counts of votes for president monitored by Bianchi, so the result was "clearly not a question of sex."]

VDT: Now about your tenure as president. I did want to ask you if you made special efforts in the program for your meeting in Baltimore and you pointed out that you made an effort to have women as session organizers. You obviously had a strong program committee. Besides your colleagues here at Maryland, Joan Kahn, Jay Teachman and others, you had Eduardo Arriaga, Susan Cochran, Henry David, Jeff Evans, Gerry Hendershot, and Charles Keely. Did they all really contribute?

PRESSER: Yes, we met in this very room [sociology department conference room], twice; we met and took on assignments, then we came back. We wanted to make sure that we listed all the key areas that we thought PSS programs should always have, so we could pass it on to Larry and others. If they wanted to use it, as PAA program chairs, it was at their discretion, but we thought we should proceed very systematically. A student of mine entered into the computer all the last five years' programs and we went over them to be sure we weren't omitting in our program an area that maybe should be covered. Each person took on a few of those areas and came in with suggested names for session organizers. That's when I asked them to think of women. When they had several names and we needed more women, I selected those names to call first.

The committee was very active in forming the program. It was not that active in implementing it. I had all the responsibility of contacting the organizers, getting things done on time, and getting people to send things in. By the way, we told the sessions organizers that we had made the titles fairly generic and, depending on the response, they could focus the titles more along what the papers were. So all those titles that you see were not necessarily specified to the organizers that way. Like Gwen Johnson-Ascadi; she was assigned a general session on mortality. It ended up being on reproduction and mortality, because the papers that she accepted were all focused on that issue.

VDT: Is that different from what it's been in the past, in your experience?

PRESSER: I don't know. I told Larry what we had done. One of the things I'm doing now is examining the change in the program over time. Since we have entered in the past five years, I've decided to go back 20 years and take the five-year period 1965 to 1969 and compare that with what the PAA did from 1985 to 1989 and we're going to submit it to PAA Affairs [see issue of Spring 1991]. It occurred to me that there were not as many papers submitted on family planning as there used to be; I remember those meetings years ago. You take a much harder look at the content of the program when you're program chair than when you're just a participant. It's now very heavy on the family and household demography. We created a lot more sessions because of so many papers coming in, but we allowed for this possibility in the initial planning. Now, 20 years ago, we also had fewer sessions and that does affect the diversity of the program.

VDT: But there were more in the late 1960s on family planning?

PRESSER: I think so; I'll be able to tell you empirically soon.

VDT: You had 84 sessions, like the year before, 1988, which is a record; eight overlapping at times. Did you nevertheless find that a lot of good papers had to be turned down?

PRESSER: I left slots open for sessions to be added. We had a couple of time slots left on Saturday if we wanted to use them. I only scheduled four on Saturday afternoon, so if we wanted to add more sessions, we could [and did; six sessions were held]. The default would be having fewer on Saturday afternoon, which are bad time slots anyway, so it wasn't a bad default. I asked every session organizer who claimed that they had many more papers than they could handle whether they had enough for another good session. Nobody who said yes didn't get their additional session. One of the problems I learned as program chair is that people don't like to reject the papers of their colleagues. They will write their colleagues what a great paper it is and then tell me it wasn't worth putting in another session for.

VDT: They tell the colleague, I regret I have too many . . .

PRESSER: And Harriet Presser says we can't have another session.

VDT: It's good enough for another session, but alas, no. Did you, like Ren Farley [1988 president and program chair], make a conscious effort to have some sessions with more give-and-take? He inaugurated the "Author-meets-critics" sessions and you had two of those too.

PRESSER: I thought it was a very good idea too.

VDT: I think he felt it was a conscious effort to get back to the give-and-take from the floor that the oldtimers recall about early PAA meetings and keep regretting there isn't more of now.

PRESSER: I put time slots on the program for all papers and left at least 15 to 20 minutes for discussion, which each organizer was told to allow. I really discouraged five papers a session, for that reason. There were to be four papers and more time for discussion. Nobody complained and I don't know what the feedback is about having or not having enough time for discussion.

I followed what Ren had written to his organizers and asked they they consider alternatives to a regular paper session, think more of a panel. Ren said his experience was the same as mine: that people want to give their papers. They don't really want to do panels. So that's a constraint.

VDT: Right. Some of the oldtimers, when commenting on panel sessions have said they've become paper sessions too. And people are not being encouraged to get up and make the talk that the early meetings encouraged. Do you think that's too bad?

PRESSER: Yes, I do; it would be nice to have. When I select what sessions to go to at a meeting, I try to go to sessions that I can't read the papers of later. In other words, sessions that are either going to have an interesting discussion or the design of the session is such that it's not just a series of papers you can read later. You're at the meeting, you have a lot of constraints on your time, you want to see people, so you try to attend sessions that produce a good exchange of ideas. I think that's important. It's very hard.

And the other thing is it's hard [to get good discussion going]. I heard that in the two sessions I

had on author-meets-critics, they were all just congratulating each other.

VDT: No, that's not true. I was at Frank Levy's "Dollars and Dreams" [The Changing American Income Distribution] and Easterlin made some constructive remarks. Of course, he was on the panel, but there were some from the floor.

PRESSER: Good, I'm glad to hear that.

VDT: It was a lively session, but unfortunately in too large a room. It was the one where you gave your presidential address. Population Today in its writeup criticized that particular room; it was too long.

These give-and-take sessions still happen in the Psychosocial Workshop, which I attend each year, but it doesn't seem to happen in any sessions of PAA. People regret that.

And, for the first time, you had workshops on Wednesday night. Do you think there are getting to be too many spinoffs?

PRESSER: I see these workshops as being in line with the kinds of things that meetings should be about. They produce exchanges that you can't read about after. Some [on Wednesday night] were on data sets, to educate the students as well as other people about existing data sets. There are only certain things you can do in the program. You don't want to give one region all the sessions; you have a limit on the number of sessions. The demographers studying China wanted to do more than what was on the program and I said if they wanted to take an odd slot like an evening, they could add a workshop to the program.

VDT: They had Thursday evening in addition [and a "Chinese Student Meeting" on Wednesday afternoon]. The last two years had Thursday evening workshops, but I was talking about those Wednesday ones, the ones that ran into the wine and beer party.

PRESSER: It seemed like the only other time to give. The alternative is not to have the workshops at all. So why not give them that. And it [Chinese demography workshop, Thursday evening] was very well attended, I understand.

VDT: Oh, packed--even before Tiananmen Square was on everyone's mind. You had a great alumni party dance. Everybody agreed that was the best ever. The year before was the first time they had put the alumni parties together.

Let's talk about current issues in PAA. A leading one, I understand from Suzanne Bianchi--and you've mentioned it--is who's going to do PAA's business, now that ASA will no longer be handling it.

PRESSER: I think for the record it has to be noted that the American Statistical Association has decided that they cannot handle us, so we have been pushed into a decision. I think what's emerging is a short-term crisis and a long-term gain. Because, as I mentioned, at the Board meeting earlier, before this crisis emerged, we had been talking about changes and I had formed a committee to consider ways of increasing our Washington presence. We had already had a committee meeting.

That committee was chaired by Jeff Evans and had Charlie Keely, Ren Farley, and Signe Wetrogan. We like to bring in the state and local demographers [Evans and Wetrogan are with the federal government]. We made a special effort to do that, because they had felt they were left out on committees. I included them on several committees this year.

VDT: Business members too--business committee members?

PRESSER: I put Hallie Kintner on the census advisory committee. I've been very sensitive to their concerns; I hope they see it.

VDT: Good point; I had that question. You met before to discuss whether there should be a Washington presence, that committee?

PRESSER: I was not on the committee but I formed it and they had decided that although there were a lot of things we could do, our best bet was to stay with the American Statistical Association.

Well, about a week after that committee report was completed, the American Statistical Association broke the news that they didn't want to have us anymore. They had management problems, I think, which reputedly were being exacerbated by handling us. It isn't strictly financial, because they're not asking us for more money. Actually they are, but just in the short run till we get out.

Over the years, there have been vibes about leaving AStatA. I remember at various times when I was on the Board, there were discussions about whether to stay with them or not. But at the last Board meeting in Baltimore, it was a different issue--not just management. We're getting bigger, particularly as a science rather than membership size, where we're really ready for a change. We're using the Population Resource Center for educating politicians about our field; they want more money to spend on their support staff to continue what they're doing for us. The demand on our finances and activities is growing, and yet we don't really have any clear organization to handle it. We have people doing all kinds of things, but don't really have someone overseeing all of it.

VDT: An executive director, in other words?

PRESSER: Now, we met yesterday with Bill D'Antonio of the American Sociological Association. They're very interested in pursuing the possibility of their handling our affairs. They're going to prepare a budget. We visited their quarters. They can give us two rooms, which might be fixed into one big room; with two people it might be more comfortable as one room.

The way we're talking now, we would have a half-time person, equivalent to an executive director, and a full-time administrative assistant, and have a lot of other services that would be handled in collaboration with ASocA. Unlike what we've had at AStatA, we'd have two different people; clearly half of a person would be fully ours and one other person would be totally ours. I don't know how the rest of the ASocA--their budget committee and council--will vote on this when we get all the figures and make a decision, but in my mind it's the right decision to be moving toward. And ultimately we should have an executive director doing this full-time. [In spring 1990, the management of PAA business affairs did indeed move to the American Sociological Association, with Jen Suter, working somewhat less than full-time, as PAA Administrator and Jenifer Kilroy--as of late 1990--working full-time as her assistant. They occupy an official PAA office--a first for PAA--on the fourth and top floor of the ASA headquarters on N Street N.W. in Washington, announced by a PAA plaque at the entrance to the building.]

VDT: Well, this is an important time in PAA history. It ranks along with the decision to move toward getting any paid help at all, which happened in the late 1960s.

PRESSER: We were getting away on the cheap. The reason everyone always went along with staying with the American Statistical Association was that they were able to do it for very little. We

were paying \$45 a year, until we just made an increase.

VDT: You mean everything that Jean Smith and Lee Decker and everyone else did cost \$45 a year?

PRESSER: Per person--of our dues.

VDT: How much are our dues? Not much more than that.

PRESSER: We were paying dues of \$45.

VDT: And all that work was being performed within the \$45?

PRESSER: That's right, and we could never do it any other way. No matter what the costs were in terms of our professional development or whatever, there was resistance to increasing our dues. Now the American Statistical Association has upped the costs of doing this for us and they're only doing it on a monthly basis until we find somewhere else. So we had to increase our dues recently to \$70.

VDT: Seventy dollars! I haven't had a notice of that.

PRESSER: I guess you haven't had a renewal yet. The renewals occur continually over the year, so yours may not yet be due. [My renewal notice, for \$70, had arrived when I got home from this interview. At least I'd been warned! JvdT.] And that's just the minimum. If we want to expand our activities substantially, we'd have to raise our dues much more. There's always been a resistance to go that way and we've sort of taken the route of going on the cheap. But, you know, we pay \$90 for IUSSP [\$100 in 1990; \$115 in 1991]. I pay, I think, \$130 for the American Sociological Association.

So PAA has always been relatively inexpensive, but we haven't been demanding much from the home office, other than printing and mailings. Everything we do is managed by officers and committees, and I think there's been a cost to the profession.

VDT: You said the flip side of that is to get more members and there should be an effort to recruit more PAA members from the much larger group of people who teach or work in demography.

PRESSER: But then you need someone who's going to be concerned about membership. We have Hewlett money which we never seem to be able to spend because we don't have anyone who can stay on top of it for a long time. We have concerns about getting minorities into the Association, yet we don't have anyone that can manage developing and implementing an outreach program to get minorities into the Association. It takes time and money. That's what an executive director who has some staff in Washington could do. There must be lots of other things that no one's had time to think about that we should be doing. Publicity, you mentioned earlier, is one thing.

VDT: A regular media outlet, right.

PRESSER: We're no longer a marginal association that's second to other disciplines. I think we have to think of ourselves differently today.

VDT: I think you're going to be a historic president in many ways, because if you make this shift it is a gain--a landmark.

PRESSER: In the long run, I think.

VDT: In PAA history. What will be your recommendations to the incoming president, Larry Bumpass? Obviously, of course, to move further along these lines you've just spoken about. And you've mentioned suggestions on the meetings. Do you still enjoy the meetings? Obviously, you do.

PRESSER: More now than ever before; I won't have to plan it again! I saw these great smiles on ex-presidents at the last meeting and I said, "I know why you're smiling; it's over."

VDT: It is a very hard year. Since 1976 presidents have been responsible for the program; before then it was the first vice-president--plus your presidential address, plus all your own work.

PRESSER: And a very important additional activity now is having to be concerned about where to go from the American Statistical Association.

VDT: How do you get time to do all these things? You really have many irons in the fire. Of course, no longer child care.

PRESSER: Now I only study child care. [Laughter]

VDT: Let me quote Dan Price's remark on you, on being goal-oriented; it's appropriate. He said [in introduction to Presser's 1989 presidential address]: "If I had to choose one concept to characterize Harriet Presser, it would be 'goal-oriented'--determination. Her education did not come smoothly for her. Her college education was interrupted by periods of work as well as her graduate education, but she never lost sight of her goals." I think that's been true of you all the way through. This has been great. Thank you very much.

PRESSER: I've enjoyed it.

ADDENDA

VDT: Harriet has brought me to lunch in a lovely dining room, with a pretty inner-courtyard garden, where we're the last people, at the Adult Education Center. You say you have NCHS committee meetings here?

PRESSER: Not committee meetings. We just meet informally with NCHS people here often for lunch.

VDT: Harriet has been talking about some interesting things she's been doing in research and her contacts with people in other organizations.

PRESSER: In September, I went to an international symposium on night work and shift work, in Italy. It's a group formed by the International Commission on Occupational Health and Safety, I believe, that's been meeting every two years in different countries, for many years. I was invited to go to their meetings after the Science article appeared [1983, on shift work]. Don Tepas, the U.S. representative, indicated that they were not trying to expand their attendance but were interested in my perspective, the demographic perspective, which was not there. There is biomedical as well as social science, but the latter are mostly psychologists. They are mostly non-Americans. A lot of the interest in night work and shift work is outside the U.S.

My first meeting was in Poland, two years ago. They alternate between Eastern and Western Europe, although they've met in Japan and Australia. They are a group that does mostly experimental work looking at the consequences for individuals of night work and shift work. They're interested in issues like the effect of light on vital rhythms, all these issues relating to the circadian rhythm. They have no sense in general of what the prevalence of shiftworkers is in their countries. They have samples of 5, 20; they talk about rural-urban differences. However, on the biomedical side it's really fascinating.

I love being in such meetings where nobody really knows you. They think of you as a token sociologist as well as demographer, because there's no sociologist there either. They're really dealing with important issues and they're receptive when you talk about how the context is broader than the laboratory. It's interesting. It's nice to be able to expand beyond the demographic and sociological audience. This has happened with the child care issue as well.

VDT: One thing about the Women's Caucus we didn't mention--it's in your vignette--is how a couple of times there were no women candidates for the Board and the Caucus proposed them. Suzanne Bianchi mentioned that she had been one of those proposed [and elected] in one of those years.

PRESSER: Wendy Baldwin was elected, as a nominee from the Women's Caucus.

VDT: I remember that. And you say that Evelyn Kitagawa . . .

PRESSER: You mentioned Evelyn Kitagawa as a person who didn't have a distinguished husband in demography and yet made president and I questioned whether or not she would have been nominated had there not been concerns directed by the Women's Caucus about the fact that women weren't getting nominated. She rightly deserved it, but a woman might not have come back into the presidency at that time had there not been the women's movement--and the PAA Women's Caucus.

VDT: How do you account for the earlier ones who got by without the women's movement--Irene Taeuber? Well, she had a husband, but not Dorothy Thomas; she picked him up but I don't think he helped her.

PRESSER: Dorothy Thomas goes way back and I think things probably were better then. In the 1950s and 1960s, women had a real setback in terms of higher education.

VDT: Not in the 1950s; that's when these three women were president.

PRESSER: But their careers began much earlier. The percent of women in higher education dropped after World War II.

VDT: They were not there, prepared to take leading roles, because they hadn't gone on for doctorates, is that what you're saying?

PRESSER: I'm saying that before World War II, I think things were better. Then there was a setback, in the 1950s and 1960s, that prevailed through all the associations, in the attitudes about women and where women should be.

VDT: That was part of the togetherness after World War II, everybody seemed to go back to home, hearth, and family.

PRESSER: I read something about Margaret Sanger--you probably know the details better--that she was actively involved in setting up the first world conference on population; she was told specifically to remove her name or people wouldn't come.

VDT: The conference in 1927 in Geneva. It was never hidden that she was the one who called that. But Raymond Pearl was brought in to give her [scientific] support and the IUSSP grew out of that and he became the first executive director.

But, of course, there's the famous story about the beginning of PAA. She was instrumental in getting a first meeting to organize it [May 7, 1931].

PRESSER: Yet I saw the notes of the meeting and there's hardly anything on her input at the meeting.

VDT: Not only that, but her name was put up for vice-president and she was told to remove it. But in this case it was not because she was a woman; it was because she was an activist. They didn't want to get into birth control.

PRESSER: You don't think the others were concerned about birth control?

VDT: They felt that would muddy the pure scientific waters. Frank Notestein wrote about that several times. We ran it as one of the history vignettes ["Keeping PAA Professionally Pure," PAA Affairs, Summer 1983]. Margaret Sanger's birth control activism, like the idea that PAA should come out in favor of legalizing abortion, was considered a political stand that PAA should stay out of. I think that was in part what people felt about the resolution on women in the profession, that it was PAA taking a political stand which traditionally they had not done. And from the very first meeting, when they asked Margaret Sanger--Henry Pratt Fairchild became the president and it was he or somebody [Frederick Osborn] who got up and suggested that she not run for vice-president.

PRESSER: That's interesting; I don't know enough about that.

VDT: What are you going to do about the new [1989] directory?

PRESSER: We have already moved on this. When we sent out the announcement of sessions for the next [1990] PAA meeting, we put in a flyer that announced that the [first 1989] PAA directory had numerous errors in it and that we were either going to have an addendum or issue a new one. If people had any changes to make, to send them to the PAA office immediately. This would include new addresses, not just errors, which were mostly errors of telephone numbers, due to a computer mistake in the printing of the directory.

VDT: What I regret very much in this latest directory is that it didn't have what people's degrees were in. That was very important for me. Only the degree years were given.

PRESSER: Why is it important to have the field?

VDT: In my case, it was important to know what your three degrees were in. Of course, I found you out in Who's Who. Petersen [Biographies of Demographers] doesn't list degrees at all.

PRESSER: I've never seen my listing in Petersen. I've never looked up Who's Who either.

VDT: Well, you're there.

PRESSER: That I know, because I sent them some requested details.

VDT: Eventually, I think, the archives of PAA will have to go into PAA's business office. Maybe not. Of course, they should be in the controlled climate that they have at a library. They're in the archive section of Georgetown's library. At the moment, they're squashed in behind the papers of Harry Hopkins. Also, I've got all these materials now in my home, boxes and boxes of tapes, transcripts, etc., which need eventually to go into the PAA archives. I'm not sure Georgetown will have more space. Originally, there was somebody specifically to look after our archives; there's nobody now. So I have not sent things there in the past few years. They regularly get PAA Affairs and Demography, but they throw them in the last box.

PRESSER: I mentioned that the committee formed to consider increasing our organizational presence in Washington decided to stay with the American Statistical Association. But in that report, they listed many options. One was to get someone at a university to be secretary-treasurer, but to do more than our secretary-treasurer currently does, to offer to subsidize them by giving them half or part of their salary for release time, and making the secretary-treasurer's role stronger, so it would be more equivalent to someone who would take care of all the materials of the PAA and would play the role of almost an executive director. Which was an interesting idea.

VDT: Almost the executive director's role.

PRESSER: Except there would be some things that you'd have to be in Washington for; it facilitates things to have someone in Washington. Like our work with the Population Resource Center.

VDT: They were thinking this could be someone not in Washington?

PRESSER: Right. Apparently other associations do that. The secretary-treasurers are at various academic institutions and they are subsidized and play a much greater role than ours. There's just so much that Suzanne Bianchi or any secretary can do with the minimal release time they're given and a lot of other responsibilities.

VDT: Some of our earlier secretary-treasurers were at academic institutions, not in Washington, like Dan Price [1956-59]. But Andy Lunde [secretary-treasurer 1965-68] explained that they realized they had to be in Washington, in part because they then made the association with the American Statistical Association [in 1967] and had to be here, because of the business attachment. And the government offices at that stage [1960s] were the only ones that would allow them to do PAA work on office time and they had the backup secretaries. No academic institution could afford to do that.

PRESSER: As I just said, PAA would pay them to do that--one of the alternatives.

Another alternative discussed by the Board recently--and this might be interesting, because we don't know where we're going yet--is to go with a management association and polish our image a bit; hire someone who really knows how to build up membership, how to generate money for our mailing list, whatever.

VDT: You mean a management association that would give us advice and we would go in that direction or that would manage us?

PRESSER: That would manage us. But these management places don't offer executive directors; they just take care of the different things that we need, sub-contract it or do it themselves. They don't require us to rent physical space, so we could avoid having to pay rent that way. They're not cheap.

VDT: We're talking about Harriet being on the Council of the Population Section of the American Public Health Association [1978-83; chair in 1983]. This came up when we said that in APHA if you belong to the Population Section, you have to belong to the whole of APHA, whereas in the Population and Environment Division of the American Psychological Association it still costs just five dollars and you don't have to join the mother entity.

PRESSER: And this was said in the context of PAA dues going up from \$45 to \$70, that may affect membership.

VDT: A lot of people may drop out when they're suddenly faced with this near doubling. [But PAA membership actually increased following the dues increase and transfer of business management from the American Statistical Association to the American Sociological Association, from 2,679 at the end of 1989 to 2,752 at the end of 1990.]

PRESSER: Going back to the APHA, when I did the program for that section, we had one session, again organized by Ruth Dixon, on what it was like for women in establishment population organizations. It was a very good session. It was women in Pathfinder, the Population Council, etc. This was in the mid-1970s, I believe.

Essentially what women were saying was that the feminists thought they were not taking enough action on women's issues and the women in these associations felt they were making a lot of waves given the nature of the organizations and they were caught in between. This theme was repeated consistently. Women's self-help health groups were very active in their efforts to empower women, which was not the goal of the population organizations. So the women were in a double-bind, because as members of the population organizations they felt they were banging their heads against the wall in their efforts for change. Yet there were complaints from feminists outside these organizations that they weren't doing enough.

It was a very interesting session at APHA and Ruth ran it beautifully. It was very informal, like we discussed earlier--what meetings should be like, what panels should be like. There was a lot of interchange; people were being very honest about their problems, not worrying about how it might sound to those not sympathetic to women's issues. I thought up the session, but Ruth did a marvelous job.

VDT: Ruth Dixon has been in the forefront. She chaired the Women's Caucus session at your meeting this year ["The Women's Movement and Reproductive Rights"].

PRESSER: Yes. Although we were students together [in the sociology department at Berkeley], Ruth attributes her going into demography to me. We were together in a class on factor analysis and she was contemplating her dissertation topic. I said, "Go talk to Kingsley Davis," and that did it for her. She went to talk to Kingsley and decided to be a demographer.

VDT: She just talked to him, didn't take a course? A number of people have said taking a course with him was what did it for them.

LARRY L. BUMPASS

PAA President in 1990 (No. 53). Interview with Jean van der Tak during the PAA annual meeting, Omni Shoreham Hotel, Washington, D.C., March 21, 1991.

CAREER HIGHLIGHTS: Larry Bumpass was born in Detroit, Michigan, and grew up in a suburb of Detroit. He obtained all three of his degrees in sociology: the B.A. from Wheaton College in 1963, the M.A. from the University of Michigan in 1965, and the Ph.D. from Michigan in 1968. He was research assistant to Ronald Freedman during his graduate student years at Michigan. In 1967 he joined the Office of Population Research at Princeton, where he worked on the final phase of the Princeton Fertility Study and on the National Fertility Study. Since 1970 he has been at the University of Wisconsin, where he is the Norman B. Ryder Professor of Sociology and is associated with the Center for Demography and Ecology, of which he was director from 1977 to 1980. Among other activities, he has been a fellow at the Economic Commission for Europe in Geneva (1974), chair of the Population and Social Science Study Section of the National Institutes of Health (1978-80), chair of the Population Council's International Research Awards Program on the Determinants of Fertility in Developing Countries (1981-85), on the Board of Overseers of the Michigan Panel Study of Income Dynamics (1984-87), and director of the National Survey of Families and Households (since 1986). He was editor of Demography from 1978 to 1981.

Larry Bumpass has been a leader in the study of U.S. fertility since his graduate student years and now in the area of family demography. He is author of a prodigious number of journal articles on these topics and coauthor or coeditor of the monographs, The Later Years of Childbearing (with Charles Westoff, 1970), Social Demography (with Karl Taeuber and James Sweet, 1978), and the 1980 census monograph, American Families and Households (with James Sweet, 1988).

VDT: How did you first become interested in demography and especially in fertility, which was your first interest? We'll get later onto your current interest in the family.

BUMPASS: I think that's an interesting story in how the NICHD training grants can work. It's a system that many of us now still try to use to advantage, in that I knew very little about population before I went to Michigan. I, in fact, only intended to get a master's degree at Michigan.

VDT: Why did you choose Michigan?

BUMPASS: In my undergraduate years at Wheaton College, I took my junior year at the University of Michigan--in part for financial reasons, because of the cost of a private school--and was excited by what I saw there. I took courses with Robert Cooley Angel, who was about 75 then; he was a close relative of Charles Horton Cooley. I took courses in anthropology and sociology. I had a very good experience in that junior year, so I decided to apply for a master's in sociology. At the time, I thought I wanted to go back to a small undergraduate institution and teach, and from the very limited perspective I had then, getting a master's degree was all it would take to achieve that objective.

The Michigan Population Studies Center did as we still do in recruiting. They went through the files and decided my grades were good enough at Wheaton that they could gamble on me, so they offered me a full fellowship if I would take one population course a semester and work on the project I was assigned to.

VDT: This [1963] was just about two years after the Population Studies Center had been formed

[1962]?

BUMPASS: That's correct. Obviously, they had just received these training grant positions.

VDT: From NICHD? Population research hadn't started there yet. [The Center for Population Research at the National Institute of Child Health and Human Development, NICHD, was established in August 1968.]

BUMPASS: I'm not sure of the auspices of that fellowship. Here at Wisconsin not too long after that they [NICHD] had a general medical science training grant, providing similar-type fellowships. There was a time later on when I was a Population Council fellow at Michigan. I believe this first position was a federally funded NIH mechanism of one variety or another. From my perspective at the time, all that meant was groceries on the table, my tuition paid, and I was willing to take a population course a semester.

VDT: You were already married by then?

BUMPASS: I was married; I married after junior year.

VDT: Was your wife working then?

BUMPASS: She was working at the Institute for Survey Research. Well, eventually she did; at that time, she was working for the School of Nursing.

The short story is that I was excited by what I did and by the people, who were obviously very exciting at Michigan at that time. So I didn't drop off the conveyor belt at the master's level, and I actively became a part of that community, working with Ron Freedman and Lolagene Coombs and taking courses from Amos Hawley, Dudley Duncan, David Goldberg, and a number of others.

VDT: At Michigan, I've heard from several sources, you were a member of what Freedman--to quote him exactly--said was "a very unusual cohort over a couple of years, a cohort that reassembled at Wisconsin," including Jim Sweet, who got his doctorate the same year you did, Robert Hauser, David Featherman, and Doris Slesinger. Norman Ryder, who established the Center for Demography and Ecology at Wisconsin in the early 1960s, said he went for Michigan graduates "because they were the best." Freedman said at one point he counted 17 Michigan PhDs in Wisconsin's sociology department. How do you account for that happenstance? Such an unusual cohort--a large proportion of you became leaders in the field.

BUMPASS: There are two parts to the answer, I think. One is the intellectual excitement and the sense of discovery and the growth of the population field at the time we were there. It was a very unique period. Ron Freedman was just in the early field stages of the Taiwan experiments. Dudley Duncan was greatly excited about path analysis and its application to the Occupational Changes in a Generation Study [resulting in The American Occupational Structure, Duncan and Peter Blau, 1967, and other publications]. Amos Hawley's work on human ecology was captivating to many of us. Certainly, the intellectual climate was a very fertile one.

That having been said, I think the most demographically informed answer is that you get random runs and it was just a stroke of good luck that we all happened to share that together and that so many of us ended up close friends at another institution for most of our productive careers. Up until Dave Featherman went off to New York to be president of the Social Science Research Council, the four of us have run daily along the lakefront--Dave Featherman, Jim Sweet, Bob Hauser and I. Not

Doris; there's another story about Doris.

When I got to Michigan, Doris was a few years ahead of me. She took some years out later for family reasons. About my second year of graduate student life at Michigan, I was supervised by Doris on a research project under Ron Freedman, so Doris taught me the ropes. And I took over a position that she had held in managing some questions on the Survey of Consumer Finance that Ron Freedman had added. Then there was a spell when Doris was a student of ours at Wisconsin before she finally finished her Ph.D. Now, of course, Doris has been chair of the Department of Rural Sociology at Wisconsin.

VDT: Ron Freedman had a paper at the PAA meeting in 1988 on the "Michigan Model of Graduate School Training of Demographers." I think you must have repeated this at Wisconsin. He made four points. Students who will eventually become creative leaders in the field are largely self-selected. The center can provide them with technical training and role models to inspire them to go beyond technique, but shouldn't interfere too much. That's the first approach, he said.

The second one is that you had the apprenticeship system, hands-on apprenticeship, so you "hit the ground running," after finishing. You worked with a professor in research--and you just mentioned that Doris was working with you on some research with Ron Freedman. Was that the one where you ended up coauthoring a paper with him on fertility expectations ["Fertility Expectations in the United States: 1962-64"] in 1966 already in Population Index?

BUMPASS: That's right. It was the mechanics of seeing the survey coding done and sort of overseeing the field operation at that level that I learned from Doris. I would produce tables and then Ron would have me write comments on what I saw in them, and then we would have a regular meeting where we would discuss what I saw and whether I was right or wrong about that and what else one ought to see in them.

VDT: You were just a first-year graduate student?

BUMPASS: I think this was by the second year that we were doing this. Much of the first year was spent in the real nuts and bolts of calculating tables on these old Marchand calculators, that would grind away forever to create just ratios, and in actually coding data from the Detroit Area Survey. A lot of real nuts and bolts, how-the-work's-done, in the first year, and then more of this professionalization into how to interpret data beginning, I think, in the second year.

VDT: Ron's third point about the Michigan model was the opportunity for interchange between students and faculty and each other; he said graduate students train each other as much as any. There were the twice-daily coffee breaks and students having desks in the same building as the faculty.

And the fourth thing was that Michigan always stressed, was strongly associated with, the use of survey data in population analysis. You obviously got your training early on. You were with the Detroit Area Study?

BUMPASS: First with the Detroit Area Study--this famous study that Arland Thornton and Deborah Freedman continue to publish out of on a regular basis. These are the same women; I was involved in, I believe, the second wave.

VDT: The callbacks. They were first interviewed in 1961, and reinterviewed by phone three times, and then picked up again in 1977.

BUMPASS: That's right. So the second wave, which would have been 1963, was the wave that I started off my graduate career coding and working on with Lolagene Coombs.

The paper that you mentioned that Ron and I did came out of the other work. There were actually papers from both streams of work, but one of the first papers came out of the questions on expected family size that were added to the Survey of Consumer Finances, done by the Survey Research Center. Again, that was one of those things where it was just my great good luck to be in on the early stages of excitement about some things in the field. This was 1963. That was only eight years removed from the first wave of the Growth of American Families surveys. In fact, the second wave [1960] had just been completed and there was the whole excitement or exploration of what can be learned from asking expectations data about fertility. Which, of course, came out of the great embarrassment of demographers at having missed the baby boom and wanting to find other methods besides extrapolation to try to figure out what's going to happen in the future. It was in that historical context that all this work with expected family size was getting under way and I was able to learn the trade in the midst of this.

VDT: What was your master's thesis, or did you have a thesis? And then your doctoral thesis?

BUMPASS: Michigan has had, and still has, a policy that doesn't require a master's thesis. You participate in the Detroit Area Study; you get hands-on experience with survey research of a kind that I think is absolutely invaluable. I think the survey work I do now in helping to oversee a project [National Survey of Families and Households] with 13,000 face-to-face interviews--none of which, of course, I do--is informed constantly by that experience I had as a student of going door to door . . .

VDT: You did?

BUMPASS: Yes, that was part of the required thing, to actually do the interviews.

VDT: Your DAS wasn't the one that had phonebacks?

BUMPASS: No. My DAS--and this, of course, just happens to be where you are in the system, and I can't tell you who was running it; it escapes me at the moment--but it was concerned with international attitudes. That was the topic of the year. And that generated some very bizarre circumstances, like the interview I tried to conduct with the 75-year-old grandmother from Arkansas who had just arrived in Detroit. She had never read a newspaper, never listened to a news program, and I was trying to interview her on her attitudes toward countries she'd never heard of--while she had a son in the room who had been in the military and knew about these places and he wanted to answer the questions. I had to keep telling him that he was not the chosen respondent; I couldn't talk to him; would he please be quiet. And then I kept recording the "don't knows" for this lady for the remainder of the interview.

And there were others, like one with a nearly deaf man, where I would shout the questions, his wife would repeat after me, shouting them, and then he would answer. So I came to a real empathy for what life was like in the field, from those very limited experiences. I fear our students don't get enough of that kind of really nuts-and-bolts experience with what the process is like.

VDT: So they can be skeptical of the data?

BUMPASS: That's right. And appreciate what it takes to get good data. I learned early on a maxim that's still very much a part of our lives: how terribly difficult it can be to get good reporting on income, because of the enormous personal resistance. One respondent I interviewed in those days was

from Eastern Europe and was paranoid about state oversight or intrusion and, all attempts on my part notwithstanding, just simply would not divulge any kind of information on income.

VDT: So that was one of your requirements for the master's. What about your doctoral dissertation?

BUMPASS: My dissertation meets a couple of principles--I guess I shouldn't ascribe them too much to the people whom I've heard articulate them, though I will hold Jim Sweet to one, whom I've often heard say: "The best dissertation is one that's finished."

VDT: I have a son whose dissertation is dragging on forever, so I can appreciate that one!

BUMPASS: Dissertations are obviously a very important part of graduate training, but I think it's very important for students to view them as a serious final graduate exercise, rather than as the work that's going to stand the field on edge. Occasionally, students are either smart enough or lucky enough to do that. But in retrospect, I find my dissertation rather pedestrian and perhaps even misguided, but it was carefully done and in the process I learned a lot of the trade. So it wasn't a waste.

VDT: What was the topic?

BUMPASS: It was on the effect of age at marriage on education differentials in fertility.

VDT: From data from where?

BUMPASS: That's where the great experience came, because I worked with data from all three, at the time, national surveys of family growth--the 1955 and 1960 Growth of American Families surveys and the 1965 National Fertility Study. So I gained a familiarity with all three of those data sets. I learned a lot about managing multiple data sets, and I learned first hand about sampling theory, because you don't get the same results necessarily . . . There's a lesson there that I guess informs everything that I do yet, in that a finding in any survey is only suggestive. I have a compulsion yet to always look for another source of data and try to get the same answer.

VDT: I noticed that in re-reading some of your publications. You said at one point that replication is often recommended in social science but seldom done. You learned to do that early on.

Who was your supervisor on the dissertation--Ron Freedman?

BUMPASS: No, Ron went off to the Center for Advanced Study [in the Behavioral Sciences] in Palo Alto, I believe, in my dissertation year. I had worked with Dave Goldberg on a number of related projects over that middle graduate period, so Dave served as the advisor on my dissertation.

VDT: Did you work with the Duncans? You mentioned that you took courses from Dudley and he was there working on path analysis.

BUMPASS: I took several courses from Dudley. I had the opportunity to interact with him on a regular basis, both because of the way the Michigan system was set up and because he and Beverly would have students to their home on a regular basis.

I also learned an enormous amount from Beverly Duncan. Beverly was one who if you would go in and ask her a question that seemed simple enough on the surface, like, "How do I find out this from census or vital statistics data?", she would then march you off to the appropriate source and spend

an hour or two hours, or however long it took, to make sure you understood how to do what it was you were trying to do. While she was at Michigan she never occupied a teaching position--she was a research associate--but she was a serious teacher to a great many of us.

VDT: She took on that role on her own initiative?

BUMPASS: Yes, it was just the kind of person she was. It wasn't an assignment; it wasn't official; it wasn't recognized in any official way. It's just the way it was.

VDT: That's interesting. I've heard a lot about the Duncans being "parents" of so many of you Michigan students. You were never associated with Ron Freedman's work in Taiwan?

BUMPASS: Never with the Taiwanese work, other than sitting in his introductory population class at the time when he spoke with such obvious enthusiasm about the work, as he would be just coming back and Tom Sun, director of the work in Taichung, would be there at the time.

My line of work at that time was solely U.S.-directed and I stayed on that trajectory for about ten years at least. And I'm still obviously primarily in U.S.-based work, but over the last ten years I've had an increasing number of non-U.S.-related activities.

VDT: Yes, there has been your work on Korean fertility, and what you're doing with the Japanese colleague you're meeting this Saturday afternoon.

What took you to Princeton? Now, I must quote Ron Freedman again [from his interview of June 12, 1989]. He said Charlie Westoff asked you to work on the National Fertility Study. That he, Freedman, had offered you a job at Michigan, but the Princeton opportunity came and he said, " `You should go there because there's a different tradition there; it's a great place.' I didn't try to keep him or sway him." But then Ron Freedman told Westoff, "Charlie, we must talk baseball here. You got my prize first baseman. You've got to give me somebody in return." And then Al Hermalin went to Michigan and you went to Princeton.

BUMPASS: I think that's a close reconstruction of history, infused with a little bit of affection on Ron's part, but it's close. I do remember when Ron called me into his office and said that Charlie Westoff--of course, I knew who Charlie Westoff was--had this position and he thought it would be ideal for me.

VDT: You mean Charlie didn't phone you direct?

BUMPASS: No, Charlie did not at first call me up directly, as I recall. Ron said that Charlie had this and he thought it would be a good idea, and from there I think probably I told Ron, yes, I thought I'd be interested in looking at it and then Charlie contacted me.

The interesting thing there is that that was really a watershed decision for me, because I hadn't completely abandoned the aspirations that had taken me to Michigan in the first place, to teach in a small undergraduate institution. I had spent four years learning the trade, and yet you've got to understand that the way I got to Michigan, which I've already explained to you, did not involve my seeking out the most high-powered place to attend. I just stumbled into it because I was going back home to go to graduate school.

VDT: Remind me where Wheaton is.

BUMPASS: Wheaton is in Illinois. It's a very small town; very small school. It's a good school, though a very conservative one.

I think even by the time I was finishing my graduate work, I had rather little appreciation of how good a place Michigan was. I appreciated what I'd gotten and I found it exciting, but I had no perspective on it--that's the only point.

VDT: Was that perhaps because the Population Studies Center had only been in existence six or seven years?

BUMPASS: Perhaps so. Of course, in the early days when I first went there, we were in some very ramshackle rooms that were above the Michigan theater. My office had a skylight that leaked water on the desk.

So, while I looked at a couple of other positions, including actually interviewing with Omer Galle at Vanderbilt, my major job decision came down between going off to a small school in California called Westmont to be a teacher or to go to Princeton and be a researcher. I wrestled long and hard over that one, because I hadn't fully resolved those early aspirations. And I resolved it basically in a manner to cover my options, that is, I figured I could go to Princeton and if I didn't like that way of life, I could go back and teach, but if I chose to go teach at this small school, I would have a very hard time ever moving back into research life. So I didn't quite make the decision yet; I just said, "Okay, I'll keep my options open by taking the research opportunity while it's here." And, of course, that turned into one of those flukes that opens career doors.

VDT: Do you think perhaps another watershed was the article that you and Charlie Westoff wrote that appeared in Science [September 18, 1970], "The 'Perfect Contraceptive' Population," in which you pointed out the demographic impact of eliminating unwanted fertility? When I asked Charlie Westoff what he considered--off the top of his head--the leading findings from the National Fertility Studies, he said the extent of unwanted fertility, and he cited that article, on which you got a lot of coverage. He said Fred Jaffe [of the Alan Guttmacher Institute] persuaded you to write it. It came out after you'd gone to Wisconsin.

BUMPASS: I think that was a key set of findings, and I think that it's not so much that we found something that people didn't know before, but rather that we were getting into clear focus something we knew and its implications not only for the time, around 1970, but for the baby boom itself. We began to appreciate how much of the fertility during the baby boom was accidental; how close to replacement fertility during the baby boom would have been had there been effective control. And that sort of set the stage intellectually for appreciating the change that then followed very rapidly with respect to sterilization.

VDT: Which you wrote on about that time [e.g., Harriet B. Presser and Larry L. Bumpass, "Demographic and Social Aspects of Contraceptive Sterilization in the United States: 1965-1970," in Commission on Population Growth and the American Future, Research Reports, Vol.1, 1972].

BUMPASS: Where we went intellectually was to that, recognizing that such a high proportion of the population was having the experience of the intrusion of unplanned pregnancy on their lives. Then the diffusion of the pill was under way at just that point in time, so people were experiencing both contraception that was not coitally related and an expectation of complete fertility control in the context of the preceding failure. That just set the stage for the very rapid turnaround of sterilization. The kind of excitement intellectually I felt about sterilization as a topic--actually, I wrote a piece on it not too long ago, still developing those themes--was that it was a more generic case . . . It was very

important in its own right, in terms of what was going on in fertility in the U.S.; it was one of the things that was helping to anchor low fertility not just then but for the future in the U.S. But in some sense more importantly--or at a different level, at least--it was important as social science. It represented a behavior that was feared, around which there was a great mythology, around which there was an enormous amount of disapproval, which turned around very quickly because of its utility. So it represented a more generic kind of process of social change, that was just illustrated with respect to fertility. When faced with a decision between a lifetime of high risk of unwanted fertility, of continuing the Russian roulette of existing methods, having been introduced to the pill and its efficacy--and yet, many women either unable to take the pill for a long time or just insecure about doing so, not knowing what the long-term health implications were--all of a sudden the mythology and fears surrounding sterilization evaporated, because it was a solution to the problem.

VDT: Harriet Presser, who'd already worked on sterilization in Puerto Rico and had done an overview paper on sterilization internationally for the Population Council, told me how you phoned her just at that point and together you wrote the paper for the Commission on Population Growth and the American Future [cited above]. She put it that, "He had his hands on the data," from the 1965 and 1970 National Fertility Studies. I'm glad you put it in this context of the larger picture.

At Princeton, you also worked on the final phase of the longitudinal Princeton Fertility Study, and you were the senior author of The Later Years of Childbearing [1970]. At that time, you ended up most enthusiastic about the longitudinal aspect of that survey. Charlie Westoff in his interview with me said he is no longer very enthusiastic about longitudinal surveys. What do you feel?

BUMPASS: The most succinct answer to that is that I spent most of the last month writing two proposals to fund a longitudinal survey.

VDT: The carry-on of the National Survey of Families and Households?

BUMPASS: That's correct.

VDT: The followup is going to be longitudinal?

BUMPASS: That's correct. We'll follow the exact same people. We will then know, following the first interview, who entered cohabiting relationships, who married, who divorced, who had their first births, when children left home, who had their parents move in with them, who retired.

VDT: When will you go in the field?

BUMPASS: We would hope to be in the field in 1992; it would be five years from the end of interviews. So, I remain very enthusiastic about longitudinal surveys. They are not a panacea in causal analysis. I'm not ascribing that just to the Princeton study. In that period of time in the early 1960s when that study was under way and through the 1960s, longitudinal surveys became one of these methodological fads that the field gets into, which often overstate the value of something. But then you don't want to throw away the good tool when you appreciate its limitations as well as its strengths. Having longitudinal data doesn't solve a lot of your causal questions; it leaves you still with the tangles of interpretation. But among the other things that I gained from the Princeton study was an appreciation for how bad retrospective reports can be for things that are not salient and especially for attitudes. So the great analytic power, as I see it, of our longitudinal design for a family survey is that we have such a wealth of family behavior patterns that we would never expect people to be able to

report retrospectively--what they were doing five years ago in terms of household time allocation, time spent with children, parenting practices, time with their parents, and certainly the attitudes they held with respect to a whole range of things, including the stability of their marriage, what they argued over--none of those things you'd be able to get retrospectively. We'll be able to look at the consequences of such variables for subsequent demographic transitions.

VDT: I think Charlie is a bit biased against the Princeton Fertility Study because he thought the social-psychological findings were for the birds.

BUMPASS: Really it was in the tradition of the Indianapolis study and nothing at that level of measurement paid off. Now I have colleagues who are doing very different kinds of psychological modeling, such as Betty Thomson, who is working on motivation with different kinds of measuring instruments, and I believe there is payoff to be had. But that's not a body of work that I'm likely to contribute to with any significance.

VDT: Is your continuing enthusiasm for longitudinal studies perhaps inspired also by your work on the Board of Overseers of the Michigan Panel Study of Income Dynamics, which has followed those 5,000 families for so long, with all their offshoots?

BUMPASS: That study was not something I worked on. I was on the Board of Overseers and I guess I got on that because of my interest in survey data, my experience with the National Fertility Study. In more recent years, especially in my work with Sara McLanahan, I have worked with data from the Panel Study of Income Dynamics. And surely there are very important issues that can only be addressed with that kind of data. One of the things that illustrates is very important to me and it's important to put my own study--I should say our study, because Jim Sweet is very much a codirector and partner--to keep our own study in perspective is that there is no single final solution to what we need. Each study design brings with it certain unique strengths and certain weaknesses, and the field desperately needs the kind of mix that we have and not one approach to the exclusion of the others. There are questions I can answer with the Michigan Panel Study of Income Dynamics that I can address with no other data sources. The proposal I've just written for some analytic work on our study involves some methodological issues that I'll be trying to understand.

VDT: Well, you certainly have the most popular data set around. I think the first five papers I heard this morning were based on the National Survey of Families and Households.

What took you to Wisconsin, in 1970? You hadn't stayed very long at Princeton.

BUMPASS: Much as Jim Sweet and I have worked for years on the National Survey on Families and Households, Norm Ryder and Charlie Westoff had been working on the 1965 and then were making plans for the 1970 National Fertility Survey. Norm had been at Princeton for part of the time I was there--I can't recall when exactly--working jointly with Charlie on this and, by one method or another, encouraged Wisconsin to recruit me to a faculty position. It was an opportunity to move from a post-doc to a faculty position--I was a post-doc at Princeton--and to continue working on the 1970 National Fertility Survey at Wisconsin, with Norm.

I recall a particular dinner at a PAA meeting where I sat down with Hal Winsborough, with whom I presented a paper in this meeting today ["The Death of Parents and the Transition to Old Age"], where he, representing Wisconsin, made the initial recruiting effort. I ended up doing a show-and-tell at Wisconsin--on our paper on unwanted fertility for that matter--with Norm Ryder shooting as many holes through it as he possibly could just to see how I would squirm under fire, but I managed to

get the job anyhow.

So then I worked at Wisconsin with Norm, under very difficult circumstances, in that those were the years--especially that first semester of 1970--when we were getting tear-gassed and they were marching around with fixed bayonets against the protesters.

VDT: Which led Norm eventually to leave, he said.

BUMPASS: Right.

VDT: When did Jim Sweet come?

BUMPASS: Jim went directly from Michigan to Wisconsin. He was there, I think, in 1967. Bob Hauser spent, I think, no more than a year at Brown and then went to Wisconsin. And Dave Featherman spent what may have been nine months at Princeton and then went to Wisconsin.

VDT: And you've all worked--certainly you and Jim Sweet have worked very closely. But you work also very closely with Ron Rindfuss [1991 PAA president]. How did that come about?

BUMPASS: Ron came to Wisconsin in a post-doc position in the early 1970s to work with Jim Sweet on a book that they did, using the own-children methods from the 1970 census [Postwar Fertility Trends and Differentials in the United States, 1977], and I got to know Ron in the course of that and we began doing some work together and we hit it off. He went to North Carolina somewhere in the 1970s [1976].

VDT: Has Wisconsin become the leading demographic training center, would you say?

BUMPASS: I wouldn't make that claim. I would without embarrassment claim that we're among the leaders. I often interview students who are looking at a number of the major centers around the country and I tell them straight out that I can't bad-mouth my colleagues and friends. Because I think this is a remarkable association and a remarkable community of people. It's truly unusual in the extent to which we feel like a community. I'm talking about demography in general. So that my colleagues at Michigan, Penn, Hopkins, Princeton, Texas, North Carolina are my colleagues, because I work with them. These are all good places and Wisconsin deserves to be amongst that top rank, but I wouldn't want to make any rash claims other than that we're very good.

VDT: You've always been on top of the demographic issues of current interest to U.S. society. You say it was just by chance that you were getting going in the 1960s when there was such tremendous interest in fertility, the baby boom, the surveys were first coming out, and you have obviously moved with the times. I mentioned that this morning I thought almost every paper was based on the National Survey of Families and Households. You really have been on top of the issues that were of current interest. Now, were you conscious of that?

BUMPASS: Again, there's a great measure of good luck. But there's also somewhere working in that--there are two strains of answers I'll try to tie together. Thinking of Ron Freedman as a master mentor, one of the things that I've admired most about Ron all these years is his sense of what matters. I think that came through in the early days, that techniques are terribly important, you've got to do things right, but the first thing is you've got to figure out what's important and then figure out how to know more about it. And so I would hope that I learned a little from Ron on that score. But there's also once again

this measure of great good luck. I've been in a career where I've been able to follow what interests me and it turns out that somebody else cares--at least part of the time.

The interest in family was really very early, because as I've come to appreciate more in recent years but I think it's sort of been an evolution, family and fertility are not different problems. Our historical understanding of the demographic transition is really about family sociology. And certainly all the work on nuptiality and the key to that, as divorce became a more visible feature of the landscape in the U.S., trying to understand divorce and its implications for fertility was essential to trying to understand what's going on in fertility. But then, by its very nature, divorce itself became increasingly an interesting topic, as has now cohabitation and intergenerational relationships. So I'm drifting further afield from just fertility, but I haven't lost my interest in fertility, and I see it really as all a piece of the same cloth. As I tried to argue in my presidential address last year ["What's Happening to the Family? Interactions Between Demographic and Institutional Change," published in Demography, November 1990], the same forces that were changing the relative emphasis on parenting and the motherhood role also affect our patterns of relationships between the elderly and their children. There are social issues at large that Ron Lesthaeghe points to, such as individualization and secularization and commitment versus personal achievement. So I really see them as a working-out of the implications of themes that were there in the very beginning.

Jim Sweet and I did our first paper on divorce around 1970, very soon after we had been sharing an office as graduate students.

VDT: You have not had too many single-author papers. How do you and Jim Sweet and you and Ron Rindfuss work together?

BUMPASS: Those are very different working relationships. They are both excellent working relationships. With Jim, it now goes back 25 years, and with Ron, at least 15. These are relationships in which there has been remarkably little stress, despite frequent disagreement. They've been relationships in which we've been able to manage disagreement in a creative way, to argue things out, one tell the other he's being an absolute fool, without it being . . . The underlying respect is so clear and so well understood that Jim can bring something in and say, "This is just absolute nonsense," and I may disagree, I may be mad, but I know he's not calling me a fool, he's just saying something I've done is foolish--and we all have some foolish thoughts a regular basis.

VDT: Does one of you write one section and one another?

BUMPASS: That will vary. Really there is no set pattern to this. On any given paper, usually we'll talk about how the analysis ought to be done, what the issues are, likely one of us on the basis of that discussion will set up the initial calculation, what have you, and we may discuss that and as a result of that that person may go ahead and draft something and then give it to the other person who, depending on how that particular interaction goes, may either expand on it or tear it to pieces and give it back.

VDT: Do you find that working with someone else increases your productivity because you're a team? You publish prodigiously. For instance, in 1989 you had three articles in Demography [on marital disruption, with Teresa Castro Martin, February 1989; on unwanted motherhood, with Sara McLanahan, May 1989; and on cohabitation, with James Sweet, November 1989] and at least one other that I identified ["Children's Experience in Single-Parent Families," with James Sweet, Family Planning Perspectives, November/December 1989]. I don't have your curriculum vitae and I'm sure there were many more. And this was the year that you were PAA president-elect and you had the meeting program to plan, and everything else. I started off by asking if working with someone else

keeps you on track, or are you just . . . How do you get it all done?

BUMPASS: There's no easy answer to that. There are some papers where the other person carries the major initiative and my involvement is more in the form of commentary, revision, suggested restructuring, and there are other papers where I play that role, and there are others that are much more of a mixed variety.

I think productivity, just the sheer volume of output, is probably increased by collaborating, but collaborating more by the input of fresh ideas than by the actual production process itself. That is to say, that collaborating probably makes more work than it saves, because you have to deal with this thing of, "You've got it all wrong and you've got to do it over again," whereas you might be able to find a journal that would accept it as it is. For example, my work with Sara McLanahan, my work with Betty Thomson, involves a set of ideas that we were jointly able to produce something on that I probably wouldn't have worked on without the collaboration. So in that sense, it increased the number of publications.

VDT: Which of your publications do you consider the most important, and why? I know that's a hard question; there are an awful lot.

BUMPASS: [Long pause] I'm not sure I'm going to be able to answer that. I would think it probably would be the ones that start a line of work or bring increased attention to a line of work, which then others may pick up and do much better and carry much further. Charlie pointed to the importance of the paper on unwanted fertility ["The 'Perfect Contraceptive' Population: The Extent and Implications of Unwanted Fertility," Science, 1970]. In substance, that was no new discovery at all--David Glass went into it in a paper on the 1954 British family survey--but highlighting it and sort of focusing attention in a way that brought a stream of other work to it.

I think that the work I've done on children's experience with single-parent families has played some of that kind of role, leading both to the demographic explanations and to the kinds of concerns that Sara and a whole large collectivity in the field are addressing in terms of the lifetime implications, both psychological and attainment. I don't want to claim credit for being the only person that did that, but that work helped contribute to that growing attention to the implications for children of marital disruption. More recently, our work on cohabitation--again, we weren't the first people to discover it; there were papers on Canada, papers on Sweden, papers on the U.S. based on other data sets by Arland Thornton--but it has helped to focus attention on the issue.

VDT: The paper in Demography got tremendous coverage, in part because Gordon De Jong [editor of Demography through 1990] has made certain that more papers in Demography are brought to the media's attention. That data, of course, was early data from the National Survey of Families and Households. Did you set up the survey in part specifically to look at these things on which you couldn't get data anywhere else?

BUMPASS: Yes. Certainly that was the case with cohabitation.

The story behind the National Survey of Families and Households is, I think, an interesting story of the way in which this profession works as a community. It was not something on which we had this great flash of insight that we then proceeded to share with the world. It grew out of a set of workshops of the national community working on related topics that Jeff Evans was getting together on a regular basis. At the end of those, we'd always ending up complaining in coffeeshops that sooner or later we were going to have to quit trying to squeeze the impossible out of data that were collected for some other purpose and we'd have to take on the task of planning a family survey to find out what

we needed to know that wasn't available. It was out of that set of concerns that Jeff Evans [of NICHD] eventually put out a Request for Proposals for design of the study.

We were not the only place that responded to that. We were fortunate enough to get the contract to do it, and then we wrote a proposal based on that contract work. And in the process of designing the study, we would meet on a weekly basis; this was a large collection of my colleagues at Wisconsin. Over the years, there have been at least eight of us involved--Mette Sorenson went off to Harvard, after that Judy Seltzer joined us later--Jim Sweet and myself taking some leadership and referee role. Each of us would take on the obligation to take an area of work for the week's discussion and try to address what had been done, what we knew that we didn't know, and how we might find it out. It was in that context that the questions on cohabitation evolved. In some other areas, we didn't do quite as well; we hope to improve next time.

VDT: Well, it's a tremendous survey--and huge: 13,000 respondents. It's the largest, isn't it? Of course, there's the Current Population Survey.

BUMPASS: Well, as I was saying earlier about the importance of a mix of surveys, no one survey can solve all problems. Part of what motivated that survey was a particular vision--not the only one, by any means--but a particular vision that we needed to be able to relate an array of family domains one to the other. We needed to be able to relate what's going on at work to fertility, to parenting, to relationships with one's own elderly parents, to one's background, schooling. The point is we covered a population of all age groups and we covered a very broad array of topics. Which means that for many important topics our sample size is far too small.

We have collected very good data on childhood history. We know who they were living with at age three; how long they stayed in a single-parent family. Even though there are data we've not analyzed properly yet, we've got these cases where the father goes away and comes back, when he comes back. We've got all this wonderful detail, but if you want to take people whose families broke up when they were under the age of three or between the ages of three and five and analyze it by anything else, you find that the questions are wonderful but you've got too few cases left to do anything.

VDT: You've answered this question as we've gone along, but let me ask: Who have been the leading influences on your career? Starting, obviously, with Ron Freedman.

BUMPASS: I think you've heard the cast of characters. Ron Freedman was a dominant figure. Dudley Duncan was a dominant figure, even though I didn't work directly with him; just in the classroom and then the personal relationship. You've mentioned the coffee breaks at Michigan that Ron had mentioned.

VDT: Not just mentioned; that happened while I was there interviewing him one June. We had to stop religiously at 10 o'clock for that.

BUMPASS: Right. That was something that was resented by a great many folks. But what it meant was that somebody who was as timid and from as sheltered a background as I was could ask Dudley a question any day of the week, because he'd be standing there with his coffee cup, staring into it. And you could walk up and say, "I've got a question."

VDT: You'd have them morning and afternoon?

BUMPASS: Right. So things that I would never enter his door to ask, there would be this time that was perfectly appropriate to raise them. So certainly the whole time with Dudley looms very large.

Certainly, I learned a lot from working with Charlie Westoff. But I named the chair I got at Wisconsin after Norm Ryder; one reason, of course, is that they like people with Wisconsin connections. It is unusual to name a chair after a living person.

VDT: You named it?

BUMPASS: I did it; I had the option. I was given this chair which you were allowed to name. That was within the last couple of years.

VDT: And that will be forever after the Norman Ryder Chair of Sociology?

BUMPASS: Only while I occupy it; that's the way that chair works. I named it after Norm because (a) he had a Wisconsin connection, which pleases the folks who pay the bills there, but far more important, because he had a dominating influence on my research life. Anyone who knows Norm can understand the emphasis on the word "dominant." It takes a while to develop an effective working relationship with Norm. In the early days, I would send him something and I would get back a single-spaced critique that was twice as long as what I'd sent him. This was in the period where he'd gone back to Wisconsin and I was at Princeton. That was the early stage.

VDT: You worked already with him then?

BUMPASS: Exactly. Norm would always think up 47 reasons why a number was wrong. Even when you suspected he agreed with you, he wouldn't admit that. He would tell you all the reasons why what you had found was probably just purely an artifact of methods or date. Several years of that kind of bombardment and it becomes a habit of mind. So this compulsion for cross-checking data, this compulsion of whenever I see a number wanting to know if it makes sense in terms of what else I know about other numbers, is something I attribute very much to Norm's influence. In addition to which I hold him in just enormous personal esteem and affection.

VDT: Very nice. You've named those three. You had mentioned Beverly Duncan.

BUMPASS: Beverly Duncan, sure.

VDT: And what about your students? You do teach, of course.

BUMPASS: Yes. Well, over the last few years, I have spent relatively little time in the formal classroom because of the heavy obligations with the National Survey of Families and Households. I've taught a seminar now and again.

VDT: Do you have any Ph.D. students?

BUMPASS: I have several in process now; I've had several over the years.

VDT: Do you have any leading ones, already making their mark?

BUMPASS: I feel I have an investment in a number of Wisconsin products of whom we're very

proud, who weren't my primary students. There are a number of students for whom I was a major advisor of whom I am very proud as well and expect great things from. I would rather not name anyone; I'd probably leave out somebody critical.

VDT: Okay. I told you I was going to be asking you what accomplishments in your career to date have given you the most satisfaction. There are just so many already--to date, of course, because you are midway only.

BUMPASS: Well, that's a much harder question than it seems. The National Survey of Families and Households as a joint product between Jim Sweet and me is obviously a major source of satisfaction. When we set out to do it, we were aware that we not only had the risk, demographically speaking, of making a large splash, we also had the risk of making a very public flop. And it went well. There were some very spectacular failings here and there in the survey, but it's being used by about 120 researchers around the world. We got the data--and this is in very large measure a consequence of Jim's work--the data were available to the public six months after we conducted the last interview. We turned it loose and it had the desired consequence that I will often open a new copy of a journal to find an article on something I'm writing on, based on our data.

VDT: Say that again.

BUMPASS: An article based on our data on a topic that I'm working on at the moment that I didn't know was being written. Having released the data, you never know when you open a journal whether you're being scooped with your own data.

VDT: You have made the tapes freely available?

BUMPASS: We sell them at cost: \$300, or whatever it is, with all the documentation.

VDT: A lot of it has appeared in the media too. You feel it important to communicate with the media?

BUMPASS: I feel an obligation there. There are limits to what one can do. I don't have a real taste for that; I really don't. Something like the cohabitation thing, if you had a taste for it or if you didn't control it, you could do that full time for a week or two, because there are an unbelievable number of small radio stations in the country, all of which only want two minutes of your time. I've learned not to return those phone calls. But I do try to talk to people from the major newspapers and news services, because I do think it's good for the field--to the extent that we can keep these things from being trivialized--it's good to make people aware that there is social science going on that somebody cares about. So I feel an obligation to talk to them.

VDT: In your PAA presidential address last year, you pointed out that your predecessors had all talked on themes that were very relevant to society at the moment: Harriet Presser [1989] on child care, "Can We Make Time for Children?"; Ren Farley [1988] on black-white inequality; Jane Menken [1985] on intergenerational obligations; and Sam Preston [1984] on children and the elderly. You said, "The recurrence of such themes illustrates the relevance of demographic research to much that matters to our society at large." I think you've just said very well that because it matters, you have an obligation to communicate your research.

And you speak to policymakers too. For instance, you've just been at a meeting of the PAA

Public Affairs Committee. Are you going to do congressional presentations?

BUMPASS: There will be additional presentations of that sort. They vary in nature. I expect they're all at some level useful. I've been involved in congressional briefings where it's largely a matter of providing background information that doesn't have any direct policy link but may help thought about relevant policy to be set in the proper context--the proper understanding of divorce trends, the levels of divorce and children's and women's experience, what have you. And then there are other instances, such as the opportunity I had a year or so ago to meet with a small group with Senator Moynihan where he was focusing much more on thinking very clearly about policy initiatives, where you do get the sense that something you're doing or say could at least run the risk of having some real impact on public policy.

VDT: Which you feel an obligation to do?

BUMPASS: While I have a few obvious political objectives I would like to see accomplished, I really feel the obligation in the spirit as adopted by the Public Affairs Committee, which is to maximize the relevance and accuracy of the information that's available to policymakers. Which means that you clearly run the risk of positions you disagree with more effectively using your information than those that you agree with, but at least you can help them get the facts straight.

VDT: Good. Now we must move on to PAA. Evidently, your first PAA meeting was 1965, in Chicago, when Ron Freedman was president. You asked Ron Freedman to introduce you last year and he pointed that out; it's the custom for presidents to choose someone to introduce them who's had a big impact on them. Ron's address in 1965, as you probably remember, was on "From High to Low Fertility: The Challenge for Demographers." He admitted that he was rather optimistic then. What else do you recollect about that meeting and your early meetings?

BUMPASS: I remember the cast of characters, because I was sort of the wide-eyed new kid on the block, as I suppose graduate students are--hopefully, they're a little more sophisticated now than I was in those days--the sense of being able to see the people that I was reading was very keen. It was quite an experience to see Kingsley Davis and the others. I got the program for that year, I don't remember them all at the moment, but the program was a Who's Who of population at the time. So it was the experience of, "Oh, that's so-and-so," and beginning to associate a face with a name.

As I said in my introductory remarks last year, which didn't belong in the printed version [Demography, November 1990] of the [presidential] address, I formed an opinion of the PAA in those first years that persisted perhaps to some extent despite the change in reality, although I think there's a core of truth in it, that is--PAA really was very small. You could know everyone.

VDT: In 1965 the membership was 1,283. That was just after it took off because of Don Bogue's solicitation of subscriptions to Demography. [Members were 660 in 1962; 802 in 1963; 1,142 in 1964. Demography, with Donald Bogue as editor, began in 1964.] The actual meeting numbers were less [452 at 1967 meeting in Cincinnati, when the records resume after a hiatus since 1935]. The meeting numbers, I believe, were around 400 toward the end of the 1960s [581 in Boston in 1968; 486 in Atlantic City in 1969].

BUMPASS: Which is a third of what we have at this meeting [1991 meeting registered 1,399; 1990 meeting in Toronto, 1,175]. I did form the sense of it being a very small association where everyone knew everyone else and by and large everyone liked everyone else, and was able rather quickly--not in

my first year, but in the first years--to begin to participate in that community, the national community. So this persisting sense of the PAA as a national community of colleagues is one that I hold dearly, even though we've grown much larger through the years.

VDT: That's interesting; no one has quite put it that way--the national community. But you must have got into it very quickly, because you got onto the Board in 1974-77. That was only a few years after your first meeting; you were very young. The "old boy network"?

BUMPASS: I guess so; I don't know.

VDT: Tell me a bit about your time as editor of Demography, from 1978 to 1981. I saw that darling picture of you in the photo display [display of "historic" PAA photos to mark 1991 as PAA's 60th anniversary meeting]. I hadn't seen that before, holding in your arms all the issues of Demography until your time.

BUMPASS: Marty O'Connell staged that one. I don't even remember where he did it; he just handed me all of those and shot a picture of it. This was at the time that I was editor. I've no idea what motivated it; I do remember him doing it.

VDT: It could have been for the 50th anniversary meeting photo display that we had ten years ago.

You followed ten years after Beverly Duncan took over the editorship from Don Bogue, which of course brought about a radical transformation in Demography, as we all know; it suddenly collapsed into a very learned journal. How did you feel about it when you were editor? It was a lot of work, I expect.

BUMPASS: It was a lot of work. I'm not quite sure what to say about that experience. It was a good experience. It was, again, one of those experiences that knits together--serves an integrative function at the institution you're at, because you can't edit a journal by yourself. Of course, my long-time friend, Jim Sweet, played a major role in helping me with the household and fertility and family-type things; Bob Mare played a major role with respect to the more formal demographic things; and Karl Taeuber with respect to distributional issues--in terms of serving as associate editors and helping me to evaluate the reviews that were coming in.

We were at that point, for whatever reason, in a hand-to-mouth situation. Unlike the recent report at the Board meeting that they [Demography staff] have three issues in advance, we would be facing a press deadline not quite sure that we had the final articles nailed down to make up the next issue.

VDT: I noticed that your last issue, November 1981, was huge--200 pages compared to 100 to 150 before then--but there was one that was less than 100 [May 1980]. You couldn't find enough to fill it, I guess; it wasn't ready. And then your last issue looked like you had thrown in everything.

BUMPASS: That, of course, caused some consternation to the bookkeepers. That was simply a matter of trying to close the books on articles that were outstanding, that is to say, articles that had not yet been resolved, to minimize the cost of transition to Omer Galle--not to be passing along a lot of the decisions.

That was a good experience, but it did not play to my strengths.

VDT: You obviously did not have any of your own articles in Demography while you were editor, but I mentioned the year, 1989, when you had three. You publish a lot in Demography. Do you

deliberately want to encourage Demography?

BUMPASS: I don't want to over-characterize the bias, but I do feel a primary identification with this collection of colleagues that the PAA represents, and so publishing in Demography is communicating most directly with that community. Now, at the same time, in my last several years, I have had some evolving interest in areas of a different sort, areas in which I'm a real neophyte, which is likely to involve me in some stranger, less clearly designed circles, and I'm likely to be publishing in journals aimed at different communities. But that tendency to publish in Demography reflects both a respect for the journal and the fact that a large part of the colleagues that I most routinely share information with . . . I see publications as part of a dialogue and not as final statements on anything, but rather as the next statement about what we know about this as we continue to think it through, and so Demography speaks to that group of people.

VDT: That was, of course, the rationale for Demography, when Norm Ryder . . . he claims he started it; Don Bogue doesn't always give him credit. Don is the one that picked up the idea and did it. And one of the rationales was that there was a certain community who would see the articles there who might not otherwise see them, even in the sociological journals--in which you also publish--scattered around; not everybody reads all those.

Now let's talk about your presidential year, which was just last year [1990]. How about planning that meeting program? It jumped to 90 sessions last year and you set the pace for 90 this year. It's just tremendous; it's exploded. What do you feel about that?

BUMPASS: That was simultaneously a very rewarding experience and about four times as hard as I anticipated. I think if anybody ever fully appreciates what's involved in doing that, we will no longer get people who agree to run for president. That cost me a good part of the year, as I think it does everybody. And it was a year that I was going to write a book, which I didn't write because of that. It's not timely now; it would have been then. I don't resent that at all. As you say, my publications are probably too many already. But it was an enormous task.

At the same time, it was an enormous task in which the cooperation and help of others was just remarkable, both in terms of the effort they put in and the quality of that effort. Once again, we're back to this hobbyhorse of mine about the community of people. It was a lot of work, because I would spend a good part of every day on the telephone, talking to people all over the country. Now I hope I didn't just play an old boys' network. When you talk to many hundreds of people, it's stretching it a little bit just to call it an old boys' network, even though when you call them you know almost all of them by first name and they're friends at some level. It was very rewarding.

VDT: What about the graduate students? You don't get their papers in there now. I've heard that in 1989, when my friend Nancy Yinger had a session, she said, "I got 53 excellent papers for this session," and, of course, four only could be chosen. Harriet Presser, who was in charge that year before you, said she saw the need to leave a couple of slots on Saturday afternoon to fill in some of these papers. What's the solution?

BUMPASS: There are at least two issues in your question. There are far more good papers than there is room on the program and there are many years in which PAA has had that. The hardest part of that job really involved the time I spent with almost 200 manuscripts that didn't fit somewhere, trying to read them myself and figure out what to do with them and to get somebody to put together sessions out of existing papers that had already been sent. So that was a lot of extra work. I think maybe a much more rational procedure is to just cut for this year and look somewhere else for next year, because not

everything can make the program.

I think the other issue has to do with students in sessions, and it is my sense that PAA probably has higher student participation rates, I would suspect, than most societies. If you go through the program--I haven't done it for this year although I know several students who are on the program. I went through the program for the year before in response to a query from students about a student session and found that there was a very substantial representation of students among persons with papers, some of whom were obviously in an apprentice-collaborative role with the senior investigators and some of whom were presenting just their own work. I thought about it and talked to others and I thought it was not helpful to students to create a play-school section.

I think the notion of having the real program and then a corner for students doesn't give the graduate students credit. In the professionalization process, they may start off in awe, but by the time they are senior graduate students, they're capable of writing the kind of papers that can get on the program, and do get on the program, and be presented as the science that they are, not as a play school. So I really don't favor that.

Now there's another mechanism that students are evolving amongst themselves, so I hear from the grapevine, which I think is just dandy, and that is for the students to organize essentially seminars amongst themselves. They take the initiative. They get together and present papers to one another, so they learn what other students are doing at other institutions. That's terrific!

VDT: At PAA meetings?

BUMPASS: At PAA. That's starting to happen. For example, at the aging thing yesterday, they at least talked about that sort of thing. I think that is wonderful. But it would be a mistake for PAA to say, "We have the real science and then we have students," because students are quite capable of the real science by the time they're senior students.

VDT: Also last year [1990], of course, a big issue was the shift from the American Statistical Association to the American Sociological Association for handling PAA's business affairs. You sort of came in on the tail end of that, that decision had been made--or forced upon us, shall we say.

BUMPASS: That's right. By the good work of others and a stroke of good luck, I was spared massive embarrassment. Because it was in the fall [1989]--I think in November but the date doesn't matter exactly--before I was to become president in January, that it was announced that we could no longer stay with AStatA. So that then left me with a specter of presiding over the demise of the Association. I could imagine things going badly at AStatA in that intervening period and the arrangements being improperly made or not at all. We could have had a serious disaster in terms of the meeting. But Jean Smith at AStatA worked very hard and her commitment to us as an association and doing that well made it so that we had a very successful meeting and suffered no great trauma on that score.

We worried about the transition in the business arrangement, both in terms of being able to do it effectively and in terms of suffering serious financial losses in the transition.

VDT: Where you almost doubled the membership fee [\$45 to \$70]--totally unannounced.

BUMPASS: We were anticipating a deficit of thirty to sixty thousand dollars the year of the transition [1990]. But the transition committee, which Jeff Evans chaired--I may or may not remember all the members at the moment--but that committee with Jeff Evans, Suzanne Bianchi, Tom Merrick, and Larry Suter, and Harriet played a major role looking for organizational alternatives. There were actually several committees that followed one after another there. [A Committee on Organizational

Management, formed in spring 1989 with Jeff Evans as chair and including Reynolds Farley, Charles Keely, and Signe Wetrogan, explored "possible alternatives for enhancing the Association's presence in Washington and better satisfying the growing demand for organizational capability in a variety of areas." Following the American Statistical Association's announcement in early October 1989 that it wished to sever ties with PAA, a subcommittee of the Board, the Negotiating Committee, was formed, with Harriet Presser as chair and including Larry Bumpass, Ronald Rindfuss, Suzanne Bianchi, Jeff Evans, and Noreen Goldman, whose work eventually led to the current arrangement with the American Sociological Association. To oversee the transition once this agreement had been made, Larry Bumpass appointed a one-year Transition Committee, chaired by Jeff Evans and including Suzanne Bianchi, Thomas Merrick, and Larry Suter. See, "American Sociological Association to Manage PAA Business Office," PAA Affairs, Winter/Spring 1990.] They worked out this arrangement with ASocA, which I had relatively little to do with, other than kibitzing from the margin, but it was an outstanding arrangement. And Jen Suter [PAA Administrator at the American Sociological Association], in particular, has done such a spectacular job that our giant-deficit year turned to profit. Of course, part of that had to do with the economics of demography, which are very strange indeed, because we raised the price and increased our membership as a consequence, contrary to supply and demand theories. We raised the dues very substantially, from \$45 to \$70.

VDT: Right--and that increased the membership?

BUMPASS: I wouldn't say that increased it, but our expectation of a serious deficit was based on common economic understanding that if you raise the price the demand is going to fall off, and it didn't.

VDT: At the end of 1990, we were at 2,752 members, and we just inched up over the last decade or 15 years [2,618 in 1975; 2,488 in 1980]. What do you feel about PAA getting more members? Should it?

BUMPASS: This is one of these things that can be moderately controversial, but I'll say it anyhow. I am rather indifferent to the size of our membership, as an issue in itself. To borrow a phrase with an unfortunate history, "benign neglect" is the policy I chose with respect to the Membership Committee and membership drives as president, and it was a conscious choice, because--it's been no secret in the course of these discussions--I love this Association and I'm not so keen on turning it into something else so it will be bigger and richer. So seeking new sources of members to increase the revenues, but in the process changing the composition of what it is, is not something I'm keen on doing. If the demography field grows by virtue of expansion of researchers doing population work and the Association grows as a consequence of that, that's just dandy.

VDT: This is a related question: What about the business demographers? What do you see as the outlook for demography and demographers in the U.S.? Are the majority of jobs now for applied demographers--state and local government and business--or is there still room for basic researchers like yourself? I think I sense in there, demographers doing research of the kind that have always made up the core of PAA. What about expanding that core to the business demographers?

BUMPASS: Yes, okay, because I don't want to be misunderstood on that point. I define within the core, by my definition, the work that's being done by the bulk of our colleagues in business demography and in state and local demography; this is the work of the profession. I think some of the boundaries that have from time to time been emphasized are largely artificial, certainly in my sense and obviously in my colleagues' sense at Wisconsin. Paul Voss is the chair of the PAA State and Local

Government Group. Paul is a valued colleague in every sense of the word and a leading demographer. The work that that community does is by my definition part of the hard core.

That, as I recall, was represented in the work in the program in those early days, in the 1960s.

VDT: Like what?

BUMPASS: Not in terms of there being an interest group of state and local government, but in that a number of the PAA members were active in government statistics programs of all sorts, both at the federal level and at local levels. That has been part of my image of the profession since those early days.

I think many of our students, many of our colleagues, will continue to do that. There's obviously an expanding role for them; that's part of the outside world discovering the relevance of what we do to what they do. And there's nothing harmful about being useful.

I'm on the committee now that Rindfuss, our president, has asked me to chair--because I opened my big mouth and raised the issue--to worry about the role of interest groups in the profession [PAA]. And there are serious issues to be worried about in terms of the organizational nature of these relationships.

VDT: Yes, I think they [State and Local Government and Business] didn't like being switched from being committees to interest groups [within PAA].

BUMPASS: Right, and the issues are very complex. They cut across everything from that one to the risk of the Association moving in the direction of sections, that is, taking this larger community and digging trenches and putting up barbed wire. We don't want to do that.

At the same time, some organizational mechanism, such as the interest group provides, is obviously critical, especially, as I've come to appreciate in my conversations with Paul Voss, for the business demographers and the state and local demographers. The sense of identity, of how do we relate to the larger collectivity, is facilitated by the existence of this organizational sub-unit within PAA. We've got to find ways to make that work even better.

VDT: Within PAA; hang onto them.

BUMPASS: Yes--oh, yes. I don't think we'll lose them.

VDT: One last question. You've had consistent funding from NICHD over the years--obviously the survey, Jeff Evans, and so on. The source of the real money for population research is NICHD, but only about 16 percent of the approved grants are getting funded, although yesterday they were saying they were getting up to about 19 percent. Perhaps you've been favored. What do you see as the outlook for basic research with funding getting more and more restricted, even for such a sexy topic--as Jane Menken put it--as population? She felt that population research had been favored over a couple of decades and that might now be changing.

BUMPASS: I'm more optimistic, again for reasons that I outlined in my presidential address, though I didn't draw that line there; that is to say, I didn't develop that point. The reason I'm optimistic is because . . . The efforts of our Population Affairs Committee are in part symptomatic, they're not just causal. They reflect the fact that policymakers as well as the business community are becoming increasingly, extremely, aware that they need to know the demographic trends and understand more about their underlying processes in order to formulate policy that costs billions and billions of dollars.

I was talking to the staffer, who is no longer with him, but had been a key staffer for Senator Moynihan. I was just blown away by the way she rattled off alternative policies where she was rounding to the nearest billion. When things cost that much, what it costs to fund research to make the policy informed is a rather minor point. I think that the kind of large increase that NIA [National Institute on Aging] has seen, partly driven just by Alzheimer's and totally out of our thing, but the funding for social science at NIA reflects an increased awareness of how critical it is that policy looking forward to an aging population understands the issues about intergenerational relationships, about frailty, about joint retirement, all of these things that are within what our colleagues do. And the society that faces multi-billion-dollar policies on limited information. I'm optimistic that what we do matters, will continue to serve us well.

VDT: What you are doing matters, because you're really on top of it. Thank you. You're due down at the business meeting to be on show with the directors.

AFTER REMARKS

BUMPASS [removing running shoes]: I'll take off these shoes I put on in the little break before our meeting time, because my other meeting ended early. I buy running shoes for the sake of comfort.

VDT: And do you exercise?

BUMPASS: I run regularly, maybe three times a week, although I'm not a marathon runner like Ren Farley. Ren runs marathon.

VDT: As you know, this is my very last interview for this series, although I regret to say I never got Evelyn Kitagawa, who was one of the rare women who made it right to the top in this exclusive sample. Do you have to be very, very special to make it right to the top of PAA?

BUMPASS: Actually, I think this profession is distinguished by its treatment of women. You go back to Irene Taeuber, Margaret Hagood, Dorothy Thomas, Harriet Presser, Jane Menken.

VDT: But there was the long gap [between Dorothy Thomas, president, 1958-59, to Evelyn Kitagawa, 1977]. You and Jane Menken--well, she was part of that cohort, although you're younger than Jane.

BUMPASS: Jane was part of my Princeton group, when I was at Princeton in that short spell, hanging out at OPR, with Jane Menken, Nancy Howell Lee, Massimo Livi-Bacci, Ron Lesthaeghe--a number of others.

VDT: We've just wondered why the young people are not coming to the business meetings at PAA [technically known as the annual "membership" meeting since the constitutional change of 1974 described here by Larry Bumpass]. Now, in the old days . . .

BUMPASS: That's a reflection of the business meetings of the old days. For all the sense of community and friendship that I described, which was real, the business meetings were often free-for-alls, because they were meetings where policy could be decided for the Association. So if something was politically hot, a source of political disagreement--for example, very keen issues on what the U.S. ought to be doing about family planning programs worldwide or other such issues--decisions could be made at those meetings that would affect the course of the Association. So people would argue greatly

on both sides of the issues.

I actually served on a constitutional committee with Con Taeuber and others in the early 1970s which set up the current circumstance, because we worried about packing the galleries, about the way in which PAA policy could be decided on a particular issue by organizing to have a lot of people there and vote, that would then determine what would be the course of the PAA. So we set up the business meeting as it is now, as an informational meeting and as a forum for discussion, but any decisions are either made by the Board or by referendum of the Association.

VDT: And people have complained it's become so boring.

BUMPASS: It does that, indeed.

VDT: Well, let's go see how it is today.

RONALD R. RINDFUSS

PAA President in 1991 (No. 54). Interview with Jean van der Tak during the PAA annual meeting, Radisson Hotel, Denver, Colorado, May 1, 1992.

CAREER HIGHLIGHTS: Ronald Rindfuss was born and brought up in Buffalo, New York. He obtained his B.A. in sociology, with a minor in mathematics, from Fordham University in 1968 and the Ph.D. in sociology from Princeton University in 1974. He entered Princeton on a National Institutes of Health training grant and from 1971 to 1973 was a research assistant at the Office of Population Research, working on the 1970 National Fertility Study. From 1973 to 1976, he was Research Associate at the Center for Demography and Ecology and the Institute for Research on Poverty at the University of Wisconsin in Madison, where he wrote with James Sweet the monograph, *Postwar Fertility Trends and Differentials in the United States* (1977). Since 1976 he has been at the University of North Carolina at Chapel Hill, where he is Professor of Sociology (since 1984) and a fellow of the Carolina Population Center. Among other activities, he has served on the Committee on Population of the National Research Council (1989-92), on the Census Bureau Advisory Committee (1983-89, chair in 1985-86), held key posts in the American Sociological Association as well as the PAA, been a consultant to the East-West Population Institute, Social Security Administration, Centers for Disease Control, Rand Corporation, and many other government and private organizations, and served as associate or consulting editor or referee for many professional journals.

Ronald Rindfuss is well known in the population world for his research and publications on the social demography of fertility, particularly in the U.S. but also in Asia, and focused primarily on the adolescent and young adult ages. His latest book is *First Births in America: Changes in the Timing of Parenthood* (with Philip Morgan and Gray Swicegood, 1988). In addition, he is author or coauthor of close to 60 journal articles published from 1973 through 1991, plus book reviews, comments, testimony, etc.

VDT [from interview introduction]: Ron was president of PAA last year, 1991. He just missed making it into *Demographic Destinies* [1991], the collection of 49 edited transcripts of interviews with PAA past presidents and secretary-treasurers done for the PAA Oral History Project, and I'm very pleased that he has agreed to this interview, which will be a first supplement to that collection.

Thank you, Ron, for making time for the interview during a meeting that will be easier for you than it was for you last year but I'm sure still very busy.

RINDFUSS: It has definitely been busy! I'm looking forward to next year.

VDT: You were born and brought up in Buffalo. That's close to *my* hometown, Toronto. We Torontonians think of Buffalo as sort of a suburb. There's very good shopping in Buffalo.

RINDFUSS: And we used to go to Toronto quite frequently as well.

VDT: I note that you and your wife Maggie were married in the summer of 1968 after you graduated from Fordham. Did you meet at Fordham?

RINDFUSS: No. We met in our senior year at high school. We actually met after we had each made independent decisions to go to Fordham, but we certainly were teased quite a bit once it became clear that we were both going to Fordham.

VDT: That seemed a bit too premeditated. And you have two sons, Luke and Rob, one of whom has just turned 20, as you said in that teenage childbearing session yesterday. Is that Luke or Rob?

RINDFUSS: That's Luke.

VDT [after biographical introduction]: How and when did you first become interested in demography, and particularly in fertility?

RINDFUSS: It was during either my freshman or sophomore year in college at Fordham. Like many others of my generation, I was sort of caught up in the excitement and push, if you will, towards mathematics as a result of Sputnik in 1957, when I was in grade school. When I got to high school, I was good in math and everyone, from family members to teachers, was pushing me in the direction of mathematics.

So my first year at Fordham, I was a mathematics major. I was taking eight credit hours per semester in math. I woke up one night dreaming how to solve a differential equation. I thought about it and decided that this was not how I wanted to spend the rest of my dreams.

At that time, I was living in a house off campus; one of the housemates was Peter Donaldson.

VDT: Ah, that explains why you asked him to introduce you last year [presidential address session at 1991 PAA meeting].

RINDFUSS: Peter was roughly two years ahead of me in school and was a sociology major. I was at this point deciding that I did not want to be a mathematics major, and then the next logical question was, well, what should I major in? Peter suggested that sociology was interesting and, in particular, I should take a course in population. He recommended the course that Mary Powers was teaching.

I took Mary's course. It was a rigorous course, to put it mildly; she had an enormous amount of analytical material.

VDT: Introductory population?

RINDFUSS: Yes. In the course, we covered the standard topics: fertility, mortality, migration. And I just found myself fascinated with some of the fertility research we were reading.

VDT: As a sophomore you were doing this?

RINDFUSS: I believe so; it might have been a junior year course. One of the books we read was the first volume of the Princeton Fertility Study by Charlie Westoff and others [*The Third Child*, 1963].

VDT: Larry Bumpass came in on that study later.

RINDFUSS: Larry came in with *The Later Years of Childbearing* [1970]. I was impressed with it and from that time on, I was interested in aspects of American fertility.

VDT: That was the only course you took as an undergraduate, as you recall?

RINDFUSS: In population, yes.

VDT: What took you to Princeton?

RINDFUSS: Like many other people at the time, I applied to the usual collection of graduate schools, including Princeton, and the ones that were within easy driving distance of New York City I visited. I was just most impressed with Princeton. Not only had I already been aware of the faculty, but it was an absolutely beautiful day the day that we went there.

VDT: Princeton does that to people!

RINDFUSS: Yes. It was springtime; the flowers were out. My future wife and I both went down there. It was a nice day and I was predisposed to going there anyhow.

VDT: And you applied for an NIH training grant or did they pick you out to get one?

RINDFUSS: They picked me out. The way it works is that the training grants are given to universities and then the universities make decisions as to which students are to receive them.

VDT: So then you and Maggie were married in August and in September you went to Princeton?

RINDFUSS: Correct.

VDT: What did she do, by the way?

RINDFUSS: At that time, the few years, she was working in the university archives. It was a job that she enjoyed a lot. She was able to learn a lot about the history of Princeton. Princeton had many famous alumni and a lot of their papers were in the archives.

VDT: Can you describe a bit what the Princeton ambiance was like at that time? You started work on the 1970 National Fertility Study before it actually went in the field?

RINDFUSS: No. Let me answer the ambiance first and then talk about the study.

OPR [Office of Population Research] at that time was in a house; it was at 5 Ivy Lane. If you can think of a building as being warm and friendly, it was a warm and friendly building. And when I first got there, everyone fit into this building. While I was there, they branched out into the building next door.

We had a good collection of faculty and graduate students.

VDT: Who were some of your fellow students?

RINDFUSS: Let's see; memory is not one of my best suits. I can think of some: Barbara Anderson was there at the time; Hilary Page was there; Leela Visaria.

VDT: Leela, of course. She and Pravin later wrote one of my Population Bulletins [of the Population Reference Bureau, on India].

And who did you work with, your professors? Because Charlie Westoff went off from 1970 to 72 to work on the Commission on Population Growth and the American Future.

RINDFUSS: Right. Charlie was commuting. He would spend maybe two days a week at the Office

of Population Research and three days a week in Washington, so he was certainly still present at OPR. The main population courses at that time were taught by Ansley Coale; he taught one in the spring and one in the fall. All the graduate students took his courses.

Then when I started working on my dissertation, I was working with both Charlie Westoff and Norm Ryder, because by that time Norm had moved from Wisconsin. Norm came--you're the historian, you probably have a better sense of the date.

VDT: In 1971. He came back because of the troubles at Wisconsin. He had, of course, continued to be codirector of the NFS. Your dissertation title was *Measurement of Personal Fertility Preferences*. Did that come from the NFS?

RINDFUSS: Yes, it used data from the 1965 National Fertility Study. And, if you remember, at the time fertility in the United States, around 1971, 72, was fairly high. There was substantial debate regarding its level: Is it too high? And was it high because people were having more children than they wanted and somehow were making mistakes--contraceptive failures, in essence--or was it so high simply because people wanted more children than would lead to replacement level fertility? This was the basic question that led to the Commission on Population Growth and the American Future.

And there was also controversy surrounding the actual measurement of fertility preferences. You can get different results depending on how you measure them.

VDT: Ideal, intended, expected fertility. Yes, Norman Ryder was famous for the articles he wrote belaboring those points.

RINDFUSS: So my thesis went into these issues in great detail. And about the same time, while I was doing my dissertation, fertility in the United States was going down fairly rapidly. I took long enough to do my dissertation that I was answering questions that weren't quite as relevant by the time I finished as when I started.

VDT: That's right. You finished in 1974. We knew [afterward] that it dropped to replacement level in 1972. Of course, it stole the thunder of the report of the Commission.

RINDFUSS: Yes, it did.

VDT: And your dissertation too!

I was going to ask if you worked with Ansley Coale--obviously, very much.

RINDFUSS: Yes, he was the third member of my dissertation committee. He wasn't the most central member because I was using data that Norm and Charlie had collected, but he served as the third member, and a very helpful member.

VDT: I can now understand why you quote Norm Ryder often: "The norm in the U.S. is for everyone to get married and have two children as soon as possible," or something to that effect.

RINDFUSS: Yes. He was actually writing that when I was a graduate student.

VDT: I see. Then what took you to Wisconsin?

RINDFUSS: It just seemed like a wonderful opportunity. I had known Larry Bumpass because even

though we hadn't overlapped at the Office of Population Research--I believe he left maybe the year I came [1971] or the year before I arrived there [Bumpass moved from OPR to Wisconsin in 1970]--he was still working with Norm and Charlie on the 1970 National Fertility Study. In fact, he played a key role in designing the questionnaire and in the analysis.

Through Larry I met Jim Sweet, and Jim had a project that was just beginning which was designed to look at trends in American fertility, using census and Current Population Survey data. They had an opening for a research associate and asked me if I wanted to go there.

VDT: And you went, and published the book, *Postwar Fertility Trends and Differentials in the United States* in 1977--actually a year after you had gone to North Carolina.

RINDFUSS: It was a great time in many respects. Officially, my appointment--I was a post-doc in the Institute for Research on Poverty and the other part I was research associate on this particular project--but for all intents and purposes, I had the freedom to look at whatever issues interested me. I didn't have any teaching responsibilities; I didn't have any recruiting responsibilities or administrative responsibilities. It was just wonderful to be able to work full time on research. And, in fact, it was a very productive time in my life as well.

VDT: And with an interesting group.

RINDFUSS: *Very* interesting group--very strong, relatively young group of social demographers.

VDT: Who have, many of them, come out of Princeton.

RINDFUSS: Jim Sweet went to Michigan.

VDT: Oh, sorry! They all came out of Michigan--the famous cohort of Bumpass, Sweet, Bob Hauser, David Featherman--and they all ended up at Wisconsin.

RINDFUSS: Right--by various routes. Bob Hauser went to Brown first; David Featherman and Larry Bumpass went to Princeton first. But fairly quickly, they all assembled at Wisconsin.

VDT: In part due to Ryder. He said he took the Michigan people because they were the best. He stole them off for the center which he had begun at Wisconsin.

Larry in his interview, as you know, described the relationship that you cemented there as particularly important. He talked often of his feeling of "a national community" of demographic colleagues. Do you feel that too?

RINDFUSS: Yes, definitely. Part of the enjoyment of working in this field is the high caliber of its members, both in terms of the quality of their research minds but also there are very nice people that work in the population field. There really is a national community that functions not just at annual meetings like this but through the telephone, letters.

VDT: Why do you think that is? Do you think that demography draws a special kind of person?

RINDFUSS: Oh, yes; I think we draw very special people. [Laughter]

I'm teasing. I don't know why it is. The topics . . . If you think of processes like fertility, mortality, migration, these are fundamental human events that lots of people really care about. Yeah, I

think there's selectivity in terms of people who can care about basic human processes.

VDT: But sociologists do too. Demographers go beyond that. Most of the leaders are quantitatively oriented also--leaders in the field of demography, like yourself.

RINDFUSS: Yes, definitely.

VDT: You came out of math. So, there might be something about being sharp enough to be able to do the research plausibly.

RINDFUSS: Could be, I don't know. I must say it's just very enjoyable to deal with nice people on a day-to-day basis.

VDT: And then what took you to North Carolina?

RINDFUSS: A job, basically. That's not a very profound answer, but there was a position open in the department of sociology. It's a department that had a long history of well-known people in the population field--Rupert Vance, for example, one of the earlier PAA presidents--in 1951?

VDT: Yes, you're right [PAA president in 1951-52]. Rupert Vance, incidentally, was nominated to be president the year that PAA met at Chapel Hill [1951]. He was nominated from the floor and elected. He was local arrangements chair that year.

RINDFUSS: And when I went there, Amos Hawley was there; Krishnan Namboodiri was there; Peter Uhlenberg was there; Dick Udry was there; Jack Kasarda arrived the same year I did. The difficulty in naming names like this is that I know I'm going to leave out three or four very important people. So, my apologies to those whom I've forgotten.

VDT: North Carolina is famous among universities offering population studies as having had what John and Pat Caldwell said in their book, *Limiting Population and the Ford Foundation Contribution* [1986], was the first and perhaps the only university-wide population program. At the time the Carolina Population Center was set up in 1966, they said, "A population component existed in 15 university departments" [page 95]. That was in many other departments besides sociology.

RINDFUSS: Yes.

VDT: And there was a big push to get foreign students to work in family planning programs. And Moye Freymann was director of the Center from 1966 to 74 and he was relieved of his directorship--you weren't involved in all of that--and by the late 1970s, the Caldwells said, "There were many fewer foreign students in population courses at the University of North Carolina, and the Carolina Population Center, under Richard Udry, had become a model of academic respectability and the majority of its work was concentrated on the United States." And that's where you came in.

RINDFUSS: Yes. I'm not sure, actually, I would agree with the characterization that the majority of the work was concentrated on the United States. In the late 1970s, there was ongoing work in developing countries as well as work in the United States.

VDT: Which, of course, you were doing too, but we'll get to that in a moment.

You were presumably more concentrated on the U.S., however, and not involved in the AID funding which came in when the Ford Foundation left off and "led to dramatic growth of some programs, especially in North Carolina" [Caldwells, p. 104].

RINDFUSS: And the characterization of the North Carolina program as being multidisciplinary is still true today. It's the only program I can think of that involves faculty members from as diverse a collection of departments as we have.

VDT: You're talking about the program at the Carolina Population Center?

RINDFUSS: At the Center. I'll not attempt to list them all but I can give you an example of the range. The Center has faculty members from obstetrics and gynecology, from maternal and child health, geography, political science, sociology, of course, economics, biostatistics--to name but a few.

VDT: That's amazing. Is that, would you say, the outstanding feature of North Carolina in the population world?

RINDFUSS: It's certainly a leading feature. I think the outstanding feature is that we have a large number of very good scholars as well. It's not just that they're diverse, but that they do excellent research.

VDT: The quality of those--a few of those. I know you're afraid to leave some out.

RINDFUSS: I'm not going to name names. There are such a large number of truly outstanding faculty members associated with the Center that I'll surely leave some out.

VDT: Now on your research. What prompted your interest in the adolescent and early adult years? In your dissertation, presumably, you dealt with the whole range of childbearing women, and also in your work with Jim Sweet.

RINDFUSS: While I was at Wisconsin, I started thinking about the general question of the sociological meaning of age as opposed to the biological or the simple statistical meaning of age. And in conjunction with that, I was also aware of a lot of the work that Norm Ryder had been doing, suggesting that if you look at the trends in American fertility that timing of first birth explains in a statistical sense much of the action in terms of the low point during the Depression, the high point during the baby boom, and then you get the decline in fertility during the 1960s and early 1970s. So, thinking about age and thinking about the particular influence of the first birth, I started doing some work on the one hand looking at the determinants of the timing of first birth and on the other hand I was doing a lot of work looking at determinants of the length of time between births, that is, from first to second, second to third, and so forth.

From there, it was a fairly short jump to recognize the particular importance of the teen years and the early twenties.

VDT: You have stressed that first births are more important than first marriage.

RINDFUSS: Yes.

VDT: You stress the social background to demographic events and changes. I would put you in the same category with Kingsley Davis.

RINDFUSS: I'm flattered! [Laughter] He's had such a long, illustrious career.

VDT: He pointed out in his great criticism of the World Fertility Survey ["The World's Most Expensive Survey," *Sociological Forum*, Fall 1987] that it didn't collect the social background. And, for instance, you and Jay Palmore and Larry Bumpass had an article ["Analyzing Birth Intervals: Implications for Demographic Theory and Data Collection"] in that same issue of the 1987 *Sociological Forum*, which had that criticism of Kingsley Davis of the World Fertility Survey. You said that the World Fertility Survey did not get background socioeconomic data. You feel that's important?

RINDFUSS: Oh, definitely. For a long time, people in our field seemed to act as if babies just occurred in a vacuum, without paying attention to the social and economic environment in which the mothers and fathers of the children lived. And I think we've been making enormous progress in the last ten or 15 years by paying *much* more careful attention to the social environment and the characteristics of young men and women who are the ones who either have births or don't have births.

VDT: On the other hand, you apply very sophisticated statistical methods to the data that *are* available--plumbing deeper and deeper, pulling out layer after layer, it seemed to me, of the data that were there.

RINDFUSS: Yes.

VDT: That, of course, shows your mathematical bent, combined with your sociological interest. Is it important now to get more detailed background data than possible in the usual national surveys, which you have used *very* well, especially the National Longitudinal Survey of High School Seniors of 1972? And for your book with Jim Sweet, you plumbed all the existing data sources. You mentioned yourself the data tapes from the 1960 and 70 censuses and the 1980 CPS. No, I'm getting that mixed up with your latest book [*First Births in America: Changes in the Timing of Parenthood*, 1988].

Was one reason for the American Teenage Study the fact that there was not enough background data in the existing data sets?

RINDFUSS: Yes, definitely. That was part of our interest, and certainly my interest, in doing--or trying to do--the American Teenage Study. [Ronald Rindfuss was codirector of this long-planned, much-anticipated, large-scale study, which was canceled in a barrage of publicity in 1991, after its approval and funding by the National Institute of Child Health and Human Development--a victim of the current extremist conservative political climate in the U.S., as Rindfuss explains here.]

In designing it, we probably spent substantially more time worrying and thinking about the independent variables than we did the dependent variables. That's not to say that we didn't think about the dependent variables and do a good job in designing questions about them, but we wanted to make sure that all the aspects of a teenager's life were covered to the best extent we could. So the design that we had would have allowed us to measure peer group influences, school influences, family influences, friendship influences, as well as community influences.

VDT: Yes, in one of your articles you said, How did teenagers make their decisions about contraception?, and it had to be all the variables that you have just listed.

Now that we're on it, let's ask why it didn't happen--the sad story of the killed-off American Teenage Study--for which, I see, you're still funded through June of this year [1992]. Is there anything one can do, or is it dead?

RINDFUSS: It will probably be ten years from now before I know that it's truly dead, but I think the probability of it appearing over the next year or year and a half is really quite low.

What happened? What kind of political processes took place?

Part of the story is that in addition to our study there was a study designed to measure adult sexual behavior, and there were some real differences in the need for both studies. In terms of adult sexual behavior, there hadn't really been any national data collected since the Kinsey data set. And the Kinsey data set, I think everyone would acknowledge, is really quite deficient in terms of conventional sampling techniques.

VDT: Remind me of the approximate date of the Kinsey work--1950s?

RINDFUSS: I think one came out maybe in the late 1940s and the other one came out in the early 1950s [*Sexual Behavior in the Human Male*, 1948, and *Sexual Behavior in the Human Female*, 1953]. And they were convenience samples; they were not probability samples by any stretch of the imagination. So for the adult study, there was a real need simply to know who's doing what with whom. And that need, obviously, was amplified or heightened by the AIDS crisis.

With respect to teenagers, we've had a series of studies looking at teenage sexual behavior and teenage contraceptive behavior.

VDT: A little aside here. You've not used Zelnik and Kantner studies in any of your publications I've looked at.

RINDFUSS: Yes, I've used them in some of my work.

As I was saying, for the adolescents there's been a long tradition of collecting data on their sexual, contraceptive, and pregnancy experience. Most of the studies have been females, but we've even had some male ones, including, I think, the last Kantner and Zelnik study.

VDT: 1979, yes.

RINDFUSS: Our feeling looking at those data sets--that is the national ones, the Kantner and Zelnik data sets and Cycles II, III, and IV of the National Survey of Family Growth--was that while they did a very good job in collecting the dependent variables--sexual behavior, contraceptive behavior, pregnancy histories--they didn't go deep enough for our tastes in terms of the independent variables. You knew relatively little about the families in which teenagers were living; at best you might know whether they were living with one or two parents. Almost nothing about how well they were doing in school, or what grade they were in, or what their ambitions were. Nothing in terms of their peer group influences that I think everyone anticipates would affect adolescent behavior. You knew relatively little about religious influences. You had a question in many of the studies on, What is your religion?, but you really didn't know what their beliefs were and in many cases you didn't know how important religion was to them. So that's what existed at the national level.

And then there were a whole series of studies that were done in the 1970s and 80s in one or two communities that would provide rich detail for the independent variables, or some of the independent variables I've just described. But you never knew whether you could generalize from one community to another community.

So what we tried to do--to use a demographic term--was to marry the advantages of the national studies to the advantages of the local studies.

And it's ironic that what was attacked in our study and what was of concern were questions

dealing with the dependent variable, questions dealing with sexual behavior. And those kinds of questions, many of them have been asked for a long time, and while we were being attacked, I knew of at least two other studies that were in the field, with federal money, asking about sexual behavior. So part of the question is: Why us?

I'm not sure I'll ever know all the answers to that. What I *can* tell you is that before we were attacked politically, the adult study was--that is, the study that Ed Lauman, Bob Michael, and John Gagnon had been proposing, at the University of Chicago. Their study attracted a lot of attention from people like Representative Dannemeyer from Orange County, California, and Senator Jesse Helms from my state. As near as I can tell based on what they've said publicly, the political concern centered around asking questions about homosexual behavior. And their concern seemed to be that, on the one hand, asking about it says that's okay, that it legitimates homosexual behavior, and on the other hand, they were concerned that the estimates of the size of the homosexual population might prove to be higher than the current estimates that are floating around. And if it came out that the homosexual population is bigger than people thought, their fear was that there would be a more potent political force.

Now, what happened? Several things happened for us. And here I don't have my notes so I won't give you any real dates.

VD: You've given several good talks on it, so it's on record elsewhere. Okay.

RINFUSS: We had submitted our proposal quite some time ago, several years ago.

VD: Who's "we"? Who are the other co-principal investigators?

RINFUSS: In addition to myself, Dick Udry, Barbara Entwisle, and Peter Bearman were involved in the planning of the study right from the beginning. Over and above that, a lot of colleagues at the University of North Carolina helped. We had a national advisory board that consisted of numerous people that you know and respect, as well as comments and suggestions from a wide variety of researchers across the country.

The proposal that was submitted was peer-reviewed; we received a very favorable score. It went through counsel, and we were in the last stage of working through the budgetary details when a request came from someone, I believe in the House--and here I don't know all the details--asking for details about our study. This in turn prompted the Assistant Secretary of Health and Human Services, James Mason, to ask NICHD to stop--or at least to hold up--funding our study until they could review it. The review proceeded--to use the old civil rights term--with "all deliberate speed." It took at least a year, if not longer. During that time, we were basically on hold.

Then in the spring of 1991, Bernadine Healy was appointed head of NIH. James Mason's office finished the review of our study and passed it to Bernadine Healy. Bernadine Healy reviewed our study, liked it--in fact, praised it in the press. There was a story in the *Boston Herald*, I believe it was, where she was quoted as saying that she had personally reviewed it, she thought it was a very good study and this was the kind of the state-of-the-art study that she wanted to see NIH doing.

It was funded in May 1991.

Then in July, Secretary Louis Sullivan was appearing on a talk show, when someone in the audience asked him about our study. I have not seen the show but apparently whoever was in the audience knew a lot about the study, asked some very pointed questions. At which time, Secretary Sullivan said, "Well, I don't know anything about it; I'll find out."

The next day there was a newspaper article on the front page of the *Washington Times*, describing what had happened. It had picked out some of the most intimate questions that we were

asking and printed them on the front page of the *Washington Times*.

VDT: The very conservative *Washington Times*--moonies.

RINDFUSS: I remember the next day, waking up, we had the Today Show on and there was Representative Dannemeyer reading our most sensitive questions on national television. And the irony for us was that we had agonized a lot about the phrasing of questions, but even more so, who would be asked the most sensitive questions. And we had elaborate screeners, such that if they hadn't engaged in other kinds of behavior, they wouldn't be asked the most sensitive questions. The youngest members of our sample would not be asked the most sensitive questions. No one would be asked such questions unless we had parental permission. So there were all sorts of safeguards that we tried to build into the system. And then to see someone criticizing our study, and reading the most sensitive questions on national television at a time when young people might be watching, just seemed terribly ironic.

VDT: Sad, sad.

RINDFUSS: And within about four days, Secretary Sullivan canceled our study.

VDT: It had already been approved by NICHD, hadn't it?

RINDFUSS: Oh, it had been approved. We received our notice of grant award on May 13th and had been working on it for two months before this happened.

VDT: Well, that's sad--such a *notorious* case! Oh, dear. Well, you've said--there's still a great need for it, but in the present political climate . . .

RINDFUSS: Yes, there still really is, and increasingly so. I'm sure you're aware of the NIH reauthorization bill, which is now sitting in the House-Senate Conference Committee. That reauthorization bill explicitly says that there is a need for NIH to do research on human sexual behavior, and it says that basically as long as there's a public health need, NIH is authorized to do such research.

VDT: Will Jesse Helms cut that out?

RINDFUSS: He tried to; he was voted down.

VDT: Well, perhaps there's some hope again, indeed.

NICHD has funded most of your work. Of course, they have stressed in the past--I've heard Wendy Baldwin speaking about this--using existing data sources. But here, they were going to fund a new survey, just as they have the National Survey of Families and Households.

By the way, did you think that, well, "Jim and Larry got their survey; I've got to get mine too"?

RINDFUSS: No.

VDT: That was just coincidence?

RINDFUSS: Yes.

VDT: Here they were going to--and *had*, actually--fund something brandnew?

RINDFUSS: Yes. I think the reason is that it was felt by people both within NICHD and others that the time was right, that we needed to go beyond descriptive studies or the small-scale community studies; that we really needed to have a resource for the entire research community that was rich in independent variables and national in scope. Just like many of the other national studies that are going on, our plan was to release the data to the entire research community just as quickly as we could.

VDT: Just like Larry has said, with his and Jim Sweet's National Survey of Families and Households, he opens up the latest journal and wonders who's going to have done what, using their data, because it has been made so widely available.

Well, that *is* a very sad thing.

What of your Asian studies; what explains them? You have done quite a lot in Asia.

RINDFUSS: Right. Early on, there were several streams of my Asian research, and they followed the distinction I was telling you before, between looking at the time of first birth and looking at the determinants of lengths of birth intervals after the first birth.

In terms of examining the lengths of birth intervals after the first birth, shortly after I had moved to North Carolina I went to a conference at the East-West Center [in Honolulu]. It was actually a conference examining own-children methods, which was a technique that Jim Sweet and I used in the 1977 book. While I was there, I ran into Jay Palmore, whom I'd met three or four years earlier. We just started talking. I was showing him some results that Larry and I were obtaining in the United States, and he was telling me about some data and interests that he had had in looking at similar issues in Malaysia. So one thought led to another and we said, "Well, it really would be nice if we could look at a number of Asian countries and the extent to which there were similarities or differences in how social processes affected birth intervals."

And to turn a long story into a short one, we applied for a grant and were successful in getting it and several other grants after that.

VDT: You did that fine article, looking at eight different countries [e.g., Rindfuss, Bumpass, Palmore, et al., "Childspacing in Asia: Similarities and Differences," World Fertility Survey Comparative Studies, Number 29, 1984], with Malaysian data from Jay Palmore and the Korean 1974 fertility survey, and the Philippines.

RINDFUSS: Yes. And so, maybe over a period of eight or nine years, Jay and Larry and I were collaborating on comparative studies of birth intervals. That's one avenue by which I entered Asian demography.

And I was also interested in the timing of first births, and when I arrived at Chapel Hill, one of the persons that I'd heard a lot about from friends at Wisconsin was Charlie Hirschman, who was then on the faculty at Duke University.

VDT: He just bought a copy of *Demographic Destinies*, here at the meeting.

RINDFUSS: Wonderful! Actually, I'd been telling him how good they are.

And Charlie and I were talking about some of his interests in Asia and I was talking about some of my interests in the timing of first births. Again, we applied for a grant and were successful.

VDT: That was NICHD too?

RINDFUSS: One was NICHD and one was National Science Foundation. So that started my other stream of Asian research.

And then since then, I've been fortunate to collaborate with a number of Asian scholars, including--well, the person I've worked with most in Asia is Apichat Chamrathirong.

VDT: It's nice to have him here [in Denver]. He was at our Psychosocial Workshop [preceding the PAA meeting], he and his other colleague from Mahidol University [Bangkok]. That's the first time I've seen them at a PAA meeting.

RINDFUSS: No, people from Mahidol have been coming to PAA meetings at least for six or seven years.

VDT: Did he work with you at North Carolina?

RINDFUSS: No. He's visited North Carolina maybe for a period up to a month, but he's never spent longer than a month in North Carolina. Nor have I ever spent more than three to four weeks in Thailand.

VDT: But you have been going to Nang Rong, this village. Why did you choose that? I should tell you that I spent two years in Bangkok--my husband was with ECAFE [Economic Commission for Asia and the Far East] from 1958 to 60--so I have an abiding interest in Thailand. You've chosen Nang Rong, a village on the Cambodian border?

RINDFUSS: Nang Rong is actually a district and we have data from 50 villages. I was not involved in the first round of data collection. Apichat and his colleagues were involved in the start. The initial purpose of that study was to evaluate a broad-based modernization program which was about to be started then.

VDT: Which also you did in the Philippines; you've done that there too.

RINDFUSS: I wasn't involved in the Philippines data collection at all.

VDT: I understand, but the data were collected to look at the effects of a similar modernization project.

RINDFUSS: Right. In Nang Rong, I wasn't involved in the first round of the data collection, but then for the second round, I as well as my colleagues, Barbara Entwisle and David Guilkey, made some suggestions. Then we wrote a grant proposal to analyze the data.

VDT: There you are looking at community variables that you would have liked to get also in your teenage study?

RINDFUSS: Yes.

VDT: Most of your research and publications involve teamwork. Is that the only way to go now?

RINDFUSS: I personally think that the quality of the research will be better if you have an effective

team of researchers, but different people have different styles. Certainly, it works best for me. I know what I'm good at; I know what my limitations are, and it's nice to have other people working with me that are good in areas where I'm weak, and vice versa.

VDT: I asked Larry this question, as you recall, how you worked together, and he said his style of working with Jim Sweet is different from his style of working with you. Give me an example of how you would work with Larry, across the universities--picking up the telephone, faxing, or what?

RINDFUSS: In many cases, yes, it's picking up the telephone. There was one project where we were collaborating and the third person was Gray Swicegood [University of Illinois]. On maybe two or three different occasions, we decided that the most convenient place to meet was in the hotel near the airport in Chicago and we would go there. It had the advantage that there were no interruptions from students or committee work or anything like that.

Sometimes, like when Larry and Jay and I collaborated, we would get together at the East-West Center and work there.

I completely agree with what Larry says, the style is different depending on who I'm collaborating with, and sometimes it's different on different projects working with the same person.

VDT: Give me an example. Do you decide how you're going to do the statistical analysis, or do you have the original ideas; do you write the first draft?

RINDFUSS: Oh, it varies a lot. I've done some work with David Guilkey and there the decision is fairly clear. He's an econometrician and he takes main responsibility for the statistical areas and I take main responsibility for the sociological and the sociodemographic end. But when I work with someone like, let's say, Larry Bumpass or maybe Phil Morgan or others, where the difference in terms of social content and statistical expertise isn't quite so apparent, it works out differently in each paper. Sometimes one person will take the lead in the analysis; sometimes we'll sit down and work it out together; sometimes one person will write an entire first draft; sometimes we'll write it in pieces and then try to convert it into something that has some overall coherence.

VDT: And you're very good about giving credit to everybody who helped you. I particularly liked the way you started out your published presidential address of last year, "The Young Adult Years: Diversity, Structural Change, and Fertility," when it appeared in *Demography* [November 1991]. You pointed out that, "Although this is, by tradition, a single-authored article"--and I must say it's probably almost the only one on which you've had *only* your name, except your first one. I noticed you had in the Research Reports of the Commission on Population Growth and the American Future [1973], your name only appeared on a very short chapter ["Recent Trends in Population Attitudes"].

RINDFUSS: Yes.

VDT: "Numerous individuals and groups provided invaluable assistance." And you went on to list many, starting with NICHD, giving the funding, and including your family. I liked that; that's how I knew your sons' names. That was a nice touch; I hadn't seen that before.

RINDFUSS: Thank you.

I really do think that the nature of a lot of demographic research right now requires the collaboration of a lot of people, and I think it's important that everyone get recognized.

VDT: Good. Incidentally, you had no title for your address even in the final program for the last meeting. We all said, "What's he going to talk about?" It had never happened before, and this year, Etienne [van de Walle] also has no title [in final 1992 program, for his presidential address], so it will be a lovely surprise. We were all wondering what it was going to be.

But, it was *so* interesting. Your singling out of the 18-to-30 adult years, that was a new age division. Did you think that up?

RINDFUSS: Yes.

Let me speak about the title first. Quite literally when the program had to be printed, there was still considerable uncertainty in my mind as to what I was going to say.

VDT: Did you have your topic, or do you mean what you were going to say about the topic?

RINDFUSS: Well, even the full extent of the topic. I decided that I was going to give myself enough flexibility that I wasn't going to put a title in the program. And then I was really keyed toward producing the oral version of the talk, so there was no need to think of a title. And after the PAA last year before I had to send off to *Demography*, I agonized endlessly over what to call it.

VDT: What did you have? You called it *something*.

RINDFUSS: I can't remember right now. I didn't have *any* working title for the longest time.

VDT: I mean as you started off the talk. I presume you had some title; I can't remember now.

RINDFUSS: In the talk itself?

VDT: Yes.

RINDFUSS: No, I didn't have a title.

VDT: Oh, you didn't!

Well anyway, your age division, 18 to 30, as you say you probably thought up, and the "demographic density" of those years. That has stuck in people's minds; I think every presidential address, all the outstanding ones, have. For instance, you recalled the "multiphasic responses" of 1963, Kingsley Davis's speech, not to mention a few of your close predecessors, Sam Preston's and Jane Menken's--all outstanding speeches.

And then there's the graph, which I love, which has appeared in several places now--the demographic density graph [see, e.g., "Figure 3: Composite of Fertility, Migration, Marriage, School Leaving, and Unemployment Rates," page 498 of Rindfuss address in *Demography*, November 1991, shows cluster of highest rates at ages 18-30, approximately]. How did you come by all that?

RINDFUSS: This graph, actually, is a good illustration of how sometimes your best thoughts are accidental or they're afterthoughts. This graph was prepared because I was looking at the component graphs and I would sometimes place one on top of the other and hold them up to the light and finally I said, "I wonder what it would look like if they were all together?" So we produced the composite graph. When I first looked at it I kind of liked it but I wasn't sure how it would go over. This one in particular I showed to a number of my colleagues, to get their advice.

VDT: That was the one handout that you had, wasn't it, that everybody had?

RINDFUSS: It was a slide.

VDT: Well, it stuck in the mind, and then it came out in *Population Index* [Summer 1991], on the cover.

RINDFUSS: Yes. And I think it was in *Population Today*.

VDT: Oh, yes! [In same issue, May 1991, with coverage of 1991 PAA meeting, as graph in PT's regular "Speaking Graphically" feature, page 2, with the title, "Young Adulthood is a Busy Time," and the note: "Don't even *try* to disentangle the lines of this graph. During his presidential address at the 1991 PAA meeting, Ron Rindfuss used it to illustrate the 'demographic density' of the young adult years, ages 18-30. This 'everything graph' followed a series of separate graphs that showed young adulthood as a peak time for several demographic measures: fertility rates, first *and* second marriage rates, unemployment, transitions from school to work, and high residential mobility rates."]

You followed in the vein of Bumpass and Preston and Menken, speaking [in PAA presidential addresses] on issues of very current interest in the U.S. It was right on target. We'll get to it a bit later.

Now, let's ask some of the questions I said I was going to ask you: Who have been some of the leading influences in your career?

You have explained Peter Donaldson [currently Population Council representative in Thailand]. I had him down here, because most presidents choose someone very influential in their career to introduce them, and there was Peter Donaldson and he was unexplained.

RINDFUSS: Yes. I've mentioned a number of them. Mary Powers is another example; she was the person who taught me the first population course I had. And then, of course, the faculty at Princeton--Charlie Westoff, Norm Ryder, Ansley Coale--which was very important in shaping my early interest in the population field.

And, of course, going on to Wisconsin and working with Jim Sweet and Larry Bumpass.

And then, quite frankly, I've been just enormously lucky in working with a wide variety of collaborators. It sounds like you've looked carefully at my vita and you know that there are a lot of people that I've worked with.

VDT: Indeed, you have. What about some of your leading students?

RINDFUSS: I was afraid you were going to ask that. Here we get into the name list. I'll not name everyone, but I'll say several things. First, let me talk about some of the post-docs that I've worked with. And again, I've been just very fortunate for, oh, the last dozen years to have every year either one or sometimes two post-docs that I've been working with. I hope they've benefited, but certainly I have. Post-docs, as I was saying before, have total freedom in their time, which means that they can not only do a lot of research but they can press me to carry my end of the bargain. It has been very rewarding.

VDT: I noticed, for instance, Audrey VandenHeuvel, who's now in Australia [coauthor on "Cohabitation: Precursor to Marriage or An Alternative to Being Single?", *Population and Development Review*, December 1990]. Is she one?

RINDFUSS: She was a graduate student.

VDT: And Joan Kahn [coauthor on "Adolescent Contraceptive Method Choices," *Demography*, August 1990].

RINDFUSS: She was a post-doc.

VDT: And Betsy Stephen [coauthor on "Racial Differences in Contraceptive Choice: Complexity and Implications," *Demography*, February 1988].

RINDFUSS: She was a post-doc.

VDT: Okay! I guessed, because now they've gone on.

RINDFUSS: Some of the other post-docs: Phil Morgan was a post-doc, as was Gray Swicegood [coauthors on the monograph, *First Births in America: Changes in the Timing of Parenthood*, 1988]. Recently, I have written a number of things with Elizabeth Cooksey, who just finished being a post-doc. Karin Brewster is a post-doc whom I'm currently working with. And I know I'm forgetting some.

VDT: Often their names are first on your articles. Is that because it's their main work and you've been in on it? Or how do you decide on who's senior author?

RINDFUSS: That's sometimes an awkward one. There's no real hard and fast rule. In most cases, it's clear who ought to be first author. Then in some cases, what happens is that we'll be working on a series of papers and while with any one particular paper it might not be clear who ought to be first author, what we'll aim for is balance over the course of two or three or four papers, so that in the long run it doesn't matter. Rather than worrying about a single paper, look at the stream of research.

VDT: And what do you regard as your leading publications and why? You've had so many.

RINDFUSS: [Laughter]. Oh--no, no. Well, in some ways, the book with Jim Sweet [*Postwar Fertility Trends and Differentials in the United States*, 1977] and the book with Phil Morgan and Gray Swicegood have been important, probably just because books *are* important. You can cover a much broader set of topics than you can with a typical article.

VDT: The material in each of your books has appeared before in articles, I've noticed.

RINDFUSS: Yes. They contain some material from articles, as well as new material.

VDT: It sums it up.

RINDFUSS: It sums it up; it allows you to have an expansive introduction and a conclusion. Books also allow you to publish some material not suitable for articles. So I would certainly highlight those two.

I think the presidential address ["The Young Adult Years: Diversity, Structural Change, and Fertility," *Demography*, November 1991] is just recent enough that I'm still very happy with it.

VDT: You can well be; it's an outstanding one.

You write so memorably. For instance, in your 1991 address: "Demographers are adept with boring data; we disaggregate." I love that. You made another funny remark yesterday as discussant in

the teenage childbearing session; unfortunately, I lost my notes.

And then [in the presidential address]: "The sparsity in middle age after the density of activity in young adult years is undoubtedly the demographic seed of the fabled midlife crisis." [Laughter] That's a lovely one!

What explains these nice touches? They're not so possible in coauthored articles.

RINDFUSS: Well, with the presidential address, I had the freedom to spend a lot of time on each and every paragraph, whereas in a normal article I often don't have the luxury of multiple re-writes. So I think that's part of it there.

And I also like writing. I enjoy the writing process. I enjoy having a few humorous touches, if you will. What often happens with my coauthored papers is that my coauthors convince me that the lines that might be humorous ought to be taken out.

VDT: That's too bad. Have you ever written for the popular press?

RINDFUSS: No.

VDT: You know there's a continuing discussion within PAA that more of our material ought to get into the popular press; it would get more attention.

RINDFUSS: I've certainly spent a lot of time talking to reporters, and not just because of the American Teenage Study. I've talked to reporters over the years about matters dealing with American fertility and marriage behavior.

I share the opinion of many that we should do more to make our research accessible to a broader audience, but I haven't done any writing along those lines.

VDT: Well, perhaps that will come.

What accomplishments in your career--to date, because as you pointed out [in letter confirming this interview] you've got a long way to go--have given you the most satisfaction? Now, I know you've had the biggest *frustration* with the teenage study.

RINDFUSS: Yes, that would rank up there with the biggest frustrations.

I think one of the most satisfying aspects of this business is to watch former students and post-docs enter the profession and blossom, basically.

And I think another aspect of satisfaction is the actual process of doing research, ranging from thinking about the problem to talking with collaborators, to spending a lot of time worrying about the coding and doing just the formalities, and then finally getting the first results. I find that satisfying, enjoyable.

VDT: That's great; means you're a great researcher.

And then I also told you I'd ask you, What do you view as the leading issues in U.S. demography over the two-plus decades you have been involved? Of course, you've been involved in demography of Third World countries too, but let's first talk about the U.S.

RINDFUSS: There's a tendency to have tunnel vision--not necessarily tunnel vision, but there's a tendency to remember the most recent controversies that you've dealt with--and you have reminded me of my comments yesterday at the session dealing with the determinants and consequences of adolescent pregnancy ["Teenage Childbearing: Non-Economic Implications," Session 16 of 1992 PAA

meeting]. It's certainly been an area that's been controversial for at least a decade, if not longer, and it's important because it has not only some very real scientific implications but there are also a wide variety of policy implications.

VDT: Didn't I hear you say, well, the papers that you reviewed, none of them were quite right?

RINDFUSS: I tempered that by saying that I really don't expect a perfect paper in this area, and I include my own work. It's an area where many of the variables are very complexly and causally intertwined, so it's very difficult to untangle.

VDT: You said each paper was adding incrementally to the knowledge.

RINDFUSS: Right.

VDT: You were rather hard on them--well, three of them were using the National Survey of Youth, which you have not used?

RINDFUSS: Correct. I hope I wasn't too hard on any of them.

VDT: No. You had a clever criticism of one of them--I've forgotten what it was--on the one that looked at cousins; that was complicated. You said you had written in a 1980 paper already, that had to do with education and fertility [with Larry Bumpass and Craig St. John, "'Education and Fertility: Implications for the Roles Women Occupy," *American Sociological Review*, 1980, pp. 431-447]. What had you written in that paper?

RINDFUSS: In that paper, we showed that while education has an effect on the time of the first birth, the causal direction the other way appears to be insignificant, that is, our results showed that there was no statistically significant effect of having an early birth on terminating one's education. And that was counter to the conventional wisdom at the time.

There continue to be arguments about this. There are a number of people that have since found results that are similar to the ones that we found; there are people who find the opposite. There were two sessions yesterday that dealt with this controversy. There will be a two-day meeting at NICHD in the middle of May, dealing with this controversy.

VDT: About whether or not education is terminated by an early first birth?

RINDFUSS: Well, it's broader than that: Are there detrimental consequences for the mother or the child of having a birth in one's teen years?

The one paper yesterday dealt with the consequences for children. There are other papers that talk about health consequences for children, socioeconomic attainment of the mother, marital stability, and so forth. There are a wide variety of outcomes that people have been looking at and talking about, and reaching different conclusions.

VDT: You haven't mentioned the fertility trends of these two decades. You said in your presidential address--and I thought that was very clever of you--that there was possibly going to be a fertility increase, and you were using data--the latest you had, I believe, was 1987, or 88.

RINDFUSS: No, 88 and 89.

VDT: Well, 1989 and 90, it *has* gone up.

RINDFUSS: Yes.

VDT: Which you would attribute to there no longer being such constraints on fertility set up by being a single mother, or a working mother, because they are coping.

RINDFUSS: Yes.

VDT: Men are becoming almost immaterial; women can do it. And then fertility did indeed go up in 89 and 90, but then it dropped again in 91, probably due to the recession. Now, that's another thing; you've stressed period effects over cohort effects.

RINDFUSS: Very much so. I'm not sure that Norm Ryder will ever forgive me for that.

VDT: Exactly! I was going to say, that this is contrary to Norm Ryder, who obviously has been important for you. Even before you told me about it, it was obvious.

RINDFUSS: Yes.

Let's go back to what's likely to happen to fertility in the future. What I tried to say in the presidential address is that a number of fundamental constraints that used to exist, leading toward lower fertility, seem to be becoming more and more relaxed. Now, that doesn't necessarily mean that fertility will go up; it just means that conditions are ripe. Think of it like these severe-weather bulletins that you get from time to time, saying that conditions are ripe for a tornado or conditions are ripe for a thunderstorm. No one's making predictions that there necessarily will be one, but that the probabilities have increased. And that's basically what I was saying. And that does run counter to what a lot of people have been saying about the future of fertility in the United States.

VDT: You mentioned the famous Charlie Westoff paper of 1978 ["Marriage and Fertility in the Developed Countries," *Scientific American*, Vol. 239, No. 6, 1978].

RINDFUSS: Yes.

VDT: There could never be anything but low fertility.

RINDFUSS: Right. Charlie would disagree with me. Larry Bumpass, I think, would also disagree with me; in fact, I *know* he would disagree with me. And a number of other people would as well. And, we'll see.

VDT: But it *did*; you don't have to see. It went up to a total fertility rate of just over 2.

RINDFUSS: Right.

VDT: But then the numbers of births went down again last year, doubtless due to the recession. You were talking about the period effects there.

RINDFUSS: In the United States, the average woman, the average couple, has two children and they

have roughly a 35-year period in which to have those children. So, for many of them, the decision involves, When should we have the children? Should it be this year; should it be next year; should it be sometime later? And when you're in a situation where the decision is about when to have children rather than whether to have children or how many children to have, you're much more likely to be influenced by period effects.

Now, if there's a recession and, let's say, the husband or the wife is unemployed, or if they think there's a higher risk of being unemployed, they might decide to postpone their fertility. Or if you have some sort of major military action, people might decide to postpone their fertility, assuming there are not exemptions--a draft based on parenthood status.

VDT: Or hurry it up--nine months later it happens. Very plausible.

We won't talk about the Third World, because I really think of you as a U.S.-based demographer.

RINDFUSS: Half of my work over the last decade, if not more, has been Third World, but that's fine.

VDT: Well, what *do* you feel about the Third World? Are you pessimistic--on the trends in fertility?

RINDFUSS: I haven't really addressed that, so, you're correct; let's not address that.

VDT: All right. Now on PAA. Do you remember your first PAA meeting? I have the list here.

RINDFUSS: Sure. I believe it was New Orleans; it was 1973.

[RINDFUSS addendum, based on recollections subsequent to the interview.]

I remember now that I attended the PAA meetings in Toronto, which were the year before the meetings in New Orleans--perhaps my aging process is accelerating.

I drove to the meetings from Princeton. I must have been preceded at the border crossing by several demographers, because the somewhat perplexed immigration official wanted to know why all these demographers were invading Canada. I remember trying to reassure him that our invasion would be quite temporary.

Those meetings were also controversial, because the hotel had a bar that only admitted men. Such a policy did not sit well with a broad cross-section of the PAA membership.]

INTERVIEW CONTINUES

VDT: You had already been at Princeton a long time. How did you manage not to go to the meetings in Atlanta [1970] or Washington [1971]?

RINDFUSS: The student culture at the time was that students didn't normally go to population [PAA] meetings.

VDT: But that's changed.

RINDFUSS: That's changed very much. I'm not sure if--I'm describing my memory of the student culture at Princeton; it may have been different at other universities. But it's now changed at most of the major training centers.

VDT: And what do you remember about New Orleans?

RINDFUSS: [Laughter]

VDT: Oh, come on; I've heard other funny stories [about first PAA meetings].

RINDFUSS: There was one graduate student from a developing country who had been separated from his wife for some time. His wife had remained back in his home country and for much of the meeting, we never saw him, and it turns out that he thoroughly enjoyed all the activities on Bourbon Street.

VDT: Oh boy! We were in the Monteleone Hotel, which is right in the French quarter.

RINDFUSS: Yes, it was. That is another thing I remember about the meeting; that the Monteleone heavily overbooked and many people who had reservations were turned away and were at satellite hotels all over the place. So there were a lot of very unhappy demographers, at least the first day.

I remember the feeling that many graduate students will describe to me now and that is how nice it was to finally put faces with the names of people whose work I'd been reading for years.

And I remember how few people I actually knew; how few people I could walk up to and say, "Hi, I haven't seen you in a while." It takes going to PAA for several years before the meetings serve the function that they do for me now, where I get a chance to see people, old friends, old colleagues, former students, and so forth. Whereas, students going for the first time, they know their fellow students, they know their faculty members from their own university, but they tend not to know many other people.

VDT: That's why I'm working hard getting my niece [Wisconsin graduate student] around here.

RINDFUSS: I think that's important.

VDT: But not many people have an aunt there.

How do you view the changes over the years? Let's talk about the numbers. In 1973 in New Orleans, there were 862 registered; that was a lowish number. Even here in Denver, which we thought would be rather low, it's almost up to 1100.

RINDFUSS: Yes. And last year in Washington, we had 1400 [1,399]. I think one way to view these numbers is we're an organization of about 2700, 2800 members [2,674 at the end of 1991], and to consistently have 1100, 1200, 1300, 1400 members show up for the annual meeting, I think speaks very well for the organization.

VDT: It does, indeed.

RINDFUSS: Most other organizations I know of, the proportion attending the annual meeting is much lower.

VDT: How do you account for that?

RINDFUSS: Great organization--filled with smart, enjoyable people. I think, compared to some of the other meetings I go to, the sessions are much better, the quality of the papers is better, the attendance at the sessions is better, and the dialogue that takes place is better.

VDT: Do you think people go to the sessions knowing they're going to be good? It's true; here the corridors are empty during the sessions; people are *in* sessions.

RINDFUSS: Yes, and that's not true in many other professional meetings.

VDT: I've heard that from many others.

You think that even though they're now up to 95 sessions, this year--even beating your almost-record of last year, 90, nine overlapping--that's not too frustrating?

RINDFUSS: I deliberately held the number of sessions to the same number that we had the year before [1990]. Yes, I worry about the number of concurrent sessions. It's frustrating when you look at a time slot and see two or three sessions that you want to attend and you know you can't go to all of them.

But on the other hand, we've had growth in the quality and the quantity of research that's being done, and it's important to allow an outlet for such research. So, there's a real tension there, and I don't think there's an easy solution.

VDT: Let's talk about your program of last year. What did you do with all the papers you couldn't fit in? Many of the interviewees have told me they were frustrated they couldn't get all of them on the program, or even more.

RINDFUSS: Very much frustrated. For a number of them, I wrote a letter saying, "Sorry, but we can't use your paper."

Of course, I don't see all the papers that don't show up on the program, because the organizers of individual sessions make choices and they will sometimes send papers on to the president but sometimes they'll just write back to the persons saying, "I'm sorry but I can't use your paper." So I have no way of knowing how many papers that were submitted actually did not appear on the program.

VDT: You were on the PAA nominating committee, I noticed, in 1987-88. What are the criteria in choosing candidates for the Board? We'll get to the presidency in a moment.

RINDFUSS: That's a good question; I don't have a good answer. I don't remember when I was on that committee whether we talked about criteria. One thing that certainly happened is that the forms that are sent out each year, asking for nominations from the membership, are distributed to the members of the committee. We all looked at them. And I know in my case they influenced me. I was amazed at how much consistency there was in certain people being nominated.

I think the other thing that at least influenced *me* is that we had lists of who had been on the Board and who had been vice president and so forth in the past. And we're still a small enough organization that we know many of the people who would be appropriate candidates.

VDT: And what is your idea of how someone gets nominated to be president? Now, you yourself were nominated quite early on. As I pointed out to you, you're the first baby boom president, being born in 1946, though Jane Menken [president in 1985] and Larry Bumpass [1990] and Sam Preston [1984] aren't much older. But, still, you're a baby boom president; you rose to the top pretty rapidly.

RINDFUSS: I guess so. [Laughter] I don't know why.

VDT: And you were running against Paul Schultz, who was running for the second time and he was defeated the second time. That seemed too bad.

RINDFUSS: I felt very bad about that. People aren't nominated to run for president unless they're well known and they've done high-quality work. The sad part about it is that one will win and one will lose, but I don't know any other way to run the system.

VDT: Will there continue to be a core interested in the workings of PAA as an institution? Now, it's always sad to see how few come to what we still persist in calling the "business" meeting--yesterday's annual membership meeting. It's held in too large a room. I think the program committee should be more pragmatic about it next year--put it in a smaller room. For several years now, it's been in too large a room, with a scattered number of people. Why is that?

RINDFUSS: Well, under the constitution, very little happens at the business meeting.

VDT: Since the constitution changed in 1974. You're the one who brought it up yesterday, that change, because we almost had some fiery works.

RINDFUSS: Yes, we did.

VDT: That gentleman who got up and wanted us to vote--was it pro-abortion? I've forgotten now.

RINDFUSS: I think it was funds for family planning in developing countries.

VDT: Okay, the kind of thing that brought up fiery debates in the early 1970s and on which the decision *could* be made at the business meeting, a vote could be taken. And then it was changed.

Larry Bumpass--I expected him to be the one to stand up and say that's no longer allowed by the constitution, because he was on the committee to bring about that change.

RINDFUSS: Oh really? I hadn't realized that.

VDT: It came up at the end of his interview last year. And you were very prompt to hand to Etienne [van der Walle] the constitution with that clause. [PAA policy is decided by action of the Board or referendum of the membership by mail, Article XIII of the revised constitution of 1974. The annual membership or "business" meeting, where policy could be decided before 1974 by votes of those attending the meeting, is now "a forum for open discussion of the affairs of the Association," Article XI.]

RINDFUSS: It was fortunate; Bob Willis had brought the constitution with him. He had it in his papers because at the Board meeting on Wednesday, we approved some changes to the bylaws of the constitution. So it was in everyone's papers, but fortunately Bob brought it with him.

VDT: I knew exactly where it was in the membership directory.

Is that all there is to it? It means the business meetings are dull, but people who care about PAA as an institution . . .

RINDFUSS: I think they care.

One of the things I've noticed in the last couple of years is that the average age of people at the business meeting is older than the average age of the organization as a whole.

VDT: Absolutely. Henry Shryock and Paul Glick and Charlie Nam were in the row behind me.

RINDFUSS: Exactly. I looked out and there were a lot of past presidents, a lot of past Board members, and they know very intimately the nature of the Association's workings, so I think it's more meaningful to them than it would be to a student member or a fairly recent member.

VDT: Well, let's suggest anyway that they have it in a smaller room next year.

Now, some of the important issues in PAA as you've seen them over the years that you have been involved. For instance, the tensions between the mainstream demographers and the applied demographers, which crop up from time to time.

RINDFUSS: They're still cropping up. As you know, I appointed a committee to look at the broad question of what are now called interest groups. If you look at the interest groups that we have right now, there's state and local government and business, China, and migration--four that we have now. And we have a request to form one on aging. And if you look at those five, there's no real coherence to them. One of them is a country; two of them have more to do with employment; one has to do with the traditional subject matter of the field. What I'm hoping the committee will do is carefully look at these issues and suggest something that will rationalize the system more than the ad hoc approach that we have right now.

One of the worries that I have--and it's come up at all the Board meetings that I've been to recently, with the exception of the one this week, and it didn't come up this week because the ad hoc committee is still deliberating--is that, on the one hand, you want to make sure that the organization serves the needs of all its members and, on the other hand, you don't want the organization splintering into a large number of special interest groups. There are other organizations that I belong to where that process has happened, and people wind up identifying with their interest group or groups and not really identifying with the broader organization.

I expect we'll have a report from Larry Bumpass's committee at the fall Board meeting; discussions have been taking place here.

That's one big area that I had to deal with.

Another area I had to deal with involved the public affairs work.

VDT: That's been changed; what big change happened?

RINDFUSS: The year before, Larry Bumpass [president in 1990] had appointed a committee that was chaired by Ren Farley to look very broadly at the Association's public affairs activities and last fall Ren came in with his report.

There was a lot of discussion, and basically the Board said that we've been in the public affairs business for about a dozen years [Public Affairs Committee established in 1979], that we've been very well served by it, that it's an extremely important part of the Association's activities, and that it was now time to routinize the committee membership structure, and that we would sever our financial relationship with the Population Resource Center.

The decision was that we would hire someone ourselves and have our own public affairs specialist. We will be cooperating with the newly formed Association of Population Centers.

VDT: Would no longer work through the Population Resource Center?

RINDFUSS: We hope to continue having close collaborative ties with the Population Resource Center.

VDT: Oh, really?

RINDFUSS: Oh, definitely. Their agenda and our agenda overlap on many issues.

VDT: You don't feel that that will mean that PAA is less in the political scene that it has been?

RINDFUSS: No, I think if anything we'll be more in the political scene. Finishing touches are being put on the arrangements now.

VDT: Very good. Quickly, a few other things. Do you think the business and state and local people will stay in PAA?

RINDFUSS: Very much so. I don't want to talk about it now, but we had a meeting yesterday that included representatives from the state and local [government] and business committees and members of Larry's committee. I do think we worked out the broad outlines of a solution to the interest group issues that will make the state and local and business people very happy.

VDT: That's good. Do you think PAA should actively recruit more members? As you know, the membership dropped somewhat last year. Though you talked about 2700/2800, it isn't; it's 2,647 at the end of 1991 [down from 2,752 at the end of 1990]. Probably a recession effect last year.

RINDFUSS: Yes.

VDT: And it seems to have been stuck at about 2,600 since the mid-1970s.

RINDFUSS: I don't have a strong opinion on that. There are some advantages to going through a very active recruitment process and there are some disadvantages. I haven't been forced to think carefully about where I come down on it, so I don't have a strong opinion.

VDT: Back to you for one moment: How do you manage your very heavy schedule? You are handling a lot of balls, and you seem so unflappable, a calm person.

RINDFUSS: [Laughter] Well, I'm not sure I'm unflappable.

One way I manage to handle it is that, as you pointed out, most of my work has been collaborative, and I've been just extremely fortunate to work with people who are talented and efficient and so forth, and that's helped enormously. The staff in the sociology department and in the Carolina Population Center have also helped enormously. There's not much else I can think of that let's me do it.

VDT: And you have a nice family life; I can tell that. I met your wife Maggie at Maryland.

RINDFUSS: That's right.

VDT: Has she worked through the years?

RINDFUSS: She's done a variety of things. Right now, she's a freelance writer; she's writing short stories. She's been doing that for the last couple of years.

VDT: Interesting! And your two sons are in college?

RINDFUSS: Yes.

VDT: Both at the same time--at North Carolina?

RINDFUSS: One's at North Carolina--he'll be a junior next year--and one's at Tulane and will be a sophomore next year.

VDT: What do they study?

RINDFUSS: The one who's going to be a junior is planning to be a psychology major, and the sophomore is undecided.

VDT: And your plans for the future? As I mentioned, you had said in a letter there would be a better historical perspective on your career if this interview took place ten years hence. I hope there will be a followup, because I'm sure you will be accomplishing a lot more.

RINDFUSS: Well, I hope so. [Laughter]

VDT: *Including* the teenage survey.

RINDFUSS: I'm not sure about that one.

VDT: Thank you very much, Ron. Let me just take a photo of you.

RINDFUSS: Okay.

INTERVIEW CONTINUES

VDT: We're just talking about the presidential address, which we're going to in a moment, of Etienne van de Walle, and you say it's a wonderful tradition that people pack into the room to hear the address.

RINDFUSS: And that's certainly not the case in many other organizations of which I'm a member. We have a tradition here of presidents spending a lot of time and working carefully on their addresses. I'm looking forward to Etienne's.

VDT: You feel that--well, it's the climax of a career; it's what you're pretty well known for. I think Sam Preston will forever be his "Children and the Elderly" ["Children and the Elderly: Divergent Paths for America's Dependents," address of 1984], and Kingsley Davis his "multiphasic response" ["The Theory of Change and Response in Modern Demographic History," 1963].

RINDFUSS: Yes.

VDT: And you will be your young adult years.

RINDFUSS: You're a good enough historian to know that we have to wait five or ten years before we know how I'll be remembered for that.

VDT: I think Etienne van de Walle is going to be either Africa or historical demography. What's it going to be?

RINDFUSS: I've talked to him; it'll be historical.

VDT: Really--not Africa! We'll see. [Entitled "Fertility Transition, Conscious Choice and Numeracy," van de Walle's address dealt with the concept of family size through history, using examples both from Africa today and Europe in the past.]

ETIENNE VAN DE WALLE

PAA President in 1992 (No. 55). Interview with Jean van der Tak at the Population Studies Center, University of Pennsylvania, Philadelphia, February 17, 1993.

CAREER HIGHLIGHTS: Etienne van de Walle was born in Namur, Belgium, and remains a Belgian national. He received a doctorate in law in 1956, an M.A. in economics in 1957, and a Ph.D. in demography in 1973--all from the University of Louvain. From 1957 to 1961, he was a researcher with IRSAC (Institut pour la Recherche Scientifique en Afrique Centrale) and demographic adviser to the government in Ruanda-Urundi. In 1961-62, as a Population Council Fellow, he took the special program in demography at Princeton. From 1962 to 1973, he was on the research staff of the Office of Population Research at Princeton, working first on the project that led to the groundbreaking monograph, *The Demography of Tropical Africa* (1968) by William Brass, et al., of which he was one of seven authors, and then on the European Fertility Project. He was a visiting lecturer in the department of demography at the University of California, Berkeley, in 1971-72. Since 1972, he has been at the University of Pennsylvania, where he is Professor of Demography, Director of the African Demography Training and Research Program (since 1980), and has been Chairman of the Graduate Group in Demography (1973-78; 1987-89) and Director of the Population Studies Center (1976-82). Many of his other activities--as a mission or committee member, conference organizer, external examiner at universities, etc.--have been related to African demography.

Etienne van de Walle is renowned in the population world for his research and publications in two seemingly very different areas--European historical demography and the demography of sub-Saharan Africa. In historical demography, his publications include *The Female Population of France in the Nineteenth Century* (1974) and several important papers on the fertility transition in France and in Europe generally and the extent to which this sheds light on the current situation in developing countries. Among his African publications are the recent monographs *The State of African Demography* (1988) and *Mortality and Society in Sub-Saharan Africa* (1992), both published also in French, and numerous papers on mortality, fertility decline, marriage, demography, and AIDS in sub-Saharan Africa generally and several focused on Burkina Faso and Mali, based partly on fieldwork that he and his wife, Francine, conducted there in the early 1980s.

In 1992 he brought these two strands of research interest together in his PAA presidential address, "Fertility Transition, Conscious Choice, and Numeracy" (published in *Demography*, November 1992)--an exploration of the history of the notion of ideal family size and the lack thereof in pretransition societies.

VDT [from interview introduction]: I thank you very much, Etienne, for giving me time for this interview. You, like Ron Rindfuss, who was president of PAA the year before you, 1991, missed out in getting into *Demographic Destinies* [1991] and this is to be another supplement to that collection of interviews with PAA presidents and secretary-treasurers.

You were born in Belgium [Namur]. Your name is Flemish but you say you are a French-speaking Flemish family.

VAN DE WALLE: Yes. The family is from Bruges.

VDT: The University of Louvain is French-speaking?

VAN DE WALLE: Yes. The city of Louvain is a Flemish city but it was a bilingual university when

I studied there.

VDT: Your studies were all in French?

VAN DE WALLE: All in French, yes. Not at the high school level, however. At the high school level, I had three years in Flemish, and my primary school was Flemish.

VDT: So you really are bilingual?

VAN DE WALLE: I was.

VDT: But your parents spoke to you in French?

VAN DE WALLE: Yes.

VDT: Where did Francine, your wife, get a degree?

VAN DE WALLE: She has an MA in social work, from Brussels School of Social Work.

VDT: But Ansley Coale claims her as one of his students.

VAN DE WALLE: She took Ansley's course. Princeton had this special program for foreign students.

VDT: She took that one-year course?

VAN DE WALLE: Yes.

VDT: Did you meet at university?

VAN DE WALLE: No. I was in school with Francine's brother.

VDT: Ah, very early on. And when were you married?

VAN DE WALLE: We were married in Brussels, in 1955.

VDT [after biographical introduction]: How did you become interested in demography?

VAN DE WALLE: At Louvain, I had no interest whatsoever. I took the licence--not quite an MA--in economics. I had one small course in demography at that time. It was a combination of law and economics; the combination was not unusual. I got a doctorate in law, which does not involve writing a thesis. It's a five-year degree. It's like a doctora in Italy. The title was very useful in the United States, because people typically don't ask you whether [it's the same as a Ph.D.]. I got my Ph.D. in demography in 1973. By that time, I was a full professor.

VDT: You were a full professor by the time you had your Ph.D. Interesting! But you say you took no demography at Louvain?

VAN DE WALLE: I took a small course in demography.

And then when I was graduating in economics, my economics professor asked, "Are you interested in a job?" And I said, "Yes. What job?" It was a job in Ruanda-Urundi. He had been asked by the governor of Ruanda-Urundi--the governor of Ruanda-Urundi was also the vice-governor of the Congo--to find someone to work on demography in Ruanda-Urundi. I was not interested in the job, but my professor said, "Go see him nevertheless."

I said okay, and I went to see this very important person, who hired me without asking me if I wanted the job. This was in Belgium.

So I decided, well, why not? So we went to Ruanda-Urundi.

The problem in Ruanda and Burundi was that they had been taking demographic surveys, starting in 1952. This was a system of yearly demographic surveys of the same sample; they were the same people visited every year. Mortality was going steadily downward from year to year and nobody really believed that this was true. So there was a need to go back and re-examine the whole thing and see what was happening: why weren't people dying? I didn't know anything about this kind of stuff, but I said, "It sounds like an interesting problem."

So I accepted the job and was hired by this very respectable research institute, called IRSAC, Institute for Scientific Research in Central Africa. It was a Belgian-organized institution, high standard and full of international scholars. Great people have actually worked there, and many of them are still working in the United States. For instance, I'm thinking of the director of the center at that time, Jan Vansina [now in the U.S.], who is a famous historian.

VDT: Did you do any demographic publications there? Because I see you became a member of IUSSP [International Union for the Scientific Study of Population] already in 1960.

VAN DE WALLE: I published a couple of articles on this population, mostly on unemployment in Usumbura, etc. This was basically my training.

I was involved in trying to solve this problem of why there were no deaths. I suggested we should change from this recurrent sample to a continuous, multi-round survey, but then there was this revolution in Ruanda-Urundi and most of my interviewers were either killed or dismembered; it was an awful story.

At that time, I was taking a survey in Bujumbura, the capital of what was at that time called Ruanda-Urundi, and suddenly a taxi stopped next to the house where I was working. A gentleman comes out of it and it was Frank Lorimer. Frank Lorimer at that time was traveling through Africa. His wife [Faith Williams] had just died, and the Population Council had given him some money to write on the demography of that dark frontier; nobody knew anything about the population of tropical Africa. He traveled around to various places where he was told there was a demographer working; there were various people here and there that he went to visit. Someone gave him my name, so he landed in Bujumbura and took a taxi and came to see me.

We started talking; he was a very pleasant person. And he suggested that I come to Princeton.

VDT: And that's how it got started! I hadn't known about that.

VAN DE WALLE: Yes, and so I started a friendship with Frank Lorimer.

VDT: Indeed. That also was the beginning of the project that led to *The Demography of Tropical Africa*.

So you went, then, to Princeton in the fall of 1961 as a visiting student, was it?

VAN DE WALLE: Frank Lorimer got me invited to a conference in Paris, a seminar on African demography, and there--yes--he suggested I apply for this program they had at Princeton for foreign students to spend one year. A number of people at that time came for this one-year program, which didn't give a degree but was a very popular program.

I came; that was in 1961. The Congo had become independent in 1960 and Ruanda and Burundi were becoming independent too, so it turned out that after I was six months at Princeton, taking courses and working on the African stuff, I got a letter from IRSAC saying, "Your institute does not exist anymore."

So Ansley Coale found some money to keep me there for one more year. He hired me on the African project, together with a number of people--Anatole Romaniuc, who was working on Zaire, and of course Paul Demeny was there; Don Heisel; and Bill Brass was there the first year. These were people who had been working in Africa, at least Bill Brass and Romaniuc.

And then, after that second year, when I was a researcher, not a student anymore, I had no job; I had no place to go back to. So Ansley asked me if I wanted to start working on the European Fertility Project.

VDT: Already in the early 1960s, on the European Fertility Project?

VAN DE WALLE: Yes. I was the last person on the African project, trying to put things together, doing some editing and cutting and pasting. So I took the boat back to Belgium, basically right from having been editing this manuscript. That was 1962. I went back to Belgium for two years.

VDT: Why was that?

VAN DE WALLE: Because of visa problems; I couldn't come back to the United States. But I was paid from Princeton. We had started the European project and it made sense to be in Europe, to look for the data. I was spending my time in European libraries collecting data for the European project. At that time, John Knodel was in Germany, collecting data on Germany.

VDT: His villages. That was already in 1962-64, very early on?

VAN DE WALLE: Yes. Then after these two years, I came back to Princeton as a research associate; the title was Associate Research Demographer. Which was *the* research position at the Office of Population Research.

VDT: Only one person was in it and you were it?

VAN DE WALLE: The others were faculty, and there was this one position which was a research position, which was later occupied by Jane Menken.

VDT: And did you at that time work both on Africa and Europe?

VAN DE WALLE: No, at that time the African project was finished.

VDT: Oh, you only worked on it early in the 1960s?

VAN DE WALLE: Yes.

VDT: When did you write your three chapters ["Characteristics of African Demographic Data";

"Marriage in African Censuses and Inquiries"; "Fertility in Nigeria"])?

VAN DE WALLE: At that time--in 1963.

VDT: You barely knew any demography and yet you were writing about Nigeria's population, with no data . . .

VAN DE WALLE: I had done my learning of demography in the field. I knew a lot from having tried to find dead people in Rwanda and Burundi. I didn't know anything about the theory, but I knew quite a bit about the practice.

VDT: Also you knew about working with missing data.

VAN DE WALLE: Yes. Actually, I think some of it was missing, but also mortality had been declining in a fantastic fashion at that time. They had a DDT program and every house of Rwanda and Burundi was sprayed twice a year and that had really decreased mortality quite substantially. I think that was the explanation. Mortality went back up when they stopped the DDT program. For some time before independence, in the 1950s, things were really happening.

VDT: So from the time you came back to OPR, you were totally on the European project?

VAN DE WALLE: At that time I moved completely to the European Fertility Project. Like Ansley, I had been moving from this African project to the European. It's less different than it seems, because after all we were working with the same kind of census data--age distributions and trying to make sense out of incomplete information--and at the same time we were working with high-fertility situations. A number of people have been working both in historical demography and in African demography.

VDT: People there, at OPR?

VAN DE WALLE: No, in general. John Blacker is another example, who wrote his dissertation on French fertility and eventually went to Africa and worked on African demography. So it's not uncommon. Demographers are people who start with data and wherever the data are; whether it's in the 17th century or 20th-century Rwanda, I think it doesn't really make much difference. You try to understand . . .

VDT: Or lack of data.

VAN DE WALLE: Or lack of data. John Knodel became a specialist of Thailand.

VDT: Then you were involved at OPR not only with the people working on Africa but also those in the fertility project.

What do you recall of OPR in the time from when you came back in 1965 until you left in 1972? You were not working then with Bill Brass; he had left.

VAN DE WALLE: Bill Brass had left; he only stayed one year. I was working with Ansley; very closely with John Knodel.

VDT: Of course. You and John came up with a number of articles together.

VAN DE WALLE: Yes. In those days at the Office there were, of course, the American fertility people, like Westoff, and Ryder eventually came, and Michael Teitelbaum was assistant professor. Paul Demeny was working there too. So there were people with whom I have remained friends. But most of them were also teaching.

VDT: And you too?

VAN DE WALLE: I was teaching the undergraduate course at Princeton, but that wasn't like . . .

VDT: Even without your Ph.D.?

VAN DE WALLE: Even without my Ph.D.

VDT: Was your French female population book [*The Female Population of France in the Nineteenth Century*, 1974] your dissertation?

VAN DE WALLE: That's right.

VDT: How did that come about? You took a while to get into your doctorate. Did Ansley say, "Look, you're writing this book; that should be your dissertation." Or what?

VAN DE WALLE: In between, while I was still at Princeton working on the European project, I was working on an ambitious manuscript on the connection between economic development and population in Africa. That left also some tracks in the literature. I had a couple of articles published, but mostly in conferences in Africa. There was a conference in Ibadan where I gave a paper, which was published by Caldwell in the conference proceedings ["The Relationship Between Population Change and Economic Development in Tropical Africa," in John C. Caldwell and C. Okonjo, eds., *The Population of Tropical Africa*, 1968]. There are a couple of papers that I wrote in those days where I was interested in the relationship between population and economic development.

VDT: That was not quite so popular then; you were a bit early on.

VAN DE WALLE: No, that was the time, for instance, when Ester Boserup was writing, and there were a number of very interesting anthropologists and agricultural scientists working--mostly English. William Allan, for instance, wrote *The African Husbandman* [1965], which is a very basic text on the relationship between density and technology.

Then about my dissertation. At that time I had this manuscript and like every graduate student who's working on a dissertation and doing something else, I was making little progress. So I had this French volume that was taking shape and I asked my professor in Louvain, with whom I'd worked, whether I could use that. He said yes, and that's how it happened.

VDT: They didn't have residence requirements at Louvain?

VAN DE WALLE: I had completed my residence requirements during the two years I went back. I was commuting to Louvain for one seminar a week.

You know, at that time Louvain was starting its demography program; Louvain has a Ph.D. in demography. I was one of their first students. That was part of my reason for getting my Ph.D. in

demography at Louvain. I was their first Ph.D. in demography.

VDT: So that worked out. What happened to your manuscript on population and economic development, other than the few papers? You abandoned it?

VAN DE WALLE: I gave it up, yes. I maintain an interest in the general topic. Actually, that kept my interest in Africa alive, and I kept reading books while I was working on Europe. I was not writing very much on Africa.

Actually, topics tend to follow you. Once you have been declared as a specialist in Africa, people are going to ask you to write papers. So I kept writing a paper once in a while, and continued to read. Meantime, I was moving into European demography, and by the time I had decided I wanted to go back to African demography, people were asking me to write papers on Europe. [Laughter]

VDT: I was going to get into that later--interesting how they dovetail.

What took you to Penn? Well, first, you say you were asked to go to Berkeley.

VAN DE WALLE: I was asked to go to Berkeley. I had a sort of charmed life in Princeton, where I was not exposed to teaching. I did not have any faculty meetings. So life was nice and I could spend full time on research. And one day I received a phone call from Judith Blake, asking me if I wanted to spend a year in Berkeley, and I said, "Yes, wonderful!"

VDT: That was out of the blue?

VAN DE WALLE: Yes. They were trying to beef up their faculty for the demography department.

VDT: Right. They had seven slots, but they never had more than three full-time people.

VAN DE WALLE: That's right. Obviously, I must have been known as being on the market, because an invitation also came, somewhat later, from the University of Pennsylvania.

VDT: You were in California when they asked you to come to Penn?

VAN DE WALLE: Yes. I guess you identify people who are likely to move from where they are, at one stage of their career. So I must have been on the list of people who might be moving, so Judith asked me to come to Berkeley.

VDT: Why do you think that; they thought you might have done all you wanted to do at OPR?

VAN DE WALLE: Probably. It's not very respectable to be a full-time researcher in academics. I thought also that perhaps at that point I should fly on my own, get out of the nest. Also, I had the suspicion that perhaps life was more than just doing research. There were students; you would get involved in academic stuff.

When I reflect on it, there was absolutely no concern in my mind about tenure. I didn't know tenure was a problem--in those days. So I went to Berkeley, and came into this very painful atmosphere of almost open warfare. Sam Preston, of course, was someone I knew, and I got to know Nathan Keyfitz. And there were other people there--Eduardo Arriaga.

VDT: The open warfare between the students and faculty, or . . .

VAN DE WALLE: The students and there were two sides of the faculty. On the one hand there was Judith Blake and . . .

VDT: Kingsley Davis?

VAN DE WALLE: Kingsley was not seen very much; he had his own institute [International Population and Urban Research].

And on the other hand there was Nathan Keyfitz. I really didn't know what the background was, but there was a funny atmosphere. Students would start talking to you about the situation and you almost had to take sides. I didn't understand what was going on, except that obviously the students were alienated from the administration, which was Judith. Students were coming to me and asking me to be on this committee and I discovered that most of the students were not asking Kingsley and Judith. They were asking me and, of course, Nathan had a lot of students, and Sam was on every committee. So it was a strange atmosphere.

I think it went back way before I came, and all I could see was a situation where students were very unhappy. And eventually I was taking sides myself without really knowing what I was doing.

The break between me and Judith probably came when I signed a letter which I should not have signed, that had been written by Nathan to whatever administrative authority was responsible. I even forget the content of the letter, but I know I signed it. I shouldn't have, because I was after all a visiting faculty member. I was simply a visitor for one year.

VDT: Was there an intention that you should stay on after that year, perhaps?

VAN DE WALLE: Eventually, yes.

VDT: You came as a visiting what--professor or teacher?

VAN DE WALLE: I was a visiting associate professor. And at some point, Judith asked me if I wanted to stay as a permanent member of the faculty.

Now, Sam was very eager for me to stay. Sam Preston will confirm that he told me, "If you stay, I'll stay." Eventually, I thought the situation was very unhappy, and basically I never intended for it to be more than one year from Princeton. I thought I would go back to Princeton.

One of the reasons why we thought we didn't really want to go bury ourselves in California was that we keep going regularly to Europe and the West coast was too far from Europe.

VDT: By this time you had children, I guess.

VAN DE WALLE: Yes, I had four teenage children, who were smoking pot [as a result of] California. [Laughter] Two children were born in Africa and two of them before I left for Africa, so we had our four children in Africa.

VDT: Oh, so you had them very early on in your marriage.

VAN DE WALLE: Yes, very closely spaced.

VDT: Were any of them students at Berkeley?

VAN DE WALLE: No, they were in high school.

Then I got an offer from the University of Pennsylvania, which was in a way much closer to Princeton and much less difficult for us to move. They offered me the position of graduate chair, to succeed John Durand.

VDT: Was he at that time chair of the Graduate Group in Demography?

VAN DE WALLE: Yes, he had been chair for five years and he wanted someone else to take over for him.

VDT: That's in charge of the administration.

VAN DE WALLE: Yes. And in my total lack of knowledge about administration, I thought this sounded like power and I could shape the place. The place at that time was aged and somewhat at risk of disappearing.

VDT: You mean the Graduate Group?

VAN DE WALLE: The Graduate Group in Demography was going through some kind of crisis. The average age of the Graduate Group was quite old. They had not succeeded in replacing themselves by hiring junior assistant professors. The only assistant professor they had was eventually denied tenure. And there was some unpleasant relationship with the sociology department. So it was touch and go whether the Graduate Group would make it.

And most of the students they were training by that time were foreign students. So if you play this game of training foreign students rather than American students for American academic institutions, you're really not setting yourself as a high-priority organization in an American university.

VDT: The Graduate Group is the administration, looking after the training of the students, right? And the Population Studies Center does the research.

VAN DE WALLE: Yes.

VDT: Why was it important that this Graduate Group should survive?

VAN DE WALLE: It probably was not that important in those days. It was dependent on the ability of the Graduate Group to attract major scholars.

VDT: And money to train those students?

VAN DE WALLE: Money was available. There was plenty of money from outside sources, like the Ford Foundation, the Rockefeller Foundation, NIH . . .

VDT: For training of students?

VAN DE WALLE: For training of international students, basically.

VDT: But Penn's sociology department did not really like it that you were concentrating on the foreigners?

VAN DE WALLE: Well, Penn's sociology department has somewhat mixed feelings about its demography component. We tended to be more wealthy and we had captured the library and so on. We were well funded. We had students who tended to keep by themselves. Dorothy Thomas was a very powerful person who kept a very tightly knit group of students, many of them foreign, around herself. The coffee-time was sacred time in the Graduate Group in Demography. So the Graduate Group in Demography was a little bit of a foreign body in the department of sociology.

Also it had fairly close connections with the economics department, through Richard Easterlin. So although Vincent Whitney had been the chair of the sociology department, this was felt to be some kind of domination of sociology by demography.

So there were a lot of bad feelings about the connection between demography and sociology. Which have persisted to this day, although they are more hidden.

VDT: Maybe that's why they don't have the Population Studies Center listed on the plaque outside the building--only the sociology department.

VAN DE WALLE: And Sam has been the chair of the sociology department for a number of years.

VDT: Since 1989 when Jane Menken took over the Population Studies Center?

VAN DE WALLE: Since 1989, but he had been sociology chair before. Sam has been uncontested as an excellent chair of the sociology department, and he is going to be replaced by Phil Morgan. And everybody in sociology believes they are absolutely wonderful. But there's also the feeling that the qualitative sociologists are dominated by the quantitative sociologists, who got all the power and all the money. So it is somewhat unpleasant--at times, let's say. This is a happy department today, but when I came it was not.

VDT: In 1973. Do you think they reached out for you because you were foreign, European?

VAN DE WALLE: Well, I'm not sure why they reached out for me. I think in those days there were very few demographers in my cohort, so a lot of people like myself were being courted. Actually, I was hired as full professor, and I never suspected that this was a great favor--not having my Ph.D. and so on. Which never was an issue. I got an offer and came, without knowing all the underground discussion that had been going on before.

Then I was chair of this Graduate Group in Demography and we had students, some American students. But one of the characteristics of the Graduate Group in Demography was that the American students they were training had a degree in demography--the only Ph.D. in demography at that time--and it was difficult for them to find jobs in academia. Most of them were not trying. Foreign students were finding good jobs in academic programs.

VDT: In their countries?

VAN DE WALLE: In their countries. For Americans, the notion was that you would get a Ph.D. in demography and then you would go to the Census Bureau or elsewhere in Washington or to a foundation. I'm thinking of Martin O'Connell, who has had a very nice career in Washington, who was one of the students when I was coming in. My first Ph.D. student was Louise Kantrow. She wrote her dissertation on historical demography, so I was the natural person to be on her committee. She is in the United Nations.

The idea was that a Penn degree was not a degree that would lead you to become a professor in

a sociology department.

VDT: Was that because it was not a degree in sociology or economics?

VAN DE WALLE: That's right. Of course, there was a market and the occasional person would make it into an academic program, but it wasn't per se.

Then the next stage was how do we replace people who are going to retire. That included John Durand, Vince Whitney, Ed Hutchinson; they were the three full professors.

VDT: And Dorothy Thomas?

VAN DE WALLE: Dorothy Thomas was gone by that time.

Then, of course, there was Dick Easterlin, who was in economics. He was at that time the chair of the economic history program, which also was actually training a lot of demographers. It also had no academic future and was eventually disbanded. People like Michael Haines, for example, were graduates of that economic history program. He is a demographic historian. He wrote a book with Sam Preston, *Fatal Years: Child Mortality in Late Nineteenth-Century America* [1991].

I took over from Vince Whitney as director of the Population Studies Center.

VDT: That was not when you first came back.

VAN DE WALLE: No, after some time as graduate chair. I was combining both positions then [Chairman, Graduate Group in Demography, 1973-78; Director, Population Studies Center, 1976-82]. For two years, I was both.

One of the functions of the chair of the Graduate Group in Demography is to find his own successor. John Durand found me and I found Sam.

VDT: Sam came in 1979, I know.

VAN DE WALLE: Sam had left Berkeley and went to Washington. We still had this kind of mystical bond. Sam thought he would stay in Washington if I came to Washington.

VDT: Why did you have that mystical bond--because of the time at Berkeley?

VAN DE WALLE: Probably because of the time at Berkeley. He thought we would be a good team. So eventually he asked me if I wanted to come to Washington, and I was invited for a visit to Washington. That led to nowhere; it was also on the West coast.

VDT: And wet.

VAN DE WALLE: But it's a nice place.

Then Sam moved to the United Nations, and he asked me if I wanted to go to the United Nations. And then I attracted him here, eventually.

VDT: It's a very incestuous field, very close-knit.

VAN DE WALLE: Yes, that's right.

Sam at that time was being courted by Princeton and there was a long discussion, a lot of

debate in his mind about whether he wanted to go back to Princeton, and he eventually decided to come to Penn. Which saved the Penn program. Suddenly the Penn program had the most desirable, the greatest scholar.

VDT: You think Sam was considered that already then, in 1978?

VAN DE WALLE: Sam for everybody, I think, was the coming star, the moral equivalent of Ansley Coale or something like that. Sam basically was the most attractive person to get to any place.

So there was no problem getting him at Penn. I think if I had aimed at a somewhat less spectacular replacement, the program might not have survived. But Sam obviously was it. He came and became graduate chairman.

Everything was suddenly rosy and he was able to attract very good collaborators, like Jane Menken.

VDT: Of course, Jane didn't come here until the late 1980s.

VAN DE WALLE: Yes, but I think that Sam being here meant that suddenly the place looked like being one of the most promising centers, that the university had decided that they wanted to expand.

VDT: The Caldwells in their book on *Limiting Population Growth and the Ford Foundation Contribution* [1986]--they're talking about Ford-funded centers, only centers that have some Third World interest--they said that Vince Whitney applied for a grant of \$1.2 million in the early 1960s. He only got \$200,000 over five years, but he did get the money, along with Michigan which was funded at the same time. At that time, Penn was not about to shift from their U.S. focus--urbanization, labor force, and migration--and concentrating on formal demography--very little on the Third World--and develop an area specialty.

Well, by the mid-1970s, then *you* started to shift the focus toward an area specialty, Africa. How did that come about?

They also say that by the 1980s, you had along with Brown and Michigan the most stable, permanent center, and you also had trained most Third World students--more than any other center.

VAN DE WALLE: Well, with Dorothy Thomas, of course, we had been training a lot of Asians, and when I came here, there were several Africans. They were on their way to the Ph.D. and so I became involved.

With my interest in historical demography, I felt a little bit frivolous. I thought I should really become more serious; I should go back to African demography.

VDT: Did you feel that everything had been done in historical demography; was that why you switched back to African demography? I had that question here, but you're telling me you thought that people might think it was frivolous. Good point.

VAN DE WALLE: I thought I could continue to do both and that historical demography would be my hobby, but that I would busily try to train people in African demography.

I thought, well, the first thing to do is to try to replace this funding that is running out.

VDT: The Ford funding?

VAN DE WALLE: The Ford funding, but the Rockefeller funding was also.

VDT: Rockefeller you had had for what?

VAN DE WALLE: We had had a small Rockefeller grant--I forget how much it was--and we were told by Rockefeller that they were getting out of this center granting.

VDT: In the 1970s, you were told by both Ford and Rockefeller they were getting out?

VAN DE WALLE: Yes. So I thought, let me rephrase the Rockefeller grant in terms of African demography and maybe that would be of interest to them. I had some preliminary discussions and then I wrote a proposal and we got site visitors and people laughed at us, "Why Philadelphia?" I remember the comment by one of the reviewers, "Why Philadelphia?"

VDT: Good point!

VAN DE WALLE: Good point. And basically my answer was, "Why not?" It seemed attractive to Rockefeller, who thought they had to do something for African demography, and here I was with some credentials in African demography.

VDT: Somewhat back, ten years earlier.

VAN DE WALLE: Those things follow you through life. Of course, I had been in Africa; I had spent four years in Africa. I had been working in Africa. The African project book was published with some delay [1968] and it was a while before it got to be known. But it's still a book that sells regularly; it's one of the bestsellers in the history of Princeton University Press.

VDT: Oh, great!

VAN DE WALLE: It was eventually re-edited and so on.

VDT: It came out in more than one edition?

VAN DE WALLE: Yes, the second edition--I was checking the library edition here--they corrected the title. On the top of the page there was a title and for a whole chapter it said, "Estimate of Morality."

VDT: So that got corrected in the second edition.

VAN DE WALLE: Yes.

VDT: So Rockefeller thought, after all, "Why not?"

VAN DE WALLE: They went ahead and supported us. Actually, there were a number of good African students. It was not difficult to find excellent students. Somehow we got the reputation as the center that was interested in Africa. And I kept occasionally going to Africa and being involved in various African activities.

VDT: Are we still talking about the 1970s?

VAN DE WALLE: Talking about the 1970s.

VDT: Okay. You hadn't yet gone to do those surveys you did in the early 1980s.

VAN DE WALLE: No. By 1982 I was ready to have sabbatical leave and I went to Mali for one year--to refresh myself in African demography. I felt, "My knowledge goes back to the 1960s." So I decided I would go back and spend one year as the Population Council . . . What was my title? Senior researcher or scholar [Senior Associate].

VDT: You went to Bamako.

VAN DE WALLE: Yes.

VDT: And that was the time that Francine also did her surveys?

VAN DE WALLE: Yes, Francine was also involved in research there.
At that time also, Sam took over the Population Studies Center.

VDT: The Caldwells said in their book that Penn developed a special interest in Africa, but no one particular country. For instance, do you have students working on data that are collected in Africa? You have African students who come to Penn, and do they take formal demography? What about, generally, students being involved in a project or country data, such as happens at Michigan being involved in the Taiwan project?

VAN DE WALLE: There was always the problem of data from the start. We were always trying to find some African data sets from surveys. Especially, we have constituted a data bank of African surveys and censuses. And then, of course, the problem was solved by the World Fertility Survey and the DHS [Demographic and Health Surveys]. So today there's no problem to have data.

But in the beginning it was difficult for these African students to have something to work on. And there was the occasional African Ph.D. who got his Ph.D. working on an American or even an historical demography data set.

VDT: You had Marvellous Mhloyi. I met her at the University of Zimbabwe in Harare in 1986. I told this story to Jane Menken and Sam [in their interviews].

VAN DE WALLE: Yes.

VDT: I was with my husband on a World Bank mission. I know she's much in demand internationally. At that time, she had collected some data from two small surveys of 120 couples in a rural area which showed that probably the contraceptive prevalence was not nearly so high as the Contraceptive Prevalence Survey showed [for 1984]. I had some correspondence with her and Ethel Churchill, who's in charge of *Studies in Family Planning*, about getting that analyzed and written up. No time; she never did it. And that seems to be the problem that Jane Menken has been going to Bangladesh to help solve. That there is somebody on site in those countries to work with them on data so they get that kind of thing done. Have you done that with Africans?

VAN DE WALLE: Well, we occasionally have. But the problem is that you get students who come here--somehow we get some way of supporting their studies--and then by the time . . . Occasionally

they bring their data with them. A couple of times they have been going back the second year or third year to collect data.

In most cases, the student is someone you want to get out, because it's very expensive to fund students for a long period of time.

VDT: You want to get him out of his country?

VAN DE WALLE: You want to get him out of the program. We are funding these students from day one, with Rockefeller money--which, by the way, we don't have anymore. So now it's becoming an even worse problem. Our Rockefeller grant has expired.

VDT: How long ago?

VAN DE WALLE: This year.

VDT: And at the moment there's no money for students?

VAN DE WALLE: That's right. I understand the foundation; they don't want to be forever saddled with a very expensive program.

But at the same time, it's very difficult to get students out of Africa to start their American studies. We know that there are good students. For instance, I'm the external examiner in Uganda [University of Makerere] today and there are people who seem to be excellent.

The last student funded by Rockefeller is a woman whom actually I was introduced to because I was her external examiner in Uganda. She's here and she still has two years of studies. We have to find the money and it's a problem.

VDT: What did Marvellous Mhyloyi write her thesis on; do you remember?

VAN DE WALLE: Marvellous Mhyloyi wrote her thesis on the Lesotho World Fertility Survey.

VDT: That was straightforward.

VAN DE WALLE: The data from Africa were coming out by that time.

Marvellous was an extraordinarily hardworking person. When she came here, we found her in a community college in West Chester [near Philadelphia]. One of our American Ph.D. students was teaching in West Chester and told us, "You know, there's an absolutely fantastic Zimbabwe woman who is lost in West Chester and could you get her into the demography program?" So we were able to fund her with our Rockefeller money. And she was a *great* worker--outstanding.

VDT: Now she feels so isolated. Well, this was five years ago [nearly seven].

VAN DE WALLE: She's not isolated. She has good resources. I think the problem of Marvellous is that she got funded by virtually everyone and that there are too many demands on her time.

VDT: Yes, you're right.

VAN DE WALLE: She's not isolated. She has, for instance, Rushdi Henin, who is a very experienced African demographer, there assisting her.

VDT: He's in Harare?

VAN DE WALLE: Yes, having spent most of his career in East Africa.

VDT: Now you've got more data to work with in Africa, both you and your students. Are you still pessimistic about it? Of course, I've read your recent article on AIDS ["The Social Implications of AIDS in Sub-Saharan Africa," *The Milbank Quarterly*, 1990], so one can be pessimistic.

And the fertility decline. I read your 1990 World Bank publication, with Andrew Foster ["Fertility Decline in Africa: Assessment and Prospects," World Bank Technical Paper No. 125], and in that you said that although there are signs of fertility decline--thanks to the DHS we see that in Botswana, Kenya and Zimbabwe. However, there's not stopping behavior. And you and the Caldwells have been having some discussion back and forth [e.g., John C. Caldwell, I.O. Orubuloye, and Pat Caldwell, "Fertility Decline in Africa: A New Type of Transition?" *Population and Development Review*, June 1992] on whether you have to have stopping behavior among older women in order to be certain that a real fertility decline has set in, or if fertility decline will come out of the attempt by young women to avoid premarital pregnancy, because it will stop their education, and contraception by slightly older [married] women for spacing and by the oldest women so they can quit having kids when they're grandmothers.

I guess my question is . . . Well, I had two questions in there. One was, are you pessimistic about Africa?

VAN DE WALLE: First, to continue on the earlier topic, I'm pessimistic about African graduate students and about the training of Africans, because the universities in Africa are a complete mess, and the people we are training increasingly are not going back, which was a phenomenon that was limited to some countries until recently. Now today it's very clear that this is happening. And I understand why--to live on a full professor's salary of \$100 per month is difficult.

This other question, am I pessimistic in general about the development of demography in Africa--mortality, AIDS, and decline of fertility? I expect fertility to decline eventually. I think the demographic transition has started. It has started a little bit after it started in the Asian countries, where we were telling you 20 years ago it was never going to happen. But it's probably going to happen on schedule at some point between now and the end of the century. You're going to have an increasing number of countries. Kenya has started; Zimbabwe has started; maybe Botswana; and there may be other places--South Africa, of course.

If historical experience is valid, a diffusion effect is going to impose the same kind of limiting behavior in neighboring countries. It's first starting among certain social classes that are poorly represented in surveys.

The Caldwells said that we're going to have a special transition in Africa that's going to be focused on young women and that spacing is going to be increasingly practiced. And I don't see what the connection is between the spacing behavior, which is very old behavior in Africa, and the transition of fertility.

VDT: In other words, you don't think spacing can be called stopping fertility?

VAN DE WALLE: What is the connection between a desired spaced birth--for example, you can postpone the first birth, the premarital birth--and this behavior which is bringing the family size to two children?

I asked the question of Jack Caldwell and he essentially said there's going to be apprenticeship of contraception outside of marriage and then people are going to know that contraception is an option.

VDT: You're very skeptical of that, obviously.

VAN DE WALLE: No, I am not. It has happened already. It's not an unnatural type of mechanism for contraceptive behavior to get in.

Now, the other question is that of a crisis-induced fertility transition--whether economic difficulties are compelling people to behave in this way--and in the historical record it's not. If poverty was a factor in fertility decline, India would be one of the low-fertility countries of the world.

I think in general the status of women is significantly affected by poverty. Even if women survive the economic disaster more easily than men because women have traditional jobs, that doesn't improve their status.

VDT: And you have said in your World Bank paper that women's status must be improved to bring about the fertility transition.

Let's get back to some of the leading questions that I had for you. Who have been leading influences on your career?

VAN DE WALLE: Ansley Coale.

VDT: He considers you and Francine his students. He told me this story in his 1988 interview. He had been down to give a lecture at Penn and 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"--he included Francine there--"and others and one of the students came up to one of these people and said, 'Does that make him our grand-mentor?'" I loved that!

Well, he sort of snapped you up--having been delivered to him by Frank Lorimer.

VAN DE WALLE: He clearly is a great intellectual influence. It's not the only one, but it's my most important demographic influence.

VDT: Ansley Coale?

VAN DE WALLE: Yes. Because when I came to the Office of Population Research, I knew very little about formal demography. I knew a lot of things that I had learned in the field, from trying to re-invent the wheel. But clearly I got from Ansley this fascination for empirical constancy of phenomena.

VDT: Empirical constancy?

VAN DE WALLE: Yes. You know, he's not a theoretician. He's an empirical wizard. He looks at the shape of a curve and says, "Well, this is the same curve [if you look at it] the same way as this other curve."

VDT: I like that--an "empirical wizard." Very good.

VXAN DE WALLE: Yes. He's a highly technical type of person, but he makes sense out of things. Also, his range of interest is very large--as an economist and as a sociologist. He has the range of interest of human phenomena. Which you find in Sam Preston as well, but which is not characteristic of all demographers.

VDT: No. Let me quote a piece where this came up in a sense, and in your historical demography. Have you seen Geoffrey McNicoll's article in the just-out [September 1992] *Population and Development Review*?

VAN DE WALLE: No.

VDT: Very interesting; it's one of the typical things that Paul Demeny will put in PDR. It's called, "The Agenda of Population Studies: A Commentary and Complaint." He is saying that demographers are becoming too technical and too narrow. Formerly they were broad-based--like Ansley Coale; like Kingsley Davis. But he makes an exception for historical demography, which he says can be very broad and will take in economic history--certainly you have taken in literature--and you have used the social field far more than other narrow, technical demographers. ["The case of historical demography, at its best intimately allied with social, economic, even environmental history, suggests that considerable success is possible"--in branching out into neighboring disciplines as demography formerly did. McNicoll, page 414.]

You're not terribly technical.

VAN DE WALLE: I'm not technical. I've made my mild technical contribution.
You know, I'm a Lexis diagram demographer.

VDT: Tell me what you mean by that!

VAN DE WALLE: The Lexis diagram is the relationship between age and time and cohort, so it's the basic representation of demographic reality, on a simple diagram. And I think that the old generation of demographers were trained basically to make sense out of the combination of these variables--cohort, time, and age. I think that's what we were dealing with. That was the contribution of demography. Right from Halley, inventors of demography were playing with these variables and then using them to make sense out of human reality. You know, you would look at a life table and say, "This tells me that there are 9000 young males able to bear arms in Augsburg"--which was one of the conclusions of Edmund Halley, in his 1693 *Degrees of Mortality of Mankind*.

VDT: Never knew that.

VAN DE WALLE: On the basis of the five-year life table. This is modeling on the basis of these very simple parameters. Ansley was basically interested in this kind of relationship.

But then the new school of demography is grinding everything through this very powerful econometric model or multivariate model, where age is basically one of the parameters or variables, and if you're a little bit clever you have age squared too. But you've lost what was demography's contribution.

VDT: What do you mean by that--demography's contribution?

VAN DE WALLE: Demography's contribution was to concentrate on this age-time-cohort relationship in an intelligible fashion. You could picture it in your mind, and you have entire textbooks of demography which are all based on the Lexis diagram. I'm thinking of the French school which does that. Pressat, for instance; there's a Lexis diagram on every page of Pressat's *Measures of Demography*.

We've lost this kind of comprehensibility of phenomena--simple, two-dimensional model. You

can't see it nicely with the multivariate tables. You've lost the intrinsic power of demography, with the strength of these black boxes through which we run our models.

VDT: Who else has influenced you? Ansley in particular, you say, was the leading demographic influence.

VAN DE WALLE: I admire Jack Caldwell very much. And I think I probably got influenced by some of the French school, like Sauvy, of course.

VDT: Did you work with him?

VAN DE WALLE: No, I never worked with him. He was one of the demographers that I went to listen to when I was a student in Louvain.

Louis Henry was someone I knew. I went to talk to him when I started the French project. I told him, "Here I am. Is there still something to say about French demography?"

VDT: You mean the French female population?

VAN DE WALLE: The French female population in the 19th century.

VDT: By the way, what happened to the second book you were supposed to be writing at that time [a larger study of French fertility decline]?

VAN DE WALLE: It's somewhat [delayed], but it's still . . .

VDT: It's still alive?!

VAN DE WALLE: It's still alive; it's not dead. Do you still want me to talk about my future plans?

VDT: Yes, but I'm afraid I'll miss that point. What happened? What are you going to do with that?

VAN DE WALLE: Well, I think I'm moving increasingly in the direction of my presidential address ["Fertility Transition, Conscious Choice, and Numeracy"] and try to write a history of family limitation. Not a history of contraception, which seems to be what people have been writing, starting with Norman Himes's *Medical History of Contraception*. There are a number of histories of contraception that have been written. The last one is Angus McLaren's, *A History of Contraception* [1990].

VDT: You mean when people stopped having more than, say, three/four children?

VAN DE WALLE: That's right. First of all, family limitation deals with families. It is family behavior--behavior of married couples. It's not the use of contraception outside of marriage, which was one of the main uses in the past.

And secondly, it's about limiting. It's not about controlling, and I think that Angus McLaren has written a history of fertility control. For instance, breastfeeding is a form of fertility control; late marriage is too. But I'm interested in family limitation; once you marry, how do you decide on the size of your family? Then you can go back and use a number of different sources, including the historical demography sources.

VDT: That was a fascinating speech. I loved your showing the portraits of noble families with two or three children. We had never had a presidential address like that before. And there were the literary sources and then, of course, the quotes from Francine's survey [of women in Bamako, Mali, 1983].

VAN DE WALLE: That's right. And I think if you look at family limitation as a type of new behavior, which did not exist in most societies of the world, you can really find some commonalities between African peasants and urbanites and pre-modern Europeans.

VDT: Some PAA presidential addresses have been on topical topics. You know the famous one of Ansley Coale which was on "Should the U.S. Start a Campaign for Birth Control [Fewer Births]?" [Boston, 1968]. And some have very obviously have been syntheses of one's research, as Ron Rindfuss's was last year, looking at young cohorts ["The Young Adult Years: Diversity, Structural Change, and Fertility"].

We all wondered what yours would be. Was it going to be Africa or was it going to be historical demography? You put them together. It was marvelous. [Laughter] And it was a very intriguing topic.

Had you been thinking about that before? How did you choose your presidential address topic?

VAN DE WALLE: Well, a bit because of this increasing fascination with this problem of this break in human behavior, which is known as family limitation. And I thought it was a way to bring back my African and historical interests.

VDT: So you really deliberately did that?

VAN DE WALLE: Yes, I wanted to put them together. And I also wanted to deal with this issue of family limitation, as the fertility decline condition.

VDT: Family limitation has to be within the calculus of conscious choice?

VAN DE WALLE: Yes. And I figured it was also a way of paying some tribute to Ansley Coale's influence on my thinking. [Coale used the term "calculus of conscious choice" in a classic 1973 paper enumerating the three necessary preconditions for fertility decline.]

But I'd also been working increasingly on this volume that I want to write on the history of family limitation, and when I was asked to become president, I immediately started thinking what I was going to talk about. At that time I was writing a chapter for a French interpretative book on the history of European population. They asked me to write about the history of the fertility decline in Europe. So it came together.

VDT: You'll never not be known as a historical demographer. Which do you prefer--the African demography or the historical demography?

VAN DE WALLE: When I retire, I'll retire in a library full of old books.

VDT: You won't retire in Africa. [Laughter] Out there in the field with a survey.

VAN DE WALLE: Probably not, although that's another attractive possibility.

But I think that's something you can do when you're increasingly crumbling, is to read on and discover voices of the past. It's so clear to us now that limiting one's family size is the normal type of behavior, but it was not to people like Montaigne, who was one of the great geniuses of common

sense. These were not stupid people. They were extraordinarily well informed and asking interesting questions. They were not asking questions about family size. So I think it's an intriguing, central concept that I was thinking about at that time, and I thought it was a good topic for a presidential address.

VDT: Great.

I was going to ask you which are your favorite publications, but maybe your favorite is still to come. But which do you think are your most influential publications?

VAN DE WALLE: In African demography, probably my chapter in *The Demography of Tropical Africa* on marriage ["Marriage in African Censuses and Inquiries"] is the one that I'm most cited on and pleased to have worked on. It stated some issues in social demography which are more central than the narrow focus on, let's say, African fertility--from the process which is central in constituting a building block of African society. You get to issues like premarital fertility, status of women, AIDS--you can go on citing these peculiar customs, like polygamy, female circumcision, whatever. You have a series of interconnected topics which can be centered on women marrying. And it has been traditionally the favorite topic of anthropologists and so it connects sociological interests, anthropological interests, demographic interests.

VDT: So that was your favorite in the African area?

VAN DE WALLE: I think that's probably the one that, looking back, I'm most likely to continue to work on.

VDT: What about on the historical side?

VAN DE WALLE: In historical demography, I think my first book [*The Female Population of France in the Nineteenth Century*] has an afterlife--in France. The French have discovered the book, somewhat late, but I keep being quoted in *Population*.

I think in a way my favorite piece is more in the cultural history of contraception. I wrote a piece called "Means and Ends in French Fertility"--or whatever the exact title is ["Motivations and Technology in the French Fertility Decline"].

VDT: Where was it published?

VAN DE WALLE: It was published in a collection edited by Tamara Hareven and Bill Wheaton.

VDT: Right, I know of it [*Family and Sexuality in French History*, 1980]. That's a favorite of yours, or do you feel it had some impact?

VAN DE WALLE: I think it's probably a favorite of mine in terms of asking questions about fertility decline which don't have a numerical answer but have to be answered in cultural terms. This was perhaps the emphasis of my presidential address.

VDT: Right.

I missed the question on some of your outstanding students--students that you're proudest of.

VAN DE WALLE: I think I would have to categorize them into American students, European

students, and African students.

Probably the students that I'm most in contact with today--I keep corresponding with and seeing--are the African students. Among the favorites there is Cheikh Mbacke.

VDT: What country?

VAN DE WALLE: He is a Senegalese. He worked with me on the data I collected in Africa. You were asking about surveys, how do your students actually get survey data? When I went to Bamako in 1982, I worked on an infant mortality survey.

VDT: The multi-round survey?

VAN DE WALLE: Multi-round survey, the IFORD survey. At that time he was a student at Penn and he came to work on the project and wrote his dissertation using some of these data. He's now the representative for the Rockefeller Foundation in Nairobi, in Kenya. So he's one.

Another one is Uche Isiugo-Abanihe.

VDT: Is this someone you've published with?

VAN DE WALLE: No, I didn't publish with him. He wrote a piece--which was part of his dissertation--on fosterage in *Population and Development Review*.

VDT: Fosterage.

VAN DE WALLE: Which is one of the strange and interesting African customs.

VDT: Where does he come from?

VAN DE WALLE: He's at the University of Ibadan in Nigeria and he was a visiting scholar here last year. He's now working on the bridewealth, which is another one of these fascinating African social customs.

VDT: Tell me, the bridewealth is when the male has to buy the bride? I get confused between the dowry and the bridewealth.

VAN DE WALLE: That's right. The husband or the male or the family of the husband giving money to the family of the woman. There's a lot of objection to the term "buying." The term "bridewealth" seems to be more politically correct than "brideprice."

VDT: Was he a student of yours here?

VAN DE WALLE: Yes, at the University of Pennsylvania.

VDT: And now among your European students.

[The tape ran out unnoticed here, but van de Walle mentioned Michel Garenne and Wolfgang Lutz in this category.]

VDT: And among the Americans, you have mentioned . . . Well, you started to say and you haven't said.

VAN DE WALLE: I'm thinking among the people whom I would legitimately claim as having made some difference. Actually, I made a list before you came [looking for list].

Among the most recent, Emily Rosenbaum, who is at Columbia, and Harold Lentzner, at CDC [Centers for Disease Control].

VDT: I had also this big question: What accomplishments in your career have given you the most satisfaction? Of course that includes many of the things we've been talking about.

VAN DE WALLE: Well, probably I can be given some credit for the Population Studies Center and the Graduate Group in Demography--to have been here at the right time and attracting Sam to settle down here.

I think also I probably made a difference in African demography--that our program has trained something like 40 PhDs from Africa.

VDT: Forty Africans.

VAN DE WALLE: Africans who have gone through here since I have come here. It's made a significant impact on whatever future African demography has.

VDT: Very good. Now I wanted to ask a question about IUSSP. Marc Lebrun, in the office in Liege [Assistant Executive Secretary], was delighted to know that I was going to interview you--Belgium's great gift to U.S. demography. Of course, you're international too.

VAN DE WALLE: You know, I'm probably the first PAA president who is not a citizen of North America.

VDT: Are you still not an U.S. citizen?

VAN DE WALLE: I'm not a U.S. citizen.

VDT: Neither am I.

VAN DE WALLE: There are Canadians.

VDT: That's right. The others would all at least have been naturalized. There was Lotka [Polish-born PAA president in 1938-38] . . .

VAN DE WALLE: Yes, and Paul Demeny [Hungarian-born PAA president in 1986. Also Louis Dublin, PAA president in 1935-36, born in Lithuania].

VDT: But they all had U.S. citizenship by the time they were president. Why did you never take U.S. citizenship?

VAN DE WALLE: I don't see the point in taking it. I'm sure that's the reason why you didn't.

VDT: Right. I like to stay Canadian, frankly. But I have a green card. Do you have one?

VAN DE WALLE: I have a green card, yes.

VDT: That's a good point. You probably are the first [foreign-born] PAA president who was not a naturalized American.

VAN DE WALLE: I always thought it would eventually exclude me, that someone would find out.

VDT: But Norman Ryder [Canadian-born PAA president in 1972-73], is he naturalized or not?

VAN DE WALLE: He's Canadian, and so was Nathan Keyfitz [president in 1970-71].

VDT: Right, but they're both naturalized now, I expect.

VAN DE WALLE: But not by the time they were president.

VDT: No? Norman Ryder had remained a Canadian?

VAN DE WALLE: He's become American since he was president.

VDT: Only in recent years! I didn't know that. And Nathan Keyfitz?

VAN DE WALLE: I believe Nathan Keyfitz is still a Canadian. So there were Canadians, but I'm the first European.

VDT: Now, about IUSSP. You've been on a number of their committees. The committee on mortality, was it?

VAN DE WALLE: Yes, I've been on mortality [Committee on Social and Biological Correlates of Mortality, 1985-89. Also IUSSP Advisory Group on Training, 1990-].

VDT: Do you think it's an important organization?

VAN DE WALLE: I think it's a very important organization. It probably has tended to have a positive impact on the field, internationally. It has given it a bit more international content than the PAA.

The PAA is making a very great effort to attract outside [foreign] members. And, of course, students become members of the PAA, but when they go back to their countries they very rarely are able to continue to pay their dues. But the IUSSP has been supporting a great many international students--bringing students, members, from various parts of the world to conferences and seminars.

They also have this intellectual focus on various topics, like mortality--bringing out books and getting them published.

VDT: There was your excellent book on *Mortalite et Societe en Afrique au Sud du Sahara* [edited by Gilles Pison, van de Walle, and Mpembele Sala-Diakanda], based on the seminar you [IUSSP] had in 1987 [in Cameroon].

VAN DE WALLE: This one was published in English [1992]. It was published both in English and French.

VDT: Well, I read the French version.

VAN DE WALLE: The French version came out earlier [1989].

VDT: There's one on the demography of Africa--*The State of African Demography* [edited by van de Walle, Patrick O. Ohadike, and Mpembele D. Sala-Diakanda, IUSSP, 1988]. I couldn't get that.

VAN DE WALLE: *The State of African Demography* was published in two languages too. If you want to have a copy, I'll get you one.

VDT: That would be very nice. The Population Reference Bureau did not have it in the library. Do you recall what committee it was that sponsored that one?

VAN DE WALLE: They wanted to have a series of white papers. They called them "livres blancs," white books, like this parliamentary tradition in England of having white books. So this would be a white book on a topic of actuality. Then apparently the idea petered out and this was the only one they produced. But it was going to be a series.

This was basically produced here.

VDT: Sam Preston tossed out the idea that you might start a journal of African demography here.

VAN DE WALLE: At one point, we had this idea that there was a need for a journal in African demography.

VDT: Sam mentioned this in his interview [June 1988]. Whatever happened to that?

VAN DE WALLE: Well, about that time the journal of the African population society came out.

VDT: I don't know about that.

VAN DE WALLE: I could have shown you a copy but it's at home.

VDT: Published out of where?

VAN DE WALLE: Out of Dakar. And the present editor is Cheikh Mbacke. It's not a very well-produced journal.

Before that they had another journal. It was called *Jimlar Mutane* and had, I think, four issues.

VDT: That was on African demography?

VAN DE WALLE: That was an African demography journal. Then by the time I wanted to produce an African journal from here--actually I wrote a number of letters and so on--this African association had become a little bit more firm and had started to get funding from outside, so they got into publishing this journal, which is coming out regularly.

VDT: Good.

Now, let's leap on to PAA. Can you remember the first meeting you attended? Here's a list of meetings. You mentioned at the beginning of your presidential address that you first heard Kingsley Davis talk about the demographic transition in one of the early meetings you attended. Now, that could have been any one.

VAN DE WALLE: Yes, and I was unable to find reference to that paper of Kingsley Davis. I'm sure I heard it.

VDT: You know, of course, he and Judith Blake were presenting their intermediate variables paper about 1964, but you probably went before that.

VAN DE WALLE: The first one I probably went to was Madison.

VDT: In 1962 [year of the meeting in Madison] you were at OPR.

VAN DE WALLE: That was the campus of the University of Wisconsin.

VDT: Norman Ryder attracted PAA there in order to publicize his new Center for Demography and Ecology.

VAN DE WALLE: I went to visit my old head of center, IRSAC, who was Jan Vansina, whom I mentioned before. I went to their house in Madison at that time, so that may be the only recollection that I have.

I have a recollection of the Philadelphia meeting the very next year [1963]. Who was the president?

VDT: The president in 1963 was Kingsley, giving his famous multiphasic response address ["The Theory of Change and Response in Modern Demographic History"].

VAN DE WALLE: I remember very distinctly Kingsley's address.

VDT: So *that's* where you heard him. Well, he wasn't talking about the demographic transition.

VAN DE WALLE: No, no. But I heard Kingsley about this idea of the paradigm of demographic transition. It was the first time I heard the word paradigm. He had picked up the idea of a paradigm from Thomas Kuhn. I'm not making it up; he gave a paper on the demographic transition as the major paradigm in demography. I was unable to identify the meeting at which I heard him.

VDT: You first heard him probably in 1963.

VAN DE WALLE: Yes.

VDT: I've just spent a day at the archives of the PAA [at Georgetown University] and I could have put my hand on the 1963 program so I could tell you if he had a paper in addition . . . Well, he wouldn't have had--in addition to his address.

What do you remember about early meetings you attended? But then you went away to Europe for two years and came back; what do you remember about the early meetings?

VAN DE WALLE: I remember this crowd of very important people, who were all obviously much more knowledgeable about anything than I was, and I was kind of scared. It was suddenly a very important occasion.

I remember having seen some people there whose articles were in the readings I was making at Princeton. I didn't really get around very much in talking to them because I was intimidated.

But at some point, I became part of the crowd and that's something that must have happened quite a bit later.

By the time of the Atlantic City meeting [1969]--that was organized by Princeton--that was probably the one where Ansley . . .

VDT: The president would have been Dudley Duncan.

VAN DE WALLE: When did Ansley give his address?

VDT: Ansley's was 1968, and that was at Boston.

VAN DE WALLE: Ansley organized the Atlantic City meeting [as local arrangements committee chair. Everett Lee, as first vice president, chaired the program committee]. Princeton was closest to Atlantic City.

VDT: Right. You remember that one, Atlantic City?

VAN DE WALLE: I remember most of them in some vague fashion.

VDT: What about the early 1970s, when there were hot and heavy debates in the business meetings on various topics, wanting PAA to get more policy-oriented--on abortion and on family planning in the Third World. Does that linger in your mind?

VAN DE WALLE: I remember there was a discussion about the women's caucus. Let me see when that would have been. I was a member of the Board. I was a director of the Population Association from 1975 to 77. At this stage, there were great discussions about the women's participation in the association and boycotting certain states where women were not admitted [boycotting--as meeting sites--states that had not passed the Equal Rights Amendment]. I remember in those days my own discoveries of the women's movement as something that was new and somehow I didn't fully understand, but I was willing to trust my female colleagues about the subject.

VDT: When did Francine go back to university or become interested? Did she get you involved at that time?

VAN DE WALLE: No, she didn't. I got her involved in demography, but she was not very politically active.

And in a way we were kind of . . . Belgium turns out to be a very traditional society, very stratified. America was somewhat more advanced from this point of view than Belgium. So in a way it was a new problem to me and to Francine.

VDT: Women?

VAN DE WALLE: This claim of women for equal status--which I was perfectly willing to grant them, but I didn't feel like there was a problem.

Then at the time of these directors' meetings at the Population Association, I remember we had some discussion on some motion that the women's caucus wanted us to vote on, and my own position--I remember stating this to the Board--was that I didn't fully understand what it was all about, but if the female members of the Board were in favor I thought that we should trust them, because they knew more than I did. I don't remember whether the motion carried or not, but my position was that I was quite willing to trust the specialists. Probably if I had been a woman I would have reacted in the same way.

Reflecting from the point of view of 1993, I can see that it seemed to me at the time that this whole thing was basically going on behind men's backs, in the same way that I was not really aware when I was at Princeton in the early days that there was a color barrier in the restaurants between Princeton and Washington--that when you drove there, blacks would not be admitted on both sides of the restaurant.

VDT: They would be in Princeton?

VAN DE WALLE: In Princeton they would, but not in Virginia or Maryland. I was not aware of any of this. Probably my feelings would have been very much in the right place, but they were never provoked in any way.

VDT: I see. What other issues do you recall through your years at PAA--were important in the organization--to you, that you were aware of?

VAN DE WALLE: To me, probably the importance of getting more developing world participation at the meetings of the PAA, because they have a unique position in the world as the one annual meeting of demographers. Although the IUSSP also has these international conferences which play the same role--there are a number of interesting series of them that produce high-quality international papers--the PAA meeting is way more important.

VDT: The PAA is more important than IUSSP?

VAN DE WALLE: To international demography. I think it [the annual meeting] is a world resource.

VDT: That's interesting. Of course, IUSSP only happens every four years [general conference].

VAN DE WALLE: Well, they have their regional conferences and they have their seminars.

VDT: Ah, the other ones.

VAN DE WALLE: But the PAA has more impact, I think--by the number of foreign people who participate. At this point, I'm the chair of the PAA outreach committee, where we're giving money--which, of course, comes from Mellon and Hewlett--to bring participants from all the various countries of the world to the meetings.

VDT: Did you have something to do with this? Because you said you've always . . . It wasn't so 20 years ago, was it?

VAN DE WALLE: This is a fairly new phenomenon. But, of course, there were always foreign students, and there were always American people writing on the demography of the world. There are now scholars who come back year after year to the PAA meeting from Europe. I'm thinking, for instance, of Ron Lesthaeghe and Hillary Page.

VDT: They were at Princeton, of course.

VAN DE WALLE: That's true of an important number of demographers in the world--they studied somewhere in the U.S.

VDT: Like Livi Bacci, who was at the meeting last year.

VAN DE WALLE: Hundreds of people who benefited from fellowships from, for instance, the Population Council, in the 1960s--like myself. There were hundreds of people who were brought to the United States.

VDT: Were you on a Population Council fellowship when you first came?

VAN DE WALLE: Yes. These fellowships, and then the fact that most demographers in the world got trained in American institutions--a good majority of present PhDs in demography in the world, or working in demography.

VDT: So you think that U.S. universities are still very important for demography generally?

VAN DE WALLE: There's no substitute; they are among the only places where you train people. Not the only places, if you include the London School of Economics and the Australian National University, and then of course you have Louvain which has been training Africans in large numbers.

But in general, you don't have anywhere else this massive training of PhDs in demography that you have here.

VDT: What about training of masters--more practicing demographers?

VAN DE WALLE: You have the UN centers, for instance in Yaounde [Cameroon] and Accra [Ghana], but these are sick institutions that are going out of business. And I don't think the kind of training that you get there is the equivalent of an American training.

VDT: So the PAA meeting gathers all these people together and you feel that is an important aspect of PAA?

VAN DE WALLE: I think it's a world resource. It has provided a kind of system of communication and standardization of the quality of research.

VDT: Okay. How did you handle the program in your year [1992] as president? You had the most sessions there had ever been up till that time; you had all this mass of material. You had 95 sessions, which is a record--there'll be more [101] this year--nine overlapping sessions at a time. This year there are going to be up to 12 overlapping sessions. What do you think about that--too much?

VAN DE WALLE: Well, I think there's been a tendency--and I helped--to maximize the number of

participants, because the meeting is actually the most important source of money for the PAA.

VDT: Good point!

VAN DE WALLE: That's a very crass point, but it was certainly a consideration.

VDT: By maximizing participation, you mean maximizing the number of papers on the program and then . . .

VAN DE WALLE: Hoping that people would come.

VDT: Okay. And you had over a thousand [1,085 registered attendees].

VAN DE WALLE: Absolutely.

VDT: Which Jen Suter [PAA Executive Administrator] thought was amazing for a recession year--in Denver.

VAN DE WALLE: Absolutely. We tried very hard to do so, and when it's to be a place like Denver people have to have a strong incentive to come.

VDT: Exactly.

VAN DE WALLE: And so with the next one [1993] in Cincinnati, I think the same kind of incentive exists. When it meets in Washington, we can get back down to a small number.

VDT: A smaller number of papers, you mean?

VAN DE WALLE: Of papers--or sessions.

VDT: We had 1,399 people at the Washington meeting [in 1991].

VAN DE WALLE: We can be more choosy. But what's important, also, is the high quality of these papers. In general, you know, you end up with all these papers that you turn down, but which are not that much worse than the ones you accept. They are the same kind of quality; they involve a lot of work. There are obvious rejects--in any conference there are people who shouldn't be there--but in general the quality of the PAA papers is remarkably high. And not all these papers eventually get published. It's a highly productive community of scholars.

VDT: In other words, you have to be there to hear those papers, know about them, because you're not always going to find them published.

VAN DE WALLE: Yes. I think probably most of them get published in some form or another.

But you end up with 90 sessions, four papers each--it's a large number of high-quality papers.

VDT: Did you enjoy your presidential year?

VAN DE WALLE: Yes. I was not a very active president, I think. Some of the presidents are, you know.

VDT: What didn't you do?

VAN DE WALLE: Well, I don't know. There were lots of things going on in Washington, organization of the profession, lobbying. One of the main issues was whether we should have a representative in Washington full-time or part-time. We ended up with Anne Harrison Clark [PAA representative for public affairs, part-time, with full-time assistant]. That was one of the main issues. That was debated under my presidency--whether we could afford it; how much we could afford, and so on.

VDT: And you were not terribly engaged with that issue?

VAN DE WALLE: I was engaged by it, but there were people who basically were much more activist than I was. I think that includes my successor, Al Hermalin [1993 president; president-elect in 1992].

VDT: Who helped start the Public Affairs Committee.

VAN DE WALLE: Yes. I was fortunately able to get off some of these activities to him; he was very active as the president-elect.

VDT: Do you think PAA should have such a representative--in the situation it now is?

VAN DE WALLE: I have mixed feelings about it. It seems to be very useful and to draw the attention of the association to some things and represent us and defend our interests.

I'm somewhat ill at ease about this whole idea that our interests are material and financial and not purely scholarly. But, of course, that's the way it is. And Washington is a very strange place, where things can happen to you. They can eliminate centers; we would never know unless we were informed.

So, I feel the need for it, but I don't feel that I should really be actively involved.

VDT: And there was the issue of the special interest groups, last year. The business and state and local demography in particular, which had been committees, were downgraded, they felt, into interest groups, and Larry Bumpass was chair of the committee looking into that. Were you involved in that issue?

VAN DE WALLE: I was a bit involved in that issue at the time of organizing the meeting, because you discover that there are a number of people who expect to have sessions and the choice of papers is in their hands, and there are very strong interests that you feel are handled with some kind of priority which maybe they don't deserve. This is an ongoing debate and I don't think it's solved by any means.

VDT: A debate about . . .

VAN DE WALLE: About whether we're going to move away from this idea of having interest groups. The Bumpass report, as far as I know, has not been acted upon or even fully approved. This is something that's going to be discussed again in the next meeting of the Board.

VDT: A small matter. PAA's number of members has fallen in the last two years. It went up the year [1990] that the transition [of the business management] was made to the American Sociological Association [PAA's own office in the ASA headquarters], and the dues went up from \$40 to \$70. Then

it's fallen, down to 2,525 [end 1992; end 1989--2,679; end 1990--2,752; end 1991--2,647]--which is about what it was in the mid-1970s. Do you think we should go out and actively recruit members?

VAN DE WALLE: No, I don't think so. It looks like a very stable curve. One can get away from the tendency to extrapolate a small movement on that curve. I think we're going to probably be stuck to that level of members.

VDT: It's been leveled off since the mid-1970s.

VAN DE WALLE: It's leveled off. But it's one of the most active and the most popular associations that exists.

VDT: Popular?

VAN DE WALLE: None of us would miss a PAA meeting. That's the only professional meeting that I go to. And it seems to be a nice group of very pleasant and interesting people.

Perhaps we should *not* attempt to increase these numbers.

The need to have a typical number of people is basically mostly present when you're thinking about having a representative in Washington or balancing your budget in some way. But if you're looking particularly at the intellectual content of the meeting and the usefulness of the association as a place where people are able to meet among themselves and discuss interesting issues, it's a very healthy association.

I wonder why the numbers are declining.

VDT: It's the recession, probably. I sent a check to join PAA to my new demographer niece, who's now working for Compusearch in Toronto--she was Paul Voss's student; got her master's at Wisconsin last June--and she still hasn't joined. I said, "Why not?" She said, "Well, my company won't pay my dues."

VAN DE WALLE: Yes.

VDT: And the second year--\$70--is a lot.

VAN DE WALLE: It's probably a lot of money.

VDT: For young people. But it is also the recession. You can see the student members are down too a bit [509 to 470--end 1991 to end 1992].

We must end. You've talked about your future plans, which is this book, perhaps, on family limitation.

What is Francine's role in your work? You've collaborated on a number of papers, based on the research that you did in Africa in the early 1980s. Have you continued to do that?

VAN DE WALLE: We continue to work on various African-related topics. We are now writing a paper on the status of women for an IUSSP meeting in Dakar. Of course, it's better to have a woman delivering the paper than a man.

VDT: Where did the data come from?

VAN DE WALLE: This is a literature review, and it's going to be for this meeting.

VDT: On?

VAN DE WALLE: Basically on the notion of the status of women, or the importance of the status of women, in African demographic research. This whole meeting is devoted to the status of women in Africa.

VDT: Popular topic.

VAN DE WALLE: It's a popular topic. It's an important topic and it has attracted a lot of papers, because the status of women seems to be so awful in Africa with the economic crisis and with AIDS, which is almost a--I was going to say an occupational hazard. A lot of young women trying to make a living in this hostile environment of the city are resorting to various forms of commercial sex, which does not have the same kind of negative connotation that it has in the Western world.

VDT: There's an excellent series of articles on women in developing countries running right now in *The Washington Post*; some are on Africa. You should read those if you're writing on women's status. It's depressing.

VAN DE WALLE: Yes. I wrote that paper on AIDS. One of the issues is women have been gaining a lot in education and labor force participation quite recently in Africa and that is removing them from the protection--which is at the same time a kind of moral protection--of their families. You would run the risk of sending your daughter to the city so she would get some kind of degree and education and prospects, and then it also exposes her to this terrible disease. So what is going to be the next reaction of those parents? They are going to try to marry these young girls off to reliable males in the village. So it has enormous potential implications for the status of women.

VDT: What happened to your four children? What are they doing? Any demographers or quasi-demographers among them?

VAN DE WALLE: Some of them have touched demography, actually. One of them was at the Futures Group in Washington.

VDT: Who's that--son, daughter?

VAN DE WALLE: That was my last one. Demographers from Washington--the crowd of the Futures Group and USAID-connected people--keep asking me about him. He's now in London. He works for Columbia Pictures.

VDT: That's a shift!

VAN DE WALLE: I have a daughter who is working on at least demography-related topics at the World Bank. She's working on poverty.

VDT: Really! What department?

VAN DE WALLE: Research--I can't really give you the exact title.

VDT: Is she an economist?

VAN DE WALLE: Yes.

VDT: All World Bankers are economists.

VAN DE WALLE: She has a Ph.D. in economics from Australia--from ANU.

VDT: How did that come about?

VAN DE WALLE: Because she married her instructor at the London School of Economics and then he got an invitation to teach at the ANU and she followed and got her Ph.D. on poverty. And then was exactly at the right place--with her Ph.D. on poverty, living in Washington--when the World Bank decided to devote its *World Development Report* [1990] to poverty.

VDT: *The* famous one!

VAN DE WALLE: She was part of the team that wrote the poverty report, and she stayed on.

VDT: Does she use the name van de Walle?

VAN DE WALLE: Yes [Dominique van de Walle].

VDT: I should know that; I know that report quite well. Where's the husband?

VAN DE WALLE: The husband is also at the World Bank. Her husband is Martin Ravallion. He has also been working on problems of famine and poverty--in Asia.

VDT: Is he English?

VAN DE WALLE: He's Australian.

VDT: And the other two?

VAN DE WALLE: Then I have one who is assistant professor at Michigan State, who also has been almost on the border of demography. He's in the political science department, and works on Africa.

VDT: And the fourth one?

VAN DE WALLE: The fourth one has an MBA from the University of Pennsylvania, from the Wharton School nextdoor. He's in New York.

VDT: What's he doing?

VAN DE WALLE: Investment banking. Two non-academics and two academic--two PhDs and two MBAs.

VDT: What an interesting crew! Grandchildren?

VAN DE WALLE: Three grandchildren.

In a way, one of the incentives to come to the University of Pennsylvania was that tuition was free [for children of faculty members] and at that time I had four children, who were spaced one year apart. So I had the world record--having four students at the same time at the University of Pennsylvania.

VDT: They all were here, at one time!

VAN DE WALLE: At one time--all of them undergraduates. And then two of them got an MBA at Wharton. So I saved heaps of money. [Laughter]

VDT: Besides having an interesting career, because I think you feel you've had an interesting career.

VAN DE WALLE: I've had an interesting career.

ALBERT I. HERMALIN

PAA President in 1993 (No. 56). Interview with Jean van der Tak at Jean's home in Washington, D.C., October 30, 1994.

CAREER HIGHLIGHTS: Albert Hermalin was born and brought up in New York City. He obtained the B.S. in mathematics and statistics from the City College of New York in 1949. From 1949 to 1964--except for two years in the army, 1950-52--he was with the Institute of Life Insurance, rising to be Associate Director of the Division of Statistics and Research. He entered Princeton as a Milbank Memorial Fund Fellow in 1964 and from there received the M.A. in 1966 and the Ph.D. in 1969, both in sociology and anthropology. Since 1967 he has been at the University of Michigan where, among other posts, he has been Professor of Sociology in the Department of Sociology (since 1978) and Director (1977-87; 1990-91) of the Population Studies Center and Research Scientist (since 1987) at the Center. He has been adviser, committee member and chair to many population-related organizations, such as the Committee on Population of the National Research Council, the Census Bureau, the Alan Guttmacher Institute, the Population Council, the United Nations, and the International Union for the Scientific Study of Population. Among his awards, he was the first recipient, in 1988, of the Robert Lapham Award of the Population Association of America, given for "contributions that blend research with the application of demographic knowledge to policy issues" and for "service to the population profession."

Al Hermalin is well known in the population world for his research and publications on the evaluation of family planning programs, generally in Asia and especially Taiwan, where Michigan was involved when he joined the faculty and still is, and for his work in developing better methods for the analysis of fertility and, most recently, his work on the elderly in developing countries. He is author, coauthor or coeditor of four books and over 60 articles and book chapters in population, as well as several publications stemming from his earlier work in the field of life insurance.

VDT [from interview introduction]: Dr. Hermalin is in Washington from Ann Arbor for the fall meeting of the PAA Board, which was held yesterday [October 29, 1994].

I thank you very much for making time for this interview, Al. I've long pursued you, hoping to end up my series of interviews with PAA presidents and secretary-treasurers with you. This will be the fourth and last supplement to Demographic Destinies, which I put out in 1991. That had 49 interviews and with this, there will be four more. I know you are interested in the series too, because you were the fourth person to actually buy a set of Demographic Destinies after I had distributed my complimentary copies.

I note from your curriculum vitae that you spent 1950-52 in the army; that was during the Korean War. Were you drafted?

HERMALIN: Yes, just a few months after the war started.

VDT: But you were based in Alabama and worked as a mathematician, so your skills weren't being wasted at that time.

VDT [after biographical introduction]: What led you into demography--and to Princeton--leaving what was obviously a fine career in the field of life insurance?

HERMALIN: I read a number of your previous interviews and I've been struck by the wide diversity

of how people get into population and demography, because it seems--as some of your previous guests have noted--that it's not a thing you know you want to do when you're 12 or 15 years old.

In my case, I think the scenario is a little complex, in the following way. As you noted in the introduction, my degree was a Bachelor of Science degree. I took math and statistics as my main areas; I took a year in every science. You had to do a year in biology, a year in physics, a year in chemistry--those were the elements of a Bachelor of Science degree as against a Bachelor of Arts degree. So it was much more focused in the sciences, though I took quite a bit of philosophy and there were requirements in history, English, etc.

Consequently, I took relatively little social science. I remember a course that I thought very little of was sociology. We were required to take one course in sociology and I remember it was an older professor who seemed to me to be lecturing from yellowed notes and saying things that were quite obvious, like "people live in families" and "families combine with communities," and that didn't attract me as a scientific challenge in the way they were posed.

When I finished my undergraduate work in 1949, I had fairly well decided to go right into work. That was prior to the time when graduate schools were booming. At least it wasn't in my consciousness that this was a very strong option. I was asked by a dean at City College about my interest in a program to help develop teachers, but it didn't seem very attractive. My image of teaching was mixed. I knew I was reasonably good at explaining things to other people, because I often would coach people or help friends with math problems. And I felt I had a fairly good empathy with what people understood and what was blocking their understanding. But as a profession it seemed to me that it would not be very rewarding; you would basically be in a classroom with a high proportion of people who didn't want to be there at that particular time or place. And that seemed to me something that I didn't look forward to. Most of my thoughts about teaching were what it would be like to teach at the high school level or in a heavy teaching environment like City College, which was not a research university at that time. Research universities were not part of my experience.

So when I finished City College I was interested in working, and actuarial work seemed to be one reasonable outlet for my training in math and statistics. I looked into jobs with a number of insurance companies in New York, of which there were quite a few. At that time, as I recall, there was a bit of a recession after the immediate postwar boom, so employment was a little flat. But there was an opening at the Institute of Life Insurance which interested me a great deal because it wasn't just straight actuarial work. They were the public relations organization for the whole insurance industry, and the Division of Statistics and Research put out a very highly regarded life insurance factbook in which we collected a lot of information from all the insurance companies or from other sources. We developed in part the statistical database for the public and for the industry. So I applied for that job and I was fortunate to get it and started working there.

At the same time, I had to study and sit for the actuarial examination series that one has to take if one wants to be an actuary, which is mainly a graded set of exams that go through different aspects of the mathematics or subject matter of insurance, starting with examinations in finite differences, in compound interest, and then moving on to the substantive areas. I passed a number of those, but it wasn't a high priority for me in a way, because we were not an insurance company and there were not many actuaries around and we were not calculating rates or doing other actuarial work.

In the early 1960s, the new president of the Institute of Life Insurance felt that we ought to intersect more with the social and behavioral sciences and understand more of them, and he asked me to start to interview various professors in the New York area about what were some of the emerging social trends that the industry ought to be aware of. And I decided to take some courses at NYU in the evening to further my knowledge of the area, which as I said was relatively limited. I took a number of courses in that time. In fact, I think Charlie Westoff was teaching at NYU, but I didn't have a course with Charlie.

I took several courses in sociology--in stratification, small groups, mass media and other fields--with Marvin Bressler and others. Marvin was not at Princeton then, but he moved to Princeton in 1962 or so just about the time I started. I was very influenced by Professor Bressler. He was a wonderful teacher and I became very interested in sociology as a science of society. I realized that one could develop important hypotheses to test in a rigorous fashion. And I also became intrigued with the challenge of teaching because Marvin was such a wonderful teacher and could make people excited about the subject matter.

So after some discussions in the family--I'd married in 1961, my son was born in 1962 and I had a daughter being born in August 1964, a month before I moved to Princeton, so it took a little discussion--but we decided to do it.

VDT: Do it--meaning . . .

HERMALIN: Meaning to go to graduate school. And Princeton attracted me because when I started to look at schools, it was much less formal in how you proceeded. They looked at it as a three-year Ph.D. There weren't any number of required courses. You took your prelim examinations when you were ready--I think they called them the general exams there--and then you went on and wrote your dissertation. In their hazy thinking at that time, they thought this would be a three-year program roughly. It turned out not to be for almost anybody.

VDT: Except Sam Preston.

HERMALIN: Yes, it could be. Theoretically, if you had your data in hand by the time you finished your second year and knew where you were going, you could get done in three years. Going back to school after a number of years of employment that attracted me; I wanted to be able to move at my speed. I had always taken some courses in New York in different fields and had done well with the NYU courses, so I felt that I could go back to school successfully and compete. I felt I wanted to be able to move at my speed, so Princeton was attractive in that sense.

I applied to the department of sociology and then I received a call from Charlie Westoff, saying, "We'd like to offer you the Milbank [Memorial Fund fellowship]." I said, "What's involved?" He said, "You just have to agree to take population courses; you don't have to become a demographer or anything else."

VDT: You had thought you were going as a sociologist, into sociology?

HERMALIN: Yes. I knew OPR [Office of Population Research] was there and I had planned to take the courses. I had some knowledge of demography; I had conducted some mortality analysis at the Institute of Life Insurance. But I was not going because I thought, "Well, OPR is there and I'm going to become a demographer." I went back to be a sociologist, first, and not a demographer.

My area of specialization remained open for a long time. I accepted the Milbank and it was wonderful being at the Office of Population Research. You had an office or a space and it was a very rich environment. I took the courses and spent a lot of time focusing on the population-demography sequence. But at Princeton at that time, you had to offer four or five fields as part of your examinations. I did a field in theory, education, methods, and demography--something like that--four fields. I was very interested in the sociology of education; I was very interested in sociological theory.

In the mid-1960s the universities were growing so fast that there were many, many openings and one had to a large extent a choice of what kind of department one wanted to go to. There were more jobs, I dare say, than there were people coming out.

So, in my third year I still had an open mind on whether I would end up in . . .

VDT: In your third year? Weren't you by that time already at Michigan?

HERMALIN: No, I left at the end of the third year. I went to Princeton in September 1964 and was in residence 64-65, 65-66, and 66-67. So in the third year, I was making decisions about my specialization and I still had somewhat of an open mind whether I would be a population specialist, a demographer, or something else.

I had moved more and more to demography for a number of reasons. First of all, I had an office at OPR and I was getting more and more involved and I was interested in it, of course. And, secondly, I sensed a certain amount of lack of closure with some of the other areas of sociology where it wasn't as clear to me that one could mount the evidence successfully to test hypotheses as rigorously as one would like, so I had moved my thinking toward population and demography. But, as I say, even in the third year as I was looking for a job and thinking about jobs, I was still somewhat open as to what direction that would take.

You asked me [in pre-interview letter] how I got to Michigan, how Michigan figured.

VDT: Before we talk about how you got to Michigan, I want to talk a bit about your time at Princeton.

HERMALIN: As I say, I became more and more drawn to population. I was working at OPR quite a bit. There wasn't a formal mentoring program at the Office of Population Research. All the people who were either being supported by them or who were clearly interested in population, they gave them first space. And it was a very exciting time because they were also running an overseas program--a one-year certificate training program--and there were a lot of visitors from overseas. You made a lot of wonderful friends.

It was an interesting environment. It wasn't that I was working for anybody, but this was a very exciting period in population studies and there was a lot happening all around you. The indirect methods of estimation were being developed at a very rapid rate. Of course, Ansley Coale was the author and developer of many of these techniques and Ansley was actively engaged in doing the research. It was not rare for him to come in and lecture and say, "Look what I just found out" or "Look at this interesting relationship." So there was a great sense of new things happening in the field. It was a very exciting time both in the formal part and, of course, in the area of family planning. I remember Ansley coming back from a meeting talking about the IUD for the first time and discussing what its implications were and what the expectations might be. We were very aware that many of the countries at that time had very high growth rates. I remember we noted that Costa Rica was growing at over 3 percent, a very high rate of growth. At the same time, lots of new things were developing. I remember as students, we'd be surprised if we looked at Ansley Coale's exams of the year before; they always looked easy. The new exams were hard because they incorporated so much new material and last year's seemed trivial by comparison.

So there was a sense of excitement, of lots of things to be studied, lots of things happening. Of course, that made it a very interesting environment and also led me more and more to want to be a part of that and to make that the focus of my work in sociology.

The office always had lots of visitors. Frank Notestein used to come by quite often; so did Clyde Kiser of Milbank; a lot of people passed through the office. So that was exciting as well. Sometimes they would give talks; sometimes we would just chat. Of course, Irene Taeuber was there quite a bit, traveling back and forth. I remember chatting with her about a recent trip she was very excited about. And, of course, Charlie Westoff was conducting a number of big surveys--Metropolitan Growth in the United States [Princeton Fertility Study] and that series with Norm Ryder.

VDT: The National Fertility Study. They were picking it up [continuing from the Growth of American Families] and doing it in 1965.

HERMALIN: That's right. They were just starting that while they were continuing the metropolitan series [1955-70]. They had three books on the metropolitan series and then they dealt with the national surveys. So there was a lot of data on fertility and family planning and a lot of discussion of what was happening in that area.

It was a very rich environment--in the formal demography, the knowledge of what was going on around the world in terms of planning and policy, as well as the survey work that Charlie and Norm were doing. You learned a lot by just having coffee there, so to speak. And people spent a lot of time there; you tended to do lots of your work there.

So it became clearer to me that that was the direction I was going, even though as I said there was always some openness in my mind as to exactly what path I would choose.

The Michigan connection came about through a visit Ron Freedman paid to Ansley. Ron, of course, by 1962 had started work in Taiwan and run the Taichung experiment with Barney Berelson. In 1964 Taiwan had started a national family planning program and Ron was going to be studying that and its development and he was looking for somebody to work with him particularly in that area and he asked Ansley whether there were any graduate students that might be interested, and Ansley said, "Well, you ought to talk to Al. This will be a big project and you want somebody who is a little older and has some administrative skills."

VDT: Oh, that's part of the reason.

HERMALIN: I think so. I wasn't there at the conversation, but I think the fact that I had lived another life besides just being a student and had some awareness of the demands of a large project was part of the discussion.

VDT: Of course, you know that Ron Freedman in his interview remembered it as--we were talking about Larry Bumpass at the time--and Larry Bumpass was lured away to Princeton to join the National Fertility Study.

HERMALIN: Right.

VDT: And Ron said that he told Charlie Westoff, "We have to talk baseball here. You got my prize first baseman; you have to send me somebody in return." And you came.

HERMALIN: Well, there was a sense of that. I forget the exact timing. Charlie and Ansley had asked me whether I'd be interested in staying on at Princeton to do some work there. It wasn't clear whether I would work mainly with Charlie's surveys or do some things with Ansley. And I was very tempted to do it. In fact, my feeling was that that would be ideal, to stay a couple of years. I remember Charlie saying, "This would a way to consolidate your work and get out some articles and get in on the ground floor of a survey before you get enmeshed in all the teaching."

VDT: Of the National Fertility Study?

HERMALIN: I can't remember how it was pitched. In part there was an interest in having me do a number of computer things; there were a lot of new developments in computers and they thought I

might want to help direct where that would be going for several projects at OPR, but also to be involved with the National Fertility Study. As I say--I want to be cautious--I can't remember the exact flavor, but there was a discussion, reasonably firm, indicating there was the opportunity to stay on if I wanted to.

And I was very tempted to do that; I thought that would be a very logical thing to do. But I accepted Ron's offer to come out and visit. I remember it was a January day in 1967--actually, it was before that. It must have been earlier, around November 1966, because I went out and I made a 180-degree turn in my thinking. I was very much taken with the Population Studies Center at Michigan. I liked Ann Arbor; I had been there once before. You mentioned my service in the Korean War. When the Korean War broke out, I was at Ann Arbor for the second summer workshop of the Institute for Social Research. I was sent by the Institute of Life Insurance to learn more about survey sampling and survey research, which was then a rapidly developing field. I took the first course Les Kish ever taught in survey sampling, that summer. That's how I met Les and we became good friends. So I always had a warm spot in my heart for Ann Arbor and high regard for the university.

I came out to Michigan not at all convinced that this was the right move, but I certainly felt I ought to go visit. I remember coming back and saying to my wife, "I think we ought to go to Michigan." I'm not sure if it was wholly rational or that I thought it through in great detail.

Part of the reason, I think, is that when you're a graduate student--and I was treated wonderfully at OPR--there comes a certain time when you say, "I ought to go out and be my own person." That struck me somewhere in my psyche. So I told Ron I would accept the position at the end of the year.

I can't remember the exact timing. But then Ron said, "Why don't you come to Taiwan, over the Christmas and New Year's break"--I was still a student at Princeton--"to get acquainted with the place and meet some of the people. It would be worth your while. You'd get a head start, even though you're going to go back and finish the rest of your third year." So I went to Taiwan for the first time in December 1966. I remember landing in Tokyo New Year's Eve, December 31st, and luxuriating in a hot bath around midnight. I thought this was an interesting way to spend New Year's Eve!

VDT: On your way there?

HERMALIN: Yes, on the way to Taiwan. In those days, you landed in Tokyo after a long trip. You had to spend the night in Tokyo and then there was a morning flight from Tokyo to Osaka, to Okinawa, and then over to Taiwan. I remember that flight quite distinctly. We spent about two weeks there and then came back about mid-January. Then I finished up my term and came to Michigan in August of 1967. So there was a fair amount of discussion and involvement even before I finished my third year at Princeton.

VDT: May I just ask about Fred Stephan, whom I see was the adviser on your dissertation? He has been mentioned in some of these interviews and he was a biggie back then, I know. What about him?

HERMALIN: Fred was in the sociology department, but he also was busy at that time trying to form the statistics department at Princeton. Princeton had very distinguished mathematical statisticians and at that time, at least, it was open whether statisticians should have their own department or be located within math. Fred was very active in that. He taught the methods class and the statistics class. He was a very kind and gentle person, who was very good at making you understand the background and the meaning of the abstract formulations. He was less interested in taking you through formal aspects of each subject but he wanted you to understand what was behind everything; so he was very successful in that.

He had a wealth of experience in different ways. He was one of the people who discovered that

one of the census errors occurred because things got punched in one column as against another. I think it related to why there were too many Spanish American veterans in Maine. He figured this out by studying the codebook. He had a very operational view of things. He wanted you to understand that when you're looking at published data you're looking at the end result of a complex operation. You want to be able to trace back through all the steps and not take things at face value.

VDT: I did want to ask a bit about your doctoral dissertation, the title of which was "Homogeneity of Siblings on Education and Occupation." What was that? You had a note on it in the Journal of Marriage and the Family, but then you seem to have lost it.

HERMALIN: We were talking about the events that shape people's careers in different ways.

My going to Michigan and choosing to work with Ron on fertility and family planning was a bit odd in a couple of respects. A paper I had written before I came to Michigan was on mortality. I presented that paper at the American Public Health Association meeting as a graduate student. It sought to trace the effects of mortality on growth rates and age structure ["The Effect of Changes in Mortality Rates on Population Growth and Age Distribution in the United States," Milbank Memorial Fund Quarterly, 1966]. Partially because of my life insurance background and knowing something about life tables and causes of death and morbidity, I always thought I would work more on mortality than anything else if I were to specialize in demography. This is one of these accidents that happen to you.

The dissertation was another example. Because of the breadth of the training at Princeton as a sociologist, I had developed an interest in trying to learn to what extent brothers and sisters end up with similar outcomes in education, occupation, and other social positions, as distinct from personality measures. The counterweight to that, so to speak, was the long literature on birth order, going way back to Galton, hypothesizing that first-born are like this and second-born are like that, etc. If you think about it that always gives you the sense that siblings are quite different, and it seemed to me the challenge was to measure how much do they resemble each other and how important is birth order vis-a-vis resemblance. So my dissertation was really looking at how important is birth order and how important is resemblance.

And the topic was interesting because it was not a usual topic for a person in demography to take up. In fact, Ansley was pushing me to do a dissertation on why life tables were changing in structure over time. But I was a little tenacious; I wanted to do this dissertation and I was interested in the topic.

I wanted to develop it further and put it out as a book so I didn't develop it into a stream of articles, as is usually done by a graduate student, because I got busy with Ron's project and got involved with other things. But the ideas behind it have been continued in the work of Bob Hauser and a lot of other people--in work about birth order and family relationships. So the concept that the shared background of sibs is important has become well recognized.

VDT: In the same vein with your later work on the importance of contextual variables.

HERMALIN: Right.

VDT: What were the data for the dissertation?

HERMALIN: The data were interesting. One of the things that was frustrating was that once I knew what I wanted to do I had to find data that had information on all the children of a family, and that's not the way they're usually collected. You get data by household and you usually only collect information

for those in the household. But I found data from a survey that was done for the New Jersey telephone system. It was part of a study on heart disease and stress, but part of the interview was to ask these people, who were basically over fifty, about all their children and the outcomes of all their children. I was given access to those data. I could not find data any place else.

Later I found that the Institute for Social Research had done a survey on educational determinants that indeed had asked about all children, but they had not coded each child separately and I would have had to go back and retrieve the actual questionnaires and recode them. In those days, it wasn't the usual way of collecting or recording information, so it was a contest to get those data.

VDT: Good. So now we're at Michigan and you've made the big switch not only to Michigan but also into fertility and family planning, which you have been involved in ever since.

The Caldwells [John and Pat] say in their book, Limiting Population Growth and the Ford Foundation Contribution [1986], that only you and David Goldberg and the Freedmans, from Michigan, were doing the international work. Is that true?

HERMALIN: Yes. The Center, like many centers, was multi-purpose.

Two things about Michigan were interesting. I was the first person to come there who was not a University of Chicago graduate, except for Dave Goldberg who had his degree from Michigan. Ren Farley, Paul Siegel, Dudley Duncan, and Ron Freedman, of course, were all Chicago graduates. I sometimes point that out, because these intergenerational flows are interesting. Later, a number of our graduates went off to Wisconsin--another generation and another set of influences. I was to a degree an outsider, the first with the perspectives that came out of Princeton's work.

Ren, of course, was doing a lot of work--he'd written his dissertation on aspects of American race differentials. And Paul Siegel was studying occupation and mobility. There was a lot of ongoing work on U.S. issues.

So I and Ron did most of the work on Taiwan. Dave Goldberg also was working on overseas things; he did a survey in Turkey and a survey in Mexico. So there was a mixture of international and domestic work. I think that was part of what Ron negotiated with Ford. Part of Michigan's heritage in population came out of studies of urbanization, going back into the 1930s--McKenzie, Hawley on human ecology, and so on. Ron moved from that background to American fertility and then into international work.

VDT: You mean that had something to do with Ford monies going into Michigan?

HERMALIN: I mean the population centers Ford supported. As I recall the Caldwells' book, Ford supported a number of centers, but I think they were very good in not trying to prescribe that the work all had to be international. They wanted to see strong population centers develop and people would be free to work on what they liked.

VDT: Hmmm. That's not the way the Caldwells put it. They put it that you had to have a less-developed-country focus. And, in fact, they said that's why Pennsylvania didn't get all the money they asked for at first, because it didn't look like they were going to move into that area, or not as likely to as Michigan was.

HERMALIN: There had to be at least some international work. That's right.

I think the tone of it was that Ron didn't want people to feel that they had to do international work. It's true that Ron was doing it, but it wasn't the case that everybody had to do it or that the people who did not do it would somehow be second-class citizens.

The money that Ford gave supported the general infrastructure of the Center and supported a lot of the training we did, but it was not given out, divided up, according to whether somebody was doing American work or foreign work. The funds did a great deal in fostering demographic work per se, and of course fostering international work as a part of that. The difference is just a matter of degree.

VDT: Let's talk about your research and your work in family planning program evaluation, which you have stressed, certainly at the beginning of your career; it has now shifted to work on the elderly.

You said in your PAA presidential address in 1993 ["Fertility and Family Planning Among the Elderly in Taiwan, or Integrating the Demography of Aging into Population Studies," Demography, November 1993]: "In many ways, family planning research is an integral part of fertility research." I thought of that as a little defensive, because you started in on doing that work at the time when some mainstream demographers were skeptical of what Donald Bogue was doing, and Ron Freedman, and Joe Stycos is somebody else that Bogue said was "one of us." Notestein had been too, of course. Do you think that family planning program evaluation research is now respectable?

HERMALIN: Oh, yes. There are two points here I think are important. One is if you pick up a general textbook on evaluation, they often make the point that with evaluation the goal is not to break new ground in the subject matter; it's to use what you know to see whether something is having an effect--either on an immediate goal or a long-term goal--or whether a program is cost-efficient. So there's a sense in which evaluation can be a fairly narrow assessment type of thing.

What I feel quite strongly about in the recent history of demography is that the attempt to evaluate the effect of family planning programs has also contributed a great deal to the field of fertility in general, by forcing us to model the reproductive process in much more detail, in understanding the adoption of contraception, and in separating specific techniques. You can see this in the first book Ron wrote on the Taichung experiment.

VDT: Family Planning in Taiwan: An Experiment in Social Change [1969]. He said you were involved in the finishing of that book.

HERMALIN: Yes, I came after it was well along but I wrote one chapter and worked on others.

If you think of the chapters by Robert Potter on multi-decrement tables. This was a way to study continuation rates and to understand how long people used contraception, but those were also part of developing the general sets of methodologies that have been used in a number of different areas. I sometimes stress in my classes that studying how long a contraceptive is used and the reasons for which it is stopped being used--whether it's a voluntary termination or something that happens; in the case of an IUD whether one expels it, gets pregnant, or whether one takes it out--is very much the same structure as looking at what happens to a marriage. A marriage starts, it can go on, it can end by widowhood, it can end by divorce--the exact same technique.

VDT: So you're saying that techniques you use in family planning evaluation can be applied in other fields?

HERMALIN: Yes. And the people who were working in family planning were developing techniques that were carrying across and also helping people to sharpen up their thinking about many aspects of fertility. Instead of just looking at fertility as something that is basically counting births, we learned a lot more about breaking down the process. If women adopt contraception while they're breastfeeding, how do we account for the fact that that may not be an efficient time or as useful a time as some other time? So this led to a lot of modeling of the reproductive process and the intervals

between births and understanding each of the components, like breastfeeding and amenorrhea, which influence them.

VDT: You talk about the people who were doing that, which includes you, of course. You began to do quite a bit of honing of the analysis methods and modeling.

HERMALIN: I did some. I guess I would say that there was a lot of synergy across different endeavors. The sort of things that Bongaarts did in breaking down the components of fertility--going back, of course, to the work that Davis and Blake started. Then there was the renewal theory, the sort of work that Jane Menken did. These weren't all necessarily evaluation, but the goal of trying to understand fertility and measure it and its components quite precisely were all parts of a general enterprise, to which the people who were trying to look at the effects of family planning contributed their share.

I may sound defensive in part because there was a time when some people who were very strong proponents of family planning just asserting, "We ought to do it, without studying it." I guess what I was saying is that those people who took it as a scientific area were contributing to the whole realm of work in fertility and family planning; it was rather seamless, so to speak.

VDT: That's a good way to put it.

Part of your contributions that I have made out were the multilevel approach to studying fertility, and looking at the context variables, which go back certainly to 1967, when Kingsley Davis was saying, "You have to look at context variables too." ["Population Policy: Will Current Programs Succeed?" Science, November 10, 1967]. Some people perhaps felt that withdrew attention from the actual family planning programs.

You work very often with models in your publications and research. Were you influenced by Ron Freedman? For instance, I very much like his funnel model, which I used for a term paper on French-Canadian fertility from the 1600s on; got an A in it--a marvelous organizing principle. Did he inspire you on that or are you just naturally tuned that way? To have a model up front; some of them rather complex, I must say.

HERMALIN: I think there are a couple of things. I think as I also said in my presidential address and as a number of other people have noted, demography tends to attract people who think in terms of breaking things down into pieces and then assessing the way pieces go together. I think people who think that way get attracted to demography and I think that mode of thinking--implicitly or explicitly, depending on where you come from--is part of the training in demography. One of our comparative advantages as a science is that we learn to think in terms of components and pieces and don't mix together things that need to be taken apart and looked at very carefully. So I think a number of us share that and, of course, we exchange specific ideas.

Yes, Ron's thinking about how to model fertility was very influential. Ron is a very good sociologist and knew that one factor was unlikely to account for all the effects. He was a very keen observer wherever he went. He saw all the changes going on in Taiwan and he realized the key question--and the challenge Kingsley Davis put down--was how do you separate what's going on in the society and in the economy from what the family planning program brings explicitly? And he tried to reflect this in his overall views and in the schematics he developed.

I also was influenced a lot by Dudley Duncan, who had developed path analysis--or introduced path analysis, to be more precise, into the social sciences, around that time. I think that key article in AJS was 1965 or 1966 ["Education and Occupational Mobility," with William Hodge, American Journal of Sociology, May 1963]. I did not know that piece until I came to Michigan and was able to interact with Dudley. I was still writing my dissertation, because I came to Michigan so early, and I

did a chapter in my dissertation using path models. That became a useful mechanism to me for carrying out some of the things that Ron was suggesting in terms of the theoretical framework. So it was a melding of the ways of thinking about things and using some of Dudley's insights and ways of approaching things as a mechanism to carry them out. I would say both of those were influential.

VDT: I like the way in your publications you suggest the economy of using existing data. Of course, you used the World Fertility Survey data, now DHS [Demographic and Health Surveys], and you went back and looked at old KAP [contraceptive knowledge, attitudes, practice] data--at least you were looking for it; I don't know if you analyzed it. Or adding just a few questions to ongoing surveys, as you have suggested in your PAA presidential address. Seems a very economical approach.

HERMALIN: I was pleased with that effort of retrieving the old KAP surveys [Hermalin, Barbara Entwisle, and Lora G. Myers, "Some Lessons from the Attempt to Retrieve Early KAP and Fertility Surveys," Population Index, Summer 1985]. And it grew out of, I think, a nice intersection of how your modeling and your theories then come together with your data needs.

It occurred to us as the World Fertility Survey was dominating so many efforts. People often said when the World Fertility Survey started that there were 500, 600, 700 previous KAP surveys--I forget the number--that had been mounted. And one reason the World Fertility Survey came on the scene was that these were of highly different quality, with different sampling designs, questions and sizes, and that we needed to have a much more standardized approach to get an overall and careful picture of what was happening to fertility around the world, particularly in the developing world. But then it occurred to me and some of my colleagues that it would be a shame if all of those other surveys got lost or were not available.

There were two things. If you think of what happened to demography, there was a time that the census was our stock in trade. And part of being a census is that it's done under government auspices and there are plenty of official hands to oversee its production and its safekeeping. As we were moving more and more into the world of surveys, there was not that same mechanism in place. Who owns a survey; who's responsible for a survey? Very often a lot of these surveys were done by a person who said, "Gee, I'd like to do a survey in some country." They find some colleagues there and get some funding. Usually there's nothing in the award that says you will make these data available; you will safeguard them; you will archive them. So we were concerned that some valuable materials would disappear.

VDT: A quick aside. I've got two boxes Jeanne Clare Ridley gave me from the Indianapolis Survey in my closet right round that corner.

HERMALIN: And the other point--to tie into the theoretical point--is that as I and my colleagues were working on multilevel modeling, we realized that some real richness could occur if we could get surveys over time in the same place and then see what would happen to context as well as to the individual behavior. So we were hoping that some of these KAP surveys would become Time 1 data to match with the World Fertility Survey as Time 2 data and start to model things in terms of what happened over time, rather than just take it all at a cross-section.

So there was both an interest in preserving these data and also seeing that they potentially could be very valuable for analytic work.

VDT: So that was your family planning research. Have you read Lant Pritchett in Population and Development Review ["Desired Fertility and the Impact of Population Policies," March 1994]?

HERMALIN: I did.

VDT: Arguing what is an old argument. He's claiming that family planning programs have little impact on fertility. It's desired family size that matters and that's influenced by social, economic and cultural conditions. What do you think about that?

HERMALIN: Well, I won't go into the technical arguments, some of which . . .

VDT: John Bongaarts did in the current issue [September 1994] of PDR.

HERMALIN: John Bongaarts and others. I think the analysis is naive along certain lines.

I think that the weight of evidence is such that family planning programs in a number of places clearly have made a difference in at least the speed and the rate at which things have changed. I don't think anybody argues that the social and economic aspects are not important. The work in Taiwan was strongly motivated by that idea. We used the areal data because we could get measures of family planning and social and economic change and test quite directly what Kingsley was saying. That is, I had measures for all these things, and I was able to show that, even after taking into account the social and economic change, the places that had stronger family planning inputs had higher contraceptive adoption and lower fertility.

I think one way or another that kind of work has shown that programs do make a difference. The very careful work that's been done in Bangladesh by Jim Phillips and all the associates there, and a number of other instances, and of course the Taichung experiment per se.

One of the things Pritchett, for example, doesn't ever talk about is the research that's done within countries. He's taking very big cuts across countries in a fairly limited way.

I think there's also an irony for those like you and me who go back a long way--one of the things I found kind of ironic is that Pritchett takes as a matter of fact that when people tell us how many children they want that that's a well-thought-through and meaningful number, where in the old days of family planning the big attack was, well, you can't learn anything from these surveys: what people tell you doesn't mean anything. Now the argument is turned around and what they tell you is exactly what they mean.

VDT: You have, of course, done a lot of work looking at what they really mean.

HERMALIN: Yes. You remember the early attacks on some of the work in family planning: "Well, of course, people will tell you what you want to hear; they want to oblige the interviewer." There was a great denigration of the accuracy or the reliability of those data. And now it's turned around and people take them as perfectly accurate, in order to spin a different kind of story. So there are a lot of interesting ironies there.

VDT: Did you influence Barney [Bernard] Berelson and Ron Freedman and Robert Lapham in their family planning effort work?

HERMALIN: No, I don't think so. I think that was a plan that Barney first developed to ask whether we can't get some measures of how well programs are functioning so that we can introduce it as a variable across countries. I think that development came mainly from the Population Council and Barney worked with Ron a great deal, and of course Bob Lapham came into the picture and worked with them as well, and Parker [Mauldin] was very interested in that. I was involved only occasionally as a somewhat friendly critic in the way the data were used and analyzed, and sometimes I was a

correspondent for countries I knew, as one of their informants.

VDT: As you say, you all were feeding into each other's research.

You did a review of the Caldwells' book in Population and Development Review [March 1987] and pointed out that the Caldwells said that in some of the population centers that Ford was funding because they were working in less developed countries the faculty did not go out to the field so much. Students were sent out there. In several instances, they sounded rather critical that the faculty did not go out to the field and stay, and I gather you didn't go for any length of time. And you sounded rather defensive in your review of their book. You explained that faculty had to stay home to mind the fort, etc.

HERMALIN: What I liked about the book and what I think that they did was make clear that there were certain structural differences. People who work in schools of public health, for example, often have the opportunity and are expected to spend long times in the field. People in other kinds of departments, in the schools of science and arts, so to speak, often have a different rhythm.

I did go to Taiwan almost every year at the beginning. I would teach for the whole academic year and then in May or June I would go out for four or five weeks, sometimes shorter stays. One or another of us went to Taiwan almost every year from when I joined Michigan in 1967 through at least to the mid-1970s or later. So it isn't a matter of fieldwork or not fieldwork.

We were working mainly with surveys. We could go out there, help develop the survey or do some training or look at the results or look at things in the field. Then come back, get the data, do some of the processing at Michigan or correspond about problems with Taiwan and work back and forth.

So it depends on the subject matter. I think with Jack and Pat Caldwell, their own style leads them to want to spend a lot of time in the field. They feel that that's the way they absorb the nuances of behavior, and that's perfectly fine and legitimate. I just had a feeling that they were perhaps thinking that should have been the modal way to proceed.

But I would argue that for the time, there was so much data to be collected via surveys and via other mechanisms that were also useful. If everybody had gone to the field and conducted community studies in those days would demography be as far developed along certain lines as it is today? I think that's an open question.

I just felt that they may have been pushing a form of research that they are very adept at, that they believe in, with less awareness of other approaches.

I think they're right in the sense that one of the things that distinguished Michigan to some extent was its long-term involvement. Ron always said, "We don't want to be a center that's one week in Taiwan and next week we're in Korea and next week we're in Thailand and grabbing some data here, or meeting there." It was a very conscious part of our program that we should develop long-term relationships with the country, with colleagues there, so they could know that we'd be there and that we would be developing a line of research and we would be helping train young staff. And we did that in Taiwan.

As a matter of fact, people used to come and say to Ron or to me, "What kind of contract do you have with Taiwan?" And it would often dawn on us that we had no contract. There was nothing but an understanding that we were interested in working with them. They trusted us to do things in good faith and with good sense. It was nothing more than an understanding that could end at any time. Ron always made clear that the data were theirs; they were basically paying for it; they had lots of control over their direction. If they wanted to talk to us, that was fine.

So we believed it was important to be there, to be in touch. We went; we sent students; we brought people there. But it was rarely an immersion type of stay. I just was suggesting that there is

more than one model.

VDT: You were saying before we started that you had been earlier this year in Taipei to give that talk at the meeting at the Taiwan Population Center?

HERMALIN: Yes, the Chinese Population Association.

VDT: So you still have very close ties.

HERMALIN: Right. In the last seven, eight years, I've been there every year.

VDT: You're still having students coming from different Asian countries to Michigan?

HERMALIN: Not as much.

VDT: That's what I wanted to ask you. Is it still important that people from less developed countries get training in the U.S.--at centers like Michigan--in demography?

HERMALIN: Oh, I think so. That's a complex question; I won't go into it too much because I don't know that I have all the answers. But I would hope that the field in general would think through now where we are in this area.

We have collectively been doing training on the predoctoral level, and to some extent with postdoctoral colleagues, over a long period. I think it's very valuable, particularly for the countries that are now starting to focus on the area, like Vietnam, who want to do a lot of work on their population, or South Africa and other countries in Africa, and perhaps some countries in the Middle East. I think there's still a great need, selectively, to help give a high level of training to people who will go back and become the nucleus of research--and perhaps program administration as well--in the population area.

Now, I think that has to be tempered with the fact that many of the countries that we once worked with now have their own Ph.D. programs, with high-level people who can do a lot of their training there.

VDT: Particularly in Asia, you would say?

HERMALIN: Particularly in Asia. There are Ph.D. programs in Korea, Taiwan, Thailand, that I'm aware of, and several others. For those, I think what would be more useful would be to think of postdoctoral stays, short visits, collaborations.

Today, with the nature of technology, it's easy to build up a collaboration with somebody where you can both have the same data; you can be in almost constant communication by e-mail or fax, not to mention telephone. So one can work jointly in a way that was very difficult for us in the early years. In the early years, the computers generally were in the U.S. A lot of the countries in Asia didn't have the capability even if they had the knowledge. So it was the sort of thing where you had to bring the data here; sometimes you could bring collaborators for a short stay; and bring results back there. And if you had problems about understanding something, you had to write long memos and wait on the mails. I remember long nights writing up consistency checks from early KAP surveys, sending back questions to Taiwan, waiting a month to get some answers from them as they went back to the questionnaires. Today the potential for working collaboratively in a real way, as equals, is much higher. I would hope that we would capitalize on that to work in a very full partnership.

So I see new opportunities at the same time that I see a need for a change--keeping some of the

old patterns, but altering them as appropriate.

VDT: In your presidential address last year, which was on the elderly . . . By the way, you had "Fertility and Family Planning Among the Elderly in Taiwan"; that was the original title. There wasn't any title in the program--the final program--and you're not the first president who has kept us all in suspense: "What's he going to talk on?" You made a little joke on it yourself: one plus one plus one equals zero--you can't have fertility among the elderly.

HERMALIN: Right.

VDT: You know that Population Today, PRB's [Population Reference Bureau] monthly, gave you the award for the best title [among presentations at 1993 PAA meeting].

HERMALIN: That's right; I liked that.

VDT: There you were saying there are new opportunities in demography for research on the elderly, which you are by now six years or so into. Is that because family planning program evaluators might be at loose ends, because the programs are working, contraceptive use is so high, fertility has come down so much in that part of the world? Or is it that you yourself are getting up to that time of life?

HERMALIN: I often wonder.

VDT: Or the world's population is getting to that time [aging]?

HERMALIN: The other version is interesting. I gave a talk once about some of the early work that I did on aging--a brown bag at the Population Studies Center--and I said, "Well, perhaps there's a direct correlation with your own age and the topics you choose." But then most of my audience didn't like that because they all had strange topics for their ages.

VDT: I asked Jane Menken that. Remember her PAA presidential address ["Age and Fertility: How Late Can You Wait?", Demography, November 1985] was more or less on women in the middle--the ones who have the elderly parents and the kids still in school--which was exactly where she was.

HERMALIN: Right. I think it's perfectly reasonable that your insights into issues--or your intrigue with them in part--grows out of your own life.

In my case, two things are worth saying. One, you should know that part of my time these days is spent very much on family planning evaluation, because I'm senior technical adviser to the EVALUATION Project that Amy Tsui is in charge of at North Carolina. This is a very large five-year project on developing new indicators and looking at the methods of evaluating family planning that USAID funded. We just started the fourth year this October. Amy Tsui is the principal investigator and I'm a senior technical adviser.

VDT: That's why you're adjunct professor at North Carolina?

HERMALIN: That's right. And I'll be spending the winter semester at North Carolina to work more intensively on certain issues that I still would like to pursue in that area. So I've not abandoned it by any means.

I think there's a whole story to be told about the history of family planning and family planning

evaluation and how it's changed. When I got involved with Amy there was a sense that I was coming back into family planning after some gap, which made it interesting to me.

As best as I can trace it, part of my intrigue with aging first came about out of my concern with fertility issues in Asia. That is, I started to say, well, many of the societies in Asia that I knew something about had these very strong family traditions, very strong expectations that older people and couples would live with their children, so what will happen to those arrangements or what pressures will those arrangements be under if families are reducing their fertility so much? And the limited question that I first started with was could it be that awareness of the fact that this might put in jeopardy some of their cherished family arrangements could be itself a factor for bumping fertility back up? We always have assumed that these things are unilinear and that they'll go in a certain way. But when we get to a point in any society in which fertility is under the control of individual couples there are things that can lead it to go up as well as to continue to go down, as you well know.

So I started with a fairly simple question about fertility and aging. And then, of course, that mushroomed into saying how do we look at aging in rapidly developing Asian societies. These societies are very interesting because they have reduced their fertility very rapidly, commensurately their populations are aging quite rapidly, and these are societies with strong views about family relationships that are quite different than most countries in Europe and North America, in terms of expectations of who you live with when you're older. So as this topic evolved, I saw it in broader terms: as an opportunity to study a very important transformation in its own right. We need to establish benchmarks of what exists now, and start to see how these societies evolve through the transition in their age structures.

So, that's how it came into being and that's where my interest has focused over the last several years. I remain very intrigued by it. There is continuity here with the thinking that Ron Freedman had in another way when Ron pushed for careful studies in Asia as they went through their demographic transition. He used to say, we can now study in detail what happens to a set of countries that we can only reconstruct in part for the history of the West. He felt that this was a very important historical opportunity. I feel the same way, in a sense, in this aspect of aging. So that's a good part of my motivation.

And also, I think as demographers, I've increasingly become aware that when population was growing fast, very quickly journalists used to say this is the population problem. And I think as I get older and see more variations in demographic patterns you come to realize that there isn't a population problem. There are dynamics in population structures and in vital rates that present challenges and opportunities to societies that they have to cope with, whether it's the size of their labor force or changes in the age structure. Japan worries about too few workers coming along and very low fertility, as does Singapore. And there's a lot of discussion about foreign laborers and immigration--these all get tied together. So there's a whole host of questions that tend to occur, and then sometimes to recede. And as demographers, I think we have to be aware to not treat any problem as the only problem or the one that will remain in existence for all time, but to be sensitive to the fact that these are the demographic environments in which societies have to carry out their work. As demographers we should help societies understand what limitations and opportunities these demographic changes and factors bring to them. So that's the way I prefer to see it.

VDT: That feeds right into my bringing up the topic of your putting your demographic research to policy use. You were the first winner [in 1988] of the Robert Lapham Award of PAA, which is given for "contributions that blend research with the application of demographic knowledge to policy issues," as well as "service to the population profession." You've obviously felt that was important.

The work in Taichung--well, the Taichung experiment fed right into the establishment of the family family planning program in Taiwan.

HERMALIN: Yes.

VDT: Ron Freedman said [in his interview] that he felt an obligation, of course, to work with the people who were setting up that policy. He didn't feel any divide between them. And that's been your philosophy?

HERMALIN: Well, I don't think I start with the premise that I want to do something that's going to change the world or change policy. I don't think I've ever looked for policy issues per se.

But, as you say, my previous comments feed into this in a way. I think there is a lot of connectedness between what we do in demography and how societies then use this information to organize programs and the like. I guess my preference is to find interesting and challenging problems that are not trivial, that are not too much a sub-specialty of something; I want the problems to have reasonable scope. And I feel that if they are of reasonable scope, then the policy implications or what they mean will be drawn out and can have influence.

But I have rarely taken my work and said, "This means that the government should set up policy A or organization B or so on." Sometimes when people who are in policy say, "Do you think A or B is a good idea?" I might be able to use existing science to say, "Well, our research shows that . . ."

Let me give an example from Taiwan, where in the early days of the program they were making IUDs available to hundreds of thousands of women a year. The question arose: how much effort should they make in going back to women who had used an IUD but gave it up or expelled it or had problems keeping it? They were about to mount a very big campaign of followup. But then because we had designed our research longitudinally, we could show that women once they adopted [an IUD] took care of themselves pretty well. Even if they didn't keep the original IUD, they got another IUD or they went on to another contraceptive on their own. Or if they did get pregnant and they didn't want the child, they would resort to abortion. So the fertility rate among women who had adopted was very low, which allowed us to say, "Look, since you have limited resources, you'd be wiser to put your money into attracting new users, making them aware of your services, rather than putting a lot of energy into your prior users."

We could use our science and our research to help advise them on decisions, but we were not designing the program for them or saying "Do A, B, C" on any regular basis. We were trying to be useful. We wanted to understand the program and learn what things intersected with it so we could give useful advice.

I like to see research used for making good decisions. But I sometimes worry about the word policy, because there was a time in the heyday of family planning programs and some other issues and concerns about population growth when people would sometimes say, "Well, I want to do policy research; I don't want to do real research; I don't want to do scientific research." And that always bothered me, because first of all, I think there's only one kind of research: there's good research and less good research. And I often would say to people who said that to me, "Look, if you're really going to use this stuff, you certainly want to be sure it's correct. Policy research cannot be second-rate research; there's too much more at stake here." If I make an error in some calculation for some idle project, who cares? If I publish it in a journal, somebody will find it. If I make an error and advise a government to spend lots of money foolishly or waste effort, that's a very serious matter. So, if anything, I want anything connected with policy research to be very carefully drawn and exceptionally well done.

So, yes, I like it when I can deal with a problem and the solution has some implications for policies and programs. But I'm more concerned that it be a worthwhile problem and that it be done right and then the policy will take care of itself.

VDT: Good. But you did inspire the PAA's Public Affairs Committee [established June 1979]. You wrote a great piece on that for the newsletter ["Formation of the Public Affairs Committee," PAA Affairs, Winter 1984]. That PAA should be more aware of and monitoring what was going on in Washington, and speak to policy makers. Why was that?

HERMALIN: That's a case of self-interest.
The story as I recall it was . . .

VDT: You found other scientists--the Endocrine Society--were much more on top of what was going on in Washington.

HERMALIN: Yes, I did that story.

VDT: Yes, I asked you to do it.

HERMALIN: I think I mentioned sitting next to Charlie Westoff [at a 1977 meeting of directors of NICHD-funded behavioral and biomedical population study centers].

VDT: Yes.

HERMALIN: There were several factors. There was the awareness that other societies, particularly the biomedical societies, were doing a lot more. And I felt there was a lot happening in demography and population research that we needed to get across, particularly within the National Institutes of Health where the behavioral side is a very small component. It was important that both the NIH as well as the appropriate congressional people understood what population and demographic research could do.

VDT: Plus preserving the money that might flow in this direction.

HERMALIN: And a realization that it was in our interest that these things be known. Once I realized that so many decisions were made from groups trying to say why their work was important that it certainly behooved the PAA to make clear the good work that our membership was doing.

There was some reticence. I think the PAA at first was very conservative. It was so desirous of being purely a professional association that the idea . . .

VDT: Which as you pointed out dated back to the days of Margaret Sanger, who helped found the organization, and she was a birth controller and they weren't going to muddy the pure waters of scientific research with such activism.

HERMALIN: Yes. One of the things that crosses every professional organization is the concern that if you seek some public outcome does that mean that people will be taking sides on public issues on which people of good will can differ. So there was always a lot of nervousness that anything along these lines was going to lead us into public stances as an association and be potentially divisive.

All in all, it was a little bit the right idea at the right time. I think we had gotten over that early nervousness; there were enough new people coming into the profession to help us understand that we could go forward without compromising our professional standards. And that led to the willingness to take some chances, as we realized that it was very much in our interest, that as a profession we needed

to have a range of data and the availability of funding and the like.

So, as I say, it was mainly reviving or planting this idea as we were coming into a stage of maturity where people would take up the idea. I'm very pleased that it happened and that I could play some role in it.

VDT: To leap forward and finish up on that. You were very instrumental in recently setting up a public affairs office for PAA, bringing in Anne Harrison Clark, who had been at the Population Resource Center at the beginning--to be, well, is she half-time or full-time now?

HERMALIN: She's still part-time.

VDT: And another person full-time--she's just left--along with the Association of Population Centers.

Etienne van de Walle, the president before you, said you were a very active president-elect in setting that up. He was not; he was still a little skeptical--didn't like to muddy the waters of pure scientific research. But he realized its importance. And you do too, obviously.

HERMALIN: Yes. It was kind of a full circle for me, because I helped set up the original committee. I chaired that ad hoc committee [Ad Hoc Committee on the Monitoring of Federal Activities Affecting Demography, formed in March 1978, leading to the Public Affairs Committee, with Anne Harrison Clark as the Washington representative] that made the recommendations. And then when Anne came on board the first time, we were very fortunate to have her and it worked out very well, sharing Anne with another organization [Population Resource Center]. So we knew Anne.

VDT: You didn't share her in the beginning. Well, she was hired by Population Resource Center.

HERMALIN: Sharing in a sense; she was working there and we were helping to support that activity. We looked for a partner that we felt would have the right stance to initiate that work. Then, as president-elect, there was a need again to look at the structure of the office and what we could afford. And the Association of Population Centers had come on the scene.

VDT: Did you help form that, by the way?

HERMALIN: No.

VDT: I want to go back to the Michigan Population Studies Center. You were the director from 1977 to 87, during which time the budget went up from \$600,000 to close to \$2 million. Did you put it more on the map than it had been, or what?

HERMALIN: Well, it did grow. But that was a period of growth for many centers and ours as well. I think the growth came in part because some of the research grants moved from being small and more tentative efforts to larger enterprises as we showed the benefits and need. We were also able to get more core support from the government than we had had before as some of those needs became clearer. There was a need for more computer expertise to move into the new era, for example.

We probably did have some net addition of people over that period which led to their activities being included as part of that portfolio.

But it was a period of growth. There was a lot of demand for training and we had money from the Hewlett Foundation for LDC training and we had the Mellon program, a special post-doc program. So there were a lot of different programs coming together. The core support from NICHD for both

training and the operation of the Center; research grants; support from Hewlett and from Mellon, which was relatively new. There was a burgeoning along lots of fronts, which I think led to that numbers increase.

VDT: Even though Ford monies fell away.

HERMALIN: Yes.

VDT: Who have been the leading influences on your career?

HERMALIN: Depending on the nature of the influence, as I said, Marvin Bressler is a big influence because I think it was his model of a learned and engaging professor dealing with important topics that helped make me decide to go to graduate school and seek a degree and career in academia.

VDT: You were looking specifically to be a professor to teach?

HERMALIN: Yes. That's another one of the ironies I should get on record. We have talked about the happenstance of how you take one road or another.

My image of academia was quite naive. I was working full time and I didn't know a lot about what was happening in academia. But when I took these courses at NYU, my thought was about teaching. I remember saying to my wife, "You know, I'm quite sure I could be a good teacher. I've done enough mentoring and explaining and informal teaching that I think I know how to do it. And it would be fun to go to a small college and teach courses and help young people understand some of the potential in sociology and social research." I really had a vision that was very different than that of an active researcher or a research-type academic career that I ended up with. That, again, was just an accident.

VDT: What do you consider more important in your career--the research or the teaching? We haven't talked at all about the teaching.

HERMALIN: I think the teaching is very important. I care about it a great deal. I think almost all professors have a kind of love-hate relation with teaching. We hate the relentlessness of it; you have to be there every Monday and Wednesday and Friday, and you have to be ready for it. But I think most of us--at least, I, once I'm in the classroom, tend to be very happy. I like to engage in the ideas; I like the thought that I'm getting across something useful; I like talking to the students informally as well as in the teaching relationship. So I've gotten a lot of rewards out of teaching, as well as lots of funny stories about what goes on in the classroom.

And, of course, at the Population Studies Center or in an academic environment where you're doing graduate training, a lot of your teaching comes by way of mentoring and working with students on your research, on their research, and seeing them through their dissertation.

I looked the other day at my shelf, because I had to find space, and counted up all the dissertations that happened to be in one place and I must have been now on well over 50 dissertation committees. And I've chaired a good many; I've probably chaired as many as anybody in the Pop Studies Center or as many as anybody now in the sociology department. I do take pride in that. I think that's important.

And to me it's also important that a lot of my students were students from developing countries who needed a lot of intensive care, so to speak, and help in the ways in American universities and in the ways of research, which were somewhat newer to them. I felt that was an important contribution.

So, I think that's been an important part of my career.

VDT: Well, we'll get on to your outstanding students. But now, influences. There was Marvin Bressler who showed you that it might be fun to teach--go into academia.

HERMALIN: Right. Then, of course, at Princeton there were a number of very good professors, in terms of the different sociology courses and seminars we had in theory and other topics. But, of course, as a demographer, the important influence was Ansley Coale, because Ansley taught the one-year sequence in population at that time and that was the sum and substance of the formal teaching then. It was our building block, and one couldn't have had a better building block in terms of formal insights into demographic processes and how to think about them than through Ansley. So that was a very important influence.

And it also was important in the sense that Ansley was not only a great formal demographer but he was doing the work on the European historical project. So it wasn't that he was not interested in substantive issues. He was--as I said--always coming in and reporting on new things. He had a very keen appreciation of the research process. He taught us to be skeptical about data--you know, that hallmark of demography of looking carefully at data. It was very well inculcated by Ansley in terms of both what he did and what he taught us. So, you could hardly think of a better grounding in the field.

Which has stood me in good stead. I taught demographic techniques for many years. I used to rotate the graduate course with Ren Farley. A lot of the insights I had into ways to get the material across built on that very good sequence. So that's a very important influence.

And, of course, coming to work with Ron Freedman was very important in my career, both in the sense of joining Ron in a very explicit project in which we were both very much engaged. He was a wonderful person to work with, first, because he treated everybody, including me, with such respect. He wanted me to feel like a full partner in the undertaking. I knew that if I had suggestions that they would be given attention and listened to.

But I also learned a great deal from Ron because he's one of the wisest people you could ever run into. He had great insights into almost everything you did. First in how you handle the complexity of all these relationships and prepare to launch a large project. He was well aware of the snares that arise in the course of research or in the course of negotiations. So you learned a great deal about that aspect.

He was eminently sensible. Of all the things that could be done, he always kept his eye on the important things and didn't get sidetracked in ways that could have been interesting but I think would have been much less productive in the long run, in terms of telling a story. As an example, it was Ron who realized that since we repeated these KAP surveys [in Taiwan] with some frequency, we could start to give a trend line to what was happening in a developing country and how these things were emerging. That seems obvious--and in one level it is--and yet as Ron said the other day, "I don't know any other country that has developed that body of data and the ability to fashion such a long, continuous series on some very key measures." Those trend articles became very important.

VDT: You're speaking of the sequence of KAP studies in Taiwan?

HERMALIN: Yes. But that was just something that was his good sense. It wasn't that we went out to do it, but we realized that we had these surveys and it was very important to tell that story in this way.

And the fact that Ron was such a good sociologist that the KAP surveys were much more than that. KAP is a misleading title: knowledge, attitudes, and practice about family planning. Many KAP

surveys around the world that I know about--and started to collect in that enterprise that you mentioned--were really very bare bones. People learned about the fertility history of the person, and the knowledge, attitudes and practice, but there wasn't too much on all the social and economic dynamics that one also needed to understand. And our surveys became richer and richer over time.

VDT: You worked on contextual variables.

HERMALIN: And we learned a great deal about what brought women into the labor force and whether they kept the wages themselves or gave money to the family and their relationships with their families. There's a book coming out, supposed to emerge from the Chicago Press just this week, that builds largely on the social and economic data from the surveys.

VDT: Which you're involved with, with Arland Thornton.

HERMALIN: Yes, Arland is the [co]editor.

VDT: Social Change and the Family in Taiwan [Arland Thornton and Hui-Sheng Lin, eds.].

HERMALIN: Arland is the major editor on our side of it. It's got plenty of fertility in it, but it's very much a social history of Taiwan, as seen through all of these KAP surveys. One reason we could do such a rich story is because the KAP surveys themselves became such broad studies.

VDT: I thought there were going to be two books on the Taiwan experience.

HERMALIN: At one time we thought of a second book that would be much more focused on the technical aspects of the fertility change. That's kind of in the abeyance at the moment. This is the one we focused on.

VDT: When I interviewed the Freedmans in 1989, there were to be two books: this family book, because Arland was so interested in the family, and then the other one. But the other one is in abeyance.

HERMALIN: Yes. I was supposed to do more on that but that collided with the work I was starting on aging. It wasn't clear where all the time and funding would come from for that second book, so we held up for the moment.

Arland became involved with Taiwan ten or more years ago and contributed to the depth of the surveys that we did after he got involved. So there have been a lot of important hands in contributing to that wealth of information.

It was a cumulative process. Ron sensed the potential in adding these important dynamics to the questionnaire in order to understand the situation. He had great good judgment in seeing the broad picture and seeing how to get the important components of it accurately, and not going down fruitless paths or paths that might be too narrow. These might be interesting but not productive in terms of some of the larger issues at stake.

And, of course, his great knowledge of the survey process--how to think about a questionnaire and how to carry it out and the high standards of quality that are so important to the validity of the results.

So, all of those things came out of working with Ron--and lots more too.

VDT: Any other influences? Talk about Barbara Entwisle. You've published a lot with her.

HERMALIN: Yes, I publish a lot with Barbara. Let me use this as an opportunity to say that one of the things I've enjoyed is working collaboratively with a lot of different people. It makes a lot of the problems that much more attractive and opens up the range of things you can do.

Barbara is an example of somebody that came to Michigan as one of the Mellon fellows, when we had that program from the Mellon foundation. She had her degree from Brown. That was the time that Bill Mason and I were doing work on formal multilevel modeling, and starting to apply that to the World Fertility Survey. We involved Barbara in that work and she became a full partner and participant. Barbara and I have worked on and off on different problems for, I guess, well over ten years now.

VDT: You have a number of joint publications, and also with Bill Mason.

HERMALIN: I went to a meeting in Bogota, an IUSSP meeting, when I was in charge of the family planning section [chairman, IUSSP Committee for the Analysis of Family Planning Programs, 1978-81].

VDT: And there you met her?

HERMALIN: No, it was after I came back; Barbara wasn't at that meeting. I was editing the proceedings of that conference [The Role of Surveys in the Analysis of Family Planning Programs, 1982] and I wanted somebody to work with me and turn it around quickly, and Barbara had either just joined us or been there for a while. So she and I edited that book for the IUSSP.

Up to that time, a lot of the questions about the availability of family planning were asked of the respondent: "Do you know of any place nearby" and "how far is it," and things like that. People were asked a lot of questions about the location of things but there was very little obtained independently of the respondent. Barbara and I realized, as we worked through the papers, that that introduced some problems--if somebody wasn't using family planning they might not be able to tell you anything about it; a person who was using a certain method might only be able to tell you about the place that she received that particular method--and that it was very important to collect some information, global data, about the community and about the environment of family planning that did not hinge on what the respondents told you.

So we had an opportunity to work with some people in Thailand who had collected exactly the kind of information we advocated and published a paper with them in Demography [Entwisle, Hermalin, Peerasit Kanuansilpa, and Apichat Chamrathirong, "A Multilevel Model of Family Planning Availability and Contraceptive Use in Rural Thailand," Demography, November 1984].

I think we started to influence the way of thinking about this kind of data--that one needs contextual data along with the individual data.

In Taiwan, technically we had contextual data along with individual data, but I had been doing analysis in two different streams. I'd been analyzing individual data and then for my family planning evaluation work I had been making use of the small area data from the 360 townships into which Taiwan is divided. I used the social and economic aggregate data along with the family planning data to show--as I said before--the relative importance of family planning vis-a-vis social and economic dynamics. But with the multilevel work, we started to put the two together explicitly. The World Fertility Survey collected some community data and then the Demographic and Health Surveys came along. And I think we had some influence in helping make sure that good data about the family planning and health environment was collected by people who went out and looked at these facilities

and measured where they were and did not rely on faulty, or potentially faulty, data based solely on respondents. So that was one line of work that Barbara and I and Bill Mason did to some extent through the 1980s, starting with the World Fertility Survey and moving on to some other contexts. And Barbara and I continue. We gave a paper at the PAA meeting last year [Entwisle, Hermalin, and Zeinab Khadr, "Delivery of Family Planning Services in Rural Communities (Egypt)," presented at the PAA annual meeting, Miami, May 5-7, 1994]. We're in the process of revising that for publication. She's been a very good colleague.

VDT: What about some of your outstanding students? You mentioned that you have been on the dissertation committees of over 50--many of them from developing countries.

HERMALIN: Yes, many of them from developing countries. I don't know what the proportion would be.

I think the people best known in the field whose committees I recall being on would be John Casterline, Rob Mare, Herb Smith, Mark Montgomery--in economics; I was the outside member there--and Judith Seltzer.

It's always fun to work with outstanding students. You learn a lot from working with them and you hope you have something to offer. But very often you're mainly serving as a facilitator of their ideas or helping them over a few rough spots. They generally know what they're doing.

I was thinking about this and I think there's a difference. When you have a very strong student who knows where he or she is going, there's a saying that the best you can do is stay out of their way. But with every student, you hope that you're making some contribution, facilitating their progress and helping them learn the ropes. And, of course, in other cases you're often doing much more explicit teaching and mentoring and helping them pull things together. Both areas of work are needed and rewarding. So it's fun. With dissertations, you usually get immersed enough in the topic that you learn a lot.

VDT: Have most dissertation topics of the students you happened to mention been data from Taiwan?

HERMALIN: No, though I've done a lot of dissertations based on our Taiwan data. One of the contributions of Taiwan has been to generate dissertations of many kinds.

John Casterline's was on Taiwan--on the changing pattern of marriage. John Anderson, who went to CDC in Atlanta, did one on the role of education in changing fertility levels but more on the shape of the transition in fertility. Other people studied certain aspects of the program. One of our Taiwanese students, for example, looked at the characteristics of the family planning workers and how much that made a difference vis-a-vis the things that they did in terms of their actual duties.

So there was a whole range of work--some of it close to family planning; some it much more general fertility, nuptiality, other aspects of the area.

We used to keep track of how many dissertations came out of Taiwan; it was an impressive number but I can't recall it.

VDT: What do you consider your most important publications, and why? Now, you've coauthored almost all your publications--an enormous number.

HERMALIN: Let me preface the answer to that. Part of the joy of being an academic is problem-solving. You have to like to solve problems and research is, to me, a problem-solving activity.

And the problems come in different forms. You can get a short burst of insight that's very exciting. You think you see something and you get very excited about it; it keeps you up at night and

you're willing to work through the night or do something equally excessive. Those are a lot of fun and they are part of what you look for as one method of reward of being an academic. That happens too infrequently in these days of being busy on a million things but at times you remember. And I think there have been a couple of things that I enjoyed regardless of their influence on the field that had that kind of impact on me--that I enjoyed, that I felt were important.

And then there are other more cumulative lines of research. I started this work, as you alluded to, on assessing the role of family planning programs vis-a-vis social and economic change. And there's been a long series of articles, or reasonably long, in which I developed this idea or carried it forward, and I think it's had cumulative impact and I'm proud of both the findings and whatever ground it broke.

So you care about both things, but they come in different flavors, so to speak, in your own history.

I was very pleased about my dissertation, that we talked about, about the siblings ["Homogeneity of Siblings on Education and Occupation"], because I felt it really was my idea; I dug out the data; I thought it was a very important problem; I was pleased with the techniques I developed to carry it through. It's always been close to my heart, so to speak, regardless of what I've done with it, and I've been pleased to see that some of these themes have emerged--not in any genealogical sense--and have come to be recognized as important.

I'm pleased with the cumulative work I've done in evaluation of family planning. I think I helped at the time when I edited some of those books that you alluded to--the one with Chandrasekaran [C. Chandrasekaran and Hermalin, eds., Measuring the Effect of Family Planning Programs on Fertility, IUSSP, 1975] and subsequent volumes.

VDT: You had not long been a member of IUSSP when you edited that book.

HERMALIN: Right. That was one of the early clarifications of the field, in terms of saying what are the different methods available to us, how do you understand the basic strategies, and so on. And I developed that in papers in some subsequent UN volumes. We used to have expert group meetings sponsored by the United Nations fertility section. There must have been three of them, every two or three years. They were held in Geneva when Gwendolyn Johnson [Acsadi] was head of the fertility and family planning section at the UN.

Those were interesting because they brought together many of the experts in the field and tried to take stock of what we knew: how do we know whether programs are making a difference; which are our best methods for testing these things. Those were challenging meetings, in which one tried to write broad papers about the state of the field and what was going on. You'll see a few of those on the list [of Hermalin publications], starting in the late 1970s. For example, "Some Cautions in the Use and Interpretation of Regression Analysis for the Evaluation of Family Planning Programs," in the book Evaluation of the Impact of Family Planning Programs on Fertility Sources of Variance [United Nations, 1982]. There were three UN volumes that came out of those expert group meetings that I think were reasonably influential. Another one was "Using Individual and Areal Data in the Evaluation of Program Impact on Fertility," in Studies to Enhance the Evaluation of Family Planning Programmes [United Nations, 1985].

VDT: You were the single author there.

HERMALIN: I mainly was, on those articles I was doing for the UN.

I'm pleased particularly--as we talked about a moment ago--about being one of the people to have drawn more attention to multilevel analysis and its potential. Certainly, that's not something I did

by myself, but I was pleased to help spread the word, so to speak, on that approach.

Among the articles that I particularly remember enjoying was one with Anrudh Jain that came out, with the cooperation of T.H. Sun in Taiwan, as "Lactation and Natural Fertility" [in H. Leridon and J. Menken, eds., Natural Fertility, IUSSP, 1980]. This was done for an IUSSP meeting around 1979, in which Anrudh gave the paper. This was one where there was kind of a flash of insight. The excitement there was the fact that we were modeling what was the effect of breastfeeding and postpartum amenorrhea on the birth intervals in Taiwan for this IUSSP seminar that was going to be held--I think it was in France--and we were using path analysis; I can't remember all the details. Some of the results we were getting didn't make sense in the way we set up these models, and the only way the model would be consistent was if we assumed that breastfeeding delayed or reduced the chance of conception even after a woman resumed ovulation. That was a daring assertion at the time. There was, as far as I know, very little biological evidence of that effect. But it appeared so clearly that that had to be happening if the data were to make sense that we asserted in the article that that's likely to be the case. And I gather at the seminar--I wasn't at the seminar; Anrudh gave the paper--it caused quite an uproar. Then later there was confirmation both from surveys that were able to study the situation prospectively and the biological evidence. So that was exciting. This was one of those papers where there was much more a sense of discovery, where you're saying, "Ah ha! This has to be the solution." Then you get excited and test it out.

So there are times when you come up with that sort of thing. I fondly remember that paper because it was so controversial and yet seemed to pick up adherence over time. Anrudh and I still sometimes send each other little clippings we find that confirm that this was on the right track. And people who work in this area discover this paper sometimes and send me an article and say, "Oh, I came across this; you might be interested in what I'm doing in this area." So a lot more has happened, but we clearly were onto an important topic and I think we did set the right tone, even if we didn't do everything definitively. So you remember that kind of article fondly.

Also working with Anrudh Jain, we did some mathematical modeling of cumulative fertility, again for an IUSSP meeting. It was a London General Conference in the late 1960s or early 1970s [Jain and Hermalin, "Fecundity Models for Estimating Fertility Over the Reproductive Period," Proceedings of the International Population Conference, London, 1969, IUSSP]. We were very pleased with the inherent modeling and I think, again, our approach was later confirmed with more sophisticated work. So you remember those with fondness.

As I say, I think it's a mixture of things. You want to be pleased and engaged with your agenda of research and think it's worthwhile. And within that, you look for things that sometimes are inspiring and get the adrenalin going and keep you going late into the night.

VDT: An aside on the IUSSP. You feel it's important? You've been involved in many committees, meetings.

HERMALIN: I was pretty active there for a few years; I guess it went into the early to mid-1980s.

Let me put it in context. Compared to many international disciplinary associations, I think the IUSSP is a much more real presence, bringing people together for seminars, colloquia, and issuing books. It has a real agenda that serves the field well--particularly in giving opportunities to younger scholars. At the Population Studies Center, as you noted, we work a lot with overseas people, but for many demographers who don't get opportunities to work with people from all over the world certainly the IUSSP serves that purpose, through their committee structures and through their seminars.

And, of course, the General Conference itself. The General Conference can be improved, but as a general conference it is much better structured and better organized than a lot of these international meetings where some groups come together a year or two before the meeting and try to assign duties

and find a place. I think our international meetings are well planned in general and bring out a wide number of people from all over the world. There needs to be quite a bit of fine-tuning on how the content of the meetings is arrived at and how one is selected to give a paper and what appears in the official proceedings. But I think certainly it is a meritorious organization.

VDT: Etienne van de Walle thinks [in his interview] that the PAA annual meetings are the important international meetings.

HERMALIN: They really are.

VDT: He thinks they are far more important than the IUSSP every-four-years conference.

HERMALIN: Talk about the PAA, I was just noting with my colleagues yesterday at the board meeting that the proportion of the people attending the PAA meeting that come from abroad is increasing, as best I can tell. I'd be surprised if we don't have 10 percent of the attendance coming from outside the country, besides Canada and Mexico. It has quite an international flavor, not only in the topics but also the number of people involved. The PAA supports a number of our international members to attend and that helps a great deal.

VDT: Thanks to you--the increased Mellon and Hewlett support [for that international outreach].

I want to get on to PAA but I do want to ask you that big question: What do you see as leading issues in demography over the years you have been involved and for the future? Can you state that in a sentence or two?

HERMALIN: I guess my wisdom, such as it exists, goes back to an earlier thing I said. I think we have to see the population component as a set of interrelated population issues that come forward at different times and in different ways to different countries. And I guess the major implication to me, if you take that as a given, is that there'll always be many issues before us, hitting different countries at different times. We have to get more adept at seeing the world that way. Which means we have to be more efficient in how we collect our data, not to be so single-minded and to see that data have to serve multiple issues and multiple needs. We have to realize that there may be little justification for a separate migration survey or a separate living-standards survey and so on. A lot of these things can be accomplished while improving many goals. That is, if one combines scarce resources you probably can get better data that serve all needs together. And I would hope that the next era of data collection and thinking would move in that direction.

VDT: Good point.

Now on PAA. Do you remember the first meeting you attended? Here's a list of them.

You must have been involved before you went to Princeton because Harriet Presser in her interview--interestingly, Harriet worked with you at the Institute of Life Insurance--said that you encouraged her to go to the 1963 Philadelphia meeting, and that was before you went to Princeton. You went to Princeton in the fall of 1964.

HERMALIN: I went to Atlantic City, I know, in 1969. Let's see. There were a lot of constraints in those days. I remember not going to Cincinnati [in 1967]; I forget why that occurred.

I once attended a meeting in Princeton; it could have been in 1955.

VDT: That was the last PAA meeting at Princeton. How come that might have happened?

HERMALIN: Well, because I was working at the Institute of Life Insurance. I was quite active all during my time at the Institute of Life Insurance in the American Statistical Association. And one of my colleagues at the Institute was an economist and we thought that some of the things the population people were doing were relevant and that we should know more about their work. So we took the train down to Princeton, to the 1955 meeting, and just showed up.

VDT: I have to make a little aside. My husband and I spent a night of our honeymoon in 1952 at Princeton, because I had a brother there, and his wedding gift to us was a night at the Princeton Inn. I have figured out that it was about two or three days before the PAA had a meeting at the Princeton Inn!

HERMALIN: I remember that meeting [of 1955]. It was somewhat of a strange feeling, because we clearly had the feeling that we were outsiders; people wondering why these two people from the insurance industry were there. It was a very small group, as I recall.

VDT: There's no attendance given for that meeting [on the list], but it must have been indeed a small group.

HERMALIN: I guess that was an omen of what was to come.

VDT: What do you remember about your early meetings? You know for sure that you went in 1969 to Atlantic City. The president that year was Otis Dudley Duncan.

HERMALIN: When you have just finished being a student, or when you are a student, I guess the main thing is getting to meet people and talk about some of the things going on.

I had left Princeton by then, but I think it was planned two years or so ahead, because I remember while I was at Princeton, OPR had some of the responsibility for helping with that.

VDT: Yes, indeed. OPR was the closest demographic center to Atlantic City.

HERMALIN: So I remember some planning for it while I was at OPR. I may have even gone down once when we looked at hotels. It was a small amount of involvement but there was excitement in setting up the meeting.

Other than that, I just remember trying to get to know people and identifying people you had read and things of that sort. I remember it was in one of the big old hotels there--the Traymore or one of those boardwalk hotels that have been demolished. I remember we had time to go swimming because I remember challenging the waves with a few other members. That is about my recollection.

VDT: What else do you remember about early meetings? By then you were at Michigan.

HERMALIN: The thing that I think distinguishes PAA [meetings], that I hope will continue and increase, is that you quickly felt at home. It was not the kind of meeting where you spoke only to a small handful of people you knew. People were very friendly. It was a relatively small fraternity of people. The numbers built up so quickly [thereafter].

VDT: Atlantic City already was 486. Miami, the one just past [1994], was 1,185. Your 1993 meeting, in Cincinnati, to everybody's surprise, was the second largest turnout--1,216.

HERMALIN: So it's about tripled in size. Of course, the fact that I've gone to almost every one in that period, means you build up friendships and acquaintances with so many people.

But everybody remarks and I always tell my new students when they're going to their first meeting, "You'll enjoy this; people are very open. Just talk to everybody and by the third meeting, you'll know quite a few people." I think that's a very important aspect of our association. If I went to the American Sociological Association meetings, I will certainly know the population and social demographers, but I could spend many hours of the day at the American Sociological meeting--just because it's so big--and not bump into anybody I know. Whereas, if you developed a kind of demographic measure--how many minutes you have to walk around the registration area or the hallway before you bump into somebody you know or that you want to chat with--I suspect the PAA has a very short time compared to the big meetings of the sociologists or the statisticians or the economists and psychologists. So, as I say, even though the meeting size has roughly doubled or is two-and-a-half times in that period, I think we retain that character. You see a lot of people talking to each other and you don't see so many people who seem to be isolates. That's always been a very pleasant part of the meetings for me.

VDT: Do you remember the issues that PAA was involved in in the early 1970s--the women's concerns, the Concerned Demographers, the move to try to get the organization to take political stances, like abortion? Were you involved in any of that?

HERMALIN: No, I don't remember that period in detail, except in so far as it intersected a bit with the work of the Public Affairs Committee. That was later.

VDT: Let's jump up to the mid-1980s, when you were on the board [1982-85]. Think of the issues then.

HERMALIN: Oh, there was the issue of who was going to edit Demography and what kind of journal Demography should be and people having views about that. The issues of where we should hold our meetings and the like. I think there were residual issues of taking stances. There still was the feeling that we should not get drawn into public stances that might be divisive. We were concerned, of course, that everybody would feel welcome wherever we went for our meeting.

But things were humming reasonably smoothly, as I recall.

VDT: Let's get up to your time. I've already mentioned that Etienne van de Walle said you were very active as president-elect [1992] in setting up the public affairs office.

Now on to your year as president [1993]. You instituted some interesting changes in how the meeting program was done. Rather than specific sessions in the Call for Papers, you had specific sessions and general topics in 14 broad categories. Why did you think that was important--to do it that way?

HERMALIN: We tried to take a fresh look at the way the program was structured. We started with the knowledge that Etienne [president in 1992] had been very successful in getting lots of sessions into his meeting.

VDT: 95--the number was creeping up every year.

HERMALIN: Yes, Etienne had many sessions, and we felt that was an important component in increasing attendance. And we were also aware that a large attendance was very important for the

financial health of the association. So, I took it as a challenge that we had to get a large attendance. And we were nervous about Cincinnati [the location], so we wanted to get a lot of people on the program.

I had followed the previous arrangement of calling for suggestions and asking whether people wanted to organize a session. And people sent in their forms and said, "Here's a session I'd like to organize." That was the way the program used to be arranged, building on sessions suggested and then having members submit papers to those sessions.

And we [the program committee] realized that that was unduly confining as the interest of the membership was getting broader. It wasn't that these topics weren't important, but to try to tell all the people working on fertility that they had to come up with a paper that fit three or four or five pre-arranged, pre-titled sessions was getting a little bit out of kilter. It didn't make sense, because people would say, "I have a good paper but I can't find where to match it up." We were frustrating the talent of our organization by not having an outlet for people with good papers, saying, "We only want a paper this year if you're going to talk about fertility in southwest Africa" or fertility as it applies to the role of husbands, or whatever. All meaningful, important titles but not necessarily the beginning and end of a topic.

So we toyed with the radical idea of doing away with all pre-arranged sessions and just saying, "You submit papers and we'll organize them for you," and people got a little nervous. So we came up with this compromise and said, "Let's keep the more interesting sessions, where we want to make sure that session exists and where there will be a good cadre of people out there who are likely to have been writing on this topic. Let's have a small number of those. But then let's open it up and just say, 'If you have things to say on nuptiality or labor force or whatever, send us the papers. If they meet our standards of quality we'll organize them and get you a session that makes sense.'"

We were confident that we would still get reasonably coherent sessions, but we didn't believe that we could decide beforehand what those coherent sessions should be.

It was a semi-bold experiment. It had all sorts of little logistical niceties. But all in all, it worked out quite well. We had the largest number of sessions up to that time.

VDT: 101.

HERMALIN: And we had a very good turnout [1,216].

VDT: Especially for Cincinnati.

HERMALIN: And the most important thing was that we received an overwhelmingly positive response. It clearly was a good direction to go and a sensible step. I don't think we received a single demur.

VDT: I have to quote to you what Jen Suter [PAA Executive Administrator at that time] said just last week; I phoned her up. She said, "The best year I had at PAA was the year I worked with Al Hermalin. He is a caring man. He was anxious to have everyone involved in the annual meeting program--especially students. And that is my [Jen's] philosophy too."

Was that in part it? Not just to get the bodies there because the more registration fees the more money.

HERMALIN: There were multiple motives, obviously. We did take to heart Etienne's advice that you want to have a large number of sessions, and that is a major force in bringing people to the meeting and one always has to realize that. But the main thrust was that we were working with a

model that was getting out of kilter.

VDT: It must have worked because Dick Udry [1994 president] followed up this year, with even more sessions, and he also had a Call for Papers that just said, Send them all in and we'll put them in these 17 categories--and there were 131 sessions.

HERMALIN: We just heard yesterday--the first tentative numbers, they haven't finished yet--that we might go as high as 139 next year [1995].

VDT: That's crazy! How can people do it when there are 10, 12, 15 sessions overlapping?

HERMALIN: There is the danger of gridlock. There are two things that are helping. One, some of these papers are done in poster sessions and nothing else goes on. And the poster sessions seem to have worked quite well last year [1994].

The other thing is that we will still try to avoid obvious conflicts, but there will be pressure on people to choose. They'll have a choice of 14 sessions at any given time and that does frustrate one. We do hope by indicating when the given papers are on--and we do insist on having one hotel so that people can hear the first two papers of session one and still catch the last two papers of another session going at the same time--that we are giving more choice.

The problem with this increasing number of sessions and the, hopefully, increasing attendance, is that given the long lead time for organizing meetings [reserving hotel sites], we are locked into the number of days that we can have a meeting. One logical approach if you have this many sessions is to start a day early and reduce the overlap. We just have no choice because we've signed contracts for so many years in advance. So I think we'll have to think that through.

But you learn very quickly. Linda [Waite, 1995 president] reported yesterday, for example, that nobody liked evening sessions. They tried some sessions in the evening and people said, "No, we just want to be with our friends; we want to go out to dinner."

VDT: Except for those workshops on Thursday evening.

HERMALIN: Yes, some workshops occur. So Linda is saying, "I'm not going to have any evening sessions."

So you learn. People did not like not having lunchhours [at 1994 Miami meeting], so there may be a variable lunchhour but there'll be at least a 45-minute break every day, so people can get together and so on.

You learn very quickly what works and what doesn't work. The learning curve is very steep and our membership is not bashful and they'll tell us what they want.

On the other hand, they seem to respond very positively to the fact that if they send in a paper that's meritorious, it will appear on the program.

VDT: And you think in the past that was not always true?

HERMALIN: No, it was turned down often because it did not fit.

If you had pre-arranged sessions and, let's say, you had four sessions [on a topic] you knew that you could accommodate no more than 20 papers, so if you had 40, 50 papers [on that topic] there was nothing you could do. Sometimes, you formed late sessions.

VDT: Harriet Presser [1989 president] did. She said she had them on Saturday afternoon; they were

the spillover sessions.

HERMALIN: Now we just go all the way through Saturday afternoon for regular sessions. People accept that. The meeting goes from Thursday morning through Saturday afternoon. And Saturday evening--if they're staying over, as many do because of the airline ticketing system [cheaper return tickets if stayover includes Saturday night]--we try to have a social event that people can do together, to get to know each other better. Dick had that boat ride last year [1994] on Saturday night, in Miami.

VDT: Oh, he did! [Jean was not at Miami.]

HERMALIN: Yes. Knowing that people are going to stay over, we're thinking what can we do, as an association, to plan something that will be fun and interesting and people will get to know each other better. I think we'll have that as a feature as long as the current commitments exist for these durations of meetings and this airline system.

VDT: Well, while the sessions are going up and the number of papers accepted is going up, the membership is going down. Let's get onto that painful subject.

The interesting thing to me is that in the year [1990] that we shifted to having our own office and the dues went up from \$45 to \$70, the membership went up. It reached its peak at the end of 1990. Everyone had said that with a dues increase, the membership will fall. But it did not. It peaked at 2,752 at the end of 1990. Unfortunately in your year [1993], it dropped quite a bit. At the end of 1993, it was down to 2,267.

HERMALIN: They gave us a number yesterday; it's back to around 2,600 [2,596 as of October 1994].

VDT: You think that was because everybody was alerted to the problem?

HERMALIN: Yes. There are a number of problems. I think, to put it in context, a couple of things happened. One, as I wrote in PAA Affairs at one time, there was so much happening when we moved to be masters of our own fate: becoming a stand-alone organization, not being served by AStatA [American Statistical Association], and being in the same building as ASocA [American Sociological Association] but really running our own operation. There was a lot to do. And I think probably in the midst of that, there was a bit of slippage. Part of it was that probably we weren't as vigilant in making sure that everybody who gave a paper at PAA was a member. Most associations, unless you're an invited outside guest, say, "This is our meeting and if you want to give a paper you should be a member." I'm not sure we were able to enforce that--not out of any oversight but there was just so much going on in that period. As you said, we were setting up our own public affairs office; we were doing everything.

VDT: It was two years later, in 1993, that members dropped most. The office was set up in 1990.

HERMALIN: But there was a lot to do. There was a great deal of effort in setting up our own bookkeeping and handling the many details of an association.

I think it's premature for me to say what caused the drop and what we need to do to get back, because that's what we're working on very heavily. We focused on this at last May's board meeting and earlier. We were concerned about it. I said to Joan Kahn when I appointed her chair of the membership committee in 1993, "The past membership committees have been very nominal

committees; sometimes you try a few good ideas. This is a very different environment; we've got to understand what's going on; we've got to look at the whole machinery." So we were cognizant that it had to get real attention.

Dick has certainly made that a major focus of his efforts this year. We have examined our reminder system and we are all writing to lapsed members.

VDT: Oh, you are?

HERMALIN: It's a big campaign. The board members each have been given names of 50 people in their area who are lapsed that they might know. We've sent 8,000 letters to members of related associations that would give us their membership lists free and we have obtained a couple of hundred memberships out of that.

So there's a very broad campaign going on, both to understand what's going on and to remedy it. I think the final word will be in six months from now, we'll have a better sense of what happened.

I think some of our members were slow to renew and we hope to change that.

VDT: They weren't jogged. Let me just put this on the record. Jen Suter did not allow members to lapse for very long; she got after them. And Ina Young [successor to Suter as Executive Administrator] has stepped back from that kind of work. She's even talked about hiring a marketing person to go after lapsed members.

HERMALIN: No, the reminders go out.

VDT: Reminders, yes, but I meant more than that.

HERMALIN: Now we're following up on lapsed members. Dick had things in PAA Affairs to encourage our members to get new members. We're sending applications to centers to make sure that they give out the material very early in the career of new students so that they understand the virtues of membership.

So there's a lot going on. Some of it is esoteric bookkeeping in a sense but real. Jen, yes, I think worked hard to make sure that people renewed. But I think in the accounting system, if I'm correct, that if I lapsed in September and didn't renew till December that for a couple of years the accounting would put in my renewal as of December, rather than as of October 1--saying that you have just not been a member for two months. That caused slippage in that people's normal procrastination was costing us--you were losing two or three months of membership, on average, from people who were procrastinating by not dating them continuously.

What I'm saying is--I don't pretend to know the ins and outs and the final report hasn't been written--but there's a mixture of accounting things to be done, reminders to be done, ways of following up related organizations, showing appropriate groups how wide and diverse the content is of our meetings. We don't capitalize on the fact that--you mentioned the attendance at the PAA meeting, upwards of 1200--we get 40 percent of our membership appearing at a meeting. Probably no other comparable association does that and we should be capitalizing on that. This is an association in which people come to their meetings and they care about the association, and I think that will help us attract new members. There's a lot we have to offer.

By the same token, I think we have to be realistic. We are a second organization for many people. Their primary association is with sociology or economics or health, etc. There's always an interest in trying out something people hear about. They've gotten involved for a time in a population-related project so they say, "Gee, it would be good to join PAA," or they have occasion to give a paper so they join that year. And then they say, "Well, I'm really not going in that direction." I think we

have to realize there is always going to be a certain number of people circulating through, because our field touches so many bases so that people will get intrigued and then maybe decide it's not for them. That's legitimate.

What we don't want to do is lose people who think we're not doing something that we are. I remember speaking to somebody who said, "I was a member but I didn't see enough stuff on my area." And I said, "You're not looking; 10 percent of our papers now are in the health area."

We have to get out the word. I've been using the beginning of the program that lays out the sessions by topic [Topic Index]. It's a very nice advertising device for us to say, look how broad and diverse our organization is.

So I'm hopeful that we will have good things to show. You can say to somebody, look, if you're a gerontologist and you think we have nothing of relevance, look at this.

VDT: And you did this program summary?

HERMALIN: Yes, when we got done with the [1993] program we decided not only to tell people what's happening every day but that we would put in a summary of sessions by topics. So in addition to the usual chronology, another innovation we made is that summary; if you're interested in fertility, here are the eight sessions you could go to. If you want to interest somebody who's in aging, you can show that we do a lot; we have six or seven sessions on aging.

That was part of the idea. Once we went to this open forum, then we wanted to find ways that people could easily find their way around the program.

VDT: Do you think PAA can rest on its laurels when it gets back up to 2,700 members or should it push ahead?

HERMALIN: No. What I gathered yesterday is that for the current set of activities that we are engaged in, we need a base of 2,900 to 3,000 members. That would put us in kind of steady state.

VDT: That would be a tremendous leap up. The peak was at the end of 1990--over 2,700. But it had been at about that level since the early 1980s, when it had about 2,500. It went over 2,000 in the early 1970s; it hasn't gotten too much bigger than that.

HERMALIN: I think also you have to recognize that demography becomes alternatively a hot and a cool topic, as far as the general press goes. Sometimes you get some movement up and down from that.

As I said, we don't yet have the final report on why we went down so fast, but we have turned it around. I would hope that if you get the membership total as of the end of our annual meeting, the membership as of May 1 or late April, you will find it back in the 2,700 range.

VDT: You mean this past meeting?

HERMALIN: No, the coming meeting [1995]. Don't forget that people are about to get notice that their paper's accepted, along with the notice that says, make sure your membership is in or you're not giving this paper. So that should pick up another couple of hundred.

VDT: So it possibly could be up to 2,900?

HERMALIN: I don't know that we'll be at 2,900 as of May 1. But I would hope that with all the activities we're doing and the good start we've made and what will happen at the meeting that not too

long thereafter we would push close to 2,900.

VDT: And that will have a lot to do with reducing the budget deficit, which is another painful thing that has happened recently.

HERMALIN: Right.

VDT: Which PAA had never had before.

HERMALIN: It never had the surplus either.

VDT: Well, it had small surpluses in 1991 and 1992.

HERMALIN: Yes.

VDT: But 1993, it went down by a lot.

HERMALIN: Well--how shall I put it? My feeling is that we are an ambitious organization for our size, in certain ways. The membership expects a lot. I'm very pleased that we are very active in public affairs, but for our size we were trying to do a lot in that area. That's a costly operation. We try to be involved with lots of things in terms of maintaining the quality of data. So we have quite a bit of outreach that we feel comes with our responsibility. And getting tooled up for that, getting it underway, has led to higher expenses.

What happens in any organization is that things can go very smoothly and then you can have a lot of headaches.

We were very efficient in getting through the transition, being set up under Jen's aegis. She did a wonderful job; kept a very tight rein on the expenses. But then in short order, we had to replace Jen, and she was covering a lot of bases--that was expensive. We had to get a new public affairs operation going and space for that, and so on. So a lot of our existing arrangements needed to be redone all at the same time. And those are very costly.

You can be very fortunate in having the right people at the right time and things go very smoothly. And then when there are transitions, they can be costly.

My own view of it is that we have made these transitions--we now know what we can afford, what are the essential elements we need in each of these areas, and we also know the realistic amounts that we can pay and we will now keep those within the bounds that we have set. In the current budget estimated for 1994 there will be a deficit, probably, but much reduced from 1993, and we are hopeful that 1995 will be in balance.

VDT: Except that in 1995, you have to get new space, and that will probably be more.

HERMALIN: We may save money doing that.

VDT: Oh, really!

HERMALIN: It's not inconceivable that we can do all of the new space at a lesser cost than the existing pieces of space.

VDT: I hope they mentioned yesterday at the board meeting that we need new space also for our archives. We've got to take them out of the Georgetown library.

HERMALIN: Yes, we talked about that.

VDT: It was a lot of work, your meeting program. Did your very large program committee help a lot?

HERMALIN: Oh, yes. You can't get everybody in the same room at the same time. But they accepted the challenge that we should do it a different way; they were willing to put in the time. Generally each of my program people were one half of a team that went through all the papers that came in under those 14 headings and organized them into sessions. Took responsibility, in most cases, for finding a chair and a discussant for sessions they set up or a chair that would get a discussant.

VDT: You must be good at delegating. Some presidents said, "When it came down to the wire, I did it all with the two secretaries in my office."

HERMALIN: Well, there's no question I worked very hard on it, in terms of time. Part of my suggestions to the board to restructure the role of the vice-president was based on the experience of that year.

Currently, we're asking a great deal of a president and a president-elect--that combination. In the year I was president-elect, it turned out I worked a lot on the public affairs office, as you said. But you're also preparing the program. You're supposed to be preparing a major presidential address that sums up some piece of your wisdom or the field's wisdom. So you're wearing a lot of different hats. You're also intimately involved--you do go to the board meetings, you are involved with the operation of the organization. And it's very hard to turn your attention to each of these things. So by the time you've locked up the program, say in late December, you've put in a tremendous amount of time--working with your committee and your two secretaries and so on. Then there's all the followup, headaches, of fine-tuning the program in January, or whatever it takes. You're still struggling to get your PAA address done. And then you're worrying about the day-to-day affairs of the association.

VDT: And all the rest of your other life.

HERMALIN: Not to mention the fact that you have a lot of other duties.

So my feeling is that if you want to have a president who can pay attention to both the broader issues of the association as they're occurring and to give some attention to long-range planning, you've got to break this up a little better. And that was the beginning of that report that Karen Mason brought in that led to the view that we should have a first vice-president that works together with the president on the program.

VDT: It used to be, of course, that the first vice-president did the program.

HERMALIN: Yes.

VDT: That's not the idea?

HERMALIN: No, we didn't go that route. That was one possibility. Some organizations do that: assume it isn't the president's job to do the program.

VDT: That's a very good point. And another innovation of yours was that you actually wrote a President's Column in PAA Affairs. As I wrote you, the constitution says that the president should

report directly to the members after his year is up. Sidney Goldstein wrote a 1977 report in PAA Affairs, following his year, but no one else ever had done it. So that was great.

HERMALIN: I think it is a desirable practice. Well, depending upon how active the president has been in PAA before his or her election--sometimes they will just have finished serving as a vice-president or board member and be very closely involved, or they might be a little distant from it. So there's a lot for the president to learn, but there are things the president sometimes wants to convey to the membership of what they see as the major dynamics of the association: what we have to be alert to; what we've accomplished; what's still to be done.

In my case, it was partially reflecting on what I learned as well as where I thought we should be going. I had served from 1982 to 85 on the board. I was fairly busy in the interim period with work at the National Academy of Sciences [e.g., chair, Committee on Population, National Research Council, National Academy of Sciences, 1985-89] and other activities. So I was going to meetings and doing my thing, but I wasn't close to a lot of the association happenings. So as I took stock of where we were going, I thought it was good time for me to reflect that stock-taking with the membership and to impress upon them how much we had accomplished. That these had been very big steps, that they were very positive in general, but they were fraught with some problems.

VDT: You have stressed the importance of international outreach, as I said earlier, getting more Hewlett and Mellon funding for that. You think that's important, obviously. You said, was it, up to 25 percent of people at the annual meeting come from overseas?

HERMALIN: I think it's more like 10 percent--international people at the meetings.

VDT: And you feel that's important?

HERMALIN: As Etienne said, it has become much more of an international meeting--our association's meeting--by its content; by the fact that so many papers are coauthored with people from overseas, whether or not they're there. Many of them are now there physically as well as there as coauthors. So it's become a very international setting for population, in the issues we raise and the geographic areas we cover. And therefore I think that's why we see more and more international researchers wanting to come.

I think that's all to the good. I think it would be unfortunate if population studies in the U.S. would become very parochial, in terms of U.S. concerns. That has not been our direction.

VDT: I think you might be more aware of that because of your work being so international.

HERMALIN: Right.

What I broached to Mellon--and Carolyn Makinson was kind enough to think it was intriguing--was to say that given that we have a good size membership that is internationally oriented, what can PAA do qua PAA in the international setting. From Mellon's standpoint, and from Hewlett's to a certain extent, there is a lot of interest in institution-building in the developing world. Sometimes existing centers, like the Pop Studies Center or others, develop collaborations with a given institution in a given country, in Thailand or Taiwan, or wherever. Other kinds of organizations may also have opportunities to do institution-building, and I was saying that given that there's a growth of population associations around the world, does it make sense for associations to start to connect with each other at that level and to exchange visits and to become aware of each other's programs and content. There are many things we can learn from each other which can strengthen the associations.

At the IUSSP General Conference in Montreal [September 1993], PAA organized a satellite meeting of population association representatives from around the world, with Carolyn Makinson's support, which was highly successful.

VDT: Whose support?

HERMALIN: The Mellon Foundation. We met and we had a large number of country representatives there. We ended up with close to 30 countries. I want to be conservative in my estimate; I have the count somewhere. Some people caught me afterwards and said, "I couldn't come to your breakfast but I'm interested."

The point is that these associations were doing a number of interesting things. They were well past the initial stage of their development. They're doing things that PAA might take lessons from, and there's a good basis for mutual exchange.

For example, in some places the association helps translate more technical documents or more technical findings into policy-oriented implications, or less technical forms, in order to give them to appropriate government ministries. In some places, the association is a very active force for population education in the secondary schools or at other levels.

There's a large array of interesting things going on in terms of the connectedness with government, with the school system, the ways they serve their membership, or the kinds of outreach. I think we can learn from one another by enhancing appropriate contacts and that will add to the capability of all the associations.

So I'm optimistic. And one of my actions or recommendations to Dick [Udry] was that we form a new standing committee, a population activities committee, that Mary Kritz is now heading up, which will basically administer these Mellon and Hewlett funds where appropriate, but also help think through how this outreach should be carried forward.

VDT: I hope it works. Suzanne Bianchi [in her interview] said a problem was that there was no one looking after those Hewlett and Mellon monies, to see how the outreach could happen.

HERMALIN: Right.

VDT: So here you have a committee.

HERMALIN: Right.

VDT: And much of the work of PAA gets done by volunteer committees.

I had a question here on what you see as the outlook for PAA. You see it reaching out more internationally, and the member numbers going up.

Do you feel that the business and applied demographers are now satisfied, now that they have been bounced up again to be a Committee on Applied Demography? They thought they were downgraded in becoming interest groups [Business and State and Local Government Demography Interest Groups].

HERMALIN: I think we're doing reasonably well on that front.

I think we always have to be careful--specialization is a fault line for every association.

VDT: You said that in your presidential address. You ended up saying: "Specialization is a fact of professional life; for broad-based associations, special interests are fault lines that can lead to rather

serious divisions. Our association has been fortunate in resisting these because of our broad interest in population studies, our respect for each other's work, and a strong desire to be cooperative and fair in the management of association business." [Hermalin, "Fertility and Family Planning among the Elderly in Taiwan, or Integrating the Demography of Aging into Population Studies," Demography, November 1993, p. 517.]

HERMALIN: I think every association develops a culture. Our culture so far has been that we care about population and the Population Association in broad terms. And I think we formally or informally reinforce that ethos. Of course, there can always be a group that feels they're not quite getting their day in the sun. But I think we've been alert to recognizing people who feel they're not being treated fairly and taking steps, as with the applied demographers or another group, and trying to make sure that they do feel fully welcome and find this an interesting and important outlet for their interests. At any board meeting I've been at where that has come up--and that has been a persistent theme--there has been a great effort to make sure we're not overlooking some group of people. Sometimes we will put together a committee and someone will say, "You know, the people in this specialty, they also know a lot about this and you ought to have such-and-such a person." And we do it.

All we can do is encourage good-faith efforts and encourage our membership to realize that there's more to be gained by being together than being apart. It doesn't prevent them from following their narrower interests and we try to accommodate for that. So I'm hopeful that we will stay one happy family.

VDT: Good. That's a wonderful note to end on, and we have no more time, but quickly. What are your future plans? You're staying totally involved, keeping on keeping on?

HERMALIN: No, I don't think so. I would like to move, within a year say, to probably half-time involvement in my research. I probably will cut back my involvement with teaching and some committee work and just focus on a couple of parts of the research that I care about, that I want to pursue further. And use that remaining time to do things that have not gotten done in the last 25 years.

VDT: How do you manage to keep it all going, so many irons in the fire--all your committee meetings and other commitments?

HERMALIN: Yes. Right now, for example, we just got an award at Michigan for a new center on aging. It's called the Michigan Exploratory Center on Aging, with support from the National Institute of Aging. I'm the first director of that center; Tom Juster is associate director.

VDT: Who?

HERMALIN: Tom Juster, at the Institute of Social Research. He's the principal investigator of the Health and Retirement Survey.

VDT: Wow! Can you take that on too?

HERMALIN: Well, that's why I want to cut down on other things and have a delimited sphere. I want to help develop that center over the next couple of years, and continue my work on the aging of Asia. And then have some time to read a book or two or do what I want.

VDT: Last thing. Did either one of your children go into demography or anything like it?

HERMALIN: Well, indirectly. My son is an economist at Berkeley. He has an appointment in the school of business and in the department of economics.

VDT: What's his name?

HERMALIN: Benjamin. Ben was an undergraduate at Princeton and he worked as a research assistant to Ansley Coale, as an undergraduate; in fact, he took Ansley's course as an undergraduate. I tease the people at Princeton when they put out their alumni list. As far as I know, we're the only father-son combination that has ever taken Ansley's course. I sent them a picture once for their annual report--a picture of Benjamin and me and my granddaughter as a potential third generation.

He went to MIT for his graduate work and did not pursue demography. But at Princeton, the undergrads have to do special papers in their last two years and he either did his junior or his senior paper with Ansley. And he had a paper accepted at PAA. One year we both were on the PAA program--I can't remember which year--when he was still at MIT.

VDT: What about your daughter; what's she doing?

HERMALIN: My daughter, Anne, went to Mount Holyoke, majored in psychology, but has been working in business with the Chubb Insurance Group, in Hartford, as a personal lines manager.

VDT: Well, they both obviously have fine role models. Any grandchildren?

HERMALIN: My son Ben has two children now.

VDT: And your wife? Where did you meet and what is she doing?

HERMALIN: I met my wife, Jolene, in New York, while we were both working for the reform Democratic club in the East side of Manhattan in late 1959. She had come to New York some years earlier from Akron, Ohio, after completing her B.A. in English, to work in advertising. We worked together on a local paper the club was putting out as a means of educating the electorate to the issues.

We were married in February 1961 and then moved to Forest Hills in Queens after our son was born in 1962, because we could not find a larger apartment in Manhattan that we could afford. We lived there until 1964, when we moved to the graduate student housing in Princeton as I started my graduate studies. Our daughter was born at the end of July in that year. As I look back, I realize it was only because of Jolene's encouragement that I took what in retrospect was such a big step: giving up a promising career in business for the unknowns of academia, with two small children in tow.

After we moved to Ann Arbor and the children got a little older, Jolene returned to work outside the home, moving into development and fund-raising for the university where she could use many of her skills in writing and organization. Most recently, she became director of development and assistant dean at the School of Music, and she is winding up more than 12 years there this December [1994], since she just announced her retirement.