

DEMOGRAPHIC DESTINIES

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

PAA Oral History Project

Volume 1--Presidents

Number 1--From 1947 through 1960

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	<u>6</u>
12	1948-49	Conrad Taeuber	12/5/73, Research Triangle Park, NC, Lunde	<u>18</u>
13	1949-50	Frederick Osborn	No interview	
14	1950-51	Philip M. Hauser	11/12/88, Chicago, VDT	<u>33</u>
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)	<u>64</u> <u>99</u>
17	1953-54	Irene B. Taeuber	4/28/73, New Orleans, Lunde	<u>74</u>
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	<u>81</u>
20	1956-57	Joseph J. Spengler	12/15/76, Chapel Hill, Rosenberg (with Hamilton and Kiser)	<u>99</u>
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	<u>116</u>
24	1960-61	C. Horace Hamilton	12/15/76, Chapel Hill, Rosenberg (with Kiser and Spengler)	<u>99</u>
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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	4/6/8/88, 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
42	1979	Charles B. Nam	4/22/88, New Orleans, VDT
43	1980	Jacob S. Siegel	6/21/88, Washington, DC, VDT
44	1981	Judith Blake	5/4/89, Los Angeles, VDT
45	1982	John F. Kantner	3/22/88, Bedford, PA, VDT
46	1983	George F. Stolnitz	1/20/88, Washington, DC, VDT
47	1984	Samuel H. Preston	6/14/88, Philadelphia, VDT
48	1985	Jane A. Menken	6/13/88, Philadelphia, VDT
59	1986	Paul Demeny	6/8/88, New York, VDT
50	1987	Ronald D. Lee	4/28/89, Berkeley, VDT
51	1988	Reynolds Farley	2/4/89, Washington, DC, VDT
52	1989	Harriett B. Presser	11/15/89, College Park, MD, VDT
53	1990	Larry L. Bumpass	3/21/91, Washington, DC, VDT
54	1991	Ronald R. Rindfuss	5/1/92, Denver, VDT
55	1992	Etienne van de Walle	2/17/93, Philadelphia, VDT
56	1993	Albert Hermalin	2/17/93, Philadelphia, VDT

FRANK W. NOTESTEIN

PAA President in 1947-48 (No. 11). Interview with Anders Lunde at the PAA annual meeting, New Orleans, April 27, 1973.

CAREER HIGHLIGHTS: [For an excellent review of Frank Notestein's career and influence, see Norman B. Ryder, "Frank Wallace Notestein, 1902-1983," Population Studies, March 1984, pp.5-20. Valuable histories of the development of demography in the U.S. by Notestein himself are "Demography in the United States: a partial account of the development of the field," Population and Development Review, December 1982, pp. 651-687; "Reminiscences: the role of foundations, the Population Association of America, Princeton University and the United Nations in fostering American interest in population problems," in Clyde V. Kiser (ed.), Forty Years of Research in Human Fertility, Milbank Memorial Fund, 1971, pp. 67-84; and "Memories of the early years of the Association [PAA]," Population Index, Fall 1981, pp. 484-488.]

Frank Notestein, "one of the architects of modern demography" (Ryder, p. 5), was born in Alma, Michigan, in 1902 and died in Newtown, Pennsylvania, in February 1983. He received the B.S. in 1923 from Wooster College, Ohio (where he met his wife Daphne), and the Ph.D. in social statistics in 1927 from Cornell, where he studied with Walter Willcox, the "father of American demography." While with the Milbank Memorial Fund from 1928 to 1936, he conducted two pioneering studies on differential fertility in the U.S., one using children-ever-born data from the 1910 census and another, with Regine Stix, based on a follow-up of patients of the Margaret Sanger Clinic in New York City. In 1936 he took charge of the Office of Population Research on its founding at Princeton at the instigation of Frederick Osborn (PAA President in 1949-50). During his 23 years at Princeton, he taught the undergraduate and graduate courses in population, coedited Population Index, contributed to the development of the demographic transition theory, directed research which resulted in four influential volumes on the population of Europe, produced for the League of Nations, and several volumes on the population of Asia, for the U.S. State Department and international agencies, and launched the Princeton Fertility Study, which continued the work on U.S. fertility begun with the Indianapolis Study. From 1946 to 1948, on half-time leave from Princeton, he was Consultant-Director of the newly established Population Division of the United Nations in New York. He played a key role in the decision of John D. Rockefeller III to establish the Population Council in 1952, beginning with the report of the Rockefeller Foundation-sponsored trip that he led in 1948 to survey public health and population trends in the Far East. He became second president of the Population Council in 1959, following Frederick Osborn, and by the time of his retirement in 1968 had developed it into the leading organization in providing research and technical assistance on the population problems of less developed countries. Notestein was also author or coauthor of over 100 published works on fertility, population growth and demography.

LUNDE: Frank, would you please tell us a few things about the early days of the PAA.

NOTESTEIN: I remember the organizing meeting [May 7, 1931] fairly vividly. Hank [Henry Pratt] Fairchild was the moving spirit and through the good offices of Margaret Sanger, he had gotten some funds from the Milbank Memorial Fund to finance the meeting. I think there were some 35 of us there, including Frank Lorimer [PAA President 1946-47], who is now in New Zealand, and Frederick Osborn [President 1949-50], among those now surviving. It was intended by Professor Osborn [\[this must a typo--it should be Fairchild\]](#) that a nominating committee put in his name as president and Mrs. Sanger

as first vice-president. But difficulties arose immediately. Frederick Osborn was a very great admirer of Mrs. Sanger but he felt rather keenly that there was a great need for a professional society which was not an action group and that it would be a great mistake if the association became an adjunct of her birth control movement, which he always supported and in which he was a strong believer. I think he even convinced Mrs. Sanger of this. I don't think she was at the meeting but in any event, he spoke of this and her name was withdrawn.

It's interesting, nowadays, I hear the youngsters worrying about the purity of science versus the need for action. Sometime ago, people were suggesting we ought to set up qualifications for demographers; no one should be entitled to membership in this superior group who was not fully qualified. Well, believe it or not, in the early days we took ourselves even more seriously. You wouldn't believe the distance we went in order to keep all power in the hands of the purest of the pure.

You may remember that the International Population Union [now the International Union for the Scientific Study of Population, IUSSP], which was started before the PAA, was not a membership organization but a union of societies and there wasn't any population society here. Gini and Raymond Pearl had gotten it together but there had to be some American institution. So Louis Dublin of the Metropolitan Life Insurance Company organized an American National Committee for the International Union. Then, in order to keep power where it should be in this nascent association [PAA], we had a College of Fellows, the Elect of the Elect. Then we had the plebeian organization of members of the Association. I happened to be on all of the groups.

I was quite a youngster then; 30 years old. I was kind of the youngster of the party. Probably I got there only because I was working for the Milbank Memorial Fund and they put up some of the money. My boss was Ed Sydenstricker. He knew that if they sent a youngster like me I would not be used as a moneybags. I got to watch the elder statesmen.

We used to meet in Dublin's office at the Metropolitan as the American National Committee and draw up a memorandum for the PAA. Then the same group would quickly journey uptown to the Town Hall Club and assemble as the College of Fellows of the Population Association. We would very seriously receive the memorandum of the committee of the Union which we had just passed, deliberate it with all due seriousness, and pass on the wisdom of our remarks to the Association. Having done so, we would quickly adjourn and reassemble downstairs as members of the Population Association and receive the superior wisdom of the College of Fellows.

It all seems so ridiculous. It took us a long time to realize that the action elements of the Association were probably less anxious to capture us than we were to avoid capture.

Things are turned around now and I think it's pretty healthy. I take a dim view of societies that decide they're going to be exclusive. As far as I'm concerned, interest enough to join is perhaps sufficient qualification. I don't like this self-selected concept; collects people into a mutual admiration society.

But the Population Association of America quickly came to abolish the College of Fellows and to open itself up. I think it was fortunate that we didn't initially start as an action group. I think it did a great deal of good for demography around the world to have a body of people becoming demographers--there was hardly such a trade then--whose primary dedication was to study. Now that studies are pretty well advanced and the fund of knowledge is much greater, I think the dangers of getting into action are less than they were.

Other reminiscences? It was a friendly association. For many years it met as the guest of Princeton University [nearly] every year; it was terribly pleasant for us at Princeton. It was a small association. It had the virtues of having economists, sociologists and, in the early days, biologists--people with quite a diversity of interests--but a small enough association. Every time I see Gunnar Myrdal--he used to be there in the old days--he remembers the pleasure of those meetings in the forties, when everyone knew everyone else, though we were each cultivating slightly different patches.

Fairchild interested Eleanor Roosevelt somewhat and she invited us to the White House. But much more interesting, she brought her knitting and came to the session on differential fertility [at PAA's meeting in Washington in May 1935]. So it was a rather small and gemütlich group of people.

It had perhaps very little influence on the course of events at the time, but a good deal of influence on the standing areas of demography. There really weren't demographers; there were statisticians. I remember in 1927 when I'd just finished my degree and was taking my new bride for a year's fellowship in Europe [studying occupational mortality on a Social Science Research Council fellowship], I started off by seeing [Dupont?], who was head of vital statistics in New York. When I told him what I was going to do, he said, "I've got the only job [of that kind] in the United States." The number of professional openings [in demography] was really very small.

LUNDE: What was the Census Bureau like in those days? Didn't they have a professional group that might be called population experts?

NOTESTEIN: Well, they had two PhDs. One was Elbert Edwards who did the economics at the time, occupational class variations. The other was Joseph Hill, who was a very considerable scholar. Later Leon Truesdell [PAA President 1939-40] came in. He took his Ph.D. in Brookings; he'd been in the Census before then. Stewart, the director of the Census, started as an office boy. I remember vividly some of us worrying about accuracy when he gave a long speech, something to the effect that you simply had to take everything people reported as true and one could never raise questions about [what was reported]. But he was a pretty good director of the Census.

LUNDE: When did Dr. Harold Dorn [PAA President 1957-58] come into the picture?

NOTESTEIN: Much later. I remember because I found out afterward, much to my surprise, that I'd been a candidate for that job. The committee decided to offer it to him and they couldn't have been more correct. Hal was a very good director of Vital Statistics and my interests were in quite a different direction.

LUNDE: Back through the years were there areas where the Association had some particular impact?

NOTESTEIN: Well, in the Depression we had a situation not at all unlike that of England where the unemployed were overrunning the relief systems and people were saying--it was almost another Malthusianism--that it was no good having cheap corn if these people were going to have more children. There was a great deal of talk about people on relief having babies. I think the Association did well having meetings and publicizing what was actually going on. Sam Stouffer did quite a few studies that got into the papers, showing that people on relief had lots of babies and that's why they got on relief. The need was there; people who had lots of kids were likely to be on relief. Then the birth rate went up a bit in 1934 and 1935 and there was a great hullabaloo about whether relief was lifting the number of births. I was able to take this by region in New York City and show that in areas where the unemployed were prevalent birth rates tended to drop rapidly and it was the well-to-do areas that were producing the children.

Then the Association was useful at the time that the [Committee on Population Problems of the] National Resources Committee was active. You remember that Frank Lorimer was [director of the technical staff]. After the Association got started, Frank raised some money from a Mrs. Robinson, a Quaker, to start a thing called Population Literature. He got Irene Taeuber [PAA President 1953-54] to do the bibliography on this and that led to two volumes. At the time, the Milbank [Memorial] Fund was paying something to the Association for a Permanent Secretary, wanting to get the Association

launched, and Frank was the secretary. Then the Resources Committee came along and offered him the directorship of this big population study and he decided it was the thing to do. In the meantime, Milbank had given Princeton enough money to hire me to set up the Office of Population Research, in 1936. Princeton was pretty keen about our having a publication and the arrangement that Lorimer had [for funding] dropped out, so it was agreed that Irene Taeuber would join the Office of Population Research and we would put out the successor to Population Literature--Population Index. The Milbank Fund encouraged that. It was thought a public health foundation would have some trouble justifying the continued support of the Association journal, but since the Fund had decided to support my work at Princeton, it could be put into the budget there pretty easily without raising any question. Of course, this was most fortunate because by this channel, Irene Taeuber was working for the organization. She started in 1936 and retired this year [1973]. And if there's a more productive demographic scholar in the world, I don't know [him/her].

Now, the National Resources Committee--this was the cast. Lorimer was on it; that was one of the Association's contributions. P.K. Whelpton [PAA President 1941-42] and Warren Thompson [President 1936-38] were doing population projections; Thompson was on the governing committee [Committee on Population Problems]. E.B. Wilson was chairman of that committee. And I guess this is the only case of explicit censorship that I've ever run into. Is that in your records?

LUNDE: No, I haven't come across that. [Notestein described this incident more fully in his "Reminiscences," op. cit., p. 72.]

NOTESTEIN: Clyde Kiser [PAA President 1952-53] was invited to do a chapter under his own signature on differential fertility. One section was on determinants. He went through the biological determinants and went on to present evidence showing that biological factors are really unimportant and all the large differences in fertility arose through the prevalence and effectiveness of contraceptive practice. The chairman--who was not a politician in the least but just, I think, a timid academic--without consulting the committee or discussing it with Clyde, just cut it all out. So the section on the determinants of fertility talked a little about biological factors and stopped. This was over Clyde's signature and without consulting him, or Frank Lorimer.

So I called up the committee and told them I was writing an editorial about this in Population Index. They asked me to hold it and said, "If there's a second printing [of the committee's report, The Problems of a Changing Population, 1938], we will permit the text to be amended in a way that shows not the major importance of contraception but the minor importance of biological factors." [Kiser was given the opportunity to alter the text in a second printing.]

I've been interested in where censorship comes from. One hears about the establishment and I guess in the demographic establishment I've been part of it. But this was the only explicit case I've heard of. It didn't come from the Catholic Church, that I know of, or the politicians. It came not from something that happened but from something somebody was scared would happen. One of our major troubles is anticipating events that don't happen. It's the only explicit case of ruthless censorship that I know of. We're having some cases now with government, politics, that I don't know too much about. But by and large, governmental influence has not been of the sort that involved either vicious suppression or vicious changes of fact.

Well, the Association was involved in that and frankly, I was pleased as punch to write an expose of the thing at the time. I wrote it in Population Index [July 1938].

This bears on how innovations get started and the role of universities and foundations. My conclusion is that most of these innovations come basically from individuals--a few of them [in demography] were academic, not many. Well, quite a lot came from Chicago. They were sociologists, pushing population very much on its merits and calling it sociology. Raymond Pearl was pushing the

biologic aspects.

I think innovations have come mainly in response to public situations. You go back to the Black Death and the Bills of Mortality; the tremendous unemployment of the Malthusian era; the unemployment of the Depression; the Great Migration--almost all of these gave rise to great over-arching general theories--generally wrong--to the Pearl population law. The events dictate the attack.

Where do the funds come from? There aren't that many vested interests. Milbank Fund's interest came from a trustee, [Thomas] Cochran of the Morgan Bank, who said he wouldn't give more money for public health until someone got worried about birth control. This came up specially sharply over public health money for China, under Ed Sydenstricker. [Rod?] had been running the Fund long enough to know that management just loves to have a member of the board driving fanatically for something that [might be controversial]. In any case, I was brought into the Milbank Fund because they [the Milbank board] told us, "You have work to do." They always doubted the wisdom of a conservatively financed institution advocating a cause. But they would advocate the study of causes--the study of the situation. So I was brought in to do research that led to the study of birth control.

The Milbank Fund went ahead for awhile. The Rockefeller Foundation--well, it went ahead and then it burned its fingers and backed off. Like any organization--it's a huge organization and every time you started talking about population, well, the science people would know they had the real solution. The malaria people would be a little scared if you'd say that dropping the death rate too fast would just complicate life; they knew they weren't complicating life--and they were not. And everyone knew that new interests took money away from the old interests. They began to finance a committee on sex [through the Committee for Research on the Problems of Sex of the National Academy of Sciences, the Rockefeller Foundation supported the research of Alfred Kinsey], but a fuss was raised and they drew out of that.

But John Rockefeller was excited about population. He'd been out in the Far East and came back with what was being worked on in many aspects of Asian life and one was population. He was worried that the Foundation hadn't given much thought to how medical and social science things mixed together. He wanted a public health man and a demographer to go out to have a look at Asia and come back and give their views on population policy to the Foundation. He called me and asked if I would like to do that. I said that would be fine but everything I knew about the Far East I'd learnt from Irene Taeuber [then working on The Population of Japan, published in 1958], couldn't she go. The amusing thing was that John Rockefeller didn't come. It's hard to remember now but the funds were young. This was 1948. The Rockefeller Foundation wasn't about to let somebody else travel under the Rockefeller name so we went with their support and all of a sudden we were taking over [establishing] another Foundation activity. We had a lovely time. The Foundation brought in Marshall Balfour and Roger Evans; we [four] visited China, Indonesia [Japan, Korea, Taiwan, and the Philippines]. We wrote up a report; it was published with the title Public Health and Demography in the Far East [Report of a Survey Trip, September 13-December 13, 1948, 1950]. Not long after that Cardinal Spellman said there were no circumstances in which he could approve of an organization that had anything to do with birth control. Well, frankly, that was just about that on the subject of birth control at the Foundation.

But amusingly, this undoubtedly helped us get money at Princeton from the Rockefeller Foundation. By now, Princeton was very proper. We were demographic; we weren't in the birth control business. This was a lovely way of assuaging some wounds. I was a beneficiary of the non-dangerous position for demographic work.

By the way, there was anxiety at the Milbank Fund in the early days because we were studying birth control. The Milbank Fund had gotten into terrible trouble over socialized medicine. I don't think the Cardinal could have done it on his own [deflected the Rockefeller Foundation from population work], but I think it was the Cardinal together with other staff members who were

concerned about their particular areas of interest.

The thing I'm really coming to is that population [funding] started at the Milbank Fund; that was a pretty small outfit. Then the Rockefeller Foundation moved in. John Rockefeller III became chairman of the board and, frankly, the Population Council was started because John couldn't get the Foundation interested. He never told me that but I know it to be true. Because he found the Foundation could not be properly interested in population, he set up the Population Council on his own.

The universities don't come off very well on this. They have to be bribed into new activity. Well, Chicago comes off well, in my judgement--in the old days; I'm not talking about the new--totally different thing. Rupert Vance [President 1951-52] and Odum [at the University of North Carolina] came off well as sponsors of innovation. But the same sort of vested interest happened in universities. All the monies come in. Everyone who is organized wants his cut. A new and different thing doesn't have professional backing--who are they? The slaughter of innocents becomes pretty heavy. At Princeton we brought in our own money and we brought graduate students. No economist wants to be a demographer. We were interested in strange people, so we did bring some students. This was an angle they weren't too concerned about. On the other hand at Yale, the international politics group came and every member of its political science department came to its session and there was trouble. You can put something in if it's not viewed as a threat.

What happened is that individuals like Warren Thompson, with old man Scripps of the newspaper chain--you've heard this story--he dug Thompson's thesis on Malthus out of the library, read it, took Thompson on his yacht out to the Far East and came back and established a foundation [Scripps Foundation for Research on Population Problems, Miami University, Oxford, Ohio]. It was meant to be much larger but there were two quick deaths [and estate taxes to pay]. I think as a maximum they never had much more than \$15,000 a year for both of them [Thompson and Whelpton] and it wouldn't have been possible in later days, except they got money from the Rockefeller Foundation. But Scripps, Cochran, Sydenstricker, John Rockefeller, Fred Osborn--well, I guess I better gossip a bit about Fred.

Fred, as I've already told you, was a key man in the organization of the Population Association. I wrote a piece about Fred on his 80th birthday, a speech at PAA ["Frederick Osborn, Demography's Statesman, on his Eightieth Spring," speech delivered by Notestein at the banquet during the PAA annual meeting in Atlantic City, April 11, 1969, Population Index, December 1969, pp. 367-371]. Fred retired when he was 39. He was well-to-do when he was born, I suppose. He was also a very successful promoter. The family had interests in the Detroit, Toledo and Ironton Railroad; pretty affluent. We had talks; I enjoyed those talks.

He said when he came out of Princeton he went to Cambridge for a year. Came back, wondered what to do. Well, socialism was in the air at the time and here he was--worried. So he went down to the public library and read. On the basis of that experience, he came up with two years of study [after his retirement, reading a course laid out by the anthropologist Clark Wissler at the Museum of Natural History]. Fred used to say, "I got into this business too late, I'll never be a technician. But I think I've studied enough now to ask this question. I think the resources devoted to the study of man are ridiculous. And I propose to devote the rest of my life to creating funds and organizations for the study of mankind." And indeed, that was what he did. He was a Princeton man; his father was a Princeton trustee. And between them, that was how population work got started at Princeton.

Fred went from that to the Bureau of the Budget in Washington, just before the war. He wanted to do some promotional things. He'd been worrying about the quality of the population, eugenics. And in his view, the preface to eugenics was to get the solid environment which would allow people to develop their inborn traits. So he went to Washington with the general view that he wanted to see if he could get free school lunches established throughout the nation, because without

adequate nutrition, inborn ability could not be developed. When the war started, he was chairman of the Advisory Committee on Selective Service, helping to strengthen the psychological and psychiatric screening for the services. He was traveling and people said when you're down in North Carolina look at why we're getting terrible rejections for high blood pressure. He came up with all sorts of theories about water and diet.

Fred was a trustee of the Carnegie Corporation and the Milbank Fund and in those roles he got resources for the population field. He did an enormous amount of mining of resources. He practically forced us into the Indianapolis Study. A tower of strength in the whole field. A man who started with the eugenic point of view was the man who precisely because of his interest in action was the one who thought we should protect the scientific character of the Association. Fred has been demography's great statesman. He was the first executive officer of the Population Council.

Who else do you need to know about?

LUNDE: Can you add more on Henry Pratt Fairchild [PAA President 1931-35]?

NOTESTEIN: Facile writer, able talker. Troubled in his last years because he was radical. Surely not a totalitarian Communist at all but surely less exercised [about Communism] than many of the establishment were. His influence was undercut to some extent because from time to time he said some nice things about Russia. He was not at all an orthodox Communist, just a man who was liberal in most particulars and at a time when things were sufficiently charged from the Depression so as to make people super-sensitive. Fairchild was an excellent founding president of the Association. His daughter was an actress--is, for all I know--I haven't seen her for years. He was a widower; a charming and very friendly person. Not a very rigorous [scholar], I never felt. I differed very much with him on the thesis of his book, People [1939]. Some people who accuse me nowadays because they view me as having carried the Population Council more heavily into birth control than it had been would be amused to know how I criticized Fairchild. His view of the drop in the birth rate was that it was [all] due to the invention of contraception. The prescription to get the birth rate down was to get birth control going [something about the Bradlaugh-Besant trial]. Our view of the demographic transition was much more in line with social change as a fundamental determinant, as I still think it is. Then you say, "What is the next marginal addition you put on within the range of your possibilities?." That was important in getting the Population Council into birth control, because at that time, that was the next possible thing to do. It was a very different thing from saying that birth control is the total explanation.

LUNDE: How about Leon Truesdell [PAA President 1939-40]?

NOTESTEIN: Did you know he was a poet? We have a book of his poems; he did this as a young student. He was quite a sentimental man; a New Englander. I forget how he got into Census [Bureau]. He didn't complete his doctorate degree till long after he'd been in the Census. It was a [monograph] from the 1910 or 20 census.

LUNDE: Do you have any recollections of T.J. Woofter, Jr. [PAA President 1940-41]?

NOTESTEIN: A sociologist, mainly interested in race, grew up with Clyde Kiser in North Carolina. Seven Lean Years is one of his books, isn't it? He wrote that from one of the New Deal agencies; good book. I'll say one thing for Jack, he was a liberal-minded sociologist in the Depression days. I think it's important to remember that he's one of the very first people to call attention to the need for cohort reproduction rates. He was pointing out early, at Population Association meetings, that no real

generation moves with the times, bearing and dying at the rates of a given year. Using period rates was artificial. He maybe didn't do it with mathematical rigor, but he saw these problems and emphasized them way ahead of anyone else.

LUNDE: When I used to talk to P.K. Whelpton, one thing I never asked was how he got started with cohort fertility. Was he, for example, influenced by Woofter?

NOTESTEIN: No, this came out of projection problems that he had. In the projection system, you pretend the events of this year are the events of a cohort passing through. Some of the stuff went wrong. And Pat said, "Well, let's do this by order of birth too. So you [project] not the childbearing experience of females age 20 this year, but the childbearing experience of childless women at age 20 this year. At what age do they have first children, second children and so forth. The assembly is a synthetic cohort. If you did this fairly age-specifically, you get one year in which 100 women have 108 children. That's a neat trick. Assuming he ran into these ridiculous things, this drove him into real cohorts. Synthetic cohorts become impossible. Pat was always looking for some way of getting a projection.

I think you probably have everything on Pat. That foundation [Scripps Foundation for Research in Population Problems] was originally two men and they were to take turns living around the world, but then the funds ran short. [Warren] Thompson was a better theorist than Pat. Pat was a better statistician; he came from agricultural economics. A very solid fellow. Clyde worked closely with him.

LUNDE: And Lowell Reed [PAA President 1942-45]?

NOTESTEIN: Lowell and Maggie Merrill, his assistant, were the best teachers I've ever come across. They have scattered around the world people who are making life tables. You remember we all had to worry about separation factors in life tables? Well, they'd all been indoctrinated with separation factors.

[Raymond] Pearl was a dynamic and important person; terribly flashy. He was arrogant and domineering, but when you got him going, he was one of these people you would just forgive. You would not hold him to the normal canon of behavior. Few people working closely with him were strong enough not to be made into Mr. Meek. Reed, who came from Maine, was a New Englander who was quite strong. Whereas Pearl was going off on laws of population growth, Reed was always interested in mathematical innovation. Pearl was the bouncer; Reed was the person who set things up. Despite his errors, he probably advanced the field more than most of the rest of us who [make pedestrian projections]. The field has often been pushed by the man who makes the dream and the wrong generalizations. Reed chaired the Advisory Council of the Milbank Fund for years. A wise man always. A very good teacher; an imaginative man. Feet on the ground. A darn good statistician who did a lot for the period.

LUNDE: Frank Hankins? He was President in 1945-46.

NOTESTEIN: I can't tell you too much about Frank Hankins. I knew him pretty well. He was at Smith College for years. A sociologist--a social biologist, a field that gets more attention now. Of somewhat rightwing persuasion. Articulate, good speaker. Interested in pushing population; interested in the qualitative aspect of population.

There was another person, Ellsworth Huntington, he was a geographer.

LUNDE: Yes, he wrote The History of Civilization.

NOTESTEIN: Also on the board of the Population Association. He had done a lot about the climate and population quality. I think the geographers always thought he was a good demographer and the demographers thought he was a good geographer. He was a terribly nice person. He had an identical twin. They were both short; both bald-headed; both wore goatees; and were both deaf and had battery sets, which were pretty large in those days. I remember one day we were having a board meeting in the Town Hall Club--Fairchild was a member and we usually met there--we were on one floor dealing with the Population Association and on the floor just below, there was his identical twin presiding over a nudist colony.

LUNDE: His book, I guess, was Civilization and Climate. A couple of more questions. One is on your association with the Association. You mentioned that you think you have been to almost every meeting of the Association.

NOTESTEIN: I wasn't there last year; my wife and I went sightseeing in Greece. That's the only one I remember missing. Now, maybe I was out of the country sometimes, but I think I've been to all the meetings. I'm sure I've paid dues to this association as long as there has been one. For 20 years or so I was on the board, partly because we published Population Index.

The Association is much more complicated now. The field has advanced enormously. The technical work is superb. I think you don't have dedicated demographic missionaries today as there were in those years. We did have a real sense of mission. Of course, I'm an old fuddy-duddy now. But I resent the people who call themselves "Concerned Demographers." In the early years there weren't any other kind. We think we were concerned demographers. The simple arrogance of these youngsters adopting this name bothers me.

LUNDE: You had to have a sense of mission in order to get this thing off the ground in those days, I imagine.

NOTESTEIN: I think so. Well, there were a lot of things. There was a core of us who were compatible. There was the Milbank Fund and later some other help that got the thing going. And then there were the university centers--Hopkins, Miami [Ohio] and Scripps, Chicago, North Carolina, Michigan, with Ron Freedman--Ron Freedman came out of Chicago. A few core people, a little core money, and a core association of demographers.

LUNDE: What do you think of this particular meeting [New Orleans 1973]?

NOTESTEIN: It's like every other meeting you go to nowadays. I understand half of it. Of course, I'm obsolete. People do the same thing they used to do. You're asked to give a paper for 20 minutes; they give one for 40 minutes. Their next posts are on the line and they don't really realize that the listener doesn't catch all the nuances by ear. Until this afternoon, I've been to [understood?] at least 50 percent of the programs--in the old days it was also 50 percent. People then talked too long and talked too dully, and I among them. I guess that's to say most of the Association continues human.

I think there's more status-seeking now, perhaps because there's a chance to get somewhere with it. In those days, there wasn't an awful lot of it. I don't mean that we suffered. The field had more openings than there were people. That's been the case for a fairly considerable time. So though when I started, Dr. Dupont said, Don't get into the field; he had the only job, the fact is jobs have been bidding for people. I had it much easier career-wise than the people who went into economics or

sociology. Now the competition is a little tougher. I think there's a little more status-seeking.

LUNDE: How did you actually get into this field that we now call demography?

NOTESTEIN: Out of boredom with economics. I was working at Cornell with H.J. Davenport, who was a magnificent teacher, no doubt about it. This was before Keynes and economics was price theory, not national economy. And it was logic-chopping--marginal utility, the price of pigs, corn, high-price land. And then I was taking statistics from Walter Willcox, who knew what a mean, median and mode were and not much else, but was a great teacher in a way. He had started teaching statistics at Cornell as applied ethics, from philosophy. His doctoral dissertation was on divorce. He began it as a problem in social philosophy, but then he learnt about Bertillon's statistical approach to divorce and this changed his whole world.

Willcox got a letter from E.A. Ross [at Wisconsin] asking him what the "replacing birth rate" was. I was set to work to get the answer ["At that time Willcox was unfamiliar with the answers already provided by Lotka and Kuczynski," Ryder on "Notestein," op.cit., p. 6]. At that time I didn't know there was a census. I worked on getting the net reproduction rate. This was just before Lotka published on stable population. I got excited. By contrast with economics, you could go to the library and work like the dickens and be quite sure that you were ahead of where you were three weeks ago; you'd found something out. Suddenly it dawned on me that births, deaths, movement of homes, marriage, these were vital events, that everything in the economy and society focused on it. And it had a central core of arithmetic, mathematical rigor. I began to say, "Why am I fooling around with other things?" It was an exciting way of looking at the world. I'm afraid it wasn't from a reformist point of view that I got into it; just the sheer joy of watching the variables behave.

LUNDE: That's what happened to me. I was interested in cultural aspects, studying the development of the arts in America, starting with music from the grassroots. I did a study at Columbia on this, starting out by studying the control of music, from the musician's view. While I was at school, I became involved in population studies and Kingsley Davis [PAA President 1962-63] came to Columbia and he stimulated me. It was something that was measurable, real, and yet had social significance.

NOTESTEIN: Well, I thought it was an exciting deal. Now, there were some other fortuitous things. Willcox was statistical adviser to the Milbank Fund. He got the Fund to commission me to look into the claims they made that health demonstrations they were running had reduced the tuberculosis death rate in Cattaraugus County [New York]. I came up with the conclusion that they hadn't. But they hired me to look into vital statistics in Syracuse and Cattaraugus County and I got some data for them and also for my [Ph.D.] thesis. Then I went on to a Social Science Research Council fellowship on occupational mortality, with which I never did anything.

LUNDE: Willcox was a very fine person from all I hear. Lived to a ripe old age.

NOTESTEIN: A hundred and three!

LUNDE: I met his son in Washington some years ago.

NOTESTEIN: Dean of the Johns Hopkins School of International Studies; did well. I think Willcox and his son are the only two emeritus--father and son emeriti--who were for some years at Cornell. Willcox wouldn't let me do mathematics; you had to sneak off [to do that]. What Willcox had was extraordinary respect for the data and the virtues of knowing the material. And then a simple

explanation. I found him exciting in the sense that he saw you into a problem and then listened to you. I found out that listening is a very great part of instruction; a little nudge here and a push there.

LUNDE: Frank, I want to help you very much for your help in this. Your reminiscences are simply marvelous.

CONRAD TAEUBER

PAA Secretary in 1939-42 (No. 4) and President in 1948-49 (No. 12).

This "interview" with Conrad Taeuber for the PAA Oral History Project was constructed by Jean van der Tak in May 1990 from the following materials:

Transcript of interview with Conrad Taeuber by Anders Lunde for the PAA Oral History Project at the National Center for Health Statistics Developmental Laboratory, Research Triangle Park, NC, December 5, 1973. (Side one of this two-sided tape is defective but was "reconstructed" by Conrad Taeuber in March 1987.);

Four chapters written during 1988-89 from Dr. Taeuber's (unpublished) autobiography entitled: "Minneapolis, Heidelberg, Madison, South Hadley"; "Washington-Potomac Fever"; "BAE to FAO"; and "Census";

Taeuber notes of March 30, 1987, in response to the query, "What accounts for the Taeuber 'Demographic Dynasty'?";

Dr. Taeuber's written responses to supplementary questions posed by Jean van der Tak, April 27, 1990.

CAREER HIGHLIGHTS: Conrad Taeuber was born and grew up in Hosmer, South Dakota. He received his A.B. degree in 1927, the M.A. in 1929, and the Ph.D. in 1931, all in sociology and all from the University of Minnesota. For some nine months in 1929-30, he studied sociology and collected data for his Ph.D. dissertation at the University of Heidelberg in Germany. He was a research assistant at the University of Wisconsin in 1930-31 and on the faculty at Mount Holyoke from 1931 to 1933. He then shifted to Washington, D.C., where he was first an economic analyst at the Federal Emergency Relief Administration in 1934-35. From 1935 to 1946 he was with the Bureau of Agricultural Economics of the Department of Agriculture as agricultural economist and administrator. From 1946 to 1951 he was with the United Nations Food and Agriculture Organization, then located in Washington. From 1951 to 1973 he was at the Bureau of the Census as Assistant and then Associate Director for Demographic Fields. And from 1973 to 1985 he was at Georgetown University, where he was Director of the Center for Population Research (1973-76) and Joseph Kennedy Professor of Demography.

He is the author of many articles and book chapters on a variety of demographic topics and coauthor with his wife Irene

Taeuber of two famous census monographs on the U.S. population, The Changing Population of the United States (1958) and People of the United States in the Twentieth Century (1971). He and Irene Barnes Taeuber formed a rare couple, both with distinguished careers in demography. They met as graduate students at the University of Minnesota and were married from 1929 until her death in 1974. Conrad Taeuber married Dorothy Harris in 1979.

Dr. Taeuber has also been President of the Inter-American Statistical Institute. His awards include the Exceptional Service Award from the Department of Commerce (1963), the Distinguished Career Award for the Practice of Sociology from the American Sociological Association (1986), and the Robert J. Lapham Award from the Population Association of America (1991) for "contributions to population research, the application of demographic knowledge to improve the human condition, and service to the population profession."

QUESTION: How and when did you first become interested in demography, and especially in rural to urban migration?

TAEUBER: During two years that I took off from my undergraduate work at the University of Minnesota, I took college-credit extension courses, taught by the high school principal in Tripp, South Dakota. These were my first social science courses and I was especially taken with one on immigration; I could relate bits of my family history to the story of immigration to the United States. This course was my first exposure to the study of population matters. There was no formal undergraduate demography course at Minnesota, but sociology courses that I took upon my return to the university got into a number of areas which would now be covered in courses called demography.

As for my interest in rural-urban migration, the shift in population to urban areas was all around us in the 1920s. The depopulation of some rural areas caused concern, as did the rapid growth of some cities. The U.S. had crossed the 50 percent urban line between 1910 and 1920. I was a rural-urban migrant myself. I was brought up in a very small town which was a service center for the farm families. Everything there was related to agriculture. My father was a Lutheran minister, whose parish initially included five congregations; later that was reduced to three.

Migration and population characteristics were more easily grasped than the field of community organization or some of the more cultural subjects which drew some of my fellow students in the graduate program in sociology at Minnesota, in which I continued after getting my AB in 1927. At Wisconsin, where Irene and I spent an academic year as research assistants while writing our dissertations for the PhDs from Minnesota, much of my work had to do with efforts to find the boundaries of informally organized rural areas. But this seemed much harder to grasp than numbers like population totals. The rural sociologists seemed to have their feet more firmly on the ground than some of the others in the sociology departments.

I had initially intended to do a doctoral dissertation based on a special study of some aspects of the movement of people from rural to urban areas in the U.S. Then in 1929 I won an International Institute of Education scholarship to study sociology at the University of Heidelberg in Germany. (Both my father and mother had come from Germany and I knew the language.) I went there in September 1929 after completing my MA and the oral examination for the PhD at Minnesota and two months after Irene and I were married. In the library at Heidelberg, I discovered a series of municipal statistical reports based on the records of Germany's population register. By law, a person moving from one place to another was required to report that fact to the central administrative office of each place. This series, which went back to 1900 in some cities, provided a body of information on migrants and their characteristics which went far beyond what any small field study in the U.S. might have accomplished. My advisers at the University of Minnesota approved of my using these data. I

spent endless hours copying these data by hand to carry back in the summer of 1930 (Xerox hadn't been invented yet and photostat copies were expensive) and these became the basis of my dissertation. The title of the dissertation, completed the following year while Irene and I were at Wisconsin, was "Migration to and from Selected German Cities: An Analysis of the Data of the Official Registration System (Meldewesen) for 1900 to 1927." (The nine months in Germany also produced my first published article--a description of traditional "Fastnacht," or pre-Lenten, festivities in Black Forest villages, written to Irene, who sent my letter to the Journal of Folklore on the recommendation of a professor of anthropology.)

When I came to Washington in January 1934, I worked in the Federal Emergency Relief Administration. Our first task was to start a series of studies of the rural population and to encourage and assist workers in rural sociology at the state colleges of agriculture to carry on such studies in their respective states.

QUESTION: Frank Notestein in his last published article, "Demography in the United States: A Partial Account of the Development of the Field" (Population and Development Review, December 1982), noted that: "The Federal Emergency Relief Administration and [its successor] the Works Progress Administration of the Depression years, with such outstanding scholars as Thomas Woofter and Conrad Taeuber, directed public attention to the problems of poverty and public relief in the Depression" (p. 683).

Now describe some of your subsequent work in the Department of Agriculture where you were, chiefly in the Bureau of Agricultural Economics, from 1935 to 1946. Notestein in this same article said: "The Department of Agriculture, particularly the Bureau of Agricultural Economics, was constantly studying and calling public attention to the problems of the agricultural population."

TAEUBER: I held a variety of positions in the Department of Agriculture. I was responsible for the annual series of estimates of the flow of population from and to farms. (Today you think of that as the work Calvin Beale is handling so ably.) That developed into liaison with the Census Bureau relating to the Censuses of Population and Agriculture. We also worked closely with research workers in the states, setting up and carrying out a variety of research projects. For a time, we in Washington were in a position to provide funds for small projects. We also entered into cooperative projects with people at the state colleges. In 1942-43, I was designated as Acting Head of the Division of Farm Population and Rural Welfare while the Chief of the division, Carl Taylor, spent a year studying the agricultural population in Argentina.

Later I became the liaison with the Office of Statistical Policies and Standards in what is now known as the Office of Management and Budget. The Federal Reports Act of 1942 provided for a system of review for all inquiries originating in the federal government which sought identical information from ten or more persons. I became the focal point for review of proposals (questionnaires) that originated in the Department of Agriculture. The ingenuity of the people who initiated data-gathering projects and who wanted to avoid clearance was in itself a matter of interest. In that position, I reported directly to the Chief of the Bureau of Agricultural Economics, Howard Tolley. When he became involved in FAO (the UN Food and Agriculture Organization), he frequently called on me for a variety of tasks, including the preparatory work which led to the establishment of FAO.

QUESTION: How did you collect the Bureau of Agricultural Economics data on "the movement of people to and from farms"?

TAEUBER: The data on the movement to and from farms were initially secured through a mailed questionnaire sent to a group of crop reporters. Through fieldwork, we found problems with that

approach. Later the survey was reorganized and the findings of the Current Population Survey [begun during the 1940s] were incorporated in the development of the estimates.

QUESTION: What did you do with the FAO, where you were from 1946 until it moved to Rome in 1951?

TAEUBER: I had participated on loan from the Department of Agriculture in the three conferences that set up the FAO [Hot Springs, Virginia, 1943; Quebec, 1945; Copenhagen, 1946]. The dynamic leader of the Bureau of Agricultural Economics, Howard Tolley, left to join the FAO and also Congress had placed restrictions on the work of the Bureau of Agricultural Economics. So my move to the FAO was hardly surprising. There I was Chief of the Statistics Division, responsible for the collection and publication of data needed by FAO. In our data collection, we were concerned not only with the traditional topics of agricultural production and marketing, but also with all aspects of rural life around the world. Worldwide, the rural population constituted the majority of people and FAO had a responsibility to keep up with the situation and trends. Our data were collected from the official national statistical agencies. At first the Forestry Division and the Fisheries Division in FAO carried on their own statistical programs. Over time, the sharper distinctions were modified and cooperation became the mode of operation. In the course of my work, I traveled extensively--in Asia (especially Pakistan, India, and China--in the summer of 1948), northern Africa, and Latin America, as well as to Europe.

QUESTION: Why did you leave FAO when it moved to Rome in 1951?

TAEUBER: It is a common experience that it is difficult to sort out the various considerations that entered into a specific action. There were several reasons for leaving FAO, or for not agreeing to move to Rome. I would not try to rank them in order of importance. FAO had changed and I assumed that under the new Director General it would be a less interesting organization. The office managers had taken hold. A minor incident played a role in this evaluation. One day I found a memo on my desk criticizing a cable I had sent. I had worked hard on that cable, trying to anticipate the reaction of the person to whom it was addressed. I was annoyed enough to take the memo back to the office from which it had come and persuaded the author of the memo that I knew what I was doing. But who would want to spend time that way?

There were two other elements. One was the fact that Irene was well established in a position which could only be carried out in the Washington/Princeton/New York area [as editor of Population Index and involved in research, writing, and consultation for Princeton's Office of Population Research, the federal government, and the Rockefeller Foundation, etc.]. Changing that career pattern for a situation in which women could expect discrimination was not appealing. The other element was the fact that our two boys were at an age when high school and college needed to be considered. While a brief exposure to a non-American educational system could be viewed as beneficial, we felt that identification with the American school system would provide the best entry into the American labor market. Perhaps we were overreacting to the memory of the Depression of the 1930s.

QUESTION: How did you come to join the Bureau of the Census and what positions did you hold there?

TAEUBER: When I was in the Department of Agriculture, I worked very closely with the Bureau of the Census, especially with Leon Truesdell who was head of the Bureau's Population Division. I also had frequent contacts with Ross Eckler and Phil Hauser, who were on the staff of J.C. Capt, the

Director. They tried to recruit me to the Bureau but at that time I was not ready to leave the work I was doing. But when it became known that I was not going to go to Rome with the FAO, Ross Eckler, Deputy Director of the Census Bureau, approached me again. The Bureau had been reorganized and the new job I was offered as Assistant Director for Demographic Fields was attractive. I took it in 1951. In 1960 that position was reclassified to become Associate Director for Demographic Fields.

The fields included the Population, Agriculture, and Housing Divisions. The Statistical Surveys Division, the Foreign Demographic Analysis Division, and--for a short time--the Geography Division were also within my area of responsibility.

QUESTION: What innovations did you introduce at the Census Bureau? For example, in the Petersens' Biographies of Demographers, you are described as "a major architect of the 1960 and 1970 censuses." Also, Notestein in the article cited above said that if you (or Philip Hauser or Arthur Campbell) had been writing that article on the development of demography in the U.S., you "would do more justice to the innovative role of governmental organizations." What were some of the innovative contributions of the Census Bureau to the development of demography in your time?

TAEUBER: I like to think that the Bureau's communications with the public were improved with the attention I was able to give. I would like to think that releases from the Bureau improved in clarity and with more rigorous attention to the underlying statistics.

Two major surveys received early attention within the Bureau, prior to the time when a separate staff could take over: the National Health Survey and the Crime Victimization Survey. The Bureau provided advice and assistance in the early stages of the World Fertility Survey. We experimented with surveys of consumer intentions to buy. We were sued by a private research firm which claimed that we had trespassed on their property. The Court upheld our view that if the government needed statistical data it was empowered to collect them. However, we eventually concluded that the results of this survey were not commensurate with the cost. The Survey Research Center of the University of Michigan has developed the survey and apparently found ways of using such data to chart the future of the economy.

You ask what would not have happened if I had not been there. The members of the Bureau's Executive Staff worked together very much as a team. All major actions, presentations to the Office of Management and Budget and to the Congress were taken up in meetings of the Executive Staff before leaving the Bureau. I was not the first to propose, for example, that the Census of Population and Housing should move forward to a mail-out, mail-back procedure instead of having enumerators knock on every door. That procedure was introduced in the 1960 census in all but the least densely settled areas of the country. I was a strong supporter of steps taken to apply the best methods to the Current Population Surveys and the decennial census.

Two proposals during my time failed to bear fruit. Much time was given to a proposed mid-decade Census of Population. Congress passed a bill to that effect but since then it has been quietly vetoed when no Secretary of Commerce included it in the Department of Commerce's budget requests.

Unlike the Census of Population, the Census of Religious Bodies is not required by law; the decision of whether or not to take it rests with the Secretary of Commerce. Such censuses were conducted in 1906, 1916, 1926, 1936, and begun but not completed in 1946. There were endless meetings with the Association of Statisticians of Religious Bodies about undertaking it in 1956 but no agreement and that census was never put forward during my time. We experimented in a sample survey with a question on religious affiliation or preference. We found that John Q. Citizen did not object to answering such a question if put on a voluntary basis but that important organizations would raise major objections to putting the question on a mandatory basis, as are questions in the decennial Census of Population.

QUESTION: What part did you play in launching the 1950 and 1960 census monograph series in which yours and Irene Taeuber's two monographs featured (The Changing Population of the United States, 1958, and People of the United States in the Twentieth Century, 1971)? As the Petersens said, in these monographs "the social implications of the data were analyzed in far greater detail than was possible in the main [Census Bureau] volumes."

Why was there no monograph series for the 1970 census?

TAEUBER: Strictly speaking, the credit for the modern monograph programs should go to William F. Ogburn. He was on a Census Bureau Advisory Committee and at one meeting asked in an accusatory tone when there would be a monograph program, like that which followed the 1920 census. The Bureau had professionally trained social scientists who could write such analyses but no funds for such a program. We began discussions with the Social Science Research Council. This led to formation of a small committee to aid in selecting topics and writers for the 1950 program. Paul Webbink of the SSRC and I became the administrative officers. I was editor of both the 1950 and 1960 series.

Because of funding cutbacks that increased the staff's regular workload, those preparing the 1950 monographs had to do so on their own time and received no royalties because any such monies that might have accrued from sales of the monographs, published by the private firm John Wiley and Sons, were plowed back into the program to reduce the sales prices to the public.

The program for the 1960 series was similar, although this time the SSRC received funding from the Russell Sage Foundation and the Equitable Life Assurance Program and the books were printed and distributed by the Government Printing Office.

By 1970 government employees' names were appearing on the technical reports they had prepared and staff members were encouraged to publish in professional journals. So it was clear we could no longer expect Bureau staff members to rush forward with their knowledge of the data, eager to get credit for a published book into their CVs. Also, they were too deeply involved with their daily tasks to make it feasible to take an extended leave in order to prepare a census monograph. The 1980 census monograph program was developed with independent funds [and only a few Census Bureau authors], which gave it more flexibility. The ability to have all the books in the hands of readers before the next census does not appear to have improved.

QUESTION: Could you give examples of "thorny" issues you dealt with at Census, such as the feminist flap over "head of household" in the 1970s?

TAEUBER: The Census Bureau reports play a seldom recognized role in our society. The desire to be recognized as a member of an important group then leads to appeals or demands for recognition in the census. Before the 1970 census--and again before the 1990 census--it was debated whether persons with dark skins should be reported as Negro, black, Afro- or African-American, or some other term. A bill was introduced in the House of Representatives to require the 1970 census questionnaire to use the term "Afro-American." Fortunately the bill died and the matter was left to be worked out by the Bureau. That questionnaire gave people the option of designating themselves as black or Negro. Other groups were involved in similar discussions: Should persons with brown skins be known as Brown, Mexican, Chicano, Mexican-American, Hispanic, or by some other name?

Traditional use would classify an adult male as the head of the household where he lives. Feminists in the 1970s argued that this was an unjustifiable assumption and the earlier practice was dropped. People now are left free to designate who is to be listed on the first line of the census forms as Person 1--the householder or any adult household member. The question of relationships of household members then becomes one of relationship to the person listed on line 1.

QUESTION: What roles did you play in the Federal Statistics Users Conference and the Council of Professional Associations on Federal Statistics?

TAEUBER: The Federal Statistics Users Conference (FSUC) was organized by business people who felt the need for action when the Secretary of Commerce indulged himself in the belief that the Census of Manufactures and the Census of Business should be dropped as part of the "get the government off our backs" campaign. They were successful in getting Congress to call for these censuses as essentials in our whole economy. The FSUC continued to exercise review and liaison functions for some years but support for it fell off as the threat of "know nothing" receded from the scene. I was part of the informal group which formally dissolved what was left [in the mid-1980s?].

I had an active hand in developing the Council of Professional Associations on Federal Statistics (COPAFS) in the early 1980S. It was an outgrowth of discussions in a small group which agreed that professional associations had a stake in the program of federal statistics. As the discussions developed, it was clear that a formal structure was needed because of the Reagan Administration's virtual elimination of the Statistical Standards Branch from the Office of Management and Budget and thus from the federal government. Earlier, there had also been an attack on the Census Bureau's Advisory Committees, which downplayed the need for technical competency of committee members. The discussions led to establishment of the Joint ad hoc Advisory Committee on Federal Statistics (JACOGS). I was vice chair of the group which arranged for studies of the situation and eventually recommended establishment of COPAFS. I was designated as PAA's representative to COPAFS and served on its executive committee.

COMMENT: You left the Census Bureau on January 6, 1973. You and Walter Ryan had been informed a month earlier that your positions as Associate Director for Demographic Fields and Associate Director for Economic Fields had been abolished "as part of a forthcoming reorganization" of the Census Bureau. (Both positions were later reinstated.) In the "Census" chapter (dated August 1989) of your "Memoirs," you describe the actions leading up to your departure, which evidently were an attempt by the Nixon Administration to gain political control of federal statistical agencies, particularly the Census Bureau, and steer them away from their "New Deal" (Democrat) "concern over social problems"--i.e.,

"pushing statistics on poverty, unemployment, racial segregation, and other social problems." You quote from a February 1981 Fortune article, titled "Behind the Bad-News Census," which included a review of actions of the early 1970s regarding the Census Bureau: "Taeuber, whose liberal views on poverty and social policy made him, he guesses, not acceptable to the forces that were then being exerted to make the Bureau more responsive to the White House, was axed in 1973."

I won't ask you to repeat here what can be read in your excellent (unpublished) "Memoirs." You conclude this chapter on the Census Bureau graciously and with your (and everybody else's) certain knowledge that "We knew that our work [at the Census Bureau] was important and that it led to actions which ultimately affected every person in the country After more than twenty years in the Bureau of the Census, I can say to my erstwhile colleagues: Thanks for the good years."

In 1963 you had been presented with the Exceptional Service Award of the Department of Commerce "for outstanding contributions and leadership in directing the 19th Decennial Census of Population, Housing, and Agriculture."

I'd like also to put on this PAA record this observation from your "prized" clipping from the New York Times of March 30, 1970, stating that all of the activity in connection with the 1970 census came together in your office: "There in the heart of it all, like the Wizard of Oz behind a curtain in the Emerald City, sits a small, bald, kind, quiet, and expert man named Conrad Ferdinand Taeuber."

QUESTION: Now, please tell something of your time at Georgetown University, where you were Director of the Center for Population Research from 1973 to 1976 and the Joseph F. Kennedy Sr. Professor of Demography at the Kennedy Institute from 1973 to 1985. Shortly after you went to Georgetown you said in your December 1973 interview with Anders Lunde for the PAA Oral History Project: "At Georgetown I hope to develop a program of research relating to policy for the United States. I have a feeling that population centers around the country have paid far too little attention to the United States. We are developing a seminar next semester to take off from the Report of the Commission on Population Growth and the American Future. We hope to take advantage of our location in Washington to perhaps provide some stimulus to full utilization of the material. At a meeting earlier this week we discussed what the federal agencies were doing in relation to birth expectations, the current fertility situation. Are we with the present fertility rate virtually at the longterm replacement level; are we headed for zero population growth; are we headed for decline? Are we at a point where the fertility rate shortly will be rising again? What can be deduced from the available data? It will give us something as to where we are and if there is any governmental action that might be appropriate to provide the basis for it. We're not going to recommend to Congress what to do. But we would like to think it's possible to develop analytical material that will be helpful in policy."

TAEUBER: My attention was first drawn to the Center for Population Research at Georgetown when I was asked by the Ford Foundation to take part in a review of the work being done at the Center. Although the work being done was rather limited, it represented an important breakthrough for a Catholic institution. On the recommendation of that review, the Foundation made a grant as had been proposed.

Some weeks after my retirement from the Bureau of the Census, I received a phone call from Andre Hellegers and we met. Further discussions led to an offer. I always felt that Dorothy Thomas and Jeanne Clare Ridley [then on the staff of the CPR] were the moving forces behind Hellegers. He offered full-time appointments for Irene and myself, with full freedom to carry on research work. I was to take over as Director of the Center. Irene died before she could take on any duties at GU. Hellegers, who was the moving spirit behind what is now the Kennedy Institute of Ethics, envisioned a relationship under which the Center for Population Research would provide the facts for the world in which the ethicists would explore matters of policy and ethics.

It never worked out fully that way. One effort that attracted attention was a conference on "Death and Dying," in which we presented some data which showed that the elimination of cancer as a cause of death would add little to the span of life. With instant immortality ruled out, it seemed quite clear that those who were spared death by cancer were prime candidates for heart attacks and strokes. The study was really the outgrowth of a query raised at a meeting of doctors where someone ventured the guess that the elimination of cancer would not reduce the costs of medical care for the population. The question and the answer attracted a good bit of attention.

One day we presented the story at a colloquium of students in the medical school. As we walked out of the room, I was next to the chief of oncology. He invited me to accompany him on his hospital rounds. When I responded that I had watched a sister die of cancer and had another who would die of that disease in the near future, he decided that I was not arguing for the elimination of research and care relating to cancer.

QUESTION: Did you start the move toward a separate department of demography at Georgetown, one of the rare such departments in the U.S.?

TAEUBER: The Center for Population Research had been established outside Georgetown's

Department of Sociology, although some of the staff members had their appointments there. The Department of Sociology was not a strong department and Georgetown had very little interest in a graduate department. We at the Center felt handicapped in that we could not offer more than an MA. The future, it seemed to us, lay in developing a PhD program in demography. We put a great deal into that effort, but there was neither support within the university nor any indication that the needed funding from outside sources was available. Later, when the university had appointed a Dean of the Graduate School, the situation changed. With the support of the Dean of the Graduate School, a Department of Demography was established for a trial period.

QUESTION: Looking back over your long career, what would you say has given you the most satisfaction?

TAEUBER: That's a hard question to answer. I think I would have to put two experiences in that category. The early years in Washington were satisfactory. There was the New Deal; there were crises. There was rather frenetic activity at the Federal Emergency Relief Administration, which became the WPA (Works Progress Administration). We felt we were doing important research, research that would be used. We had a hard time keeping up with the demand from the administrators. We had the resources to do the work that was needed and we also had the resources to stimulate a good deal of research in the states. With relatively small funds, we were able to keep alive some of the research activities that were threatened by the drying up of state and federal funds. We were able to stimulate a considerable number of people who subsequently did very important work.

The other satisfactory experience would be with the Bureau of the Census to have had the opportunity to develop a program of demographic statistics which has meant an ability to provide much of the information which is needed in a form which is needed and to make the data available to researchers in developing countries in a way that has enabled them to take advantage of the data sources.

QUESTION: Who have been some of the leading influences on your career? Presumably that would include Pitirim Sorokin, the eminent Russian-born sociologist at the University of Minnesota who, as you describe in a chapter of your "Memoirs," was a "dynamic lecturer" and often would drop by the graduate students' "bull pen" in the basement of Folwell Hall to engage you in exciting discussion. Presumably it includes Irene Taeuber as well.

TAEUBER: I was in contact with some remarkable individuals in the course of my career. Yes, I would include P.A. Sorokin. Also Carl C. Taylor, Howard Tolley, Andre Hellegers, Ross Eckler, and Lowry Nelson.

The relationship with Irene provided not only the normal interaction of individuals but also the broader stimulation resulting from the fact that she was much more tied into the academic world than I was and I had the benefit of many of those contacts.

COMMENT: In your 1973 interview with Anders Lunde, you indicated that you and Irene worked quite separately on your own research, aside from the times when you collaborated, as on the two census monographs. It's interesting that two people, with worldwide reputations in the field of demography, developed their skills and reports and texts rather independently within the same family.

TAEUBER: Well, the organizational ties were different and the organizational responsibilities were different. I spent much more time on the administrative end, whereas she was tied in with Princeton's Office of Population Research where she had a relatively free hand to carry on research projects.

QUESTION: Your children are also outstanding in the demographic area. What accounts for the Taeuber "Demographic Dynasty"?

TAEUBER: Of course, the two boys knew that their parents were into something called "demography." Their mother was affiliated with the Office of Population Research. Their parents, singly or as a pair, were always going to or coming from a meeting which involved population. For a number of years, when I was Secretary of the PAA [1939-42], there were mass mailings which could be done with the help of youngsters moving around the dining-room table, stuffing envelopes, stamping them, and sorting them.

Richard showed an early interest in mathematics, manifesting a real appreciation for some of the elegant proofs in plane geometry. His major in college was in economics. At some stage when he expressed an interest in statistics, we went to Raleigh, NC, to talk with Gertrude Cox who headed the program there. When he returned from his stint in the Navy, he took statistics as his major field. At one point he was working at Oak Ridge and the work there involved population projections and other work in population.

Cynthia [as of 1991, in charge of statistics on the homeless within the Population Division, Bureau of the Census] did graduate work at the University of Georgia, where she worked with Everett Lee. Some time after she was married to Richard, she worked at the Southern Regional Demographic Group. By the time that she and Richard were divorced, she had developed some standing in the field under the name Taeuber and decided to keep it as her professional name.

Karl [Professor of Sociology at the University of Wisconsin, Director of Wisconsin's Center for Demography and Ecology, 1980-85] did his undergraduate work at Yale with some concentration on sociology. There was very little emphasis on demography there but he took the one course that was available. His graduate work at Harvard was also in sociology. At that time, there was little work in demography at Harvard. His dissertation was on residential segregation in the United States. He spent some of his graduate student time at the University of Chicago at their population center. There he and a fellow graduate student were married. That accounts for the entry of Alma [Senior Scientist, Center for Demography and Ecology, University of Wisconsin] into the family.

QUESTION: What do you see as leading issues for U.S. demography in the future?

TAEUBER: Population growth overall and differential rates of growth will continue to be matters of concern. Whether it is a call for increasing the birth rate to assure stability and perhaps slow growth, or a call for a negative growth rate, or something in between, the rate of population growth will continue to be an important topic for the U.S.

There is continued need for analysis of the rates of increase in subgroups. Analysis of growth rates in other parts of the world will become more important. We will be much more concerned with the broad relationships between population change and social and economic change, both here and elsewhere in the world. The shift of non-Hispanic whites from a majority to a minority of the U.S. population--recently highlighted in Time magazine--will draw much interest.

QUESTION: What do you see as the future for U.S. demographers? Are all the jobs now for applied demographers, in government and business? Are there still jobs for demographers working on basic research?

TAEUBER: The business community is not the only target for trained demographers. We have just begun to see their growing role in our everyday life. Changes in the structure of families will continue

to require analysis. The structure of metropolitan areas will receive much attention.

QUESTION: Now on PAA. When did you first join the PAA?

TAEUBER: I recall attending a meeting in 1933; it was held in a small room. [On May 12, 1933, PAA held a joint dinner meeting with the New York chapter of the American Statistical Association in New York City. There were 17 in attendance.] I probably became a member of PAA in the late 1930s. There were not many members. I remember we met at the Princeton Inn and held the dinner meeting in their dining room. [Earliest Princeton meetings of PAA were in fall 1936 and May 6-8, 1938. The next one was May 16-17, 1941.]

The OPR [Office of Population Research] at Princeton was the center for PAA activity during the early years of the organization. The meetings were easily accommodated at the Princeton Inn. Though it was small, the Association needed a Secretary. I was asked to assume that role [1939-42]. Governmental regulations on such a role for persons in civil service were less rigorous than they are now. There was no problem in finding space for that activity and in recognizing that some of my time and that of my secretary would be devoted to that work.

Later, I was also Secretary of the American Sociological Society (now Association), which involved my also being Managing Editor of the Society's official publication, the American Sociological Review. After the U.S. entered World War II, we put out a special issue of the Review 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 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.

QUESTION: What do you recall of the major figures in PAA during the early years?

TAEUBER: Louis Dublin [President, 1935-36] was a fascinating figure. I read his autobiography a few years ago. He arrived in New York penniless. From the ship, his father brought the family to a boarding house where he had rented space and they walked there from the ship, carrying the luggage. Louis took full advantage of the educational opportunities that were available to immigrant groups in New York City. Louis was a short person, very earnest, very friendly, very human. He was deeply devoted to the work he was doing and very competent. He was devoted to the improvement of public health and improvement of living conditions, especially of the poor. He worked very closely with Alfred J. Lotka.

Lotka [PAA President, 1938-39] was also a short person. He seemed very much more reserved. He worked closely with P.K. Whelpton [President, 1941-42] in the development of methods of measuring deaths and births and the "true" rate of natural increase, recognizing that current birth and death rates were not necessarily good measures of the long-run trends.

During the planning phase of the 1970 census, we conducted a series of meetings in various parts of the country to learn what users felt they needed from that census. One meeting was in Florida. When we found that the meeting was to be held not far from the retirement community where Dublin was living we invited him to join us and he did. He spoke to the group, with special attention to his services on advisory committees for earlier censuses and placed the whole development of census content and techniques into historical perspective, which was a very useful contribution to the meeting.

It was another illustration of his devotion to the cause of federal statistics.

Another person who played a major role in the early days was Halbert Dunn, who was the first chief of what was then known as the Office of Vital Statistics, now the National Center for Health Statistics. Harold Dorn [PAA President, 1957-58] of the National Institutes of Health was very active in the Association, a very effective member. There was O.E. Baker of the Department of Agriculture.

There were a number of people who were in and out. Howard Tolley, who was Chief of the Bureau of Agricultural Economics for a number of years, was concerned at one time. A person who was quite active was Margaret Hagood [President, 1954-55], who was at the University of North Carolina and later in the Bureau of Agricultural Economics. Henry Shryock [President, 1955-56] and Paul Glick [President, 1966-67], who were at the Bureau of the Census, have been active members for a long time. There was Regine Stix of the Milbank Memorial Fund. Frank Boudreau, who was head of the Milbank Fund for many years, provided very active support, not only to the demographic work at the Milbank Fund but later to the work at the Office of Population Research.

Other names from PAA's past include Leon Truesdell [President, 1939-40]. He was Chief of the Population Division at the Bureau of the Census for many years.

Frederick Osborn (President, 1949-50] was a phenomenon, physically a giant, mentally a giant, with wide-ranging interests and an innovative mind.

W.F. Ogburn was again a very outgoing person, worked closely with his students [at the University of Chicago and Columbia University] and took an interest in many things. He served at the Bureau of the Census on an advisory committee. I guess the fact that we had a series of census monographs goes back primarily to the push that W.F. Ogburn gave for many years.

Then there were Raymond Pearl and Lowell Reed. Lowell Reed [President, 1942-45] came to us through a concern with public health. He had worked with Pearl in formulating the logistic curve, which at the time was believed to provide a model for human population development. There were Reed and his assistant Margaret Merrell, a very quiet person who somehow was always in the background. Reed was also a very outgoing, friendly, delightful person; very much concerned with his students, pushing his students. He served for many years as chairman of a roundtable which the Milbank Fund ran every year on a variety of issues, some of them demographic, in the health field. Reed was always the charming chairman of those meetings.

Raymond Pearl was a somewhat more forbidding presence--tall. He was one of a group that frequently met with H.L. Mencken in Baltimore; he and Mencken were close personal friends. Pearl was very fond of good food, good drink, good conversation. He was a mental giant, of course, and active in the Association.

QUESTION: What were the principal concerns of members of PAA in the late 1930s when you first joined?

TAEUBER: The National Resources Planning Board set up a Committee to review the population situation. This was done in the report, The Problems of a Changing Population [1938]. Frank Lorimer directed the technical staff. With the trend toward replacement fertility, there was much concern over the prospect of a cessation of population growth and possible population decline if then current trends continued. The U.S. situation was paralleled in northern and western Europe. The Committee recommended no action by the government of the United States. In the Association, as in public discussions, there was concern over the prospects and a good deal of skepticism about the ability of governments to affect such trends, as well as questioning of the desirability of having governments make such attempts.

There was a strong element of eugenics at that time. The differentials in fertility, with poorer families having more children than economically favored groups, were noted with a good deal of

concern over the possibility that there would be a dysgenic effect and a lowering of the level of intellectual ability. The development of new measures of the underlying trends and the possibility of eventual population decline led to much discussion of population quality.

QUESTION: What other issues do you recall as being of particular interest in PAA meetings over the years?

TAEUBER: There were some rather intriguing methodological developments in measuring population growth, many of which can be attributed to Pat Whelpton. Lotka also brought them up; they worked very closely together. The whole development of demographic methodology came up again and again.

The Indianapolis Survey, which was a pioneering effort to interview women and their husbands in relation to contraceptive practices, family size, and the like, occupied a good deal of attention. The need for and eventual development of the Population Council occupied a certain amount of attention at one stage.

Of course, in more recent years [the early 1970s], there has been a broadening to the rest of the world. There has been a good deal of concern with development in other parts of the world--evaluation of family planning activity, family planning programs, with the development of population policies. I think in the early years there may have been a bit more of that when the issue was: Can government policies, as in Germany, France, Belgium in particular, actually bring the birth rate back up?

This interest in the rest of the world, particularly the developing world, of course, has been reflected in the program in recent years. In the forthcoming meeting [1974], there is the money to bring participants from other countries. We've always taken advantage of participants from other countries [who were in the U.S.]. But in developing this program, we will invite a number of participants from other countries, hopefully one from mainland China.

QUESTION: Going back to the years when you were Secretary (1939-42) and President (1948-49) of PAA, what were some of the activities and issues that would be of interest today?

TAEUBER: From the very beginning, there was a continuing concern with the possibility that the organization might become not a scientific organization but be taken over by family planning and other activist groups. This concern was parallel to the concern in the International Union which brought into its title "for the Scientific Study of Population." There was concern in the drafting of a constitution for PAA and setting qualifications for membership. Though it was recognized that both the national and the international organizations owed a major debt to Margaret Sanger for providing the impetus that got them started, she recognized the validity of the desire to have an organization that would objectively review data and policies and trends.

The early group did have a variety of backgrounds and interests. Henry Pratt Fairchild [first PAA President, 1931-35] had been especially concerned with immigration and had written one of the early texts on that subject. Osborn had a strong interest in human genetics. Frank Lorimer [President, 1946-47] first came into the field through his collaboration with Osborn in the preparation of their book, Dynamics of Population [1934]. Lorimer might be considered one of the first full-time professionals in the population field. Irene and I met Frank and Faith (Williams) Lorimer soon after we were established in the Washington area. Faith held a high position in the Department of Labor. Frank did some teaching at American University. He was elected Secretary of PAA early on and decided that PAA could serve its members with a current bibliographic service. Research reports of interest to demographers were then scattered among many journals, including foreign-language journals which

were not readily available to most research workers in the U.S. He began this service to the field with the modest Population Literature, asking Irene to help him. This was taken over by the newly established Office of Population Research at Princeton and Irene became editor of the renamed Population Index, with an appointment on the staff of OPR, although she could do most of her work in Washington in the Library of Congress, which has a long policy of providing space to outside scholars.

Frank Notestein [President, 1947-48] and Clyde Kiser [President, 1952-53] were at the Milbank Foundation, engaged in demographic research, when PAA was established in the early 1930s. These studies included early studies of the use of contraceptives and also of migration from the U.S. South to the North. Frank Hankins [President, 1945-46] at Smith College had a strong eugenics bent. O.E. Baker of the U.S. Department of Agriculture was an early student of the movements of rural and especially farm population. He was concerned that the movement of persons from high-birth-rate rural backgrounds into the city environment with its tendency to low fertility would have dysgenic effects.

The interest in population trends in other countries came very early. It was stimulated in a major way during World War II.

QUESTION: The fear that the wrong kind of people might be brought into the Association has re-emerged from time to time throughout PAA's history. It was in evidence, for example, when a small committee including you and Forrest Linder debated possible changes in the constitution and procedures of the Association [See: Paul Glick, "PAA Committee on Organizational Management: 1966-67," PAA Affairs, Summer 1982]. Do you think that attitude has changed?

TAEUBER [from 1973 interview]: I am not aware that that has continued. The membership has grown rapidly [to the early 1970s] and there are members who are decidedly activist. But it is my impression that they are a smaller proportion of the membership than was the case in the earlier years. There was a group that called itself "Concerned Demographers" which functioned within the organization [late 1960s; early 1970s]. And there have been efforts to shape the programs of the annual meetings more in the direction of action programs. But I feel that there is much less of a feeling of "us" and "them," and no real pressure to keep people out of membership.

QUESTION: You were among those who recommended that PAA action on policy issues of interest to members should no longer be determined by votes taken among those present at the business meeting held during the annual meeting. Why was that?

TAEUBER: When we recommended that [in the early 1970s], we were moved by the fact that the Association had grown so much that its affairs could not be handled by the actions of the membership who were present at an annual meeting. The Board of Directors, which meets oftener than once a year, can become familiar with the background of issues and give thoughtful attention to proposals which come from members of the Association. An informational membership meeting such as we now have provides an opportunity to present some proposals for reaction by the assembled group and also an opportunity to bring to attention matters which the Board of Directors considers of sufficient interest or merit to warrant such exposure. [See, Constitution of the Population Association of America, Inc., revised July 1974, Article XI, Section 2: "The annual meeting shall include a general membership meeting as a forum for open discussion of the affairs of the Association."]

It is no longer possible to whip up a resolution at the last minute and have it adopted at the general membership meeting. [This meeting, held on the first day--Thursday--of the annual meeting has been officially the "membership" meeting since the constitutional revisions of 1974, although informally still called the "business" meeting.] Whether it is war in Central America, the hazards of nuclear energy, or opposition to a decennial census, there is provision for bringing this up for appropriate

action, and also for assuring that the Board of Directors does not avoid taking appropriate action. The provision for initiative referendum provides a means of checking any Board that deviates widely from the views of the membership. [See, Constitution, *ibid.*, Article XIII, "Initiative Referendum."]

QUESTION: You gave no presidential address at the annual meeting held during your presidential year [Princeton, May 28-29, 1949]. Why was that?

TAEUBER: The problems of feeding the world population had been the focus of public discussion, highlighted perhaps by William Vogt's alarmist statements in his book, Road to Survival. At the dinner meeting where ordinarily I would have given a presidential address, we arranged for statements by a panel of speakers, specialists in a number of the fields that were affected. These papers and those of the other four sessions of the meeting were later brought together in a booklet issued by the Office of Population Research, with George Mair serving as editor [George F. Mair, Editor, Studies in Population: Proceedings of the Annual Meeting of the Population Association of America at Princeton, New Jersey, May 1949, Princeton N.J.: Princeton University Press, 1949].

I had originally thought that it might be useful to have Vogt as a member of the panel. But when a leading soil scientist made it very clear that he would not participate if Vogt--with his propaganda and alarmist disregard of fundamentals of scientific method--were to participate, the focus shifted from a confrontational event to statements of scientific knowledge which was fundamental to any consideration of the possibility of increasing food supplies at a rate which would at least keep up with the prospective rate of population growth.

QUESTION: It has been suggested that publication of the proceedings of this meeting--the only time PAA meeting papers have been published in full in a separate monograph--was viewed as a way of getting the Roman Catholic position on fertility and birth control into print--that is, the paper by Reverend William J. Gibbons, S.J., "The Catholic Value System in Relation to Human Fertility," in the session on "Value Systems and Human Fertility." Is that your recollection?

TAEUBER: I am not aware that publication of the papers of the 1949 meeting was done to get a Catholic position into print. It would have seemed an unusual bargain to print a larger volume in order to get a minority statement into the record. My recollection is that the imbalance between population growth and the growth of resources, as had been put forward by Bill Vogt and others, sparked an interest in the whole relationship. My effort was to get a more reasoned statement into public view.

QUESTION: Other interviewees in this series have said that Kingsley Davis, the discussant in the session on "Value Systems and Human Fertility," attacked Father Gibbons, or the Roman Catholic position, sharply at the session. What do you remember of that confrontation?

TAEUBER: The mention of Kingsley Davis's attack on Father Gibbons arouses only the picture of a young, unusually fervent person attacking a mild-mannered priest who had stated the position of his Church. Kingsley was not then noted for mild-mannered behavior, nor would that be listed among his strong points today.

QUESTION: What do you see as the outlook for PAA?

TAEUBER: The field of demography has grown so rapidly that the Association has had to change its character. The Association has grown rapidly, which is a reflection of what's been happening at universities and research institutions and government agencies around the country and undoubtedly

this will continue. And I think the Association will need to continue to make room for the wide variety of viewpoints of people dealing with population. After all, we're talking about people and human beings have a wide variety of interests.

QUESTION: Although PAA has grown, its membership is still only about 2,700 [2,752 at the end of 1990]. That is small compared with other professional organizations. Do you feel PAA should actively recruit members among, for example, business demographers?

TAEUBER: The rise of the role of demography in the business world is probably only the beginning of a movement that will grow rapidly. It has its dangers to the field and PAA will need to watch this carefully. PAA has not worked out an effective, judicious pattern of relationships. Without full-time professional leadership, there will be less effective action by PAA than would be desirable.

PHILIP M. HAUSER

PAA Secretary in 1942-46 (No. 5) and President in 1950-51 (No. 14). Interview with Jean van der Tak at Dr. Hauser's home, Chicago, November 12, 1988.

CAREER HIGHLIGHTS: Philip Hauser was born in 1909 in Chicago, where he still lives. He received all his degrees in sociology and all from the University of Chicago: the Ph.B. in 1929, the M.A. in 1933, and the Ph.D. in 1938. He was at the Census Bureau in Washington from 1938 to 1947, as Assistant Chief Statistician in the Population Division until 1942, Assistant Director of the Bureau to 1946, and Deputy Director in 1946-47. He returned to the University of Chicago as Professor of Sociology in 1947. However, when the Director of the Census Bureau died in 1949, he was asked to serve as Acting Director to complete the planning of the 1950 census, which he did on a part-time basis in 1949-50, commuting from Chicago.

On his return to the University of Chicago in 1947, Dr. Hauser also became the first Director of the Chicago Community Inventory, later called the Population Research and Training Center and then the Population Research Center. He remained Director of the Center until his retirement in 1979.

Among many other posts, he was the first U.S. Representative to the United Nations Population Commission, from its establishment in 1947 to 1951. He has been a consultant for the UN and other organizations in many countries of the Third World, including Thailand, Singapore, the Philippines, Burma, Malaysia, Indonesia, South Korea and other countries in Asia, Africa, Latin America and the Middle East.

He was chairman of a committee at the East-West Center in Honolulu which developed the terms of reference leading to the creation of the Population Institute at the Center. Besides the Population Association of America, he has been president of the American Statistical Association (1962), the American Sociological Association (1967-68), the Sociological Research Association (1961), and the National Conference on Social Welfare (1973-74). His publications include 32 books, chapters in 50-60 other books, and over 500 articles.

VDT: What led to your interest in demography?

HAUSER: In 1929-30, after I had my bachelor's degree from the University, I accepted a teaching assignment at what was then the Central YMCA College of Arts and Sciences, now Roosevelt University. The course I was asked to teach included one on population. I had never had a course in population. I taught the course relying largely on Warren Thompson's textbook [[Population Problems](#)]. He was the dean of demographers in the U.S. at the time. I managed to get along reasonably well by spending enough time to stay at least two or three weeks ahead of my students. A year or two later, I had my first course in population, taught by Professor William Fielding Ogburn--a graduate course at the University of Chicago.

After his course, I became very much interested in the field and in its various related areas, including economic demography, social demography, methodological demography and so on. However, I had no intention at the time of becoming specialized in demography. I should point out that in 1932, when I began teaching at the University of Chicago, I was assigned to teach in the College as well as the Department of Sociology. Included in the college curriculum was a section on demography, which I was very happy to teach. In 1934, having spent the summer in charge of an office receiving and editing schedules for a survey being done by what was then the Federal Emergency Relief Administration (FERA), which later became the Works Progress Administration (WPA), I was invited to join the research division of FERA in Washington, on leave of absence from

the University. I went to Washington intending to spend only a year there. Actually I stayed until 1936. I came back to the University of Chicago in 1937 and 1938 as a faculty member, during which period I finished my work for a Ph.D.

While in Washington, I served as assistant chief statistician of a survey ordered by the Congress on employment, unemployment, and partial employment. Other people working on that survey included some from the Bureau of the Census.

Through an interesting happenstance, after returning to the university in 1937 I got into a conflict with the President, Robert M. Hutchins. President Hutchins abolished the rank of assistant professor, to which I had been promoted with an increase in salary. As a result, I accepted an offer to come to the Bureau of the Census as Assistant Chief Statistician of the Population Division. My first assignment was to help plan the 1940 population census.

VDT: You must have been the leader of the "class of 1940" that I've heard so much about--the group of demographers who arrived about that time.

HAUSER: Right. As a matter of fact, as Assistant Chief Statistician, I had a hand in employing many of them. Now, finally, a direct answer to your question. I regret it took so long to tell you.

VDT: Exactly what I want!

HAUSER: The answer is that I then spent about ten years in the Bureau of the Census, with responsibilities spreading beyond the population census. With a staff, I helped develop many of the innovations in the 1940 census. But the point is, after working about ten years in the population field in the Bureau of the Census, that became my specialization. I had assumed I would teach somewhere and I was prepared to go into research in social psychology, in criminology, two or three other sub-fields of sociology; I had not made up my mind. But the job in the Bureau of the Census determined my field of specialization--demography.

VDT: It must have indeed. Frank Notestein in his last article, published just before his death in Population and Development Review in December 1982 ("Demography in the United States: A Partial Account of the Development of the Field"), wrote that he was writing about the part of the development of demography that he knew about. He said that if Hauser or Con Taeuber or Art Campbell were writing it, they would write it differently because they would know more about the contribution of government agencies to the development of demography in the United States.

Tell me a bit of the importance of the Census Bureau in the development of U.S. demography. For instance, you said there were some innovations in the 1940 census. I know the 1940 census wasn't used as much as it should have been because it came out during the war. What were those innovations--some you were responsible for?

HAUSER: Well, I was among those who made the decisions and I would say it was a shared responsibility. My boss, Dr. Leon Truesdell, was in a position to approve everything. Being of the old school, not trained as a demographer or statistician, Dr. Truesdell, nevertheless, was open-minded and accepted most of the innovations that I and the staff proposed. Staff members included Henry Shryock and John Durand, for example, and a large number of others; we worked as a team. The innovations we introduced and for which I guess I could take major professional responsibility included such things as the substitution of a question on internal migration for questions related to birth abroad and international immigration.

VDT: The question "Where did you live five years ago?".

HAUSER: Yes, that was one of the innovations. Then there was the use of sampling of subjects which could be covered by the census, because by having a relatively small sampling of the population who were asked additional questions, it became feasible within the limit of our appropriations to tabulate the additional data.

VDT: The long form?

HAUSER: Right.

VDT: What was the sample size that year, one in what?

HAUSER: You're straining me; that's now over 40 years ago. I guess it was about one in 20. It was the first time that was done.

Another innovation was a substitution of a question that made it possible to get better quantitative data on education--years of school completed instead of the old question on literacy; whether the respondent was literate or not. Incidentally, you may be interested to know that on my desk there were two letters signed by the Commissioner of Education, one of which said in effect, by all means adopt this new question on years of school completed, even at the expense of getting rid of literacy. The other letter took the position that we should not stop asking the literacy question, even if we could not ask the years-of-school-completed question. Two different divisions in the Office of Education had written these letters and the Commissioner had signed both of them.

VDT: You decided which made better sense and chose it?

HAUSER: Yes, the conflict enabled us to make up our own minds.

Other innovations. We introduced the "class-of-worker" for us to identify whether the person at work was an employee, own-account worker, government worker, or an unpaid family worker.

Among the most important innovations was the adoption of something we had experimented with in that survey of 1937 that I mentioned earlier. We adopted what has become known as the labor-force approach to the study of the working population, which made it possible to study employment and, for the first time, unemployment in a way that could provide usable data. The Bureau of the Census had experimented with trying to get unemployment figures on previous occasions but without success; they weren't worth publishing.

VDT: You used the question "Were you looking for work last week, or last month?"

HAUSER: Well, there were a series of questions and the order of the questions indicated what classification we could then adopt for determining employment and unemployment or, for that matter, at that time we had to make provision also for persons doing WPA work. The first question, yes, was "Were you at work or doing something else last week?" In that approach we did what had never been done before--set a specific time referent, the week prior to the census, as a basis for determining just what the person did during that time period. Prior to that, in the 1930 census for example, one was asked his occupation. A reply such as "railroad engineer" became defined as someone who was a gainful worker. But the man who was a railroad engineer might have been retired for 15 years. This had no relevance to the labor market situation in a specified time period. The new approach primarily gave us a fixed time period based so far as possible on what was actually being done. It led to a

complete redefinition of a workforce of persons who were either employed or seeking employment during a specified time period. It was approximating, as best we could, the actual labor market situation.

VDT: Was that on the sample long form?

HAUSER: No, that was on the 100 percent form.

VDT: Of course that was during the Depression and there must have been the urge to get unemployment figures.

HAUSER: Exactly. And also we wanted it because this was the anchor around which we got occupational and industrial information as well as class of worker--which, incidentally, the United Nations calls industrial status, that is, worker employee, private account or whatever.

VDT: You must have had nothing but encouragement from Congress in 1940. There was no opposition to what questions were being asked in the census?

HAUSER: Right. Well, there was one other innovation which created a bit of a stir and that was we asked money income. That was the first time the income question was asked, although there were questions relating to such matters in earlier censuses. Abraham Lincoln, for example, had to report the value of his property. Back in the 1860 census they began to get information about wealth and economic information. But there was quite a stir. A Congressman named Tobey created real controversy and advised people not to give the answer on their income because he thought it was a violation of privacy. In fact, because of him we had to publish separate materials so that a person who did not want to reveal his income to the enumerator was able to report it directly to Washington.

VDT: And very few were used; I read that somewhere.

HAUSER: About a hundred thousand. We printed hundreds of thousands of the forms and only a small fraction of them were used.

VDT: Turned out that people didn't mind telling it.

HAUSER: That's right.

VDT: Did you have something to do with the Current Population Survey, which, of course, became the vehicle for collecting labor force information?

HAUSER: Very definitely. I was party to the development of the Current Population Survey. I was still then Assistant Chief Statistician in the Population Division. But we worked closely with a research division in the Bureau led at the time by Morris Hansen. He was partly responsible for the actual sampling technique, including the area sampling approach.

VDT: That had started already--the monthly survey?

HAUSER: Well, what happened was this. John Webb, who was a statistician with the FERA/WPA at the time I was--we had overlapped--had done a survey trying also to get a count of the unemployed.

The thinking that he put into it was useful to those of us who decided to use the 1937 Census of Employment, Unemployment, and Partial Employment with a new approach to measurement of the workforce, abandoning the gainful worker approach. Instead of asking the occupation question without time referent, the question we asked in that 1937 enterprise was "Were you at work last week?" or "Were you seeking work?" Now, there were some changes of one kind or another but that approach used in the 1937 survey was adapted for use in the 1940 census.

Webb had also done a current sampling survey and after the 1940 census those of us who were doing the planning began to think of a monthly current population survey, the major purpose of which was to get the monthly count of the employed and unemployed. In addition, we had in mind a rotation of questions, so in effect we could update census information--the decennial census information--all during the decade.

VDT: What a tremendous contribution that was because that is the source of demographic information between censuses. But, of course, when Janet Norwood announces the unemployment figures every month, the whole U.S. hangs on her words.

HAUSER: Yes. The Bureau of the Census used to announce it. The Bureau not only collected the data but used to announce the results. It was switched to the Department of Labor--the Census was given some things, the Bureau of Labor Statistics was given others, including the privilege of announcing unemployment figures. But the survey is still done by the Bureau of the Census.

VDT: It must have been exciting in those days.

HAUSER: It was.

VDT: Tell me about it. You would go back and forth to each other's offices with these great ideas?

HAUSER: Oh yes, we'd have staff conferences. We introduced another kind of thing; we not only developed new methods and new questionnaires but we tested them, in advance of the census.

VDT: The pretests?

HAUSER: Yes, we brought that in before the 1940 census; first time that was done, to my knowledge. We had two kinds of pretests. One was just going down a corridor or on a street corner without any respect to sampling, asking the questions, largely to determine whether or not people could understand what you wanted and whether the response was of the type you intended to get. We called that the "hot-house approach." The other was a pretest based on a scientific sample.

VDT: A city or a rural area?

HAUSER: Right, we did both.

VDT: In those days, of course, there were enumerators who came to your house and asked questions.

HAUSER: That's right; not mailback. Mailback came in as one of the innovations with the 1950 census, when I was Acting Director, after the Director, J.C. Capt, died.

VDT: You stayed just about a year at that time?

HAUSER: It was more like a half year, through the period of getting the schedule frozen and the procedures worked out, after which a political man was brought in to be the Director, appointed by the President.

VDT: Peel.

HAUSER: Incidentally, after Peel resigned, I was asked whether I wanted to become the Director. But after ten years in Washington, I had decided I much preferred my professorship; wanted to get back to the university.

VDT: I understand that one reason you were asked to be Acting Director at the Census was that you were considered a "wunderkind" in the late 1930s.

HAUSER: Yes.

VDT: That's the word Norman Ryder used when I interviewed him. And you were a protege of Henry A. Wallace, who was the Secretary of Commerce.

HAUSER: I was not his protege. On the contrary, when I was Deputy Director of the Census, for reasons I've never learned, I got a call from Henry Wallace one day to come up and converse with him. He insisted that I become Assistant to the Secretary of Commerce for Policy. I was still Deputy Director of the Census and had both titles, but I had already become Deputy without any Wallace connection. I might conceivably have stayed at the Bureau indefinitely, but having gotten into the political climate with Wallace, there came a time when I definitely wanted to get out of Washington.

VDT: You did take that position, as Assistant for Policy to Wallace?

HAUSER: I had no choice; he was my boss. I had the two positions simultaneously. But that's another story.

VDT: But you admit that you were called a "wunderkind"?

HAUSER: Well, no one ever called me that to my face. But let me tell you something that is relevant. Dorothy Thomas was a consultant to the President's Committee on Economic Security, of which Professor Witte of the University was Chairman. When I joined the FERA IN 1934, as my first assignment I was detailed to become a staff member of that committee and got to know Dorothy Thomas. I was somewhat taken aback by a letter she wrote to the Secretary of Commerce after I had accepted the position of Assistant Chief Statistician of Population in the Bureau of the Census in 1938. In accordance with bureaucratic routine, the letter landed on my desk for response over the signature of the Secretary of Commerce. What the letter said, in effect, was it was a great pity that such a terrible mistake had been made in appointing to this position a young person still wet behind the ears instead of a qualified professional. So you see, Dorothy did not regard me as a "wunderkind."

VDT: Great! You were pretty young, still in your twenties. Who had that tremendous faith in you; just knew you'd be good?

HAUSER: Well, I was interviewed by Truesdell and he decided I could do it. My record was good.

VDT: You dazzled him with your knowledge of statistics?

HAUSER: No. I'll tell you another story; I'm not sure you want to print this. I had lunch with Truesdell in the official dining room at the Department of Commerce for my first interview. He was a taciturn New Englander; he rarely said much more than "yep" or "no." The first question he asked was, "Do you have any children?" He knew from the form I filled out that I was married. And my answer was, "I don't know." He looked at me strangely and said, "How can you say that?" And I said, "I came in for this interview leaving my wife in labor. At this point I do not know, literally."

Well, he actually grimaced; it was almost a smile. Then he asked a few other formal questions and we sat there looking at each other. The silence became difficult for me--I'm a little prolix, I'm afraid. Finally I broke the silence by asking him whether he had ever heard of such-and-such and he said no and I told him what was, by any criterion, a filthy story--the kind men told to each other before you women got emancipated; now we tell them to you too. He heard the story--presumably a good story; I can't remember what it was, but it would be obscene. And he sat there with an unchanged, glum expression; the silence seemed to last forever. I was thinking to myself, "Well, I guess I've blown it; this is the end." He finally turned and said, "Dr. Hauser, do you know more stories?" And I spent the next half hour or so telling him one filthy story after another. Before it was over he was laughing, which, I'm told, wasn't easy for him.

VDT: Wonderful! (Much laughter)

HAUSER: After that we got into the specifics of the job. When I first started to work with Truesdell, I would dash off memoranda on plans for the census. I would get back the memoranda with blue-pencil marks correcting my grammar. He would correct my memos even after he had decided not to accept the proposals. Needless to say, I was not happy about the blue penciling. Having had a couple of years of Latin in high school, I was very grammar conscious. I confess that to brush up on my grammar, I consulted an English grammar textbook--I forget which one. In due course, I received a memorandum from Dr. Truesdell which contained some grammatical mistakes. I could not resist the temptation to blue-pencil his memorandum, which I returned to him. After that, I never again got a memo of mine blue-penciled.

VDT: You were a brat wunderkind!

You mentioned William Ogburn, who was your professor at Chicago. He was one of the few people working in the population field at that time.

HAUSER: Yes, from whom I got my first course in population, as well as two or three courses in statistics. Will became a colleague later and a good friend when I was on the faculty. In 1932 when Will took a term off on leave of absence, Sam Stouffer taught a course in statistics and Sam was interested in some of the demographic methodology; he also became a good friend of mine. When I came in to begin my graduate work in 1929, Sam was finishing up as a Ph.D. and we became good friends as well as classmates. Sam went into demographic methodology more thoroughly than Will had, so I got a little more out of that.

VDT: I know that Ogburn was more a broad-based sociologist.

HAUSER: Right--a pretty good statistician, but he didn't go into the details of construction of life tables or stable population theory, etc. I picked that up partly with Stouffer and partly with the reading

I did after that.

VDT: Self-taught!

HAUSER: Well, yes. If you get a good foundation it's easy to build on it.

VDT: Now, when you came back to the university . . .

HAUSER: I came back in 1947 and then went out again for six months to be Acting Director of the Census Bureau and never had a leave of absence from the university. I was teaching my courses on Saturday, flying back and forth for six months, which demonstrates how stupid people can be.

VDT: You decided you really wanted to come back to academic life, that you'd had enough of political life in Washington.

HAUSER: Right. Henry Wallace got fired for giving a speech I helped to write.

VDT: You're pointing up to your pictures of all the distinguished people. [The photos included Henry Wallace, Averill Harriman and Harry Truman.] Was Harry Truman a friend?

HAUSER: No, but I testified before his Senate committee a number of times and got to know him. And I worked some with his assistant Clark Clifford, now a very distinguished lawyer in Washington. That leads to a lot of other stories, but irrelevant for your purposes.

VDT: Right, now back to Chicago. You said the Population Center already existed at that time.

HAUSER: What happened was that I'd been interviewed in 1946 by Professors Ernest Burgess and Louis Wirth. They had received a 60 or 70 thousand dollar grant from the Wieboldt Foundation. They used to have a big department store, but they're out of business now; they had their own foundation. The grant was to do studies of the Chicago area. The title they gave to this enterprise was the Chicago Community Inventory. They recruited me to come back as a professor with the understanding that I would become the director of this enterprise. They indicated that they would not do much until I arrived in 1947. So when I came back in 1947, this organization and money were waiting for me.

Then in due course my own interests were wider than just studying the Chicago metropolitan area--I'd kept up doing demographic studies about the United States and then increasingly the world. We still did the Chicago thing but changed our name after some years to the Population Research and Training Center and finally to the Population Research Center. By then I was getting money from various sources, the most important of which was the Ford Foundation--we'd get government grants too--that enabled me to provide fellowships to students from abroad.

VDT: Why did you turn abroad?

HAUSER: I was interested in national demography, world demography, and what was clear was that the most severe population problems were in the Third World. But in the Third World there was practically nobody who could be called a demographer, who could begin to provide the basic data for policy, which they badly needed. So I began to give fellowships to a number of Asian, Latin American, and eventually African students, whom we trained to be demographers to study the situation in their own countries.

Along the way, the Ford Foundation created a committee of which I was chairman to explore the situation in Asia and to make recommendations in regard to the possible funding of demographic and family planning projects. Other members of the committee were Oscar Harkavy and Dudley Kirk. The committee, together with Oscar Harkavy's wife and my wife, traveled through Southeast Asia, Indonesia, Ceylon, and India [in 1963]. The purpose of the trip was to explore the prospects for setting up programs for demographic research and family planning and to make recommendations to the Ford Foundation on where and how monies might be dispersed. Once we had produced a Ph.D. with demographic specialty, they would fund a population center in that person's home country.

VDT: There's a reference to this in the book by Jack and Pat Caldwell, Limiting Population Growth and the Ford Foundation Contribution (1986, p. 74). When did you manage to accomplish that?

HAUSER: The first Ph.D. student that led to a center being created was Dr. Mercedes Concepcion; a center was set up at the University of the Philippines in Manila in 1964. She was the Director and with subsequent reorganization, she became Dean of a separate unit of the university--Dean and Director of the Population Institute of the Philippines.

The second one that I had a hand in founding was the Population Institute at Chulalongkorn University in Bangkok, Thailand. This was funded by the Population Council and the Ph.D. who took his degree here was Visid Prachuabmoh. Interesting footnote: His daughter has just passed her prelims and taken an M.A. at the University of Chicago and is working on her Ph.D. dissertation. She'll go back to this institute as one of the staff, eventually maybe director there. Lovely girl.

VDT: Is he still the director, her father?

HAUSER: Unfortunately he died from leukemia in his forties.

VDT: Sad. How nice that the daughter carries on the tradition.

HAUSER: Then a similar combination of things led to the creation of the population center at the University of Indonesia. The first director was Dr. Iskander, who was not trained at the University of Chicago but at Princeton. The population center in Indonesia was one of the products of the committee's work.

VDT: You obviously went to visit or be an adviser at all these different institutes that began. Did you go several times to the population institutes at the University of the Philippines and Chulalongkorn, Indonesia--and also India and Pakistan?

HAUSER: Many times, to all of them. And for that matter, I also visited population institutes in advanced countries. For example, a number of times I visited the population institute in the Ministry of Health in Tokyo.

VDT: Your first trip to Asia, with the committee for the Ford Foundation, was about 1963?

HAUSER: Right. Harkavy was already working for the Ford Foundation. He became the key person in the disbursement of monies for population purposes.

VDT: The first population centers you mentioned are spinoffs from your original population center?

HAUSER: Right. Then there are many others in which I didn't have quite as direct a relationship as those.

VDT: Did you go for a month or two at a time in these different places?

HAUSER: Most of the time, with one exception. In Burma I spent a year, in 1951-52. That was for the UN. I was there as a statistical adviser to the government of Burma and also the government of Thailand. I was resident in Burma but Thailand was less than an hour away by airplane. I did many things there, some definitely relating to demography, but just as much relating to the development of statistics and census activity as well.

I've been to over 70 countries and in most of them I either was a consultant for population matters or census and general statistical matters, survey matters, or gave lectures at the universities, or in some cases, did all three of them, in the same country. I did so over 35 years.

VDT: Well, you obviously were operating many strings. I'd like to read you a quote from my interview with Norman Ryder (Princeton, May 11, 1988) about you and Frank Notestein.

HAUSER: I'd be glad to hear it; Norman is a good friend.

VDT: He said: "Philip Hauser, along with Frank Notestein, was an extraordinary example of the importance of what you might call a research entrepreneur. These people seem to me to have been of extraordinary importance, extraordinary as organization creators, extraordinary as the people who managed to inspire research funds to come in our direction, and people who played a major role in shaping the field" and he said not just by your own industry and creativity but also by your choice of individuals, your students and fellow faculty members--giving these very able people room and opportunity to do what they do best.

He was speaking in particular at one point about your bringing in Otis Dudley Duncan. Could you talk a bit about your colleagues at Chicago like Duncan and Nathan Keyfitz, as well as some of your leading students.

HAUSER: Dudley Duncan was just finishing his Ph.D. when I returned in 1947; I was on his dissertation committee. He developed into, and I think still is--although he's retired now--the best scientist in sociology, including demography. I say that with conviction. He is an absolutely superb researcher and methodologist, and much of it through dint of his own effort and concentration and personal development, more so than through formal training, though he had enough formal training on which to build. I think his chief professor was Ogburn and he's written some fine things about Ogburn, including a book on Ogburn in the University of Chicago monograph series.

Dudley and I were collaborators--with the first large monies we had for research from the Air Force. After the war, I had occasion a number of times to visit Maxwell Field in Montgomery, Alabama, where the Human Resources Institute was located. The head of the institute was Ray Bowers. I was consultant to that institute. In connection with the questions they had raised, they provided funds with which we did research. Duncan and I wrote a paper summarizing our Air Force activities. Dudley did much of the methodological work on that series of studies and helped develop segregation concentration and other indexes. What the Air Force was interested in was how they could get better quantitative statements of population concentrations for both defense and bombing purposes. Defense was for the U.S.; the bombings were for other countries. We worked on the U.S. part and did a number of studies for them.

Dudley got married to Beverly Davis, who became one of my students and took her Ph.D. She

died just this past year. Beverly was a superb student and a superb researcher. We did one book together called Housing a Metropolis: Chicago (1960). She and Dudley were one of the best research teams that ever existed in the U.S. or, for that matter, elsewhere. They did beautiful work. They published a book, The Negro Population in Chicago (1957), which was a trailblazer in many ways. It indicated that Chicago was probably the most segregated large city in the northern United States. And it stimulated other students to do the same--Karl Taeuber, for example. He took his degree at Harvard but he spent two or three years at the Population Research Center doing his dissertation. And he got married to Alma Ficks, who was also one of our students; she took her Ph.D. here. And they're both doing yeoman work at the University of Wisconsin.

VDT: Tell me about Nathan Keyfitz.

HAUSER: Nathan Keyfitz was a unique character. I can say he was one of my students but I have to qualify that by pointing out that he had an international reputation as a top statistician in the Bureau of Statistics in Canada before he came to Chicago to do his Ph.D. He had made significant contributions, statistical and demographic, to the census operations of Canada. And late in his life--I guess you'll have to check it out; he was then in his forties, fifties--I think it was in the 1950s that he came here.

VDT: He got his Ph.D. here in 1952. He's offered to do an interview tape on his own in Indonesia, where he's just returned; I'm frustrated I can't be there [see Keyfitz interview of December 31, 1988]. He continues to divide his time between Indonesia and the International Institute for Applied Systems Analysis in Vienna.

HAUSER: I've been in correspondence with him in Indonesia. We had in common a man who became very powerful in Indonesia, adviser to the President, with whom Nathan still works (Widyoyo).

When Nathan came, he was already an international statistical and demographic authority, but he didn't have a Ph.D.; I was on his thesis committee. [Keyfitz did his Ph.D. course and residence requirements at the University of Chicago in the early 1940s and returned in 1952 to defend his dissertation.] I want to tell you that the word from the graduate students here was that they wondered whether any of his faculty members were able to understand his dissertation. He used advanced statistical techniques, analysis of variance, in doing his dissertation and it was a first-rate piece of work. Nathan could always do first-rate work. So when I say he was one of my students, you've got to take that with qualification. He had already arrived and the Ph.D. was just frosting on the cake.

VDT: How about others of your leading students that you're proud of?

HAUSER: Let me deal with the American ones first and then the foreign ones.

VDT: You already have a list, under your wonderful reading machine. This one is: "Outstanding Students--American."

List prepared by Philip Hauser, November 12, 1988:

DUNCAN, Beverly	1957	WINSBOROUGH, Halliman	1961
FARLEY, Reynolds	1964	EPTING, Gladys	1982
GALLE, Omer	1968	LI, Angelina	1980
GRONBJERG, Kirsten	1974	KITAGAWA, Evelyn	1951
HENDERSHOT, Gerry	1970	MASON, William	
COSTELLO, Michael	1977	TSUI, Amy Ong	1977

LIEBERSON, Stanley	1960	HODGE, Robert W.	1967
MERTENS, Walter	1966	CLOGG, Clifford	1977
MUGGE, Robert	1957	SULLIVAN, Teresa	1975
SHELDON, Eleanor	1949	MATRAS, Judah	1962
XENOS, Peter (a.k.a. Smith)	1970	KEYFITZ, Nathan	1952

HAUSER: I've done no ranking.

VDT: No. Oh my, look at them. Beverly Duncan at the top; we've already talked about her. There are Reynolds Farley, Omer Galle, Kirsten Gronbjerg, Gerry Hendershot, and so the list goes on and on.

HAUSER: Well, these were among the best ones.

VDT: Nathan Keyfitz is at the bottom.

HAUSER: He was an afterthought. I can claim him technically as a student, but I've already qualified that. I would say one of the best was the name down the list and that's Evelyn Kitagawa.

VDT: Tell me about her. I understand that you are the godfather of her daughter.

HAUSER: Yes. My goddaughter, by the way, having received offers of scholarships for tuition plus \$7500 living expenses from Harvard, Yale, Chicago, and Princeton, is now at Princeton studying art history, with special reference to Japanese. She refused all those scholarships because she received a Mellon fellowship which she could use for three years of graduate work at any institution she selected, with tuition plus a \$10,000 per year stipend. Imagine that!

VDT: Wow! Did she do her undergraduate work at Chicago?

HAUSER: No. She was a student here at high school, the Laboratory School, and grade school, but she did her bachelor's degree at Oberlin.

VDT: In art history?

HAUSER: Yes, she had a double major. She's brilliant and beautiful; amazing woman.

VDT: Is she an only child?

HAUSER: Yes. She was born to Evelyn when Evelyn was in her forties. Evelyn was one of my students; she finished in 1951. She came with her husband, Joseph, who is also an amazing person. He's an Episcopalian priest. Born in Japan, but he came over here to do some studying and was entrapped after Pearl Harbor and put in a concentration camp. That's where Evelyn met him; she was working for the agency that was running the concentration camps. Romantic.

She came to Chicago. She had been a math major at Berkeley, California; bright as a whip. Became interested in demography and I was able to give her a fellowship. She went through everything that we had to teach her, absorbed it all, and was among the top half dozen, I think, that I had occasion to train. Top half dozen, among other things, in methodology, utilizing her math background. She is an excellent methodologist. She became Associate Director of the Center and in

due course, I was even able to persuade the Department of Sociology that it was not undesirable or immoral for a female to become a member of the faculty.

VDT: Was she first female on your faculty?

HAUSER: No, I don't think she was the first, but it was very rare. I developed a technique that made it work pretty well. She was Assistant Director of the Population Research Center under each of the titles that we had and listed in the faculty as a Research Associate of the faculty and, I think, with some professorial rank. Eventually, we were able to put her on the faculty as full professor. She became my successor as Director of the Center. She's done superb work. We did one monograph together.

VDT: The groundbreaking book on Differential Mortality in the United States: A Study in Socioeconomic Epidemiology (1973).

HAUSER: Right. Great methodology there. We both were creative but I think she did the dirty work to get it through. It's one thing to be creative; it's another thing to implement it. She did the implementation, and that's why she got the senior authorship on it. It was a very good study.

VDT: Charlie Nam told me that he and Lillian Guralnik developed the census record-matching system that you used, in a pretest for the 1960 census.

HAUSER: Right, that we worked on. I had a grant of \$1,017,000 from the National Institutes of Health. It was a lot of money at that time, but I'll tell you something interesting. This was a study that the National Center for Health Statistics wanted to do and the Congress would not appropriate the money for the study. And so the grant we got, we gave most of the money back to the Bureau of the Census and the National Center for Health Statistics for working up the confidential census data for us.

VDT: Private enterprise really helping the government! The money came from the government and it got laundered through you.

HAUSER: It got laundered through us back to the government. And Evelyn is, of course, professor at one of the best universities, one of the best research centers.

VDT: What about Reynolds Farley, who is just finishing up his tenure as PAA President?

HAUSER: He was one of our students; did a lot of his work with the PRC in demography. For a while, was he not head of the Michigan Population Studies Center? Another student of mine, William Mason, is now head of that center, of which Ron Freedman was first director. I used to half claim Ron Freedman as a student too. He was finishing up when I came back in 1947 and listened to some of my lectures.

Beverly Duncan and Omer Galle became academic professors of the highest quality in first-rate institutions. Eleanor Sheldon became president of the Social Science Research Council. She was well trained by us at the University of Chicago.

VDT: Great. And Amy Ong Tsui, now down at Chapel Hill; she worked a long time with Don Bogue.

HAUSER: Right, and Don Bogue can probably claim her as a student. Actually, it's a funny thing, all of them were students not of my center or his center but of the Department of Sociology and most of

them had courses with both of us. The centers didn't do any teaching; the centers . . .

VDT: I want to ask you about the logistics of those two centers. And the last American I'll ask you about is Teresa Sullivan, now at Texas.

HAUSER: A superb student with a flair--she's got one of the best abilities to write of any student I ever had. And since she's been at Texas, they've also made her head of a women's study program and she's also current editor of the Sociological Monograph series, the Rose monograph series.

VDT: Now, let's look at your outstanding foreign students.

List of Outstanding Students--Foreign, prepared by Philip Hauser, November 12, 1988

ADEWUYI, Alfred	1977	Nigeria
ANDERSON, Patricia	1977	Jamaica
FLIEGER, Wilhelm	1967	Philippines
CHO, Lee-Jay	1965	Republic of Korea
CONCEPCION, Mercedes	1963	Philippines
JILLANI, M.S.	1962	Pakistan
MISRA, B.D.	1965	India
MISRA, J.K.	1969	India
CHANG, Chen-Tung	1973	Singapore
PRACHUABMOH, Visid	1966	Thailand
SUJONO, Harjono	1972	Indonesia
HASHMI, Sultan	1970	Pakistan
BAGCHI, Sourendra	1972	India
GARCIA, Maria del Pilar	1978	Venezuela
SIRIPAK, Wiwit	(M.A. 1965)	Thailand
KONO	(short-term)	Japan
COSTELLO, Marylou		Philippines
PIAMPITI, Souvaluck	(M.A.)	Thailand

HAUSER: Lee-Jay Cho would be the first one I'd pick, perhaps the one who has become the most prominent. He is now Vice-President of the East-West Center and Director of the East-West Center Population Institute.

VDT: And Mercedes Concepcion, you've mentioned her.

HAUSER: Yes, she became, as I say, the Director and then Dean of the Population Institute in the Philippines. A very brilliant person and a profoundly good speaker and good thinker. She's had a lot of impact in the international scene, not only in Manila but through a couple of different organizations, some of which we jointly organized. We had something called the Organization of Demographic Associates (ODA), that included people who were heads of institutes in Asia; it's dead now. She's had terrific influence. She's also represented the Philippines on the UN Population Commission.

VDT: And been President of IUSSP.

HAUSER: Right. She and Lee-Jay Cho have probably had the most distinguished careers.

VDT: There's Haryono Sujono of Indonesia. I didn't know he'd studied in the U.S.

HAUSER: Oh, yes. He's now head of the family planning organization in Indonesia [BKKBN]. He did most of his work with Don Bogue, but he had courses in demography with me as well.

Don, may I say--you'll be talking with him--has a mission. We've had something less than completely congenial relationships over the years. He's had a tendency to feel that any student that worked with him in his center was that center's student, period. And the fact is that's not true. The students were students in the Department of Sociology; that's where they got their degrees. And they took courses with anybody in the department, including not only Don but me and other members of the department. So that in a sense, anyone with whom they took courses could claim them as students. There are probably over a hundred students of the Department of Sociology who had training in demography now distributed throughout the world.

VDT: Now that we're on to Don Bogue, let me just ask you why he split off. I suspect that he decided that family planning research was a separate branch. I re-read or read for the first time, on the plane coming here, your rather critical review of his article in the Berelson book (Bernard Berelson et al., eds., Family Planning and Population Programs, 1965, reviewed by Philip Hauser in "A Book Review Article," Demography, Vol. 4, No. 1, 1967, pp. 397-414). You were quite critical of Don Bogue's approach to family planning at that time, or at least the idea that family planning research was a separate . . .

HAUSER: Well, let me put it this way. I brought Don to the department on the basis of his published research. When he first came in, I made him Associate Director of the Population Research Center. But when the opportunity came to get a center of his own, he immediately took that over--the Community and Family Study Center. The man who had started that left for other. . .

VDT: I did not realize that Bogue did not start it; that it existed already.

HAUSER: It had previously existed. But he changed its character completely and used part of the name--the "family"--but went increasingly into the family planning thing. Now, as I saw Don, in due course he was paying 100 percent attention to that, doing nothing in relation to the Population Center.

As I see Don--a very able, very competent person, and probably the most prodigious worker that I have run into; day and night.

VDT: Next to you.

HAUSER: Well, I don't think I ever matched Don in that respect.

He became what I regard as a crusader and his crusade often led him into what I regarded as irresponsible situations. One of the things he published, for example, was an article called "The End of the Population Explosion" (1967). Have you read that?

VDT: Well, "Declining World Fertility" (Population Reference Bureau, October 1978) came out of that and I was the editor of that Population Bulletin. And we got into trouble, even though I spent hours on the phone trying to persuade Don Bogue to tone it down.

HAUSER: That's right. I think that he projected in 1967 that the population of the world in 2000 would be four billion something [4.527 billion--revised upward to 5.8 billion in the 1978 Population

Bulletin]. He was ignoring everybody else's projections.

VDT: It was way off.

HAUSER: Right. It [current projections for 2000] still is the 6 point something billion that it was at the time he was saying it would be four plus billion.

I've never felt anything but friendly toward him; I've got no animosity. And I was just as happy to see him go his own way, because I could not go along with what I regarded as extreme crusading efforts that had, as a matter of fact, no place in a department of sociology at the university. What he was doing should have been part of a separate family planning agency. That's not a function of the Department of Sociology at the University of Chicago. And the administration saw it the same way.

VDT: You did point out in that review article that when he got around to evaluating projects, it was not a good idea for the program director to do the evaluation. In that case, you talked about the impact of the Chicago experiment regarding black--Negro--fertility. You pointed out that the results were quite incorrect.

HAUSER: Right. But I think he's made a tremendous contribution. He's just now become emeritus, you know.

VDT: Yes, but he's hardly retired. He's got his own Social Development Center.

HAUSER: In any case, the new Ogburn-Stouffer Center, which includes the Population Research Center, contains the scientific research part of what was his center [Community and Family Study Center] and is now being overseen by Marta Tienda.

VDT: They've just done that in the newly opened quarters? [New center for population, etc., officially opened on the University of Chicago campus, November 11, 1988.]

HAUSER: Right

VDT: And you've given some of your books to them?

HAUSER: Two or three days ago, I gave them a copy of each of my 32 books and "over 500 articles." They have not only the books I've written or edited but also another 50 to 60 books in which I have a chapter. They're all in a special case at the center. These were in addition to approximately 6,000 books which I made available to the University.

VDT: Wonderful! There are few institutions that can point so definitely to one man who started it all. How about the United Nations? Could you say a bit about your time there, as the first U.S. Representative to the Population Commission (1947-51).

HAUSER: I summarized my experience there in an article in a special issue of the UN Population Bulletin [Philip M. Hauser, "The Early Years of the Population Commission, Population Bulletin of the United Nations, Nos. 19/20, 1986, A Special Issue in Commemoration of the 40th Anniversary of the Population Commission, pp. 2-5].

VDT: Tell me something that you might not have said in the article. I've heard from Ansley Coale,

who followed you as U.S. Representative, that it was very divisive in those days. You could not bring up the subject of family planning, it was so sensitive. Was that your experience?

HAUSER: It was sensitive, but I would not agree that it could not be brought up. In fact, I think I have some reference to that in that article. Thinking back, it seems to me we raised questions about population control during the first five years of the Commission. I think it became a more sensitive matter later on, with changes in the Administration and an increased voice of the right in the United States.

VDT: Certainly that's true now. You think it was wider open then--not even the Catholics objected? The Catholics were vocal opponents of family planning then [late 1940s; early 1950s].

HAUSER: They were, but they were split; some were not.

VDT: Eisenhower was President then?

HAUSER: Yes.

VDT: Well, he was very much opposed. He said it was the most inappropriate thing the President could get involved in. It wasn't until 1965 that Johnson came out and put the U.S. firmly in the camp of population aid.

HAUSER: Right. And now this Administration with the radical right point of view is going in the other direction.

VDT: It is discouraging. You said an important part of the Population Commission's work was to advise the Population Division and Statistical Division on their research agenda. Did you have something to do with encouraging the Demographic Yearbook?

HAUSER: Oh yes. I recall that we also, during those five years, requested the Secretariat to do the study which came out in two editions, The Determinants and Consequences of Population Trends (1953 and 1972). In my article there's a listing of the things we did and that is among them.

VDT: One time during those years you brought Sauvy and some other Commissioners to a PAA meeting. [May 22-23, 1948, meeting at the University of Pennsylvania, Philadelphia. Guests at the dinner were: Alberto Arca Parro (Peru), Alfred Sauvy (France), Germano Jardim (Brazil), as well as Philip Hauser. Frank Notestein, then PAA President, substituted an "informal discussion" among these guests for the usual presidential address. From Population Index.] Most of you on the Commission then were demographers?

HAUSER: Yes.

VDT: Unlike those who came somewhat later.

HAUSER: That's right, including General William Draper, who was never a demographer, but he was a wise and well-intentioned man.

VDT: Well, he raised a lot of money for the field. Now I have a big question: What do you see as

important issues in demography over the years you've been involved? We've touched on it already, because we've spoken of your concern over the population explosion. At the time you put out The Study of Population (1959), the great bible of the trade, you stressed then in what was supposed to be a brief for the National Science Foundation on demography as a science, the division between demographic analysis, which deals with demographic variables, and population studies, which deal with non-demographic variables. However, you then and always have encouraged a meshing of the two.

HAUSER: Right.

VDT: Another quote from the Norman Ryder interview. He said: Phil Hauser "also played a very important role as an ambassador to the outside world. He, unlike most demographers, is able to be at home and talk convincingly to the outside world, with the world of business and industry and commerce, and policymakers, and talk on major issues with some technical facility in a way that doesn't bore them silly. People like that are very important to our profession. Most of us find the role very uncomfortable and don't do it well. Yet somehow or other the health of the profession depends on interchange between the different worlds."

HAUSER: I won't disagree with that. And as a matter of fact, because of that I'm able to spend my retirement in this place.

VDT: He's living in a very lovely building on the tenth floor, with a view over Lake Michigan. You have done a lot of consultant work for business?

HAUSER: That's right.

VDT: You obviously were one of the first demographers to write and talk about the population explosion to the outside world. For instance, you published in 1960 the book The Population Explosion: World, National, Metropolitan. Then there was "The Population Explosion--U.S.A.," which ended up as a Population Bulletin (Population Reference Bureau, August 1960), which you'd given as a speech at a conference in Dallas (National Conference on "A New Look at the Population," cosponsored by the Dallas Council on World Affairs and Newsweek magazine, Dallas, May 19, 1960). You then published the book, The Population Dilemma (Philip M. Hauser, ed., 1963; second edition, 1969). All of this was before Paul Ehrlich and The Population Bomb (1968), when the world seemed to become conscious of the population explosion. You were talking about the population explosion both in the developing countries and in the U.S.--one of the first.

HAUSER: That's right. Incidentally, I introduced some language that I did not take very far. I talked not only about the population explosion, but also "implosion" and "displosion." By population implosion, I was referring to the increasing concentration of people in relatively small amounts of space--metropolitanization--and the way in which this would, and has, completely altered much of our institutions and way of life. The "displosion"--an interesting point, the word was first suggested to me in this use by Norman Ryder. I was looking for a piece of language that would rhyme, referring to the increased heterogeneity of population, which definitely also created many kinds of problems, including all these forms of racial, religious, and nationalistic conflicts. That's the "displosion." It's a word I took out of the dictionary and Norman suggested it. All three are very fundamental, problematic areas in which the demographer, in my judgment, should be well versed and able to contribute to in a research way.

VDT: Well, you certainly have bent many demographers in that direction.

We haven't solved the problem of the population explosion. Are you discouraged that some of your earlier prognostications, which were tempered, I won't say really pessimistic, but you were not optimistic. You tried to tone down Don Bogue. You pointed out in the early 1960s that if world population growth continued as it was, the total would be 7.5 billion in the year 2000. Everyone knew it wouldn't be that. But the UN medium variant projection at that time was 6.1 billion; it's still 6.1 billion.

HAUSER: I think I deal with that problem of projections in those articles I did. The only projections I quoted were those of the UN, which, as you know, were presented as "low, "medium" and "high." The medium projections which I favored as the most reasonable of course assumed more declines in fertility than the high projections.

VDT: Yes, you were always very careful to point out that assumptions are important.

But now, this past year, the Population Reference Bureau's World Population Data Sheet (1988) shows that population in 2000 is likely to be closer to the UN's high variant. [That Data Sheet projected 6.178 billion for 2000. UN 1988 assessments, published after this interview, revised the medium variant projection for 2000 up from 6.122 billion, 1984 assessment, to 6.251 billion.] The world population growth rate has been stuck at 1.7 or 1.8 percent for years. Do you find that discouraging?

HAUSER: In some ways. Not so much discouraging--I don't think that's the right word. My reaction to it is it's too bad, because the world pays a very high price for it. And, yes, it's discouraging, but it shouldn't be discouraging in the sense that we stop studying it or trying to find ways to deal with it. I don't like to use the word "discouraging" in the sense that you turn away from it. And, I would add, that despite the problems and the fact that the world population explosion has not come to an end, as some hoped it might, we have made progress. There are definite signs of progress in the decrease of fertility in some of the Third World areas.

VDT: Did you know that Thailand is down to a total fertility rate of 2.5, according to the latest Demographic and Health Survey?

HAUSER: Right, and the NICs--newly industrializing countries--have done great things in reducing their fertility--South Korea, Singapore, Taiwan, Hong Kong. All the ones that are making progress on the economic front are also making progress on the fertility front.

But certainly it is disheartening--I would rather say that than discouraging--to see what little progress has been made in, say, Pakistan.

VDT: That was one country where you thought--I'm not sure in which article--there was a possibility that it would reduce fertility, and it hasn't. And India?--not so much.

HAUSER: Not so much. Pakistan not; Bangladesh certainly not. Of course, China has been successful, though I think by some means that are perhaps abhorrent. I don't cotton much to a situation in which the husband beats the hell out of the wife for having a daughter.

VDT: Have you been to China?

HASUER: Oh yes, I was there on a UN mission. We had a week in which we had a roundtable. I was representing North America; Concepcion was representing Asia; someone else was representing the Arabic countries. There was a division of labor, about five or six of us there, and each of us for our own areas discussed with some 20 representatives of universities, mostly, and other institutions interested in population in China. Very interesting experience. My wife accompanied me; she's been dead now for five years, next month. The Chinese government gave us a guide, who stayed with us throughout our journey, and in each locality we'd pick up local guides. We really had a very good insight into China.

VDT: It's an important country for any demographer to have been exposed to.

HAUSER: Certainly.

VDT: A bit more on projections. Charlie Nam pointed out that one of the things that was so much fun at early PAA meetings were the continuing debates that went on. One he recalled in particular was between you and Joseph Davis of Stanford on projections. Joseph Davis said that because all the demographers' projections were going screwy--which was true; those done before the baby boom did indeed go screwy--that demographers were charlatans. Now, I think you were defending them.

HAUSER: I remember that, although I don't remember the details.

VDT: I guess you'd still say--with a grain of salt--that projections are simply projections; you crank in the assumptions and people have to be careful what the assumptions are.

HAUSER: I've always said, and I've always taught my students, that projections are not to be confused with predictions.

VDT: Right. But they're still worth something--that's what you were telling Joseph Davis.

HAUSER: Much superior to ignorance.

VDT: Another theme of yours that I think was important was the importance of getting good data.

HAUSER: Right.

VDT: You pointed out in your 1967 Demography review of the Berelson book that, for instance, the KAP studies did not measure response error.

HAUSER: That's right.

VDT: And also, apparently already back in 1954, you recommended sample vital registration collection in countries like India, and later India did that. Were you the one that suggested that to India?

HAUSER: Yes. I published an article on sampling of vital registration areas; I think it appeared in the Journal of the American Statistical Association. The Indians, who followed our literature, eventually adopted that thing which I recommended. I would regard that as one of my more important suggestions from the standpoint of potential, but too many countries have not adopted it. The sample vital registration system would give you accurate vital statistics way before you could complete a vital

registration system on a 100 percent basis.

VDT: In your book Social Statistics in Use (1975), you dealt with the social statistics that are used in the United States, starting with the Census Bureau and so on. What do you think of the current defunding of federal statistics-gathering?

HAUSER: That is one of the indexes of the complete stupidity of the present Administration. The one way to make sure that you cover up your stupid actions is to make sure that you reduce the knowledge about it, which is what happens if you reduce statistics.

VDT: What do you think of the current plethora of mathematical demographers? You said you trained yourself a lot in the mathematics of demography; obviously you were competent but you were not a Nathan Keyfitz, for instance.

HAUSER: No, I never went into mathematical demography very much. I went into it enough to be able to read and understand what was involved, without necessarily tracing through every item and every equation. I think that mathematical demography is one of the fields which definitely must be cultivated--has been cultivated, and has made a great contribution. There should continue to be such contributions. But I think that we need more than has yet occurred in integrating the significant findings of the mathematical demographers with social and economic and political consequences. I think there's not been enough, partly because the social and economic demographers have sometimes not been able to read the mathematical demographers. And partly because the mathematical demographers have become so intrigued with methodology that they have not followed out to deal with what the implications are of what they're discovering.

Now, I think there are two exceptions to that, whom I think have done pretty well in their efforts to integrate--Nathan Keyfitz and Ansley Coale. They have traced through and considered and analyzed implications of their findings on the social and economic scene.

VDT: You talked in The Study of Population about attempts to build up demographic theory. When I was trained in demography at Georgetown in the late 1960s and started reading your books, I was almost taught that the demographic transition was the only theory that existed in demography.

HAUSER: Yes.

VDT: In The Study of Population, you sort of dismissed optimum population and even Lotka, but that was more in the mathematical part, and certainly the efforts to get a psychosocial theory, which was tried with the Indianapolis Survey and also with the Princeton Fertility Survey and found wanting.

HAUSER: Yes.

VDT: What about the demographic transition? It's been pretty much destroyed, I think, by the European Fertility Study done at Princeton, which showed so many exceptions.

HAUSER: Right. But then the demographic transition theory served a very useful function, which all theory is supposed to do. The point to developing a theory is--and I think I state this explicitly in that book with Duncan [The Study of Population]-theory is a generalization drawn from reality in a way that has implications for further research, heuristic implications. The purpose of a theory is not to prove but to do research to see the way in which it misses the important thing, the way in which it is

lacking, the way in which there are variations from the generalization. And the most important function a theory performs is to stimulate empirical research which destroys it. After it has stimulated empirical research it's done its best job. That's the way I look at it.

VDT: Great way to put it. Which do you consider your major publications and why were they important? Your favorites, among your 32 books, your chapters in 50-60 books, and more than 500 articles.

HAUSER: It's a hard question to answer. I would start with the empirical ones. The three most important empirical studies I've made, I would include Differential Mortality with Evelyn, Housing a Metropolis with Beverly Duncan--these are real empirical researches--and I would include my own two-volume publication, published by the federal government, called Workers on Relief in the United States, in 1935.

VDT: I didn't know about that. Your name was on it?

HAUSER: Sure. The first volume is about two inches thick and was done by me only, with my name on it. And the second volume, about an inch thick, I did with one of my good friends, Bruce Jenkinson, who did the mechanical things involved.

VDT: Where did you collect your data?

HAUSER: Let me take you back to 1935. I was in the division of research of FERA, then WPA. FERA was going in transition from FERA--Federal Emergency Relief Administration--to Works Progress Administration. What they needed for policy was who are the people who are unemployed, so they could plan what kinds of activities could provide them with useful employment. There were three thousand some counties in the United States. Every county had a FERA office. The office contained the names of all the persons who were getting relief. I was put in charge of the project; I sometimes call this my 12 million dollar postgraduate fellowship. What I did was design a plan to go to every relief office, every county in the country, and have them, with instructions I provided, select a sample of the names and characteristics of what was on the relief rolls in that county. That sample was then sent into headquarters in Washington, where under my supervision it was compiled into data. If you'd come two days ago, I could have shown you the volumes; they are now over at the new center. Some of my books, incidentally, are included among a few books of previous presidents of the American Sociological Association now on display at their headquarters.

The first volume had the age, sex, and education, as I recall, of the workers on the relief rolls, some six point something million. The second volume was their occupations and industries from which they'd come, as well as age, sex, and general characteristics. Those data provided a basis for planning work projects, which was the function of WPA. Those were my first two big publications and what was involved in both of those volumes was a lot of demographic statistics. And I used sampling to get it. In fact, it may be one of the first things that ever came out in a federal government publication using sampling. At the end of the volume I had a table of sampling errors, so that you could read potential sampling error in any statistics the volume contained.

VDT: And you were just in your mid-twenties at that time! You say those were the three empirical publications you're proudest of. What are some others?

HAUSER: The one I wrote called Population Perspectives (1960), based on a series of lectures I gave

at the College of the Pacific in the state of Washington. I feel that work was important because it was an effort to present what I regarded as the different aspects of the work I'd been doing for the public at large. Population Perspectives had a pretty good circulation. The Population Dilemma, which I edited, had some of the best demographers do chapters--there are a couple of different editions with variations in contributors--was done under the aegis of the American Assembly. That's the outfit created by Columbia University. Up until a few years ago--I haven't been receiving the American Assembly lists since--it was their most successful publication; it had gone through over 150,000 copies. I regarded that as important because it was distributing knowledge to the general public.

VDT: Speaking to the outside world, as Ryder pointed out.

HAUSER: Right. Then on the technical side--you see these are different categories--I would say The Study of Population and then, almost my most recent book, World Population and Development: Challenges and Prospects (1979), which I did for the UNFPA to celebrate their tenth anniversary.

VDT: Excellent. You ended up that by saying, "We can choose between Doomsday and Dawnsday"--I liked that.

You write so well and edit--obviously you're an excellent editor.

HAUSER: This is just part of having good training. The University of Chicago is a good place to get trained.

VDT: You must have had some good training before that; you mentioned Latin in high school.

HAUSER: Yes, and I've got to admit that I was a good student. My undergraduate work was straight A's. And pretty much the same in my high school work, although it suffered from time to time because for a good part of my high school I spent eight hours a day delivering Western Union messages, from four to midnight. And not only doing the regular high school but I was doubling up; I did my four years of high school in three years.

VDT: And you were a Western Union messenger! On a bicycle?

HAUSER: No--Ford automobile.

VDT: How old were you? Were you allowed to drive before you were 16?

HAUSER: At that time you didn't need a driver's licence, but you were not supposed to drive before you were 16, I think. I first began doing that when I was about 14.

VDT: Actually driving for a living--oh boy!

HAUSER: Well, it was the only way I could manage to get to school. My father was an immigrant.

VDT: What accomplishments in your career have given you the most satisfaction?

HAUSER: I think that would be the accomplishment represented by sending a batch of superb students to many parts of the world--Asia, Latin America, and Africa. I don't think anything can beat the satisfaction one gets from seeing an untrained brilliant mind become a trained professional, to go

out and do a professional job.

The other thing that I guess would match it was the opportunities I've had to conduct research on my own and to create research with somebody that helped others do research. I think those things would be. . . I've had honorific offices and so on, but those are momentary and don't particularly count. As I've told you, I've been president of five organizations.

VDT: Which were they?

HAUSER: The three major ones--and I don't think there's anyone else who has ever done this--I was president of the Population Association, the American Sociological Association, and the American Statistical Association. Then a fourth one was the Sociological Research Association, kind of a select group, and then the National Conference on Social Welfare, an organization concerned with welfare and social work.

VDT: You were never president of IUSSP, but you were one of only 29 Americans of the 147 persons who were invited to become individual members when IUSSP was reorganized after the war.

HAUSER: That's right.

INTERRUPTION

VDT: We're talking about all your beautiful furnishings, Oriental artwork and so on, from your travels. You went to the East-West Center in Honolulu regularly for a while?

HAUSER: Yes, for about 14 years in a row, I spent my winter quarters in Hawaii as a research fellow. Those were not opportunities to be a beach boy, but as represented by my publications, were the most productive parts of my year. I had an office; I had access to secretaries, to computers, to the library. Most people didn't know it was cheap to phone me from the United States, so I wasn't run daffy by phone calls. And students would see me occasionally but not that often. I was really able to get productivity that was higher than any other time in my life.

VDT: Great. Well, we were talking about the IUSSP; the fact that you were one of just 29 Americans among 147 members invited when the IUSSP was re-formed after World War II on an individual basis.

HAUSER: Right. And I felt that having been an office-holder in a way that was probably much too much in this country, and having experienced the decreases in productivity that office-holding meant, I very definitely avoided and discouraged any efforts to hold office in IUSSP. I figured I had done my share of office-holding.

VDT: Now finally to your recollections of PAA. Can you remember the first meeting you attended? You are listed as giving a paper in 1940, at Chapel Hill, in the Howard Odum session, and your paper was called "Economic Differentials in Neonatal and Infant Mortality." Was that your first meeting?

HAUSER: I think I'd been at PAA meetings before that. What was the first year that PAA met?

VDT: What was called the "first annual meeting" was 1932, in New York. Then 1933 and 1934 in New York. Washington in 1935. Were you at that famous meeting--the one where Eleanor Roosevelt came and knitted during the sessions?

HAUSER: Yes, that was among the ones that I attended. Although what I really remember was the series of meetings I went to at Princeton. When did they start?

VDT: Princeton began in 1936 and then it was on and off at Princeton until 1955.

HAUSER: I went to practically every meeting there, and practically every meeting after the 1935 one in Washington.

VDT: You can remember that 1935 meeting. Were you in the select group that was invited to Mrs. Roosevelt's private quarters?

HAUSER: No.

VDT: You were a "wunderkind," but. . . [The occasion of the "select group" of demographers being invited to Mrs. Roosevelt's private quarters was in January 1943. See Clyde Kiser interview, April 1973, above.] Do you remember her being at the regular session [in the 1935 meeting]?

HAUSER: Oh yes. I remember her because--remember she was chairman of the Human Rights Commission at the UN and I was on the Population Commission and I got to know her at the UN; our paths crossed from time to time. I never knew her very well, but enough to say hello. Prior to that I had no dealings with her.

VDT: You were at the 1942 meeting in Atlantic City, the last one before PAA closed shop for the war, because that was the one where you were elected Secretary. In those days, secretaries were elected; now they're appointed. And you remained Secretary until 1946.

HAUSER: When you mentioned this earlier today, I'd forgotten it completely.

VDT: So you can't tell me what you did as Secretary, because you don't even remember being it.

HAUSER: That's right.

VDT: In 1942 and again in 1951--that was the year you were president and the meeting was at Chapel Hill--Con Taeuber keeps telling me these stories, and others too, about the blacks. Blacks were rare at the early PAA meetings. The few times they did come in the early days caused problems: Atlantic City and again at Chapel Hill. Do you recall those incidents?

HAUSER: Yes, and regretted them very much. There was also one session in which I had quite a brawl with Kingsley Davis. Father Gibbons, a Roman Catholic priest who had a favorable attitude toward family planning--and the point is the Population Association, having appointed a committee, invited him to come to a session--I've forgotten what city this was--and present the Catholic view on birth control. He gave a paper and I thought it was a great paper, with the most detail I had ever heard about the theological position of the Church with respect to family planning. He explained in terms of Catholic dogma and general procedures the attitude of the Church. It was the best explanation I'd ever heard, although I'd done reading on it. After which, Kingsley Davis got up and excoriated the man, practically ripped him into shreds, for the Catholic position against family planning. He used virtually every adjective that was pejorative that you can imagine.

Well, there was a deadly silence after Kingsley sat down. And I just couldn't stand it. I got up

and said I thought it most inappropriate and rude for anyone to attack Father Gibbons.

VDT: It was at the meeting of 1949 at Princeton. A book was published of the proceedings of that PAA meeting--the only time that was ever done--in order to advertise the view of the Catholic Church. Anne Lee sent me a copy not long ago. [Studies in Population, edited by George F. Mair, Princeton University Press, 1949. Father Gibbons's paper was called, "The Catholic Value System in Relation to Human Fertility," followed by Kingsley Davis, "Values, Population, and the Supernatural: A Critique."]

HAUSER: I ripped into Kingsley, saying it was most rude and uncalled for to criticize the way he did a guest of the Population Association who was good enough to explain to us the position of his church. "Now, that didn't mean we agree with it, but why did you have to tear this man apart? He performed a service for us." And Kingsley and I were enemies temporarily.

VDT: I think you must have been the peacemaker on other occasions. Lincoln Day told me a story of a session with you in the chair and Kingsley Davis was reading a paper with an early version of intermediate variables and Frank Lorimer--they were always sparring with each other in fertility sessions--got up and intimated that Kingsley had borrowed his ideas from some of his, Lorimer's, articles. And Kingsley said, "I read some of your papers, Frank, and I thought they were all hogwash." Lorimer got up and pulled off his coat, and you said, "Gentlemen, repair to the alley."

HAUSER: I remember that.

VDT: I gather there were lots of fun debates at those early meetings; you all knew each other well.

HAUSER: Oh yes. I'll tell you one thing about a meeting at Princeton that I think deserves recording. Again, I'm sure you'll use judgment about what you publish. After an evening session, we repaired to somebody's room at the Princeton Inn. There were a half dozen of us and our wives. As was usually the case then, the men sort of split off at one end of the room and the women at the other. It turned out I was at the edge where I could hear conversations in both places; participate in the men's conversation but hear the women's conversation. The women were mostly talking about the problems they faced with their teenaged daughters and what do you do about this necking and petting. How do you deal with this whole problem of the adolescent girl.

Among the people there were Gunnar and Alva Myrdal, who were good friends of mine. And one of the women turned to Alva and said, "What do you do about this problem in Sweden?" Alva's response was one that made the American women practically fall off their chairs. She said, "We do not have such problems in Sweden--petting and necking. Our children have wholesome sexual intercourse." The American women practically collapsed!

VDT: Wonderful! You were a member of the old boys' network--and old girls' too, because it included Irene Taeuber, Dorothy Thomas, and Margaret Hagood--who ran everything. There was the College of Fellows, which PAA had from the beginning [until the 1950s], who elected 12 members of the Board [six more were elected by general members], and the Board elected the officers from the College of Fellows. There was not direct election of the officers.

HAUSER: That was at the very beginning, before my time. As I recall it, from the time I began attending the meetings, I think there were elections. I was not party to that cabal.

VDT: Okay. However, there was a smallish group that ran things. Of course, you knew each other

well.

HAUSER: Oh, we knew each other well but I don't remember participating in discussions of who would be nominated for what. Maybe Con Taeuber could tell you. You know he's a bit older than I am.

VDT: He's in pretty good shape, I see him often in Washington.

HAUSER: I talked to him on the phone just last week and he wrote me a note. He says, though, his Parkinson's is getting worse.

VDT: Yes it is, but he comes to meetings and is determined to keep on.

Let's talk about the year you were president of PAA [1950-51]. The meeting was at Chapel Hill [May 12-13, 1951].

HAUSER: I'll tell you one thing I remember that I did. . .

VDT: You spoke on the UN Population Commission [at the dinner], but your main speaker was William Ogburn, on "Population and Social Change."

HAUSER: That's right. I chose Will to give the presidential address for me for the very good reason that he was the man from whom I had my first course in population, who had made, I think, very significant contributions, but who had never been recognized in any formal way by the Population Association. So I deliberately asked him to please give a paper instead of my giving a presidential address.

As I remember, I introduced him with some poetry, I think it was from Auden. It was a most appropriate reference to knowledge and statistics and so on. I'm not sure either of the author or the poem. I'm handicapped with my eyes; I can't go back to my files. My eyesight is bad, but I'm in good physical condition.

VDT: That poetry doesn't appear in the Population Index that reprinted William Ogburn's speech. But it's nice to know about that and your reason for choosing William Ogburn, who spoke on "Population and Social Change," urging demographers' attention to other social variables not directly associated with births, deaths, and migration, but important also in effecting social change. What you have said--not keeping a divide between demographic analysis and population studies.

The year you were president, Jay Siegel gave an analysis of the membership at the meeting [in a paper on "The Teaching of Demography"]. There were just 382 members and 161 of them were at universities. He said then that a great many more people were giving population courses at universities than belonged to PAA. What do you think about the growth of the membership? It really hasn't grown too much; it has fluctuated at about 2,600 since the mid-1970s [2,655 at the end of 1988; 2,752, end 1990].

HAUSER: As I remember, back then--Jay Siegel may have brought this out--a disproportionate number of the members was from the Bureau of the Census. The growth of the PAA had the salutary effect of increasing the range of discussion topics, thus diminishing the proportion of census procedures and data which were discussed. At the beginning, attention was focused mostly on research. As the membership increased, the proportion of persons interested in family planning increased and there was some tension, as a result, between the researchers and the family planners. Now, I'm not opposed to social engineering, but at the time I thought that the Population Association

should be essentially a scientific research organization and not be concerned with social policy and have a family planning mission. Well, as I look back on it, I would agree that it was probably better to get the family planning emphasis into it than not to have family planning emphasis at all. But I think I might have preferred--certainly at that time--a differentiation between the research function and the engineering function.

VDT: That's been a bone of contention in PAA from the beginning. Margaret Sanger, who helped call the first meeting, was asked not to run for vice president, because that would dilute the scientific purity of the organization.

HAUSER: Yes. You know what gave me one of the greatest thrills I ever had. I was invited to come down to Princeton for a meeting. I think it was the time that General Draper was beginning to raise funds; it was a fund-raising enterprise and I was invited to give a talk about population. As I remember, I dealt with various aspects of population problems, including problems in the Third World, and the implications for the economy and living standards. After that talk--it was a group of probably not more than 40 or 50 people--Margaret Sanger came up to me and said, "As you know, I've been working on family planning and population control for many years. I never understood as well how important it was until I heard your talk tonight." Needless to say, I was thrilled.

VDT: Great. So you have the appreciation, the accolade, from the lay people, the activists, as well as your professional colleagues.

HAUSER: I would hope so and I appreciate it very much. After all, no one gets put out by having people say you're doing okay.

VDT: Seeing these lovely family pictures here reminds me I forgot to ask an important question--about Robert Hauser, your nephew, who was obviously influenced by you. How did that come about--that a family member went into demography after you?

HAUSER: Bob was living in Chicago and for a while lived with me and my wife while he took his bachelor's degree here, in economics.

VDT: He's the son of your brother?

HAUSER: Yes. He had two sons. In the edition of Who's Who in the U.S. preceding the one that just came out, on one page there are four Hausers. I'm one of them; my son is one of them; and my two nephews are the others.

VDT: I don't know about your son.

HAUSER: He teaches Japanese history at Rochester University and he's been chairman of the department of history there for a couple of terms. He's pretty fluent in the Japanese language and literature.

VDT: He must have had his world widened by your travels.

HAUSER: Very definitely. And I had him with me in Japan when he was eight years old.

VDT: There are photos on the wall of the four of you: you and your wife and a son and a daughter.

You could always go around saying, "I have two children," but you were lucky you had a son and a daughter.

HAUSER: Being a demographer, I knew how! I tell you what's hard for me to believe is that boy will be 50 years old next year and my daughter was 47 last month. She is in Ann Arbor. She spent some years teaching and working with emotionally disturbed children, through the Department of Psychiatry at the University of Michigan. She's raising one of my grandsons, now nine years old, and her husband is a professor of literature at Wayne State, but he'll be at the University of Michigan this coming year. He's had great success as a novelist and short-story writer: Charles Baxter.

VDT: I also forgot to ask you if you can tell me a bit more about Fred Osborn, who was PAA president just before you, among many others things. He was a good research entrepreneur too, I gather.

HAUSER: There are two things about Fred that come to mind immediately. He's the only man I've ever seen able to put his foot on top of a desk this high and put his elbow on his knee. Try that sometime. He was six feet, seven inches tall. Also, at the old Cosmos Club in Washington, he was able to reach over the gate, which otherwise was closed to everybody else, and lift the latch.

Fred was not a demographer, never was, never would be. But he became very much interested in demography, largely through association with Frank Lorimer. They wrote this book together [Dynamics of Population, 1934], which was Frank's book, not Fred's, and Fred became so interested that he became a philanthropist of demography. Fred made his first half million dollars in selling a railroad to Henry Ford and he amassed quite a bit of wealth. He was a very intelligent man; extremely motivated, particularly on the social engineering side. He was not a scientist but he appreciated what science could do and supported it. That was his major contribution.

The other thing I remember. Fred was six feet, seven inches tall; his wife, I think, was six foot two; and all of his six children were over six feet. When they walked down Fifth Avenue of New York, traffic practically stopped! He was a great guy. I really got to know him and enjoy him very much as a friend.

VDT: And Rupert Vance, who came after you as PAA president. He was an extraordinary person too, I gather.

HAUSER: He was, in spite of his physical handicap. I never knew Vance very well. But from reading his works and what meetings I had with him and hearing him talk and give papers, I regarded Vance as one of the true emancipated intellectuals in the South. He was far advanced over many of his Southern colleagues. He perceived the world with breadth. Alongside of him, many other Southern faculty people were just parochials.

VDT: We have dealt with the outlook for world population growth; one isn't going to give up hope, you say, even though it's not as encouraging as it might have been. What about the outlook for demography in the future? In some of these interviews I've asked this: Do you think the big questions in demography have been answered, so that young demographers, in order to make a mark, have to sort of chip away at the corners, because, for example, the demographic transition theory has done its thing, as you said? What lies ahead for demographers in the U.S.?

HAUSER: I think what lies ahead for demographers may be evidenced by this new Ogburn-Stouffer Center I saw dedicated yesterday at the University of Chicago--the infrastructure provided for close

collaboration between sociologists, economists, and other social scientists. If you think of demography as essentially concerned with fertility and fertility control and population explosion, there's no future there, that's been pretty well finished; we don't need more information about that. But if you're thinking of population studies, in the sense in which I defined it in that book with Dudley Duncan [The Study of Population], that's scarcely been touched. It has a tremendous future, I think. In relating population variables to economic variables, social variables, historical variables, genetic variables, biological variables, there's a tremendous field of opportunity that has scarcely been touched. Moreover, there's room for much more work in what I've called the population implosion and a heck of a lot more work in the population dislosion--implosion being metropolitanization and dislosion being the increasing heterogeneity of population sharing the same living space.

By the way, after having used this language, it occurred to me one day, because I had a general broad training in sociology, including our historical figures, that in one sense I've just invented new names, or used names, that Durkheim had presented in the 19th century. He talked about size and density and heterogeneity of population without following through in the demographic detail with which I and other demographers are concerned. He was talking as a sociologist, but he perceived that the size of a population, its density--where I use the word implosion--and its heterogeneity had tremendous implications for social institutions and human behavior. And in a sense, with much more emphasis on the demographic things, I quite unconsciously bowed to Durkheim.

VDT: Sociology was engrained in you.

HAUSER: Right. And then Louis Wirth, of course, had written a splendid classic article in which he used size and density--"Urbanism as a Way of Life." He was a good friend of mine as well as a former teacher; he died prematurely at age 52.

VDT: You've had splendid traditions behind you but you certainly set up a splendid tradition for those who come after you. And thank goodness for your longevity and your productivity. Just a little bit of this has been captured this afternoon, but I thank you very much. It's been a wonderful interview.

ADDENDUM

VDT: Phil has thought of one more thing relevant to PAA.

HAUSER: A couple of years ago, I received a letter from the Hoover Institution. They wanted my materials to create an archive of what I had published as part of what they're trying to do in the history--apparently trying to trace the development of demography in the United States. I wrote back and told them--and this is what I thought I'd note for you--that all my papers and books are at the Regenstein Library of the University of Chicago. The archivist has about 26 linear feet of some of my materials. And you see those four boxes there and there are all the drawers in this desk--these are materials I'm going to offer to the archivist in addition if he wants them, because I haven't added anything since I retired in 1979 and I've been active for ten more years and have articles, books, papers to add to those archives.

VDT: The Hoover Institution wrote to Andy Lunde, asking for copies of the tapes of the interviews he had done in the 1970s; I think Dudley Kirk had told them about the interviews starting. But PAA decided that, no, they belonged to us. Unfortunately, some of those early interview tapes are mechanically defective. So I'm redoing those I can and carrying on.

CLYDE V. KISER

PAA President in 1952-53 (No. 16). Interview with Anders Lunde at the PAA annual meeting, New Orleans, April 26, 1973. See also the following interview of December 1976 with Kiser, Horace Hamilton, and Joseph Spengler together.

CAREER HIGHLIGHTS: Clyde Kiser was born in 1904 in Bessemer City, North Carolina, where he has again lived since 1974. He received the A.B. in liberal arts in 1925 and the A.M. in sociology in 1927, both from the University of North Carolina in Chapel Hill, and the Ph.D. in sociology from Columbia University in 1932. As a member of the technical staff of the Milbank Memorial Fund in New York City from 1931 until his retirement at the end of 1970 (at which time he was Vice-President for Technical Affairs), he was deeply involved in the Milbank Fund's pioneering promotion of fertility and population studies in the U.S. and also in Latin America. (Kiser has described these activities in "The Work of the Milbank Memorial Fund in Population Since 1928," in Forty Years of Research in Human Fertility, proceedings of a May 1971 conference honoring him on his retirement from the Fund, edited by himself, 1971; and "The Role of the Milbank Memorial Fund in the Early History of the PAA," Population Index, October 1981). Among these activities, he, along with P.K. Whelpton, played a leading role in the conceptualization, planning, organization, and analysis of the 1941 Indianapolis Survey, progenitor of a subsequent long line of major U.S. fertility surveys. (He was coeditor with Whelpton of the five volumes summarizing results of that survey, Social and Psychological Factors Affecting Fertility, 1946-58). From 1942 to 1975, he was also a senior research demographer with the Office of Population Research at Princeton University, where his wife Louise Kiser was coeditor of Population Index from 1945 until shortly before her death in 1954. He was Adjunct Professor of Sociology at New York University in 1945-46 and served on many U.S. government committees dealing with population and fertility. He is the author or coauthor of several books and many articles on U.S. population and fertility in particular, including The Fertility of American Women (with Wilson Grabill and P.K. Whelpton), 1958, a 1950 census monograph, and Trends and Variations in Fertility in the United States (with Grabill and Arthur Campbell), 1968, a 1960 census monograph.

LUNDE: Clyde, would you tell us some of the things that you recall about our early organization?

KISER: It happens that I started work at the Milbank Memorial Fund in October 1931. That was the very year in which the Population Association of America was organized. There was a so-called organizational meeting in 1931 [May 7] which I did not attend; in fact, I knew nothing about it. But the following spring of 1932 [April 22-23] was the time of the first full-fledged meeting of the Association and at this meeting there was not only a scientific session but also a ratification of the constitution of the PAA.

Now, you ask about the circumstances surrounding the organization of the Association. The demographic background is somewhat as follows. As you know, this was a period of low fertility. The immigration question had been solved in that the immigration laws were enacted in 1924. So the people who were interested in population, studying in the different colleges, began turning their attention more and more to problems of this country, national problems. In the meantime, the birth registration area was greatly improved. It was in 1933 that the last state of the union, Texas, was admitted to the birth and death registration area, thereby taking the whole country into the registration area.

In 1927 Margaret Sanger, the birth control person in this country, managed to get money and

stir up interest in holding an international population conference in Geneva. The following year, 1928, the International Union for the Scientific Investigation of Population Problems was organized and Raymond Pearl of Johns Hopkins University was elected first president. The International Union was organized on the basis of national committees rather than individual membership, so that it was necessary for a country to have a so-called national committee. The leading students of population so organized themselves on a rather informal basis into an American National Committee and this little group then stimulated the organization of an association. For a time, there were two groups in this country, the group of the American National Committee and then the group of directors of the Population Association of America.

After the Association was organized in 1931, provision was made whereby the directors of the Population Association of America would also serve as members of the American National Committee. The International Union was disrupted by the Second World War and in 1946 it was reorganized on an individual membership basis rather than a national committee basis. The national committees continued for a while. As I recall, though, the U.S. members of the International Union made up the American National Committee until the whole idea of the American National Committee was abandoned, about two years ago.

Now, you asked about the character of the Population Association of America. As I recall, in 1932 when we had our first annual meeting, the total membership was probably around 50 [67 persons registered for the 1932 meeting]. The first meetings were rather small; they were at the Town Hall Club [in New York City]. They had one fairly large dinner meeting. I recall that Frank Notestein gave a paper on differential fertility at that meeting.

The first really large meeting was the one in Washington in 1935 [May 2-4]. Roosevelt had come in as President, he'd formed these New Deal committees, and there were a lot of new government agencies working in fields in which population was a factor. The place of the meeting was the Willard Hotel. At an afternoon session, Carter Goodrich gave a paper on migration that stimulated a lot of interest. Mrs. Roosevelt attended that session. Fred Osborn was an old friend of the Roosevelt family, so he went over to the White House and escorted her to the meeting. It so happened that he and she sat right behind me. I was rather embarrassed once. I wanted to get a good look at Mrs. Roosevelt, so I turned around and stared at her. She glanced up at me as if she disapproved of what I was doing, so I turned back.

At the time of that meeting, Mrs. Roosevelt had a tea party for the members; she invited the whole membership. The actual numbers at that time were, I should think, probably not more than 75. But there were a good many government workers there too. I don't believe that all the government workers were invited to the party. It was a fairly goodsized party.

At the dinner meeting of the conference, Frank Notestein gave a paper of which I was coauthor. We were told that we should wear black tie, so we wore black tie. Henry Pratt Fairchild presided. Fairchild was first [PAA] president and he was president from 1931 to 1935. I don't know what the constitutional provisions were about presidents in the beginning, but Fairchild was re-elected just as a matter of course for the first four years. After that Louis Dublin was elected for one year and it was pretty much on an annual basis until World War II, when [Lowell] Reed served for about three years [1942-45].

LUNDE: Was there any activity during the war period?

KISER: We missed three meetings, as I recall, 1943, 1944, 1945. There was not much activity; a lot of people had gone to war.

LUNDE: What about the period when you were president, in 1952-53. What were the issues in those

years?

KISER: Those meetings were in Cincinnati [May 2-3, 1953]. We went out Thursday night; got there Friday morning. In those days, they were much concerned about population problems in the underdeveloped areas, the demographic consequences of modernization. Notestein and his group at Princeton had discovered a rather consistent tendency for modernization to bring decline in death rates before birth rates are affected, thus modernization tends to be followed by a period of rapid population growth. And partly because of the activities of the WHO, death rates were tumbling all over the world.

So we met in Cincinnati and one of the things I was worried about was the possibility of a parliamentary snag at the business meeting at which I was to preside. An important matter to be decided was whether there should be an amendment to the constitution of PAA regarding the election of officers. Before that time, the officers were elected by the Board of Directors. There was natural discontent among the people. Those who were against the change pointed out that this was the way business organizations did it; you elected the directors and they elected the officers. People were represented through the directors. But the people were naturally not satisfied with that, so they did manage to get that change in the constitution. Thereafter, the officers were elected directly by the membership and it was specifically provided that there should be at least two names proposed for all offices except for secretary.

The meeting in Cincinnati carried through Friday and then on Saturday morning, Warren Thompson had arranged for buses to come to take us from Cincinnati to Oxford, Ohio [location of the Scripps Foundation for the Study of Population Problems]. So we had our Sunday meeting in Oxford, Ohio. And Thompson and Whelpton did a very good job as hosts; they treated the whole Association to a steak lunch.

LUNDE: Interesting that you should mention that. When I first went to NCHS, Harry Rosenberg and I took a trip out to see Whelpton, to see what he was doing for us on cohort fertility for a publication which later Arthur Campbell prepared for us. We were walking across the campus [of Miami University] and we bumped into Thompson; couldn't believe it. He was still spry and active and very anxious to talk to us. We had a wonderful luncheon with him.

What was the topic of your presidential address, Clyde? Do you remember?

KISER: I certainly do and it's somewhat relevant to this. It so happened that in 1953, if you take 1932 as the year of formal organization, the Population Association was 21 years old. So the title of my address was "The Population Association Comes of Age." I gave a history of the first 21 years. It was published in Eugenical News [December 1953].

Incidentally, there had been one previous presidential address of that kind. In 1942 in Atlantic City when Whelpton was president, Fairchild himself gave a ten-year history of the Association. He told the story much better than I have about Margaret Sanger's role in the formation of the Association.

LUNDE: What was the early Association like in terms of its meetings and atmosphere and all that? For years the members met at Princeton. The first meeting I went to, when Kingsley Davis was at Columbia, was at Princeton. There was a certain flavor about meeting at the Inn there, which was certainly a lot different from what we have today, with increased membership and the big-town atmosphere of our meetings.

KISER: Yes. Well, as I said, the meeting in 1935 was our first large meeting, in Washington. The very next year, in 1936, we met at Princeton at the invitation of Frank Notestein who had just gone down to Princeton the preceding September. This was a meeting in the fall, in October, as I recall.

Notestein, as you know, went from Milbank to Princeton in 1936 to head the new Office of Population Research. Also took over Population Index, which previously had been Population Literature.

It was a small group. Of course, in those days there was only one session; everybody went to the same session. They had probably 100 people at that first meeting at Princeton. President Dodds of the university opened the meeting and welcomed the guests. The Office of Population Research put on a little reception at the Princeton Inn; a cocktail party in the afternoon. For a while, every other year they'd meet some other place and then they'd meet at Princeton. The people seemed to like Princeton. It was fairly convenient. It was pointed out that Princeton was more or less in the center of the membership of the Population Association. Later, as we began getting more Western members, the center of gravity moved westward.

I believe the last meeting at Princeton was in 1954 or 55 [1955]. Margaret Jarman Hagood was the President and she had Carl Taylor, the agricultural economist, give a speech [in place of her presidential address]. After that the membership was pretty large, so they began meeting in other places. Now, of course, though we still have some way to go before we're the size of the [American] Sociological Association, we do have to pick the big cities to get a place to accommodate us.

LUNDE: Were the meetings pretty intimate?

KISER: Yes, they certainly were. In fact, I have mixed feelings on the Association getting too large. Everyone knew everyone else; everything was on an informal basis. I suppose one of the prices you pay for growing is what we witness--we're getting large.

LUNDE: To go back a moment, you were telling me earlier about your visit to the White House during the 1935 meeting and you said something about Rupert Vance being taken up in FDR's elevator. Tell us that story.

KISER: We were invited for tea, so immediately after the meetings in the Willard Hotel, we went over to the White House. Most of us started up the stairway there and a flunky came out and told Vance, who of course was on crutches, that they had an elevator for him. So they led him to a little elevator which Vance thought had been installed for President Roosevelt. This was the tea in 1935.

Well, about eight years later, in January 1943, there were about a dozen demographers that were invited to the White House, invited by Mrs. Roosevelt, and the invitation read something like this: that the recipient was invited to the White House for dinner on a given night in January 1943 to honor the work of Henry Pratt Fairchild. I happened to be one of those. I can't remember all of the names, but I do remember several. Mrs. Roosevelt and Vice President Henry Wallace were there. Orson Welles, Fairchild, Leon Truesdell, Frank Notestein, Professor Ed Hutchinson of Pennsylvania, and Warren Thompson; I distinctly remember some of the comments he made. I don't recall whether Whelpton was there or not.

We were first met in the hallway--we had our credentials with us and were able to get by the gate. We had a little reception, drinks, then went into State Room for dinner. I remember I sat next to Orson Welles. After dinner, Mrs. Roosevelt took us up the elevator to her private apartment, where she showed us a few rooms--up there on the next floor. I recall seeing the large photograph of Abraham Lincoln. After we got into her apartment, Mrs. Roosevelt settled down in a comfortable chair, took out her knitting, and told us that she didn't plan to say anything, that she wanted us to start talking while she listened. Fairchild acted as presiding officer. He outlined some of the outstanding population problems of the day as he saw them and tried to get the conversation rolling. I recall we got into questions of race, immigration, and urban-rural differentials in fertility. I remember that Thompson quoted one of his bits of wisdom in commenting on the fact that birth rates seemed to be

low in cities. He said, "Well, with apartment-house dwelling it's a little like animals; you can't breed animals in captivity."

LUNDE: When you look back over the years, what do you think were some great moments of the PAA--some of the important things it either did or that it stood for?

KISER: I think the first great moment was this meeting in 1935. There were various government agencies that were concerned with population. You had the migration from farms to the cities; there was the Depression problem. So I think this meeting in 1935 not only put the Association on its feet, it also demonstrated to some of the leaders the relevance of population to national government problems. [PAA's 1935 meeting of May 2-4 in Washington was called "Conference on Population Studies in Relation to Social Planning." PAA's third annual meeting was held on May 3.]

I think the next big peak probably came with the Second World War. Notestein's work at Princeton was something of which the Association can be proud. He involved the Association in the project he tried to carry out. Before the U.S. got into the war, between 1939 and 1942, Notestein had an invitation from the League of Nations to carry out studies of population trends in Europe and the probable trends after the war. The man who pushed Notestein was A. Loveday, director of the League of Nations Economic, Financial and Transit Section, which moved from Geneva to Princeton during the war. He became acquainted with Notestein's work and he commissioned him to carry this out. At several of our PAA meetings, Notestein and his coworkers would present their findings; they did that each year for two or three years. I think he always valued the advice he'd get from the members of PAA. Then after the war, Notestein had a similar invitation from the Office of the Geographer of the U.S. State Department to make similar studies of other parts of the world, the underdeveloped areas in particular [in Asia]. I think that era was one of which the Association can be proud. It was in that time that Notestein formulated the concept of the demographic transition.

From my own point of view, one of the most important things was the meeting of the IUSSP in New York City in 1961--important to me personally, because I served as chairman of the local arrangements committee. Dudley Kirk called me from the Population Council and asked if I would do this and said, "If you will do it, I for one will see that you get plenty of help." So I did it under those terms. I thought I would have a lot of subcommittees that would help. So, for instance, we had a subcommittee on recreation for the group; we chartered a boat to go up the Hudson River. One duty I undertook was to receive the manuscripts that people submitted; any member of the Union can submit a paper. We accepted finally a total of about 130. The average length was about 20 pages. Some had to be typed over, they were all messed up; some of them had to be translated into English. We undertook to offset these papers after they were in shape and make 750 copies and send advance copies to the membership who'd expressed an intention of attending the conference. 130 papers, 20 pages each, 750 copies each amounts to a pretty large stack. We had very little room in the office [at the Milbank Fund], so I had to stack them up in my own little office. By the time the thing was finished, my office looked like an eskimo hut of papers, just a little path from the door to my desk. So I, naturally, derived a good deal of satisfaction out of that meeting.

Since that time, it would be difficult for me to say what are the important highlights of the Association. But the Association does try to discuss timely topics at its annual meetings.

LUNDE: What do you see as the future trend of the Association?

KISER: I don't know, but if you go by the recent past, I think we can look forward to a continuation of rather rapid growth, a continued increase in the dues, and to the necessity of meeting in large hotels in large cities. I think at this meeting here at most of the periods we have three sessions going

simultaneously.

LUNDE: What were the papers at early meetings like as compared to those produced today? I have an impression that some of the early papers were perhaps philosophic and very broad in scope. Many of our papers today seem to be quite esoteric and pointed to a very fine area of investigation.

KISER: I think there's something to that. Back in the days of E.A. Ross, they were concerned with large topics of immigration, size of population, and things of that sort. I think E.A. Ross had great respect for the inductive method but he never did much in the way of scientific studies himself; he had the overall approach. So it was in demography. Take the very first PAA president, Henry Pratt Fairchild. He was a very good speaker; he needed no amplifier and he never needed any notes. Some people felt he traded a little too much on that ability. And Louis Dublin, Warren S. Thompson--they were all good men who could make a good speech without writing a paper. As you get on down the line, you find people who were interested in--interested in broad subjects, of course--but they tended to do research on small, narrow topics.

LUNDE:: What was your impression of Henry Pratt Fairchild as a person?

KISER: He was a very interesting man. I succeeded him at NYU, not as head of the department, but I began teaching the courses he'd taught there when he retired in 1945. I think the first time I saw Henry Pratt Fairchild was about 1929 or 1930. Warren S. Thompson was coming through New York at the time to go to Europe and old Professor Tenney told us that Fairchild and Thompson were having a meeting at some hotel at lunchtime and that his class in sociology was invited to go; so we did. Thompson was then a fairly young man. There was also a speaker there, a commissioner of immigration, I believe. He didn't know much about demography but he was under the impression that birth rates in Italy were increasing because he knew that the total number of births were increasing each year. Fairchild and Thompson assured him that the birth rates were actually decreasing.

Fairchild, as I said, had an important role in planning the Association. Incidentally, Fairchild was simultaneously the first president of the Population Association of America and of the Eastern Sociological Society, which was also formed in 1931.

LUNDE: What about the American Eugenics Society, when was that organized? You were active in that.

KISER: I was. [He was president in 1963-69.] The Eugenics Society goes back a long way. Some of its early history is not too savory today to some of the societies. If you go back to about the late 1920s, you'll find some highly respected people who were members of the American Eugenics Society. R.M. MacIver, I don't know whether or not he was an actual member but I recall that he wrote a testimonial praising the society for what it had done in connection with some abortion laws. The American Eugenics Society really had a pretty good start in getting across the immigration quota legislation of the 1920s [National Origins Act]. They would profess that they were not concerned with racial inferiority and superiority, but they put it in terms of the desirability of having immigrants of the same cultural background. But Madison Grant was one of the members of the early Eugenics Society. Not long ago I saw an old clipping of a New York Times review of Madison Grant's book and they praised it [The Passing of the Great Race, 1916?].

LUNDE: What about Louis I. Dublin? He was president in 1935-36.

KISER: He was the second president of PAA. Louis I. Dublin began writing articles for the New York Times. He'd have articles in the Sunday issue, on the aging of the population, so he was becoming pretty well known in the middle 1920s. Dublin was rather a curious fellow. He became second vice president of the Metropolitan Life Insurance Company. I suppose he was a very good administrator and he certainly put together--and sold to the Metropolitan Life--the work in population. And we should always give him credit for bringing Alfred Lotka to the Metropolitan. But they were a rather incongruous team. Dublin was the man who could sway audiences; he was not a technician. Lotka on the other hand was a technician and he was a little timid about talking in public. But they got along well together, and Lotka himself would always defend Dublin. If you asked Lotka why Dublin's name came first on an article--one of their famous articles was "On the True Rate of Natural Increase" which came out in 1925 in the Journal of the American Statistical Association--if you asked Lotka why Dublin's name came first when you knew very well that Lotka had done most of the calculations, he would defend Dublin and say "We're coauthors."

LUNDE: How about Leon E. Truesdell [PAA president 1939-40]?

KISER: I first knew Truesdell in 1928 at the Bureau of the Census. I went to the Bureau of the Census as a dollar-a-year man in connection with a study I was carrying on. It finally terminated as my Ph.D. thesis at Columbia [Sea Island to City: A Study of St. Helena Islanders in Harlem and Other Urban Centers, 1932]. This was a study of Negro migration from St. Helena Island to Harlem and other urban centers. T.J. Woofter, Jr., was the man who headed up that project and he cooked up the idea that if I'd go to Washington and compare the names of the people enumerated on the island in a special census in 1928 with those on the census record for 1920 and if I'd take into account the death deficit, I could have a reasonable basis for knowing who had migrated from the island. So I did that. Truesdell was then a rather young fellow. He had black curly hair. He's still living; he's now, I guess, 93 or 94.

LUNDE: I had a letter from him just the other day, indicating his continued interest in the Association and in the development of the historical end of it and saying a few words about his recollections. It was fascinating to hear from him; I couldn't believe it.

KISER: Truesdell was a good administrator, I think. Paul Glick used to say that when he went to Truesdell's office [in the Bureau of the Census], he'd put his jacket on; he wouldn't go in there in his shirt sleeves. Truesdell was a good Republican; he came from New England. I believe he said that if Hoover had been re-elected he would have become Director of the Census. Hoover was not re-elected but Truesdell did stay on as chief statistician. As you know, he was [PAA] president in 1939-40. He gave his presidential address at the 1940 meeting down in Chapel Hill and he told about plans for the 1940 census.

LUNDE: The Association met two or three times in Chapel Hill [1940 and 1951]. What brought them to Chapel Hill, of all places?

KISER: Rupert Vance was in Chapel Hill [University of North Carolina]. Howard Odum was in Chapel Hill; he was not a member but he had Vance on his staff. And T.J. Woofter [PAA president 1940-41] had close connections with Chapel Hill.

There are some interesting things about meeting in Chapel Hill. We met down there in 1940, when Truesdell was President. The next time we met there was when Philip Hauser was president; that was in 1950-51. There was one well-known Negro who was a member of the Association who

attended the meeting [of 1951]. And Odum, who was head of sociology and certainly no one could accuse him of being anti-Negro--he devoted his life to a study of the Negroes--but he was also a person who didn't want to defy convention too much, particularly if he thought it would hurt his cause. Now, he said, "We have one well-known man there; I won't mention his name unless you want me to," and they canceled the dinner. Daniel Price had arranged the dinner, but Odum told Price he better cancel the dinner. He said, "We'd lose more than we gain." Some people got pretty sore about that. Dorothy Thomas got sore; she would never go to the South again for a meeting until or unless they made suitable arrangements for the nonwhite members.

The next meeting in the South was in 1954 at the University of Virginia [Charlottesville] and Irene Taeuber was President that year. As you recall, we had a little trouble. There was one man from the West Indies there and the local arrangements committee had guaranteed us that they would have the racial question well in hand, that they would make suitable arrangements for the nonwhite; there'd be no discrimination. Well, actually when the chips were down, they had the same thoughts that Odum did; they might lose more than they gained. They got him a good room in a nonwhite neighborhood and, of course, the members wouldn't accept that and they threatened to boycott the meeting unless they put him in the same place that we were in. Well, I think this man told Irene Taeuber that he liked his room.

LUNDE: Do you remember who the chairman [of the local arrangements committee] was? We've had an inquiry about him; they want to give him an honorary degree and wanted to know about his Association activities. I was at a loss, because I couldn't find anything in the records.

KISER: It was Lorin A. Thompson. [Conrad Taeuber, who was more involved in this incident, says the PAA local arrangements chairman was Lambert Molyneaux. Conrad Taeuber letter to Daniel Price, July 14, 1982.]

LUNDE: His university wants to give him an honorary degree. I wrote to Hugh Carter who was [PAA] secretary at that time [1954] and Hugh told me that he remembered an incident at the meeting and he thought that Lorin Thompson had handled it quite well under the circumstances.

KISER: He did. He finally got this man put in the hotel where the rest of us were. [See further description of this incident in the 1974 Hugh Carter interview below.]

LUNDE: It's interesting that you should remember that, because other members do too. When did this whole business get cleaned up?

KISER: They had had an episode earlier, when we met in Atlantic City in 1942. There was a man who was finally affiliated with Howard University but he died several years ago; he was a promising demographer. [Lionel Florant. See Shryock interview below.] I think he was a student of Frank Lorimer's at American University. They gave this man a room in a private home, I believe, and Frank Lorimer went to the hotel man and said, "Now listen, you run this hotel, but I can tell you this, this man has good connections and he'll raise a stink." So the compromise--I think it was a compromise--was that he could have a room on the second floor of the hotel if he did not use the main stairs. Now, I don't know whether they accepted that, but that was the compromise handed out. They probably accepted it.

LUNDE: How times have changed. That's no longer a problem. But as you recall, a year ago in Toronto our ladies raised an issue over access to the men's bar. I don't know if we've got anything

happening here [see Harriet Presser on "happenings" in New Orleans, 1973].

KISER: I notice we have a women's caucus this year. But I was thinking the other day that something had changed this year. For the last two or three years, we've had the group of young beatniks wanting more riots, but they're not here this year.

LUNDE: No, they're not. For a time there, it looked like they'd take over one of those meetings.

KISER: Up at Princeton University there's an old fellow who teaches history; he retired a year or two ago. He was talking about changes in the students' attitudes in the last two or three years. He was bemoaning the fact that we no longer have the students who threw all the demonstrations and locked the professors out. He said he'd rather have that than the indifference he encounters now.

LUNDE: When I asked some of the ladies today at noon what they'd like to get involved with, they all wanted to go see the flood crest of the Mississippi; it's supposed to be the highest in 200 years here today. I'm just waiting to see what will happen at the business meeting tomorrow.

You knew Herbert A. Bloch, didn't you? He was my first instructor in statistics as an undergraduate, in 1932 at St. Lawrence University.

KISER: We had a seminar together at Columbia University. Frank Ross, professor of statistics, used to get the graduate students in sociology to come up to Hanover, New Hampshire, in the summer. It didn't take much urging. He told us, "There's no summer school at Dartmouth. I know I can get you a study in the library, it's cool up there." He did that for two or three summers. One summer there were several of us there. The Blochs were there; I was there; F. Galdau, a fellow student from Romania who later became a priest; Bob Kutak and his wife; and John Innes. Ross lived over in Thetford, Vermont, 12 miles away. We'd go over to his place sometimes and have a steak fry. This was in 1929 and 1930. I think the Blochs were there in 1930.

LUNDE: Did you and Herb receive your Ph.D. together?

KISER: I think he received his in 1934. He was about two years behind me, in that it took him that much longer to write his thesis. And I'll tell you this, the first time I met you was at the Shoreham Hotel at a meeting of the Sociological Society. You were with the two Blochs.

LUNDE: I remember that very well.

KISER: And also I had the privilege of attending a service that you conducted for Bloch. That was a very touching service.

LUNDE: I've often thought that if Herb were alive today how interested he would have been in what has happened to the Population Association.

KISER: I liked his wife very much too; her name was Belle. The four of us went down to White River Junction, about three miles from Hanover, to see an old country stage show out in the open, "Uncle Tom's Cabin." One old man played two or three roles. Herb got quite a kick out of that. His thesis was about conflicting loyalties, wasn't it?

LUNDE: Yes it was. Later on he got very involved with social deviation--delinquency and social

disorganization. I guess his books are still quite used.

Let me just ask you about T.J. Woofter, Jr.

KISER: He died only about a year ago. T.J. Woofter came from Georgia. His father was in the field of education. Woofter came to North Carolina in the summer of 1927 to teach sociology and cultural anthropology. Unfortunately, Woofter was addicted a little too much to the bottle; you've probably heard of some of the things he got into. He was a very astute man. People who knew him would say that he was the man who could put his finger on and state the nub of a problem, and if you can do that, you can begin to find ways to solve it. He worked with CIA a long time. He was once with the Southern Interracial Commission, which was the forerunner of what is now the Southern Regional Council, devoted to Negroes. He was a good friend of Frank Ross of Columbia and they got together and hatched up the idea of studying the Negro culture in St. Helena Island. It was those two who rather shaped my career. I was at North Carolina in 1927. I planned to try for the Ph.D. degree there, but they got this study together and I got roped in on it. Then after I got the St. Helena, Ross and Woofter put their heads together and said, "Why doesn't Kiser come up to Columbia for a year and follow up some of these Negroes in Harlem and then come back down and get the degree?" After I got to Columbia and got my classes started, I just stayed up there.

LUNDE: When did you go to the Milbank Memorial Fund?

KISER: In October 1931. This was before that. The study of Negro culture in St. Helena Island started in the fall of 1927. I went down to St. Helena--that's right off the coast of Beaufort, South Carolina. Beaufort is between Savannah and Charleston. The Rosses used to go down there. That's the reason I went to Columbia.

LUNDE: What are you doing right now? You're supposed to be retired.

KISER: I retired January 1, 1971. But I've been trying to write a little history of the Milbank Memorial Fund.

LUNDE: When will this be published?

KISER: I've turned it in; I know there are revisions I must do. It will be published within a year or two [The Milbank Memorial Fund: Its Leaders and Its Work, 1905-1974, 1975].

IRENE B. TAEUBER

PAA President in 1953-54 (No. 17 and first woman president). Interview with Anders Lunde at the PAA annual meeting, New Orleans, April 28, 1973.

CAREER HIGHLIGHTS: Irene Taeuber was born in Meadville, Missouri, on Christmas Day, 1906, and died in 1974. She received the A.B. degree from the University of Missouri in 1927, the M.A. in anthropology from Northwestern University in 1928, and the Ph.D. in sociology from the University of Minnesota in 1931. She and Conrad Taeuber (PAA President 1948-49) met as graduate students at the University of Minnesota and were married in 1929. Their two children, Richard and Karl, are also demographers. Irene and Conrad Taeuber taught at Mt. Holyoke from 1931 to 1934 and then moved to Washington when he took a post at the Federal Emergency Relief Administration. In 1935-36, she was coeditor (with Frank Lorimer) of Population Literature and from 1937 to 1954, although she continued to live in Washington, she was coeditor of and chief contributor to its successor Population Index, published in Princeton for PAA by Princeton's Office of Population Research. She was Research Associate of the Office of Population Research in 1936-61 and Senior Research Demographer from 1962 to 1973. She directed the Census Library Project of the Library of Congress and the Census Bureau in 1941-44 and served as consultant to many U.S. and international agencies. She was vice-president of the International Union for the Scientific Study of Population in 1961-65. Her publications, which run to 13 pages in a bibliography published after her death in Population Index (January 1974), covered the full range of demographic topics in the United States, Asia, and many other parts of the world. The Population of Japan (1958) is recognized as her magnum opus. She was also coauthor with Conrad Taeuber of two famous U.S. census monographs, The Changing Population of the United States (1958) and People of the United States in the Twentieth Century (1971).

LUNDE: Irene, can you tell us a bit about the history of our organization?

TAEUBER: You've talked with a lot of people about the Association. You've gotten something of the nature of the group-- its exclusionary tendencies with reference to birth controllers; its concept of its own purity but also its uncertainty about that purity in that it did not put "scientific" in its title as the International Union did. I'd like to give a bit of a different perspective on the development of this association.

If you go back to the late 1920s, there were a few spots in the country where population analysis was developing. One of those was Warren Thompson, who had gotten the Scripps Foundation [for Research in Population Problems] interested in the dangers of the growth of the Far Eastern population. Thompson brought in P.K. Whelpton and where they started was with Thompson's continuing interest in the Far East and China and his broad general interests, along with Whelpton's interest in the development of demography, population projections. You had these two men in a basically isolated area.

At the Metropolitan Life Insurance Company, you had, again, two men: Louis Dublin, who was a great humanitarian in that he strived for the immigrants and the reduction of mortality, paired with Lotka, who was one of the most imaginative statisticians that we've had in the field, who did more for theoretical demography than anyone else.

At Hopkins, you had Lowell Reed and Raymond Pearl, again a diverse pair, whose skills complemented each other and who made Hopkins the classic center for the logistics theory of population growth and the earliest of the studies of the reproductive history of women.

There was E.B. Wilson at Harvard. And then there was Frederick Osborn, who belongs to the noblesse oblige class which is so rare, who went into this despite wealth and background, who worked with and basically had a protege in Frank Lorimer. This again became a productive team.

In the census of 1930, you had the perfecting of traditional methods of census-taking. Vital statistics were still tabulated in simple form in connection with the census. Statistics-collection had not been modernized.

There were basically no population problems so far as the nation was concerned. There had been the great problem of immigration in the early part of the century, but with the Act of Exclusion, that problem was solved. The farmers were moving to the city, but this was regarded as a natural process. Birth rates were going down. The concern was on the margin--people who wanted to spread family planning but for the rights of women, not for population purposes--sort of a feminist movement against men. Some concern over depopulation was beginning, in association with the work of these few demographers. And it was about this time that the Milbank Memorial Fund employed Frank Notestein and Clyde Kiser to do population research.

Now, the natural process of the development of the Association in this period was the contact between the very small number of specialists in the field, and that is basically what the early Association was. If they had not had scholars, if they had had open meetings, the Association still would have been very, very small.

The real development in the field began with the 1940 census, which was the first professional census. It was also in this period that Hal Dunn was brought into the Office of Vital Statistics. There was the move to expand professionalized [staff?]. There were all the tremendous demands of the war period for data and for analysis. In the international arena, the appropriate staff of the League of Nations [Economic, Financial and Transit Section] moved to this country. The Scripps Foundation had been established [1922]; this was the population research center. The Office of Population Research at Princeton was established [1936] as really the first professional demography training center, other than what Walter Willcox taught at Cornell. So, I think we do not have to damn ourselves for being establishment exclusionists.

But, however much we were, this was a small group which fulfilled the function of getting together the small number of professionals in the field, of having them talk and stimulate each other. It provided the basis from which projects such as the Indianapolis Fertility Study could be developed. The Milbank Memorial Fund played a major role in providing funding and Frederick Osborn in handling funding, making it possible for people in the field to go into field studies. All this moved along with the increasing sophistication of statistical analysis in general.

After the Second World War, with the GI benefits, we had the first major influx of students into the field. In the same period, we also had the beginning of training of students from foreign countries. We had the beginning of United Nations involvement with the Population Division. We had an expansion of trained people in a wide range of universities.

A small "in" group had carried the field to date. A significant change took place in the 1953-54 period. It was the 1954 meeting that achieved the democratization of the PAA. The democratization of the Association meant basically the liquidation of the existing system whereby the members elected the Board and the Board elected all the officers. With democratization, the members elected the officers. That occupied two or three annual meetings of the Association. We all virtually memorized Robert's Rules of Order, because part of the blocking strategy was to use other rules of order on us. At the Charlottesville meeting in 1954 [when Irene Taeuber was president], we finally achieved the amendments of the constitution to provide for direct election. [Clyde Kiser claims that this amendment of the constitution occurred during the 1953 meeting when he was president. See his interview of April 1973 above.]

LUNDE: Fascinating development, isn't it?

TAEUBER: It is. I think our friends who are feeling guilty for having been an elitist establishment are in part still classifying themselves as elite. We were a very small group. Nobody else knew this field. In this early period, there was the question of what we were to be called. Pat Whelpton and other people wrote articles in which they talked about the people who were working in this field as "populationists." And some of us decided that whatever we were or were not, we were not going to be "populationists." So at conferences we started slipping in the word "demography."

That is how a new field got itself an organization. The development of the field and development of the organization went together. We've seen an intensification of that in recent years. With the movement of plans and programs into the field of fertility control, we have again the question of what demography is, what the field is, what the relation is between various sections.

LUNDE: Yes, this came up at one of the sessions this morning.

You said something, Irene, that nobody else has pointed out, or at least you've implied it, that the Population Association has actually been a kind of nucleus around which the field of demography itself has been able to form.

TAEUBER: And also in which the field of population studies has advanced. Because as this field has developed in this country, there has been [peer demography?]. There was the attempt at California [Berkeley], which is now apparently terminated, for a department of demography. There is the move at Pennsylvania for a degree in demography. I think there's an M.A. in demography at Chicago. But on the whole, the American demographer has been a specialist in another field, who was also competent in the population field, which was relevant to this other specialty.

So almost all of us were members not only of the Population Association. In the past, the major recruitment area for demographers in this country was sociology. So we were members of the PAA; we were members of the American Sociological Association; we were also members of the American Statistical Association, because we were quantitative.

Population institutes were established, but they are not organic parts of universities; they are basically external agencies. They are likely to have core people who are on university faculties with tenured positions. But the development of teaching has also been important and this has been associated with development in the university departments. It has been a process of the person who has a degree in economics and is also sophisticated in the techniques of demography, goes to a university and he introduces a course in demography and the students come. The geographer who has training in demography introduces a course. So demography has developed along with the broad field of interrelated population studies. And PAA has developed along with the other associations.

LUNDE: How did you first get interested in this whole business?

TAEUBER: From a multi-disciplinary background; being interested as undergraduate and graduate in a rather wide area of the field. I have my degrees with majors in sociology and economics. My major electives and activities moved from a zoology department and human biology to an M.A. thesis using [anthropological data]. I did a thesis, in the sociology department, on "The Inheritance of Pigmentation in the American Negro," which Raymond Pearl published in the first or second issue of Human Biology [Volume 2, September 1929, pp. 321-381]. So I became a discovery and protege of Raymond Pearl.

Then my interests broadened in Washington in the 1930s, when Frank Lorimer and I started Population Literature. We were both among the disenfranchised who had spouses in the federal

service. We would get a little grant to pay ourselves retrospectively and get out another issue. We did that for two years. Then when the Office of Population Research at Princeton was established, this became an obvious thing they could do, so they enlisted me in Population Index.

LUNDE: Did Population Literature have the same format, the same content, as Population Index?

TAEUBER: Very similar. Having continued with this, it was to the advantage of Index that I was in Washington; you did see the world literature. In the meantime, I'd been involved with problems of a changing population. What attracted me so was that all we knew of demography was generalized. . . was basically Swedish demography. I found myself in Washington. I was not interested in teaching sociology at a local university and I did not want a government job, because I had two young children. So I decided to survey the world and determine what areas of research would intrigue me, that would put me off in a corner with no time pressures, would not be headline-getting at all. This process in a short period of time led me to a great interest in China, but there wasn't anything you could do on it. The approach to China--the most interesting area in the world--was obviously the question of what was happening in Japan, which is non-Western, non-Christian, non-everything. But it was urbanizing and industrializing and it obviously had declining fertility. If you look at this region, then the companion area for Japan was obviously Java, Indonesia. I reconnoitered these areas and discovered that the sources for Japan were not in the United States or any of the libraries in the Western world. I got a bit from moving into analysis of Indonesia.

In the meantime, of course, I'd gotten mentally involved in the demography of Europe, during the war.

So it was sort of accidental. But it was within this framework of picking what I thought were the interesting and significant questions.

LUNDE: But you've done a lot more, haven't you, in subject areas other than simply Japan and the Far East?

TAEUBER: Well, the only area I've neglected is Africa. The nearest I've come to Africa was the UN 1948 report on the population of Tanzania. Other than that, I've stayed away from the African part. The Latin American connection came with the Index. Living in the Library of Congress, I also did the General Censuses and Vital Statistics in the Americas [1943]. But in terms of analysis, my work basically has focused on either the Western Pacific or the U.S.

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TAEUBER: This is the other side of women's lib--the society which offers few opportunities to women when we happen to be married or has rigid requirements as to the kind of job we have when we have children. What this world--which may or may not be passing--gave was opportunity to educated women. How widely spread it was, I just don't know. But the great advantage that you have--or that I had--is that I was in a setting where I could be and remain fundamentally interested in a subject area. And deciding what I did on the basis of what I was interested in doing, somehow I did the important topics. The world moved along and, by sheer chance, the subject areas I was interested in suddenly became the world's priority areas.

LUNDE: Back a few years ago, you and I were talking, I guess it was at the Indian embassy or something, and you mentioned that your next big project was to be China. Whatever happened to that?

TAEUBER: I've been involved in that to the hilt in the last few years. We're producing now a monograph not on China but on the population of the Chinese cultural area. If you define this broadly enough, the Korean culture is based on that of China. The Chinese themselves have moved into the rim around China. So you can begin the analysis for Japan and for China in the middle of the [18th] century with the registration system. Over the century from 1850 onward, you can analyze records of areas which are either Chinese or Korean. Some of the most valuable records are in areas of Chinese or Korean population which the Japanese conquered and on which they then imposed the Japanese [culture]. Of course, with the extension of Imperial Japan, you have the magnificent data for the 40 or 50 million Chinese in Manchuria. And there are the data for Taiwan.

Then you have all kinds of special studies, occasional studies of one sort or another, throughout this period, for the mainland of China. So far as China itself is concerned, the only data that will ever exist for the pre-Communist period now exist. Nobody will ever have any more, so that you can do whatever is possible. So far as the People's Republic is concerned, its population age six and over at the time it did the registration of 1953 was the population the Nationalists had studied at age zero and over in 1947. Now, you don't have the migration data for the People's Republic; that is the great hiatus. Nonetheless, the demography of all those populations is extraordinarily uniform. There is the swift [demographic] transition in all the populations, without exception. There is obviously transition in China. And I think that rather than hold all the data in the files until the People's Republic release their data--which may be one year, ten years, 20 years--the thing to do is write it up. That has the great advantage of making the uncounted volumes of statistical sources known not just to students here where these are foreign languages, but also in countries where there are no problems because all the students are Chinese.

LUNDE: Going back to the Population Association, do you remember anything about the first meeting you went to, where it was?

TAEUBER: I'm a bit blank on early meetings, because . . . You see, my older son was born in 1933, my younger one in 1936. And with a husband, household, children, and career activities in which one is interested, the things that were skipped were meetings and conferences. And particularly anything that took me away. So my consistent attendance at the meetings didn't really come until about 1940.

LUNDE: I see. Do you remember any of the issues that interested demographers in the 1940s and in the 1950s, when you were [PAA] president?

TAEUBER: The great interest in the 1930s, of course, and the great interest through the war years was the imminent decline of the population of the United States. There were even bills introduced in Congress in the 1940s to get the birth rate back up. A population toboggan slide was predicted. So you had on the one hand the deep concern with depopulation and on the other hand a concern with regional distribution and stimulating migration. Getting higher levels of living among the Appalachian poor and the blacks of the South, and so forth. I think this question was relevant, because as long as I've known demographers in the Association, they have always been concerned--with a missionary zeal--about the South. Now, after the war, the concern eventually became the population explosion, the upward movement of fertility, the projections of astronomic numbers sometime in the future. There were studies to assess how you could prevent these births which were leading to an overpopulated America.

I did a comparison recently with an article from Milbank on the development of policy. The initial policy development was the immigration commission of 1910. This was a successful one that resulted in the Oriental exclusion; that solved the problem.

The second was the Committee on Population Problems of the National Resources Board, which issued The Problems of a Changing Population [1938]. At that time, the problem was the declining fertility, plus the pileup in rural areas. There were no actions taken on the basis of that commission report, in part because its area was not one on which there was national consensus.

The next great move was President Johnson's committee and the Nixon Commission on Population. This commission was appointed within the context of the population problem being essentially one of the limitations on urbanization and population redistribution. It's obviously too complex to go into briefly. The report of the commission [Report of the Commission on Population Growth and the American Future, 1972], like the 1938 one, again had no national consensus as a basis for immediate implementation of policy. This was in part because we again got our commission at the end of the trend, so that by the time they came out with their report, the dynamics had shifted in the other direction [the baby boom was long over and U.S. fertility was down to replacement level]. The Problems of a Changing Population came out in 1938 and by the 1940s the birth rate was moving up.

LUNDE: Irene, what do you think of the Association as it is today? You mentioned earlier that the original Association was pretty intimate.

TAEUBER: But the present Association is intimate, small, compared to . . .

LUNDE: The American Sociological Association.

TAEUBER: How many people are at the American Public Health Association meetings?

LUNDE: Oh, seven or eight thousand.

TAEUBER: Or the Association for Asian Studies? This is still a fairly small association. One of the things I don't understand is why it isn't a much larger association.

LUNDE: Do you think that's because there have been interests in the past which have been against advertising the Association to a wider audience?

TAEUBER: No. I think it is in part because population remains a multi-disciplinary field. Take this meeting [New Orleans, 1973], many people have come because of the attraction of meeting people from diverse fields. Now, to have an intellectual area which overlaps people in many disciplines, the Association has to see that it maintains a central core of concern with population. If it became a massive association which welcomed anything that included any population and was relevant in any way, it would fritter itself away to where it was no longer [exclusive?] in population and no longer had an impact. I think the great contribution of the Association in the last several years has been in providing the forum in which those who are involved in fertility action programs can learn to communicate with demographers, and vice versa. In which you have common scientific and technical respect and understanding of work in great areas that overlap and in which one complements the other.

LUNDE: Wouldn't that explain in part why the Association remains somewhat small?

TAEUBER: As long as it refrains from become the crusading, headline-grabbing type of organization, then it remains small. If it becomes that type of organization, it makes no contribution to scientific study. The problem of size seems to me to come only in terms of the resources available.

LUNDE: I remember when Don Bogue put out that particular ad on Demography [in the early 1960s, advertising Demography, of which he was first editor (1964-68) as a benefit of PAA membership] and the membership jumped overnight, it seemed to me, from about 800 to about 1,200 [802 in 1963; 1,142 in 1964, 1,375 in 1966], and we seem to have been increasing slowly ever since.

TAEUBER: But the fertility control field seems to me to be precisely comparable to what I would like to call the mortality control field. There are small sectors of the whole area of medicine and public health that overlap areas with demography. We don't study the techniques of open heart surgery. We do have common concerns in major causes of death, patterns of mortality, and rural-urban socioeconomic differentials. I think there is a similar thing in this active interest in the fertility field. There are only limited areas of overlap. The demographers are not experts in techniques of contraception; these studies are not demographic studies. At the same time, there are demographic techniques of evaluation which may be marriable. As I say, there are overlap areas. But the great majority of supervisory and administrative staff in the fertility field are not demographers. The demographers will never become cognizant of the problems of program administration. But there are overlap areas. In this initial mailing [responses to Demography advertisements], you may have gotten a fair portion of the people who were concerned with the overlap areas.

TAEUBER: Among the people who preceded you as president, there were a number we don't know much about. Did you happen to know Lowell J. Reed [President 1942-45]?

TAEUBER: Lowell Reed was one of the major statesmen of the demographic as well as the public health field. He was professor of biostatistics at Johns Hopkins. He became basically interested [in demography] with this Pearl-Reed development of the logistics curve. Pearl and Reed introduced this to demography and developed it. Lowell Reed himself, who was a New Englander, not only became dean of the School of Hygiene and Public Health at Hopkins, but finally president at Hopkins. Reed was chairman of the annual meetings of the Milbank Memorial Fund in all the years in which the Milbank Fund worked in population. He was the demographer/statistician who, almost more than anyone else, was the responsible person for that.

HENRY S. SHRYOCK, Jr.

PAA Secretary in 1950-53 (No. 7) and President in 1955-56 (No. 19). Interview with Jean van der Tak at Dr. Shryock's home in Southwest Washington, D.C., overlooking the Potomac River, February 5, 1988.

CAREER HIGHLIGHTS: Henry Shryock was born and grew up in Baltimore, Maryland. He received the B.A. in mathematics from St. John's College, Annapolis, Maryland, in 1932; did graduate work in sociology at Duke University; and received the Ph.D. in sociology from the University of Wisconsin in 1937. In the fall of 1936 he joined the newly established Office of Population Research at Princeton as its first Research Associate. In 1939 he went to the Census Bureau, where he had worked for short periods earlier, and remained there for 30 years, mostly as Assistant Chief of the Population Division. From 1970 to 1987, he was a lecturer at Georgetown University's Center for Population Research and the succeeding Department of Demography. He has been a consultant for the United Nations in India and Chile, for the U.S. Agency for International Development and the Population Council in Korea, and for the Organization for American States in Surinam. Among his many publications, focused particularly on migration and techniques of demographic analysis, he is author of Population Mobility within the United States (1964), coauthor (with Jacob Siegel, PAA President 1980) of the landmark two-volume manual, The Methods and Materials of Demography (1971; revised edition, 1980), and coauthor of Systems of Demographic Measurement: The Conventional Population Census (1976).

VDT: How and when did you become interested in demography?

SHRYOCK: As you know, I went to St. John's College in Annapolis. This was several years before they instituted their famous Great Books program, by the way. Although I had a major in mathematics, I received in my senior year a senior fellowship which enabled me to do anything I wanted to do. I was frankly having increasing difficulty in keeping up with advanced calculus and differential equations and this was in the Depression and I was more concerned at that time with social issues and human behavior. So I took my first course in statistics, which seemed to be a sort of in-between area between social science and mathematics. I also took several courses in economics, one of which was taught by Professor Julian Duncan, who was [later] a member of the Population Association of America. His specialty was the railroad [ministry] in El Salvador. In his seminar, we were given various term paper topics and I chose the one on the Italian population surplus; this was back in the days of Mussolini and Mussolini was in favor of sustaining the already high birth rate and checking the emigration which was still going on to North and South America.

I worked on that during my Easter vacation and decided to come over to the Library of Congress for a week to study things there that I couldn't get at Annapolis. Duncan gave me introductions to two statisticians or demographers. One was R.R. Kuczynski, who developed the net reproduction rate; he was at the Brookings Institution. The other was a fellow named Max Sasuly. Well, it seemed that they didn't get along at all, so if I saw one I couldn't see the other. As it happened, Kuczynski was out of town, so I saw Sasuly. He told me a lot about population sources and I also visited the Brookings Institution, which was then on Lafayette Square. I wrote this term paper, and I ran into all sorts of measures I hadn't heard about before, such as the net reproduction rate. And I discovered an immediate love for population statistics. So I continued.

Then I went to Duke University for one year of graduate work.

VDT: Why did you choose Duke?

SHRYOCK: I got a scholarship. I was going to go closer to home [Baltimore] in economics and wanted to take sociology at Johns Hopkins, where there was an old school classical economist, Hollander, who thought that sociology was only a fit course for a ladies' finishing school. So I went to Duke. My mother got me interested in sociology; she'd studied it at Goucher.

So I went to Duke for a year and I was rather disappointed. They didn't have any statistics courses at all at that time, and Professor Ellwood was an old school sort of normative sociologist, who had no use for quantitative research. But I ran onto a very interesting professor in the economics department by the name of Joseph J. Spengler, who taught a course in population, and I took three or four of his courses. At the end of the year, Professor Ellwood said if I couldn't make up my mind as to whether I wanted to be an economist or a sociologist perhaps I should move on.

By that time, I'd applied to the University of Wisconsin, which was a fortunate change. I got another scholarship there. I'd been attracted to Wisconsin by Professor Edward Alsworth Ross, who was a towering figure in the field of sociology. He was chairman of the sociology department and he was also interested in population. He'd written a book called Standing Room Only? [1927], which was translated into German as Raum fur Alle? and I read that in order to pass my requirement in German reading ability.

But Ross was in his declining years and I discovered there was an up-and-coming faculty member by the name of Samuel Stouffer and the other graduate students said, "You should get well acquainted with him." So I did. My major was in social statistics under Stouffer. Ross and Stouffer had a joint seminar on population.

Then Stouffer suggested that I apply for a predoctoral Social Science Research Council fellowship and that I go to the Bureau of the Census, where they were having difficulty at that time making postcensal population estimates. I put in a proposal and was accepted and became a "special agent" at the Census Bureau, for which I was paid a dollar a year by the government. I never collected the dollar; that was apparently the only way they could employ me and give me access to their materials. I stayed there for nine months and finished my dissertation on postcensal population estimates. This was in 1935-36; I'd finished all my coursework. There I met John Durand, who was a sort of intern, and we did some research together on population estimates.

VDT: Wasn't he already at Princeton's Office of Population Research?

SHRYOCK: No. He had just finished up at Cornell University, where he'd been the research assistant to Walter Willcox, the eminent demographer who lived to be a centenarian and who'd also been at the Census Bureau before. John's uncle, E. Dana Durand, was a director of the Census Bureau during the Theodore Roosevelt administration.

Apparently the Census Bureau liked what I was doing, so that summer they hired me to work on a regular salary.

Meanwhile, somebody put me in touch with Frank Notestein, who had been at the Milbank Memorial Fund, but for whom the Office of Population Research had just been set up at Princeton University. I met him, probably, at a PAA meeting. [The first PAA meeting at Princeton, the fifth annual meeting, was held in the fall of 1936.] De Witt Clinton Pool, Notestein's administrative boss at Princeton, came down and interviewed me. I remember we had lunch at the Metropolitan Club here in Washington. Well, I was hired and I went up to Princeton in the fall of 1936.

At that time, our staff consisted of Frank Notestein, who was the director of the OPR, myself--I was a research associate--John Durand, who was our graduate student, in economics, who worked part-

time at the office, and a secretary named Martina DeHoll.

VDT: You were the research associate--somewhat senior to John Durand?

SHRYOCK: I was about to get my Ph.D.; he hadn't done any graduate work up to that time. I had done all my work when I went there in October 1936. I had submitted my thesis; I actually got my degree in June 1937. My thesis was on postcensal population estimates in the U.S. We developed some methods using what we called symptomatic data, which are still used to some extent by the Census Bureau, although they've introduced other methods as well and elaborated on that one. Some of my earliest publications were on postcensal estimates.

VDT: Can you tell me a bit about those very early days at OPR?

SHRYOCK: I spent three plus years at OPR. In January 1937, I got married to Annie Frances King, whom I'd met while waiting on tables in summer school at Duke University. I was working on a master's thesis on child labor in the Depression. I never finished it because when I got to Wisconsin I realized that my statistical methodology was inadequate. But I did learn something about interviewing for the first time, interviewing children who were out of school and out of work. Annie Frances, who was there getting some necessary sociology so she could go on to the school of social work at William and Mary, helped me do my interviewing.

VDT: You got an early start at hands-on experience in collecting data, which I think some of today's demographers might lack.

Going back to OPR. I interviewed George Stolnitz a couple of weeks ago; he was there in the 1950s. But you were one of the earliest ones.

SHRYOCK: I remember meeting George Stolnitz; that was when I'd finished at OPR and gone back to the Census Bureau in 1939.

At OPR we were working on several things. One was that we had to edit Population Index, which we'd taken over in its third year. Irene Taeuber and Frank Lorimer had been handling it on their own [in Washington]. Frank Lorimer sort of bowed out [to become Technical Director for the Committee on Population Problems of the National Resources Board]. The name was changed from Population Literature to Population Index. By the way, I think it was De Witt Clinton Pool, the man that interviewed me, who came up with that name.

Then we had some fertility data on own children under age five. They were unpublished data for the east North Central states from the 1930 census fertility question that Notestein had copied from the census schedules some years before. I was working on those. I never did finish that study. Notestein got one published article out of it ["Differential Fertility in the East North Central States: A Preliminary Analysis of Unpublished Tabulations from the Family Cards of the 1930 Census," Milbank Memorial Fund Quarterly, April 1938].

I did some part-time teaching; I'd been a teaching assistant at Wisconsin. At Princeton they had a course in junior readings in economics, in which some of the economics students participated. We read things that applied in fringe fields of economics--Malthus, for example. It was a sort of tutorial. I met the students individually and they turned in a term paper once a month or so.

There were some old-line economists in the economics department, like Kenmerer and Fetter. Fetter had been at Stanford with E.A. Ross when he was kicked out by the Southern Pacific Railroad for what they thought were radical activities advocating the silver standard or whatever. Fetter didn't quite agree with Ross but had resigned in protest from Stanford at that time. Ross then moved to Wisconsin and Fetter moved to Princeton. The attitude of some of them in the department of

economics toward Notestein, who was attached to that department, was one of jealousy, because he was spending most of his time doing research and very little teaching. They thought that doing research should be a reward for 25 or so faithful years of teaching.

VDT: These students whom you had at Princeton were actually in other departments?

SHRYOCK: They were all in economics. At that time, there was no sociology department at Princeton. That came after I left. Kingsley Davis and Wilbert Moore and some others came in and established a sociology department. Fetter always insisted, however, that the economics teaching include social economics--sort of institutional economics a la John R. Commons.

VDT: So you were teaching this tutorial at OPR as well as doing research. What then took you back to the Census Bureau?

SHRYOCK: The 1940 census was coming up. I'd worked with Leon Truesdell, who was Chief of the Population Division. Philip Hauser had just come in as Assistant Chief and I'd met him a number of times at the University of Chicago and so on. At that time, I think there were only three or four PhDs in the whole Bureau--two were Calvert L. Dedrick and Stuart A. Rice, both sociologists--and one M.D. who was in charge of Vital Statistics, which was not transferred to the Public Health Service until 1946. I was offered a job and thought I'd like to work on the census; I'd met people who had worked on the 1930 census. They had just a small skeleton staff between censuses at that time. There were no Current Population Surveys or anything of that kind to keep things going.

So I came back. I thought I'd just be there for the duration of the census period, but I never went back to Princeton. I stayed there for 30 years or so.

Hauser gave me a choice of what I wanted to work on and I chose migration. I'd done some work on that previously. That first term paper I'd written at St. John's on the Italian population surplus, I'd brought that up to date somewhat at Spengler's suggestion for a population problems course I took with Spengler at Duke. In working on population estimates, I got convinced that the key to population estimates was better measures of internal migration. The U.S. had birth and registrations systems then--they were nearly complete--but we had no population register like Sweden's, so we had to find various indirect ways of measuring migration. And the 1940 census was the first census to have questions on current migration--residence five years ago. Since 1850 they'd had a question on the state of one's birth but nothing current. So I was put in charge of that.

VDT: Did you think up the questions?

SHRYOCK: No, Truesdell thought up the questions. They were determined before I got there. Unfortunately, the wording led to some difficulties, so we had to have a special coding and editing section to deal with these obviously wrong and ambiguous entries. I was put in charge of that and we eventually had 30 clerks just working on these problem portfolios. This was part of the preparation of the 1940 census data for publication.

VDT: Were those questions on the 100 percent schedule?

SHRYOCK: Yes. There were just a few questions on the sample questionnaire.

VDT: How large was the sample at that time?

SHRYOCK: I think it was three and a third percent. I'm trying to remember if fertility--children ever born--was on the sample or not. Paul Glick was in charge of that, assisted by Wilson Grabill. Glick was actually first under Richard O. Lang, a Chicago sociology Ph.D., who left early in World War II to work for Averell Harriman. W. Edwards Deming and people he recruited worked out the sample design for the first large-scale census sample, used with the 1940 census.

VDT: I recently read your article on "Data Collection in the United States Census" in the International Encyclopedia of Population [1982] where you dealt with such things. A sample of three and a third percent seems miniscule compared to the one we may now have for the 1990 census.

SHRYOCK: Well, we eventually had a 20 percent sample. I'd have to refresh my memory on whether we started off with that in 1940 or moved to that in 1950. Of course, in 1950 we put a great many more items, including migration, on the sample. Fertility was on the sample. I don't think fertility was ever a 100 percent question, but I may be wrong.

VDT: I've heard tell of the "class of 1940" at the Census Bureau, the group that came in about that time and you got to be quite well known. Can you tell me something about that and who those people were?

SHRYOCK: The story behind that was this. This was in the Depression. There were still lots of college graduates and even PhDs who didn't have good jobs and the 1940 census was a good opportunity for those in statistics and some other social sciences. W. Edwards Deming hired some of them to work on the sampling. He was a sampling specialist who had come from the National Bureau of Standards. He's still living here in Washington and carrying on a consulting business; he's well over 80. He's the one who introduced quality control to Japan. He spent several years at the Census Bureau; that wasn't his main background. I think he got his Ph.D. in physics.

He hired a number of these underemployed statisticians. There were Joe Daly, whom I'd known at Princeton and who had just received his Ph.D. in mathematical statistics under Sam Wilkes, Bill Madow who's now retired at Stanford, and a great many others. A lot of them got their jobs off a fingerprint register, for some reason or other. They came in as junior professional assistants and were paid the magnificent sum of \$1400 a year. I had a princely salary; I was what would now be a GS9, which was then called a P3, and got \$3200 a year. Incidentally, when I first went to Princeton as a research associate in the OPR, I got \$2500 and the Princeton instructors mumbled about that--as their professors did about Notestein--because Princeton instructors were only making \$1800 a year.

VDT: Instructors have always felt they were underpaid. Others of the "class of 1940"--did Paul Glick come at that time?

SHRYOCK: Yes, he did.

VDT: I know Jay Siegel came about 1942.

SHRYOCK: Yes, he came later, and so did Henry Sheldon and Hope Eldridge. They used that have a joke that when people got up from their desks for lunch or a break you were deafened by the jingle of Phi Beta Kappa keys.

VDT: So you were an elite group!

SHRYOCK: Many of these people went on to quite prominent positions. A. Ross Eckler, who was a Branch Chief in the Population Division in 1940, rose to become Director of the Bureau. Ed Goldfield, who was in charge of international statistics for the Census Bureau at one time, is now at the National Science Foundation. Sam Greenhouse and Marvin Schneiderman, good friends of mine, are statisticians at the National Cancer Institute. A great many of them advanced not only in the Census Bureau, where they became assistant chiefs, chiefs, assistant directors and so on, but in a great many other government agencies as well. As you know, the Census Bureau was responsible for the introduction of scientific sampling into government data-gathering, so these were disciples who went out and helped establish up-to-date statistical methods in other agencies, foreign as well as domestic.

VDT: It must have been a wonderful place to work.

SHRYOCK: We had a great deal of esprit de corps. We got along very well--fighting the old fogies who were very dubious about this sampling.

VDT: Did you have anything to do with setting up the Current Population Survey?

SHRYOCK: No, I didn't. The Current Population Survey was really brought over from the Works Progress Administration, which was then in its decline. I worked there one summer when it was the Federal Emergency Relief Administration. They developed what was called the Monthly Report on the Labor Force, which was done in collaboration with the Bureau of the Budget, under Margaret Martin, and with the Bureau of Labor Statistics under Seymour Wolfbein, and Gertrude Bancroft had worked at WPA under Fred Stephan. That activity was transferred during the war to the Census Bureau. At that time, I was away for two years in the South Pacific, doing operations analysis with the Far East Air Forces, and when I came back that was already well established.

They changed the name to the Current Population Survey to protect it from being taken over by the BLS. That effort eventually failed and the publications functions were taken over by the BLS in the late 1950s, but the Census Bureau retained the fieldwork and tabulation. The analytical work was in the Population Division at that time where I was Assistant Chief, so I had contacts with it.

Furthermore, they began carrying all these supplementary inquiries, dealing with questions other than the labor force. One of the earliest of those was on migration, having to do with how much wartime migration had been stirred up and then later, whether or not these people were going back to their old homes after the war. Of course, they weren't.

VDT: "Where were you a year ago?"--those questions, asked once a year?

SHRYOCK: Yes. So I came on early for that and I guess I was instrumental in getting that established on an annual basis, which it still is. That was one of my pet interests, that annual migration survey. Of course, we also had supplementary questions on fertility and marital status and education and other social and demographic topics.

VDT: One of the big things you did in the last years before you left the Census Bureau, you and Jay Siegel were preparing the monumental Methods and Materials of Demography, which was published in two volumes in 1971 and in an abridged volume in 1980. That has become the bible for students in the field. Can you tell something about how that got started and about putting it all together?

SHRYOCK: That was actually my second book. I'd written what started out to be a census monograph for the 1950 census. That was called Population Mobility within the United States. It was

published in 1964. I didn't finish it in time to be published in the series, because unfortunately I went off to India for a year during the period I was working on it, so I got behind. But, fortunately, it was published for me by Don Bogue at the University of Chicago, with whom I'd had many contacts at the Census Bureau and elsewhere.

To get back to Methods and Materials of Demography, we were approached on that by people at AID--Joe Cavanaugh and some others were the contacts. They wanted a reference book, a how-to-do-it work manual, for training demographers around the world and approached Siegel and myself and we agreed to do it. We got some support also from the International Statistics Office in the Census Bureau, which was then under Charles Lawrence, who had spent a lot of time in Korea and other places overseas.

Now, Siegel and I had squirreled away some drafts of old chapters which we had worked on some years before with Margaret Hagood, who was at the Department of Agriculture. We three were going to write a book of somewhat the same scope on a much more modest level. Then she died rather tragically and we had laid that aside. So we went back to those drafts and were able to make some use of hers and our own.

VDT: Did you and she at that time--in the 1950s--think such a comprehensive manual was lacking?

SHRYOCK: We definitely did, that was why we were working on this. A.J. Jaffe had published a demographic handbook which was very useful, but it didn't cover the field systematically.

Siegel and I soon saw that we couldn't handle this whole job ourselves in the time allotted, so we began recruiting a number of other prominent authors--Abe Jaffe, Paul Demeny, Paul Glick, and many others--who were all duly recognized in the editions of the books that were put out by the Census Bureau, although I'm afraid they are somewhat slighted in the commercial one put out [in 1980] by the Academic Press. I had to spend almost as much time organizing, editing and rewriting some of these other chapters as I did doing the chapters that I wrote myself. So it was a tremendous job and we were always way behind schedule and way beyond budget, so AID and the Census Bureau were despairing that we would ever finish it. But fortunately, we finally did.

VDT: Were you able to work on it in office time? You had to do your other work too, of course.

SHRYOCK: I tried to do both, but eventually I could see that I couldn't do my regular work, so I was put on the book full-time. That didn't mean that I didn't work overtime almost every night and every weekend for two or three years.

VDT: A tremendous job, indeed, but it did become the bible of the field. I must admit that one of the handiest things I find about it is I put the two volumes together and sit on them and it makes me just the right height for my typing table.

You mentioned your year in India and you spent some time in Korea. What took you to those places and can you tell a bit about them?

SHRYOCK: Start with India. I went there for the UN, but it was largely financed by the Population Council; they put in a lot of money and recruited me. I got leave of absence from the Census Bureau, this was in 1957-58. We went just for seven months, which was all I could get away for.

Dorothy Thomas, who was a good friend of mine--we had family ties back in Baltimore--had gone over there and set up this research and teaching program, which was to take students both from India and from the other ECAFE countries. They got funding from the government of India [and the UN] and had an Indian advisory board. It was located outside Bombay, in Chembur; then they

changed the postoffice address to something else.

We didn't have our own quarters at that time. We were housed in the quarters of the Tata Institute of Social Sciences, which is still there. Tata is the big Parsi industrial complex in India. Our good friend there was a criminologist who had studied at Ohio State, an Indian named J.J. Panakal. K.C. Zachariah was there then as an instructor, along with K.V. Ramachandran, who taught statistics and had gotten a degree shortly before from the University of North Carolina. Parker Mauldin, Margaret Bright, and I were pretty much the teaching staff. Our chief was K.C.K.E. Raja, a descendant of the Rajas of Malabar who had received St. Thomas when he came over to establish the Thomist Christians very early in Indian history. He had studied public health in London as a young man and had his doctorate from there.

It was called the Demographic Training and Research Center. Now it's called the International Institute of Population Studies. The Indian government has taken a much more prominent role in running it than they used to. They still get people from the other Asian countries and it's grown. I think Don Bogue came there just after I left and many other prominent demographers--Japanese, Czech, American and so on--have been there for long or short assignments.

VDT: What was your next overseas post--Korea or Chile?

SHRYOCK: Korea came first. This again goes back to the Population Council. I got leave from the Census Bureau to go there three times. The first time was in 1963 and that was to help them get up official population projections into which they could tie their five-year economic plan. I was surprised that they took those as seriously as they did; they became a bible after a while.

The second time, in 1966, I concentrated on improving their rather miserable vital statistics system and didn't make too much progress on that. I learned a lot about it and visited Taiwan to see why Taiwan was getting along so much better. Taiwan was more of a police state than Korea at that time and registration was tied in with their I.D. system.

Then I went to Korea finally in 1970, still working on vital statistics. They were taking the census and I helped with their census planning, including their migration questions, of course.

I went on quite a few field trips and saw a great deal of Korea. The countryside is beautiful and since I was interested in mountain-climbing, I always managed to get in a lot of hiking on these field trips.

VDT: You obviously have had close ties with the development of demography in the less developed countries, because you also went to Surinam later on in the 1970s.

SHRYOCK: Well, Chile came first. I went there in 1971, before the last days of President Allende, to CELADE in Santiago, the sister UN institution to the one in India. I worked with Juan Carlos Elizaga. Elizaga succeeded Carmen Miro as director of CELADE--and he later married Pinochet's sister. He had some data he had collected on migration in Caracas and I helped him with those tabulations. He'd already written a book about a Santiago migration study and John Macisco had worked on the one they'd done in Lima.

I also did some lecturing, I must admit mostly in English. I had some Spanish but it wasn't good enough to lecture in. Jay Siegel was able to lecture in Spanish when he was there, and Don Bogue, I was told, tried to lecture in Spanish as well.

VDT: You have always been interested in teaching as well as research?

SHRYOCK: Somewhat, yes.

VDT: You went on to many years at Georgetown. Can you say how the students have changed over the years?

SHRYOCK: Well, I came to Georgetown during the unsettled times of the late 1960s and early 1970s.

VDT: During the early days of very small classes. Well, they're still smallish at Georgetown.

SHRYOCK: They were small again when I left. Actually, they were fairly large when I gave my first course; I guess about 10. I never got up to more than 12 or 15 in migration; the last few years, I had only three, four or five. However, the general introductory demography courses did get to be quite large--25 or 30.

VDT: Is that a reflection on migration, do you think? It's always been a little stepsister to fertility.

SHRYOCK: Possibly. Although, you know, we had three courses on the components of population change--fertility, mortality and migration. Migration drew as many or more than mortality most of the time and sometimes it had as many as fertility. In the last years, it had fewer.

VDT: Do you think interest in migration is picking up? U.S. students should be interested in migration, which is going to become the largest component in population change in the U.S.

SHRYOCK: That's true. But the Georgetown curriculum has been completely changed in the last year or two. There's no longer any semester course on migration as such; it's interwoven with other courses. The accent now is not so much on training theoretical academic demographers, though I must say most of our graduates have not gone into teaching, but on applied demography, where we seem to be able to compete better and for which there is more demand--applied demography in business and in state and local government.

Now there's been a revival of migration at Georgetown, what bids fair to be a more influential program, perhaps, at the university level. I just attended a few days ago the inaugural ceremony for Charles Keely, for whom a chair has been set up in honor of the late Dean Herzburg. He will also teach international migration in the Department of Demography. There's a group at Georgetown under Father Bradley which is interested in refugee problems, some research but more on a humanitarian action basis. Keely will be on their staff and also on the staff of the Center for Population Research.

VDT: International migration was a leading issue back in the 1920s and you are saying it now seems to be making a comeback?

SHRYOCK: Well, we're concerned about illegal immigrants and even legal migration accounts for a very large percentage of our annual population growth as natural increase continues to decline.

VDT: The most recent Census Bureau estimate is that migration--taking into account their estimate of only 200,000 net illegal immigration--comes to 28 percent of U.S. annual population growth.

SHRYOCK: That's the level it was in the last decade of the 19th century and in the first decade of the 20th it approached 30 or 40 percent.

VDT: Presumably it will again. One question I was going to ask you was what do you see as the

leading issues in demography over the years you've been involved. Now you've pointed to migration, your specialty, which is coming to the fore again.

SHRYOCK: We still have major measurement problems in migration. With the present administration's emphasis on economy, the Office of Management and Budget was proposing to eliminate both migration and fertility altogether from the 1990 census sample schedule. Fortunately, they've put them back now. The only items they completely cut out were some of the housing items.

VDT: I noticed that your PAA presidential speech in 1956 in Ann Arbor was on changing definitions of Standard Metropolitan Statistical Areas--perhaps that was a measurement problem.

SHRYOCK: They're still haggling over Standard Metropolitan Area definitions. I was a rather strict constructionist. I thought they were making too many and too small metropolitan areas. But I fought a losing battle.

The main pressure for more metro areas continues to come from commercial interests--TV and newspaper owners and the like. That is partly because of Chamber of Commerce boosterism, but also the big national advertisers take the largest markets. So the larger your metro area, the more advertising you get. The suburban metro areas tend to get overlooked. If you're in Orange County, which is closely integrated with Los Angeles, all the advertising revenue from big companies goes into L.A. papers and the Orange County people say, "If we had a separate metro area we could get advertising in our own papers and on our own TV and radio stations."

VDT: What's given you the most satisfaction in your career?

SHRYOCK: I enjoyed my working associations at the Bureau of the Census very much. Some of my longtime friendships were made there--John Durand, Paul Glick, and others. I've enjoyed getting to meet more young people at Georgetown.

And I've enjoyed many of these foreign assignments, although I admit I was sometimes discouraged with the physical conditions in India, where we had no air conditioning, either in our hotel or the office, and we'd ride out there, the dusty route 13 miles every day, in a cramped little Fiat 500 with four of us in there, including the driver, and I'd get home dead tired and wanting to go out at night to the movies--it was about 85 degrees, with humidity likewise. So I felt at times I was working at about 40 percent efficiency while I was in India.

I guess Methods and Materials of Demography has given me a great deal of satisfaction.

VDT: I would think so, because it will go on forever in one shape or another. You laid the basic foundation.

SHRYOCK: Unfortunately, after four printings the Bureau dropped it. The two-volume one is out of print at the Government Printing Office. You can still buy the Academic Press condensed version.

VDT: I'll hang onto mine; it will be worth a fortune someday.

SHRYOCK: It's been translated into Thai and Polish. I don't know whether either of these ever saw the light of day but the translations were finished and we were approached to see if we could help in finding a publisher. At this distance, we couldn't do much about that.

VDT: Now on PAA. You have said that you know you first joined PAA sometime in the mid-1930s

when you were a graduate student.

SHRYOCK: I was at OPR in 1936, so I attended the meeting in the spring of 1937.

VDT: Actually, there was no PAA meeting in 1937 because the IUSSP had a meeting in Paris.

SHRYOCK: Frank Notestein went to that meeting and so did Leon Truesdell. I didn't go; I was much too junior then.

VDT: You probably went to the Princeton meeting in the fall of 1936 [fifth PAA annual meeting; the first in Princeton].

SHRYOCK: I certainly did.

VDT: You said you believed Louis Dublin was president at the time of your first meeting; he was president in 1935-36.

Dr. Shryock made some observations on early PAA meetings, personalities, and issues at the beginning of an interview with Anders Lunde at the New Orleans PAA meeting, April 26, 1973. Here are some quotes from that interview (of which the tape was inexplicably cut off after about 10 minutes):

SHRYOCK: I remember a person who really towered over the organization in a number of respects and had a good deal of influence and never got to be president, because of his untimely death, I suppose, was Raymond Pearl of Johns Hopkins University.

In those days, we used to meet at least half the time in Princeton. One of my earliest recollections is being at that delightful place, the Princeton Inn, which is now a girls' dormitory, I understand [coed, as of the late 1980s]. I think that's one thing we've lost in becoming a larger organization, being able to take advantage of the amenities in the small comfortable inns in college towns, which no longer can accommodate a group of our size.

Probably 50 or 60 would come to those meetings. You would know just about everybody there. If you look at those early programs, you see there were relatively few sessions going over a day or a day and a half--never competing sessions--which made it nice in one respect. You had a complete discourse, everybody being exposed to the same papers.

LUNDE: Are there early meetings that particularly stand out in your memory?

SHRYOCK: I don't think of any landmarks or watersheds. Offhand I can say I heard a great many good papers over the course of the years, particularly by people like Frank Notestein, who was always a prescient speaker, and some delightful ones by Lorimer and Hauser and others.

I remember some interesting business meetings. One occurred I think at Princeton. I was secretary at that time [1950-53]; Rupert Vance was president [1951-52, and would have been president for the 17th annual meeting of April 19-20, 1952, in Princeton]. This was at the time when we were switching over from having the officers elected by the Board of Directors to being elected by the membership. Vance and some others--Dudley Duncan--thought of themselves as Young Turks in favor of this move. I was rather lukewarm. I was never one to think that organizations of this type benefited from maximum democratization. There were a number of people, maybe among the elder statesmen, who were not very keen on this and we got into a terrible parliamentary tangle. There

seemed to be two schools of thought as to whether we should be following UN parliamentary rules or Robert's Rules of Order. I thought we were never going to get ahead.

LUNDE: Of course, it was decided to have the membership vote for the officers.

SHRYOCK: That's right. Now, of course, the membership also elects the nominating committee; again a move about which I'm not very enthusiastic.

LUNDE: Others have recalled problems with black members attending early meetings in Chapel Hill and Charlottesville.

SHRYOCK: There was an earlier tempest in a teapot. We had one black member in the early years who didn't show up at the meetings. But one time in Atlantic City, in 1942, we met at the President Hotel there and Lionel Park, a rather young guy, did come. The hotel wasn't going to give him a room and the executive committee had a hurried get-together and threatened to move the meeting away from there and the hotel caved in.

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VDT: Can you tell me about the early women stars in PAA--Irene Taeuber, Margaret Hagood, whom you mentioned as an inspiration for Methods and Materials of Demography and who was PAA president the year before you, and Dorothy Thomas, whom you also mentioned?

SHRYOCK: I'll start with Dorothy Thomas. I knew her longest because she grew up in Baltimore. She went off to Barnard and then to the London School of Economics. Her aunt and uncle lived across the street from my Shryock grandparents in Baltimore and she used to visit them and I used to visit my grandparents. Now, I never met her on those occasions, but I used to play with her cousins. So I heard a lot about her and her career through the family.

I didn't actually meet her until I went to American Sociological Association and PAA meetings. Then she came east again from Berkeley to Philadelphia after the war and I saw her quite often. I was a consultant on some studies they were doing at the University of Pennsylvania and used to go up there several times a year. She in turn was on our advisory committee at the Bureau of the Census for several terms. And I was briefed by her before I went out to India.

One of her chief interests, of course, was migration. She'd worked with the Swedish population register and was strongly in favor of having a population register in the U.S., which I'm much in favor of in theory, although many people think we'd be bogged down in administration and it would be extremely expensive in a country this size. I remember once talking about a population register at a meeting and advancing some of these objections, which were concurred in by Forrest Linder and others. Dorothy was a discussant to my paper and got up and said I was obviously suffering from "census fatigue."

Incidentally, when I was at the institute in India, they had the inaugural conference for which they brought in a lot of prominent people. Dr. Tachi from Japan was there and Gunnar Myrdal. He and Alva were good friends of Dorothy Thomas; they had worked together in Sweden. So I renewed my association with him on that occasion. I remember talking with him during an interminable bus ride from Bombay up to Poona.

VDT: Alva Myrdal was Swedish ambassador to India then and he was working on Asian Drama.

SHRYOCK: That's right. He was working with Ester and Mogens Boserup, the Danish economists.

VDT: Was Dorothy Thomas there at that time?

SHRYOCK: No, she was ahead of us. She had laid out the program for the most part and then several of us were recruited to do the implementing and the first teaching and research supervision--Parker Mauldin, Margaret Bright, and myself, as well as the Indians I mentioned.

VDT: Margaret Bright and Dorothy Thomas were very good friends.

SHRYOCK: Yes, she'd been a student of Dorothy's. She has spent most of her subsequent years at Johns Hopkins in the School of Hygiene and Public Health.

VDT: I knew Dr. Thomas when she was at Georgetown at the end of her career. I recall she invited students to her home; a wonderful woman. What was she like as a person--or as a demographer, of course?

SHRYOCK: She was a wonderful demographer. She was not a statistician in the modern sense, and I guess I'm not either. But she had a good practical control of quantitative data and made some noteworthy studies.

She was a marvelous person with her students. She gave herself completely to her students. She entertained them and kept up with them. She was a real mother figure for her students, including K.C. Zachariah, who became her executor.

She was a very good hostess. She could lose her temper very frequently and she had some longterm feuds with some other demographers whom I won't mention, which was rather unfortunate because I was friends with both parties. She held her last seminars in her home up in Linden Hill, Bethesda. I was entertained there a great many times along with her students, and you were too.

VDT: Yes, when I was a research associate at Georgetown, where she went at the end.

SHRYOCK: Irene Taeuber died about that time [1974]. She was going to join our staff, had it not been for her untimely death.

VDT: I'm sorry; I never knew that. Now, tell me about Irene. Of course, everyone thinks she was a phenomenon in the field too.

SHRYOCK: She was a real workaholic. She lived on coffee and cigarettes; she never drank anything alcoholic. She worked for years at the Library of Congress, where she did her reviewing of publications and got all these annotations up for Population Index, which she shipped to us at Princeton. Then we had this typist who typed them up for lithographing, whatever they call it now, offset. John Durand and I had to proofread every single issue for the time we were there. Apparently when Norman Ryder came along he rebelled and someone encouraged him, so they took that off the junior staff--Norman Ryder was junior staff member then. Another one was George Stolnitz, who was an early Milbank Memorial Fund Fellow, succeeding in the tradition of John Durand at Princeton. Stolnitz was one of the "class of 1940." He was a sort of assistant to Abe Jaffe; Jaffe helped get him the fellowship at Princeton.

VDT: He also rebelled?

SHRYOCK: It was someone on the staff, I can't recall her name, she credited herself with getting junior staff out of proofreading. It could have been Louise Kiser.

VDT: Well, I gather Irene was a fantastic person. Someone I haven't heard much about but whose name ranked large in those days was Margaret Hagood. Tell me a bit more about her.

SHRYOCK: Margaret Hagood came of an academic family. Her father was president of Mary Baldwin College in Staunton, Virginia, at one time. They had a large farm. They were raised on a farm in Newton County, Georgia, the same county in which Jerry Combs grew up. Margaret majored in mathematics and she taught at a sort of girls' finishing school up here in Forest Glen after she got her A.B.

Then she decided to go into social science, so she went back to school at the University of North Carolina, where she worked with Howard Odum and Rupert Vance and Catherine Jacher and so on and wrote her first book, called Mothers of the South [1939], a small book. Her magnum opus before she came up to Washington was Statistics for Sociologists, which went through two editions. The first she wrote by herself, the second was written with Dan Price [1952].

Then she was hired by Carl Taylor, the rural sociologist, to come up to the Department of Agriculture and be the social statistician on their staff. She worked right here in Southwest Washington where we are now. She determined that she had to make a name for herself in research in her first few years there, so she got a room in a rooming-house in a very rundown area of Southwest but within a short walking distance of the Department of Agriculture and spent all of her time writing and going back and forth at night to her office. Eventually, she got over that and moved to a nice apartment in another part of town.

VDT: Was she married then?

SHRYOCK: No. She was married earlier to a dentist, Hagood--her maiden name was Jarman--but they were later divorced. She had a daughter, who had a rather stormy adolescence and early life. She ended up joining the Zionists and going off to a kibbutz and marrying a major in the Israeli army and she wrote several successful novels in Hebrew, which were translated and published in English. She's still in Israel. Margaret Hagood visited there. This daughter had had a couple of children by a previous marriage, but she had some by the Israeli major as well. Some of Margaret's grandchildren couldn't speak any English at all.

VDT: Margaret Hagood died rather young, didn't she?

SHRYOCK: Yes, she did. She had a heart condition. I don't know whether you want to keep this on the record or not, but her friend Hope Eldridge--she worked for me at the Census Bureau; she took over my population estimates work. She'd studied under Margaret Hagood at the University of North Carolina; she got her Ph.D. there. She'd been working for many years in physical education at the women's college of the University of North Carolina in Greensboro. She went back to school; got her Ph.D. in sociology. She was quite interested in urbanization, so we had interests in common. She stayed at the Census Bureau until a couple of years after I got back from the war and then they were setting up all these UN agencies, including the Food and Agriculture Organization.

Hope Eldridge left the Census Bureau and went up to the United Nations, in Lake Success at first. John Durand was recruited; he was on the staff of the Population Division. Forrest Linder was there in the Statistics Office. Hope had the job of getting out the very first Demographic Yearbook.

What ruined her career at the UN, unfortunately, was that she and her husband had been very active in the Henry Wallace campaign for President; he ran on the Progressive ticket in 1948, I think it was. The Progressives were supported by the Communists, so people who had been active in this movement were very suspect during the McCarthy era. And Hope and a number of other Americans at the UN were grilled by the infamous assistant to McCarthy, Roy Cohn.

She suffered through that. She was fired along with other Americans; she took the Fifth Amendment to protect some friends. Unlike other governments, all UN employees who were American had to be approved by the U.S. government. I don't know if that's still the case, but it was then. The witch-hunters, the Red-hunters, traced the friends of people whom they were then getting after, and the trail led to Margaret Hagood. She was told she was up for a loyalty investigation and because of that she was unable to make a trip to the first World Population Conference in Rome in 1954.

I think that sort of broke her spirit. She went downhill physically from that time, until she finally died of a heart attack about ten years later, but she was in very poor health during most of those years. She was very embarrassed by all this. I don't know why she should have felt at all guilty; she'd never been particularly politically active herself. But she felt she was somehow being disgraced by all this. She was told by the investigators, "Now, you mustn't discuss this with anybody else," which was a silly imposition.

VDT: What a sad story. That McCarthy era . . .

SHRYOCK: I was hauled up too. I got off much lighter than any of these others. But when I came to Washington, there was something called the Cooperative Bookshop and I joined that. They gave dividends. I soon recognized that all the book dividends were by extreme leftwing authors, so I quit after a year. But that somehow got into my FBI dossier, and in the McCarthy era after the war, they called me in to explain what I'd been doing, would I name all my friends whom I suspected of being Communist and so on.

VDT: Good heavens--just because you'd belonged to something with "cooperative" in the title?

SHRYOCK: That's right. For one year.

VDT: Your mention of the international conference of 1954 reminds me of the IUSSP. I noticed that you were one of the 29 Americans among the 147 people who were first invited to be IUSSP members when IUSSP switched after the war from membership by national organizations to individual membership. Could you tell a bit about your involvement with IUSSP [International Union for the Scientific Study of Population]? Were you, for instance, involved in the IUSSP meeting held in New York in 1961?

SHRYOCK: I remember being there. I don't remember whether I had a paper at that time or not; seems to me I was busy working on my first migration book. But I was at the first IUSSP meeting after the war, in Geneva in 1949, and there I had several assignments. The person in charge of our delegation was Henry Pratt Fairchild of New York University, the very first president of PAA. He was quite an imposing character. Quite a gourmet. He was a friend of Oscar of the Waldorf, I remember. He also was suspected of some leftwing activities, with regard to immigration groups. He belonged to some organization and he used to take the membership file home from his office every night and hide it under his bed during the McCarthy era, I was told. He was instrumental in organizing the American delegation to that first IUSSP meeting after the war, along with Frank Lorimer, who was the secretary

of IUSSP at that time. He was the executive director, based in the U.S.

At that 1949 meeting, Wilson Grabill, Calvin Beale and I had a paper forecasting, projecting, fertility for the U.S. I was the only one of the three who went. Frank Lorimer also had asked me to defend the late Thomas J. Woofter, who was the first one along with the Frenchman Pierre De Poid to get onto the idea of generation reproduction rates, rather than cross-sectional ones that had been developed by Dublin and Lotka. Lotka was giving a paper then in which he was very critical of Woofter--sort of brushing this off as, "Well, everyone had known this beforehand and it wasn't very important." P.K. Whelpton had told me that Woofter's work was what really inspired his getting into cohort fertility, which Whelpton developed and led to some of these field studies and so on. So I was supposed to get up and comment on the paper by Lotka, the great man, defending Woofter, which really put me on the spot. I was sort of a friend of Lotka too. But that went off not too badly.

This was a very primitive meeting. We had no simultaneous translation then. When anybody spoke, someone had to get up and give a precis of that in the other official language--French or English; I think there were only two then.

VDT: I met you at the two IUSSP meetings I've attended, Mexico in 1977 and Florence in 1985. Have you attended all the PAA meetings since you joined and all the IUSSP every-four-years meetings?

SHRYOCK: No, I'm afraid not. I've been to about 95 percent of the PAA meetings since I joined, I guess. I missed some during the war, though they were really suspended then.

VDT: They were suspended from 1942 to 1946 because of the war.

SHRYOCK: Yes. Then I missed two or three in the late 1970s and early 1980s because of my late wife's illnesses.

IUSSP--I think I went to about half of those. I went to the one in Geneva. In fact, I went on thereafter to a meeting of the International Statistical Institute in Berne. Then the Rome one in 1954 was mainly a UN population conference, although the IUSSP held simultaneous business meetings and cosponsored some of the sessions. The next one was Vienna in 1959, just after I got back from India. Then New York in 1961. Then Belgrade in 1965, which again was a UN population conference. Then I think the next one was Mexico, and finally Florence.

VDT: You were asked by Andy Lunde [in the abortive 1973 interview] if you remembered some issues of note in PAA and you told a bit about the problems with black members in the early days. You said there had been a black member back in the 1930s.

SHRYOCK: That was A. Reid, he was a professor of sociology at Haverford College. We had one other black member, Lionel Florant, who was just getting started on his professional career and he came to the meeting in Atlantic City in 1942 and they made an issue of his staying in the headquarters hotel. The Board of Directors, or whoever was handling the local arrangements, headed I guess by Phil Hauser, handed an ultimatum to the hotel management: unless they let Florant stay there, they would pull out and go to another hotel. We won on that issue.

VDT: Was it still a problem when you had the meeting in 1954 in Charlottesville?

SHRYOCK: I don't remember it's being a problem at all. [But see, e.g., interview with Hugh Carter, above.]

VDT: Even though that was well before the 1960s?

SHRYOCK: Have you looked at that photo I gave you of participants at the 1954 meeting? I think there were some blacks in that picture.

VDT: I think there were too. Of course, we have very few blacks in the field even today.

On a personal note. You are the greatest traveler and hiker. You have done marvelous things with the walking clubs around Washington. How do you come by your love of the outdoors?

SHRYOCK: I've always been a great walker. The first time I climbed Old Rag in the Blue Ridge, in 1935, I was taken there by John Durand and his brother Bob. Then my wife and I started going there on our own, on holidays. Then I moved to Prince George's County near the Census Bureau in Suitland, in 1946, and one of my neighbors in the same block, whom I'd met earlier because he was a member of the Population Association, was W. Rulon Williamson, who was the first actuary in the Social Security Board; he'd come from the Travellers Life Insurance in Hartford. He had climbed the Matterhorn and had been a longtime member of the Appalachian Mountain Club in New England and the Potomac-Appalachian Trail Club here in Washington. He was my neighbor and acquaintance and finally I learned that in order to go out on their excursions one didn't have to qualify by walking the length of the Appalachian Trail from Maine to Georgia. So I began going out regularly with him.

Then as they cut back a bit on their weekend bus trips, I got involved with two of the other local hiking groups, the Wanderbirds and Capital Hiking Club. I've led hikes for these over the years. When I was in Bombay, we started off before dawn one time and climbed a hill that I gather you can't climb any more because it overlooks an atomic energy installation. And, as I said, when I had the chance in Korea I did a great deal of hiking.

VDT: And that's what you plan to do more of, now that you have more leisure--continue walking?

SHRYOCK: Yes. My first wife liked the outdoors and she was a very good outdoors cook. Having been active in both Campfire Girls and Girl Scouts, she had done a lot of overnight camping. My present wife, Pauline, I met in the Wanderbirds Hiking Club. She's a devoted hiker and also likes to travel, fortunately.

VDT: I don't think you would have had time for anyone that wasn't walking--they had to run to keep up with you.

I asked you earlier about the "Junior" in your name, which has always intrigued me. I've just seen the photo of your father and you explained the "Junior"--Henry S. Shryock, Jr.

SHRYOCK: The reason I never got rid of it was, I guess, mainly inertia. The first Henry Soladay Shryock was my great-grandfather, who made some of this furniture you see here.

VDT: In Baltimore?

SHRYOCK: Yes. He was from the Shenandoah Valley in Virginia, raised on a farm there, overlooking areas where I still hike. He left the farm to become a cabinetmaker, came up to Alexandria, married a girl there, moved to Baltimore. Eventually he had a furniture factory, got into banking and so on. I'm the third of my name. We skipped a generation; my grandfather was not named Henry.

VDT: And you were the fourth generation in Baltimore?

SHRYOCK: That's right.

C. HORACE HAMILTON, CLYDE V. KISER and JOSEPH J. SPENGLER

Joint interview with Harry Rosenberg at the Carolina Population Center, Chapel Hill, North Carolina, December 15, 1976.

CAREER HIGHLIGHTS (see above for Kiser)

C. Horace Hamilton was PAA President in 1960-61 (No. 24). He was born in 1901 and died in 1977. He was a rural sociologist at North Carolina State University, Raleigh, from 1931 to 1936 and again from 1940 to 1977. From 1967 to 1971, he was also Associate Director of the Carolina Population Center at the University of North Carolina in Chapel Hill. His research and publications focused especially on internal migration in the U.S. and the demography of the South.

Joseph J. Spengler, PAA President in 1956-57 (No. 20), was born in 1902 in Piqua, Ohio, and died in 1990 in Durham, North Carolina. He received all of his degrees in economics and all from Ohio State University: the A.B. in 1926, M.A. in 1929, and Ph.D. in 1930. He was Assistant and then Associate Professor of Economics at the University of Arizona in 1930-32 and 1933-34, interspersed by a year (1932-33) at Duke University, Durham, North Carolina. He returned to Duke in 1934 and remained there until his retirement, as Professor of Economics and eventually Director of Graduate Studies. His many publications in population economics dealt with such themes as the history of population and economic theories, migration policy, changing rates of fertility, and the concept of optimum population.

ROSENBERG: Horace, I would like to start with you and ask you what your first association was with the PAA.

HAMILTON: I first joined the PdoubleA when I was at North Carolina State University. I was doing research mainly with the agricultural experiment station, the agricultural economics department and rural sociology. Among other things, I was studying some of the state's social problems and needs connected with the Depression. But I had this interest in population. Publications from the 1930 census were just coming out--in 1932, 33, and I guess even 1934. The first word I remember getting about the Population Association was a letter from Frank Lorimer, who had written a book with Fred Osborn on the Dynamics of Population [1934]. He was secretary of the Association [1934-39] and asked me for 75 copies of a little book I had put out for the experiment station at North Carolina State on rural-urban migration in North Carolina to be distributed to PAA members. I called up the experiment station and asked them to ship them off. I was tickled to death that somebody was going to read the damn thing.

I see from the member list that Joe Spengler has that my name was on there in 1935 and I guess that's when I joined. They had a meeting in 1935 in Washington and I probably attended that one.

Among my earliest memories, of course, are some of the people who are here, especially Clyde Kiser. He was our old buddy in the department of sociology here [University of North Carolina at Chapel Hill], back in 1926 and 1927, working on juvenile delinquency in [Garrett?] County, North Carolina.

I was joining other organizations about this time. My first, of course, was the American Sociological Society; I joined that about 1927. There were some population papers being published there and the rural sociology section. Later in the 1930s, 1936, we began publication of the rural

sociology journal; we called it Rural Sociology. About the same time the American Sociological Society--they called it Society at the time--dropped the American Journal of Sociology and published their own journal, American Sociological Review. So there were a lot of things happening about that time. I became conscious very quickly that if I was ever going to get any work done I'd have to quit joining organizations--or quit going to all of them. We also had an organization called the Southern Agricultural Workers Group, not farm laborers but professionals working in colleges and universities. We had meetings of that in the South, usually in Atlanta or Birmingham or New Orleans.

The big thing that impressed me about the Population Association is the same thing that impressed me about other organizations that I had joined during the early 1930s and that was the opportunity to meet with people that I knew only by name and at a distance. I got most of my satisfactions out of the personal and professional contacts. I see the first president was Henry Pratt Fairchild. I remember an article he wrote in Harper's Magazine on population, the 30 or 40 percent increase in births in one year, 1920 or 1921, after World War I. I remember Sorokin of Harvard talking about that. He got the idea that we were going to have overpopulation because of this increase.

Henry Pratt Fairchild was president from 1931 to 1935. It must have been an informal organization in those early years, possibly centering pretty much around Fairchild. Following Fairchild, here is Dublin (1935-36), Warren Thompson (1936-38), Lotka (1938-39), Truesdell (1939-40), T.J. Woofert (1940-41), Whelpton (1941-42). I met all of those people. I knew of their work before the Association started and I utilized their methods, particularly this method of computing a stable population, true rate of natural increase.

SPENGLER: The record probably would bear out that Fairchild was the prime mover who put this thing over. I mean he was the type. I remember when we were together at the first big meeting on birth control, organized by Margaret Sanger in 1934, with Amelia Earhart and Katherine Hepburn and so on, and Fairchild was the real pusher. I don't mean this is the pejorative sense; good organizer. I don't know if we would have had an organization if it hadn't been for Fairchild, who had done work those many years on immigration, this, that and the other. What always struck me was that his brother was a very conservative economist at Yale, wrote on income distribution. He had this book called [Furnace Fairchild?], all his students used to alliterate this thing. I think you really more or less felt this way--the importance of Fairchild as the organizer, pusher. Osborn may have gotten, put up more money and so on, since he was interested.

HAMILTON: I never did know Frederick Osborn. Evidently he was one of the great men in the early history of the Population Association. The man I knew first about in this field was Warren S. Thompson. He had written his doctoral dissertation on the population of China, I guess. I had first met and heard him talk about China.

SPENGLER: I think Warren ran into Scripps while he was making a trip to China and that's how Scripps got interested in this thing and put up the Scripps Foundation [for Research in Population Problems].

KISER: Notestein in one of his articles says that Scripps was interested in world population. He went up to Columbia University and was going through the card catalogue and ran across a reference to Thompson's book [dissertation on Malthus] and got impressed with Thompson and induced him to go with him on a trip to the Far East, in his yacht. They were gone for about a year and when they came back they had the plans for the Scripps Foundation.

HAMILTON: I think the Scripps Foundation played a big role coordinate with the Population

Association of America. There were Thompson and [P.K.] Whelpton at Scripps, Clyde Kiser at the Milbank Memorial Fund. Whelpton was an agricultural economist and taught down at Texas A & M College, where I had gone to school. I didn't meet him while I was there.

There were interlocking relationships. In the early 1940s, I was offered a job by the commissioner on hospital care of the American Hospital Association. One of the first people they checked me out with was Warren Thompson--wanted to know if Hamilton would do any good as a demographer on their staff. Warren evidently gave me a good recommendation, so I got the job. It moved my salary from about \$4500 up to \$8000 just within one year. Is Warren living?

KISER: No, he died about nine years ago. He'd been out of his mind for a couple of years.

HAMILTON: T.J. Woofter is another man I would meet at all the meetings and enjoyed contacts with him. In fact, Woofter asked me to write a paper, do a study on rural-urban migration in the Tennessee Valley. So we did that. Conrad Taeuber [PAA president 1948-49] sort of alarmed me. Back under the old FERA [Federal Emergency Relief Administration], he wrote a series of bulletins on the rural population and collectivization. [Something about Ohio State.]

SPENGLER: I played volleyball at Ohio State with [Charles] Lively. He was an exceptionally good volleyballer. If you got first choice, you got him. The ag economists went to Ohio State. I thought the ag economists were much more realistic than the rest of them.

KISER: I went to the population conference in Paris in 1937 [IUSSP] and Lively was there and he told me that Thompson had offered him the job in Scripps before he offered it to Whelpton.

ROSENBERG: Joe, can you tell about your first association with the PAA?

SPENGLER: I was interested in this thing [population] very early, because when I lived in the country we had a very good library in my hometown. So I'd take the stuff home, particularly if it looked like the weather was going to be bad, because I wouldn't have to go to school, for which I was always grateful. I got hold of Malthus's Essay one time and you know he has a couple of formulas in there, forecasting population. I told my father that and he said that was a lot of malarky; nobody could forecast the population. I guess he was righter than I was.

So I had this interest from then on, which was back when I was in--I must have been about freshman high school. I went to Ohio State and I wasn't aware yet of [A.B.] Wolfe's things, but I had a very good professor of ancient history and he liked me and took me to lunch and was telling me about Wolfe. So then I got in with A.B. Wolfe immediately and I think he was one of the charter members of the PAA. So my interest in this was very early. But I cannot remember the first meeting I went to. See, I was at Duke in 1932-33 and then I went back to Arizona in 1933-34. I got one trip from Arizona to Cleveland in 30-31. Times were hard and people were being paid in [warrants?] the year I was at Duke.

I clearly remember going to a meeting when we were all invited to the White House. This must have been the Washington meeting in 1935. What I remember then was there was a man named Frank--L.K. Frank--in the chair. There was some argument--I never felt that social sciences should be pushing anything or propagandizing, though I did think that all issues are subject to examination, and I felt a certain hostility to concern with birth control matters. Of course, Norman Himes was an exception to that. I think that upset Frank a little. I had the feeling that we were enraging everyone.

A lot of my work was with Kuczynski, because I was at the Brookings Institution in 1926-27--the Institute of Economics--and Kuczynski was there working on his two books on Europe. I was

studying the movement of fertility in New England, mainly, because that's where the data were best. So that's what I wrote on, and migration and so on. Then I went back to Ohio State.

I think Wolfe and Brookings and all got me involved in the PAA. So as I had an opportunity to go to the meeting--maybe the Washington one was the first, or the previous one. So I got in around the beginning.

KISER: The first annual conference [organizing meeting; May 7, 1931] was in New York and the following three meetings, 1932, 1933, 1934--at the Town Hall Club. Fairchild was a sort of resident manager of the Town Hall Club; his office was there. In 1935 we came to Washington.

ROSENBERG: I'm interested in this reception that Eleanor Roosevelt gave. What was the occasion?

SPENGLER: It wasn't much of a reception. We just marched down, arrived, Fairchild stood next to Mrs. Roosevelt and introduced each of us and we shook hands. I thought the White House very stuffy. At the Brookings Institution we'd look out and see the White House and I always referred to it as "your inferiority complex."

KISER: I remember that while we were in session, Eleanor Roosevelt came over and made some remarks to the group. She sat right behind me. Fred Osborn brought her. I wanted a good look at her, so I just turned around and looked and she gave me this stern look. She sat there and knitted while Carter Goodrich was giving his paper on migration and economic opportunity.

SPENGLER: Rupert Vance was there. She was especially solicitous with Rupert; he was crippled by polio.

KISER: At this White House reception, a young guard saw us all starting up the stairs and saw Vance come along on his crutches, so he crooked his finger and took him into a little elevator.

SPENGLER: One thing that struck me early in the game was how many people were interested in population. I was just looking at something I'd forgotten about--a chap named Punke, he was down at Georgia State Women's College in Augusta. I don't remember him from Adam, but the thing that struck me was he had an interest in this. We had some correspondence. Like Horace, I had the feeling that there were a large number of people interested and you had an opportunity to get acquainted.

I bought Lotka's book [Elements of Physical Biology, 1924?; Analyse demographique, 1939?, or other?]. I thought that was the greatest book I read in the first 50 years of my life. What it showed me was the rich variety of matters that [centered on population]. This was what I got out of Wolfe's course on population; you had to study geography and this, that, and the other. As an economist, when people asked me what I was doing and I said I'm working on population. "Well, what's that?" I just let the fools suffer in silence and never paid attention. My wife said I was really rude. The thing that caught my eye and what led me to introduce a course on it just as soon as I could was that you had a tremendous range of materials; all kinds of things emerged here. As far as an economist was concerned there was nothing else.

I first took the course with Wolfe. Then when he went away once, I took it over for him, I think for a term. I went to Arizona and introduced it there even in my second semester or second year, when I had some freedom to put something in. And when I came to Duke, I immediately introduced it there. I was supposedly a labor economist. What I got from Wolfe was a wide range. Check the journals; he wrote a tremendous lot on this. I've been tempted to write a lengthy paper on his contribution. But that's how it came.

Then things just kept enlarging and you had more and more angles. And down here in the South--as long as we were poor in the South and it's hell to be considered other than poor--we were the scheduled classes, so we got a certain amount of money from the SSRC [Social Science Research Council] without being questioned.

ROSENBERG: What's that you have there?

SPENGLER: That's the proceedings of one of our meetings, "The Third Annual Southern Social Scientists Research Conference in New Orleans, 1937." We had a regional meeting every spring; Raymond Thomas was the chairman. Rupert Vance went every year and we would get a car--four or five would go. We got the money. There was nothing to splurge on, of course. This was about the South--a major theme was always the population problem in one character or another, much of it on agriculture and migration and so on. Simon Kuznets came down one time.

ROSENBERG: Was it always focused on population?

SPENGLER: No, that was a component. But you had a number of people in the South who worked in this and then you got tied in with the population. In addition--as I remarked to Horace--Rural Sociology came into being and my own feeling was that up to the time of World War II that was the best sociological journal in the United States if you were interested in demography.

So there was a general kind of syndrome of concerns. And this fed the Population Association. I think we brought a considerable impact from the South into it, because these urban characters from up in New England didn't understand anything about agriculture anyway. We were able, I think, to give a certain orientation it might not otherwise have had.

ROSENBERG: Do you think you can tell that when you look through the officers--the impact of Southerners on the Association?

SPENGLER: Well, you've had Rupert Vance [PAA president 1951-52], who's a distinguished demographer from the very start, continuing somewhat, I think, Odum's concerns with regionalism.

KISER: T.J. Woofter, Jr. [president 1940-41], too. Odum's Southern Regions was published in 1936.

SPENGLER: Yes, Woofter was particularly interested in what you call the labor force replacement ratio or something. Several people here in the sociology department. These things all touched upon the population excess here in the South. We had a particular orientation when we were working locally and we also had a good empirical orientation, because there was so much to go on.

ROSENBERG: Was a major concern at that time with labor excess in general, let's say, during the 1930s--as a population issue?

SPENGLER: Well, the high rate of fertility among the least privileged agricultural people, particularly the blacks. Whether it was white or black, you had a very heavy fertility and they therefore didn't have the opportunity to elevate their kids upward.

You had the shortage of education and the South spent their small budgets relatively more on education than a good many places, but you couldn't get any federal help on education to speak of, as I recall.

You had mobile human capital and you were here and it's all growing up in the South. That

was beginning to move North, because we'd had the previous experience of the heavy movement during World War I. I think that comes out with Kuznets and Dorothy Thomas and those studies. So that was mobile human capital. We didn't turn it into technical terms. The economists talked about the "learning effect." Well, every damn dog is subject to a learning effect. So we didn't fancy it up; we just looked at it in the country. So there was a real sense about what was going on. We had Social Forces too and then Rural Sociology and these fugitive publications. Of course, Horace knows a lot more about these than I do. But that's what made an impression on me.

You had another thing that could have been tied in; you had the National Resources Planning Board set up in Washington, about 1932 or 33. You had two books on trends in the U.S. First there was one during Hoover's time, in the 1920s [William Ogburn?--headed the staff of Hoover's Research Committee on Social Trends], and then there was one in which Whelpton and Thompson had their first projections, that was published by McGraw Hill [Population Trends in the United States, 1933]. And the sequel to that, the National Resources Planning Board report [Problems of a Changing Population, 1938]. There were all kinds of inquiries. A good deal of this stuff grew out of Southern demographers, I think. Some of the work that had been done sort of fed into that, stirred it up.

I was always interested--as I think Warren Thompson was--in city size. My point was that a big city ought to be burned out, and that's now been confirmed. I had an argument with the New York Times editor on that. I did a piece on city size and migration. It never got published because we went to war just as I was finishing up. This is by way of saying that the interest ranged fairly widely in the National Resources Planning Board.

I think in the history of empirical social science, say from 1929 on, you'd find a whole network of things that fed into each other, to which in a way the demographers contributed a great deal. Not so much in technological terms, although they did very good empirical statistics, I thought, but in the sense of pointing to empirical issues they felt affected man's material welfare. I've been trying to interest one or two graduate students over at our place [Duke] to do something like this.

ROSENBERG: That would be somewhat an intellectual history?

SPENGLER: Yes, it would. But I think it would also throw light on the strengths and weaknesses of how you organize.

This is a little aside from the Population Association, but my point is that we had a whole network of things, because we had a network of interrelated problems. I know that Horace had a lot that he was working with on this. And you, Clyde, up there where you were [Milbank Memorial Fund in New York], you were working at you might say the other side, except for the measurements.

INTERRUPTION. Talking about Rupert Vance as tape resumes.

SPENGLER: One thing that always struck me about Rupert, you never could do anything whatever to help him. We were at VPI and he went up three flights of stairs to do a radio program. The only time Rupert would accept any assistance from me was when we were in Philadelphia, maybe when I was president [1957 PAA meeting, when Spengler was president, was in Philadelphia]. It was windy as hell; just bitter on that elevation. And I sheltered him because his circulation couldn't contend with that. That's the only time it was ever possible for me to extend any assistance.

Rupert introduced me when I made my presidential address ["Aesthetics of Population," published as a Population Bulletin of the Population Reference Bureau, June 1957].

HAMILTON: He gazed out over the audience and said, "I know this audience is a typical demographic table--all broken down by age and sex." That's funny. I can't remember your speech but

I remember that. I never was much of a joke-teller but one of the great joys I always had was at these meetings and hearing Vance tell some off-color jokes.

SPENGLER: Two people that always beat anybody else. One was T. Lynn Smith. Clyde Kiser would always have three or four up his sleeve; he'd try them out on me first. T. Lynn always used to have several on blacks.

HAMILTON: Woofter was awfully good too. I thought a lot of T.J. He was very good on methodology--undercounting of blacks in the 1940 or 1930 population census. He pinned down that they'd undercounted 150,000. He was very much interested in that.

When I talked to Woofter about one problem I had, he suggested using another method and he was right, but I wanted an argument at that time. I later recognized that he knew what he was talking about.

On the Southern regions, T.J. Woofter had a special methodology for delineating regions, special kinds of regions, sort of out of character with Odum's generalized approach. But I wonder if it wasn't good to take off from some of Woofter's work, because he adapted Hotelling's method of factor analysis of central components, first components.

KISER: If I might make a serious remark, someone once defined the South as that part of the country in which if you wanted something good to eat you had to go to a Jewish delicatessen.

ROSENBERG: Clyde, could I ask you how somebody from Gaston County developed an interest in population? Do you come from a big family, for example?

KISER: No, there were five of us--three boys and two girls. And my own household is four.

Well, I came to the University of North Carolina as a freshman in 1921. I dropped out and taught in high school a year. Then I heard about Odum's work here, through Jennings Rhyne. Jennings told me he would support me in an application for a fellowship. I didn't get a fellowship, but I got a scholarship that first year when I came back to work on a master's degree. Then after I took the master's degree, I planned to go on but I got hooked up with the study of St. Helena Island, a project Woofter was running there.

SPENGLER: What was your master's?

KISER: My master's thesis was on liquor law violations in Durham and Person counties.

Frank Ross--he used to be editor of the Journal of the American Statistical Association--he and Jack Woofter were doctoral students together at Columbia and they were good friends. So they hatched up the idea of this study of St. Helena Island. I got hooked in with that. We ran into the fact down there that a lot of these Negroes had moved north, although this was not a place where Negroes were downtrodden so much. They were poor but they owned their own little plots of land. And they've had very little in the way of racial tension because there are very few whites there. But still they were moving. So they hatched up the idea that it might be well if I transferred to Columbia, if they wanted me there, and collect some data on Harlem Negroes and then come back to North Carolina for my degree.

Well, I got up there and I liked the group up there pretty well. I became good friends with Frank Ross. As a matter of fact, I gave blood to his infant daughter--I was her blood type--my first year up there; so I happened to fit in very well with him and his family. So I stayed up there for the degree. I was already initiated into demography more or less--Negro migration--and I took Chaddock's courses in vital statistics and population.

Then in the summer of 1931, Chaddock showed me a letter he had from Frank Notestein. Frank Notestein said they had a lot of 1900 and 1910 census data on children ever born that needed analyzing and they had a fellowship for a year, for which I applied. So I went to the Milbank Memorial Fund in 1931. And I learned pretty soon there about the PAA. My immediate supervisor was Frank Notestein and Sydenstricker was above him. So I learned that a young organization, the PAA, was just beginning. They had had their organizational meeting on May 7th, 1931.

I might say a word or two about the circumstances of that organization. In my PAA presidential address, "The Population Association Comes of Age" [1953, published in Eugenical News, December 1953], I mentioned predisposing causes and immediate factors responsible for the Association. Among the predisposing factors was the increasing interest in demography during the 1920s. The Scripps Foundation started in 1922. Pearl and Reed had developed their logistic curve and Pearl had written Biology of Population Growth and at Scripps, Thompson and Whelpton were starting work on their projections. At the Metropolitan [Life Insurance Company], Dublin and Lotka had come out with "On the true rate of natural increase" [1925]. And in 1928, the Milbank division of research was started to do research on population.

The immediate factor I saw was the stimulus given by the formation of the International Union [for the Scientific Investigation of Population Problems/IUSSP]. The International Union was formed in 1928 and that grew out of a World Population Conference held in Geneva in 1927. Margaret Sanger organized that. Margaret Sanger did more for getting population started than we give her credit for. She approached an anonymous source for money for that 1927 conference and she got it. She approached the Milbank Fund for money for the International Union and she got it. She approached the Fund for money for this first little organizational meeting of the PAA and she got that. Now the amounts were relatively small. For the PAA it was just \$600 to pay Thompson and Whelpton's fare and a few people coming up from Washington, things of that sort. But she got the money on behalf of Fairchild. Fairchild did the legwork in getting the organization started. The Fund supported the International Union the first three years of its existence almost in full. It gave, I believe, \$10,000 a year, or \$30,000 for the three years 1928-31, and the Rockefeller Foundation chipped in with additional support.

The PAA had its organizational meeting May 7th, 1931. In that article I mentioned the first meeting of the American National Committee. The International Union was organized not on the basis of individual members but of national committees, so we had to have a national committee. The first meeting of that American National Committee was held February 4th, 1931. Lotka was the secretary. He gave me the minutes of that meeting and I quoted from that in that article. The first paragraph read something like this: "Louis I. Dublin opened the meeting by stating that he had been asked by the president of the International Union for the Scientific Investigation of Population Problems to become chairman of the American National Committee, in accordance with the organization of its executive committee. Dr. Dublin had accepted and in conjunction with Professor Fairchild and a group that had met at the latter's invitation, he had invited a small group to constitute the American National Committee. Those attending were: Louis I. Dublin, elected chairman; Alfred J. Lotka, elected secretary; and then H.P. Fairchild, C.E. McGuire, Lowell J. Reed, Clarence C. Little, and P.K. Whelpton."

Now last night I was digging through some old files and one of the things I read was a letter from Lotka to Edgar Sydenstricker, March 3, 1931, just about two weeks after that February meeting of the American National Committee. He wrote:

"Dear Mr. Sydenstricker,

In accordance with a motion carried at a meeting of the American National Committee, held in New York on February 4th, 1931, the chairman has prepared a draft of statutes, of which a copy is enclosed. The several members of the committee are hereby requested to communicate to the

chairman any comment or suggestion that they may have to make regarding this draft in order that he may be able to send the statutes in final form to Dr. Carr-Saunders for publication."

Raymond Pearl was the first president of the International Union. Lotka gave the list of the original members of that American National Committee: Louis I. Dublin, C.E. McGuire, vice-chairman, and Alfred J. Lotka, secretary-treasurer. And members: O.E. Baker, Department of Agriculture; H.P. McGuire; James W. Glover, the life table man; George W. Kosnak, editor of the American Journal of Obstetrics and Gynecology; Dr. Clarence C. Little of Harvard Club--he was a biostatistician and he once was the president of the University of Michigan--and Dr. Raymond Pearl and Lowell J. Reed, both from Johns Hopkins, and Mr. Edgar Sydenstricker of the Milbank Fund, Warren Thompson and P.K. Whelpton.

Now, I might just say a bit more about that first meeting of the organization [May 7, 1931]. According to Fairchild, there were about 38 there. Notestein in one of his articles recently spoke of the overlapping membership of the American National Committee and the Population Association. Fairchild was the first president of the Population Association; Dublin was the first chairman of the American National Committee. I wonder if there was some jockeying between those two for a position as the arm of the International Union. But the American National Committee was the first on the scene. It stimulated the formation, I think, of the Population Association.

SPENGLER: Did you know anything about C.E. McGuire. He was at Brookings; a Catholic. I had lots of arguments with him when I was at Brookings. He was a brilliant man, very sharp and all, but a profound Catholic, and, of course, I had run-ins with him periodically on this. He was the correspondent to the London Economist too. I didn't know how he happened to get in [the PAA], because he had an ideological concern, I thought, rather more than a substantive concern.

KISER: Frank Lorimer said that Margaret Sanger wasn't concerned too much by science but she stressed the importance of having the backing of science in her movement and she didn't get very far with the International Union. She set up the Union, but it was not going to be an activist organization. She couldn't get very far with Dublin. Dublin opposed birth control on moral grounds. So probably for that reason, I thought there might have been some jockeying for position. Or maybe Margaret Sanger thought she'd take a second chance on the PAA and on Fairchild, because she hadn't been able to get very far with the International Union. But she didn't get very far there either. Notestein says here--he spoke of the fact that she had managed to get some money from the Fund to help set up the PAA. And he said--this is from his article in my book ["Reminiscences: The Role of Foundations, the Population Association of America, Princeton University and the United Nations in Fostering American Interest in Population Problems," in Clyde V. Kiser, ed., Forty Years of Research in Human Fertility, Milbank Memorial Fund, 1971, pp. 67-84]:

"It was expected that she, Mrs. Sanger, would be elected first vice-president. Largely because of Frederick Osborn's influence, her name was withdrawn. Osborn, a great admirer of Mrs. Sanger, persuaded the meeting, and I think Mrs. Sanger, that the fortunes of the field would be advanced if the new Association were to guard its scientific character and keep free from attachment to the birth control movement." [page 70]

Now, from the very beginning, they adopted a scheme to guard the scientific character. They formed what they called a College of Fellows. These were the purest of the pure. Notestein describes it:

"We went to organizational lengths beyond all lengths to keep out all but the purest of the academically pure. I still remember when about a dozen of us would meet in Dublin's office at the Metropolitan as members of the American National Committee of the International Union for the Scientific Study of Population Problems and draw up a memorandum to the new Population Association of America. We would then adjourn our meeting and quickly travel to the Town Hall Club, where the same group would assemble as the College of Fellows of the new Population Association of which we were the creme de la creme. As such we received the memorandum from the American National Committee, pondered its merit, and passed on the results of our superior wisdom together with notice of action taken to the body of the Association. The College then hastily adjourned to reconstitute itself as the Association and receive with gratitude the result of the College's mature wisdom. It really took us an incredible time to realize that the birth controllers and other action groups were probably less eager to capture the academics than the academics were to avoid capture." [pp. 70-71]

SPENGLER: In recent years, there's been a great resurgence of interest in fertility and family planning, which is really birth control, and all of these new people that have come in on family planning have now joined the Population Association and almost swamped it. That's gone full cycle. But I think the group interested in the scientific approach is so large now and dominant with the journals--Population Studies in England, Population in France, and our Demography and Population Index. By the way, do you know anything about this new World Population Society?

KISER: They're people in Washington. It's sort of an alarmist group, I guess. I couldn't make much out of it.

SPENGLER: I couldn't either; I didn't join it. Funny thing, I belong to the International Union and their dues now are up to \$40. They haven't followed all my other societies, like American Sociological Association, Population Association, Southern and so on, which have reduced the rates for elderly people down to practically nothing. The members of the Union get sort of a double rake. Your dues to the International Union are supposed to include subscription to Population Index along with Population Studies and Population. But we American members are paying for Population Index twice, that is, we get it along with Demography as PAA members.

KISER: I heard that American members of the Union would get a reduced rate because of that. [IUSSP members get a reduced rate in PAA because their subscription to Population Index is covered by IUSSP dues.]

KISER: I joined the Population Association before that first annual meeting [1932] but I didn't get into the College of Fellows. I was just a young squirt. John Innes and I attended that first annual meeting together in the spring of 1932 at the Town Hall Club. I think we were about the only two there--besides the birth controllers, Mrs. Sanger maybe--who were not members of the College of Fellows. The College of Fellows was just a sifting device. It was made up of the very first charter members, the professors and the bigshots. They were to guard the scientific character and keep the birth controllers out.

HAMILTON: The fact that Frank Lorimer wrote me for the 75 copies of that bulletin--it was published in 1934 and he wrote me right after it came off the press--there must have been a relatively small number of members in 1934. [PAA records show "around 100" members in late 1934-early 1935.]

SPENGLER: What about Walter Willcox?

KISER: Willcox was never president of the Association. He attended the early meetings all right. He even attended that [IUSSP] conference in Paris in 1937. I remember he attended a meeting at the Shoreham Hotel in Washington. He must have been well over 80 then, but he walked all over Washington. I went to his 100th anniversary at the Cosmos Club.

SPENGLER: He was over 100 when he died [103]. Got into Ripley [Believe It or Not] for that. I remember going to the Columbia Club for dinner around 1950 and he was sitting there eating with two or three people. I had corresponded with him on something and I went over and shook hands. He seemed quite spry; it was amazing, he was 100 years then.

KISER: After he got to about 100, he offered to be examined periodically by any medical group that might be interested and he did go several times to be examined. Notestein said he enjoyed being thumped by the doctors who examined him.

ROSENBERG: What was the secret of his long life?

SPENGLER: Good parents. I think a combination of genetics and culture factors. Individualistic factors too. I think nutrition has a hell of a lot to do with it and he probably got started on that. Then I think that genetically, some people do not generate excessive cholesterol or something like that--other factors that kill people off.

ROSENBERG: You say that Margaret Sanger was instrumental in generating a lot of resources and enthusiasm but that she somehow was kept out of the mainstream. Did she accept that role?

KISER: I think so. She must have seen what was happening. After all those things happened, she was quite willing to cooperate with a study of her birth control patients--Regine Stix and Notestein followed up 5,000 patients of the Margaret Sanger Clinic. She would have conferences with Notestein and Stix and take issue with them when she thought they were not showing birth control up in as good a light as she thought it should be, but she did things pretty good.

SPENGLER: Do you remember the journal called The Birth Control Review? I wrote things for that. Got invited to the first conference. You had old Charlotte Perkin--all these suffragettes--most entertaining conference I ever attended in my life. There was some pretty good stuff in that review and a lot of nonsense too. But it was in some degree a medium at a time when there was no other journal rather closely oriented [to population].

HAMILTON: I remember the year before I got married, I decided I ought to know as much as I could about birth control techniques. I wrote a personal letter to Margaret Sanger and asked her to send me any information she might have. She sent me an article and wrote me a letter and gave me the brand and name and everything which I could buy. I thought it was very instructive and to the point. As a matter of fact, I sent it to my prospective bride before we married. I wanted her to be sure of what was going to happen.

SPENGLER: She married a wealthy man, Mr. Clee, who lived in Arizona. She moved to Arizona not long after I came East. She was closely associated with the university people; very highly respected in

Tucson. She was very comfortable in her older years.

I remember one time I made a speech to women on how much wealth they owned and how much more they would own and how it got concentrated when they had smaller families. The Catholics hopped on me--"this stinking, vicious. . ." The president of the University of Arizona, he was a man of parts, he never paid any attention to people who hopped on his faculty--obviously it was a scholarly argument that I presented. I was just describing how family size affected how much wealth they had. There was quite a bit of writing in those days on wealth--this was about 1933.

You remember General Francis Walker? Well, a student of mine was writing on the significance of legislation by our Congress having to do with Indians. He had grown up on a reservation himself. He told me--and I hope I'm remembering this correctly--when Walker was made superintendent of the 1870 census, he didn't have an office. So they made him head of the Bureau of Indian Affairs and that way he got an office. He had a young yellow-haired military s.o.b. named Custer and he told Custer, "Well, if you want to make a name for yourself. . . . [go after Indians?]" This was an easy way to do it.

KISER: Did you know that Willcox was the Census Bureau's chief statistician for population about 1912?

SPENGLER: He didn't have much in the way of high-powered techniques; he had simple methods. But he made a big impact. I got my vital statistics from E.B. Wilson, Harvard.

KISER: I wrote a chapter for Frank Lorimer in Problems of a Changing Population with a part in it on factors underlying group differences in fertility. I gave the biological factors and then went on to show that biological factors are not so important and most differences were due to differences in contraception. Without asking me, someone cut out the contraception part and it just ended on biological factors, attributing all these fertility differences to biological differences. Who did this? Old E.B. Wilson. [This incident is described more fully in the Notestein interview, above.]

SPENGLER: He was a mean bastard. He and Pearl had some way of ascertaining whether the other planned to go to a certain meeting and if he was to be there, the other wouldn't go. You know why? Old Pearl received an appointment at Harvard once and, according to the story, E.B. Wilson blackballed him. Blackballed him so that for a day or two Pearl was out on a limb. He'd already resigned from Johns Hopkins and then his appointment at Harvard was canceled because of E.B. Wilson's objection. But they took him back at Hopkins.

HAMILTON: Raymond Pearl [roused] a good deal of controversy on the logistics curve and that method of projections.

SPENGLER: Wolfe wrote a critique of it. Whelpton, in his first set of projections, his components method, he criticized the logistics curve.

HAMILTON: I lucked out pretty well on trying to make projections--a short method of projections of population from one decennial census to another for small groups. Came out in Social Forces in 1962. I wrote more papers and did more work with a practical interest after I retired as head of department and also when I was away from the department, like I did in Chicago with the American Hospital Association.

SPENGLER: I think Ed Hutchinson married E.B. Wilson's secretary. What did Wilson say to her?--

"Keep your mouth shut." He had a first-rate mind and he was a son of a bitch.

HAMILTON: Getting back to what PAA was like in the early days of my association with it and some of the central characters, I think two people that contributed, man and wife, as much as anybody else to PAA were Conrad Taeuber and Irene Taeuber. Irene, you know, edited Population Index for years.

I remember we had a meeting in Charlottesville [1954] and there was going to be a Negro invited, George Roberts, Jamaican, and Conrad Taeuber made arrangements to have him put up at the [Jefferson] Hotel. When he came to the hotel, he just assumed that arrangements had been made, but they wouldn't register him. Conrad Taeuber went through the ceiling on that. I think he got it worked out, but it was a nasty situation.

It's too bad Irene had a premature death. She had an offer to leave Princeton and go to Georgetown as professor at a very good salary. Princeton found out so they upped her salary and kept her. But she didn't live there; she lived in Washington.

ROSENBERG: Horace, you said you had some thought on your older recollections of PAA.

HAMILTON: I was a generalist on nearly everything--rural sociology, agriculture--and for that reason, I decided at one time that I was going to quit fooling around with population. I was going to stick to rural sociological surveys. I remember telling Dan Price or Rupert Vance that I was going to let the university over here do the armchair research and I was going to do my surveys. Well, I couldn't stick with that because the guy after Truesdell in the population division in the Census Bureau advised me to read a paper at one of the PAA meetings on net migration, techniques, and so on. I did it reluctantly. But gradually--you know, a man writes a paper or series of papers on a certain area and he begins to get tabbed--"Well, Hamilton is the guy to do that paper or say something about that."

ROSENBERG: Joe, what are your recollections of some of the early issues the Association was concerned with?

SPENGLER: I can't remember the issues too well. The thing that sticks best in my mind--something that sticks best in my memory respecting other organizations with which I was associated in the early salad days--was you had a sufficiently small group so we could hear most of the things that were presented. Furthermore, we knew each other, sort of speaking acquaintances, and there were people there with whom we had deeper common interests and it was easy to make contact and carry them on. You didn't have to plan ahead, because you played these things by ear. So what I liked was the smallness of the organization, rather than the conflict. The opportunity I had to get little views on things or lines on things that I could possibly make use of in my own research and teaching. Not so deep, but something I could jot down and stick in my head and go home and it would fester in my brain, even help me to teach or scholar or both.

Now you get these damn big markets. You can't have that anymore. There it was easy since you were all members of the same bark. You could go say, "I'd like to chat with you about this for a moment," and that was just standard protocol. That's what I remember. We got rid of most social scientists of all sorts and if we could restore that situation, I would be all in favor of it--provided I could survive the process.

HAMILTON: The universities would not pay traveling expenses to a faculty member unless they were delivering a paper. I got into trouble on that at Texas. I wanted to go to Detroit one year and deliver a paper on the social effects of the mechanization of agriculture, but they wouldn't pay my

expenses. I said, "I'm reading a paper there." They said, "We don't pay people's expenses just because they've got a paper. You have to have some other reason to go." So I paid my expenses that time, several hundred dollars, and I had to take it out of my meager salary and I didn't like that at all. I published it in I think it was Rural Sociology. Had a lot of fun writing it; Lynn Smith got me in on that. Incidentally, a lot of papers I've written have not been papers that I thought of myself, but papers other people persuaded me to write.

Later on, Everett Lee was chairman of the social science section of the American Association for the Advancement of Science, which was meeting in Cleveland in about 1961 or 62. He wanted me to write a paper at first on the Negro deserts the South. I didn't like that word "deserts" so put it "The Negro Leaves the South" [published in Demography, Vol. 1, 1964]. I got more requests for reprints on that paper, and also for it to be reprinted in other books and in other connections. At the same time, I got a request from Duke University--Edward Thompson and McKinney, I believe, were publishing books on the South. I had written this paper on the Negro and found it very easy to expand the same subject matter to whites and blacks and they thought so highly of it they put it Number 3 in the book.

I got some more mileage out of that paper. I was invited to address the alumni of North Carolina State University--they come back once a year--and the dean wanted me to make an address. I told him, "I have a paper which is going to be published and if you'll let me read it, I won't have to do any extra work." I did. The chancellor was there and all the other bigwigs and little wigs. I got a lot of mileage out of that paper, because people that I had met and known through the Population Association knew that this was something down my alley.

ROSENBERG: Clyde, could you say something about your feeling about the issues the Association was involved with and how it changed? The whole question of size is something I'm somewhat sensitive to, even over the brief period I've been associated with it. It has grown so rapidly. Last year for the first time, I went to the meeting of the Southern Regional Demographic Group, which is quite a small group. They have about a hundred people at their meetings. You get to know people over the two days on a face-to-face basis and it's much more personal. Leaves you feeling much better. People really talk to you about your presentation and you see them again at dinnertime and it's a very pleasant thing. I believe the Association size has been a real factor in changing the nature of it--at least as I hear you talking about all the people you interacted with through the Association.

KISER: Well, as I said, the original membership was around 38, according to Fairchild. I did a little spot map by state of them and most of them lived either in or around Washington or New York. The first three meetings, as I said, were held in the Town Hall Club and they were pretty small rooms. But then in 1935, the attendance did jump, because of the interest of the government workers. That topic in Washington was the relation of population to some of the New Deal programs. [Conference on "Population Studies in Relation to Social Planning," May 2-4, 1935. PAA fourth annual meeting was on May 3, 1935.]

The organization was different too. It was in some respects more loosely organized in those days.

HAMILTON: They didn't have sectional meetings; just one big program.

KISER: That's right. Everyone heard the same thing; they had one session going. It wasn't a three-ring circus. Now it's about 24 varieties.

HAMILTON: Clyde, you worked in the field of fertility--the tremendous Indianapolis Fertility Survey--we migration people got to feel maybe the fertility people were running off with things.

Dudley Kirk talked a bit about that in his presidential address ["Some Reflections on American Demography in the Nineteen Sixties," published in Population Index, October 1960].

SPENGLER: Yes, I believe he said--well, Warren Thompson used to say, "You can only be born once, but you move a hell of a lot."

KISER: And in another respect it was more tightly organized than now. The members voted on the Board of Directors and the Board of Directors appointed the officers. Furthermore, the Board of Directors was self-perpetuating. They would reappoint themselves. They didn't have this rule about not serving consecutive terms. So there were lots of complaints about a tight little group running things.

HAMILTON: I remember when that fight came to a head; I don't remember what year it was.

KISER: I'll tell you. It happened--not because of anything I did--but it happened in 1953, when I was president, that they did amend the constitution to put officer election on a membership basis. It was Con Taeuber who wrote and said he was going to propose this to the members. I rather resented it, because I had enough to do anyway getting ready to leave and then I had to read up on Robert's Rules of Order. But I think the thing worked pretty well. Phil Hauser was a bit pugnacious, so I wanted him on my side. He was the one I chose to introduce the motions one by one and then we'd vote on them. After the meeting was over, Irene Taeuber said, "You practically had that memorized" [Robert's Rules]. I sure did.

HAMILTON: Phil Hauser was president in 1950-51.

SPENGLER: He was president when they met here in Chapel Hill in 1951. We had two meetings here--1940 and 1951.

KISER: At the 1940 meeting here, Truesdell gave his presidential address on the 1940 census.

SPENGLER: Let me tell you what O.D. Duncan said about Truesdell's address. You know how exciting a speaker Truesdell is. O.D. Duncan [Senior], you remember him, says to me, "Joe, I had to listen to Buckshot [Fall-in-a-trap governor]; damn sight more interesting than this."

KISER: I remember Odum got pretty fed up too. I believe he presided that night.

HAMILTON: But Truesdell was a great man. He's still living too, isn't he?

SPENGLER: He is; he's 90 years old. He's a good man, but he's a hell of a poor speaker. He was the man who introduced me to Phil Hauser. Phil was his assistant for a while [in the Population Division of the Census Bureau].

KISER: Well, it's really a lot of scope for interesting relationships and experiences. I think the trouble with all of our social science organizations--just like the AMA almost--they have a House of Delegates and they no longer attempt to have a mass meeting of everybody.

SPENGLER: There's one thing I miss now compared with the early days and this is not so much a function of change in size, it's something else. People were very knowledgeable about the facts of life

in the early days. Now you have a bunch of young men--paralyzed monks or whatever you want to call them--who don't have much real interest in the world or any sense of it at all. So as soon as you shift away from a methodological feature you're apt to be pumping in a dry well. In the early days, people had a pretty wide range of knowledge of economics [and other things]. It might not have been scientific, but at least you had a basis. Now they don't know about any of those things and therefore you get these peculiarly circumscribed notions of [concomitants of] human behavior, despite the improvement in methodological techniques.

HAMILTON: I was thinking that too, Joe. We've had the computer and all the improvement in technology, methodology. Yet I don't believe we can predict any better now than we could 25 or 30 years ago. The facts should be in the computer. What makes a difference is not the computer; it's that they speed things up. And the stuff comes out of that thing. . .you get a stack of computer output that thick. What the hell are we going to do with it? Straight into the garbage can.

ROSENBERG: I wrote to Science magazine--there was an article by Sklar and Berkov on the trends in U.S. fertility and it said something about a resurgence in U.S. fertility. I thought there were some problems with their analysis and I wrote Science at the end of last week and they told me they would accept [my article]. Basically, I challenged what they said; it was on the basis of one year's data in California.

HAMILTON: The latest month that the monthly vital statistics came out of Washington shows a tremendous drop in births, both in the state and in the nation. The childbearing women, they've had their two kids and this depression, the housing [costs], is developing and beginning to show an effect.

ROSENBERG: But I was thinking about what you said about the use of the computer and refinement of methodological techniques. And at the same time you see a great narrowing in the capabilities of understanding and specialization. It's fragmented the Association, I think. PAA is just enormously fragmented now into small groups.

HAMILTON: There's one thing they do now in most big organizations which helps the little man trying to get a start, give a paper. They have a section called contributed papers. Anybody that can't get on the regular program, the bigtime, he can always send in a contributed paper.

SPENGLER: I hear a lot of people at the Census Bureau watch that very carefully. They use that as a basis for getting to the meeting.

HAMILTON: There's a similar thing. You know this magazine of England called Nature. Very early they adopted the principle of open submission. Anybody who had an idea, no matter what [could be published]; they didn't submit it to a bunch of referees. One of our statisticians out here now at RTI [Research Triangle Institute] worked with me on a problem and he got an idea while he was working on it and wrote it up--just a short couple of pages--and sent that to Nature; got it published. And I never did get my paper published. The damn referees in the American Statistical Association, they wanted to be snooty about it.

SPENGLER: This would make an interesting study. Nature has had a profound influence over the years on scientific thinking. If one could compare the role that Nature has played in England over, say, 50 years with some comparable journal or two or three, I would think this would demonstrate Milton Friedman's point that if you've got a lot of liberty without imposing arbitrary rules, you get more

product. I've often skimmed Nature to see if anybody's written something new in there.

ROSENBERG: Clyde, what are some of the books and materials you think might be useful in this project? For example, the history of the Milbank Fund, Forty Years of Fertility Research [Proceedings of a Conference Honoring Clyde V. Kiser, New York City, May 5-6, 1971, edited by Clyde Kiser, Milbank Memorial Fund, 1971]. Obviously it's a central kind of thing.

KISER: That's right. There are three historical articles in here. My own is, "The Work of the Milbank Memorial Fund in Population since 1928." Then Notestein's piece, "Reminiscences: The Role of Foundations, the Population Association of America, Princeton University and the United Nations in Fostering American Interest in Population Problems." And then Frank Lorimer, "The Role of the International Union for the Scientific Study of Population."

Then this other little red book of which I've given each of you a copy. It's called, The Milbank Memorial Fund: Its Leaders and Its Work [by Clyde Kiser, Milbank Memorial Fund, 1975]. There's a little history of population here but not too much. I devoted this mainly to public health work.

There is another good book, the proceedings of the World Population Conference in 1927, edited by Margaret Sanger and published in London by Arnold. Another book is the proceedings of the 1931 IUSSP conference in London; that was the first annual meeting. It's edited by G.H.L.F. Pitt-Rivers. He was an erratic guy but he was pretty bright. He was the one who really caused a ruckus in that Paris meeting in 1937. He was something of a Nazi and he wanted to kick the Czechoslovakians and several of the other Communist countries--or those he thought were on the verge of Communism--out of the Union. There was a lot of argument. The Germans and Nazis there wanted to give papers in which they talked about the master race and all of that. In planning the congress, according to Lorimer, they put all those papers in the same session and let Frederick Osborn be the chairman. At the end of the session, Osborn quoted Voltaire: "I disagree with everything you say, but I would give my life for your right to say it."

SPENGLER: Gini organized a meeting earlier and Sorokin invited me to give a paper there, which I gave on state and population, or something like that. Later on Gini had another one; I gave a paper to it too. [The IUSSP'S 1931 first conference was planned for Rome but transferred to London when it was learned that it would be used as a platform for the proclamation of Mussolini's theories. Gini, however, organized an international population conference in Rome, securing the "attendance of a considerable number of foreign scholars." Frank Lorimer in the article cited above, p. 89.] This brought together a good many demographers. One thing that struck me was the small knowledge Americans had of the degree of population study in Europe. There were a tremendous number of pretty good workers and fairly good statistics. The Hungarians had done pretty good work way back. But except for people like Wolfe who were well educated, there was small comprehension of the nature of the work that was done there. We were somewhat provincial, except for the connections that we had with the British.

DUDLEY KIRK

PAA President in 1959-60 (No. 23). Interview with Jean van der Tak at Dr. Kirk's home, Stanford, California, 1989.

CAREER HIGHLIGHTS: Dudley Kirk was born in Rochester, New York, but moved as a small child to California and grew up there. He received a B.A. in political science from Pomona College in 1934 and an M.A. in international relations from the Fletcher School of Law and Diplomacy of Tufts University in 1935. From Harvard he received an M.A. in sociology in 1938 and Ph.D. in sociology in 1946. He was with Princeton's Office of Population Research from 1939 to 1947, where he was author of the influential monograph, Europe's Population in the Interwar Years (1946), and coauthor of The Future Population of Europe and the Soviet Union (1944). He was also Assistant Professor of Sociology at Princeton in 1945-47. From 1947 to 1954, he was demographer in the Office of Intelligence Research of the U.S. State Department. He was Director of the Demographic Division of the Population Council from 1954 to 1967. He returned to California in 1967, where he has been at Stanford as Professor of Population Studies with the Food Research Institute and also with the Department of Sociology, of which he was Chairman in 1975-76. He has published widely on such issues as population trends in Europe and Latin America, migration, the demographic transition, and population growth and economic development. He has served on the U.S. Committee on Health and Vital Statistics and as a consultant to the U.S. Agency for International Development and the Ford Foundation, among other groups.

VDT: Dr. Kirk did a first interview for the PAA oral history project with Anders Lunde at the PAA meeting in Philadelphia in 1979 [April 26, 1979]. However, the tape of that interview is flawed and also he has now added ten more years to his distinguished career. So I thank you, Dudley, for agreeing to this second interview.

How did you become interested in demography? You mentioned in your 1979 interview that your demographic interest grew out of your interest in geography.

KIRK: Yes. In high school and even before, I was intrigued with numbers, had fantasies about numbers--population growth of cities and of nations, of tribes, people and so on. But that interest wasn't crystallized because in my high school, of course, there was nothing on population and no such course at Pomona College.

In college, however, I wrote a long paper, which took quite a little research, on the future of Los Angeles County. The county supervisors at that time had a big study of the future of Los Angeles County. This was fascinating, because it assumed a good deal of population growth but nothing like what has happened. There were 2 million at the time I wrote that paper in 1933 and 9 million now. The forecast was that the population growth would be accompanied by a growth in the network of public transportation, particularly what we called the "big red cars" of the Pacific Electric; people commuted from our town, about 35 miles away, into Los Angeles on these big red electric trains. They had no idea at all--nor did anybody at that time, really--that the growth of Los Angeles would be a matter of freeways and automobile transportation. There was no forecast of the freeway mentality and culture.

So I was interested in population and population problems. Then I went to the Fletcher School [of Law and Diplomacy]. At Pomona, I had been in political science and I was interested particularly in international relations. I was at Fletcher a year.

Then my father called my brother and me in and said, "I'm going to give each of you \$2,000"--

which was a lot of money at that time--"and you can do anything you want with it. You can start a candy store or continue with graduate work or take a trip or whatever." At age 21, you can imagine what I chose, and what my brother chose. We went around the world, spent a year.

We started off on a Japanese freighter across the Pacific. Spent a month in Japan and six weeks in China. Then to Malaysia on a Norwegian freighter; the captain just couldn't wait to get to Singapore, because he loved to play golf. We went to India, spent quite a little time in India, and then to London, where we spent the spring term [of 1936] at the London School of Economics. Then came back to the U.S. and I went to Harvard as a graduate student; my brother went to the University of California.

At Harvard I was a major in sociology. I took Ed Hutchinson's course in population, enjoyed it tremendously, and went on and wrote a dissertation under Hutchinson on the Nazi population policy. My conclusions were that the success of the policy in raising the birth rate--which they did, very substantially--was as much as or more the result of re-employment as of population policies. About one third of the labor force in Germany was unemployed in 1933. With rearmament, there was a tremendous revival in employment and people had children.

VDT: You got your Ph.D. in 1946 from Harvard, where you'd gotten an M.A. in 1938. Meanwhile, you'd gone to the Office of Population Research at Princeton in 1939. When was this dissertation written?

KIRK: I wrote it at Princeton, really, but through those years, I was mainly engaged in the work of the Office of Population Research.

VDT: In your 1979 interview, you said you were really hired by Irene Taeuber for the Office of Population Research, who interviewed you sitting on a loveseat in the Hay Adams Hotel in Washington when you were there attending your first PAA meeting [May 12-13, 1939].

KIRK: Yes, she was delegated to interview me. You remember Irene, how very intense she was. You could say that she hired me as much as anyone did. Of course, Frank Notestein was the director.

VDT: You said that the job paid \$2,500 a year, on which you were very comfortable.

KIRK: Yes, it really was a good salary.

VDT: At the Office of Population Research, they were doing the four books commissioned by the League of Nations. You were a coauthor on the Notestein book, The Future Population of Europe and the Soviet Union, the first one to come out [1944], the one in which Ansley Coale did the projections. What part did you do in that book?

KIRK: I wrote a couple of chapters. We were all involved; this was a team effort.

VDT: Then there was your own important book, Europe's Population in the Interwar Years [1946]. In that book, you used the expression "demographic transition." You also called it the "vital revolution." Who was the first to use the actual expression, "demographic transition"?

KIRK: I think it was probably Frank Notestein, though I'm not sure. We demographers were a very small, compact group; we had a lot of interchange with each other. So it's really quite hard to pin down something like that--who was actually the first to use the expression.

VDT: Of course, the concept was being developed just at that time. In your book, you stressed the

importance of modernization--the threshold of development that would tip off fertility decline. You wrote a lot about the modernization threshold later.

KIRK: Later I did, yes.

VDT: Do you still believe that modernization is necessary first--that there can be no fertility decline engineered by direct family planning programs without waiting for associated socioeconomic development?

KIRK: No, I don't believe that. Modernization is certainly important, but a country like Indonesia, which has an effective population policy, has reduced the birth rate substantially. And, of course, the outstanding example is China.

But I did write a couple of articles on modernization and fertility decline in Latin America, where in those days there was no antinatalist policy. I was enamored then of the threshold hypothesis; wanted to test it out. And it worked well in Latin America, that is, it predicted when the birth rate was going to go down, by the degree of modernization. There were certain critical points and when a country got beyond that point, the birth rate began to go down. Frank Oechsli of the University of California and I developed an index of modernization that included education, health--ten items, a whole series of socioeconomic variables--and it worked very well in Latin America. But, I have to admit the threshold hypothesis doesn't work elsewhere. And I don't believe it works in Latin America now, because the whole culture is now attuned to birth control, the idea of reducing the birth rate--not only the government but the people. So the birth rates are going down quite fast in most of Latin America now.

The threshold hypothesis was intriguing because with it you could, hopefully, predict how fertility was going to go. But I don't think it's worth much now.

VDT: Well, the rapid fertility decline has also slowed down in some countries.

Back to your book, Europe's Population in the Interwar Years. It was very influential and has been much praised. You pointed out to Andy Lunde in the 1979 interview that at OPR you were creating a small library of books on population, which were rare in those days, with the four League of Nations volumes and Irene Taeuber's book on The Population of Japan [1958] and Kingsley Davis's book The Population of India and Pakistan [1951]. You were prescient in your book. You foresaw that there would be no permanent increase in Western European fertility after World War II, which was true--there was a bump of a baby boom to about 1964 and since then fertility has been very low. You said that was due to growing "individualism." I was struck by that because that's the term now used to describe the motivation for Europe's low fertility. Dirk van de Kaa used it in his Population Reference Bureau Population Bulletin, "Europe's Second Demographic Transition" [March 1987], that I edited, and he built on the work of Ron Lesthaeghe, who also used that term, which you had already used, back in the 1940s. Also, you found pockets of high fertility--the Netherlands then, Central Russia, and Albania. You wrote more later on Albania.

KIRK: Yes, Albania is the last underdeveloped country in Europe. Unfortunately, they haven't taken a census in a long time and the vital statistics aren't very reliable. Fertility has now gone down, but how much, nobody knows.

VDT: How did you get your data for that book? You had a tremendous array of data. You had an appendix table with even the gross reproduction rates for 600 areas.

KIRK: Europe historically, for a long time, has had much better vital statistics than we had in this country. They are quite reliable and you could compute reproduction rates. The total fertility rate hadn't been developed as a measure. We computed reproduction rates just for fun.

VDT: You described that in the 1979 interview, all of you sitting around doing net and gross reproduction rates, inspired by Lotka. It was a tremendous collection of data in that book.

KIRK: And I think I mentioned to Andy in 1979 that when I go to Europe now, I love to look at the minor civil areas--the departements of France, counties of England--and think about how they were 40 years ago and what it is now. Of course, fertility has now gone down.

VDT: Yes. But fertility has gone up again slightly in Sweden and such countries--up to 1.9 total fertility rate in Sweden in the 1989 PRB World Population Data Sheet [and 2.0 in the 1990 Data Sheet]. Fertility is now lowest in Italy [1.3].

KIRK: Italy is curious. I think people still have an image of Italy with a lot of bambinos. It's just amazing--the lowest in Europe!

VDT: Frank Notestein, of course, was a dominant character at the Office of Population Research and you say he remained a friend of yours all his life.

KIRK: I had two major mentors in my demographic career and the first was Frank. I also had a tremendous respect for the other, Frederick Osborn. He was a very tall man, a very remarkable man--a true American aristocrat in the best sense, that is, in the sense of having responsibility to go along with prestige. He felt that he had a responsibility to go out there and help the country. And he did. And while he was never a technical demographer as such, he was in a sense our statesman. He got funds for us and you know that he essentially started the Office of Population Research at Princeton; he got support for it. He was a Princeton alumnus, of course.

VDT: I understand he first approached Harvard and they turned him down.

KIRK: That's true. He was glad, of course, to have it at Princeton. Then later when he had helped to found the Population Council, I worked for him there. Fred Osborn was a very dear, close friend.

In demography per se, the founder of the field in this country, in a way, was Frank Notestein. Yes, he and I were close for over 40 years, from 1939 till his death in 1983.

Frank was director of the Office of Population Office from its beginning. When I got there in 1939, he had a tiny office. There was Frank, me, a secretary named Martina Evans, and Ansley Coale, then a graduate student. That was OPR. Henry Shryock and John Durand had been there earlier. We were in three rooms on the second floor of a building on Nassau Street and exactly opposite my office was that of a podiatrist.

I remember a colleague in the economics department saying, "How can you find enough in the field of population to spend your time on?" That was sort of the attitude. Population was something aside, just a figure that you put in but you didn't do anything with it.

So that was the start, and, of course, it grew. Irene Taeuber worked at the Library of Congress in Washington and she sent up the abstracts for the Population Index. She would do the bibliography and she wrote most of the "country items" too.

VDT: She was writing those lead articles on the population of different countries anonymously, didn't get a credit line on them.

KIRK: Well, she did a tremendous amount anonymously and a tremendous amount over her name too. She'd come to Princeton to consult. Irene was always a bit eccentric in her manner of things. If you said something, she'd say "No, and . . ." but then she'd agree with you. If it began with no, it would end up with yes. Of course, she was a remarkable woman.

Also, Frank Lorimer stood out. He was another person I always admired very much. He was in Washington at the American University but he came up to Princeton to consult on his book on the Soviet Union [The Population of the Soviet Union, 1946], because it was published by OPR.

VDT: I want to talk about him later, because in your 1979 interview you cited him as one of the broad-gauged people working in population in the early days.

Why did you leave the OPR to go to the State Department?

KIRK: It was an opportunity, really, to advance demography. I was the first person in the federal government to have the title "demographer." Later on the Census Bureau had demographers. I was in the Office of Intelligence Research, which was the successor of OSS [Office of Strategic Service]. I was responsible for population research in the State Department, later joined by Earl Huyck and Chris Tietze. There was something called National Intelligence Surveys, which were conducted for most every country in the world. We were responsible for the opening demographic section on countries X, Y and Z. We spent a lot of our time on that.

I became head of a division of research dealing with South Asia and Africa, huge territory! When India was partitioned, I had responsibility for drawing up what would be the fairest line between India and Pakistan, that is, in terms of the population of Muslims and Hindus. This was so the U.S. would have a view. Of course, the specific geographic delineation was done by the British, and they did a pretty good job.

VDT: As far as your figures were concerned, it was about the best place the line could have been?

KIRK: About the best they could have done. It was very hard, because there were minorities on both sides.

Of course, there were other things. I was involved in political aspects too. I had a background in political science and international relations. Every day, we had a review of intelligence that was coming in and I was there supposedly as an expert on the Near East and South Asia and Africa.

When they were going to construct the Aswan dam in Egypt, the United States opposed it. Dulles was Secretary of State and he didn't think it was a good idea. We sent up a statement on how we thought it was a good idea, the dam should be built, with U.S. assistance. But Dulles's secretary, who was in charge of looking into it, said he wouldn't dream of presenting our statement to Dulles. This illustrates our frustrations when Dulles replaced Acheson as Secretary of State.

VDT: You told Andy Lunde about the rather unsophisticated methods you used to make population projections, because, well, there were no computers then.

KIRK: There were no computers. We did a lot, but they were short-range projections; not really anything fancy.

VDT: Then you went to the Population Council, which had been established in 1953, was it?

KIRK: It was contractually established in 1952, but established an office in 1953. I came in 1954. There were Frederick Osborn, Margaret Cramer, who was the accountant, Catherine Glazer, who was the secretary, and myself, and that was it.

VDT: Again you came in right on the ground floor. Frederick Osborn was the first director?

KIRK: That's right. Frank Notestein had proposed me to Fred, but it was Fred who persuaded me to join the Population Council. It was a very pleasant experience working with him. And with Frank too, as I have said. [Frank Notestein left OPR in 1959 to succeed Frederick Osborn as second director of the Population Council.]

VDT: In your interview ten years ago, you said you were there through the "real excitement period" of the Population Council, when it . . .

KIRK: Was just starting up.

VDT: It was also the time of orthodoxy. The latest Population and Development Review has a lead article by Dennis Hodgson on "Orthodoxy and Revisionism in American Demography" [PDR, December 1988]. By orthodoxy, he means the idea that rapid population growth was impeding development in less developed countries and could be brought down by direct intervention, family planning programs, without waiting for socioeconomic development. Of course, the Population Council was one of the leaders in that policy; that's what he implies. Do you agree with that? Of course, John D. Rockefeller set up the Council because he felt something had to be done and the Rockefeller Foundation was drifting away from interest in population. In the 1950s the Population Council mostly concentrated on research and you were head of the division for demographic research.

KIRK: Yes, but we expanded a great deal. We didn't really feel any great conflict between the idea that modernization promotes reduction of the birth rate or that family planning does. From the first, we were promoting the family planning idea. And almost from the first, we had a scientist at the Rockefeller Institute doing research on contraceptives, though this was modest at first.

I think my own greatest contribution to the Council was the selection and nurturing of Council Fellows.

VDT: You chose the Population Council Fellows?

KIRK: Yes, at first; later on we had a committee, but I had a lot of say in choosing them. I administered the program, which began in the mid-1950s.

VDT: These were young people brought in from less developed countries to study at American Universities and the idea was to build up demographic expertise in their countries--that they would go back to their countries?

KIRK: And to get a viable national group, large enough to grow by themselves. A few years ago, I looked at who were the leading demographers in less developed countries and over a third of them had been Population Council Fellows. Isn't that something!

VDT: Who were some of them with whom you had a hand? What about those who went to the

University of Chicago: Visid Prachuabmoh from Thailand and Mercedes Concepcion from the Philippines and Haryono Suyono from Indonesia? Were they your Population Council Fellows?

KIRK: Yes.

VDT: And Iskander from Indonesia?

KIRK: Yes. He changed his name because of prejudice against Chinese names in Indonesia. He went to Princeton.

VDT: Can you think of some others like that?

KIRK: Yes, indeed. There was Mohamed El Badry (from Egypt), Alvaro Lopez (Colombia), Gustavo Cabrera (Mexico), Carmen Miro, S.N. Agarawala, P.M. Visaria, Lee-Jay Cho, Saw Swee Hock, Etienne van de Walle, Thomas Frejka, and many others. Perhaps more familiar are the Americans who were Population Council Fellows, such as Judith Blake, Paul Demeny [originally from Hungary], Reynolds Farley, Robbert Potter, Harriet Presser, and Joe Stycos.

VDT: Did you go out to the countries to meet them; you traveled a lot?

KIRK: I did indeed, to find suitable recruits. And we had groups sent out for more general surveys.

VDT: Yes, you and Phil Hauser and Bud Harkavy went in late 1962 on a trip to Southeast Asia.

KIRK: Yes, I still have the notes we wrote. Then, of course, demography was pretty primitive in most of these LDC countries.

VDT: I would think so. How did you go about finding people who were even interested in coming to study in the U.S.?

KIRK: Once we had one person in the country who'd been here, then they would find others suitable to come, to build up a nucleus of demographers in the major countries.

VDT: The trip you made with Bud Harkavy and Phil Hauser, that was the beginning of the connections with the Philippines and Indonesia and Thailand, those three countries in particular. Did you know they gave a dinner for Bud Harkavy at this last PAA meeting [in Baltimore], that you missed?

KIRK: Yes, I was sorry to miss that.

VDT: He looks just the same as he always has, very youthful.

Those must have been exciting years in the Population Council. You felt anything was possible. The idea was to build up expertise for demographic research in these countries?

KIRK: That's right--a "critical mass." We thought it was smart to start with research and with research, as Bernard Berelson used to say, "invented here." They'd listen much more to an expert from their own country--not think of it as something that was . . .

VDT: Thrust on them by the U.S.

KIRK: Yes. We were too cautious really. We didn't realize how much latent possibility there was. We were very cautious about urging countries to have family planning programs and all that sort of thing. The culture in a lot of these countries was not so unfavorable to the idea of family planning as it was in Western culture. We were sort of reading from Western culture all the opposition--the Catholic opposition, the whole elaborate machinery of government as opposed to family planning.

VDT: In the United States, yes.

KIRK: In Europe also. We were sort of projecting from that, and we were wrong.

VDT: Except for the Philippines, which has a strong Catholic church, but that's the American influence in a sense.

KIRK: But we could have pushed faster.

VDT: You mean in Thailand, Korea, Singapore--they were all just waiting?

KIRK: Yes. Not only the governments but also the people were beginning to be concerned about population growth.

VDT: Did you have anything to do with the KAP studies, which of course were showing [a demand for contraception]?

KIRK: Oh yes, the Population Council really started that. We financed quite a number of those. Of course, later on they spread with AID and different sources of funding.

VDT: And the KAP studies led to the World Fertility Survey. You talked about getting Thais to speak to the Thais and so on. I'm always interested in Studies in Family Planning, the string of authors on each article and there'll be three or four Thais and one American, sometimes the senior author, sometimes the junior author at the end, or sometimes in the middle.

KIRK: Yes, we were very careful on that. It was hard for them at first, because they were new to demographic research. But I think the program is very successful. We always had a difficult time persuading--by that time, the Ford Foundation, or even the Rockefeller Foundation--to give a large amount of money for Fellows.

VDT: Most of the money came from Ford, through the Population Council?

KIRK: Yes. Ford and Rockefeller always referred to us as the retailers; they were the wholesalers. We had a hard time explaining to them the importance of fellowships. From their point of view, fellowships were not a good investment, because they don't show early return. The foundation likes to see an early return. And in this case, it may be five or ten years before anything shows. They were generous, of course, but they kept wanting us to justify it every year.

VDT: You brought students over for one-year, two-year courses, or. . .?

KIRK: Some of them would get a master's degree and go back, but our objective was to have them go

for the Ph.D. and really get fully trained.

VDT: So they could go back and set up a department or center for demographic research?

KIRK: Yes.

VDT: Did the Population Council try to follow them up?

KIRK: Yes, we supported them when they went back. We gave them financial assistance for projects, or on occasion, we'd finance a position for a Population Council Fellow in the government or a university.

Then we were very interested in the regional centers, as the one in Santiago, Chile, for Latin America, CELADE, which Carmen Miro led so effectively, and the one in Chembur, near Bombay, which was later taken over by the Indian government.

VDT: When you say the Population Council was interested, does that mean some financial support came from the Population Council?

KIRK: Oh yes. We largely established them, in terms of money. Well, CELADE, of course, was under the Economic Commission for Latin America, ECLA.

I remember one horrible experience I had there; I went down quite often. There was a meeting of an advisory committee for CELADE, presided over by the head of ECLA. We met about 10 o'clock and they elected officers and I was elected vice-chair. I assumed this was sort of honorary, because the Population Council had given the money. But at 11 o'clock, the ECLA man announced that he had to catch a plane for Mexico and he left and who was put in the chair?

VDT: There you were in the chair, one hour after it began!

KIRK: Yes, and I was the only non-Spanish-speaking person there, except for Nathan Keyfitz, who had extremely heavily accented Spanish, but he could get along. There was a man from the statistical side who sat beside me, a Castillian Spaniard, spoke beautiful Spanish, and I could understand every word he said. He spoke excellent English too. The rest was all the way down to Caribbean Spanish, which has no s's, and Chilean Spanish, which is pretty bad.

At first they gave me a nice girl to translate for me. I was hearing it in Spanish in one ear and then the English in the other ear half a sentence later. I was driven up the wall, and went to Spanish alone. It was a three-day conference, and I just struggled through it, with the help of the Spaniard, the statistician. I got so I could understand what they were talking about. But I didn't know whether they were for it or against it. The Spanish was more formal than it is now and they'd say, "As my distinguished colleague has said so beautifully, in such perfect language" and so on and so on.

What I got to listen for was the little word "pero"--"on the other hand." That was followed by the real substance of what they had to say. I learned more Spanish in those three days than in all the time before, or since.

VDT: CELADE was already taking on students at that time, in the 1950s. quite early?

KIRK: Yes, it was about 1955. Now it's independent and, as I said, the Indians have taken over the Bombay center.

VDT: Did you ever go out to teach in any of these places?

KIRK: No, we got good people to go. Dorothy Thomas was one; she went to Bombay. I remember she had a pet mongoose, because there was brush around the center and snakes. Mongooses eat snakes, so she had this pet mongoose to protect her.

VDT: Don Bogue was out there early on.

KIRK: That's right. The Chembur center turned out some very good people, and still does.

VDT: The Population Council was very important at that time.

KIRK: The accomplishments we were making were rewarding, I thought. And, of course, the Council was growing.

VDT: You were head of the demographic division; Sheldon Segal was head of the biomedical division . . .

KIRK: He became head; he wasn't the first one. The third was the international division. The demographic division became the Center for Policy Planning.

VDT: Frank Notestein was head of the Population Council from 1959 to 1969, then Bernard Berelson, then George Zeidenstein came in the early 1970s. Did you have anything to do with the Parker Mauldin-Berelson research that began to look at the relative contributions of socioeconomic development and family planning, when they had to prove that family planning had a net impact on fertility decline?

KIRK: No, I'd left the Council by then. I left in 1967.

VDT: Why did you leave and go to Stanford?

KIRK: I was attracted to Stanford and I felt that the Council was getting to be too much of a family planning rather than a research organization. And that followed on more and more. Zeidenstein pushed aside Paul Demeny, who was head of the Center for Policy Planning. He displeased Zeidenstein, because he wasn't policy-oriented enough.

VDT: That just happened last year or so, but it had been coming to a head for a long time. I interviewed Paul last June in New York, just after that decision had been made. However, he has plenty to do.

But you yourself felt in the 1960s that the Population Council was becoming too program-oriented, too family-planning oriented; you were more interested in research?

KIRK: Yes.

VDT: And that entered into your decision to come to Stanford?

KIRK: Also my family. We spent a year here at Stanford at the Institute for Advanced Studies in the

Behavioral Sciences, a think tank, in 1964-65. I took a year's sabbatical from the Council. And my wife and children had liked it so much, they urged me very much to go when I was offered the opportunity. So we came back. I joined the Food Research Institute and the department of sociology. Later I had a joint appointment.

VDT: Tell me something about your work at Stanford.

KIRK: I got very interested in Mexico and Latin America. We did this threshold hypothesis idea. I was very much interested in the acceleration of fertility decline.

VDT: Which you felt was happening?

KIRK: It was happening. As you pointed out, in some countries it has slowed down. But in those days--and I think I was one of the first to point that out--if you projected these things ahead, it appeared that fertility was likely to go down faster in the less developed countries than it did in Europe. And it did.

VDT: Indeed, in such countries as Costa Rica and Mexico.

KIRK: I think more particularly in Asia.

VDT: In your 1979 interview you said that the ten countries where fertility decline had been fastest had all been islands or--there was another word you used; I think you meant peninsulas.

KIRK: Islands and peninsulas, yes: Japan, Malaysia, Singapore, Hong Kong, Indonesia, Sri Lanka, South Korea, which might as well have been an island, in view of its political separation from North Korea and the Asian mainland. The demographic transition proceeded faster than in mainland countries; they're more exposed to cultural influence from outside. Japan, of course, is an outstanding example.

VDT: Right. Its crude birth rate dropped from 34 to 17 in one decade--due in part to abortion, which wasn't so much used in other places.

Ten years ago you were also working on migration in Mexico.

KIRK: Within Mexico and Mexico to this country. But I got quite interested in the food problem in Mexico and went to conferences on that issue in Mexico. They had the feeling their food was being taken from them, with vegetables grown in Baja California and Sonora and cattle from other areas being exported to the U.S. And they were interested in alternative supplies.

One of the most interesting dinners I've ever had in my life was at a conference, given in the botanical garden of the University of Mexico. We had a dinner of possible foods--snakes, insects, molluscs, exotic seafoods, wild vegetables.

VDT: You ate these things because that might be all there was left to eat?

KIRK: Well, the Mexican sponsors of this conference were exposing us to other possible things to eat, perhaps for public relations. We were on TV. They focused in on me, perhaps to see if a gringo could eat all the "delicacies."

VDT: The idea was the Mexicans felt they could not feed their growing population because their land was being exploited for food grown for the U.S.?

KIRK: Yes, whereas in fact, you go barely outside of Mexico City and you see very primitive, rainfed, agriculture. Corn and beans are the main stuff. In the north, northwest in particular, the area that sends vegetables to us, it's a highly developed agriculture, entirely irrigated. The people of European background had developed entrepreneurially; things moved ahead. But the Indian population follows the old ways; it's hard to change. I got interested in that.

VDT: At that time did you bring in the population angle?

KIRK: Oh yes.

VDT: If Mexico continued to reduce its fertility, it would have fewer people to feed?

KIRK: I wrote some articles on the future population of Mexico, predicting a rapid decline in fertility. But even so, there's a tremendous cohort of young people.

VDT: You also have worked quite a bit on migration within Mexico and to the U.S.

KIRK: Yes. Curiously enough, the Mexican border states didn't grow as much from net migration as one might expect, because a lot of migrants that went to border towns went on across to the U.S. It was sort of a staging ground for migrants from northern and central Mexico.

It's not commonly recognized in this country that these areas are selective of more European types. The more southerly areas are more Indian. At the National Museum in Mexico City, the handsome girl guides, mixed Indian and European, will tell you about the culture that has amalgamated. But you go to Oaxaco and that's primarily Indian. We went to a wedding there, which was held in what had been a convent, and from the balcony we looked over the crowd--I suppose there were 200 people there--and I saw only one person with an Indian face. The upper class minority is definitely European in appearance. I'm citing this to show that prejudice really exists. But I've wandered a bit from demography.

VDT: No, that's migration, that we're talking about. In your PAA presidential address of 1960, which we'll get to later, you wanted demographers to look more at migration. I think they are doing so now.

KIRK: Yes, I spoke of migration as the "stepchild of demography."

VDT: You mentioned some leading influences on your career--Fred Osborn, Frank Notestein . . .

KIRK: And Frank Lorimer.

VDT: Tell me about Frank Lorimer. I've heard what a wonderful character he was; how he married again, that New Zealand nurse he met in Africa. They went to live on a commune in New England, had a child when he was 79, and went out to New Zealand.

KIRK: They adopted another daughter, Petra's illegitimate niece. He also had adopted children by Faith, his first wife.

Frank Lorimer illustrated what I see as a great difference from those presently in demography. He had very broad interests, a broad background; he'd been a minister. At that first meeting of PAA I

attended in 1939, 50 years ago, there were 17 participants, who gave papers, wrote joint papers, discussed papers and so on. Only 17, and 14 of them were then in Who's Who or a subsequent edition. And I doubt very much whether the 529 participants that I counted in the 1988 meeting program--think of it, 529 as opposed to 17!--I doubt very much whether they're going to have as high a proportion in Who's Who.

VDT: What do you think accounts for that, that population then drew such eminent people?

KIRK: That was before it was very firmly located in sociology. Several disciplines would be represented in these meetings, broad-scale people. Frank Lorimer, besides the ministry, studied anthropology. He and Fred Osborn wrote this book at the Museum of Natural History in New York, Dynamics of Population [1934].

VDT: Who else was outstanding at PAA meetings?

KIRK: Phil Hauser. As you know, he had a large influence at the Census Bureau. He was at all these PAA meetings. And there was Henry Shryock, John Durand, and Lowell Reed, for whom I had a great deal of respect. Lowell Reed was a biostatistician and a very level-headed person. He was very good at chairing meetings, because he'd get to the heart of the discussion, keep it focused. He was at Johns Hopkins.

VDT: You mentioned him in your 1979 interview. You felt he was a real inspiration in demographic methodology.

KIRK: Yes. Well, as a biostatistician, he came at the field from a different point of view. And I remember we had John D. Black of Harvard, an agricultural economist, so our meetings drew from different disciplines.

Of course, demographers come from different fields today, but we're more specialized now.

VDT: It's become a field in its own right and perhaps within that field . . .

KIRK: That's right, specialized. As you point out, I'm in migration. But, of course, there's a whole range of specialties dealing with population.

VDT: And in your time, you felt that you would like to steer clear of too much emphasis on numbers rather than their meaning. You said you came from the pre-computer age; you had your students do your computer programing for you when it was necessary.

KIRK: Well, after I came here to Stanford. When I came in 1967, the computer age was just beginning.

VDT: You mentioned to me at a PAA meeting not too long ago that you felt more should be said about Warren Thompson. You noticed that he hadn't even been mentioned in the International Encyclopedia of Population [1982].

KIRK: And Whelpton, of course. The two of them were at the Scripps Foundation and they were always participants in the early meetings. Warren Thompson's book [Population Problems] was the textbook in the field for years, generations. It had five or six editions. I had a lot of respect for him.

And I liked Pat Whelpton very much. Pat originally was sort of in Thompson's shadow.

VDT: What about Kingsley Davis? I'm going to interview him two days from now. Is he among your leading influences?

KIRK: I wouldn't quite place him in that category; he isn't much older than I am. I first knew him when I was at Harvard as a graduate student. He was teaching at Smith and came over and talked. Then we were at Princeton together.

VDT: Did you perhaps have differences of opinions?

KIRK: Yes we did, sometimes.

VDT: Norman Ryder in his interview with me last year said he had been asked at the New Orleans PAA meeting just before that who really came up first with the term "demographic transition." He felt that Kingsley Davis had used it in the volume of the Annals of the American Academy of Political and Social Science on "World Population in Transition" that he edited in 1945, but that he'd borrowed that from you. You had published an article, using that expression, in 1944, the year before.

KIRK: Well I had, but I never thought of that. I never put those things together.

VDT: As you said earlier, you were all working on it together.

KIRK: But--I suppose I shouldn't say this--but Kingsley has been a prickly character.

VDT: That is exactly the expression that Norman Ryder used!

KIRK: Well, I hesitate to put it on the record. But he's an extremely able person. Of course, he was chair of sociology at Berkeley for some years.

VDT: And you were at Stanford. Well, I guess you've both now mellowed. (Laughter)

KIRK: He was offered a job at Stanford too at the time he left Columbia, but Berkeley offered him \$2,500 more, so he went to Berkeley. Our local demographic history might have been quite different, if he'd come to Stanford.

VDT: What about your students while you were here at Stanford?

KIRK: I had a real difficulty because there was no demographic setup and I was in the Food Research Institute. I had difficulty in getting students who were interested in demography on fellowships. All the graduate students now come on fellowships and the departments distribute the fellowships and I didn't get a lot of those. So I didn't get the cream of the crop.

VDT: Those interested in demography would not necessarily come to Stanford?

KIRK: That's right, and I wouldn't blame them. My students were largely converts from other fields. Some of them have stayed in demography and some have gone into other fields.

VDT: Will that change now? I understand there's a new demography center with Fred Pinkham as

assistant director, who came from the Population Crisis Committee.

KIRK: It's a center for population and resources; it's not a department. Brian Arthur really started it. He was quite good at getting money and they got money to support this center and hire some people. But then he got tired of it and dropped it and it went over to an Australian who is interested in population biology. So we have a group that meets from time to time, but we don't have really a core group of demographers.

VDT: Perhaps that center will entice more potential demographers to Stanford, although there's the competition from Berkeley nearby.

KIRK: That's true. In any event, I never really developed a demography department or a demographic center of any consequence. I did have other demographers here and we did joint research. I had big classes and graduate students, although the majority did not go into demography. Of course, population was a big thing in the late 1960s and early 1970s. And Paul Ehrlich is here. He was more of a thunderer than I was.

VDT: And still is. I heard him recently; he's still harping on the same theme.

KIRK: He outshone me. So, I couldn't say that I've been altogether happy here professionally. My family has been happy and that was important to me.

VDT: California, a special life, and Stanford is a special atmosphere.

KIRK: Yes.

VDT: You mention Paul Ehrlich, who is still preaching the urgency of reducing rapid population growth in developing countries, and perhaps he's right. Meanwhile, there's the new revisionist approach to the problem--what do you think of that?--the idea that population growth is only one factor in economic development and, of course, at the 1984 Mexico City population conference, the Americans said it was a neutral factor. The National Academy of Sciences report in 1986 [Population Growth and Economic Development: Policy Questions] came out with the idea that population growth, while still important, was not the overriding important thing in development.

KIRK: That was discussed at the PAA meeting in San Francisco [April 3-5, 1986]. Nathan Keyfitz was a visiting professor here then and he was very much upset by it and wrote impassioned letters and articles.

VDT: He felt it was downplaying rapid population growth? And what do you think of it?

KIRK: I thought it a good report. As you say, there has been a lot of revisionism. I guess I could be put in the category of revisionists. The issue is whether or not you can have economic development, at least rapid economic development, in a rapidly growing population, and there's actually no empirical basis for saying you can't. You have lots of countries with rapidly growing populations who've had economic development too.

VDT: Brazil--and also Mexico, at certain times, not always.

KIRK: Right.

VDT: There's a funny expression used about you in that Dennis Hodgson article that looks at the shift from orthodoxy to revisionism. He says that in the 1970s, you, Kingsley Davis, Clyde Kiser and Frank Lorimer were labeled "eugenic demographers" by the feminists, because they felt you were supporting family planning programs to bring down the birth rate at whatever cost in developing countries and they felt that perhaps had eugenic, racist overtones.

KIRK: I've been called that because of long association with Frederick Osborn, and I was president of . . .

VDT: The Eugenics Society?

KIRK: Yes, though the name was changed to the American Society for the Study of Social Biology. I was president of that for three years [1969-72]. And I've always felt there really is a eugenic aspect of this.

In a way I hate to go on record for saying this, but I think there's a real problem in the Western civilization in that we are approaching a stationary population and the rest of the world, the less developed world, is rapidly becoming an increasing proportion of the total population. Since I have a background in political science, I see that as a power problem too. Because as these countries get developed, and particularly as China gets developed, their large populations are going to be a tremendous asset. That's a debatable question, of course, but I think so. I think that sheer size is going to have a very great effect on our position. In the past, Western civilization was a rapidly expanding civilization in numbers, in population, as well as in technology. I see us having to face a major readjustment in which power is going to go to other countries. And maybe we'd be better off if we had more people. I've spoken about this.

Yesterday, we went out to a restaurant and there was an Irish woman and she had six children, a beautiful family, and I told her I admired her family. Because--this is a difficult thing to express really--numbers are really going to count. In Western Europe they're beginning to be concerned about low fertility and I predict that we are going to be concerned about it. Maybe not in five or ten years, but by the year 2000, something like that.

VDT: You also said in your 1979 interview that you had been watching the growth of the Hispanic population in this country, which will become an increasing proportion of our population.

KIRK: Yes. California already has a majority of minorities in the schools; the non-Hispanic whites are a minority in the schools. Those children are the population of the future.

VDT: And that's something we should think about?

KIRK: We should think about it. I waver. Sometimes I think we ought to admit Hispanics, we shouldn't be as selfish as we are about our standard of living and our possessions and so on. I waver between that view and that we ought to keep them out. I guess I lean more toward letting in a moderate amount. But we'll have to have immigration if we're going to keep up the total population.

VDT: Keep up our population growth or . . .?

KIRK: Population growth and ultimately our population size--unless we have some rise in fertility.

As you point out, in Sweden and some countries the birth rate has gone up.

VDT: Slightly. In the U.S. too. In the latest Population Reference Bureau Data Sheet [1989], the U.S. TFR rounds up to 1.9; it had been rounding up to 1.8.

KIRK: The increasing minority population in the U.S. is a problem, I think, but I don't think it's an irresolvable problem.

VDT: What do you see as leading issues in demography over the years you have been involved? We have covered rapid population growth in less developed countries. In your 1960 PAA presidential address, which you had first titled "Population Shibboleths of 1960," then when it was published in Population Index it had gotten a very sober title ["Some Reflections on American Demography in the Nineteen-Sixties," Population Index, October 1960], you decried the lack of attention to migration, which you saw as an important variable at the local level, in particular, in population change. Don't you feel there's now more attention to migration in the field of demography?

KIRK: I think there is more attention, but it hasn't really grabbed a large proportion of demographers, and for a very simple reason: mathematical demography is in the ascendance. Births and deaths are such nice variables--they're unique, they happen only once, at least as far as we know--and births and deaths are variables that you can use mathematically very easily. Migration is entirely different, because you can migrate more than once, you can migrate in a thousand different directions, you can migrate for different times and periods. It's a very messy variable. And demographers have shied away from it--mathematical demographers entirely. Meredith John, who is here at Stanford, she teaches a course on demography and there is only one hour devoted to migration, because she's a mathematical demographer.

You've heard me say before that if you go below the national level in this country, the most important demographic variable is migration. If you talk about local areas, you have to be concerned primarily with migration, because birth and death rates aren't that much different. Migration is the important variable, and it's neglected because it's difficult to deal with in a mathematical framework. In that 1960 address, I referred to migration as a "stepchild of demography," which I think is the case even today. As you say, there's a lot of attention to it within demography, but it's within the context of a tremendously expanded attention to population study as a whole. It's still not a leading interest in demography.

VDT: And you feel it should be?

KIRK: I feel it should be because, you know, the true test of science is prediction and if you're going to predict population change below the national level, and sometimes at the national level, you have to make some prediction about migration. Now, the Census Bureau really dodges that issue by just assuming some arbitrary constant amount of migration.

VDT: Indeed, 600,000 net migration.

KIRK: Right, and it isn't based on anything except taking a number.

VDT: Well, it's based on legal migration and a bit thrown in there for illegal migration.

KIRK: Yes, but it's based essentially on what's happened in the past. I understand why that should be. But I do think it needs more attention and what brings about migration, in a sociological context and

economic context.

VDT: Sociological and economic causes of migration?

KIRK: And consequences too. I have this feeling about the whole field of population that we don't really deal enough with the consequences of population change. We talk about economic consequences in a broad sort of way, but the revisionists have looked at this more closely and they say the effects of population growth are different from what has been conventional wisdom among demographers.

VDT: In your 1960 presidential speech, you did say that you felt there should be more emphasis on consequences. You felt that demographers got hung up on causes because they were so stunned by their projections of the 1930s that turned out so wrong because they didn't foresee the baby boom or rapid population growth in developing countries, so they shifted back and instead of saying what was going to happen to population, it was safer just to describe trends and deal with causes, possible causes.

KIRK: That's right. Migration is not an easy field to study. As I say, I spent considerable time studying it as it happened in Mexico. The data are primitive because in the census, you only have state of birth and then their present state. So it's hard to get a current basis of migration in Mexico.

VDT: They don't have anything like the U.S. Current Population Survey with "Where did you live five years ago, or last year"?

KIRK: They're beginning to, but they haven't done it systematically to cover the whole country.

VDT: They're having a survey of that type?

KIRK: Well, they've had fertility surveys but they don't have a national population survey.

VDT: Which of your publications are your favorites, or those you feel were most important? Of course, your book, Europe's Population in the Interwar Years.

KIRK: At the time, I thought the articles I wrote on the accelerating decline of fertility in less developed areas were important. There were several articles that referred to this. Perhaps the best was "A New Demographic Transition?" in the National Academy of Sciences, Rapid Population Growth [1971].

VDT: You had a fine article in Population and Development Review in 1979 ["World Population and Birth Rates: Agreements and Disagreements"]. Was that one?

KIRK: No, that was about errors in population estimates. Another article that I particularly liked was on "Factors Affecting Moslem Natality" in Bernard Berelson [et al, eds.], Family Planning and Population Programs [1965].

VDT: What accomplishments in your career have given you the most satisfaction?

KIRK: I suppose that the greatest satisfaction I've had is what has happened to the Population Council Fellows and what I had to do with it. I had to recruit funds to run the fellowship program. We used to meet the Fellows when they arrived by air. We paid a lot more attention to them then than later.

VDT: I forgot to bring up a very important occasion--Paul Demeny and his thrill at how you picked him out, in Geneva, when you heard from Frank Lorimer in 1957 that here was this Hungarian who had gotten out of Hungary after the Communist takeover--officially, because he was at official meetings in Geneva and he decided not to go back to Hungary. His professor in Hungary got in touch with Frank Lorimer who got in touch with you and within a month there you were in Geneva, interviewing him at the Beau Rivage, a hotel that still exists. You had lunch there and within a month or so, here he was, a Population Council Fellow, arriving in the United States to go to Princeton and you had him met at the airport in New York.

KIRK: I did a detective job to find him. He had a sort of mail drop with a professor at the university. I went to the apartment where this man lived and the concierge didn't know anything about Paul Demeny and I didn't know the professor's name, so he said, "Well, go around and try each apartment." I knocked on each door and several of these nice Genevois invited me in to have coffee! They hadn't heard of Paul Demeny either; I was just a strange American who had knocked on their door. I finally did find the door of the professor but there was no one there. So I went down to the police station. You know, everybody has to register in Geneva.

VDT: If you come to be a resident in the city, right. I know, because we lived some years in Geneva. Two of my children were born there and I even went to the Institut des Hautes Etudes Internationales, where Paul was briefly.

KIRK: And the gendarme said he was going to the university but he didn't have his address. I went to see Gunnar Myrdal [then director of the Economic Commission for Europe], just before he went with his wife Alva to India where she was Swedish ambassador, and his English secretary called the university for me--my French being very bad--and found out where he lived and I sent him one of those telegrams and that's how I got in touch with him.

VDT: He didn't tell me that complicated story but he talks about the impressive lunch that you had at the Beau Rivage and how you snapped him up so quickly and made certain he got to the U.S. and to Princeton. Do you have any other dramatic stories like that about choosing Population Council Fellows?

KIRK: No, I can't think of any quite so dramatic.

VDT: Now let's turn to your reminiscences of PAA. We don't have to dwell on that too long because you wrote such a fine vignette on "PAA Meetings Over the Years" for PAA Affairs in the Spring 1983 issue. I love that one. In the book in which I'll put together excerpts from these interviews, I'll also include the "vignettes of PAA history" and that's an important one.

Your first meeting was in Washington in 1939; it was at the Hay Adams Hotel. Irene Taeuber interviewed you for the job at OPR. Lotka was president then and gave a dramatic talk. I believe it was Kingsley Davis's first meeting too and he was so impressed with Lotka that he applied for a Social Science Research Council fellowship to go study demography, this wonderful field. [Davis had already received this fellowship. See his interview, below.] You wrote of the early meetings, often at Princeton, and there was such a difference from later on, because there were single sessions to which you all went, it was a small group, and so on.

KIRK: They were a small group. There were 17 participants in the 1939 program. And now there

are--I counted 529 in the 1988 program.

VDT: We've talked about several of the notables in the early years. What about Horace Hamilton, who was president the year after you?

KIRK: He was at North Carolina State University, did quite a little work in population, but I didn't know him too well.

VDT: Could you tell me about Kurt Mayer, who was secretary-treasurer [1959-62] during your time? He moved back to Switzerland.

KIRK: He invited me to go to Brown once to be a professor there, but I turned him down.

VDT: In 1960 when you were president, you switched the days of the meeting from Saturday and Sunday to Friday and Saturday. [The meeting was in Washington, D.C.]

KIRK: I think this is an important change that has occurred. Our meetings had been Saturday and Sunday and we were not subsidized, we paid our own way, and we didn't take time off from work. We came on our own funds and used up our weekends for this. I changed it to Friday and Saturday because I thought people were entitled to have a little weekend left, and the Association was agreeable to that.

VDT: Also in your year, you had a double session on the Saturday afternoon. That was one of the early years of double sessions. Anne Lee says they started at the 1957 meeting in Philadelphia. People then had to make a choice. Up to that time everyone had always attended all the sessions. [The first double session occurred the year before, 1956, on the morning of the first day of the meeting at the University of Michigan, Ann Arbor. At the 1957 meeting at the University of Pennsylvania, there were double sessions in four time slots.]

KIRK: That's right. It was a family affair; we all got together.

VDT: You felt that was true even at your meeting in 1960? Unfortunately, there's no record of how many attended that 25th annual meeting. Andy Lunde prepared a list of all the meetings and he had numbers at the early meetings [through 1935] but for many years no record was kept of how many actually registered. You might have had, say, a couple of hundred or so? Seven years later in Cincinnati in 1967, there were 452.

VDT: It was growing very fast.

VDT: In 1960 there were only about 500 members altogether; the half or less than that would have attended the meeting.

KIRK: I have my old programs going way back, but we've moved; they're somewhere in boxes in the garage.

VDT: But the programs don't tell you how many attended; those who paid the registration fee. I presume you had a registration fee?

KIRK: Probably a dollar. Do they have the attendance at the 1939 meeting?

VDT: No. There were 38 at the first "organizational" meeting in May 1931 and then for some reason there's some record up through 1935 [200 at the fourth annual meeting on May 3, 1935, in Washington, D.C.], and then not until 1967, 32 years later, did they again have some record of the numbers who were there. The latest meeting [Baltimore 1989] has the record so far, nearly 1,200 [1,193, later surpassed by the 1991 meeting in Washington: 1,399].

KIRK: 500 participants.

VDT: Five hundred plus, it must have been, on the program this year in Baltimore, the meeting you missed--which would have been your 47th or 48th meeting?

KIRK: I've missed four meetings.

VDT: Only four meetings in all those years since 1939!

KIRK: So somewhere I have all the programs, going way back.

VDT: In your vignette, you said you felt that the meetings had lost something by growing so.

KIRK: I think it was inevitable. But--I have a biased point of view, I guess--but I think the meetings were more pleasurable then. We knew all the people; we were all at the same sessions; we talked about the same things. It was a family affair, as I say, and we all were interested in each other's personal affairs. But, of course, this isn't the way a science grows; we had to have this expansion and specialization. But the specialization has brought some narrowing of interests. As I mentioned, there were people of broader interests in the early days; the papers tended to be broader.

VDT: Yes, it's becoming more and more specialized, even at the meetings. This year, for instance, they had specialized workshops on Wednesday afternoon and evening; they'd never had them on Wednesday evening before--before the beer party! As well as Thursday evening, which began a few years ago. There were a lot of workshops this year on China, an increasing specialty. But you still find the meetings pleasurable?

KIRK: Oh, of course.

VDT: You started the dinner meetings of the oldtimers, those over 70 who have been members at least 35 years. That continued this year; I'm sure they missed you. That's very special.

Do you think that PAA has a network of camaraderie that is perhaps missing in other professional organizations?

KIRK: Yes, more so than, say, the American Sociological Association or the American Economic Association, which are tremendous, gigantic things. But there's such a difference. Our programs in those days were two or three pages. Now they're a hundred pages [127 pages in 1989]. We'd have four or five sessions then and now there are 84. There were 84 in New Orleans [1988].

VDT: There were 84 this year, eight overlapping at a time.

KIRK: Eight overlapping! Well, the audience is not as attentive. There isn't the same level of discussion, not as it used to be, usually just a few random questions. The meetings were more significant in a sense. There weren't people moving in and out all the time, that sort of thing; there was more attention paid to it.

VDT: But you still go.

KIRK: Oh yes. I'll be there at Toronto [1990].

VDT: My hometown. My first meeting was the last meeting in Toronto, in 1972. At that time I was embarrassed that PAA was meeting in that hotel, the King Edward Hotel, which was shabby when I was growing up and it was shabby then. That was the hotel that turned the women away in the bar and the Women's Caucus rose up in wrath and said, "We will never again allow PAA to meet at any hotel that has any discrimination against women." Well, women could go in, but to a ladies bar, where you had to have a male escort; that was an old Toronto custom.

KIRK: I remember even in New York, at lunchtime, there'd be two or three floors of restaurants, and they always had one that was for women only, accompanied by men.

VDT: I guess Toronto was carrying on that custom. Next year the meeting is in the Royal York Hotel, which when I was growing up was the largest in the British Empire. It's still an old dowager of a hotel but it's been refurbished, so I hope it's a better place.

After all its growth, PAA is still rather small; it's fluctuated at about 2,600 members since the mid-1970s [2,679 at the end of 1989; 2,752, end 1990]. That's far smaller than other professional organizations, like ASA and the American Economic Association. So, it's still rather elitist, don't you think, by the standards of other professional organizations?

KIRK: It depends on what you mean by that word. If you mean in terms of specialization, it is, I think, though the huge organizations have tremendous numbers of programs on all different subjects. I don't think I'd use the word elitist. I think we felt a bit elitist in the early days, but now it's such a huge organization, that it's not so elite.

VDT: What do you see as the outlook for world population growth? Are you depressed by the fact that world population growth seems to be stuck, the rate hasn't gone down?

KIRK: Yes, I am depressed about it. I am depressed that Western civilization countries face the prospect of losing population. I think it's not healthy for a country to lose population, because then it becomes aged, lacks youth. It isn't the fact that there are fewer people so much as the effect in terms of age distribution. A gerontocracy isn't a healthy kind of society.

VDT: The other side of the coin of the rapid population growth in developing countries?

KIRK: That's right. Even the Japanese are now getting concerned about an aging population. This will be a problem. But most of the world is a long way from that. China maybe . . .

VDT: They are beginning to be a bit concerned about how distorted their age structure will be by their one-child-per-couple policy, though they're easing up on that.

KIRK: Yes, they are. And the one-child-per-family idea was never quite as effective as people like Ravenholt thought.

I see imbalance. The developed countries, if anything, need a higher fertility rate and the less developed countries, if anything, need a lower fertility rate.

You know, when they talk about the consequences of population change, it's forgotten that in 1939, the concern was a declining population in the U.S., in developed countries--Enid Charles's book, The Twilight of Parenthood, and so on. Somebody said to me a long time ago, "You're in a good field, because demography and the population problems of the world are going to be around a very long time. It's a good field to be in." And I still think that's true.

VDT: You must have enjoyed your career and felt it was important.

KIRK: Yes, that's right. I did.

VDT: May I ask you about your current interests? You and your wife are working with the California State Mental Health Association, is it?

KIRK: We have a son who is in the Napa State Hospital, in the wine country. Ruth is president of the Napa State Hospital Alliance for the Mentally Ill. There is a California Alliance for the Mentally Ill and there was a meeting of that, which I went to, that conflicted with the Baltimore PAA meeting. We think that we're making a real difference. I'm sure Ruth is making a difference. The other day we were in Sacramento talking with the state senators and the head of the Ways and Means Committee of the Assembly about budget and research problems. We spend a great deal of time on this, she particularly. Napa is about an hour and a half away from here and she goes up two or three times a week. She works tremendously hard at it, and very successfully. She gave a wonderful speech night before last for the volunteers--250 volunteers--who work at the hospital. And she does a wonderful job.

VDT: And important in this Administration--"kinder, gentler"--more voluntarism in this current era.

KIRK: "A thousand points of light."

VDT: Indeed; you're adding several. One last question. Cary Davis of the Population Reference Bureau said he once discussed beekeeping with you. Did you ever do that?

KIRK: Yes, I used to keep bees, in high school and college. Here, I had an observation hive in the bedroom. The bees were behind plexiglass and there was a little plastic tube through a hole in the wall with a little landing place outside and I could watch them inside the hive, in the bedroom!

VDT: Like in a nature center. They didn't fly around in your bedroom?

KIRK: Occasionally they did and Ruth was very disturbed about it. I once had a party for sociology and they came and looked at the bees.

You know, I'd forgotten about this. This is one of the main reasons why I got into demography; I was interested in bees and social insects. I kept ants. I'd dig up an anthill, capture the queen, these big red desert ants, and they would pick up larvae and I'd put them in a Mason jar and they'd dig down. Then I'd put a wooden collar around the top of that, put the whole thing in a field box buried in the ground, with sawdust around the jar. So when I wanted to look at them, I could push the sawdust aside and see their underground tunnels.

I was very much interested in social insects. My first ambition in life was to be an apiarist, a beekeeper. They paid \$3,000 a year for apiarists then.