

# **DEMOGRAPHIC DESTINIES**

## **Interviews with Presidents of the Population Association of America**

### **Interview with Charles B. Nam PAA President in 1979**



This series of interviews with Past PAA Presidents was initiated by Anders Lunde  
(PAA Historian, 1973 to 1982)

And continued by Jean van der Tak (PAA Historian, 1982 to 1994)

And then by John R. Weeks (PAA Historian, 1994 to present)

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## CHARLES B. NAM

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

**CAREER HIGHLIGHTS:** Charles Nam was born in Lynbrook, N.Y. in 1926. He received a B.A. in applied statistics in 1950 from New York University and the M.A. and Ph.D. in sociology in 1957 and 1959 from the University of North Carolina at Chapel Hill. He worked as a statistician with the Census Bureau from 1950 to 1953 and with the Manpower Research Branch of the Air Force in Montgomery, Alabama, in 1953-54. From 1957 to 1964, he was at the Census Bureau as Chief of the Education Statistics Sector and later of the Education and Social Stratification Branch of the Population Division. Since 1964, he has been at Florida State University in Tallahassee, where he has been Professor of Sociology, Chairman of the Department of Sociology (1967-81), and Director (1967-81) and then Research Associate of the Center for the Study of Population, which he founded. Among other activities, he has served on the Census Bureau's Population Advisory Committee (1978-81), as a consultant for UNESCO, the East-West Population Institute, and the Government of Indonesia, and is a frequent guest lecturer. He was editor of Demography from 1972 to 1975. His broad-ranging publications in education, social stratification, general demography, migration, mortality, and population socialization include the 1960 census monograph, Education of the American Population (with John Folger, 1967), Population and Society: A Textbook of Readings (1968), The Dynamics of Population Change (1976) and Population: A Basic Orientation (1984) with Susan Gustavus Philliber, The Socioeconomic Approach to Status Measurement (with Mary Powers, 1983), and Our Population: The Changing Face of America (1988).

**VDT:** What led to your interest in demography?

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

**VDT:** Indeed, in one generation!

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

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

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

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

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

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

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

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

**VDT:** Hands on!

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

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

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

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

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

**NAM:** I don't know what the motivation was, but I was one of about three of the original 20 in the Population Division that was still around. Stan Greene, who worked in labor force, was another. Yes,

Eisenhower was cutting back on the budget tremendously, so on top of the usual phaseout after the census, we had severe budget cuts.

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

But they shouldn't have done that; I didn't belong. And the question was how long I could remain there before they realized I wasn't a Russian specialist. I didn't have Russian language skills; I was just a plain old junior demographer. I stayed there a year or so, and then they discovered me and I was laid off finally. This was in 1953. I was actually unemployed for about two months, looking for work in Washington. It was difficult because every agency was laying off on account of the Reduction in Force and the budget cutback.

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

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

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

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

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

workers and I kind of trailed after them to see what they did.

**VDT:** Do you remember who some of them were?

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

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

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

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

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

**VDT:** What was your topic?

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

My prospectus was to take census data, mainly the 1950 data because the 1960 census hadn't been taken yet; there's very good information on socioeconomic characteristics of nationality groups by generation. I must have hand-calculated about 150 statistical tables and each one had nationality groups, looking at their educational and occupational distributions. Talk about multiple standardization as a technique in demography! My proposal was to control for the effects of age,

urban/rural residence, and sex, and I did multiple standardization, nine categories, on each distribution--several hundred of them. I knew I couldn't do it by hand. I needed a calculator and I couldn't afford a calculator. So I went down to the Marchant calculator company in Washington, D.C., said I was working in the government but was a graduate student as well, working on a dissertation: Could they let me rent a calculator at some reduced rate? Apparently they were impressed with my sincerity and let me have it at no charge for up to three months.

**VDT:** Wonderful story!

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

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

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

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

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

I first met him when I was at Maxwell Field, Montgomery, Alabama, but I had contact with his writings before then, because when I was working with Paul Glick and Henry Shryock, they kept a small library in the office at the Census Bureau, and one of the books I picked up and read was All These People [1945], which Vance had written about the population of the South--one of the first descriptive and analytical studies of the Southern population. I was fascinated because it was more than the humdrum census statistics; he put life into them. It was real "social demography," as we sometimes say today. I found it more attractive than the kind of census publications that come out. So when I met him down at Montgomery, Alabama, and he talked about coming to Chapel Hill, I already knew a lot about his writings, and I was eager.

He drove a car, despite the fact that he had non-functioning legs. He had a steering wheel with hand brakes and so forth until he got more feeble in later years. He was a great inspiration for a lot of people. He had a lot of students who went on and did very well in the sociological profession, several

in demography. Dan Price was one of his students; Margaret Hagood, another former PAA president, was a student of his.

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

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

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

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

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

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

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

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

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

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

**NAM:** There's a long story behind that too. It deals with Vance as a demographer and ecologist. About a year after Vance died [1975], I wrote Mrs. Vance and said, "How would you feel about my pulling together some of his published works, some articles, excerpts from some of his books, into a small volume?" I thought the University of North Carolina might be interested in it; he'd been there so long. She said, "I think it's a wonderful idea." However, a fellow named John Reed, then in the history department at Chapel Hill and later a sociologist at Chapel Hill, just prior to Vance's death had

talked with him about doing a volume on him, because Vance was not only a demographer, he was one of the last of the Renaissance people: he wrote for history, for economics, he wrote in the humanities. He was a great admirer and personal friend of H.L. Mencken; he wrote some articles for Mencken's magazine out of Baltimore. And he did a lot on the South as a region. Reed and Daniel Singal wanted to put together particularly Vance's works on the South. When they started, they thought about including demography but there was just too much stuff, so they talked to the University of North Carolina Press, which was going to publish this work, and they said, "Why don't you just drop out the demography." They kept in one article, on Virginia's population, but it was more a regional than a population concern. They eventually published it as a sort of tribute to him and his work [Regionalism in the South: Selected Papers of Rupert Vance, 1982]. It didn't include any population.

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

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

**VDT:** I hope we'll get that for the PAA archives file on Vance [Nam duly sent a reprint of the article]. You must have taken his influence tremendously to heart, because your interests and publications in demography have covered such a range. I want to get to that, why you have interests in so many facets of demography. But let's go back to your second time round at the Census Bureau, working on the 1960 census. You ended up doing a 1960 census monograph.

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

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

**NAM:** When Paul Glick asked me to come back to the Census in 1957, the opening he had in mind was for somebody to head up the educational statistics work. So I didn't get into very basic demographic material; I was working with social statistics, first education statistics, then into other things, but all in the socioeconomic sphere. When they set up the census monograph series for 1960, actually they first asked John Folger, who had been doing work with the Southern Regional Education Board and other educational organizations and had been trained in demography, if he would do the education monograph. He agreed on condition that I would be a coauthor. We worked it out that we would each do half of the book; we divided up the chapters. This is a curious thing, but at that time he had left the Southern Regional Education Board in Atlanta and was dean of the graduate school at Florida State University. I had no idea I was ever going to go there. He was in Tallahassee and I was in Washington; every once in a while he'd come up and we'd talk about our progress. Henry Shryock and Paul Glick were very good to relieve me of some of my duties to help me do my chapters. There were several people working on monographs, including Irene and Con Taeuber, who were doing the People of the United States in the 20th Century volume [1971]. Irene, who was then editor of

Population Index and used to shuttle between Washington and Princeton, was given an office in the Census Bureau.

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

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

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

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

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

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

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

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

**VDT:** Fair enough! I still think you did a tremendous job with the two of you in different places. It seems to me it's taking everybody a lot longer getting out the 1980 census monographs. The Suzanne Bianchi-Daphne Spain book on women [American Women in Transition] was the first out, and that was only in 1986. Jay Siegel just handed in his first manuscript for his aging monograph. You people must have worked faster.

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

**VDT:** Was that the first time there were monographs?

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

**VDT:** Who inspired those monographs--the Taeubers?

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

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

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

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

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

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

We started from scratch. There were some resources. Those were the days when money, federal money in particular, was still plentiful and you could do something, in contrast to today. This was the fall of 1964 when I got there.

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

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

Mary Powers had been an assistant of mine at the Census Bureau and just before her, Ed Stockwell had been my assistant. There were a lot of people who are well established demographers

today who came through that Population Division at the Census Bureau: Harry Rosenberg, Bob Parke, John Beresford, David Heer, Patience Lauriat. We all worked together under Paul Glick. There's an old photograph of us and we're all junior people [photo of Social Statistics Branch, 1960, donated to PAA archives by Paul Glick and displayed at the 1991 60th anniversary meeting in Washington, D.C.]. It's amazing how many of them really stuck in the profession.

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

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

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

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

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

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

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

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

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

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

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

**VDT:** It was in Princeton.

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

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

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

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

geography and other things. And it's got something important to say.

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

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

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

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

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

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

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

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

**VDT:** Do you have a copy?

**NAM:** No.

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

**NAM:** We've been good friends over the years. There's an interesting thing about the Census Bureau, which is one of the reasons I didn't stay. People talk about the "1940 cohort" of Census employees: Glick and Shryock, Henry Sheldon, Dave Kaplan, and so forth. The Census Bureau wasn't much of an agency and the 1940 census was the first one where they really built up a professional staff. So all the young recent Ph.D.s who were coming out trained in the social sciences were recruited to the Census

Bureau at the same time. By the time I got there, they were mostly 15 to 20 years older than I was. They weren't anywhere near retirement yet and they were occupying all the key positions. Where was I to go after I got . . .

**VDT:** Sounds a bit like the baby boomers.

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

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

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

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

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

**NAM:** I'd like to add something when you talk about the breadth of my interests. I took a real turn in my major interests about a dozen years ago, and you noticed that I've been doing a lot of mortality research in recent years. I think that bears some explanation. Again, there's a long history about it. I always had an interest in mortality analysis, but never had a chance to practice it. Working at the Census Bureau, you wouldn't think I had an opportunity to do anything in mortality. But when I returned there in 1957, I met Lillian Guralnick, then with the National Office of Vital Statistics, the

predecessor to the National Center for Health Statistics. She had been doing occupational mortality analysis and published a number of things with Iwao Moriyama. She said, "We need to get better data on occupational mortality, because the occupational reports on the death certificates are terrible." So we started talking and came up with an idea; I don't know whose idea it was but we worked together. That was to take death certificates and match them to the census records of the same people. You find from the death certificates all the people who died, let's say, in the four months following the 1960 census, who would have been enumerated in the 1960 census, though you don't know they're going to die, and you go back and find their records in the 1960 census. In effect what you're doing is identifying a subgroup of people enumerated in the 1960 census who were about to die. What that enables you to do is to take all the information on their death certificates and link it to their whole census record, not only their personal record but the family record, the area they lived in, and it would be a tremendous storehouse of knowledge. Not only would you have good occupational data from the census, you'd have everything else from the census.

**VDT:** And you were allowed to do that?

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

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

So Lillian contacted Phil Hauser and Evelyn Kitagawa at the University of Chicago and that was the beginning of the famous Kitagawa-Hauser study of social and economic differentials in mortality, which was a monograph in the American Public Health series of Harvard University Press [Differential Mortality in the United States: A Study in Epidemiology, 1973]. Lillian and I were the ones who conceived the study, but Chicago sent it in as a research proposal. It got the money from NIH. Evelyn, as project director, got Lillian and me involved. Lillian handled all the death certificates at the Vital Statistics end; I handled the Census work, including the matching of records at the Census end, because it had to be done confidentially. We created the blended records and then turned it all over on computer tapes, without identification, to Evelyn, who did the monograph and worked long and hard on that study. There was another case, like the Coleman study, where I had a substantial input, and Lillian Guralnick, but we appear as a footnote, or an acknowledgement at the front of the book.

**VDT:** But you really inspired the whole thing!

**NAM:** Yes. Of course, we got that Milbank article in 1959, which is evidence that we pretested the whole idea, but it was on occupational mortality initially. So I got my interest whetted in mortality analysis, but didn't have an opportunity to follow up at the Census Bureau, because they have no mortality data, except for the matching study. When I got to Florida State, I was so involved in building up the program that I didn't do much in the mortality area. I always had it in mind. Then

along the way I found a niche and for the last ten to 15 years I've been trying to make that my concentration. You can see from my publications list, I've done quite a lot in mortality, and I feel I'm just really getting going.

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

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

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

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

**VDT:** Population education, isn't it?

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

Population education, which is trying to teach people about the causes and consequences of population, is one component within the broader context. It relates to my publishing of textbooks, but it also goes back to the fact that when I really learned how to write, I felt that something the field needed was to communicate better, that demographers needed to be speaking to the public. There were very few people in the profession who have done that. Kingsley Davis was sort of the model for it. He was berated by many of his colleagues in the profession for writing articles, even in The New York Times. He wrote one on population and they said, "That's not a professional journal; you're belittling yourself." People had a very purist view of demography. My notion was that he was entirely right. I had some long talks with Kingsley on that. And I said, "I'm going to try to popularize demography as much as I can."

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

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

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

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

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

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

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

**NAM:** I don't feel that way myself. I was talking with one of my sociological colleagues a few weeks ago, who'd come from Johns Hopkins so I think very highly of him, and he said something like that to me. I know I've done a number of things. In terms of my own personal standards, I don't feel I've really achieved the real accomplishments I would have liked to achieve. There aren't any great ideas associated with my name.

**VDT:** Would you have liked that?

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

**VDT:** In the U.S.?

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

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

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

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

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

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

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

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

**VDT:** People going into state and local government?

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

**VDT:** And also in business?

**NAM:** One of our graduates, Louis Pol, now at the University of Nebraska [later Rollins College, Florida], wrote the first textbook on business demography [[Business Demography: A Guide for Business Planners and Marketers](#), 1987]. We're very proud of him. We have three people who are

now in schools of business: in Tennessee, Pol in Nebraska, and one other. They are in schools of business, but they're seen as demographers. We were doing it long before it was fashionable.

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

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

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

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

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

**NAM:** I hope it works out.

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

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

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

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

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

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

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

**VDT:** You said you took courses with him.

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

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

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

**VDT:** No, it was you!

**NAM:** Yeah, but that's not the whole story. Dorothy Swaine Thomas signed in at the Inn, I think, as just "D.S. Thomas," and since the Inn was small and even though there weren't a lot of us, they encouraged people to double up. If you'd arranged in advance to share a room with somebody that was fine, but if not, they paired people off. So they paired Kantner off with D.S. Thomas! When he got up to the room and found out what was going on, it wasn't quite right. That's how I became Kantner's roommate. They assigned her to somebody else's room and when I came in, they assigned me to the room with Jack Kantner. [Kantner later told Nam that he also recalled that "D.S. Thomas" was first assigned to the room of a male PAA member, but it was not his.] Kantner and I didn't know each other before that time. He was right out of Michigan, being a protege of Amos Hawley. We met, became good friends, and a year or so later, I got to work with him at the Census Bureau in International Population.

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

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

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

**VDT:** You were typing when you wrote?

**NAM:** I've usually written everything in longhand.

**VDT:** Do you use a word processor now?

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

**VDT:** Tables and all?

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

**VDT:** What about Phil Hauser?

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

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

**VDT:** Did you ever know Dudley Duncan?

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

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

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

At the same time, I was trying to convince Con Taeuber and others at the Census Bureau that

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

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

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

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

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

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

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

**VDT:** Intermediate variables.

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

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

**NAM:** It's in there. I have a lot of Kingsley Davis in there. I was very much influenced by Kingsley's work; got to know him better in later years. He was arguing the social structural point of view and intermediate variables. Frank Lorimer had gotten very involved in the more anthropological approach to fertility, spending a lot of time in Africa and focusing on cultural factors of fertility. So when there was a fertility session, inevitably it was Kingsley Davis doing the social structural approach and Frank Lorimer arguing for the cultural approach to understanding fertility. Today as you look back, they were probably closer together than they seemed to be at the time. But everybody looked forward to the session on fertility, which there always was, and knowing that at some point the two of them were going to get up and start arguing.

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

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

**NAM:** That's right. So my fondest memories are back in those earlier days when the program wasn't so elaborate, but more importantly, we had these open debates. People enjoyed them greatly.

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

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

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

**VDT:** Longer life expectancy of older blacks?

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

Ansley Coale and a lot of his students, proteges, argue that it's an artifact of bad data at the older ages, which is differential for blacks and whites, and there isn't a crossover. Russell Thornton and I wrote an article on that in the late 1960s ["The Lower Mortality Rates of Nonwhites at the Older

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

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

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

**NAM:** Yes, I think I might.

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

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

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

**NAM:** You get some of that.

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

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

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

**NAM:** That's right.

**VDT:** And they also should be in smaller rooms.

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

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

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

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

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

**VDT:** He was president in 1972-73.

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

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

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

**VDT:** Name one of your innovations.

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

**VDT:** In Population and Development Review.

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

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

**NAM:** I missed that.

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

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

**VDT:** You made a conscious effort?

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

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

**NAM:** That was a cohort effect.

**VDT:** What do you mean by that?

**NAM:** In the early group of PAA people, there were a fair number of women compared to most social science organizations, and they were established people and moved right into officer slots. But

somehow women didn't get recruited into the graduate training programs for a long while.

**VDT:** Were they all home raising the baby boom?

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

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

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

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

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

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

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

**VDT:** Who's that?

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

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

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

**VDT:** Concerned Demographers?

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

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

**NAM:** I think in all fairness, of course, there's been discrimination about women to some extent in PAA, as in other organizations. But I think PAA's record is quite good compared to most organizations. If you look at the relative numbers of men and women in the Association relative to the number of offices filled by women, I think we have a much higher percentage than most organizations. But we have one of the worst records of any association on blacks, and part of it is because of the nature of the subject matter and the fact that we're a very quantitative social science.

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

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

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

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

**VDT:** But you said "more applied."

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

**VDT:** Julius Wilson?

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

recruit blacks to demography, particularly as demography got more and more quantitative, more and more methods-oriented.

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

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

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

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

**VDT:** That has been an issue through the years. They talk about the few times there were blacks and PAA met in the South and there were problems with having blacks stay in the same hotel as white members. It happened rarely, because there simply weren't any black members.

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

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

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

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

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

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

Alumni Party.

## CONTINUED

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

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

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

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

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

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

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

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

**VDT:** But we didn't get a picture!

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

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

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

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

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

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

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

**VDT:** That was a memorable occasion.

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

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

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

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

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

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

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

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

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

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

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

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

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

## THE PROGRESS OF DEMOGRAPHY AS A SCIENTIFIC DISCIPLINE\*

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### INTRODUCTION

Among the fields of intellectual endeavor to which students and professionals turn their attention, there are those which are firmly grounded in universities, government, and in other settings of pure and applied learning, and there are also those which are tenuously established in the scholarly world. Despite its considerable development as a specific subject of accumulated knowledge, demography falls into the latter category. It has, at best, probationary status as an independent sphere of learning and its practitioners are usually regarded as marginal in their adopted fields. It is significant that in academia demographers are ordinarily labeled sociologists or economists. They are regarded as statisticians or social science analysts in governmental institutions and as planners or management experts in business and industry.

This uncertain place of demography is plainly indicated by its treatment in the most recent edition of *The Encyclopaedia Britannica*, the fifteenth. This includes a substantial review of the branches of knowledge. In his introduction, Mortimer Adler arrives at a configuration of five divisions of knowledge: logic, mathematics, science, history and the humanities, and philosophy. Sections under the rubric of science are history and philosophy of science, the physical sciences, the earth sciences, the biological sciences, medicine and affiliated disciplines, the social sciences and psychology, and the tech-

nological sciences (Propaedia, pp. 692-694).

As we would expect, the concerns of demography and population are reflected in a number of these categories. However, references to the subject matter of demography are often incidental to the main topics—demography is ignored as a field of knowledge in its own right.

Within the division of mathematics, we find such demographically relevant areas as mathematical modeling, calculatory aids, analogue and digital computation, numerical analysis, and various aspects of statistics, including statistical inference, estimation, hypothesis testing, and structural analysis. These methodological tools are presented as having wide applicability, but no specific mention is made of their relationship to demographic study (Propaedia, pp. 719-721).

Population is, however, acknowledged as a component of the biological sciences. The development of cell theory, the rise of embryology, and the emergence of genetics (all during the nineteenth century) are cited as significant bases for the establishment of specialties such as ecology, eugenics, and population biology; they are shown to have facilitated profound discoveries in reproductive biology, forming the basis for the comprehension of fecundity and for the development of new contraceptive technology. They also provided the base for medical advances which had effects on both mortality and fertility behavior (Propaedia, pp. 738-741).

Developments in medicine and affiliated disciplines have been of far-reaching importance in their link with demogra-

\*Presidential Address presented at the annual meeting of the Population Association of America, Philadelphia, Pennsylvania, April 27, 1979.

phy. Discoveries concerning blood circulation; the beginning of sub-specialties in surgery, obstetrics and gynecology, and pathology; various improvements in the treatment of disease; advances in physiology; and the emergence of bacteriology are cited as nineteenth century accomplishments. Chemotherapy and immunology were advanced in the twentieth century, along with new understanding of nutrition, tropical medicine, and surgical techniques. Public health programs became a reality and special concerns about the medical condition of infants and the elderly were emphasized. These areas have all been germane to population thought, if not central to it (Propaedia, pp. 741-745).

The authors of the *Encyclopaedia* seemed to have difficulty outlining the social sciences and psychology—this is reflected in their treatment of demography. Social science is acknowledged to have been a dynamic area whose disciplines have significantly changed their character over time. Sweeping social changes; philosophical influences of Marx, Freud, and others; the introduction of empiricism and quantitative methods; and newer theoretical currents created specializations and cross-disciplinary approaches. Along with economics, political science, anthropology, sociology, and social geography, social statistics is identified as a separate discipline emerging in the nineteenth century. However, demography per se is not identified as a separate discipline or as a growing field, and no mention is made of population in the outlines of anthropology, economics, political science, or psychology (Propaedia, pp. 746-751).

Demography, referred to as "the study of the structure of human populations and population movements," receives its only citation under sociology, as a cognate discipline along with criminology, penology, social psychology, and human geography. What does it mean to be a cognate discipline of sociology and not of other disciplines? Extensive articles in the *Encyclopaedia* on "Population" and on

"Demography" provide no particular justification (Vol. 14, pp. 814-824; Vol. 5, pp. 575-577). The first of these dwells on the history of world population change and on theories of population, with more attention to biological, economic, and political factors than to sociological ones. The article on "Demography" is confined to a brief discussion of censuses, vital statistics, life tables, and a few other quantitative techniques. The *Encyclopaedia*, in short, gives nothing more than a suggestion of what this cognate discipline is like as a comprehensive area of study.

More specific attempts to define the discipline have certainly been made. The treatment of "Population: The Field of Demography" in the 1968 edition of the *International Encyclopedia of the Social Sciences* offers a view of demography as "the quantitative study of human populations" (Kirk, 1968). It is acknowledged that a broader use of the term involves causal analysis with deep roots in sociology, and the incorporation of a number of social, economic, and biological variables; however, only those aspects of the study which are concerned with "measurement and quantitative uniformities" are regarded as truly demographic and other aspects are relegated to other disciplines.

The most comprehensive appraisal of the field is found in Hauser and Duncan's *The Study of Population* (1959), now celebrating its twentieth anniversary. Drawing on some 28 contributed articles from scholars in demography or allied disciplines, and on their own well-informed perceptions of the subject, the editors take stock of demography as a science and present their assessment of its status. Defining demography as "the study of the size, territorial distribution, and composition of population, changes therein, and the components of such changes, which may be identified as natality, mortality, territorial movement, and social mobility," they distinguish between "demographic analysis" and "population studies," the former concerned with the study of components of population variation

and change and the latter with not only population variables but also relationships between population change and other variables. Demography, they concede, may be conceived narrowly as demographic analysis or broadly as including both demographic analysis and population studies. However, demographic analysis may be regarded as the core of demography as a science. Proceeding from these premises, demography is evaluated as embodying "all the essential elements of scientific outlook and method" (Hauser and Duncan, 1959, p. 21).

A definition of the scope and content of demography is certainly crucial to a proper assessment of its accomplishments as a science. The Hauser-Duncan formulation has been widely accepted by students of the subject, and *The Study of Population* has probably done more to advance the field than any other single document. At the same time, one might argue that the distinction between demographic analysis and population studies, while an attempt to give structure to the discipline, has unwittingly contributed to the professional schizophrenia and marginality which has characterized demography in recent years.

What makes demography a less unified discipline than other disciplines? Is it its failure to be recognized as meriting departmental status in universities? This occurs mainly in the United States, certainly not in most parts of the world, and would appear to be a consequence of the late emergence of the specialty in academia and of the rigidity of traditional higher education. Is it a lack of developed data, methods, and theory? Hardly, since its data and methods are heavily borrowed by others and its theoretical frameworks are expanding. Is it perhaps that demography is, nevertheless, highly dependent on other disciplines for its concepts and variables? What other scientific discipline is not so dependent? Sociology relies on anthropology and psychology for such basic notions as culture, social organiza-

tion, personality, and socialization. Economics turns to mathematics, physics, and political science. Psychology looks to biology and the social sciences. No scientific discipline is isolated from the rest. The interdependence of concepts and methods among the many fields of study is a matter of record.

What, then, identifies a unique discipline? I maintain that it is the existence of a substantial and critical subject matter which plays a crucial role in understanding universal phenomena. The established sciences have long been seen to have this centrality of purpose; the newer sciences have had to justify their existence in that respect. Advances in the social sciences have accompanied the growth and increasing complexity of societies and the need to understand them. Scholarly disciplines can thus be viewed as functional intellectual necessities in a changing world. The older disciplines have not outgrown their usefulness, although many have adapted to modified circumstances. The birth and maturity of other disciplines depends on their utility for the generation of knowledge.

That so many disciplines have now incorporated concerns with population is a response to the growing importance of population structure and change in human societies. Has not the scope and intensity of concern with population reached that point when its status as a unique discipline is clearly justified? If so, what is that unique character?

#### THE SCOPE AND CONTENT OF DEMOGRAPHY

Just as the substantial and critical subject matter of biology is the structure and function of various units of life as they interact with the broader environments of which they are a part, just as the central concern of economics is the allocation of resources in the context of supply and demand and extraneous physical and social conditions, just as the essential subject matter of sociology is the way in which various social forms (groups, institutions,

and aggregates) influence social behavior and are in turn influenced by it, so the substantial and critical subject matter of demography is variations in the size and structure of human populations and how these variables interact with other aspects of societies.

Demography is thus concerned with not only size, but the composition, distribution, and characteristics of populations, and how these change. It is concerned with the internal dynamics of population as well as its external dynamics. The former leads us to study the basic components of demographic change (mortality, natality, and residential mobility) and how they shape population composition, distribution, and characteristics. The latter orients us to the broader societal determinants and consequences of the internal dynamics of population. It is clear that no other discipline views the workings of the population-societal linkages in a systematic manner, although several disciplines incorporate the study of particular population linkages as they are related to the central concerns of those disciplines. It is this integral view of population-related phenomena which establishes demography as a unique discipline.

This functional definition of demography renders certain distinctions made in the past—demographic analysis vs. population studies and formal demography vs. social demography—arbitrary and possibly inhibitive of theoretical and methodological advancements. Such terminological classification may have restricted some of us in formulating study designs, in the collection of data, and in alerting ourselves for serendipitous discoveries. The fact that demography has assumed a broader and integral definition in recent years can account, in great part, for its substantial progress.

Within demography, some of us may choose to focus on certain aspects of its internal dynamics while others dwell on certain external ones. Yet, we do not remain impervious to the work of each

other. For example, every student of age composition is aware of the societal forces that produce variations in the components of population change even if quantitative measures of those components will suffice to estimate an age structure. And those same students are sensitive to the societal implications of varying age composition. Awareness of these related interests may very well alter our assumptions about the population dynamics we investigate and direct us to better formulations of our research problems. The relatedness of determinants, structure, and consequences of population phenomena is an essential aspect of demographic understanding.

Similarly, the view of demography espoused here requires that we search for variables and approaches that will help the explanation and prediction of population phenomena, regardless of the intellectual domain in which they are usually treated. Given our central concern, it is unimportant whether our concepts have biological, psychological, anthropological, sociological, or other roots, or are indigenous creations. Likewise, if a method advances knowledge, be it a favorite technique of another discipline or our own invention, it should be welcomed. When we say that demography is becoming more multidisciplinary or interdisciplinary, we are recognizing that concepts and methods are, in fact, being borrowed and integrated.

It is also necessary, so long as our orientation is to the broader but integral concern, that we not limit our interests to a particular unit of analysis. We can direct our attention to temporal changes in population aggregates independently of the individuals who make up the aggregates, or to a subset of particular individuals as they move through the life cycle (Ryder, 1964). Even if we assume that our central focus is on aggregate phenomena, at times smaller aggregates can best be understood as groups with interacting members, and certainly what attention we can give to other units, such as the family, the couple, and the individual, will en-

hance our comprehension of aggregate population dynamics.

In addition, the scope of our discipline permits contributions to be made at various levels and in different forms. Some may stop at a description of structures or processes, others may be preoccupied with causal analysis, and others may dwell on prescriptive or policy formulations. All are legitimate dimensions of demography, provided that they are based on the universal canons of scientific investigation.

#### PROGRESS OF THE DISCIPLINE

Within this framework, what progress has demography made as a scientific discipline? What gains in data, methods, and theory can we point to? What might have limited our progress—can we overcome those barriers?

A cursory examination of population research in recent decades shows that demography has been securing its past accomplishments while, at the same time, questioning its former achievements and making new contributions to knowledge.

In the realm of data, the field continues to be the envy of others. The census, mainstay of demographic investigation, has been introduced in a number of areas of the world which have previously lacked population information. Improvements in census enumerations have taken place in other countries. We are rapidly moving toward that point in time when censuses, with all of their limitations, will provide at least rudimentary knowledge about the size and structure of virtually all of the earth's population. Because censuses are taken relatively infrequently and cannot cover very much subject matter, we are highly dependent on other data sources for interim statistics and detailed facts about population dynamics. Registration of marriages, births, and deaths have been crucial in developed nations. Their evolution in developing countries has been slow and difficult and, although continuing progress is being made, conventional vital registration cannot be de-

pended upon for essential measurement of demographic change. Population registers and various other record systems have occasionally been useful, but they have not added substantially or universally to the data core.

The sample survey has recently emerged as the demographer's most important data-collection tool. Its flexibility and relative economy have permitted frequent assessments of fertility patterns in the developed world and now provides the basis for fertility measurement in less-developed regions as well. It also promises to serve as a tool for learning more about the dynamics of mortality change, residential mobility, as well as other population processes. When linked with other data sources, the survey provides for indirect calculation of vital events as well as extension of knowledge about persons covered in censuses and registrations. Considering the gradual progress in more basic data approaches, special note should be made of ingenious developments in estimation of population statistics given limited and/or defective information, in which methodologists have capitalized on the observed regularities of certain demographic phenomena and on the means of logic and mathematical and statistical techniques. Our data base, to be concise, has never been stronger and can only improve.

It has sometimes been argued that the store of data available to demographers is inadequately utilized and that the methodological imagination of data users lags seriously behind the development of statistics. It must be realized, first, that most demographic information serves a broad community or societal purpose, and that much use of that information is of a simple descriptive nature because it is employed to meet governmental or business administrative objectives. Moreover, the types of demographic data available for a long time did not inspire methodological innovation, and the largest segment of data users had few theoretical interests. Even professional demographers were apt

to be content with reporting bare facts, eschewing interpretation of the information. Methodological sophistication has progressed along with improvements in both data and theory. The quest for causal understanding as a basis for policies and action programs gave rise to new analytical techniques and to the borrowing of methodology from other disciplines. We have not only incorporated sound techniques for evaluating, adjusting, and, if needed, estimating data (many such techniques having been originated by demographic specialists), but we have also advanced in our methods for analyzing data. It has been possible to decipher fertility trends by partitioning birth patterns into cohort and period effects, and into temporal, structural, compositional, and pure dimensions through standardization and other statistical control of basic rates and ratios. Extended application of conventional life table techniques to various biological, social, and economic data has enlightened us with regard to the potential effects on mortality levels of eliminating certain causes of death, to aspects of fecundability and contraceptive efficacy, and to the lifetime probabilities of various marriage and divorce conditions, labor force activities, and other social processes. Most importantly, with the greater availability of elaborate survey information, linked record data, and public access to census and vital statistics files for individuals, and with rapid developments in computer processing and retrieval of large data sets, it has become more feasible to use a variety of multivariate analytical techniques to measure associations between variables and to proceed toward causal interpretation of demographic phenomena (Winsborough, 1978). As a result, our conceptual models can be specified and tested more rigorously. In short, our arsenal of research methods, some devised, others borrowed, has become a strength of the field.

One might argue that the mark of a mature discipline is the wholesomeness of its theoretical formulations. Can they in-

tegrate our existing knowledge and direct us to new areas of research? It has often been said of demography that it is data rich and theory poor. Vance (1952), addressing this association 27 years ago, asserted that "There is great need for the development of integrated theory of a high order to serve as a 'binder' for demography's diverse and particularized findings." We have not been without our theoreticians. In the post-World War II era, we paid homage to the masters—Khalidun, Malthus, Quetelet, Marx, Gini, Dumont, and others—but found them to be either too abstract and lacking in specifics or outmoded. The bright theoretical light of the postwar period was transition theory and most demographers were content to invest their intellectual capital in that framework. We have since subjected transition theory to critical investigation and, while the animal is still alive, we are today not certain of its species or its life expectancy. But we are attempting to picture its appearance and flesh it out. More importantly, we have pursued other lines of inquiry and have arrived at a variety of conceptualizations which offer promise of meeting theoretical needs of the discipline. Followers of Lotka have greatly elaborated stable population analysis so that our theory of internal population dynamics rivals theories of the physical sciences. With regard to external population dynamics, we have had numerous theoretical proposals which hold high hope for explanation of demographic phenomena and for eventual integration into a broader population theory. These include multiphasic response theory, the relative income hypothesis, the intermediate variables framework, ecological perspectives, and various micro theories of decision making. Application to our data of existing frameworks from psychology, economics, sociology, anthropology, and other disciplines has enriched our theoretical thinking. The issues of demographic journals today are replete with theoretically based articles, in stark contrast to those of the past. We no longer fall be-

hind our fellow disciplines in theoretical development, and a merging of lower-order propositions into a theoretical whole is now as conceivable in demography as in any of the social sciences.

#### THE FUTURE PROGRESS OF DEMOGRAPHY

In order to build upon these accomplishments, we will not only have to extend our work but also bring about some needed new orientations and altered organizational forms. The following principles will need to be accepted if substantial gains in the progress of demography as a scientific discipline are to be realized.

First, we must recognize a distinction between demography as a discipline and demography as a profession. In order to advance the former, it may be helpful but not necessary that we show progress in the latter. The discipline has grown more in recent years than its professional development would suggest. It is the interest in population variables and their theoretical importance in understanding societal phenomena which has led to demography's stature as a discipline. Whether there are academic departments of demography, titles of demographer, or new or larger professional associations concerned with population will not determine the viability of the discipline as much as its functional importance in knowledge networks. It can be argued that the greatest contributions to knowledge in our time have emerged in evolving disciplines which intersect the traditional disciplines, such as molecular biophysics and linguistics, despite the fact that these newer disciplines have not acquired the status of the older ones. Admittedly, disciplinary efforts can be sustained effectively by strong professional activity and recognition, but we need not wait for acceptance of these in order to achieve results in our science.

Second, a comprehensive view of population in the context of society requires that we refrain from pointing to demography as a discipline which is primarily sta-

tistical. To be sure, the field relies heavily on quantitative measurement and analysis as do other disciplines which are concerned with quantitatively measurable phenomena. But, as in most disciplines that depend on statistical methods, the techniques of data collection, reduction, and analysis are means to a substantive end. It is within the realm of demography that population-related phenomena are described, population-related generalizations are formed and tested, and population-related programs and policies are devised, all with scientific rigor. Understanding and explaining population phenomena is what demography is all about.

Third, as Lorimer (1959, p. 165) has put it, "The concept of 'pure demography,' except as the skeleton of a science is . . . an illusion. Significant demography is necessarily interdisciplinary." This does not mean that demography must draw upon other disciplines for all of its concepts, theories, data, and analytical forms. On the contrary, we have been fortunate in having a wealth of original research discoveries and inventions by persons working only within a formal demographic framework. But the larger field has benefited greatly by the contributions both of those whose principal interests lay in other disciplines and of those who have been mainly concerned with demography but have reached out to incorporate knowledge from other disciplines. During the past decade, the involvement of scholars from other disciplines in concerns of demography has broadened considerably. Yet, the narrow view of demography which has often been portrayed in some disciplines has limited participation in demographic analysis. For example, a perusal of sociology textbooks and reference material will show that, more often than not, demography or population studies are treated primarily as descriptive data about societies or communities or as a set of methodological tools for sociology. Sociologists as a group have been overridingly unimaginative with regard to integrating population analysis within the

theoretical and analytical frameworks of sociology. Likewise, economists, psychologists, anthropologists, and those from other disciplines have been restrained in dealing with demographic topics. As demography is increasingly seen as the study of determinants, structure, and consequences of population, with all of its ramifications, we should expect more multidisciplinary and interdisciplinary attention to the field.

Fourth, every scholarly field is subject to being either facilitated or impeded by its organizational context. The objectives of demography require a more facilitating organizational context than it has had until recently. In neither national and international or private and governmental circles has demography received the attention it deserves. Population, more often than not, tends to be considered in crisis terms rather than as a critical element of a continually changing social scene. Where it does receive attention, it often is heeded in only a partial perspective. Frequently, greater note is made of some dimensions of the total population process than of others, the neglected areas sometimes being the more crucial ones. Program thrusts commonly evolve from the judgments of a few influential persons with particular focuses on population matters. As a consequence, the comprehensive approaches which are generated by the general wisdom of the field are ignored or paid lip service. There are, of course, some signs of greater responsiveness to the call for a broad demographic agenda. A number of governments have formed special commissions on population, increased research activity is taking place in universities and elsewhere, and more professional associations are recognizing the relatedness of population subject matter by establishing sections or divisions on the topic. However, many of these responses have been aborted, inadequately funded, or half heart-

edly supported, and there is a need for more varied and extensive organized efforts to deal with the several aspects of population developments. The Population Association of America, and other population-related organizations, must speak forthrightly in support of activities which will advance the discipline.

Nevertheless, a look back at where the field has come from and where it has gone cannot help but lead to the conclusion that demography's progress as a scientific discipline has indeed been enormous. Admired by our disciplinary associates, recognized by the communications media, and called on for advice by political strategists and administrators, it can be said that we have truly arrived professionally. And what we bring with us is a collection of data, analyses, and theories, centralizing on human population phenomena, which promise to help immeasurably in comprehending our complex world.

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