

# **DEMOGRAPHIC DESTINIES**

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

### **Interview with Otis Dudley Duncan PAA President in 1968-69**



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)

With the collaboration of the following members of the PAA History Committee:  
David Heer (2004 to 2007), Paul Demeny (2004 to 2012), Dennis Hodgson (2004 to  
present), Deborah McFarlane (2004 to 2018), Karen Hardee (2010 to present), Emily  
Merchant (2016 to present), and Win Brown (2018 to present)

## OTIS DUDLEY DUNCAN

PAA President in 1968-69 (No. 32). Interview with Jean van der Tak at Dr. Duncan's home in Goleta, California, May 3, 1989.

**CAREER HIGHLIGHTS:** Dudley Duncan was born in Texas in 1921 and grew up there and in Stillwater, Oklahoma, where his father, sociologist Otis Durant Duncan, was on the faculty of what became Oklahoma State University. He received all his degrees in sociology: the B.A. from Louisiana State University in 1941; M.A. from the University of Minnesota in 1942; and Ph.D. from the University of Chicago in 1949, following three years of service in the U.S. Army during World War II. He was Assistant Professor of Sociology at Pennsylvania State University in 1948-50, at the University of Wisconsin in 1950-51, and at the University of Chicago in 1951-56. At Chicago, where he remained until 1962, he was also Professor of Human Ecology and Associate Director of the Population Research and Training Center. He and his wife Beverly, whom he first met when she was his student at Penn State, were married in 1954. Beginning work together at the University of Chicago, they became one of the outstanding research teams in U.S. demography. From 1962 to 1973, they were at the University of Michigan, where Dudley Duncan was Professor of Sociology and Associate Director of the Population Studies Center. From 1973-83, he was Professor of Sociology at the University of Arizona in Tucson, and from 1984 to 1987 he was Professor of Sociology at the University of California, Santa Barbara. He retired in 1987 and was granted Professor Emeritus status there at UCSB. Beverly Duncan died in 1988. Dudley died from prostate cancer in Santa Barbara in 2004.

Among Dudley Duncan's many other posts relevant to demography, he has been chairman of the Committee on Social Indicators of the Social Science Research Council, served on the Census Bureau's Advisory Committee on Population Statistics, and was a member of the Commission on Population Growth and the American Future. His honors include election to the National Academy of Sciences (1973) and receipt of the Samuel A. Stouffer Award in Methodology from the American Sociological Association (1977) and of the Irene B. Taeuber Award for Excellence in Demographic Research from the Population Association of America (1991). His long list of influential publications include some 20 books, among which are The Negro Population of Chicago (with Beverly Duncan, 1957), The Study of Population (coedited with Philip Hauser, 1959), Metropolis and Region (with Beverly Duncan and others, 1960), Statistical Geography (with Ray P. Cuzzort and Beverly Duncan, 1961), The American Occupational Structure (with Peter Blau, 1967), Socioeconomic Background and Achievement (with David Featherman and Beverly Duncan, 1972), Introduction to Structural Equation Models (1975), and Notes on Social Measurement (1984).

**VDT:** How and when did you become interested in the general field of demography?

**DUNCAN:** I was first exposed to that in an undergraduate course at Louisiana State University, taught by T. Lynn Smith. He later wrote his material up in his book, Population.

**VDT:** Your father was a professor of sociology at Louisiana State, is that it?

**DUNCAN:** Well, the year I was a senior there [1940-41], he was there completing his Ph.D. He spent just one year in residence at Louisiana to get his doctorate. He had done most of the work at Minnesota and failed his prelims. Then he went back and passed his prelims but couldn't get a dissertation accepted, so he had kind of given up and Smith invited him to come to Louisiana and spend a year there and Louisiana gave him the degree. It happened to be my senior year at college, so

I went with him.

**VDT:** He'd had that long gap between his graduate work and finishing his doctorate?

**DUNCAN:** He had many gaps in his career. He was a country boy and had to walk four miles to go to high school. He got into college because World War I made it possible to finance his start at college. Then he was a school teacher for a number of years before he graduated from college. He finally got his master's degree at Texas A. & M., but his graduate work came to a crashing halt two years later when he failed to pass his doctorate prelims at Minnesota. He took a one-year job at Louisiana at that time and then moved to Oklahoma A. & M. College--later Oklahoma State University--which is where he stayed the rest of his career, and life.

**VDT:** So, you had taken a course with T. Lynn Smith?

**DUNCAN:** Yes. I went to Minnesota the next year, 1941-42. That was the fall that the Japanese bombed Pearl Harbor. I was then almost 20.

**VDT:** You finished your undergraduate degree very early.

**DUNCAN:** Yes, I finished at age 19. So, I was foreseeing being drafted and I had only the one year. In the spring, my Dad gave me some tables that he'd compiled down in Oklahoma on birth registration statistics. He had age at first birth and he'd classified the women according to where they lived, in urban areas, open country, or rural farm areas. He did that work with the Oklahoma vital registration people and then gave me the tables to use for a thesis. That was sort of a demographic thesis. It wasn't very good, but I published a little paper on it in Rural Sociology, the first paper I ever did. It came out after I was in service ["Rural-Urban Variations in the Age of Parents at the Birth of the First Child," Rural Sociology, March 1943].

**VDT:** How did you happen to choose to go to the University of Minnesota?

**DUNCAN:** I got an assistantship there and Bill [William H.] Sewell was a great influence on me. He was my father's colleague at Stillwater [Oklahoma], but my Dad had met him in 1935 when he was back at Minnesota to take his prelims and try to get started again on a degree there. He hired Bill for Stillwater in 1937. The Sewells were very close friends of the family. I did babysitting and housecleaning for them and Bill would talk to me about his work. I took my first course in sociology with him, a course on rural sociology.

Then in my senior year, I went off to Louisiana with Dad. He urged me to come back to Oklahoma A. & M. College, where he was head of the department of sociology, to do a master's degree, but Bill advised me that that would not be a good idea and I should look around for other opportunities. I'm sure he recommended me in a warm way to Minnesota, so I got a nice assistantship there. It paid \$600 and tuition; that was ample to live on for the year.

**VDT:** You managed to finish a thesis, get a paper out of it, do all the coursework, in two semesters?

**DUNCAN:** It was three quarters. I wrote the thesis mainly in June and July and had to go back in August and take one additional course and have my oral exam. I went into the service in October 1942 and was in the service until January 1946.

**VDT:** A.U.S., it says in Who's Who.

**DUNCAN:** Army of the United States. That's the official name of the World War II army to distinguish it from the regular army. I had various assignments, training programs and so on. I never went overseas or had any combat or significant military experience, to tell the truth. They passed the GI bill and after you came out you could go wherever you wanted to, so I went to Chicago.

**VDT:** Why did you choose Chicago?

**DUNCAN:** It's hard to say. I've written a bit about that in my autobiography [Autobiographical Statement, prepared for the National Academy of Sciences, January 1974, on being elected a member of NAS; with addendum, August 16, 1983]. I was interested in the sociology of knowledge [Louis Wirth] and various things. It wasn't particularly demography that I went there for. I took my major in social psychology and methods and theory in the Chicago Ph.D. program. After I'd finished my undergraduate degree and my master's with that exposure to demography, I still was not committed to demography at all. I had other interests, and still do. But I studied with W.F. Ogburn and found him the most congenial professor I had. So I ended up doing a dissertation with him. He had a project and was able to hire me for the year that I used to write my dissertation. That was on a topic designated for his project.

**VDT:** Was that An Examination of the Problem of Optimum City-Size, published as a book in 1949?

**DUNCAN:** It was actually published only in 1980 by Arno Press in one of those projects to reprint dissertations. It's just a photocopy of the typed dissertation.

**VDT:** Was William Ogburn's project, which allowed you to do your dissertation, connected with the Chicago Community Inventory?

**DUNCAN:** No, Ogburn had no connection with the Community Inventory; that was Louis Wirth and Phil Hauser. Ogburn's was Carnegie money for social effects of technology and he got interested in the possibility that if we dispersed cities they would be less vulnerable to atomic bombs--sort of a naive idea. He thought one angle of that was to see what would be lost if you broke up large cities, made them smaller somehow. So he asked me to tabulate characteristics of cities according to size. I looked up a lot of fugitive tables; I didn't do much original compilation myself. I just assembled all the material, made it into a dozen chapters or so, and called it a dissertation. It was a descriptive thesis, but it still has some interest today. Julian Simon wrote me recently, saying he's going to write on correlates of population density and he thought my material would still be useful to him. So that was a sort of demographic thesis, though not entirely demographic. But I still was not committed to demography. I did not have any firm intention of specializing in that.

When I got on the job market, I had various offers, but by far the best was from Penn State. They said, "What could you teach?" I listed several things and finally said, "Well, population." They said, "Oh, that's something we need," so I got designated to teach population. I began offering a standard undergraduate course in population problems and a graduate course in methods of population research. I was at Penn State for two years, 1948 to 1950, then I had one year at Wisconsin, 1950-51, and then I went back to Chicago on the faculty in the summer of 1951.

**VDT:** Was that where you always wanted to be?

**DUNCAN:** I did not have any such aspiration. I thought Penn State was a very fine job and would have been prepared to stay there happily the rest of my life. I had a part-time research job at the

Agricultural Experiment Station which I thought was very satisfactory. Bill Sewell had such a job at the beginning of his career. That was demographic research. I had an unpublished study on differential fertility and a small published article on fertility of the village population ["Fertility of the Village Population in Pennsylvania, 1940," Social Forces, March 1950], which was patterned after a paper that Lynn Smith had published, showing how you can use census tables to get child-woman ratios for the village population.

**VDT:** Down to that low a level--census tracts?

**DUNCAN:** No, it wasn't census tracts in those days. The census showed small incorporated places. You could get tables on age-sex distributions for very small places and classify them by size and their characteristics to infer fertility.

**VDT:** Why did you leave Penn State if you enjoyed it?

**DUNCAN:** Because Bill Sewell was at Wisconsin and he got a better job for me. A fine hand; he influenced most aspects of my career. He went off to the Navy during the war and when it was over, he had an offer from Wisconsin and never came back to Stillwater.

**VDT:** I certainly think of him as connected with Wisconsin.

**DUNCAN:** That's been his lifelong location. The only other job he had was at Stillwater. I was in Wisconsin for a year and W.F. Ogburn wrote and asked if I would like a job at Chicago. I was disinclined to take it. I thought Chicago was a very rough place and I didn't particularly want to go there.

**VDT:** The city?

**DUNCAN:** The city and the university. In terms of the competition in the faculty and the scholarship standards and so on, it scared me. But I talked to Bill Sewell and he said, "I wish you would stay here, but I think you should go for the sake of your career." So I went. And as Robert Frost said, "That made all the difference." [Laughter] I'm sure, though, you can't prove that.

**VDT:** You came to Chicago in 1951 and you and Beverly were married in 1954. Did your book with Beverly, The Negro Population of Chicago [1957], and Social Characteristics of Urban and Rural Communities, 1950 [with Albert J. Reiss, Jr., 1956] grow out of the Chicago Community Inventory, that became the Population Research and Training Center?

**DUNCAN:** Social Characteristics of Urban and Rural Communities, 1950 was a 1950 census monograph, sponsored by the Census Bureau and financed in part by the Russell Sage Foundation, I think.

**VDT:** You were young to be picked to write that.

**DUNCAN:** That was based partly on my doctoral dissertation. In 1950 they put out a special report on urbanized areas, classified by size. I guess they thought that since that was a new topic they wanted a monograph to report those data and I guess they knew about my dissertation and asked me to do that. I asked my friend and classmate Al Reiss to work with me on it. That had nothing to do with the Community Inventory, except that I did the work there and used their facilities. The Negro Population

of Chicago was a project done at the Community Inventory, which was established early in the postwar period by Wirth and then when Hauser came back he sort of took it over.

**VDT:** He was asked to come back and be the head of it [see Hauser interview].

**DUNCAN:** Yes. He was on the faculty as a professor, of course. When I went to Chicago, he invited me to work on a research project for which he'd gotten funds from the Air Force. That lasted two years and that was sort of what cast the die, which turned me into a human ecologist and then demographer, for a decade or so.

**VDT:** That project was working for the Human Resources Research Institute of the Air Force. Phil Hauser [in his interview] said you did the methodological work for that and helped develop segregation and concentration indexes.

**DUNCAN:** Beverly and I did practically all the work, if you want to know the truth of the matter. There were a few other things done that were okay, but we were the backbone of that project. Phil brought in the resources and oversaw the thing, but he was not active in the research itself. He never published anything on it, except as coauthor of a short summary report. We published some things on segregation indexes, a variety of articles that were related to that.

Then the Negro Population was a bit later. After the Air Force project was over, the Community Inventory didn't have any significant financing. So Phil went to the City of Chicago--I can't remember which agency--and a guy there whom he knew agreed to put up some money to keep the Community Inventory in business. We did little reports; that was the formula. Beverly did the bulk of that work. One of the things they were interested in was the changing distribution of the Negro population. We did one short report and then extended that into the complete book that was published.

**VDT:** Phil Hauser in the interview I had with him last October said that book was a trailblazer. It indicated that Chicago was probably the most segregated large city in the northern U.S.

**DUNCAN:** We did not make any comparisons of cities in that book. That was done later by Alma and Karl Taeuber, who did the comparative study of segregation indexes and so on. What we showed was not so much the degree of segregation as the consequences of segregation when the Negro population is increasing rapidly--what happened when the blacks flooded into the city but the residential areas did not expand to admit them. We showed that in many census tracts there was what we called "piling up." You had increasing population over the 1940-50 decade without any increase in the housing stock, just more people going into the same space. And that was in effect heightening segregation; the fact that you had intensification of the black occupation of their part of the city as compared to the rest of the city. We explored those processes in minute detail by comparing the 1950 and 1940 census-tract characteristics, tract by tract, using regression lines to describe the average changes in groups of census tracts according to the percentages of black population at the beginning and end of the decade. We worked out a typology of tracts that we called "piling-up tracts," "consolidation tracts," and "invasion tracts"--something like that.

**VDT:** Phil Hauser said Beverly had been his student and she was a superb student and researcher. He said, "She and Dudley were one of the best research teams that ever existed in the U.S. or, for that matter, elsewhere. They did beautiful work."

**DUNCAN:** Well, that's true. I'm not embarrassed to say that.

**VDT:** Great! Now about The Study of Population [1959], which I think of as the bible of demography, in which you described demography as a social science for the National Science Foundation. You and Hauser wrote the first five chapters on "Demography as a Science" and you wrote the chapter on human ecology. What do you think/remember about your part in it?

**DUNCAN:** You said most of it. Harry Alpert wanted a review of a discipline that would show that there is science in social science. He went to the National Science Foundation as a kind of consultant and was asked to look at the social sciences, which had been deliberately excluded from the National Science Foundation when it was established. Actually, I looked into that a bit and wrote an analysis of the way it worked; that was probably about 1947. Harry was there and despite the fact that the social sciences weren't supposed to be covered by the program, he was investigating the possibility of gradually introducing social science work. He decided to sponsor a survey that would show there was some branch of social science that was worthy of attention as scientific. He thought of demography and asked Phil and me to look into that. I don't remember the details of how the project got started. But he put up money which enabled us to give honoraria and pay for translations and other expenses of preparing a large collaborative book of that kind. So we thought of names and wrote to people and put the whole thing together. It turned out to be gargantuan and uneven in quality. Then Phil and I had to write some kind of introduction, so we wrote "Demography as a Science."

**VDT:** And you wrote the chapter on human ecology. By that time you were a human ecologist?

**DUNCAN:** Yes. Well, I started teaching that course; that was one of the teaching duties I took on about 1952. Amos Hawley's book had come out about that time [Human Ecology: A Theory of Community Structure, 1950]. It seemed to put a lot of things in perspective that I hadn't been able to assimilate before, so I was very enthusiastic. I worked very hard and developed a good course. And we thought of the work at the Community Inventory as work in human ecology, in the old Chicago tradition, the Park tradition, both in terms of methodology and statistical techniques and in terms of the idea of ecology itself.

**VDT:** The old Chicago tradition of . . . ?

**DUNCAN:** Robert E. Park--and Ernest W. Burgess, of course. They had talked about human ecology from the 1920s and that was thought of as one branch of the Chicago tradition.

**VDT:** Let's talk a bit about your colleagues of Chicago. You've talked about Hauser; you gave a few insights. Many people have said Hauser was so good at getting money and putting people and projects together--an entrepreneur, in a sense, of the field. Do you feel that?

**DUNCAN:** Well, he was very good at administration. He got other people to do the work. That was his philosophy of administration, he liked to say, "to give his assistant a headache." [Laughter]

**VDT:** He said he's had his name on 32 books, chapters in 50-60 other books, and over 500 articles. But I guess they were mostly collaborative efforts?

**DUNCAN:** Not all of them, but the things he did with us were collaborative. He himself never did a research project, in the focused sense of that term, at the Chicago Community Inventory or the Population Center. He did a lot of the kinds of volumes that he's famous for, conference proceedings and symposia, things of that kind. He was good at organizing those, recruiting talent, involving people, editing the stuff. He's a very fluent writer and speaker, of course. Brilliant guy. If he hadn't

been so good at that public relations stuff he could have been a great researcher. I don't think he's a great researcher; I think he's a very good synthesist and analyst, but largely using results worked out by other people. He'd started that as an official of the Census Bureau. He was in the demographic program [population division] for a while, but he quickly moved up the echelons where he was no longer directly responsible for the production work, but for administering and planning. He got into the fairly high-level policy advisory positions before he was done, working with Henry Wallace and so on.

He came back to Chicago while I was still a student, about 1947, and I sat in his course on population. That's something I didn't mention before. That was an influence at Chicago, though I still didn't see it as a specialty. I thought since he was new on the scene and I was tired of those other guys, I would take a course with him. It was not a great course. He used his old course notes that were heavy on the history of population controversies and doctrines and spent about ten weeks on Malthus, Marx, and Myrdal, all that stuff, and then about the last two weeks, we started learning about net reproduction rates, techniques, and so on, which was the part I was interested in.

He was a dynamic, very charismatic fellow. He was attracted to me when I was a student and he was very supportive and once I was back on the faculty, he turned into a great friend. The Hausers were very close friends of the Duncans for that entire period. We were in their house not less than once a week. We enjoyed them so much.

**VDT:** Who were other close colleagues at Chicago?

**DUNCAN:** At the [Population Research and Training] Center, Evelyn [Kitagawa] was the other mainstay, along with Beverly. The two of them did not have faculty positions. Phil and I were teaching, so we were only part-time at the Center, but Evelyn and Beverly were there full-time. And Don Bogue, of course, joined us as a member of the faculty.

**VDT:** Tell me about Evelyn; she is one I have yet to interview.

**DUNCAN:** She's a very competent, level-headed person, a very good friend. Her husband is a minister and he married Beverly and me and Evelyn had a reception for us after the wedding, so that shows you that they were very good friends.

She had worked in the War Relocation Authority during the war and then come to Chicago and done her doctoral dissertation. She finished a bit after I did, so I didn't know her as a student. But when I came back on the faculty three years later, she was involved in the Community Inventory and working full-time. She worked at that time on the labor mobility study and Al Reiss was working with her on that. I guess that kept up for three years and then she went off into other kinds of study. We never collaborated directly; we were just associates in the running of the Center and so on. We saw each other twice a day at coffee breaks. I have enormous respect and affection for Evelyn. She was really one of the very few friends that Beverly ever had. Beverly didn't make friends very promiscuously, and Evelyn was one.

**VDT:** You were on the dissertation committee of Nathan Keyfitz. He had been at Chicago as a student only briefly and came back in 1952 to defend his thesis. He didn't come to the faculty till 1963, when you had gone to Michigan.

**DUNCAN:** That was the only time I intersected with him, except on visits. We were never in the same location as coworkers.

**VDT:** Repeat that nice story you told about his dissertation defense, when he was so much older than

most students.

**DUNCAN:** I don't recall whether I was on his dissertation committee, but I was part of the examining committee. They pressed people into service for the oral exam. I'm sure I must have read some of the dissertation--and the abstract, of course--and I was perplexed by the methods, which were a bit fancier on the side of design and analysis than I had learned. I'm sure I struggled to ask questions that would not be too stupid, but I was really adrift. After the examination, Everett Hughes was telling somebody how it had gone and he said, "Nathan was very kind; he didn't flunk the committee."

**VDT:** Somebody else has told me that the students were all saying, "Who was examining whom?"

**DUNCAN:** Nathan was not presumptuous in that way. He was very respectful of all of us, even the least qualified. He knew we were all out of our depth. He's a very fine gentleman; he would never be presumptuous in a situation like that.

**VDT:** Were you and Beverly and Evelyn more or less running the Population Research and Training Center while Phil Hauser was out doing other things?

**DUNCAN:** On the day-to-day basis, we were there. He would come in from time to time; he would be in Washington or Tokyo or any place around the world. He was very faithful about meeting his classes, but that still wasn't at the Center. When he had a moment, he'd drop by and sign letters and kid the girls. And if money was needed, he would get busy and do something. And he would talk with us about what we were up to. But for the most part, he wasn't doing the day-to-day work. I guess he saw his job as keeping the organization in business and getting some good people to do the work.

**VDT:** Did Third World students, like Mercedes Concepcion, Visid Prachuabmoh from Thailand, and Iskander from Indonesia, start to come in the 1950s while you were there?

**DUNCAN:** Yes, they were one of Phil's specialties. He'd make these contacts with the statistical offices and universities in these countries and then write and ask them to send students. I was never particularly involved in that. I can't remember having any of those students as someone who worked closely with me. That was more or less Phil's baby, and later on, Don Bogue.

**VDT:** Don Bogue was still technically on your Center staff at that time, even though he was branching off with his Community and Family Study Center too. Isn't that correct?

**DUNCAN:** He was at Scripps Foundation in Ohio, with Whelpton and Thompson, and he was sort of commuting for a while. I think he came once as a visiting professor for one quarter, something like that, to fill in. You see, Chicago's department of sociology kind of fell apart. Wirth died; Blumer left for California; Ogburn and Burgess died. That left Hauser, Hughes, and Lloyd Warner as the full professors and then some other people. They were floundering and trying to get courses taught. Joe Spengler came at that time; that's how I happen to know him. But he just came as a visitor to fill in by teaching. And Don was doing that. After that was over--maybe it was partly at my urging--we recruited him to be a regular member of the faculty. But he always wanted to do his thing and he never cared to involve himself directly in the immediate affairs of the Community Inventory and the Population Center. So after a while, he ended up setting up his own organization, the Community and Family Study Center. I can't tell you the details of that.

**VDT:** I know there were some difficult relations.

**DUNCAN:** Well, there were some. We tried two or three times to have a cordial working relationship, but somehow it didn't work. You had people with strong egos, his and mine, strong wills, and so on. We didn't hit it off the way Phil and I did. But I still had a great friendship for Don. He and Betty were friends of Beverly and me; we'd go out with them to shows.

**VDT:** I'd like to talk about the development of your different research interests. Here's another quote from Hauser. He said: "Duncan developed into--and I think still is, although he's retired now--one of the best scientists in sociology, including demography. I say that with conviction. He's an absolutely superb researcher and methodologist and much of it through dint of his own effort and concentration and personal development, more so than through formal training, though he had enough formal training on which to build." You've already given a little insight into that.

Also, Nathan Keyfitz, in the interview he did for this series, said that "Duncan's work always inspired me--his ability to learn a brand new field, his ability to get at the empirical aspect of a subject, and his ability to cut through nonsense." I'd like to ask how you got into model-building. Keyfitz mentioned that you had more influence than any other person in developing the use of models in demographic research. I could go on and give one of your own quotes.

**DUNCAN:** Go ahead.

**VDT:** For example, your basic model, set out in The American Occupational Structure [with Peter Blau, 1967], for the study of intergenerational occupational mobility--the idea that the education and occupation of the head of the family of origin, and also the family size of the family of origin, influence the son's education and in turn the son's occupation. That's all laid out neatly in--well, it's in several places. You wrote, the three of you [Duncan, David Featherman, and Beverly Duncan] wrote in Socioeconomic Background and Achievement [1972], which was the sequel to The American Occupational Structure, this quote, which I like very much. I thought perhaps this is your philosophy of model-building: "A good model serves not only to rationalize and interpret a pattern of empirical relationships but also to raise questions whose answers require further empirical inquiry and/or modifications of the model. Thus the long-run course of research in an area of inquiry may be guided, more or less explicitly, by an incremental strategy of model-building" [page 9]. Sounds like you?

**DUNCAN:** I wrote that and I recognize those words. That's not necessarily the way I'd want to put it now, so I'm not clear whether you want me to think about how it was then or how I think about things now.

**VDT:** That's interesting that you say you've changed. However, then.

**DUNCAN:** Well, it was a long story. I got back into the stratification area by a kind of accident.

**VDT:** Why do you say "back"?

**DUNCAN:** I'd done a little study of that at Penn State, in a rural community, which the head of the department of rural sociology asked me to do. He'd started the study and the student who was going to run it had to leave town for health reasons. So, on a week's notice, I was asked to take charge of that study, and I did it with my assistant, Jay Artis. That was published in an Experiment Station bulletin and an article or two. But I didn't have any way of pursuing that interest in stratification for a number of years; I crossed over into the human ecology area.

But along in the mid-1950s, [Iwao] Moriyama and [Lillian] Guralnick at the National Office of

Vital Statistics, as it was called then, asked me to develop a classification of occupations that they could use to code death certificates, which they thought would be useful in studying socioeconomic differentials in mortality. So I took on that project; I forget how we got funding for it. My friend Al Reiss was simultaneously working on the completion of a study of occupational prestige that had been started at NORC in 1947 under the leadership of Paul Hatt, who had died. It occurred to me at some point that I could relate those two things by using the socioeconomic census data to predict the prestige scores of occupations. I worked that out and that was included in Reiss's book ["A Socio-Economic Index for All Occupations," Chapters 6-7, in Albert J. Reiss, Jr., and collaborators, Occupations and Social Status, 1961].

**VDT:** You were using 1960 or 1950 data?

**DUNCAN:** 1950 detailed occupation characteristics. We ransacked that book, made summary measures for every detailed occupation on all the characteristics and then started culling through those data. But when I saw the possibility of relating it to prestige, that changed the emphasis there and dictated the way in which the results were finally reported.

Once I had that, I realized that if you had a set of numbers for scoring occupations, you could use the scores for correlational analysis, the way Ogburn had taught me to do it. Kitagawa, as I mentioned, had been working on the labor mobility study and they had mobility tables that they had put together. So I got Bill Hodge to work with me and we used their mobility tables. We coded the son's occupation and the father's occupation and calculated the correlation coefficient between the two. We weren't the first to do that, but almost the first. Then we thought about introducing other characteristics, which were available on the labor mobility schedules: the education of the guy, whether he was a veteran, and age, to get a cohort breakdown. We published a little paper called "Education and Occupational Mobility" [American Journal of Sociology, May 1963], which had a path diagram in it, a very rudimentary thing, just a three-variable diagram--father's occupation, son's education, son's occupation--a little triangular pattern. That, I think, was the first honest path diagram in sociology. There was some mention of path analysis by Stuart Dodd in his Dimensions of a Society, but he didn't use it; he just mentioned that it existed.

I had inherited from Ogburn a set of reprints, which included a paper by Sewall Wright, the inventor of path analysis, "Statistical Methods in Biology." I started studying that, but I wasn't making much headway. It was difficult for me. At the same time, [Hubert] Blalock was working on causal inference from non-experimental data, following the lead of Herbert Simon. He was about ready to publish his book on that and I wrote to him that "I think path analysis is doing much the same thing; it's the same general idea." He wrote something to the effect that it was a different approach. But it is the same approach. Simon came at it from econometrics; Wright had come to it out of biometrics. Wright had even done work in econometrics which I don't think Simon knew about. It was a kind of independent, emergent thing. About the time Blalock's book was published, I saw how to put the thing together and wrote the article on path analysis.

In the meantime, Peter Blau had been trying to start a stratification study. He was a member of the International Sociological Association committee on stratification and they wanted to start studies in every one of the major countries. Blau was to try to get one going in the U.S. So he'd talked with Clyde Hart at NORC about doing a study there and they had tried to design a study and get financing, but were having no luck. Then Phil Hauser said to Peter, "Why don't you get the Census Bureau to do it as a supplement to the CPS [Current Population Survey]?" At that point, Peter figured he would need help to work with demographers, to understand what the CPS was and how to use it. So he asked me if I would like to work with him on that. We had taught a course together; we were young colleagues and we had respect for each other, although our styles of work were quite different. I didn't have anything better to do and said, "Sure." So we went to the National Science Foundation and got a

very sizable grant for the time and commissioned the Census Bureau to put a supplement into the CPS of March 1962. That's how those things were sort of happening in parallel. It's hard to say now what preceded what and how one idea led to the next one.

**VDT:** You say your study with Peter Blau, The American Occupational Structure [1967], used questions on the March 1962 CPS?

**DUNCAN:** Yes. They would not give us access to the tape; at that time, it was thought to violate confidentiality procedures. So they said they would tabulate the tables we wanted.

**VDT:** Could you give me an example of the questions?

**DUNCAN:** What was your father's education when you were about 16 years old? How many siblings do you have? What was your oldest brother's education? What was your first job? There was a very small number of questions, maybe a dozen.

**VDT:** This was the only time such questions were asked in the CPS?

**DUNCAN:** That was the first time. They were subsequently asked in 1973 when the replication was done, but I had nothing to do with that. We started it at the Bureau, so that was an innovation there. And that was at Phil's suggestion. But I had not done that with the idea of using path analysis; that was a separate thing being pushed simultaneously. At the time we specified the tables, I still did not have the idea of using path analysis. It was sort of by accident that we specified a lot of two-way tables, which could then be used to calculate correlation coefficients, and then we made them into input to path analysis. That was not premeditated; it was an afterthought. I had different ideas, like indirect standardization; I wasn't sure how I was going to do it. But the path analysis came up and Peter was enthusiastic about it, so we decided to invest in that, although we also used other methods--multiple classification and so on--in the end.

So that all kind of came together. Everything in my work from the late 1950s to the mid-1960s, that whole decade, went into stratification primarily. And it happened very fast.

**VDT:** I have an interesting side comment on that. When I interviewed Charlie Nam for this series, he said that at the Census Bureau about that time, he was also working on developing indexes or data for studying intergenerational social mobility.

**DUNCAN:** Yes.

**VDT:** He said he was using only census data. I think he implied that you were using some non-census data, meaning the NORC data, which Reiss had. He gave the impression that you were competitors in a sense, working in the same field. But he was very pleased that you sent him a copy of the Albert Reiss book, Occupation and Social Status of 1961, in which you had two chapters, and inscribed it: "From one SESer to another." He said, "I treasure that." Maybe you didn't get the impression, but he felt you were a bit rivals.

**DUNCAN:** Well, there were a lot of people doing that. The guy who did it first was the Canadian, Bernard Blishen. He had done it with census data for Canada. Then Don Bogue did it, using census data and some kind of factor multiple techniques to weight education and occupation. August Hollingshead had gotten out his two-factor index of stratification. So everybody was doing it. The novel thing in mine, which sold it I think, was using the census data to predict the prestige score,

because the prestige score had a kind of aura of mystique about it.

It ended up that all those indexes were just different versions of the same thing, because they were using the same basic inputs, to wit, the income level and education level of the occupation. I think that's sort of a given; you can summarize it in alternative ways, but it's the same stuff however you summarize it. There are some details about how it's done that make some difference, but it's all variations on a single theme. There were a lot of people and I'm sure they came at it from different points of view and independently, in part. I knew about some of that work before; I think I even mentioned it in the Reiss book. I did not know about Nam's, I think, until his was under way or maybe even done; I'm not sure.

**VDT:** It came out as a Census Bureau study, I think. He was trying to get the Census Bureau to get more into that field and wasn't getting much encouragement.

**DUNCAN:** I didn't have anything much to do with that. They had a composite measure of social stratification--that was not the same thing as this occupational measure--which I took strong exception to. I wrote a bitter, devastating--I guess it was devastating--critique of that to the Census Bureau. It died. I don't know whether it was because of my influence, but it didn't come to anything. They were experimenting with a composite measure, which would combine education, occupation, income and so on. The other thing is occupational status. Did Nam tell you about why they left out the farmers from his publication?

**VDT:** No. Tell me that story.

**DUNCAN:** If you look in Nam's first publication, he lists all the occupations except "farmers and other farm occupations". They were simply deleted, because his technique showed their very low status and Conrad Taeuber said, "You can't publish that; the farm congressmen will be on my neck." So they were calculated by the same formula, but not published.

**VDT:** Interesting! Was that about the time you were on the Census Advisory Committee on Population Statistics?

**DUNCAN:** 1964 to 1973, it says here [in his curriculum vitae].

**VDT:** I have a note in my files, which can't be right because your being on that committee came later, that you tried to get a question on religion into the 1960 census. Did I put that in the wrong file?

**DUNCAN:** We had a COPS, Committee on Population Statistics, in PAA and we were advocating inclusion of that question for the 1960 census. And I think that it was a suggestion of mine that made some religion statistics available. They had done a sample survey of religion in 1957 [March 1957 CPS], but it was suppressed. I think there was a table in the Statistical Abstract, but that was all [Paul Glick and Wilson Grabill put out one Census Bureau report, but a second more-detailed report was suppressed; see Glick interview]. In a [COPS] meeting one day, I said to Con, "If somebody used the Freedom of Information Act, I wonder if those statistics could be pried out of the Bureau." He said, "Well, I'll have to look into that." And the first thing you know, it was made known, though not widely advertised, that if you would pay for the cost of reproducing the tables, you could have the tables from that 1957 survey. I had them; I no longer have them. I'm sure they exist in various libraries and archives. My suggestion, I think, led him to think of a way in which that could be accomplished without causing too much trouble. But we went ahead and made a recommendation for inclusion of a religion question in 1960 and there was a lot of serious discussion at the Bureau, but it

was very firmly axed in the end.

**VDT:** By...?

**DUNCAN:** Well, I can't tell you that.

**VDT:** The Rightists, the Catholic forces, or what?

**DUNCAN:** The Catholics, as I understand it, were not opposed. They would like to have that. What they did not want was another Census of Religious Bodies, which would show the value of church property and things of that kind. But they wanted to know about church membership.

**VDT:** Was that in the 1957 survey?

**DUNCAN:** No. That was the Census of Religious Bodies. The last one was taken in 1936. That was supposedly on a decennial basis, [1906, 1916], 1926, 1936, and another one was scheduled in 1946, but it was never done.

**VDT:** Why the Census Bureau?

**DUNCAN:** That's an aspect of the history of Census operations you might not know about. It was not a demographic survey. It was a questionnaire to the organizations, churches, that the officials of the church were supposed to fill out and return. They asked about membership, but it was not a population survey; it was just their own records. Those data were used in the early days of research on religion and demographic people would compile the memberships by counties and correlate them with various characteristics.

The 1957 [CPS] survey was a different idea. It used the usual sample survey procedure and asked people, "What is your religious preference?"--something like that. That gives you an enumeration not of organizations, churches, but of people according to their preferences. Bogue had previously compiled stuff, using NORC and other private surveys to get essentially the same information. There were no new facts, except more detail and a larger sample in the CPS data.

**VDT:** But the U.S. Census Bureau has never been persuaded to put religion in the decennial census.

**DUNCAN:** Well, the Bureau has been persuaded, but not the people who have to put up the money. I'm not sure whether the Bureau would want to do it now. I'm not sure I would want to do it. I think that was a mistake.

**VDT:** Why?

**DUNCAN:** I think the Census has no business asking people things that are matters of their private conscience. My views on that have changed a great deal. I don't trust the Bureau any longer.

**VDT:** Oh, you're one of those.

**DUNCAN:** It's politicized. I was naive and I was made more sophisticated by the run of events. I don't think we should use the Bureau for those kinds of things.

**VDT:** What about the standard questions that are now asked? Well, they will be in 1990--they'll ask

whether you're Hispanic or not--on the short questionnaire.

**DUNCAN:** I don't know about that. Objective characteristics are simply matters of public knowledge anyhow. Your age is a matter of record because your birth certificate's on file, so you go and ask somebody his age, that's just a convenience, ascertaining information that's already there. Your income is something you have to report to the government for tax purposes. Questions that are in interactions with strangers, such as "What's your job? What kind of job do you have?", are not intrusive. But if you ask a person, "What is your religion?", or you ask, "Are you very happy, pretty happy, or not so happy?", you are asking things that the government has no business asking people. People should not be required to report to the government on things like that. The census is, of course, compulsory. And therefore people are being compelled to answer questions that in all conscience they might not find they should be asked. I no longer have that great thirst for data I had when I was younger, that would lead me to override these kinds of considerations of the ethics of statistics. Quite the contrary.

**VDT:** Can you think of some data that you were thirsting after, recommending that they be collected by the Census, during the time you were on the Census Advisory Committee? Or any outstanding issues at that time, when you were that closely connected with the Census?

**DUNCAN:** We wanted them to get Social Security numbers and match records between the census and the Social Security. That, I think, foundered because of the impracticalities of it. It turns out that some people have multiple Social Security numbers and the matching is by no means easy or foolproof; it's a very cumbersome procedure. And that, of course, would have fed into the idea of data banks. Again, that is something I think we have to worry about more than I worried about it at the time. All I was thinking about at the time was how neat it would be if you could get good occupational mobility data with matched records.

**VDT:** You would have in the Social Security, where and in what jobs you were at different periods and you match it up with the census characteristics of that person?

**DUNCAN:** It just takes a little imagination to see what fabulous analyses you could do with that. But that presumes, (a) that it works, and (b) that it's politically feasible, and I think neither of those happens to be true. Of course, the census is on very hard times; you've got a different kind of society now. It's not at all clear that in 1990 people will submit to being counted. Or if they do have a count, whether it will get large fractions of people who are elusive, like transients, illegal aliens, and people of that kind.

**VDT:** Well, the 1990 census is a tremendous issue right now, although it's a fait accompli; they're going to still count those who are willing to be counted, whether or not they are illegal. In your work on intergenerational occupational mobility, one place you summed it up was in your PAA presidential address, "Inequality and Opportunity" [Population Index, October/December 1969]. You said there that your studies had shown that up to that time--through the 1962 data--most occupational mobility in the 20th century had been upward; more than 50 percent had higher prestige occupations than their fathers. But isn't there some downward mobility now, because of the baby boomers, who cannot attain the positions of their parents because there are so many of them?

**DUNCAN:** I haven't tried to follow those trends since about the time of that presidential address; I turned to other matters. Then my former students, Bob Hauser, the nephew of Phil Hauser, and David Featherman set up this repeat study, which was done in 1973, and they were planning it some years earlier. When they started that, I redoubled my resolution not to have anything further to do with that

line of work, so I would not be looking over their shoulder or in any way inhibiting them about having to conform to my pattern or work or anything like that.

**VDT:** Why do you say "redoubled"?

**DUNCAN:** I had other things I wanted to do by that time. I felt I was played out on stratification; had done everything I could think of and was sort of at a dead end. And my pattern has always been to work on something very intensively and satisfy my own curiosity or what I think I can do and then move on to something different, perhaps quite different, so as to get a fresh start.

**VDT:** That feeds into the next question. I wanted to talk about yours and Beverly's time at Michigan, that change. Did that also coincide with your change in research interests?

**DUNCAN:** The occupation study was started at Chicago, but it really didn't develop until I was at Michigan. The survey was done in the spring of 1962. I left Chicago in the summer of 1962 and the tables didn't come until 1964, something like that, after I was at Michigan, so all the analysis was done at Michigan. That was my main preoccupation, apart from one paper on human ecology, I think, until the late 1960s.

**VDT:** What had taken you to Michigan, the switch from Chicago to Michigan?

**DUNCAN:** Al Reiss was at Michigan by that time. He called up and said, "Would you like to come?", and I asked Beverly about it and she said, "Well, let's look into it." She wanted to get out of Chicago, I think--that's the way I remember it; she didn't remember it that way. But she encouraged me to look into it.

**VDT:** The city or the university?

**DUNCAN:** Well, the city [Chicago] was a horrible, dirty place. I guess we were just restive and Michigan seemed attractive; they seemed to have a new center being formed and a reason to come onto the scene there and it seemed to be a very lively place and a change in climate. When I went out there, I could see the stars at night. So it was the right place at the right time. And that was a very good period at Michigan; we worked very hard.

**VDT:** Tell me about your time there. The Freedmans were your colleagues, of course--another famous couple doing demographic research. Were you at all involved in Michigan's research to developing countries, to Taiwan in particular?

**DUNCAN:** No. We begin with the same story; that was Ron's baby, not mine. I didn't have much to do with the foreign students. He would bring those students in and involve them in his projects, which would often be in their countries. I would work with the American students.

**VDT:** You apparently were involved with the Detroit Area Study, because you used the data in Socioeconomic Background and Achievement [1972].

**DUNCAN:** That was the program of the department of sociology. Each year they did a survey on a different topic; they had a different faculty member do that. The topic rotated according to what people were ready to do something and proposed a topic. I think it was around 1969 that Eleanor Sheldon asked me to write a memo about social indicators and what should be done there. I wrote that

one of the things that could be done was to replicate old studies. I illustrated some replications that had been productive and established guidelines for that kind of work; then I surveyed the possibilities for good replications. One was the DAS [Detroit Area Study], which had been going at that time for about 17 years. I said replication of some of the early studies there would give us measures of social change over a period of 12 to 15 years, so that should be done. I got so enamored of that idea that I followed it up myself and volunteered to be the faculty sponsor and talked to Howard Schuman, who was the DAS director, and he was interested. So we worked closely together on designing that study and he was a participant in that.

**VDT:** The DAS was phoning back to the same group of people every year?

**DUNCAN:** No, it was just an annual sample survey of metropolitan Detroit, typically a small sample, usually 600-700 cases, although sometimes larger if they had supplementary funds. The samples were independently drawn. There was no panel feature, no longitudinal thing, unless a particular investigator decided to include that feature.

**VDT:** So that was the time that you switched to your interest in social indicators, in the late 1960s?

**DUNCAN:** Yes. Late in the work on occupations, we got into the poverty theme. Pat Moynihan was having a periodic seminar on poverty and we got out the books on poverty research. He was out of his political job with the Johnson administration and found a job at the Harvard/MIT Joint Center for Urban Center. In that interim period, he was interested in poverty policy and formed this seminar and invited people to come. We ended up writing these books.

Related to that, I guess, was the social indicators thing, which had grown up with some people who made a report to the Air Force, of all things--Bauer, Gross, and Biderman--they proposed an initiative on social indicators and a committee was formed by HEW. I was on that advisory committee; it was chaired by Dan Bell. Eleanor Sheldon got into it with Russell Sage. She and Wilbert Moore got out the book on Indicators of Social Change [1968]; Beverly and I each wrote a chapter for that. After that work was done, she raised the question, what's next?, and asked me to write the pamphlet I've just described [Toward Social Reporting: Next Steps, Russell Sage Foundation, 1969]. That led me into the DAS. We were then writing about social indicators and measures of social change and so on. Then Eleanor Sheldon got to be president of the Social Science Research Council and formed this project to set up a center in Washington [SSRC Center for the Coordination of Research on Social Indicators] and an advisory committee, which I chaired.

**VDT:** Eleanor Sheldon had been a student at Chicago so you knew her?

**DUNCAN:** Yes, we were classmates at Chicago. Actually, she had been the first director of the Chicago Community Inventory, before Phil Hauser was on the scene.

**VDT:** So she put you on the social indicators advisory committee of the Social Science Research Council?

**DUNCAN:** Yes, that was all schemed up between her and me. She talked with me about that and I agreed to do it on certain conditions, and we kind of did it my way.

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

**DUNCAN:** Well, I said who I was interested in working with and who I was not interested in working

with and how it would be set up and I helped her recruit the guy who directed it, Bob Parke. He had been on the staff of the Commission on Population Growth [and the American Future]. By the way, I got on that commission through Moynihan. That was how I got to know Parke and after that was over, I told Eleanor that he was a good man. It happened that she was doing this social indicators thing at that time, so she fitted him into that. I had a lot to do with getting that set up that way and I stayed with it for three years and then moved off.

**VDT:** Let's backtrack to Michigan, the colleagues you worked closely with--besides the Freedmans, whom you must have seen all the time, even if they were doing their Third World thing and you were doing . . .

**DUNCAN:** We were just doing things in parallel. We went to brownbags once a week and listened to each other talk, but that was the closest connection I had with the Freedmans, except for one or two papers. I did a paper on fertility, using his and my data together. We did a collaborative study [Duncan with Ronald Freedman, Michael Coble, and Doris Slesinger, "Marital Fertility and Size of Family of Orientation," *Demography*, No. 2, 1965].

**VDT:** Let's talk about your students at Michigan--and at Chicago. Norman Ryder said that you and Beverly were great mentors to the students at Michigan. And I've heard from others how the students would flock around you at PAA meetings, which meant others could see you were great mentors. Ryder said that the University of Wisconsin, where he started the population center, drew its staff from Michigan graduate students, because they were the best.

**DUNCAN:** That's true. We had a lot of good students. I hesitate to say who the good ones were, because there were so many of them.

**VDT:** Who were some that stand out? You mentioned Bob Hauser, for instance.

**DUNCAN:** Bob Hauser was not my student. He was in a course or two with me, but he was never on my project. The main thing I had to do with his dissertation was putting him in touch with Al Reiss, who had done a study in Nashville, and I thought if Bob got hold of those data, it could be a beautiful study. And it was. So I helped him get into that and he modeled his work on the Blau-Duncan kind of stuff. But he had not been involved with my research before.

David Featherman, on the other hand, was my assistant, a very good assistant, starting about the time Blau and Duncan [*American Occupational Structure*, 1967] was finished on the followup project that led to the second book [*Socioeconomic Background and Achievement*, 1972]. He was the assistant throughout that. He was a social psychologist and I wanted to get more into the social psychological side, which was distinct from the demographic side of that work. We were exploiting opportunities to do that, so he was one of my main assistants there. But a lot of those others--Jim Sweet, Larry Bumpass, Paul Voss--they are all at Wisconsin now; they were in my courses, but they were not my proteges or assistants.

**VDT:** Name a protege, other than Featherman--let's say both at Michigan and at Chicago. Of course, there's Beverly, who was your number one protege.

**DUNCAN:** Yes, she was my best student, sure. Hal Winsborough is another one now at Wisconsin. He saved their neck on their center. They were in real trouble because they had set up this Center for Demography and Ecology but nobody could run it properly, so I told Bill Sewell to hire Hal Winsborough; he was at Duke at that time. He'd been my student at Chicago and had collaborated on

the book on metropolitan structure [Metropolis and Region, by Dudley Duncan, Richard Scott, Stanley Lieberman, Beverly Duncan, and Hal Winsborough, 1960].

Stanley Lieberman was a student of mine at Chicago, who's been prominent in later years. Richard Redick was one of the earliest. He worked in mental health statistics, spending his whole career with the government in Washington; very good worker, although not in research particularly. Robert W. Hodge, Bill Hodge, was my assistant on the measurement of occupational stratification, the first paper on path analysis that I mentioned. He died just recently, a very premature death, a great loss. So those were some outstanding people at Chicago.

At Michigan, I think of Hauser and Featherman, Bumpass, Sweet, Voss--I may omit names and I hate to single any out. Another good student there was Mike Coble. He went into computer work and he's still the backbone at the Michigan computer facility. He decided he didn't want to pursue academic sociology--for which I don't blame him--but was very good on the computer. He was my main help when I first went to Michigan--and all the way through, in terms of getting the job done. And Ruthe Sweet, Jim Sweet's wife, worked with me. She had been at Duke with Hal. She didn't stay in sociology either; she became a midwife.

**VDT:** A midwife! About as far as you can get . . .

**DUNCAN:** A very interesting person. Well, it's close to some aspects of our interests.

**VDT:** What is your impression of the problems they had with the two population centers at Michigan, the Population Studies Center and the Center for Population Planning?

**DUNCAN:** Ron started out to have a relationship with Les Corsa [Center for Population Planning]. They lived in the same building with us, but we kept our distance, and Ron gave up after a while.

**VDT:** You were connected with which center?

**DUNCAN:** Population Studies Center. Freedman was the founder and director of that. For a brief period, I was director [associate director, 1962-73; director, 1967-68]. Then David Goldberg and a succession of other people have been director.

**VDT:** Who have been leading influences on your career? You've mentioned several, of course--Bill Sewell . . .

**DUNCAN:** Sewell first; Ogburn second; Phil Hauser third. Those three.

**VDT:** May I ask you about Ogburn? Phil Hauser said that you had done a series of monographs on him; what was that?

**DUNCAN:** It's just a collection of his writings, called William F. Ogburn on Culture and Social Change [1964]. It was published in the Heritage of Sociology Series of the University of Chicago Press. That project was run by Morris Janowitz and he asked me to contribute that volume. So I wrote an introduction about Ogburn, his career and accomplishments, and then had a selection of articles and chapter excerpts.

**VDT:** Was that before or after his death.

**DUNCAN:** It was after his death. I think he died in 1957; that book was about 1964. I did that after I

went to Michigan.

**VDT:** What about J.J. Spengler, whom you mentioned early on?

**DUNCAN:** Joe came to Chicago, as I mentioned, as a visiting professor to help do some of the teaching chores. When I got to know him a little better, he said, "Why don't we do a reader?" I guess he met Jerry Kaplan, who had a series of readers in sociology. We talked and decided that to include all the things he was interested in and I was interested it would be more than one book. So we said, "Let's give Kaplan a real scare; let's propose two books." And Kaplan said, "Sure, go ahead." So we did [Duncan and Spengler, Population Theory and Policy: Selected Readings and Demographic Analysis: Selected Readings, both published 1956].

Kaplan was the entrepreneur who founded the Free Press, which later consolidated with Macmillan and he moved to New York. But in those days, he was just a shoestring entrepreneur on his own, developing as a publisher. He used these readers to build a market for his books.

**VDT:** What are your recollections of the work of the Commission on Population Growth and the American Future? You said that Moynihan was instrumental in getting you on the commission.

**DUNCAN:** Yes. He'd been taken on as the domestic adviser to Richard Nixon and Congress had passed the law that we'd have a population commission. That was Republican politics--a payoff to John D. Rockefeller, who wanted this. But Nixon wanted it done his way. So he had Moynihan be involved in the selection of people. The legislation said the President would appoint the commission. It was not exactly a presidential commission, where the President forms it and runs the whole show, because it was chartered by the Congress, in legislation prescribing what the commission would do.

**VDT:** Moynihan got you onto it. What's your impression of it? It was set up in 1970 [chartered earlier], because of the concern then with rapid U.S. population growth.

**DUNCAN:** Yes.

**VDT:** But by the time the report came out in 1972, U.S. fertility had dropped to replacement level.

**DUNCAN:** Yes, but there has never been a commission as successful in getting its recommendations adopted, has there? [Laughter]

**VDT:** Instantaneously!

**DUNCAN:** We argued about abortion bitterly. Had a terrible, knock-down, drag-out fight about abortion and then in 1973, the Roe v. Wade decision came out. So all the things that we were interested in seeing happen happened anyhow--but not because of us.

**VDT:** Of course, the only public attention it ever got was because Nixon attacked it [the report] for those two points--its proposal for legalizing abortion, which happened anyway, and [proposal for providing contraceptives to teenagers].

**DUNCAN:** You know about the television program? The Commission made a television version of that report that was aired on public television. The networks wouldn't touch it because they wanted their news departments to control the presentation and we refused to do it their way. Public television took it, but they would only take it if it was used as one segment of a program and then followed up by

a debate. They brought in the Right-to-Lifers and one Jesse Jackson. And Jesse Jackson and I--on television--I remarked that I thought our last two presidents had been exemplary to the American people in regard to family size; both Nixon and Johnson had had two children. And Jesse said, "Oh, that Nixon--he's too cold." [Laughter] That was about all I said on that program. There was a lady who came in with a pickled fetus; she had a name for this creature and was just insulting to Mr. Rockefeller.

**VDT:** All of this was live television?

**DUNCAN:** Yes. There first was a canned program, narrated by Hugh Downs--not too good, but okay. Following that was a live debate. Mr. Rockefeller was there and not all the Commission, but a half dozen of us, and then these people who came in. Ben Wattenburg was one of the critics, the lady from the Right-to-Life group and Jesse Jackson, I remember; I forget who else.

**VDT:** Well, you really do remember the work of the Commission!

**DUNCAN:** The Commission was a great experience. Had a very good staff; Charlie Westoff [and Robert Parke] ran the staff.

**VDT:** The Commission itself met periodically during the two years?

**DUNCAN:** We met once a month; it was pretty often. We took the work very seriously and we had hearings in three different communities. I was at Little Rock and Los Angeles; sat there and listened to the Right-to-Lifers, the environmentalists, and so on--whoever showed up and wanted to bend our ear.

**VDT:** There's a great set of research papers, the seven volumes.

**DUNCAN:** Yes. It's a monumental study and it was right. It was time to slow down growth; it's not slowed enough yet. We didn't have any luck getting through a strong immigration thing. It was Grace Oliveros, a Mexican-American lawyer, who was very keen about the illegal problem. It wasn't looming so large at that time, but she saw the problem and wanted us to make a strong stand on trying to control illegal immigration, but we didn't.

**VDT:** It was a Mexican-American woman saying this?

**DUNCAN:** Yes, that was the interesting part of it.

**VDT:** That certainly has become the issue since then for the U.S.

**DUNCAN:** Yes. There were quite a few issues that we fingered in there--some of them a bit too delicately, but they're there.

**VDT:** What do you see as leading issues in demography over the years you've been involved? You got more into social indicators and so on, but think in terms of demography. The issues that the Commission dealt with--U.S. fertility, population growth was considered too high in 1970. Fertility, of course, declined to replacement level in 1972; it's been 1.7, 1.8 since 1976 [2.0 in 1989]. The baby boom and bust--has that been something that concerns you?

**DUNCAN:** No, it doesn't concern me a bit. [Laughter]

**VDT:** Why do you say that?

**DUNCAN:** As soon as I finished my PAA presidential address ["Inequality and Opportunity," 1969], I quit paying any attention to demography. I got into survey analysis, the social psychological stuff, and that's where I've been since 1971.

**VDT:** Okay, that answers that. You've also made it pretty clear that you've not been concerned with rapid population growth in less developed countries.

**DUNCAN:** Well, I think it's deplorable and a threat. I've always felt that, but I haven't done anything about it in either a policy or a scholarly fashion.

**VDT:** But you have been concerned with questions of social policy. The Petersens say that in your entry in their biographies of demographers ["Duncan's work exemplifies a fruitful combination of demography, urban studies, and mathematical techniques, almost always linked to questions of social policy," in William Petersen and Renee Peterson, Dictionary of Demography: Biographies, 1985, p. 285]. You felt it was important for social scientists to talk to policymakers?

**DUNCAN:** I don't know about that. I did not get into social science to reform the world or to have a role in setting policies. My agenda was to be a scientist and to try to make statements that had a little more authority and basis for them than the usual journalistic social critic might have. From time to time, I wrote sort of essays and studies oriented to policy issues. The last one was on nuclear energy. I wrote quite a good paper on that, which came out of my experience on the energy committee of the National Academy of Sciences.

**VDT:** Where and when was that paper published?

**DUNCAN:** That was back in the late 1970s ["Sociologists Should Reconsider Nuclear Energy," Social Forces, September 1978]. I was at Arizona at the time I was serving on that committee. I got discouraged with the way it was going and resigned. But I'd read enough about energy to have some thoughts on it.

I wrote a paper once on air pollution and human ecology. I wrote a paper on riots. I wrote a paper on "Inheritance of Poverty or Inheritance of Race?" [in Moynihan, On Understanding Poverty, 1969]. From time to time, I've written papers that tried to use my research experience to say things I thought I could say with a little more confidence as a consequence of having done the research. Too many social scientists want to put out thoughts on all policy issues, just give their thoughts. It seems to me that's not our role. Our role is to report what we think we've learned and try to help the community to make up its own mind about policy. So my philosophy there is a little bit different from some people, I guess.

**VDT:** Tell me something about your time at the University of Arizona in Tucson and here at Santa Barbara.

**DUNCAN:** At Arizona, we got out from under the necessity of running a large research organization. Both Chicago and Michigan were organizations, where you had to be responsible for a lot of other people's work. At Arizona, we just did our own projects. Beverly and I worked together some; we worked separately some. But largely with secondary analysis. We had the Detroit data collected in 1971 and I spent most of the next ten years analyzing those data; most of that was done at Arizona.

**VDT:** That was one year of the Detroit Area Study and you took a larger sample?

**DUNCAN:** Yes, we enlarged the sample; got money from the Russell Sage Foundation. They had been involved in getting the thing started. I tried to use that as a demonstration project for how you could create social indicators by replicating surveys and I pushed that idea later in the social indicators committee. We did a bit on that; we had something to do with archiving survey data. They're not very prominent ideas anymore, I guess, but they seemed like good ideas at the time. I wanted to do a thorough analysis of that survey and I kept working on that. I learned new statistical methods. This led into the last phase of my research, which has to do with Rasch measurement models.

Georg Rasch was a Danish mathematician who was asked to do some work in psychometrics and he came up with a new approach that has, in my opinion, profound implications for all social measurement. I was trying to exploit those ideas and get people interested in them. That's why I spent the last seven or eight years of my career largely on that, as well as the book on social measurement [Notes on Social Measurement: Historical and Critical, Russell Sage Foundation, 1984].

**VDT:** Of which I brought along a copy. I ordered it from the publisher and I hope you'll autograph it before I leave.

**DUNCAN:** I'll be glad to.

**VDT:** I also tried to get a copy of The American Occupational Structure but everybody was out of it, including the publisher, the Free Press of Macmillan, but they were expecting new stock this month--I presume of the paperback reprint of 1978. That leads to another question. Which of your publications do you consider most important, and why?

**DUNCAN:** Notes on Social Measurement is the only book I care about anymore. The others were all right at the time, but they're obsolete. That one's not obsolete.

**VDT:** That's an interesting way to put it. Notes on Social Measurement came out in 1984, from Russell Sage. You did a number of books for Russell Sage, didn't you?

**DUNCAN:** Yes, I was astonished to see how many different things I had done for them.

**VDT:** And you consider the other books obsolete. Well, I'm not sure they are. The Free Press was out of The American Occupational Structure when I phoned a month ago, but they were expecting new stock, because it's still a standard.

**DUNCAN:** I can't say why.

**VDT:** I have to admit I've only heard of it. I didn't come out of sociology. I did demography at Georgetown, but I came out of history, many years ago. So I'm not a sociologist, but of course anyone even peripheral to sociology has heard about that book.

A final question on your career, both in demography--you said that career ended with your PAA presidential address--but also since then: What accomplishments in your career, looking back over it, have given you the most satisfaction?

**DUNCAN:** I'm not sure that it's given me any satisfaction.

**VDT:** Why is that?

**DUNCAN:** Well, it's hard to say. I don't know exactly how I became a social scientist. I think it was kind of following leads of my family. Bill Sewell was my role model and I've mentioned people who encouraged and helped me. I found I could do it. I was successful; it wasn't too hard. But I'm not sure it was worthwhile. I wish I'd done something else.

**VDT:** Like what?

**DUNCAN:** Almost anything. [Laughter] I don't know if I could have made a real scientist or not, or a mathematician. I probably couldn't have been as successful; those are harder to do than sociology. I would have liked to have done music, but I wasn't good enough to be a musician.

**VDT:** You're now trying it. Had you an early interest in music?

**DUNCAN:** Oh, yes. As a schoolboy, I played violin in orchestras and ensembles. I enjoyed that a great deal. But I didn't have enough motivation to practice carefully and I wasn't cut out to be a professional musician. But I tried composition as a boy.

**VDT:** On your own?

**DUNCAN:** Yes, without any instruction, just as an amateurish thing. I've been doing that off and on all my life. But now I don't have to do sociology, so I'm doing it much more.

**VDT:** You are composing for electronic equipment. It looks very complicated.

**DUNCAN:** It is.

**VDT:** We're sitting in front of a keyboard that has an enormous number of notations; a tremendous number of buttons.

**DUNCAN:** The buttons don't have anything to do with the notation. The notations are fractions that have to do with just intonation, which is now possible. Just intonation is an old theory. It dates back to the Greeks and the Renaissance, but it's been revived in recent years as a consequence of Harry Partch, whose picture is over there. He's the guru of just intonation. We have a small organization, less than two hundred members. It's focused in San Francisco and I've been an enthusiastic member and supporter of that organization for the last two or three years. I do all of my composition in just intonation, which sounds different--sounds better than the conventional equal-tempered music of Western Europe, which is what we use in both our symphonic and pop music. Just intonation is used in other music around the world, Oriental and African and so on. Those people have better sense than Europeans about what sounds good.

**VDT:** Do you write this down? Does it get recorded?

**DUNCAN:** It's written into the computer. Just like a word processor, this is a music processor. It has a screen.

**VDT:** There's a monitor, screen, here, looking like a normal monitor or TV screen.

**DUNCAN:** A TV screen.

**VDT:** That's fascinating. I was going to end up with this, but let's jump back to your connections with PAA, which is why this series of interviews began. You said you haven't had much connection with PAA in recent years, but just sort of think back on PAA. Can you remember the first meeting you attended?

**DUNCAN:** It was at Princeton, probably in 1949, I suspect.

**VDT:** 1949 was in Princeton, yes.

**DUNCAN:** I went to Penn State in 1948, teaching population, as I told you. One faculty member gave me his collection of Population Index; he wasn't teaching population anymore. I thought I should subscribe to it. I wrote to the Association or to Princeton and asked to be put on the subscription list and the price of subscription. I got a letter back, I think from Paul Glick, inviting me to be a member. So, I went to Princeton. I went fairly faithfully to those meetings for a number of years. They were often at Princeton; then they started moving to different parts of the country.

**VDT:** There were meetings at Princeton in 1949 and 1950, and in 1955; that was the last one at Princeton. What stands out in your memory about the early meetings? Few people, of course; everybody went to the same session. This is a list of meetings that Andy Lunde prepared. He has Princeton in 1949, 1950 . . .

**DUNCAN:** 1952. You skipped that one. I have special personal reasons for remembering that one, which we won't go into.

**VDT:** Beverly . . . And the ambiance at Princeton, which so many people have said was so pleasant.

**DUNCAN:** Well, you know, Frank Notestein, Frank Lorimer, Warren Thompson, Pat Whelpton, and Fred Osborn would stand up and comment on the papers in a very measured, precise way. It was a good intellectual affair. We had only one session at a time; everybody went to the same session and paid close attention. The discussions were animated and scientific. It was just stunning if you wanted to be a scientist. This was one of the things that consolidated my devotion to population--those meetings.

**VDT:** The 1969 meeting, when you were president, was at Atlantic City, presumably before the days of the casinos. That wasn't far from Princeton and apparently it was a fairly good place for professional meetings. There were 486 people. At the latest meeting [1989], in Baltimore, there were almost 1,200 [1,193]. I have a nice little story. Lincoln Day told me that he and Alice Day--they were at Princeton at that time--went down from Princeton with a young woman colleague and introduced her to just a few people and turned her loose. Three days later, on the way home, she couldn't get over what a congenial atmosphere there was, everybody talking to everybody else. And Lincoln said she wasn't so pretty, so that wouldn't have been a reason for anyone to approach her, necessarily. Obviously, PAA meetings still had that flavor somewhat in 1969. But it's huge numbers now. Aside from your speech, do you remember that?

**DUNCAN:** I remember that's when we had the beginnings of these caucuses and students going around . . .

**VDT:** I've heard that.

**DUNCAN:** Some of that occurred and I had it under control when I presided. I didn't take any guff off of those people. We got into some argument, I forget about what, in the business meeting and Eli Marks kept wanting to talk. He was primarily a survey guy and a statistician, but he was coming to population meetings. He kept standing up, popping up and down like a jack-in-the-box and shooting off his mouth at great length. Finally, I just said, "You're out of order." Sat him down.

About the students--I remember some guy from Wisconsin who had red hair which was about a bushel-basket size around his head. That was the beginning of a period of unrest, and the sequel was in the next year or two when Keyfitz was in the chair [president at the time of the 1971 meeting, in Washington DC]. That's when they passed all those outrageous motions. Beverly became very agitated because those young women got up and alleged that the Population Association had not been a place where women could enjoy any success. And we've had more women presidents than any organization that I know of--Irene Taeuber, Margaret Hagood, Dorothy Thomas [to that time; four more since the early 1970s]. It was founded by Margaret Sanger.

Those [presidents] were great people. Not just great women, but great demographers, great scientists. And Beverly had great respect for them. They made it on their merits; it wasn't impossible. So, Beverly called those women down, in the meeting itself, and talked with them afterwards, trying to make them understand that people had been there before they showed up.

I was very upset at those meetings, because they were passing resolutions about abortion and so on, which had historically never, never been anything the Association did. We'd always taken the position that ours was not to take policy stands but to provide the scientific basis for discussion of policy issues, so that people could come together and have different positions. We had Catholic members who would come and members of different persuasions. We'd talk about demography and then they would go back and could write their own policy position papers. But people in the organization, like Chris Tietze, I remember him specifically supporting the abortion resolution. And he knew better than that, because he'd been a part of the organization for a long time and had known this historical thing. He got carried away because he was a partisan on that issue. Well, I was a partisan for it; I was on public record in the report of the Commission on Population Growth. There wasn't any doubt where I stood on the matter, but it was not something I felt the Association should be doing. And that was when I decided it was no longer for me.

**VDT:** That was in the early 1970s?

**DUNCAN:** Yes. So, we quit going to meetings. I didn't go to another meeting, I think, until Judith Blake asked me to come chair one of those luncheon sessions.

**VDT:** That would be 1981, when she was the president; it was the 50th anniversary meeting.

**DUNCAN:** I may have been to a meeting in New Orleans [1973] or some place, too--one or two meetings was all I ever attended after that.

**VDT:** That was what concerned you--that they were taking policy stands?

**DUNCAN:** Yes. And the other thing that concerned me was this professionalism business that got under way. It was the [Forrest] Linder committee [on organizational management, 1966-67]. I was [first] vice-president at the time [of 1966 meeting] and Cal Schmid was president and he was scared to chair the meeting, so he asked me to chair it. So, I chaired and I ruled with kind of a heavy hand. I remember too that I circulated a memorandum; maybe Ansley did too, I'm not sure.

**VDT:** This is what happened. Paul Glick wrote a vignette on the committee, in that series for PAA Affairs ["PAA Committee on Organizational Management: 1966-67," PAA Affairs, Summer 1982]. The committee was formed in 1966. It really was formed to decide what should be done about the increased membership; the membership had tripled with Don Bogue's promotion of Demography. They had to have a paid business office, at least that was a consideration. But the committee came up with the idea of active recruitment of many more members, establishing two classes of membership, professional standards for membership qualifications, and a small grants program for research. Now here was your answer, in a letter to Paul Glick, quoted by him in his vignette. I've got it marked [on this copy].

**DUNCAN:** "A general hazard in the recommendation to formulate professional standards." He [Duncan] also wrote: "Horror of horrors, that the PAA should commence a small grants program (with whose money?). I am not personally aware of projects so small that any of several present granting agencies would not consider them." [Laughter]

**VDT:** Simultaneously, Ansley Coale had written--he was the president-elect--and he violently objected to the committee's ideas and said he would not carry them out when he was president. So that whole thing was shot down.

**DUNCAN:** So for a year or two, we managed to hold the fort. But it came through anyhow, later on.

**VDT:** What do you mean--professional qualifications for membership?

**DUNCAN:** Well, the whole bureaucratization of the organization--creation of a newsletter, having professional historians write on the history . . . [Laughter]

**VDT:** I'm not a professional historian, and it's all at my own expense, I'll have you know. I'm a demographer; it's my own interest.

**DUNCAN:** All these accouterments of an organization--a staff in Washington and all those things. It overtook all the organizations. It happened first with the American Sociological Society, which even had to change its name. It became the American Sociological Association. [Paul Glick, in his interview, said the change was made to escape the acronym ASS.]

**VDT:** What's the difference between a society and an association?

**DUNCAN:** A society is--is a large society. It's where a group of scholars or scientists meet and discuss their scientific or scholarly problems. An association is a professional organization where people meet to forward their interests as employees, functionaries, and so on.

**VDT:** But PAA had been association from the beginning--the name.

**DUNCAN:** Well, that's the word; the name, of course, is inconsequential, but the concept of the PAA was that of a large society. They fought with Margaret Sanger about that and beat her down. They said, "We will come and we will have scientific sessions. Period." Then we started having resolutions about public policy issues and we started having committees for this and officers for that and so on. The whole thing ballooned into what you now have--a mass membership group that forwards the professional interests of some category of people called demographers, rather than confining itself to the scientific interests of a discipline called demography. It's a very distinct thing. I couldn't get that

through Cal Schmid's head. I'm sure a lot of people resist the distinction. It's very real. The historical transition was being made in this period. And a large number of organizations have made it at different times, at different rates. It was confounded and mixed up with the unrest of the civil rights movement and all those student riots, things of that kind that came along about the same time; it was part and parcel of that. I suspect those things accelerated it, in various organizations--women's caucus and black caucus, this, that, and the other thing--using the organization as a vehicle for interests other than the scientific analysis of the subject matter. I'm proud that I stopped it for a little while, but it was something that one couldn't do indefinitely. I didn't have the power base or influence after I was out of office.

**VDT:** Well, thank you. I'm glad you said that. It answered some questions that I had. I want to talk a bit more about your presidential address, "Inequality and Opportunity." Obviously, it resulted from your research. I was struck by your pointing out that racial discrimination was the greatest factor in unequal intergenerational mobility between blacks and whites--the gap between black and white earnings--and that discrimination had to be attacked directly, because public policies in education and occupation wouldn't eliminate the gap otherwise. Was that because civil rights was such an issue at that time?

**DUNCAN:** No, that goes back to the Moynihan poverty seminar--the idea that there was a cycle of poverty, that poverty was destined to recapitulate itself in every generation.

**VDT:** Culture of poverty.

**DUNCAN:** Culture of poverty. And our work had not suggested that was true. It showed quite the contrary; that our older generations of people got out--they moved up. Of course, some moved back down. That meant just the opposite of the cycle or culture of poverty--except that generation after the generation, the blacks were clearly disadvantaged. So I wrote the piece called, "Inheritance of Poverty or Inheritance of Race?" The principle of a cycle that is destined to recapitulate itself was applicable to the disadvantage of blacks but not to the disadvantage of other people who happened to be poor in any particular generation. Most of them get out of poverty. That's the main thing that happened; if they're in poverty they get out. Some dropped back down--there's circulation. But with blacks, it has a very different and additional mechanism, a discrimination mechanism. I was trying to use our research to sort of calibrate how that was working.

So I wrote a paper in Moynihan's volume and the speech to the PAA merely recapitulated some of those results. It was just a summary of my work on that topic--maybe the last thing, or one of two or three things that I wrote, trying to sum up what I thought I had learned from a decade of work on that topic.

**VDT:** It dealt with social mobility, which figures in that famous definition of "what is demography"--the field of demography--in The Study of Population. That's not a very popular aspect of demography now. Why do you suppose that is?

**DUNCAN:** I don't know. I haven't had anything to do with demography, or social stratification, for a long time.

**VDT:** Was it a part of the field in the 1960s? Obviously, not a focus, because, of course, fertility, mortality, and certainly migration--but social mobility . . .

**DUNCAN:** Well, you see, I learned to think about population as having four parts--fertility, mortality,

distribution, and composition--because, after all, the census collects data on age, sex, occupation, this, that, and the other thing. So we get a snapshot every ten years of the composition of the population. If you look at distribution, that's where people live. The dynamic aspect of distribution is migration--people move around. The dynamic aspect of composition is social mobility. That's just a logical, kind of a taxonomic thing.

Now, social mobility often has a somewhat narrower meaning of going up and down the social scale. But it can be extended, as did Sorokin in his book, Social Mobility. He extended it to cover all changes of any kind of status, whether socioeconomic or civil status. Some changes that we make, like from being a minor to being an adult, are irreversible, of course. Others are changeable; you can move back and forth, be a Democrat and then a Republican. There's nothing logically distinct about enumerating the population according to political party or religion as compared to enumerating them by marital status or something else. They are all definable characteristics of individuals which you can aggregate and get statistical distributions for them. And you can do them at more than one point of time. It's better than not having them. It came about in The Study of Population just as a kind of logical consequence of thinking through what is the subject matter of demography. It had no necessary relationship to my interests in occupational change. And if people don't see it that way, that's their tough luck. [Laughter]

**VDT:** Thank you. I guess social mobility is tainted with the idea of the struggle of the blacks, perhaps.

**DUNCAN:** Yes. Well, all these things get ideological appendages. That's why we can't do much with social science concepts. As soon as the social scientists work out a way of ordering their data then the larger community picks it up and uses it for propagandistic and agitational purposes. That makes it questionable whether you can have a social science. That's why I say I'm not sure I like the fact that I've spent my life being a social scientist. I think it's an incoherent enterprise.

**VDT:** Well, if everybody were not to work on things that were liable to be attacked, I guess a lot of things wouldn't get done. But okay. On a lesser level, at the same dinner meeting where you gave your 1969 PAA speech, Frank Notestein gave his wonderful tribute to Fred Osborn, who was celebrating his 80th birthday ["Frederick Osborn: Demography's Statesman on his Eightieth Spring," Population Index, October/December 1969]. I gather you invited Fred Osborn to come, specially, to that meeting?

**DUNCAN:** Well, those guys put the arm on me. They said they wanted to have a tribute for Osborn.

**VDT:** Who put the arm on you?

**DUNCAN:** It wasn't Frank directly. Somebody proposed that and I acceded. I respected him, of course. I was glad to give up my time to him; I wanted to make a short speech anyhow. It might have been Dudley Kirk--one of those people in the Notestein circle spoke to me about that [see Duncan's afterthought below].

**VDT:** It could have been Dudley Kirk, whom I interviewed two days ago. He's always been very history-minded. He's the one who in recent years has invited those who are over 70 and more than 35 years a member of PAA to have a little extra gathering at PAA meetings, a dinner. Kingsley Davis always boycotts those; he considers that age discrimination. He refuses to have anything to do with his age peer group.

Fred Osborn--who had done so much for the field, moneywise and otherwise--you in your talk referred to his book that had just come out [The Future of Human Heredity, 1968], one of the latest

books on eugenics, in a sense, which, of course, wasn't considered . . . Well, of course, now everybody cringes when anybody connected with demography gets labeled as a eugenicist. Yet some of the early people in the field, like Fred Osborn . . .

**DUNCAN:** And Frank Lorimer. I took a little interest in that about 1950 and wrote one paper on whether the intelligence was declining.

**VDT:** I read that ["Is the Intelligence of the General Population Declining?" American Sociological Review, August 1952].

**DUNCAN:** Lorimer and Osborn had concluded that was true. But all the direct measurements of change in IQ scores showed they have gone up, rather than coming down. So I wrote a paper summarizing that, commenting on it, and had a little exchange with Lorimer in the columns of the ASR [American Sociological Review].

**Note: From Dudley Duncan to Jean van der Tak, letter written after this interview on the same day, May 3, 1989.**

"In the interview you asked how it came about that a tribute to Mr. Osborn was included in the session of the PAA where I gave my presidential address. I indicated that someone connected with him called me and made the suggestion, possibly Dudley Kirk. Now the name has come to me. I feel sure it was Parker Mauldin. I believe he was at the Population Council at that time, but am not sure of that. Nor do I know what his connection with Osborn was, possibly not direct but via one or another demographer who did have some association with Mr. Osborn.

"Osborn was part of the 'old guard' that included Lorimer, Notestein, Kiser, and others who ran the PAA when I became a member. Later a group of us, led by Margaret Hagood, put through a reform of the constitution involving a less restrictive nomination procedure and mail ballot for officers. Margaret used to write letters that began, 'Dear Dan, Don, Dudley, Phil, and Rupert'--that is, Dan Price, Don Bogue, Dudley Duncan, Phil Hauser, and Rupert Vance. Despite this modest venture in insurgency, I had great respect for that old guard, including Mr. Osborn, with whom I was on cordial terms. At one time I served as VP of the Eugenics Society, until I learned that it was not an annual post but a perennial one, when I proceeded to resign."

---

**VDT:** In 1969 when you were PAA president, Beverly became editor of Demography for three years, following Donald Bogue. Don Bogue wrote about his tenure as Demography editor in a PAA history vignette on "How Demography was Born" [PAA Affairs, Fall 1983]. I've been responsible for all these vignettes. I pursued Don for a couple of years to get him to write this vignette about his rather controversial tenure, that ended with his big thick issue on family planning with the red inverted Indian family planning triangle on the cover ["Progress and Problems of Fertility Control Around the World," special issue of Demography, Vol. 5, No. 2, 1968]. That created a lot of stir, because--in the vein you were pointing out--here was the beginning of PAA's scientific role evidently yielding to advocacy--advocating family planning. That was the general feeling of the membership at that time.

**DUNCAN:** Yes.

**VDT:** At the end of his piece, he'd said: "Dudley and Beverly Duncan, as the second editorial team,

were very instrumental in shaping Demography into the stance it has today." You pointed out, rightly--and I should have checked as editor of his piece--that it was Beverly who was the editor.

**DUNCAN:** It was hers entirely. She consulted with me about whether she should take the job.

**VDT:** Did Don ask you, ask Beverly, or how did that come about?

**DUNCAN:** I don't remember. I don't think I was asked, but if I was, I turned it down. [Paul Glick says, in his interview: "The first editor after (Donald Bogue) was Dudley Duncan's wife Beverly. She was my pick for that. I thought she was a businesslike person and would do a good job. And she did."] She asked if I thought it would be a good thing to do and I said, "Well, if you want to do it." It seemed to me it was a professional contribution that was important to do, a worthwhile thing to undertake, so if she wanted to do it, she shouldn't hesitate to accept. She pondered that in her own way and decided to do it, without asking my permission or anything else.

I had no more to do with the editing of that than any other colleague at Michigan, perhaps a little less. She used all of us very sparingly as article referees and advisers--only if something came along that was directly related to our own interests. Otherwise she went outside to other people--Ansley Coale or Nathan Keyfitz or whomever she could find who was a good referee. When she got their reports, she would read them carefully and then would make up her own mind. She didn't do what many modern editors do, just abrogate the responsibility of making the publication decision to the advisory editors. It infuriates me when I encounter an editor who won't tell me what he thinks of my paper; he will only quote what his anonymous referees have told him. I think that's a shabby way to run a journal. It doesn't make a good journal; it makes a mediocre journal. Editors should have strong ideas about what is good work and should follow that policy. And if an organization doesn't like those ideas, it should get another editor. That makes a good journal. The other way is to have a journal that takes anything that comes in, but it's some kind of bureaucratic stuff that always averages out everything; it guarantees homogeneity and mediocrity. So Beverly had a very distinctive style there. She typed up the tables herself or at least designed them so they would be economical to reproduce from the typescript, rather than typesetting.

**VDT:** That early on!

**DUNCAN:** She abolished footnotes--to the great pain of some of our wordy colleagues who like to be lazy when they write. They cost money, to set those footnotes at the bottom of the page, so she said, "Well, you can have references, but no footnotes."

**VDT:** Just putting the author name and publication year in parentheses beside the place they were referred to and having the publications listed alphabetically by author at the end?

**DUNCAN:** Yes, scientific style, but eliminating the footnotes.

**VDT:** Norman Ryder claims credit for starting that style, when he was not an editor of Demography but of something else.

**DUNCAN:** Yes, perhaps. That's probably true. Other journals were adopting it.

**VDT:** It must have been a lot of work. You mentioned that she taped a little piece on . . .

**DUNCAN:** "Sending Copy." I made a copy for you to take home. ["Sending Copy," by Beverly

Duncan. Uses the sound of a clacking keyboard to depict the increasingly frantic pace of getting copy out, on time.] She had rather high standards, so it was never clear that she was going to have a full issue on time. It was a last-minute photo finish to see whether there would be a complete issue. And then, of course, getting all that ready for the printer. She wanted to maintain her deadline; she didn't want to fall behind of a publication schedule, the way many editors do. So she was very conscientious and she always found that stressful as the deadline approached.

**VDT:** Was she trying to do it all herself or did she have someone else helping with the typing, copyediting?

**DUNCAN:** She had a secretary at the Population Center, Mary Scott, who was typing the tables. Beverly would block them out. I don't remember for sure, but Mary probably typed them. But Beverly did a lot. She did an interesting thing once. Ansley had a Japanese student, who was a very good demographer, I guess, but just totally inarticulate in English. This fellow submitted an article and Beverly surmised that it was at Ansley's direction, so she contacted him to find out if he thought the work was sound and he said, yes, he thought so. Well, she simply rewrote the paper, put it into English, and sent it to Ansley and said, "Have I got the demography right? I tried to fix the English, did I get the substance wrong?" And he said, "No, this is just right." So the paper was published as a contribution by this Japanese student, but Beverly was in effect the second author.

**VDT:** I know all about that as editor of the Population Bulletin at the Population Reference Bureau. I rewrote several--a lot.

**DUNCAN:** Those things are not advertised, but this is what a conscientious editor may do. A less conscientious editor may send it back and say, "Revise and resubmit." But she wanted good stuff and when she thought she had something good, she'd work hard to put it into shape so it could go through.

**VDT:** And she did all this along with her regular job?

**DUNCAN:** When she got critical reactions, she didn't just use them to crush the author, but she would endeavor to work with the author if she felt there was something there that could be salvaged or developed. She would work with the author in a gentle, non-directive way, but try to get the author to correspond directly with the critic--not through the intermediary of the editor--and say, "You two guys get in contact. I don't fully understand the issues here. If you talk to each other, you'll reach agreement much sooner than if I try to mediate."

**VDT:** How interesting! That's rare.

**DUNCAN:** She did innovative things that were not advertised but I was seeing that made it into a quality journal. That was her objective--to get rid of those fat volumes that Bogue had put out that included lots of slop and low-quality stuff. I think that set the tone for the rest of the history of the journal. I don't know what it's like now; I haven't seen it . . .

**VDT:** You don't read it anymore?

**DUNCAN:** No. I quit reading it a long time ago.

**VDT:** Do you read anything in population? I guess not.

**DUNCAN:** I read nothing in social science. I quit reading almost everything except what was directly

related to my research, oh, maybe ten years ago. I started reading the classics, Greek and Roman literature in translation.

**VDT:** In your field, because, of course, you used those extensively in Notes on Social Measurement.

**DUNCAN:** Yes, it was a coincidence. I'd just started doing that at the time I got the invitation to write the Notes. I had thought of someday writing on the history of statistics or social measurement. But I was not ready to start that, hadn't decided when I would start, when Merton called and asked if I would write on social measurement. Quite independently, I had gotten interested in reading poetry and had heard about a guy named Homer, so I thought I better go read Homer and, sure enough, I liked it. I was lucky to hit the Fitzgerald translation of the Odyssey and it was just beautiful; I was enthralled by it. So I had launched onto the pursuit of reading of that kind at the same time that this invitation came along, and I saw that the history of social measurement went back to the very people I was reading. So I began to make notes about my reading and included those in places. I think it gave the book the historical depth that it wouldn't have had otherwise.

**VDT:** It's called, Notes on Social Measurement: Historical and Critical.

**DUNCAN:** Yes.

**VDT:** It's a very interesting book. I can't say I followed it too well.

**DUNCAN:** There are some parts that are very easy to follow and other parts are specialized.

**VDT:** You had switched your interests to, well, this broader field already while you were at Arizona [1973-83] and while you were here at Santa Barbara [1983-87; then emeritus]. Were you still teaching sociology when you came to Santa Barbara?

**DUNCAN:** Yes, I taught here until 1987, several things.

**VDT:** You were teaching general sociology, or specialized?

**DUNCAN:** I didn't have much chance of teaching my specialty of statistical methods of survey analysis, but I taught an undergraduate course in research methods; I taught a course in stratification several times. I don't think I taught social change, but I gave a few seminars to a few students. I didn't have a very successful teaching career here.

I had a great teaching program at Arizona, had a sort of key course in the graduate sequence. It was the second course in statistics, but I turned it into a course in the methods of analyzing qualitative categorical data, log-linear models and the like, and then used it kind of as a warmup in how to write research reports. I had the students write a short report every week that got them introduced to the idea of how you get results and then report them and discuss the implications and their bearing on ideas of the discipline. By the end of that course, they had enough confidence to start a master's thesis or something of that sort. I trained a lot of people and had some good students there. So that was very successful.

When I came here [University of California at Santa Barbara], they already had people who were doing the statistics. They didn't want to let me in on that; I taught the course once. And I did undergraduate teaching; I didn't have great success with that. This is a lousy university, by the way, a shabby excuse for a university. The reason for coming here was to have a good climate to retire in. It has a good climate and I'm retired. [Laughter]

**VDT:** Perhaps that's as good a place as any to stop. We've already talked about your most interesting pursuit in composing electronic--just--this is a just notification?

**DUNCAN:** Just intonation.

**VDT:** Do you think your musical interest has grown out of your mathematical bent?

**DUNCAN:** Well, just intonation calls on your skills in algebra and in fractions and logarithms. It's still a modest amount of mathematics, but there is a mathematical aspect to music. So I get some fun out of that. But that is not where it began; it began as a social activity. I just learned a little music, read about it, listened to it, collected records. Over there . . .

**VDT:** Two shelves-full!

**DUNCAN:** That's a small part of my collection. It's a life-long interest.

**VDT:** You know, there are several demographers who have this musical bent. Just among those I have interviewed, there is Jack Kantner, whom I interviewed in Bedford, Pennsylvania, where he's retired. He has the trumpet in his background--one of the horns; he's the director of the annual music festival in Bedford and his son is in the Grand Rapids orchestra.

**DUNCAN:** Good!

**VDT:** Then Sam Preston, one of the younger demographers, writes country and Western music, quite seriously. He's sent out several pieces, hoping to get published.

**DUNCAN:** More power to him, that's fine.

**VDT:** And Paul Demeny was a bassoonist in his youth.

**DUNCAN:** That's another who distinguished himself. He was a colleague at Michigan.

**VDT:** He was offered a job in the orchestra in his hometown and was seriously considering it, because it wasn't sure that he would get into university in Hungary under the Communist regime because his father had been a judge. But he did get to university and gave up the bassoon.

Then, of course, you know about Joe Stycos and his piano. He's a jazz pianist from way back. And Lee Bouvier, a good friend of mine from his Population Reference Bureau days, made his living as a jazz trumpeter until he was 33 and went back to university. So I think perhaps there's a connection.

You said you've written your autobiography, is it?

**DUNCAN:** This is a short statement that was written for the National Academy of Sciences. It's kind of a pre-death obituary. [Laughter] The Academy publishes memoirs about its extinct members. When they die, somebody has to write up their achievements and so on, so now they have members write about themselves before. Somebody will rewrite it, I guess, when I die. It was written at the time I was elected, in 1973.

**VDT:** Just like Sam Preston. There are not too many demographers in the Academy.

**DUNCAN:** Jane Menken was just elected; I got the notice yesterday.

**VDT:** Oh, fantastic! She and Sam have usually been neck-in-neck in their careers.

**DUNCAN:** Ansley Coale is a member and, obviously, he has been instrumental in bringing their work to the attention of the scientific community at the Academy. One of the first members was Kingsley Davis. Demographers got in early because they had the quantitative methods and a relation to the biological sciences. That was the foothold for the social sciences.

**VDT:** Isn't Nathan Keyfitz also a member?

**DUNCAN:** I'm not sure. There's the problem about Nathan's citizenship; he was a Canadian citizen for a long time.

**VDT:** Born there, of course. I expect now he's a naturalized American.

**DUNCAN:** I think he is a member [correct].

**VDT:** You're the first one who's told me about this pre-obituary autobiography for the NAS. Is there anything you can write about music, the work you're doing, or will you just leave that record in the computer?

**DUNCAN:** I have a forthcoming tiny article, my first article in music theory, called "Septimal Harmony for the Blues." That means you use ratios that have sevens in them--seven/four, seven/five, seven/six. The blues typically flats the seventh, the fifth, and the third. My argument is that it not only flats them, but it makes them in a certain ratio of the seven variety--seven/four, seven/five, or seven/six. I took the "St. Louis Blues" and put it into just intonation with these septimal ratios and it sounds better to me that way than any other way. That article comes out in the journal of the Just Intonation Network, called 1/1. There are a little less than 200 members.

**VDT:** And there is your guru, whose name again is . . . ?

**DUNCAN:** Partch. He died about 1974. Subsequently, a group of young San Francisco musicians got interested in pushing this and they formed the organization, so I work with them a bit.

**VDT:** Is that one of the reasons you came to California, besides the climate?

**DUNCAN:** No, it happened after I came here. I loved Harry Partch's music, but I didn't understand the theoretical basis of it until just a few years ago when I read his book. And by coincidence, I saw a notice about the organization, so I joined up with them and started going into this subject.

**VDT:** Well, that is a very good reason for being here, even if the university is not . . .

**DUNCAN:** It's been fortunate to be in California without coming for that reason, but, yes, you're right.

### **Addendum**

**VDT:** A little afterthought; there is nearly always an afterthought in these interviews. There is a

picture of Dudley with a research colleague at Arizona, who composed an "Ode to O.D.D." in his honor.

**DUNCAN:** She's the wife of my research assistant at Arizona. They're both now at Tulane University and she teaches music and he teaches sociology. She had written this composition at South Carolina and it was performed in New Orleans in November 1988, along with some other work of hers.

**VDT:** And beside this, there is a picture of his daughter, from Dudley's first marriage [to Beverly, who died in 1988, prior to this interview].

**DUNCAN:** Eleanor Duncan Armstrong. Her husband is Dan Armstrong and they are both assistant professors of music at Pennsylvania State University. She's a flutist; he's a percussionist. They're called the Armstrong Duo when they play together. And she's also in the Pennsylvania Quintet at Penn State, the usual woodwind quintet, and she's kind of taken the leadership there.

**VDT:** They play all over the state?

**DUNCAN:** They play in that region of the country, in Ohio and Pennsylvania and so on. They're making an expedition to Connecticut sometime. They will in the future have a broader venue, I'm sure, because they're just becoming well known; they've been in existence only about five years. They're about to issue their first recording and about to commission a composition for the first time.

**VDT:** Obviously, there's a strong musical bent in your family. And you kept up your interest through the years, but you were kind of fighting it, I guess. That's interesting.



## IN MEMORIAM

Otis D. Duncan  
Professor of Sociology  
UC Santa Barbara  
1921-2004

Otis Dudley Duncan, one of the most influential sociologists of the 20th century, died of prostate cancer in Santa Barbara, California, on November 16, 2004. Duncan was instrumental in advancing the discipline of sociology through the use of advanced quantitative methods. Duncan was “the most important quantitative sociologist in the world in the latter half of the 20th century,” said Leo Goodman, University of California-Berkeley.

Duncan’s best-known work is a 1967 book that he coauthored with the late Peter M. Blau, *The American Occupational Structure*, which received the American Sociological Association Sorokin Award for most distinguished scholarly publication (1968). Based on quantitative analyses of the first large national survey of social mobility in the United States, the book elegantly depicts the process of how parents transmit their social standing to their children, particularly through affecting the children’s education. This work was subsequently elaborated by Duncan and other scholars, to include the role of cognitive ability, race, and other factors in the transmission of social standing from one generation to the next.

The book’s impact went far beyond its analyses of occupational mobility. Using survey data and statistical techniques, the book showed how an important sociological topic could be analyzed effectively and rigorously with appropriate quantitative methods. The work helped inspire a new generation of sociologists to follow and pursue quantitative sociology.

Robert M. Hauser, University of Wisconsin, said, “The most important thing about Dudley Duncan’s studies of social stratification was not the specific findings, though they have stood up well across the decades, but that they provided a framework for cumulative scientific work that challenged, extended, and compared those findings across time and place.”

Duncan introduced “path diagrams,” “path models,” and “path analysis” to the discipline of sociology, and he used these statistical tools in the Blau-Duncan book and his other studies of social stratification. Path analysis was first invented by Sewell Wright, a renowned biologist and evolutionary theorist. A path diagram and a corresponding path model describe a set of equations summarizing complex scientific ideas in terms of statistical relationships. Jointly with Arthur Goldberger, an eminent econometrician, Duncan worked on the relationship between path analysis and other statistical methods in the social sciences. They showed that path analysis models were closely related to the simultaneous equations models of economics and the confirmatory factor analysis of psychology. These three different ways of analyzing certain kinds of data can be included within a single general framework, called “structural equation models.” Today, structural equation models are widely used.

After contributing to the development of structural equation models, Duncan worked on other advanced quantitative methods for use in sociological research. In particular, he contributed in important ways first to “loglinear methods,” which are now used widely in the social sciences, and then to “Rasch models,” which were introduced by George Rasch, a Danish statistician, for educational testing. Duncan’s research pertaining to loglinear methods and their application included many articles on important sociological topics (1974 to 1985); and his research pertaining to Rasch models and their application also included many articles on important topics (1983 to 1990).

Duncan’s sociological interests were wide-ranging and evolved over time. His 1959 survey of demographic research (with the late Philip Hauser) literally defined the field of social demography. With Harold Pfautz he translated Maurice Halbwachs’ classic *Morphologie Social as Population and Society: Introduction to Social Morphology*. He invented a measure of the social standing of occupations (the Duncan Socioeconomic Index). With Beverly Duncan, he introduced an index of residential segregation between whites and blacks and conducted a thorough study of racial segregation in Chicago. His studies in Human Ecology culminated in a methodological book, *Statistical Geography: Problems in Analyzing Areal Data*. Together with a group of graduate students, the Duncans mapped out the hierarchical economic and social relationships among metropolitan areas and between those areas and their hinterlands. They also carried out pioneering research on changing gender roles in America. In the 1970s, Duncan led the development of indicators of social change in America. Before his retirement in 1987, he devoted almost all his attention to fundamental issues in social measurement. The main product was the 1984 book *Notes on Social Measurement*, which in his own estimation is his “best book.” He was also very proud of his most fully developed mathematical-theoretical article, which presented a solution of a problem that had vexed some of the leading social scientists of the time: “Why do people’s verbally expressed attitudes so often seem unrelated to their actions?”

Duncan established a new intellectual tradition in sociology that built on a longstanding tradition in demography. While some sociologists earlier tried to model sociology after physical science, Duncan was disdainful of the search for supposedly universal laws of society that would mimic those of physical science. The central tenet in Duncan’s new paradigm for quantitative sociology is the primacy of empirical reality. Quantitative tools would not be used to discover universal laws that would describe or explain the behavior of all individuals. Rather, quantitative analysis summarizes empirical patterns of between-group differences, while temporarily ignoring within-group individual differences. Examples include: socioeconomic inequalities by race and gender, residential segregation by race, inter-generational social mobility, trends in divorce and cohabitation, consequences of single parenthood for children, and rising income inequality. Over time, social scientists can improve their understanding of the world by incrementally adding greater complexities to their analyses.

Duncan was a legendary mentor to graduate students. Many of his former students went on to make important contributions to quantitative sociology, and to have highly successful careers as sociologists. In reflecting on his career just before his death, Duncan remarked about himself that “of all his achievements, he was most proud of the record of outstanding achievement in quantitative sociology racked up by so many of his former students.”

Duncan received numerous awards and honors. He was elected to membership in three learned societies: The National Academy of Sciences, The American Academy of Arts and Sciences and The American Philosophical Society. He was also awarded honorary degrees by the University of Chicago, the University of Wisconsin, and the University of Arizona. And he was President of the Population Association of America in 1968-1969.

Duncan was born on December 2, 1921, in Nocona, Texas. He received most of his precollegiate education in Stillwater Oklahoma. He completed his BA at Louisiana State University in 1941 and his MA at the University of Minnesota in 1942. He then served three years in the U.S. Army during World War II before completing his PhD degree in sociology at the University of Chicago in 1949. He was on the faculty in the Departments of Sociology at Penn State University, the University of Wisconsin, the University of Chicago, the University of Michigan, the University of Arizona, and the University of California-Santa Barbara.

After retirement in 1987, Duncan was active in electronic music composition, in writing articles on music theory, and in the design of computer graphics.

Before his death, Duncan briefly returned to quantitative research. He wrote articles on the prevalence of creationism, the rising public toleration of atheists, the increasing number of people who specify “none” as their religion, the increasing public approval of euthanasia and suicide for terminally ill persons, and on some controversial statistics regarding gun use.

Survivors include Dudley Duncan’s wife Beatrice, his two sisters, Mary Anne Stone and Barbara Doze, and his daughter, Eleanor Duncan Armstrong, an eminent flutist based at Pennsylvania State University. Earlier in his life Dudley Duncan was married to Rose Mary Tompkins, and Beverly Davis, both now deceased.

Leo A. Goodman, Professor Emeritus, Sociology, University of California, Berkeley

Yu Xie, Professor of Sociology, University of Michigan

David L. Featherman, Professor of Sociology, University of Michigan

Robert M. Hauser, Vilas Research Professor of Sociology, University of Wisconsin

Halliman H. Winsborough, Emma Welsch Conway-Bascom Professor of Sociology, University of Wisconsin



---

Inequality and Opportunity

Author(s): Otis Dudley Duncan

Source: *Population Index*, Oct. - Dec., 1969, Vol. 35, No. 4 (Oct. - Dec., 1969), pp. 361-366

Published by: Office of Population Research

Stable URL: <http://www.jstor.com/stable/2732518>

---

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



*Office of Population Research* is collaborating with JSTOR to digitize, preserve and extend access to *Population Index*

JSTOR

## CURRENT ITEMS

INEQUALITY  
AND  
OPPORTUNITY

Let me begin with definitions of the terms appearing in the title. Inequality refers to the dispersion of the distribution over a population of any of the rewards and status distinctions conferred by a society on its members — such as income, wealth, level of living, leisure, prestige, recognition, power, authority, skill, information, civil liberties, welfare, or life chances. Only some of these items have been so quantified that we can compute an actual measure of inequality, and for even fewer of them do we have replicated measurements taken periodically. According to Herman Miller's analysis, there has been no trend toward either increasing or decreasing inequality in the distribution of income during the past two decades, although there may well have been some reduction in income inequality between 1929 and the end of the second World War. <sup>/1</sup> Incidentally, the failure of that trend to continue was correctly forecast by Joseph J. Spengler in 1953. <sup>/2</sup> We have no time series, as we have for income, on the concentration — that is, inequality — of the distribution of power in American society. The lack of such measurements has not prevented the formation of strong opinions on the subject by commentators and social critics.

Opportunity refers to the probability of finding one's self at a high, medium, or low position on any of the scales of status or reward just mentioned, insofar as that probability is a function of the circumstances of birth and rearing in a given family of orientation, locality, region, cultural or ethnic group, or social milieu. Restriction of opportunity is synonymous with social stratification, the essence of which is the inter-generational transmission of differences in status, whether of greater or lesser degree.

Stratification, therefore, could be lessened in two ways: first, by reduction in inequality itself — as in the lowering of Gini's coefficient, defined for the Lorenz curve of income distribution — for in this case there is less difference in status to be transmitted between generations; and, second, by decreasing the magnitude of the regression (or other appropriate measure of relationship) of filial on paternal status, such as a reduction of the degree to which the schooling of members of a birth cohort depends on the race, rural-urban residence, and schooling of their parents.

The measurement of opportunity is complicated by the factor of growth, which produces a rise in the mean level of achieved status over time, between generations, and in successive cohorts. American white men born in 1900-1904 received an average of 9.4 years of schooling, as compared with 7.4 received by their fathers, a difference of 2.0 years. For those born in 1930-1934, the corresponding figures were 12.0 and 8.7, produc-

---

Editor's Note. — This is the revised text of the address delivered by Otis Dudley Duncan, University of Michigan, President of the Population Association of America, at the banquet on the evening of April 11, 1969, at Atlantic City, New Jersey, as part of the annual meeting of the Association.

ing an intergenerational difference of 3.3 years. /3 This may have been a cohort for which the intergenerational difference was near the peak for all cohorts born in the twentieth century, inasmuch as the acceleration of educational attainment for cohorts born in the first third of the century will be echoed in an acceleration in the attainment of the parents of those born in the succeeding third.

However one may project the long-run trend for the future, it is clear that all our measurements for the past are affected by upward mean shifts. The consequence is that intergenerational status comparisons generally reveal a greater prevalence of upward, than of downward, mobility. An estimate for 1962 is that 25 per cent of men aged 25 to 64 had experienced long-distance upward mobility, while another 25 per cent underwent short-distance movement up the occupational scale. Some 28 per cent were at very nearly the same occupational levels as their fathers, while only 15 per cent had undergone short-distance downward mobility and seven per cent long-distance movement down the scale. /4 Thus, fully 50 per cent of the population clearly experienced upward mobility while only 22 per cent clearly experienced downward mobility. Yet the high prevalence of upward mobility in this population does not gainsay the reality of stratification, that is, restriction of opportunity. One way to express it is by means of the regression coefficient of 0.4, which measures the degree to which the son's status depends on the father's. Another way is to display origin-specific destination probabilities. For example, one's chances of being a professional worker, if one's father was so classified, were seven times as great as they were if one's father was a laborer: 41 per cent versus 6 per cent. /5

Such findings on the extent of intergenerational mobility and the degree of intergenerational status transmission do not have a self-evident interpretation, either from the standpoint of their explanation or from that of their consequences.

We should probably not wish to attribute the entirety of the correlation between son's and father's status to social stratification, i.e., to restriction of opportunity. For one thing, there is substantial evidence that intelligence, as we now measure it, has a rather high heritability. /6 Knowing this, one might be inclined to explain the intergenerational correlation in status achievement by parent-offspring resemblance in intelligence. Although this topic is shrouded in uncertainty, we do not have to rely wholly on conjecture. A rudimentary multivariate causal model provides a rough quantification of the respective influences of ability and parental status on level of occupational achievement. /7 The model shows their influences to be somewhat overlapping but preponderantly distinct. Meritocracy, therefore, is widespread in the American population, inasmuch as ability does count, irrespective of family background. Restriction of opportunity is likewise widespread, inasmuch as level of social origin has an influence, irrespective of ability. That both statements are true is a complication of social reality as well as of analysis. Analysts who are intolerant of ambiguity may join the ideologists, for whom questions have clear and decisive answers.

Interpretation of the facts on social mobility from the standpoint of consequences must be a bit more speculative. Given both the sizeable intergenerational correlation of statuses and the intergenerational upward mean shift in status, a man's achievement can well be limited or re-

stricted by his social origin, while yet placing him at a level rather higher than that of his origin. How he may experience this subjectively is imponderable. Certainly it is curious that an effort to measure "subjective achievement" produced correlations with objective measures of income, occupational status, and educational attainment no higher than about .4 or .5. /8 Respondents clearly do not apply to themselves with ruthless consistency the scales of success or failure implicit in a social grading of roles. Or, perhaps, they adjust their status aspirations after the fact. This much we can say with some confidence: the variable, social mobility, has proved to be a less portentous factor in the formation of intergroup attitudes and in the determination of differential fertility than had been anticipated. /9

As the concepts have been elucidated here, inequality is static, opportunity dynamic. Cross-sectional distributions serve to index degrees of inequality at a point in time, and the comparison of distributions for different points in time provides a starting point for the diagnosis of trend. Sources of inequality can be evaluated to a limited degree by the standard demographic techniques of decomposition. The paradigmatic studies by Dorothy S. Brady, by A. J. Jaffe and R. O. Carleton, and by Herman Miller suggest the advisability of decomposition by birth cohorts followed by systematic comparison of inter- and intracohort changes in distribution. /10 Annual population surveys could provide much more grist for this analytical mill, were the importance of the cohort strategy more fully appreciated.

The decomposition of changes in inequality, however, is only a step toward dynamic analysis. The notion of opportunity invites us to view social stratification as a process, one characterized by a number of causal parameters that can, in principle, be estimated. Research inspired by this point of view will not necessarily lead to a standard, compact set of simple indices, but rather to a succession of more carefully specified and elaborated models. Such work will have a decreasing degree of resemblance to classic demographic analysis, but will put increasingly rigorous challenges to our systems for compiling demographic information.

The most egregious restrictions on opportunity in American society are those associated with race. It is important to understand that what is at stake here is the intergenerational transmission of liability to discrimination on the basis of race, and only secondarily the transmission of the generally low socio-economic status of the parental generation in the black population. An illustrative result for young adult men will convey the point, although the specific magnitudes are subject to uncertainty. /11 In 1964, the earnings of Negro men aged 25 to 34 were about 55 per cent as great as those of white men of the same age. The dollar gap amounted to some \$3,000. Eighteen per cent of this gap could be attributed to the disadvantageous social origins of Negro men, indexed by the educational and occupational levels of the heads of the families in which they grew up. An additional three per cent was due to the racial differential in size of families of orientation. Some 22 per cent of the gap not already accounted for arose from differences in the realized mental abilities of white and Negro children, as revealed in standard tests. Apart from differences in family size and socio-economic level, and apart from differences in mental test scores, length of schooling accounted for two per cent of the income differential. Another 12 per cent turned on the differences between

white and Negro men in the occupations followed, net of the factors of education, mental ability, number of siblings, and social origins. Altogether, the calculation accounts for some four-sevenths of the income gap. The remaining 43 per cent is due to the fact that Negro men in the same kinds of occupations, with the same amount of schooling, with equal mental ability, having come from families of the same size and socio-economic position, had annual earnings only three-fourths as high as those of white men with the stated average characteristics of the Negro men.

To repeat, only 21 per cent of the \$3,000 income gap could be explained by the Negroes' disadvantage with respect to social origins and size of family of orientation. The remaining components reflected a cumulation of disabilities specific to racial status. An implication is that no amount of attention in public policies and programs to any single disability — whether intellectual, educational, or occupational — could by itself eliminate the greater part of the gap. Compensation for the legacy of past discrimination, in the absence of marked change in the pattern of current discrimination, could reduce the gap by only one-fifth. On the other hand, the elimination of discrimination in the current situation, while leaving Negroes handicapped with respect to the residue from disabilities experienced in the past, would produce a situation in which such a residue would move rapidly toward zero in succeeding generations. /12 The society need not hesitate to eliminate racial stratification on the supposition that such good work will be undone by a putative "culture of poverty."

Just as we lack firm trend data on many aspects of social inequality, so it proves difficult to establish firm estimates of the trend in opportunity. My reading of the evidence is that there almost certainly has been no increasing rigidification of the social structure of the kind that was once anticipated on the basis of such developments as the closing of the frontier, the diminution of immigration, the lessening of fertility differentials, and the decline in the role of independent entrepreneurship. Observations on both educational and occupational mobility are consistent with the supposition that opportunity — defined as above, in abstraction from the fact of growth — has remained about constant in terms of secular trend over the period since the first World War. /13

The foregoing summary describes findings which, I fancy, are "value-free" in the sense that their acceptance into a body of verified understandings about social stratification should depend on their correspondence to reality and not on the values of the scientists who developed them. But a value-free science need not be produced by value-free scientists. The motivation for our research in this field is presumably a widely shared concern with such values as a belief in human dignity and a desire for social arrangements that will encourage realization of human potentialities. The scientist as such is no expert in the formulation and appreciation of values, but he may be of service in indicating how different values can interact to give rise to ambiguity in attitudes toward social problems.

Equalitarian values in one or another version are widely held in our society. In the light of these values, many critics find cause for dissatisfaction in the current performance of our social system. Two complementary objectives of deliberate social change may be distinguished: the enhancement of opportunity and the reduction of inequality.

Some consequences or prerequisites of increased opportunity should be observed. Movement toward parity of the circumstances under which persons develop is a movement toward a situation in which the capacities and dispositions with which they are born account for an increasing share of the variation in their achieved statuses. For, as Frederick Osborn has pointed out, "trends toward the equalization of educational opportunity" will have the result that "hereditary ability wherever found will have a better chance to show itself." /14 Project such a trend to its limit, and you end up with the pure meritocracy, depicted by Michael Young as a social system with its own peculiar ways of denying satisfaction to human impulses and its own institutional vulnerabilities. /15 Another implication is that to reduce stratification presupposes the neutralization of the acquired advantages that a fortunate family might wish to pass along to its offspring, as well as the elimination of the disadvantages the less fortunate family might otherwise confer on its children. A substantial equalization of opportunity may well require an intervention much more far reaching than we have yet seen on the part of the society and of the state into the processes of nurture and early socialization, hitherto almost monopolized by the family. I do not comment on the relative importance of the values of equal opportunity and family solidarity; I merely note their incompatibility in an ultimate sense. /16

On the strategy of reducing inequality, as distinct from giving everyone the same opportunity to enjoy success — or failure — we need to know more about status and reward as incentives. If rewards are made independent of performance, that is, if failure is abolished, need we worry about the source of motivation for achievement? Or does the solution lie not in the abolition of failure — or success — but in its redefinition? Are there, in other words, rewards that can be distributed to the accomplished without diminishing the supply of alternative rewards available for distribution to the less accomplished?

One does not have to be a prophet to foresee that our present passion for demographic and economic growth will some day be superseded by a concern for population quality and ecological balance. When this inevitable adaptation is effected — inevitable, that is, barring the catastrophe that will forestall all adaptation — perhaps it will seem more natural to espouse a hierarchy of values in which our assessment of men will not depend so heavily on their ability to "get ahead."

#### NOTES AND REFERENCES

1. U. S. Bureau of the Census. Income distribution in the United States. By Herman P. Miller. A 1960 Census Monograph. Washington, Govt. Printing Office, 1966. Chapter 1.
2. Joseph J. Spengler. Changes in income distribution and social stratification: a note. American Journal of Sociology 59(3):247-259. Nov. 1953.
3. Beverly Duncan. Family factors and school dropout: 1920-1960. Final Report, Cooperative Project No. 2258, U. S. Office of Education. Ann Arbor, University of Michigan, 1965. P. 43.

4. Peter M. Blau and Otis Dudley Duncan. The American occupational structure. New York, John Wiley and Sons, 1967. Table 4.5.
5. U. S. Bureau of the Census. Current Population Reports. Series P-23, Technical Studies. No. 11, Lifetime occupational mobility of adult males: March 1962. May 12, 1964. Table 1.
6. Arthur R. Jensen. Estimation of the limits of heritability of traits by comparison of monozygotic and dizygotic twins. Proceedings of the National Academy of Sciences of the United States of America 58(1):149-156. July 15, 1967.
7. Otis Dudley Duncan. Ability and achievement. Eugenics Quarterly 15(1):1-11. March 1968.
8. Idem. Contingencies in constructing causal models. Pp. 74-112 in: Edgar F. Borgatta, Ed. Sociological methodology 1969. San Francisco, Jossey-Bass, 1969. Tables 1 and 2, pp. 78 and 91.
9. Robert W. Hodge and Donald J. Treiman. Occupational mobility and attitudes toward Negroes. American Sociological Review 31(1):93-102. Feb. 1966; Blau and Duncan, Op. cit., Chapter 11.
10. Dorothy S. Brady. Age and the income distribution. Research Report No. 8. U. S. Social Security Administration, Division of Research and Statistics. Washington, 1965; A. J. Jaffe and R. O. Carleton. Occupational mobility in the United States, 1930-1960. New York, King's Crown Press, Columbia University, 1954; Miller. Op. cit., Chapter 6.
11. Otis Dudley Duncan. Inheritance of poverty or inheritance of race? In: Daniel P. Moynihan, Ed. On understanding poverty: perspectives from the social sciences. New York, Basic Books, 1969.
12. Stanley Lieberman and Glenn V. Fuguitt. Negro-white occupational differences in the absence of discrimination. American Journal of Sociology 73(2):188-200. Sept. 1967.
13. Otis Dudley Duncan. The trend of occupational mobility in the United States. American Sociological Review 30(4):491-498. Aug. 1965; Idem. Social stratification and mobility: problems in the measurement of trend. Chapter 13 in: Eleanor B. Sheldon and Wilbert E. Moore, Eds. Indicators of social change: concepts and measurements. New York, Russell Sage Foundation, 1968.
14. Frederick Osborn. The future of human heredity. New York, Weybright and Talley, 1968. P. 103.
15. Michael Young. The rise of the meritocracy: 1870-2033. Harmondsworth, Middlesex, Penguin Books, 1961.
16. Bruce K. Eckland. Genetics and sociology: a reconsideration. American Sociological Review 32(3):173-194. April 1967.