

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Reynolds Farley PAA President in 1988



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

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

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

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REYNOLDS FARLEY

PAA President in 1988 (No. 51). Interview with Jean van der Tak at Jean's home, Washington, D.C., February 4, 1989.

CAREER HIGHLIGHTS: Reynolds Farley was born in 1938 in Harrisburg, Pennsylvania, and grew up in Akron, Ohio. He received a B.A. in liberal arts in 1960 from Notre Dame and an M.A. and the Ph.D. in sociology in 1963 and 1964, respectively, from the University of Chicago. He taught sociology and demography at Duke University from 1964 to 1967. Since 1967, he has been at the University of Michigan, where he is Professor in the Department of Sociology and Research Scientist at the Population Studies Center. He was Assistant Director of the Population Studies Center from 1969 to 1979. He was a member of the Advisory Committee on Population Statistics of the Census Bureau in 1975-81 and chair of that committee in 1980-81. He has been a Visiting Scholar at the Census Bureau, most recently in 1989, and serves on the advisory committee for the 1980 census monograph series.

Reynolds Farley is perhaps the authority on the demography of blacks and black-white demographic differentials in the United States. He is the author of many articles on the black population and three monographs: The Growth of the Black Population (1970), Blacks and Whites: Narrowing the Gap? (1984), and the 1980 census monograph, coauthored with Walter Allen, The Color Line and Quality of Life in America (1987).

FARLEY [elaborating on biographical introduction]: My father took a job with B.F. Goodrich in Akron, Ohio, at the start of the World War II boom, so we moved from Harrisburg, Pennsylvania, to Akron when I was three years old and I lived in Akron until I went off to college. My mother lived there until the last year or so. I've lived in the Midwest all my life, except for the three years early in my life and the three years I was teaching at Duke University in Durham, North Carolina, 1964 to 1967.

VDT: Currently, Ren is Visiting Scholar at the Census Bureau, here in Washington. You've followed Omer Galle, haven't you?

FARLEY: Omer was Visiting Scholar at the Census Bureau a couple of years ago.

VDT: Interesting. I'll tell you later why that conjunction is interesting. To begin at the beginning, how and when did you become interested in demography and what led to your special interest in the demography of blacks?

FARLEY: I became interested in demography certainly by my first year in college. When I was a freshman at Notre Dame, I took a course in population from Donald Barrett, who had studied at the University of Pennsylvania. The course was something about world population growth and with a great deal about U.S. population growth and differential fertility in the United States. At that time, it struck me that demography was pretty interesting.

You could ask me what kinds of things I did for relaxation or liked to read when I was in high school or even before. I can always recall reading a fair amount about history, particularly U.S. history. And I can remember as a child paying a great deal of attention to statistical compilations of trends in size of cities, when states got large, when states got small, relative size of countries--things like that. So I got some early interest in both sort of population size and U.S. historical trends. As I was in college, it struck me that demography would allow you to invoke the skills that historians would

use, as well as looking at what happened to population size. So that is when I started getting interested in population.

VDT: You certainly capitalized on those early interests, because your history of the blacks, for instance [The Growth of the Black Population, 1970], obviously was drawn from the history of the United States in general.

FARLEY: Yes, it does fit in. The second year I was in college, the Population Association was meeting in Chicago [May 1958] and that professor, Donald Barrett, encouraged me to go up to the meetings and I did. I remember I went up on a Saturday morning and came back on Saturday night; Notre Dame is only 90 miles from Chicago. That was the first PAA meeting I attended.

VDT: I was going to ask you that later.

FARLEY: 1958 was Chicago, then the next year was maybe Providence?

VDT: Right--1959. I have the list here. 1960 was Washington. 1961 was New York.

FARLEY: I didn't go to that one. Then 1962 was Madison; I was at the Madison meeting. 1963 was Philadelphia; I remember going.

VDT: Your memory for the dates is astonishing!

FARLEY: 1964 was San Francisco. I didn't get to the 1964 meetings, but I've been to all the meetings since 1965. When were the meetings in Ann Arbor [University of Michigan]? It was back quite a while.

VDT: Yes, in 1956. Ann Arbor was just that one time.

FARLEY: Ron Freedman says that the party [cocktail before the meeting banquet] was given at his house and Amos Hawley's house. They lived next door to each other and the group was small enough then that they could have a party split between the two houses.

VDT: Right. Amos Hawley told that story in his interview. Did you take demography all through your undergraduate years?

FARLEY: I think I took only one course in demography as an undergraduate, that first year.

VDT: So why did you pick it up and what took you to Chicago?

FARLEY: When I got to be a senior in college, I knew that I wanted to go to graduate school and that I was interested in population studies. So I applied to study at Chicago and at Michigan. I think I also applied to Michigan State, because I had a faculty member who said it would be quite straightforward for me to get some financial support if I went to Michigan State, but there wasn't a great deal of demography going on at Michigan State. There was one demographer who did internal migration. I wanted to go to the University of Chicago or Michigan and there was a financial . . . I guess I got some support from both places, but it looked more attractive at the time to go to graduate school at the University of Chicago. I graduated in the spring of 1960 and started in at Chicago in the fall of 1960.

VDT: And were you immediately swept up in the Population Research and Training Center?

FARLEY: Yes, I was, indeed. When I got to Chicago, I served an apprenticeship working with Dudley and Beverly Duncan. I was very pleased; I learned a great deal.

VDT: Were you working on a particular project with them?

FARLEY: It was several different kinds of projects and I worked with some of the more advanced graduate students, but it was on population distribution, particularly in the Chicago area.

VDT: That resulted in which of their books?

FARLEY: I think they had already published their "housing a metropolis" volume [Philip Hauser and Beverly Duncan, Housing a Metropolis--Chicago, 1960]. The Negro Population of Chicago [Otis Dudley Duncan and Beverly Duncan, 1957] was published even before that, wasn't it?

VDT: I think so. I've just transcribed my interview with Phil Hauser and all these books came up.

FARLEY: They were frequently doing things on population growth in Chicago and the Chicago suburban ring. When I got there, the 1960 census data had not yet become available but people were getting ready to analyze those data. We were still working on growth of Chicago's population back to World War II. I remember we were looking at neighborhoods which were close to the El lines, close to public transportation, growing more rapidly than other areas. And, of course, there was Dudley's interest in how people were distributed across the city--interest not only in black-white differences but also occupational groups, income groups, differentially distributed throughout the Chicago area. So I started working on those kinds of issues and enjoyed it very much.

VDT: And that must have led to your interest in blacks. There's a high proportion in Chicago, isn't there? What was your Ph.D. dissertation on?

FARLEY: My dissertation was entitled Negro Cohort Fertility. The stimulus there was a mix between Dudley Duncan, Beverly Duncan, and Don Bogue. Dudley and Beverly, with their quite liberal viewpoint on social equity, were very interested in the fact that in almost every indicator, blacks seemed to be worse off than white. Why was there so much segregation? Why was the occupational situation so much bleaker for blacks in Chicago than it was for whites? Beverly and Duncan would talk about that a great deal. And I recall at the time, Gary Becker, who's gone on to great fame as an economist, was propounding his ideas that the market economy is equitable and if blacks are not getting such good jobs or if the quality of black housing is inferior to that of whites, it must be because blacks don't have the abilities to get better jobs or don't have the money to buy better housing. And Beverly and Duncan would point out: Could there be systematic discrimination? Perhaps there were factors such as realtors would keep blacks out of neighborhoods. And Gary Becker would say, "That can't happen, because if one realtor keeps blacks out of an area, some other realtor will realize there's great profit to be made by introducing blacks, and so in the market economy, you can't really have discrimination." I remember those kinds of fascinating arguments and that certainly stimulated me.

Dudley and Beverly had me, as an apprentice, work on things, showing where blacks lived, showing some of the characteristics of blacks, how they differed from whites. So I got into racial issues by looking at what was happening in Chicago. If that had been an era when there was a large influx from Poland or Russia or something, I suspect Dudley and Beverly would have had students like myself looking at what was happening with European immigrants. But at the time, Chicago was

receiving a very large influx of migrants from the South. Indeed, they were getting off the train at 63rd Street, just below the university, and the area around the university was becoming largely black. There was great controversy about the university wishing to maintain its presence in a safe area as a largely white area--a fear that blacks were going to overrun the area, make whites unwelcome, increase the crime rate and so on. Even before World War II, I believe, there were discussions about whether the university should move lock, stock, and barrel, way out to the suburbs.

VDT: Was the university in the midst of the riots of 1917?

FARLEY: The riots were just north of the university. The university had a long history of involvement with studying racial and social change in Chicago. The definitive work on the Chicago riots was officially published with the author as the Commission on Race Relations in Chicago, I believe, but the book was actually written by Robert Parke and Charles Johnson, both of whom were at the University of Chicago. So the university's exposure to the risk of racial conflict was well known, I think, even before World War II. And many of the faculty members saw the university as having a kind of moral obligation not to run to the suburbs, and the university didn't run to the suburbs. But there are racial problems around the university today, which are related to the racial makeup of the area.

So, one set of the interests that I had in blacks came from the Duncans, Phil Hauser, and other people on the staff who were pointing to the dramatic changes occurring in the demography of the city of Chicago and their implications. The other stimulus I got was from working with Don Bogue, actually taking a course or two with Don Bogue. Don by this time had become very outspoken in advocating lower fertility rates around the world. Don, more than some professors, occasionally showed signs of being a bit of a missionary in the classroom.

VDT: "Missionary" is a word often used about him.

FARLEY: It was clear. Certainly, he would have been very respectful of any student who disagreed. But Don could get quite enthusiastic about efforts to reduce fertility around the world and in Chicago. I remember how Don, more or less as an aside, was talking about how blacks at one time in the United States had very low fertility. During the Depression decade, reproduction rates for blacks were close to unity and the black population was on the road to zero population growth. Then it became clear by the late 1950s and early 1960s that black fertility was very, very much higher than white fertility. And I remember Don in the classroom saying, "How can this be? We don't understand this." Here blacks had had low fertility rates and suddenly they have not just a baby boom after World War II but they go on to extremely high fertility rates.

I recognized when Don said that, or shortly thereafter, that that was a very interesting question, that would involve some historical work to determine why black fertility rates were low in the 1920s and 1930s and how come they were much higher in the 1960s than they were for whites. I guess I thought about it for a year, a year and a half, and decided that would be an interesting thing to do as a dissertation.

Don always spoke extremely highly of Pat Whelpton. Pat Whelpton unfortunately died [in 1964] before I could meet him. He had an article coming out in Demography, it must have been the first or second issue, and he died after it was submitted. I remember Don asking me to go over the galley and proofread it and make sure it was appropriate. Pat Whelpton himself, I think, could be somewhat difficult in certain circumstances, but Whelpton's cohort fertility analysis was a major breakthrough, disaggregating the tempo of fertility. And Don Bogue said it ought to be done for blacks, people assumed to have a history of fertility going from high fertility to low fertility to very high fertility. He was the one that recommended that I do a cohort analysis of black fertility, and that's

what I did in my dissertation.

By the time I got around to doing my dissertation, Dudley Duncan had left for Michigan. My M.A. thesis, which was a large project, entitled Suburban Persistence, was done under Dudley Duncan's guidance. What I was looking at there was that many suburbs seemed to get a kind of social class identity when they developed right after World War I. Some suburbs seemed to attract, in that period of five or ten years after World War I, a high-status population; others had a sort of blue-collar population; others a sort of middle-class population. Then after World War II when suburbs increased their population size dramatically, it seemed that they didn't change the kind of people who were there. Suburbs which after World War I were very high in status seemed to be very high in status as late as 1960. Why wasn't there more shifting of status?

VDT: What an interesting topic for an M.A. thesis--very ambitious! You used census data, of course.

FARLEY: Yes, census data. Dudley was the one--with a lot of help from Beverly Duncan--who put me onto that project. Then Dudley went to the University of Michigan [in 1962], so I knew I couldn't work with him for a dissertation, so I worked with Don Bogue on cohort fertility, at that time Negro cohort fertility.

VDT: Well, I should tell you that Phil Hauser--I always ask people whom they consider their leading students--had a list, many of whom he shared with Don, but he had a list of just 21 Americans, including Beverly Duncan at the top. Nathan Keyfitz was an afterthought, because Keyfitz came in middle life, just to add a Ph.D. to his already illustrious reputation. You are on that list, and you and Omer Galle are next to each other on that list. Just 21--those who've gone on to outstanding careers afterwards. So Phil considers you a leading student of his too, but you actually did your dissertation with Don Bogue.

I must say that Hauser said that Don Bogue liked to claim that any student who worked with him was his student and his student only. However, Hauser pointed out that you all got your degrees from the department of sociology and you did take courses in the two different centers with the professors who were there.

FARLEY: Yes. Well, Phil was very helpful when I was at the University of Chicago. He was a large presence and probably still is a large presence today. You knew that Phil was around. Phil would say things to you that even as a graduate student you didn't agree with and once in a while you were suspicious about the validity of some of the things he said. Phil could extemporize and exaggerate a bit. But he was a very important person and very supportive. What was unusual was that Phil was, I thought, extremely supportive of graduate students at all levels at a time when he didn't particularly need to be. I mean, he was an important figure in the University of Chicago's system; he was an important figure in national politics. When I was there, he was at one point very interested in becoming commissioner of the schools in the city of Chicago.

VDT: He ran for office?

FARLEY: No, that was an appointive office. But he was quite interested in that, before Ben Wallace got the job. So Phil had a public presence that went beyond that of even most of the faculty at the University of Chicago, many of whom, like George Schultz, have been public presences all the time. But Phil was very supportive of graduate students at all levels.

VDT: But you didn't have as close relations with him as you did with Don Bogue?

FARLEY: That's right.

VDT: Let's talk a bit more about Dudley and Beverly Duncan, who were, of course, enormous presences in U.S. demography. I hope to interview Dudley on my trip to California in April-May. He's now retired in Santa Barbara and Beverly died about a year ago.

FARLEY: Beverly and Duncan were extremely important for me. When I first got to Chicago, working as an apprentice, I worked more with Beverly than with Dudley. Beverly would lay out interesting things to do and have us do them. She was very supportive of me throughout. It was after I got to the University of Michigan that Beverly said that it was really very appropriate for me to specialize in the demography of American blacks.

VDT: It wasn't really until then, 1967?

FARLEY: I'd been doing it, but I never thought of myself as specialized, as she was suggesting I could be. And I found that very helpful, even after I'd been a professor for three or four years, for her to suggest that was really a good thing to do.

VDT: They were really a very close-knit team in every way; worked beautifully together as well as being married.

FARLEY: Yes.

VDT: Was there an age difference between them, because she was a student a little later than he? A student of Phil.

FARLEY: Her obituary didn't mention her age, but I thought she was about 61 when she died [early 1988]. I can recall her mentioning when she was born and I think it was 1927, 1928 [1929].

VDT: She died prematurely then.

FARLEY: That's right. And Dudley was born in 1918, was it? [1921] Yes, there was an age difference, but it wasn't great.

VDT: And Don Bogue, tell me what he was like as a professor. You said he was such a missionary in classes. In the mid-1960s, for instance, he was making his extraordinary projections of world population at 4.5 billion in 2000--if there was a crash family planning program. But he still was a good professor and obviously you worked well with him.

FARLEY: A very stimulating professor. He taught the demographic methods course at the University of Chicago, which was required of all demography students. He stimulated students to do very good work in that course and a lot of collaboration among students. We used to spend a lot of time trying to figure out exactly what was going on: what were these methods, these techniques. He was very stimulating in that regard.

On the other hand, even when I got to Chicago, he was getting money to more or less establish clinics in Chicago or to facilitate the delivery of birth control information and there was some faculty resistance to that. I don't suppose it was so much the purely mechanical side of it as it was the idea that, "We at the University of Chicago are research investigators. We do not go into the community, particularly we don't go into the community with condoms and encourage people to control their

fertility." But Don was doing that already. Then by the time I left Chicago, Don was making a number of claims about the effectiveness of these programs he had initiated in Chicago and I think many of the faculty were skeptical--not only about sophisticated design, but you need a long-run period to show that your intervention program has actually lowered fertility. And I think very many other demographers were skeptical of Don, on the one hand because of his missionary zeal, but also because when Don got a program in place, other people thought Don very shortly thereafter claimed that the program was effective.

VDT: That was brought out particularly . . . The one time there was a book review in Demography was in 1967 [Vol.4, No. 1] and Phil Hauser reviewed Bernard Berelson's [editor] Family Planning and Population Programs [1965], which was the proceedings of a conference in Geneva in 1965, and he lit into Don Bogue's contribution to that and particularly the Chicago experiment, the fertility and low-income population experiment. He said he, Phil Hauser, had irrefutable independent proof that there had not been a drop in Negro fertility in Chicago in the 1960-65 period, which Bogue was claiming there had been. He went on to say that people who set up a program should not be the evaluators of that program.

FARLEY: Yes. That kind of strain went on among the faculty at Chicago at that time. Some of it was interesting; it made it a more dynamic place to be. But Don was irrepensible. I've talked to his daughter recently and he is as irrepensible now as he was then.

VDT: I was supposed to interview him last Monday night. He was going to be in Washington one day, but it turned out he did his business by phone. He's very elusive. When I was in Chicago last November, he was in Ethiopia, China, and Mexico, drumming up business for his new Social Development Center. He says we'll get together at PAA in Baltimore in March. I'm taking that with a large grain of salt [but we did]. Where's his daughter?

FARLEY: His daughter is a social work student at the University of Michigan. She worked for a while and I think he would say that she's a kind of part-time dissertation student at the University of Michigan. She also works as a computer programmer. She told me Don has learned Portuguese and is bringing up 85 people from Brazil to learn how to analyze data at his center and then he's going off to Fiji or somewhere. It sounds as though Don is putting in the 80 hours a week that . . .

VDT: Everybody says he's the most prodigious worker that ever was in any field.

FARLEY: As I recollect, Chicago was very, very stimulating, but faculty members there, including some of the demographers, had big egos.

VDT: Including Dudley?

FARLEY: They sometimes insulted each other. You're telling me how Phil Hauser was saying that Don Bogue had no evidence that his program worked. Well, that kind of thing went on with some frequency.

No, Dudley is a very appropriate person; he would never insult someone else. But in Dudley's very careful exposition of his own analysis and work, it would become clear that his work was far superior in care and execution than that of some other people on the faculty. So there **was** that kind of competition.

Don Bogue, by the time I got there, was running the Community and Family Study Center. He used those auspices to get some of his activist programs under way. But that was a kind of

demographic setup. It was different from the Population Research and Training Center, which housed Phil Hauser and Dudley Duncan. So there were different centers, sometimes competing a bit for students. Betty Bogue was the supervisor for employees at the Community and Family Study Center. I think she had the same kind of work habits that Don had and she expected other people to have those work habits also.

VDT: Driving themselves?

FARLEY: Yes.

VDT: Did you ever work with Evelyn Kitagawa?

FARLEY: I did work from time to time with Evelyn--less Evelyn than with the Duncans or with Don and Phil Hauser. But she was around at the Population Research and Training Center.

VDT: These professors all seemed to produce the most prodigious amount of work. Did they have graduate students doing a lot of that? For instance, Don Bogue took on the editorship of Demography, in addition to everything else he was doing at that time. The first issue listed a lot of people, his colleagues and graduate students, who helped. You're not there. That was 1964, so you must have been on the verge . . .

FARLEY: I was on the verge of leaving then, yes.

VDT: A lot of these projects had to have been done by graduate students. For instance, Phil Hauser published 32 books, chapters in 50-60 books, and over 500 articles. My husband said he must have written an article a week! A lot of this must have built on the work of graduate students.

FARLEY: Yes, I think graduate students had a role in it. But I think those were very hardworking individuals, who literally did turn out a tremendous amount of work by working many nights, evenings. And, of course, the 500 articles are not 500 distinct articles. Phil did write for a popular press, to some degree. They were very thoughtful articles. For a while after every census was taken--Phil had findings from the 1950 census, findings from the 1960 census, and I think he even went on and did some findings from the 1980 census--and those were kind of overview articles. Phil, of course, had Evelyn Kitagawa to help him with some things and Beverly Duncan helped him with some things. Dudley and Hauser collaborated on a number of books. But they were productive individuals.

VDT: They certainly were. Who were some of your fellow graduate students at that time?

FARLEY: Well, I certainly remember Omer Galle; I was very close to Omer Galle as a graduate student at Chicago. Bill Hodge was there at the time; Bill's teaching out at Southern California now. Paul Siegel, who's at the Census Bureau now, was a graduate student there. A fellow I worked with when I first got there, Robert Fenier--I don't know what happened to him; he was from Montreal. Judah Matras was there, finishing up graduate studies. There were a wide variety of students. Those in population tended to see each other most frequently.

VDT: And then you went to Duke. What took you to Duke?

FARLEY: I was getting my degree in 1964 and I was interested in becoming a college faculty member and I hoped to go to a place where there would be some interest in population. Hal

Winsborough had graduated from Chicago some years earlier and had worked with Dudley Duncan. I think Hal taught for a year maybe at Iowa and one year at Ohio State and then went down to Duke, maybe in 1960, 61. And he had created something of a population program at Duke. Kurt Back was there; Joel Smith was there [and Joseph Spengler]. So there were a few demographers at Duke. Hal was interested in doing more with demography and I was interested. It seemed like a good place to teach, so I went to Duke.

VDT: For three years only. Then the lure of Michigan . . .

FARLEY: Yes, Michigan had a stronger demographic program and Ron Freedman was the person who got in touch with me and eventually convinced me that I should move to the University of Michigan. So I moved up there in the fall of 1967.

VDT: And it must have been a great place to be--the Freedmans, both of them, and the two Duncans.

FARLEY: The two Duncans were there and David Goldberg was quite active at that time. Leslie Kish has always been interested in demographic issues, so it was a great place to be.

VDT: Tell me about Leslie Kish. I must admit that I didn't know much about him and you invited him to introduce you, your PAA presidential address last year ["After the Starting Line--Blacks and Women in an Uphill Race," PAA presidential address, New Orleans, April 22, 1988], and one always chooses the person, it seems, who has been the great influence in one's career to do that.

FARLEY: Well, I don't know that Leslie was a great influence in my career. He wasn't on my dissertation committee or anything of that nature. Leslie is just a perfect European gentleman, who has always been . . .

VDT: European? Where's he from?

FARLEY: He is of Hungarian ancestry; he was born in what's now Romania. Lots of Hungarians lived in what's now Romania and Leslie was born there and came to the United States sometime in the mid- to late 1930s. Leslie is just a very supportive scholar at the University of Michigan and I've enjoyed having him as a colleague in the 22-23 years I've been at Michigan. There is not a great intellectual connection. Leslie's contribution to our field is in survey sampling; he's trained probably more survey samplers than anybody else. I haven't had a course in survey sampling. I haven't designed a sample. So it's not such a close intellectual feeling with Leslie. It's rather that I thought he was a very appropriate person to do that--introduce me.

VDT: And Ron Freedman? You never really worked with Ron, I presume, because his interest has been in Third World demography.

FARLEY: Yes. Ron and I have not worked on a paper or something like that, because our interests have been in different directions. Ron has been my colleague for 24 years and certainly we talk all the time and so forth, but we haven't written anything together.

VDT: Well, who have been the leading influences in your career? You have pretty well covered them already, presumably.

FARLEY: Yes, I have. Donald Barrett is the man who in some ways got me interested in population,

but there was not a great deal of research going on where I was an undergraduate [Notre Dame]. At the University of Chicago, I think the stimulating influences were Beverly Duncan, Dudley Duncan, Don Bogue, Phil Hauser. Those are the individuals whom I remember most as people who helped me on the way.

Nathan Keyfitz came to teach at Chicago in my last year. I took one course from Nathan. Had he been there earlier, I think I would have liked to see if I could have done the kinds of work that Nathan has stimulated, namely, mathematical modeling. I found that very fascinating and interesting. But by the time I took a course with Nathan, I already had my dissertation under way. I knew there were many students who stuck around Chicago for ages and I didn't want to do that. I wanted to get out and not be a permanent graduate student. I remember talking with Nathan and Nathan saying that if I were interested in that kind of demography, he would encourage me to stay around. But I think it was a question of time. I was conscious of the fact that developing skills to become a mathematical demographer required at least a year or maybe two more years of graduate school and I found what I was doing very stimulating, so I didn't follow up on Nathan's suggestion, although he was certainly very helpful at that time.

VDT: You certainly got your Ph.D. very promptly, in those four years from the time you went to Chicago.

FARLEY: I think that came from working with the Duncans. The Duncans had an image of projects with a deadline and I think that's a very good model to have. There were other faculty members who didn't see deadlines and who, as the project went on, would find new data, new analytic techniques, and the project would expand. The Duncans on the other hand would say, "In the next 18 months we want to do such and so and we'll do it." And they were pretty faithful about meeting those deadlines.

VDT: Then their students were expected to write up their part in the projects; get it done?

FARLEY: Right.

VDT: Chicago didn't have the sort of implicit deadlines that Princeton had? I've heard that Princeton, the OPR [Office of Population Research], expected people to do their PhDs in three years from the B.A. I don't think that many people have done it, and in recent years, no. But Sam Preston did, and there's the phenomenon, Alvaro Lopez, from Latin America [Colombia], who did it in about 18 months.

FARLEY: Right. Lopez's dissertation was not a data dissertation. It was a modeling of . . . was it fertility and reproductive growth? [Problems in Stable Population Theory, 1961, an extension of Lotka's work.]

VDT: That's right. Well, sometimes models take much longer. I've got a son doing a modeling exercise for his economics dissertation for Stanford and it seems to be taking about ten years.

FARLEY: There were a lot of graduate students at Chicago who had spent five or ten years at graduate school. I didn't want to become like that.

VDT: At Chicago, even at that time?

FARLEY: Even at that time. Chicago didn't have a lot of undergraduate teaching opportunities; you couldn't be a teaching assistant for that long, in most cases. So these people would scrape together a

living in certain kinds of ways and that looked pretty unattractive to me. I knew I was going to have had enough of Chicago at the end of four years, so I'm glad I finished up.

VDT: Good for you. Let's look at the other side of the picture. Who have been some of your leading students in the 25 years, now, that you've been teaching? Some of the students you're proudest of.

FARLEY: At Michigan we have a fairly large faculty in the sociology department and a fairly large faculty at the Population Studies Center, so maybe there's a little bit less direct mentoring or sponsorship that there was at the University of Chicago. By that I mean we certainly encourage students to work with a large variety of faculty at the Pop Studies Center. For some obvious reasons, in recent years a large number of students have chosen Ron Freedman or Al Hermalin as their dissertation directors.

Then we have students who have worked with faculty. Suzanne Bianchi comes to mind; she's now our secretary-treasurer of the Population Association. She worked as a teaching fellow in the Detroit Area Study, that project back in 1976. [Investigating racial residential segregation in Detroit. Farley was principal investigator of the project.] Her dissertation was about changing household composition in the United States and its implication for poverty and women and children.

Diane Colisanto worked with that project, who is now at Auburn. She worked with me and I supervised her dissertation. Barry Edmonston worked on a dissertation having to do with metropolitan distribution, suburbanization of people and industries. Barry taught for a while at Cornell and will be going to the Urban Institute; he worked with me. Paul Voss, I supervised his dissertation. He's been very successful at the University of Wisconsin; he's a state and local demographer.

VDT: Now on the PAA Board, one of the first business demographers.

FARLEY: Business or applied demographers, on that board. One of the more recent students whose dissertation I supervised is Robert Wilger, who has moved over to Wisconsin, working in Paul Voss's shop.

A woman by the name of Patty Gwartney-Gibbs worked with me on how women and men get slotted into jobs--the different kinds of occupational achievements that women and men make. She's now teaching at the University of Oregon, so I was pleased to work with her as a student. Shirley Paget, who's working at the Institute for Social Research at the University of Michigan now, did her dissertation on racial attitudes in the Detroit area, using partially the Detroit Area Study data and other data gathered by the University of Michigan. I was pleased to be on her dissertation committee.

So there's a variety of students I've worked with over the years. That's one of the advantages of being at a place like Michigan. We now have a particularly talented group of students. It's a good opportunity for faculty members; it's a good opportunity for students too.

VDT: Indeed. It's a great place, and so is Chicago. You have been at the two top centers, just about.

BREAK

FARLEY: Jean, are you going to write this up?

VDT: What I plan to do--did you ever see the "proposal" I submitted to the PAA Board? I've now done 20 interviews and one from Nathan Keyfitz is on the way. He's done his own taped interview from a schedule I sent to him in Indonesia. I hope to do 35 or 37 interviews [actually did 41]. Andy Lunde in the 1970s did about a dozen. Some of those tapes are mechanically defective. For instance, the one with Henry Shryock goes dead after ten minutes. I've redone one with him. The one with Ron

Freedman was pretty good but I'm going to do another one with Ron too. Similarly, I've done another one with Ansley Coale; he did one in the 1970s.

So I hope to interview all past presidents who are interviewable and reachable. I've eliminated two, Joseph Spengler and Calvin Schmid. Joe Spengler is still alive but has Alzheimer's disease [died in late 1990]. He was interviewed. Harry Rosenberg interviewed three people at once--Clyde Kiser, Joe Spengler, and Horace Hamilton. That didn't work out very well, but at least we have their voices on tape. Another one was done with Clyde Kiser and he's excellent. I won't repeat that one. I won't repeat one with Con Taeuber [but see "constructed interview" above].

FARLEY: Con is all right? I saw Con recently.

VDT: He always turns up at meetings, but he has Parkinson's disease. He's bright and alert. He's always sending me stuff for the archives.

FARLEY: I don't know about Clyde Kiser.

VDT: Clyde Kiser is fine, but there was an excellent interview with him in 1973.

FARLEY: How old is Clyde?

VDT: He's in his eighties [born 1904]. There was an excellent videotape interview with him in that series [done in 1985] on directors of national fertility surveys, instituted by Barbara Wilson and William Pratt, so I feel he's pretty well recorded. I went to Chapel Hill for interviewing, but Bessemer City where he lives is 200 miles away. Bessemer City is where he grew up; he went back to his native land when he retired from the Milbank Fund.

FARLEY: Is his wife all right?

VDT: I don't know [Louise Kiser, coeditor of Population Index, died in 1954], but Clyde was in fine shape three and a half years ago when he came for his interview for the fertility survey directors. I got pictures of him. There was a photo of him and Barbara Wilson in PAA Affairs from that time, when there was a little party for him at Barbara Wilson's. [See Barbara Foley Wilson, "Videotaped Interviews about American Fertility Surveys," Vignettes of PAA/U.S. Fertility History, PAA Affairs, Winter 1985.]

One other past president who is still alive whom I won't try to get to is Calvin Schmid. He's 89, lives on an island off the coast of Washington state. Henry Shryock visited him a few years ago. I gather he's really very alert [at time of Shryock's visit], but he's a bit far removed.

And I've been trying to do all the secretary-treasurers too and one of those, Kurt Mayer, went back to Switzerland in the mid-1960s. I'm debating whether I'll write him and ask if he would do a tape as Nathan Keyfitz has done, whom, of course, we had to have in this series.

I still have a group of people out in California and Arizona. I hope to get seven in one fell swoop in April-May. I will interview the Freedmans in Ann Arbor in June, en route to Canada, and there are a few other outliers, like Sid Goldstein in Providence [interviewed December 1990, also Alice Goldstein].

Then I will have all the tapes and transcripts and, of course, they are available to scholars to study. But I think they're not very accessible when buried in our archives at the Georgetown library--and our archives at the moment are sitting behind the papers of Harry Hopkins, so I'm not looking forward to going back through them. And I'm doing photographs; I will do a photo of you before you go. I hope to put out a very personal book, with excerpts from these interviews, along with the

vignettes of PAA history we've had since 1981 in PAA Affairs and these lists that Andy did of the meetings, where they've been and how many were there, the officers and Board members and memberships totals from the beginning. And I'd like to get permission from Population and Development Review to reprint that last article of Frank Notestein's of December 1982 ["Demography in the United States: A Partial Account of the Development of the Field"], which is a pretty good history of U.S. demography up to the 1970s. Maybe some other bits and pieces [e.g., papers on PAA's beginnings given in the "PAA at Age 50" session at the 1981 50th anniversary meeting]. This is not by any means intended to be a formal history of U.S. demography or PAA, more a collection of materials, but I think it should be of interest. Interesting and fascinating [which is why VDT later decided to assemble a collection of the full, edited interview transcripts, the full flavor of which would be lost in mere excerpting for the "Selective History" described here]. Now I'm talking too much.

Here is the photo that Jean van der Tak took of Reynolds Farley when she completed this interview with him in 1989.



FARLEY: That could lead to a very complete history of U.S. demography. I'm thinking of Jean Convert's book on the history of survey research in the United States, University of California Press; unfortunately, it sells for something like \$59, so it's not easily purchased. It's very important for important for social scientists, survey research. She takes it up to 1960, very, very competently done, looking at it in somewhat the same perspective you are, namely, personalities, and how things get from one institution to another. Not so much through looking at the journals, but how did people contact and what kinds of feeder funds became available. If funds were not available to support research, why did a place like the social science group at Columbia have such a predominant lead in survey research around the time of World War II but then lose it after World War II? What were the intellectual traces? She lays that out very, very well, and I would like very much to read the same thing for demography, and you are in the position to do that.

VDT: Well, I agree, except I'm not going into such detail as that. For instance, I'm not probing for where the funding came from. Part of that is covered by the Caldwell's book on the Ford Foundation contribution [Limiting Population Growth and the Ford Foundation Contribution, 1986]. Of course, that was for university centers that dealt with Third World population, not for the others or programs on the U.S. side. I don't have such high pretensions.

Andy started the series of interviews to get a hold on PAA history because he, as secretary-treasurer in the late 1960s, found there was nothing on record. Of course, he had that excellent session at the 50th anniversary meeting in Washington in 1981, which covered very well the early history of PAA [papers by Lunde, Frank Notestein, Frank Lorimer, and Clyde Kiser]. This later history will have a bit on the later history of PAA; it's not all that exciting. But I think personalities are interesting. And

demography is still a small discipline, relatively, and all so interconnected. Those who were students of Phil Hauser, Keyfitz, for instance--Kingsley Davis comes up time and again--knew each other as students. And, of course, the select sample of those who've risen to the top in PAA obviously would be intertwined. But I hope this will be a contribution to someone who will do the definitive history of demography in the U.S.

Interestingly, a friend of mine who graduated from the University of Toronto two years before I did, Sylvia Wargon, who is at Statistics Canada, has just written to tell me that she has been officially detailed to write a history of the development of demography in Canada, so they have thought that that would be important there too. It's too bad someone at the Census Bureau doesn't sit someone in an office to do it for the U.S. It's a much bigger story.

FARLEY: It's a much bigger story. It strikes me it has to be done by someone who has got a lot of experience. It's harder for a younger person to do, probably, although you might find some historian who might do it, if someone wants to write a dissertation in history.

VDT: Well, for someone as a final project just before retirement--that's the way with Sylvia--it would be ideal. I had thought of this as a perfect retirement project. My husband forced me to retire early [to travel widely]; I loved my job at PRB [as editor of the Population Bulletin]. But this I knew I wanted to do [complete the interview series]. Andy asked me to carry on as PAA historian in 1981. I think Con Taeuber put him on to it, knew I had a historical interest; my undergraduate major was history. And I was in Washington; they needed someone to be close to the archives, which Andy set up, with the help of Con Taeuber and Tom Merrick, who was at Georgetown then.

FARLEY: Well, if you ever have any doubts about it, I think a 200-, 250-page history of demography in the United States would be a very important intellectual enterprise.

VDT: Well, it is. Ralph Thomlinson once said he was interested in that--I don't know why that should be--but he was going to have to get funding and be put someplace to do it.

FARLEY: It's very hard to get funding for that. As you say, this is a pre-retirement project. You need a time span to do it. A person who would not surprise me if he attempted it would be Bill Petersen.

VDT: True enough. He's enormously prolific and, of course, he's had the experience. His Biographies of Demographers [with Renee Petersen] are invaluable; it gives a sketchy background of each of you, at least to the early 1980s. And he did the Dictionary of Demography [1985, in two volumes, of which Biographies is Volume 2]. I also think he'd be a good one to do it. Maybe I should go see him when I'm out in California, just talk about this in general [did not]. I presume he knows what I'm doing.

FARLEY: He's a Dutchman, isn't he?

VDT: He might have been. I remember reading an article of his about Dutch fertility; he knew more about it than most people.

FARLEY: I thought he'd spent a couple of years in the Netherlands.

VDT: I spent a year in the Netherlands just after I was out of Georgetown and PRB approached me: Would I write an article on why Dutch fertility remained stubbornly high? And, you know, the year I

was there [1970-71], it collapsed!

FARLEY: I was going to say, it's not stubbornly high now!

VDT: I was the first staff member of Dirk van de Kaa's institute, the Netherlands Interuniversity Demographic Institute. I sat in an office all by myself and set it up. Then Dirk van de Kaa came; we overlapped for three months.

FARLEY: Italy's got about the lowest fertility rate in Western Europe now, except for Denmark.

VDT: Italy has the lowest [1.3] at this point. But the Dutch fertility plunged down in 1970-71. What happened was that suddenly abortion was available; it was not really legal but people could get it easily. And the pill suddenly became available too. And fertility just collapsed. So my story was pulled out from under me.

Actually, I collected some data on their internal migration trends. They were making an effort to deconcentrate the Randstad, as they call it--the heavy concentration in the west of the country--and some government offices were being moved out. PRB, though I didn't work for them then, asked me to collect data and I went to see Henk Heeren and ter Heide, who were working on that. I gave PRB these data and they sat on them. Years later, Gordon De Jong wanted to do some work on population redistribution policies in the Netherlands and he heard from the Dutch that Jean van der Tak had collected these data, and I got them from PRB for him. Well, that's me. I want to get back to you.

FARLEY: One other question. Are you interviewing the living editors of Demography?

VDT: No, I'm not; perhaps I should. Well, I think the Demography story ended with Don Bogue [first editor, 1964-68]. You know the famous story of his thick issues, ending up with the family planning issue ["Progress and Problems of Fertility Control Around the World," Volume 5, No. 2, 1968], with the inverted red Indian family planning triangle on the front and the slogan, "Stop at two or three," which caused such a flap. Then, of course, Demography collapsed into the skinny little thing it became under Beverly Duncan and has not changed very much since.

FARLEY: No, it hasn't changed too much. I always thought Don was the one who started Demography.

VDT: He did. Well, Norman Ryder had the idea.

FARLEY: Norm Ryder takes a lot of credit. I wouldn't want to get those two people in the same room, but I think Don's version and Norm's version don't overlap very much.

VDT: That's interesting; tell me.

FARLEY: My impression is that Norm thinks he started Demography.

VDT: He had the idea.

FARLEY: I may be wrong, but I think Norm believes he did much more than the idea. He was asked to be the first editor and was prepared to do so and something happened that was quite believable: Norm had some falling out and it looked as though there would be no Demography.

VDT: Falling out with whom?

FARLEY: I don't know with whom. It must have been with whoever was responsible in the PAA for initiating Demography. Only after Norm turned them down, whoever this committee was, crawling on their hands and knees, came to Don Bogue and Don . . .

VDT: Wow! That's not Don's version.

FARLEY: That was not the version I've heard from Don; that was not the University of Chicago version. But I get the impression . . .

VDT: I interviewed Norm and he said he had the idea of Demography but Wisconsin didn't have the money to support it, nor did Michigan; he went to Michigan. Then finally Don said he could get the money, through the Community and Family Study Center. Don wrote a vignette on the start of Demography in PAA Affairs ["How Demography was Born," PAA Affairs, Fall 1983]. The Ford Foundation was supporting his center and through them he had the secretarial help and he corralled all his students and colleagues to help. He didn't mention the National Science Foundation. After that article appeared, Paul Glick wrote and said, "He happens not to have mentioned that the National Science Foundation came in with \$30,000 for the next three years" [after the first edition of Demography].

FARLEY: That was a substantial grant then.

VDT: Indeed. Glick [PAA president, 1966-67] and Calvin Schmid, president before him, got the money. Then the family planning issue had cost so much money that Don . . . Don felt his thick issues were justified because he'd sell them to libraries, and indeed he did. In the end, the Community and Family Study Center absorbed all the costs of that 1968 family planning issue. But Ansley Coale said--he was president then--he got these irate phone calls from people saying, "What are we doing getting into the family planning business?" But it was a tremendous issue, invaluable. And that first volume, in 1964, that was a bible of demography. What else is there in the history of Demography, that became sober, scholarly, skinny?

FARLEY: It became thin, and Population and Development Review took on . . . It's a different thing; the articles are not necessarily reviewed in the same fashion as Demography is. There's the very formal review of articles before they appear in Demography, but Paul Demeny, I think, has quite a degree of editorial freedom to publish or not publish what he wishes in PDR [but they go first to reviewers]. I guess many people look upon that as a more dynamic, lively journal than Demography.

VDT: For instance, Paul Demeny pointed out in his interview that he has his book review section, which is often controversial. And Demography, why did they never have a book review section? There was that one book review by Phil Hauser, in 1967, and as I mentioned he criticized Bogue heavily. I surmise that perhaps that's why no other Demography editor ever dared have a book review section. [A book review section appeared in the May 1991 issue of Demography, edited by Avery Guest as of 1991, announced as the first of a three-issue series of reviews of the 1980 census monographs.]

Paul Demeny says that the tradition in other disciplines, in economics, is that when a book comes out it's criticized. But demographers, he says, are very sensitive, know each other too well, perhaps, and seem to take criticism as a personal affront. Now what do you think of that?

FARLEY: I think the reason there's no book review section in Demography is a financial one. Namely, every editor I've talked to or whom I've heard about when I've been on the Board or one of the officers of the Association, all those editors say, "We have many more good manuscripts than we can publish now." So the editors have been reluctant to sacrifice what they see as good manuscripts to devote six, eight, ten pages to book reviews. I think that's the reason. Nobody ever said, "We're going to add 50 pages to Demography next year so you can have book reviews." I think that's the primary reason.

VDT: It might be.

FARLEY: But the other reason you're suggesting is an interesting one. It [demographers] is a small group of people. There are some people--you've mentioned them already--who would write pretty critical reviews, and I think Kingsley Davis would be another name to add to that list. But there are very many other people who work so closely, collaborate so much, know their research grants are going to be evaluated by other demographers, that there would be implicitly, I think, perhaps some inclination to tone down otherwise hostile reviews.

VDT: I gathered that with Paul Demeny, too. He said also he finds it difficult to find people who will agree to write reviews. He says people see no political future in it; in other words, you don't get that much credit for it, in your resume.

FARLEY: If it's a 300- or 400-page book, you might ordinarily scan it for your own purposes or read a chapter or two. But if it's to be a review, you've got to read every page of that book and think about it. So it's a lot of hard work sometimes.

VDT: The bulk of reviews can't make much difference in your own bibliography. They certainly do, I guess, in The Economist, which will take off on some book.

FARLEY: I don't think they do among sociologists. I don't think that when you look at a person's vitae, you pay much attention to what books they've reviewed. Certainly, one strategy would be for an editor of Demography to have a review symposium where a person instead of just reviewing one book could review or discuss the merits of two or three books. But that's even more work for the reviewer.

VDT: Or the other way round: two or three people discussing one book, which has happened often in PDR, say, with the Julian Simon book [review symposium on Julian Simon, The Ultimate Resource, reviewed by Peter Timmer, Ismail Sirageldin and John Kantner, and Samuel Preston, PDR, March 1982].

FARLEY: Among other things I wanted to initiate as PAA president last year was to get some serious discussion of books in our field. So I added those two "Authors-meet-critics" sessions.

VDT: I was going to ask you about that; that was very noticeable.

FARLEY: The Teitelbaum book [Michael Teitelbaum and Jay Winter, The Fear of Population Decline] and the other book about women, the 1980 census monograph [Suzanne Bianchi and Daphne Spain, American Women in Transition]. I don't know if those were successful sessions or not, but I wanted to stimulate . . .

VDT: Now we're having them this year [1989 PAA meeting]; They're on the program I got yesterday.

FARLEY: In certain cases, with people who're particularly controversial like Julian Simon, you could pair his book against a book presenting a very different view and use that as a theme for a stimulating session. If you can do it for the meetings, I don't know why you can't do it for Demography or something else.

VDT: Indeed. Well, I had that on my list for when we get to PAA. Now I want to talk a bit about your work in black demography. I'd like to ask a straightforward question that has always puzzled me: Why are there so few black demographers and so few blacks in PAA?

FARLEY: There are very few blacks. Last year I tried very hard to see that there was a representation of blacks on the program.

VDT: You did try last year? Was it last year that Robert Hill was there? He's a sociologist.

FARLEY: No, I don't think so. He may have been there two years ago. Last year we did have Don Deskins, Walter Allen . . .

VDT: He is a black, your coauthor? [on 1980 census monograph, The Color Line and the Quality of Life in America, 1987].

FARLEY: Yes. Why are there so few blacks in demography? Phew! Give me a speculation.

VDT: Well, I've heard, of course, that there was a concerted effort at the University of Chicago to bring in black sociologists, from Atlanta University and so on. I knew one, John Reid, who . . .

FARLEY: John Reid eventually ended up in Washington, didn't he?

VDT: Yes, he did, and he was author of one of my PRB Population Bulletins, on blacks ["Black America in the 1980s," December 1982].

FARLEY: With Bill O'Hare, I guess?

VDT: No, Lee Bouvier. Actually, Lee wrote a lot of it and so did I, but John Reid's name alone was on it, and he did a good job with the media coverage that came afterward. But as I understand it, they did make an effort to bring blacks from Atlanta University, for instance, out of the South to Chicago.

FARLEY: Yes.

VDT: And then someone has said, there was no money in demography. Blacks who were that highly trained could go into fields where they were more recognized or highly paid or whatever. Maybe that's wrong. There are more black sociologists, for instance, Julius Wilson.

FARLEY: That's true. There are more black sociologists, but there aren't very many blacks in any of the academic disciplines. Education and social work probably have the greatest representation. Economics, being a very large field, there are numerically more blacks there, but there aren't very many. There certainly aren't very many blacks in demography. I know at Michigan, certainly at Chicago, we tried to recruit black students from the United States. We have a moderately high failure rate from white students--people who don't just necessarily fail out but they don't complete their

degrees. But we've been not so successful with black demographers, at Michigan or elsewhere. I was co-chair on Tom Viest's dissertation committee. He is on a post-doc now. He was interested in black-white differences in infant mortality and did some good work on that topic. But I don't know if he's going to go into demography or something having to do with public health.

There are a few black demographers, but not many. Larry Carter at the University of Oregon had some interest in demography for a while. Claudette Bennett has a degree from Howard University, out at the Census Bureau; I would include her in that group. But it is a small pool of individuals.

VDT: Did you in a sense see an opportunity there? Of course, you said that the Duncans encouraged your seriously considering concentrating on black demography. Was there a lacuna?

FARLEY: Well, the Duncans certainly tried to recruit blacks into the field. One of my colleagues was Nate Hare, who got his degree in sociology and taught for a while on the West coast in the Berkeley area. But after teaching sociology with demography for a number of years, he decided to go back and get a degree in clinical psychology. I believe he got a second Ph.D. and, so far as I know, he's a clinical psychologist now. So, yes, there are very few black demographers. Leroy Stone is one who comes to mind, a Jamaican and a Canadian.

VDT: Oh, he's originally Jamaican? I've just read the section in your Color Line that points out that the thesis that West Indians do much better is all balderdash!

FARLEY: Dubious, yes.

VDT: I've just finished the Color Line--fascinating, marvelously done book--but so somber. There is just no progress.

FARLEY: Yes.

VDT: And that's been true of the reviews of the book. I was so proud to see it reviewed on the front page of the Washington Post book review section a year ago. And also delighted to know it was out; you got it out so fast, in 1987, one of the first of the 1980 census monographs to appear. And the review of Blacks and Whites: Narrowing the Gap? [1984] in PDR [March 1986]. They all end up saying you are so somber; the outlook for blacks is depressing. Is that the way you feel?

You may be even more depressed now, arriving in the District of Columbia, where we've had a record number of homicides in this past year and in the past month, January 1989, the highest in any month ever--and mostly blacks, if not all. You had predicted that in your 1980 article ["Homicides in the United States," Demography, May 1980], where you pointed out that homicide rates for blacks were up and that the increasing availability of guns was going to make the situation worse. And that's absolutely true. We're a living example of it here in the District of Columbia.

FARLEY: Yes, it is. The situation is not a good one. There certainly is some growth of the black middle class, but the racial gap in the United States is very large. My own impression is that there is a reluctance to talk about this. It's a reluctance, perhaps, on the part of many whites. It's a sensitive topic. If you're a white politician, there's not a great deal to be gained from talking about the racial gap, the history of racial hostility. The blacks for the most part do not have a tremendous amount of social or economic power. A small segment of them may, but the resources for change in the black community are not great.

I don't know whether one should be pessimistic or optimistic. Certainly, there are some signs of blacks moving into important positions. And I would speculate that if a black leader has a certain

degree of talents or a certain array of signs, namely, college degrees, they can enter into a middle class, where they will face, I presume, no more than modest and maybe very little racial discrimination. But it seems to be a small proportion--20, 25, 30 percent--of the black population moving into that middle class.

We assumed on the basis of what was happening from the late 1950s through the mid-1970s that there would be a rapid expansion of black middle class population, that poverty rates would go down gradually or rapidly. But since 1974 on almost all economic indicators, there's been no contraction between blacks and whites, with the exception of black women who in terms of occupations and earnings are doing quite well compared to white women.

VDT: Well, that is a depressing outlook. And you talk about the few blacks in positions of prominence and you're in a city where unfortunately our black mayor [Marion Barry] is not doing very well [and this was before Barry was charged with drug use].

FARLEY: That's right. People have pointed out that as blacks became predominant in cities, they certainly would elect mayors. But very often they are cities with immense financial problems. Cleveland, Gary, Detroit, the first cities to have black mayors, are not the financial bulwarks they were 30 years ago. Industry has moved to the suburbs; the middle class population, black and white, has moved to the suburbs. The financial encumbrances in the cities are much greater than their abilities.

VDT: That's happened in D.C. too.

FARLEY: Yes. D.C. is really different in many ways from all other cities. In some ways, it's becoming a city of the rich and the poor.

VDT: Yes, it's a frightening city--the divide between the whites and the blacks and also there's an extreme divide between the rich and the poor. Well, it must be an interesting city for you to be in for six months!

FARLEY: Washington is always an interesting city to be in. Washington has a larger array of what used to be called "stable" neighborhoods. I don't mean necessarily whites. There are stable black middle class neighborhoods in Washington--out by Walter Reed Hospital, for example. Washington is a city of neighborhoods. You can find a few of these in Detroit, but proportionally they're less numerous in Detroit than they are in the city of Washington itself [District of Columbia]. There may be hope for Washington in some regards, if the drug problems are ever solved. No one seems to have a solution for that.

VDT: No. Well, that's an inexhaustible topic. But your books are excellent; I want to talk about them now. They've been praised for your "careful presentation of both sides of the argument." For instance, you say higher black fertility can be due either to the minority group hypothesis--minority groups want to produce more. I'm interested in that because one of my first papers was on French Canadian versus Anglo Canadian fertility and that was a favorite hypothesis at that time, that French Canadians produced because they wanted to keep up. And the same in the Netherlands, between the Catholics and the Calvinists.

FARLEY: Jean, the solution for high fertility is to get you to study it! You studied the Dutch right before their fertility went down; you studied the French Canadians and now the French Canadians have very low fertility.

VDT: Absolutely. You looked at the minority group hypothesis versus the social characteristics hypothesis--that there's something about certain groups that want to have high fertility. You carefully did, indeed, present both those arguments. I'll ask you straightforward: Do you feel as a white demographer studying black demography that you have to be exceptionally careful about looking at all sides of each issue--indicating the data, which you do exhaustively?

FARLEY: I think the answer is yes. But if a black demographer were studying black fertility, I think he or she ought to be equally careful. I mean, in the social sciences there's such a strong tendency to look for one causal factor that I think all of us have to be very suspicious of that, whether we're explaining why people vote for one candidate or why women are now working in greater numbers than they used to. There is such a tendency to say: "This is the cause." In particular, if you're going to write a 500-word essay for the newspaper, you want something in the popular press, you can't go through all the various perspectives which you really ought to go through when you're looking at something like this. And when you come to the basic question that I've been trying to address for the last few years, namely, how the civil rights revolution occurred but left a very large gap between blacks and whites, particularly in economic status. There you get very many single-factor explanations. People saying: Well, it's because manufacturing jobs are no longer available to blacks, or it's because there's something deficient about black culture, or black families are unstable and that's the real reason, or white racism is still as persistent as it used to be. All of those single-factor explanations are unsettling when you read them, but people defend them.

And in this town [Washington], everyone thinks a little bit about policy and if it's policy, it's quite often going to be targeted toward one factor, and that may not be the real factor or the only factor or it may be a vast oversimplification to say that one factor . . .

VDT: Like Charles Murray on welfare.

FARLEY: Sure. Murray says blacks are taking advantage of welfare because welfare gives them as much income as work.

VDT: So now they say those on welfare should work.

FARLEY: That idea permeated the Reagan administration, at least for the first couple of years, with George Gilder, Murray, and goodness knows how many other people operating as if the availability of welfare was the only problem for the black community.

VDT: So you see a need to temper that?

FARLEY: Yes. I don't think it's particularly because I'm a white person studying blacks. I think if I were doing something else I would hope I would be just as . . .

VDT: A careful scientist.

FARLEY: Yes.

VDT: That also shows in your referencing of your books, which is exhaustive. You must have read everything ever written on blacks and the history of population in the U.S. I learnt a lot from your books. And you have been praised for having lovely, clear tables and graphs, and you obviously make an effort to have them appear close to the place in the text where they're being discussed. Do you make a conscious effort?

FARLEY: Yes, sure. I've been helped a lot in that book [Color Line] and in the Blacks and Whites book. There was a woman at the Population Studies Center who was our editor, Cathy Duke, and she was just extraordinarily helpful in cleaning up our prose and making sure the tables and charts were legible to her as well as to everybody else. She played an important role in doing that.

VDT: And you got out your 1980 census monograph, The Color Line, pretty rapidly. The 1980 census monographs are carrying on the splendid tradition of the 1950 and 1960 monographs, but they've been slow to appear. Suzanne's and Daphne Spain's [American Women in Tradition] was the first [1986]--and yours was second?

FARLEY: I think ours was third. The Politics of Numbers volume by William Alonso and Paul Starr came out maybe two or three months before ours [in 1987].

VDT: But you made a tremendous effort, obviously, and got it out.

FARLEY: Yes. I don't see any reason for putting census monographs off to the end of the decade.

VDT: Indeed, it seems very frustrating, although you had the post-1980 data. And you've been on the advisory committee for the monographs. Have you been working with some others on an actual monograph?

FARLEY: The advisory committee worked to select authors and give a bit of the reading of the manuscripts as they came in, but Charlie Westoff [committee chair] did most of that. That committee has not met since the Pittsburgh meetings of the PAA [1983].

VDT: Tell me about your work with the Census Bureau advisory committee [on population statistics, 1975-81]. For instance, you were on the committee around 1980; were you involved in the problems of worrying about the undercount?

FARLEY: Yes, we were somewhat concerned at that time. But the outside advisory committees, I would say, play an advisory role which involves more suggesting and reacting to Census Bureau proposals. The advisory committees are not given carte blanche to come in with new suggestions. The Census Bureau doesn't say: "We've got some troubles with undercount; please come up with a plan to solve the undercount problem." Rather, the Census advisory committees, it seems to me, are presented quite often with work in progress at the Census Bureau and suggestions are sought. It is not the kind of technical board that bureaucracy might go to and say: "We've got to do something. We don't know what to do. Please tell us what to do."

VDT: What were some of the issues at the time you were on the committee?

FARLEY: One set of issues had to do with what would be tabulated from the 1980 census and how things would be published--those kinds of fairly mundane but important issues. Of course, there was concern about the undercount as the 1980 census came out. We were presented with a tremendous amount of information about the publicity campaign for the 1980 census. But, again, the advisory committee might make a few suggestions, but for the most part they were not advertising or promotion experts.

I think one of the more important ways that the advisory committee actually influences policy at the Census Bureau is that one or more members of the committee who are experts in an area will be alerted on some issue that is arising at the Census Bureau and those people will call the appropriate

person at the Census Bureau with suggestions or reactions. So the advisory committee acts to stimulate this sort of interchange, but that is not always done formally. Sometimes calling up and saying: "Look, you have four ways to tabulate or you have six different measures you can give us on fertility, this is the one you really ought to give us." That kind of informal exchange.

VDT: Have you done that?

FARLEY: Oh, sure.

VDT: On what, for instance?

FARLEY: There was some of the informal work on retaining questions for the census of 1980. And on the 1970 census, I remember working with Wilson Grabill at one point . . .

VDT: He was still working then?

FARLEY: Yes, he worked through the 1970 census. I recall suggesting and encouraging him to put in some measures of variance of fertility, which I think he did put in one of the 1970 reports.

VDT: I guess you corresponded with him. He's the famous deaf person.

FARLEY: Yes, a Gallaudet graduate.

VDT: And have you had much to do with encouraging more work on the undercount? That always seems such an issue with blacks, particularly young black males.

FARLEY: I don't think the Census Bureau needs any encouragement. My impression is that they devote a great deal of resources to it. They approach it statistically, statistical modeling, and of course they went through the furor, which involved Barbara Bailar, of a possible resampling of the population and the actual adjustment of the census counts. At the same time, they now have a group working with ethnographers to describe who is it that's not going to be counted in the 1990 census. There are a tremendous array of efforts going on at the Census Bureau. I don't see why they are so criticized, because we are not a police state; you don't have to fill out the census form under the guidance of a policeman or something of that nature. There are a lot of people just being slovenly, being indifferent, or something else will lead them to not fill out the census forms. I don't know how you push those people to fill out the form. Plus, the other real problems of: Are there housing units which are missed or people in households who are missed? The Census Bureau says it does a tremendous amount of research on this. So I don't think they need any more prodding. I'm sure they would appreciate some secret way to make sure everybody is counted, but we don't have that.

VDT: What are you doing now, in these six months at the Census Bureau?

FARLEY: That period has to do with evaluating the quality of ancestry data in the United States. The 1980 and 1990 censuses have a question asking everybody their race and another asking if they are of Spanish origin or not, which really screens out the 7 percent of the population or so who are Spanish. Then a sample of the population in both 1980 and 1990 are asked a question: What is your ancestry or ethnicity? And people can write in as many as they want. The Census Bureau typically codes their first two ancestries. The Census Bureau has not done a great deal to evaluate the quality of those data. For instance, are people consistent? If you ask them their ancestry at one point and come back six

months later are they consistent? About 12 percent of the population refuses or does not answer the ancestry question. Is that because they don't know their ancestry? There's another 6 or 7 percent who write down "American," and there's a great debate among scholars . . .

VDT: What do you think about "African American," the latest way to call blacks?

FARLEY: Well, race is the race question, but blacks will also be asked the question about ancestry. I've proposed since I've been there that there be a separate code for African American. Those who write down African American or black or colored or Negro--that they all be given a separate code.

VDT: In the ancestry question? Some people will write black or Negro in the ancestry question, having already had the race question?

FARLEY: A few people answer white by race, but will recognize that they have some black ancestry. That's extremely rare.

VDT: The answer to the question is wide open; there are not eight categories or something?

FARLEY: Wide open. In the Census Population Survey, they show you a flash card with 30 groups and you have to pick one of 30. I think Number 30 is "other." But for ancestry in the census, they will have 600 and some codes for what people write in.

VDT: What did they write in 1980, for instance?

FARLEY: The most common response in 1980 was English and about 24-25 percent of people write English. In the 1980 census, about 49 million people said they were English; 49 million said they were German; about 49 million said they were Irish.

VDT: You could write in as many as you liked?

FARLEY: Right. They coded the first two only.

VDT: What is your ancestry? Are you Irish?

FARLEY: I would say Irish to that question. But if someone pushed, I'd say, "Yeah, there's some German ancestry, also maybe some French ancestry." So that's the problem you get into.

Frankly, I think that for people who are in racial minorities and people who are first or second generation immigrations, you get something on the order of consistent information from the race, Hispanic, and ancestry questions. For people, whites primarily, who are third and higher generation Americans, it's not clear that you get much consistent reporting on ancestry. Some people know they're Dutch or Greek or whatever, but after three generations there's likely to be intermarriage and there's probably either some misinformation or lack of information about the country or nationality of origin of the people who came. And what do those answers mean? That's a subjective concept and some people will write religious groups, which the Census Bureau will not code. Others will write language groups. If someone writes French, we don't know if they're referring to a language group or to a country. Or a term like Moravian used to refer to Czechs at the turn of the century, but it's also commonly used to refer to one of the Protestant religions.

So there's a fuzziness about the ancestry concept and it may disappear from some future censuses. The question comes up, we will in the foreseeable future, anyway, continue to identify racial

minorities and Hispanic groups, but for that very large other group of us, will we be asked to give something in the line of an ethnic or ancestral origin, or will we be just not-Hispanic, not-Filipino, not-black? I don't know how it will turn out.

That's what I'm working on now--the quality of the data. There was a 1986 pretest of the 1990 census in which about 61,000 people were asked questions about race, Hispanic origin, and ancestry. Then some 18,000 were reinterviewed about six months later and asked the same questions. So I'm looking at how consistently did people report ancestry, among other things. And I'm also finding, for instance, that a lot of those who don't report on race don't report on whether they're Hispanic and they don't report on ancestry. I don't know exactly what that means. A lot of people don't know their ancestry and another significant fraction of population say they're Americans, and maybe we ought to accept them now.

VDT: Now let's talk about PAA. You have identified Chicago in 1958 as the first meeting you attended, and you've gone to almost every meeting after that. Here's the list of meetings since the beginning which Andy Lunde compiled. What do you remember about that first meeting?

FARLEY: I remember Phil Hauser discussing a book. I don't remember which book; I do remember it was interesting. [Philip Hauser was discussant of a paper presented by William Petersen on "Critical Evaluation of the 'The Study of Population--An Inventory and Appraisal,'" the landmark review of demography as a discipline of which Philip Hauser and Otis Dudley Duncan were co-editors, published the following year, 1959.] The only person who stands out is Phil Hauser. I remember spending a Saturday, which was probably May 3rd, given that at time the meetings were on Saturday and Sunday [1958 meeting was May 3-4]. Then the next meeting I attended was 1962 in Madison.

VDT: That was the last meeting on a university campus [University of Wisconsin]. Norm Ryder is very proud of that meeting. He got it up to Madison and you met in a smallish conference center on the campus.

FARLEY: It was a lovely warm spring weekend. We had had in Chicago that year a rather horrendous winter, so I remember it was a very pleasant climate. It was May 4th and 5th. It looks like that would have been the last of the two-day meetings? I see some later; I thought we switched to three-day meetings quite a while ago, but maybe not. [1966 in New York was the last two-day meeting.]

VDT: Was that Friday-Saturday or Saturday-Sunday?

FARLEY: I think Madison was Friday-Saturday. [Dudley Kirk in his later interview said he inaugurated Friday-Saturday meetings when he was president in 1960.]

VDT: You already had double sessions then. They started with one double session at the Ann Arbor meeting in 1956. The next year at the University of Pennsylvania, there were three double sessions and Anne Lee, who was running the local arrangements committee with Everett Lee, said people were furious with that. Now we're up to eight simultaneous sessions. There were 84 sessions at your meeting last year [1988] and will be 84 again this year. What stands out in your memory about the early meetings you attended?

FARLEY: I was very interested in the outspoken exchanges of views about demographic issues that I recall at the Madison meeting [1962], the earlier Chicago meeting [1958], and the Philadelphia meeting in 1963, which were the first three I attended. I found it very stimulating, as a graduate

student, to hear people talk about various things. I can remember asking a question or two from the audience at the Philadelphia meeting.

VDT: You dared to stand up?

FARLEY: Yes, while I was still a graduate student. It was April of 1963.

VDT: And everybody knew everybody else?

FARLEY: To a modest degree, yes.

VDT: Who were some of the leading lights then? You mentioned Phil Hauser dominated the 1958 meeting, as far as you can recall.

FARLEY: I don't remember any great detail on individuals. I knew the people at the University of Chicago and Phil Hauser used to introduce us to a large array of other demographers at the meetings. I don't remember any particular papers given at the meetings in 1962, 1963, and so forth. 1965 was the first meeting I gave a paper. That was in Chicago. It was on my dissertation research, black fertility trends in the U.S.

VDT: Do you remember outstanding meetings and issues over the years, say, in the early 1970s? Everybody says now the business [membership] meetings are so boring, because there are no hot issues. In the early 1970s, there were the Concerned Demographers, who wanted to get PAA more involved in policy issues, and the women's issues.

FARLEY: I remember at the 1967 meeting in Cincinnati, at the business meeting there were very strong claims made by the quote/unquote Concerned Demographers about the need for the organization to become more an activist group. And I suspect it was at that meeting, although I can't remember certainly, that some people pointed to the eugenics movement in the early 1930s and how we dealt with the eugenics movement in the 1930s as not only distinguished from demography and would that bad precedent be somehow a harbinger of what would happen if PAA became more of an activist group, as some people were advocating at that time. I remember that one in particular. But the following year in Boston [1968], I don't remember discussions of that nature. Concerned Demographers eventually published a newsletter a few times, so there was that kind of an issue.

I don't remember a very activist what you might call Women's Caucus. There has been something of a Women's Caucus, but we've now had seven presidents of the Association who were women; we've had women on the Board for a long time.

VDT: The Women's Caucus was formed in 1970 and became quite aggressive in their demands. PAA quickly went along with gathering more data on women in PAA and the profession in surveys that went out to members and that women should have more rights within the PAA and so on [see Harriet Presser on "The Women's Caucus in PAA," Vignette of PAA History, PAA Affairs, Winter 1981]. After that, Beverly Duncan walked out of the meeting and I think she even renounced her membership in PAA. Apparently, she did not go along with the position that women should be accorded affirmative action, say.

FARLEY: I can remember Beverly talking about that frequently at the University of Michigan--her strong opposition to either a real quota or something that looked like a quota for women. I think she felt very strongly that women could contribute on the basis of their own merits and didn't need

something like a quota system to help them.

VDT: How has PAA changed over the years and do you feel the changes have been for the better? There is the obvious change of numbers.

FARLEY: There is the one of numbers. The scope of what is done by demographers has changed over the years. To be sure, there were some early indications of what would come later. But public health as a field which incorporates demography has expanded such that at the meetings now there are lots of session where something having to do with public health and demography are very prominent.

VDT: You mean mortality? We've always had that.

FARLEY: Not only mortality, but, for instance, lactation and its effects on fertility, or to some degree, diseases and their effect on fertility, and now AIDS--that kind of melding of interests of traditional public health and demography. It seems to me that's a very big development.

Economic demography is now something different from just economic development, so it seems to me that's another major development--something having to do with economic issues in demography.

There have been a couple of other attempts that have never gotten very far in PAA--psychology in demography. There are psychologists in PAA and there may even be a section in the American Psychological Association that deals with population.

VDT: There is: Division 34.

FARLEY: Okay, but that's not quite as prominent in the PAA. I thought at one time it would become . . .

VDT: Jim Fawcett and that group didn't take off. But we have our Psychosocial Workshop; that's where they are. That's like PAA in the past; it has single sessions, 70 people there. I go to all of them, because my former boss, Henry David [director of the Transnational Family Research Institute], is very active in that.

FARLEY: So that's one area that has not taken off. The other thing, I think, is the increasing number of demographers who specialize in some foreign area. In the early days, there would be a few people who were specialists in Asia, a few who knew a bit about some of the European countries. Now we have sessions specializing in a country. So we have that kind of international focus, much more detailed and specified than . . .

VDT: You mean more detailed than a general interest in Third World demography?

FARLEY: That's right. If you'd had a session on Africa 20 years ago, you might have had a hard time filling that. Now we could have a session on West Africa or AIDS in Central Africa or population trends in sub-Saharan Africa--all those sessions would fill up, I think, because of the increasing specialization.

VDT: Increasing specialization, of course, means proliferation in numbers. Do you feel that the numbers of sessions are getting a bit out of hand--84 sessions, eight overlapping? Or is it inevitable, to be accepted?

FARLEY: I think you want to encourage membership and you satisfy the members if you provide them with opportunities to be on the program every two or three years and in that regard it's good to have a lot of sessions. I think most of us would have a taste for a modest-sized organization, rather than a huge organization.

VDT: By modest-sized, you mean . . .well, let's talk about meetings. Modest-sized, you think would be 1,100? That was the number [actually 1,115] at your meeting [1988], which was the record number so far, except for the Washington meeting in 1981 [1,167].

FARLEY: I think 1,100 is a good number. I would guess 8,000 is a bad number, because you are very isolated. It's my impression that even the American Statistical Association meetings, which run in the neighborhood of 3,000, in some ways that's too large. But that's partially a matter of my taste. I don't know if there's any law about organizations dividing when they get very large. I say that because the American Psychological Association has been going through its splits between theoreticians and practitioners. The anthropologists are very likely to split. Political science as a professional organization has gone through some difficulties because of the high degree of specialization--need for the various wings to have their own journals. So there may be something quite dysfunctional about a professional organization reaching the size of 15,000 or something of that nature. I don't know.

VDT: You think the PAA is safe with 2,600? It's fluctuated at that for a number of years; it can't seem to go up [2,679, end 1989; 2,752, end 1990] [But a peak of 3,532 in 2015].

FARLEY: No, it hasn't gone down, but it hasn't gone up.

VDT: Why do you think that is? People have often pointed out--Jay Siegel first in a paper at a meeting in the 1950s ["The Teaching of Demography," 1951]--that there are hundreds more people teaching courses in population than there are members of PAA. Is PAA still fairly exclusive--those who are really doing research in the field? Although we now have more and more applied demographers.

FARLEY: Yes, but they're doing research--those people who are estimating state populations or looking at health issues in local areas. I think PAA is a professional association for people who have a pretty serious commitment and spend a lot of their time either teaching demography or doing demographic work. I imagine there are courses taught at small colleges or community colleges, but the one who teaches a demography course is probably teaching three or four or five other courses and is not a member of all those professional associations.

Certainly, there are a lot people who are doing demographic work in marketing. We have expanded there; I think we are increasingly tapping into that area. There are very many people who use some elements of demography in that kind of work. We also have had, I think, increasing contacts with and memberships from geographers who do things with population materials, so we have had some expansion there.

VDT: It's the European tradition that demography came out of geography, much closer than here--territorial distribution.

FARLEY: Yes. I think geographers were pretty rare in the PAA for a number of years and I don't think we've ever had a geographer as president. But I think now we're seeing more people whom we would identify as geographers coming in.

VDT: And a couple of sessions cosponsored by the Association of American Geographers.

Let's go back to the problem--if you want to call it that--of the 84 sessions, eight overlapping. I've asked people before what's the solution--another day? Of course, more and more is being added. This year [1989], for instance, there are even workshops Wednesday night before the beer party, and many more on Thursday night. Add another day, or . . . ?

FARLEY: Well, I suppose that's something the Board and the president will have to wrestle with in the future. Adding another day would impose extra costs on those who want to come and it's not obvious that it would increase the attendance if you add another day. Maybe there's merit in doing it. I don't have any strong feelings about it.

VDT: What about being very strict on the number of times people can appear on the program? Of course, it's been pointed out many times you have to give people the opportunity to give a paper; their way is paid if they give a paper. So that explains in part the proliferation of sessions. But what about being very strict: you can be only a chair, only one author, or coauthor, or whatever? Now some people get around that rule and appear two and three times on the program.

FARLEY: Well, they shouldn't be on more than twice, but they do get on twice. Maybe the solution would be to have a person appear only once or even once every other year. You could have such a rule. The question would be, would the quality of the papers and the overall quality of the meeting be compromised if we went in that direction. We don't know how to measure--at least we don't try to measure--the quality of these sessions. But I've thought there were some of those 84 sessions which were really so weak that they should have been wiped out, or if there's one or two good papers, they should have been put together with some other good papers.

None of these are easy decisions to make. Because maybe you've got an excellent paper on what's happening in Chile and another excellent paper on population redistribution in Edmonton and you can't put those two together in one session. So I don't know what the solution is. I think basically our meetings are pretty open to anyone who wants to get on. We might go to nine overlapping sessions [and did in 1990 and 1991]--who knows?

VDT: Let's talk about your plans last year. Many oldtimers, of course, deplore the lack of free exchange from the floor that was possible when the meetings were much smaller and everybody went to the same sessions--as you said, you had the nerve as a graduate student to stand up and ask a question. With the meetings much larger, that's not possible. Did you . . . well, people have praised your author-meets-critics sessions of last year. That gave people a chance to participate. Plus the panel sessions are supposed to give people more chance, but people have also said those have degenerated into paper sessions too. So there's not so much exchange from the floor.

FARLEY: It's very easy for any session to degenerate into a paper session, in that someone thinks that they're going to have 15 minutes and they want to put on a good show in that 15 minutes, so they work very hard on a paper and don't like to be interrupted by somebody telling them that their ideas are loose or something of that nature. So, yes, I think we have a problem in having informal exchange. But we did have some panel sessions.

And I had one other session which I hope was somewhat innovative last year, where Cam Gibson from the Census Bureau and Gerry Hendershot from NCHS were supposed to do a kind of show-and-tell with graphics, Gerry doing things about data from the National Center for Health Statistics and Cam doing things from the Census Bureau ["Current Trends in United States Population: A Visual Presentation"]. I don't know if that was a success or not, but it was meant to be something other than the regular paper session.

VDT: Harriet Presser must have decided it was a success, because she's having something similar sounding this year [computer demonstration sessions].

FARLEY: I tried to encourage poster sessions, but I got a very small number of people. There are so many things in demography where a person with five good posters could illustrate their findings-- trends in some disease, trends in fertility, in migration. If they were to post five or six good figures and then stand there for an hour to talk to whoever wants to come up to talk to them about it, it would be a useful exchange. I tried to initiate that and I think there were five or six poster sessions, meaning people who were there talking about their findings as illustrated in some set of charts or graphs. That's been very popular in other associations.

VDT: Harriet again is going to try that this year, I see. I frankly have never gone around to look at those. Did that come out of your concern about good graphics?

FARLEY: It came out of good graphics, and alternative ways. As you say, there is something a little formal, sometimes dry, about a man or woman getting up and reading a 25-minute paper. Poster sessions are an alternative. Particularly if you're in a specialized area or you're doing preliminary work, you can discuss it presumably on an informal basis with other people who're interested, maybe get together, go out for a drink or lunch with them, and presumably that would exchange information in something other than a formal session. The roundtable luncheons do that too, not that they are all that successful.

VDT: No, it's a little noisy. I was going to ask you about the roundtable luncheons, because that was obviously a vehicle for you. You seemed to have made a conscious effort to draw in the oldtimers. Last year, you had six past PAA presidents among 25 who were in charge of those roundtables.

FARLEY: Yes.

VDT: For instance, Amos Hawley, whom I interviewed just before that. That was wonderful; he hadn't been there in years. Of course, Judith Blake had been, and you had some others like Ansley Coale, who is still a regular.

FARLEY: I was hoping to get Joe Spengler, but as you mentioned--I had asked George Myers if Joe could do it and he told me Joe had Alzheimer's. I asked Don Bogue to do a table on the history of demography, but he was going to be somewhere South of the border and couldn't do it. I think I had two or three other past presidents.

VDT: You had Judith Blake, Ansley Coale, Richard Easterlin, Hawley, Con Taeuber, and Charlie Westoff.

FARLEY: I think I asked a couple more, besides Spengler and Bogue. At one point, I had Ron Freedman down for a luncheon table, but he then gave a paper . . .

VDT: An excellent paper on the University of Michigan population center. I was especially pleased with that. Then there was the Kingsley Davis session on "Two Centuries after Malthus: The History of Demography."

FARLEY: Yes, which drew a very good crowd. I frankly thought that would not draw a large crowd.

VDT: That's an interesting thing. Norman Ryder in his interview said that in this country there has not been a tradition of interest in the history of demography. There is in France--of developments since Malthus or even before. And he was hoping to do something on that. You talked earlier of your interest in my project; you feel there should be a history of demography in the U.S. How about you taking it on--20 years from now?

FARLEY: Maybe, I don't know.

VDT: Very good. How did you organize your preparation of the program last year? Incidentally, do you know when they shifted from the first vice-president being in charge of the program to the president?

FARLEY: No. I heard that Jane Menken [1985 president] resisted--thought that it was too much for the president to be in charge of the program. But I think I do know. Because when Evelyn Kitagawa was going to be president in 1977, she was going to be obligated to organize the program twice.

VDT: You mean she came from being first vice-president to president?

FARLEY: Yes. [Kitagawa was first vice-president in 1974-75, responsible for 1975 meeting program.] And she asked me to organize the meeting, so I organized the 1977 meeting in St. Louis. [The meeting program became the incoming president's responsibility in 1976 with Sidney Goldstein's tenure, 1975-76. He too had been responsible for the program two years earlier, 1974, as first vice-president, and asked Charles Nam to organize the 1976 meeting.]

VDT: That was the year the terms of office were switched to the calendar year for officers [except secretary-treasurer]. So you were an old hand--you did it twice. Did you have a committee last year? You didn't try to do it all by yourself?

FARLEY: Oh sure, I did it mostly by myself.

VDT: Jay Siegel claims he did it by himself [in 1980], except three women in his office at the Census Bureau helped him. He said that's a stupid way to do it, but his committee members didn't contribute much. You did it mostly by yourself?

FARLEY: Yes. I found it difficult to use a committee because of the tremendous overlap. I thought maybe I could have a committee and have one person do fertility, another do mortality, one doing developing countries, and another doing the U.S., or make some set of categories like that. Then you're inundated with suggestions for papers which are the effects of infant mortality on fertility or mortality in developing countries and you realize there is no way you could sort those papers out. So I ended up doing the program, because I thought I would be forever calling up one sub-organizer and telling him or her to call up some other sub-organizer because the paper overlapped.

VDT: The organizing consists of designating people to be the chairs and organizers of sessions and fitting papers into those sessions, or vice versa?

FARLEY: No, first you get the organizers of sessions and the title of their session. Now, people write in with suggestions and I--as I think most other program chairs do--you take the suggestions. Some people suggest themselves, others will suggest "anybody," in which case you find one, but that's the pool of candidates to be organizers. Then the list of that set of organizers is sent to all the members

and members are told to send their papers to the appropriate organizers, but a lot of members send their papers to you or organizers send the papers to you. So there's a lot of sorting out of papers to be done as the program goes on.

VDT: But you can pretty well take the initiative in deciding what topics are going to be covered?

FARLEY: Oh yes, as long as you can find an organizer. Last year, for instance, I wanted to make sure that AIDS was on the program appropriately.

VDT: It was on it several times.

FARLEY: That was Wendy Baldwin. I called Wendy: "Who should I get to do AIDS?" I got some suggestions from people who volunteered themselves, like Andrei Rogers wanted to do something on training issues in demography, but in other cases you had to scout somebody out. I tried very hard to get someone from the University of Montreal to do something on the French population of the United States, since we were meeting in New Orleans--the French heritage population, people like Acadians who were shipped out of Acadia by the British--and I did not. I thought there was somebody up there who would be working on the history of the French population of Canada who would also be going occasionally to Louisiana to look at the contacts or something, but I never got any positive response on that. I did get one session there on Cajun life, supposed to be Cajun demography.

VDT: And you also had the Cajun band at the alumni night. Did you start the alumni night party--having all the different universities join together?

FARLEY: Yes. The different universities used to have parties in hotel rooms separately, and sometimes the hotel would come around and shut parties off because people were too rambunctious. So we put that together and hired the band.

VDT: It worked well.

FARLEY: I thought it worked well.

VDT: The dance floor wasn't very good; we were dancing in a carpeted corridor. But otherwise, it was great fun.

FARLEY: I wasn't sure whether it was going to be a success or not, so I didn't want to reserve a big room for that. Also, we knew it was going to be noisy, so it had to be away from other places in the hotel. So that wasn't quite ideal, but it wasn't a total disaster.

VDT: I thought that was a great idea. Other years, you'd go to your university party, in my case it was Georgetown, and you felt a little diffident about dropping in on others.

FARLEY: But you did. None of those parties could be exclusive. People didn't know how much liquor to bring. Some hotels tried to make you pay a fee if you brought liquor in, because of their local regulations. So to avoid that hassle, we had the one alumni night party.

VDT: And that's going to happen again this year. Another thing you have introduced is the fun run. Was the first one at Washington, in 1981, for the 50th anniversary meeting? I proudly show off my T-shirt from that one.

FARLEY: The first one was in Philadelphia in 1979, which was a lovely fun run through Fairmont Park, finishing on the steps of the art gallery. It was after Charlie Nam's presidential address on a Friday afternoon--a lovely, warm Friday afternoon--5.:30 or 6 o'clock and dusk was just gathering when we finished on the steps of the art gallery. Then we went around the capitol in Denver [1980] and we went up Rock Creek Park in Washington [1981]. San Diego [1982] was a very ugly site; the hotel there was ugly.

VDT: I agree with you. It was my first time in San Diego and it was a disappointment. We were isolated . . .

FARLEY: We were isolated. We did a fun run there. We had to go on some highway; there wasn't much traffic, but it was unpleasant.

VDT: And it was an overcast week; there was no sun. Were the fun runs after the first one in the morning--Saturday morning?

FARLEY: Yes, they were all in the morning.

VDT: And the T-shirts. I've got mine right here from 1981, green, with "PAA 50 years," and you turn it around and it says, "Running reduces downtime."

FARLEY: We had T-shirts for Denver also; that was the first time. There was one another year with a PAA fun run that Dudley Poston organized. I think we've had T-shirts for three runs.

VDT: You're obviously a jogger.

FARLEY: Yes [and a marathon runner].

VDT: And another outstanding thing about Ren is that he's always looked about half his age. Everybody thought you were too young to become PAA president.

FARLEY: But Sam Preston looked very young when he was giving his address [in 1984].

VDT: But he was young.

FARLEY: I think Jane Menken is younger than I am and she looks young.

VDT: You're just about a year apart.

FARLEY: Jane and I are very close; we've talked about that. Sam is younger than either of us, I think.

VDT: Yes, about four years younger than Jane. Now I want to talk about becoming president, how you work your way up the old-boy/old-girl network to become president. In a couple of interviews I've had, we've talked about the fact that two top people run against each other for the presidency: one wins, one loses. And the losers are outstanding people, like Chris Tietze. Last year, Joe Stycos lost to Harriet Presser. I think the gender issue was involved there somewhat. And you ran against Paul Schultz. Then these people sort of drop, which is unfortunate. Have you got some comments on that? How do you get, first, in the position of being nominated to be president?

FARLEY: I think almost everyone who has been nominated to be president has had a long involvement in demography and in the Association, through committee or other service. It seems to me these days the nominations committee selects two distinguished people, either of whom is very well qualified to be president. I think unlike some other associations, we don't put a loser up against a winner. I think in some elections for professional associations, there's kind of a designated victor and a person who is on the ballot for some other reason. Now that's good that you have two strong candidates running. But it does mean that both of those people are likely to be quite invested in the thing and one of them will be disappointed.

VDT: It's unfortunate. Apparently, some people have run twice for the presidency, but I would think you decide, no, that's it. For instance, with Joe Stycos--he's much older than Harriet--some people felt it was a bit unfortunate, because he will not run again. Harriet could have run again.

FARLEY: Yes. Have there been many people who've run twice?

VDT: I don't know; I'll check that.

FARLEY: Have they been elected?

VDT: I don't know. That's a historical detail to check. [Paul Schultz ran again in 1989, for president-elect of 1990, and lost out to Ronald Rindfuss.]

You still enjoy the PAA meetings?

FARLEY: Yes, definitely.

VDT: Even though they've gotten very large and it's sometimes hard to see everybody one wants to?

FARLEY: We do have the beer party the night before, which I think is pretty functional in letting people get in contact with other people. And we certainly instructed Ed Bisgyer [former manager of PAA business affairs at the American Statistical Association] to get us hotels where there was good space for informal meetings. That did not happen in San Diego, but that was the extreme in lousy meeting space. Most other places have had good places for individuals to meet other individuals.

VDT: Although Norm Ryder complained that at the hotel in New Orleans, there was no place to sit down in the open public area.

FARLEY: He's partially right, but with the homelessness issue being what it is, I think there are going to be darn few hotels that have the traditional old lobbies with lovely places to sit. Anybody can come in off the streets and sit there.

VDT: That's not something I realized. You mean that's why hotels have not wanted to have seating . . .

FARLEY: Many hotels, if they have public areas, they're either small, they're unattractive, or they're on the third floor so that they can discourage someone from coming in.

VDT: Final questions. Obviously, you've had a wonderful career. You've enjoyed it, right?

FARLEY: Yes. But it's not quite over, Jean.

VDT: Right! You still have decades.

FARLEY: I want to do some traveling.

FARLEY: In connection with your career?

FARLEY: No, not in connection with my career. It would be fun to take off three months and travel sometime, which I've never done.

VDT: You have been very involved in U.S. demography, demography of blacks. That's unusual among demographers now; most have had international demography as part of their work too. And you've had Ron Freedman as a colleague, who is involved in the Third World. Is it just that you did not have time for that?

FARLEY: I think it was not only time but sort of deliberate choices on my part. When I came to Michigan, Ron asked me several times would I be interested in getting involved in the Taiwanese fertility project. I thought about it, but never pursued it because I found it very interesting to work on U.S. population trends, minority groups in the United States. So my work has been almost entirely on the United States. I would perceive probably continuing to do that. But if another opportunity presented itself . . . I think increasingly I'm getting into not just blacks in the U.S. but sort of the assimilation or competition among ethnic groups, minority groups, the new immigrants coming into the United States. I can imagine working on what's happening to our country as increasing numbers come from Asia and Latin America. I might get abroad sometime, but I haven't done so thus far.

VDT: Your career has been basic research. Is there still room for that in demography? Now, increasingly, demographers are going into applied fields. Is that where the future lies, or is there still a chance for someone to have a career such as you have had and people like Ron Freedman and Phil Hauser?

FARLEY: Phil has been applied and I would say that Ron has had some applied elements in his research. Who knows what the future of demography holds? I would think, though, that as has been the case in the past, the problems that demographers address are often contemporary problems, be it fertility, mortality, immigration, assimilation questions, and that funding to do research is going to come from foundations or governmental agencies interested in that problem. So just as in the past, I would think that in the future a lot of the research will be addressed to contemporary issues.

On the other hand, we certainly do give accolades to people who work on different things. John Knodel's work on 17th century European villages is very different from what most applied demographers do. And I think we still do have an interest in those issues, demographic history and some of the mathematical modeling that goes on in demography. It's pretty abstract, something that economists or even mathematicians might do, rather than somebody else. On the other hand, if you were going to simulate the spread of aging in a society, I suppose you'd use some very sophisticated mathematical model. It seems to me demography has always had quite an applied element to it.

And I think that's why the future of demography is reasonably bright, that we're going to continue to have a whole series of issues about population. As we reach zero population growth, there's going to be one set of issues. If our population expands rapidly, that's another set of issues. If our growth is entirely attributable to immigration from Latin America and Asia, that's going to raise another set of issues. We're going to continue to have in the United States, it seems, lots of migration and shifting distribution. So we'll be having a lot of challenging things for demographers to do.

VDT: For basic research, in addition to applied . . .

FARLEY: You draw a line between basic research and applied. I guess I see those two things as pretty much the same. You've got a set of issues and it stimulates some scholarly activity and many people who do the research are going to abstract from that one problem, while they may solve that problem, they will write more generally on the topic. So I don't think there's quite that dichotomy.

VDT: That's interesting, because others, more traditional demographers, would make the dichotomy.

FARLEY: Yes.

VDT: Certainly, of the applied demographers who work in state and local government and in business. But you don't?

FARLEY: No. I think of some of our better graduates from the University of Michigan who have gone out into applied work . . .

VDT: You mentioned Paul Voss.

FARLEY: Sure. And others who've gone on to commercial marketing, for instance. I guess I don't see the huge distinction. Now, there is a distinction in what's rewarded. If you're working for a marketing firm, I assume that writing scholarly books doesn't get rewarded the same way that if you're working for a university and start writing books.

VDT: Do you think that applied demographers should continue--at least some of them--to get doctorates, or is a master's degree program, sort of a journeyman apprenticeship training, enough? You have, for instance, your program of Robert Groves at Michigan. I've inquired, because I have a niece at the University of Western Ontario, where there's one of Canada's few population centers, in her last year of sociology and interested in going into business demography. There's also Paul Voss's program at Wisconsin [where the niece ended up].

FARLEY: Yes. I think for a lot of positions a master's degree is very appropriate. I suppose it's the unique set of skills that a person brings to the job. There have been some very distinguished demographers at the Census Bureau who didn't have doctorates.

VDT: Yes, Jay Siegel.

FARLEY: Jay Siegel, okay. A master's degree is very appropriate for certain circumstances. Again, it's the individual skills and interests and so forth. Who was our last president who didn't have a doctorate? Jay Siegel, I guess [president in 1980].

VDT: Jay Siegel has not--he's very conscious of it--and Art Campbell [president in 1973-74]. Perhaps some of the very early ones did not.

FARLEY: Clyde Kiser, Vance?

VDT: Yes, they all did. But Jay Siegel and Art Campbell did not and, funnily enough, Jay Siegel at least is very conscious of that, because he felt that being in the accepted group, you had to have a doctorate. But certainly he went on to write books and has taught in universities everywhere.

FARLEY: That's true. My guess would have been that there were more presidents who didn't have doctorates, but now that I look at this list, I realize that they all did. Whelpton [president, 1941-42] did not have a doctorate, I don't believe.

VDT: That's the other outstanding exception. Jay Siegel mentioned him.

FARLEY: What about Truesdell [president, 1939-40]?

VDT: Truesdell did. I've just transcribed Philip Hauser's interview and he was recruited by him, interviewed by him, for the job at the Census Bureau and he kept referring to him as Dr. Truesdell.

FARLEY: Truesdell died within the past five or six years. He lived to be quite old and I think he wrote poetry. [Notestein in his interview confirmed that Truesdell was a poet and said he "didn't complete his doctorate until long after he was in the Census Bureau."]

VDT: Phil Hauser described him as a taciturn New Englander, not very forthcoming. Anything more on presidents? Joe Spengler--why were you after him last year?

FARLEY: I was very impressed with Joseph Spengler. When I went to Duke, he was the senior demographer there and he was among the most scholarly individuals I have ever met, tracking down obscure references which might bolster a point he wished to make. I was very impressed with Joe Spengler. He's also an Ohioan. Came from Piqua, Ohio; they pronounce it "Pickway" in Ohio.

VDT: Did you ever know Rupert Vance?

FARLEY: No. And I did not know Margaret Hagood.

VDT: She died at a reasonably young age. There's a sad story about her. Henry Shryock attributes her death in part to her despair over the fate of her friend Hope Eldridge, who was a victim of the McCarthy era, hounded out of her position at the United Nations, because they claimed she had belonged to Communist organizations, which was quite untrue. Margaret Hagood also had a heart problem.

FARLEY: Con Taeuber's appointment at the Census Bureau was held up a year or so because of the McCarthy stuff. Con had written something about social change in Russia in the early 1930s, I was told--I don't know this for a fact. Maybe he even visited Russia in the 1930s. And somebody picked out this article he had written, probably 15, 25 years earlier, and that delayed his appointment at the Census Bureau [in 1951] during the McCarthy era. [Conrad Taeuber describes this affair in his interview. The article Ren Farley recalls was a special issue of the American Sociological Review put out after the U.S. entered World War II "with information about our wartime ally, the USSR. The Soviet-American Friendship Society informed its members of the existence of this publication and that it could be ordered from Conrad Taeuber (managing editor of the Review) at the Department of Agriculture. In 1951 when I was to join the Census Bureau, my necessary clearance by the FBI took much longer than expected. From my FBI files, which I obtained years later under the Freedom of Information Act, it was clear that the special issue of the Review had been the focus of their extra-long investigation. Lowry Nelson in describing the McCarthy era in Washington in his autobiography wrote of being questioned in his office by an FBI agent on the loyalty of Conrad Taeuber to the U.S. Lowry had no doubts on that score. The FBI finally sustained that judgment."]

VDT: Oh, dear. Maybe this is why he is so conscious of this. He recently sent me some material he'd found: a sort of disclaimer, signed by Hope Eldridge, saying, "I have not had anything to do with the Communist party," and a newspaper article on the fact that Dag Hammarskjold had gone along with this business and some UN employees were dismissed because of the allegations from the U.S. government. The UN system was supposed to be above that.

FARLEY: Yes.

VDT: Did you know Irene Taeuber?

FARLEY: Oh, sure. Irene was a very nice person. I remember being interviewed by her at Michigan; she was doing some survey of some kind. I guess I've known most all the presidents, some of them not very well. I didn't know Harold Dorn. I've met Cal Schmid, but I didn't know him very well. John Durand I met. Going back toward the earlier ones, Con Taeuber, Frank Notestein, Frank Lorimer--I did not know Whelpton, and the early ones I did not know at all. But I think I've had a good bit of contact with all the more recent presidents of PAA.

I remember one story of Joe Spengler. He used to talk about going to the PAA meetings, maybe in Princeton, and taking I guess it was the Seaboard Interline Railroad up from Raleigh. He used to go up and sit with Rupert Vance and he somehow used to try to get Rupert to order something in the dining car and he knew Rupert wasn't going to finish it all and then he could finish it--scallops or something. Joe Spengler used to have lots of stories like that; he was a very interesting person. Too bad you can't interview him.

VDT: He was a storyteller besides a scholar?

FARLEY: Oh, yes, he was a storyteller.

VDT: He was still writing and publishing in the early 1980s. He's considered one of the great overall theorists in the field, like Kingsley Davis.

FARLEY: Yes.

VDT: What do you think about Kingsley? He's having another session at the coming meeting and he's involved in a new book.

FARLEY: And he had the thing in the op-ed page of the New York Times.

VDT: Oh, yes! Retirees play golf and cost us so much ["Our Idle Retirees Drag Down the Economy," New York Times, October 18, 1988].

FARLEY: I know Kingsley; I don't know him intimately.

VDT: I think you're going to be another Kingsley, because you keep yourself so fit. Maybe you won't be producing children when you're 79. Do you have children?

FARLEY: No, don't have any children.

VDT: Where did you meet your wife?

FARLEY: I was teaching at Duke and she was a graduate study in psychology at Duke.

VDT: What is she doing in these six months in Washington?

FARLEY: She got a letter yesterday from the District government. She's a forensic psychologist. When she was here last time, she worked with the District's . . .

VDT: When were you here last time?

FARLEY: I was here for a sabbatical leave in 1981 and 82. The Reagan freeze had just come into effect. She worked with the agency then. It's the forensic division of the superior court of the District of Columbia. So this time, she finally got appointed to a job there, which barring cutbacks on the part of Marion Barry or something, she will start a week from Monday.

VDT: Despite our indefinite D.C. budget, she nevertheless got a job. Great. Thank you very much, Ren.

FARLEY: This was a lot of fun. I enjoyed it; it's interesting. You've got a vast amount of good information.

VDT: Absolutely. It's so interesting and revealing. I learn a bit from each of the interviews, for instance, your telling me that Norman Ryder's view of the establishment of Demography is not quite right.

FARLEY: I said it's not quite Don's view. I was a young man at the time, but Don and Norman, I think, have different views.

VDT: And the fact that I just finished transcribing Phil Hauser's interview and there you are on his list of 21 outstanding American students at Chicago. Of course, there are the foreigners too.

After the Starting Line: Blacks and Women in an Uphill Race

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"Mighty causes are calling us—the freeing of women, the training of children, the putting down of hate and murder and poverty." (DuBois, 1909:74)

In the decades following the Depression, the nation addressed the question of the place of racial minorities in our society. This debate—and the sometimes violent civil rights struggle—occurred concomitantly with a reexamination of the roles of women.

The struggle for equal rights for blacks was carried out in three major areas. First, efforts were directed toward razing those barriers that whites had erected to maintain their social isolation from blacks, an isolation based on the assumption of black inferiority. On the legal side, the Brown decision [Brown v. Board of Education, 347 U.S. 483 (1954), 349 U.S. 294 (1955)] in theory struck down state-imposed segregation, and on the activist side, bus boycotts and lunch counter sit-ins led the country to recognize the inequity of racial segregation. (For descriptions, see Bloom, 1987; Carson, 1981; McAdams, 1982; Morris, 1984; Whalen and Whalen, 1985).

Second, both blacks and women sought economic opportunities. The right to be employed or promoted on the basis of ability rather than skin color or sex was certainly not widely supported four decades ago (Burstein, 1985:Ch. 3; Schuman, Steeh, and Bobo, 1985: Ch. 3). Gradually we adopted the view that race should not be the criterion determining who gets a job or how much he or she earns. Perhaps in the future gender will be seen as unimportant in the labor market.

Third, the enduring fight of blacks to obtain the same political rights as whites reached a peak. Supreme Court rulings overturned the ruses used by southern states to keep blacks off the voting rolls; and in 1965, 45 years after the Nineteenth Amendment gave women suffrage, a voting rights bill guaranteed blacks the franchise (Garrow, 1978).

These efforts to overturn discrimination took place concurrently with major demographic and economic shifts. The movement of blacks from the agricultural South to the urban North led to higher earnings, an influential black electorate, and a black middle class. It also meant, however, that a growing share of blacks found themselves isolated by the residential segregation that characterized northern metropolises. (For analyses of these trends, see Massey and Denton, 1987; Taeuber and Taeuber, 1965; Van Valey, Roof, and Wilcox, 1977; Wilger, 1988).

Rather than focusing on the isolation of blacks from whites in neighborhoods or in public schools, I will turn to issues concerning the integration of blacks into the economy and the changing composition of black families. I will argue that if we are to understand the continuation of high rates of poverty among blacks, we must examine the consequences of three major social changes: the civil rights revolution of the 1960s, shifts in employment and industry, and most important, the social and economic roles of women. In brief, the civil

rights revolution and the easing of gender barriers are intertwined and have enormous consequences for the economic status of blacks, especially that of black women and children. Furthermore, changes in the economic and family status of black women are leading indicators of what is likely to occur among whites.

The Changing Economic Status of Blacks: A Search for Explanations

For three decades following the Depression, the economic status of blacks and whites improved, but the gains on many indicators were greater for blacks, and thus their relative deprivation declined (see Farley, 1984; Farley and Allen, 1987; Freeman, 1976; Levitan, Johnston, and Taggart, 1975; Newman et al., 1978; O'Neill, 1986; Smith and Welch, 1986). A useful summary is the poverty rate. Figure 1 illustrates that the poverty rate, which is based on a household's pretax cash income, including transfer payments but excluding in-kind benefits (U.S. Bureau of the Census, 1987a:App. A), declined among blacks from a level of about 90 percent in 1940 to a low of 30 percent in the late 1960s. There was a parallel reduction among whites, although the improvement was smaller, leading to a decrease in the relative odds of blacks being impoverished.

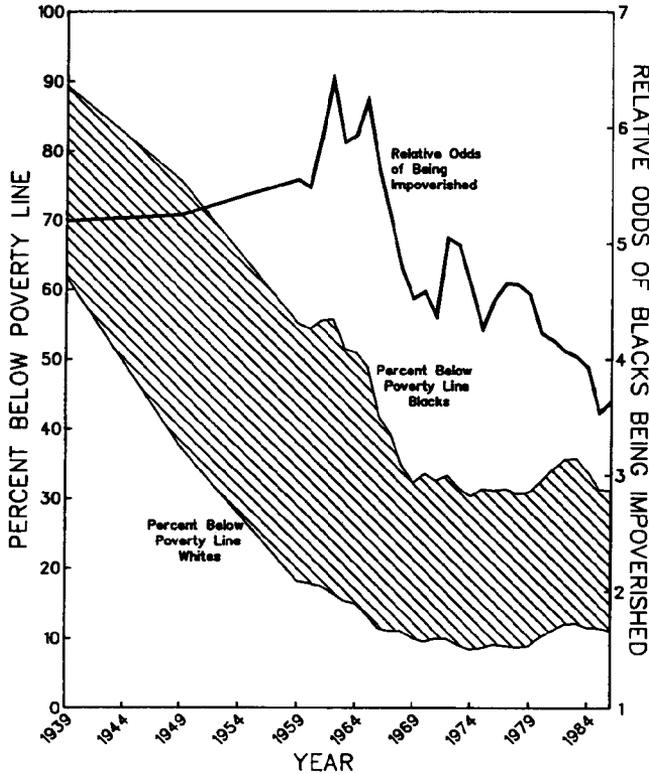


Figure 1. Percentage of the Black and White Population Below the Poverty Line and Relative Odds of Blacks' Being Impoverished, 1939-1986 [This figure shows the percentage of the population living in households that had pretax cash incomes below the poverty line. In 1987 this was \$11,203 for households of four persons. Data for 1939, 1949, and 1959 are from decennial censuses; for other years, from the Current Population Surveys. Figures for blacks for 1960-1967 are estimated from data for nonwhites. Source: Ross, Danziger, and Smolensky (1987), U.S. Bureau of the Census (1975, 1986, 1987b).]

The last two decades stand out because the poverty rate has fluctuated within a narrow range: roughly one-third of the blacks and one-tenth of the whites lived in poverty (U.S. Bureau of the Census, 1987b:Tables 1 and 2).

There is controversy about the reasons for the limited improvements in the status of blacks and vituperative debate about which policies should be abolished or initiated. Antidiscrimination measures and federal civil rights laws, many of them bolstered by encompassing court orders, were in place for the last two decades but not in the earlier period. The racial attitudes of whites have become more approving of equity for blacks (Schuman, Steeh, and Bobo, 1985:Chs. 3 and 5), the racial gap in educational attainment has narrowed (Smith, 1984), and the test scores of black children are gradually moving closer to those of whites (Burton and Jones, 1982). These changes should have narrowed the economic gap, but as Figure 1 shows, they did not.

Why has the relative status of blacks not improved? There are four popular overarching explanations. One view, forcefully argued by Murray (1984), Sowell (1981, 1983:Ch. 4), and others (Gilder, 1981:Ch. 12; Loury, 1984; Williams, 1982), contends that many blacks are marginal workers without the skills or training to achieve at high levels. The well-intentioned transfer programs that were initiated or expanded during the War on Poverty, it is assumed, trap many blacks because they offer a dole just about equal to what marginal workers might earn. Blacks are seen as overlooking opportunities open to them, rather than imitating their ethnic predecessors who came to cities earlier this century and struggled successfully to create economic niches, primarily because of the crutch of welfare.

Second, there is the contention that macroeconomic shifts combine with demographic trends to keep blacks at the bottom of the ladder. In the 1960s version of this hypothesis, it was asserted that residential segregation confined blacks to central cities, where jobs were disappearing, while employment was increasing in the white suburbs, thereby exacerbating racial differences in earnings (Kain, 1968; Noll, 1970). In the more recent version, it is argued that in many metropolises, there has been a disappearance of those entry-level jobs, especially in manufacturing, which once employed numerous black men (Kasarda, 1985; Wilson, 1985).

Third, for a quarter-century we have heard arguments that the problems of blacks are fundamentally rooted in family structure. In the 1960s, Moynihan (U.S. Department of Labor, 1965), Lewis (1966), and Banfield (1968) suggested that residents of urban ghettos shared a culture of poverty, meaning that they were often not successful as employees, largely because they lacked the psychological skills needed to succeed in school or on the job. This occupational failure, in turn, prevents blacks from establishing and maintaining families. Wilson (1987:Chs. 3 and 4; Wilson and Neckerman, 1986), in his trenchant descriptions of Chicago's ghetto, elaborated on the Moynihan view by implying that the dearth of employment for black men precludes their establishing stable families and leads black women to expect little from them.

Fourth, there are forceful, although not convincing, arguments that racial discrimination has not been basically altered. Changes in white attitudes are viewed as superficial, and the basic practices that facilitate the entry of white men into powerful positions are presumed to be still operative (Pinkney, 1984).

Economic Changes Among Blacks

Before accepting any of these explanations for the continuing high levels of black poverty, we must examine what has happened. We find two apparently contradictory trends when we look at the economic status of black men. First, their wage rates have gradually moved closer to those of whites, although the racial differences certainly have not been eliminated (Farley and Allen, 1987:Ch. 11; Hirschman and Wong, 1984; O'Neill, 1986;

Smith and Welch, 1986). This convergence of earnings occurred both before and after the Civil Rights decade. Indeed, it continues into this decade, since a comparison of data from the 1980 and 1987 March Current Population Surveys shows relative gains in earnings for black men. If we consider another index of the incorporation of black men into the economy, namely the occupations of employed men, we also find that the status of blacks has improved vis-à-vis that of white men.

But a second contradictory trend among men is the decrease, especially since 1970, in the rate of employment and labor force participation. Figure 2 reports the number of years blacks and whites would be employed, unemployed, or out of the labor force as they aged

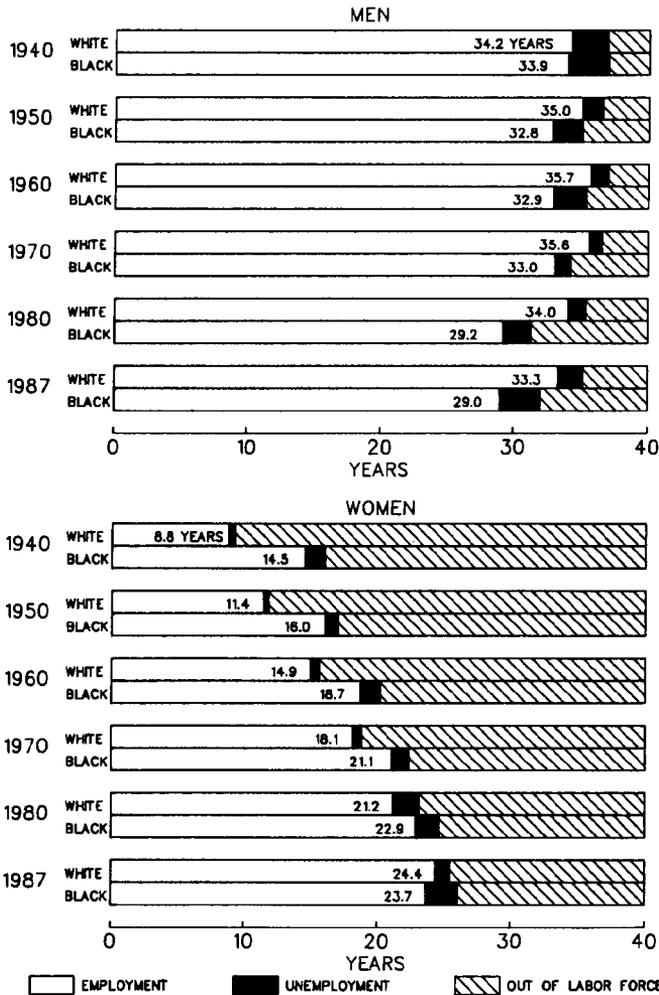


Figure 2. Expected Years Spent Employed, Unemployed, and Out of the Labor Force, According to the Employment and Labor Force Participation Rates of 1940-1987, Black and White Men and Women (These are period measures that show the number of years a person would be employed, unemployed, or out of the labor force if he or she lives from the ages of 25 to 64 and the rates of a year are unchanged. This assumes no mortality. Source: Microdata public use samples from the censuses of 1940-1980 and from the March 1987 Current Population Survey.)

from 25 to 64 years according to the rates observed between 1940 and 1987. This synthetic cohort approach summarizes the rates of one year in the same way a total fertility rate summarizes birth rates. Unemployment rates fluctuate with economic conditions, but there is an unambiguous secular trend toward more years spent out of the labor force on the part of black men.

The outcome of these conflicting trends has been to dampen but not eliminate economic gains. Figure 3 shows the expected earnings of black and white men as they aged from 25 to 64 years if they experienced the employment rates and earnings of given years. This is also a period measure that summarizes per capita earnings—not income—for persons 25 to 64 years old.

In 1940 a black man could expect to earn, in his adult lifetime, 42 percent as much as a white and in 1970 55 percent as much; by 1986, there was a modest improvement to 60 percent. If black men in 1986 had the same rates of labor force participation and employment patterns as white men, they would have earned 69 percent as much, revealing the large effects of current racial differences in employment.

What about women? Changes for black women have been similar in some regards but

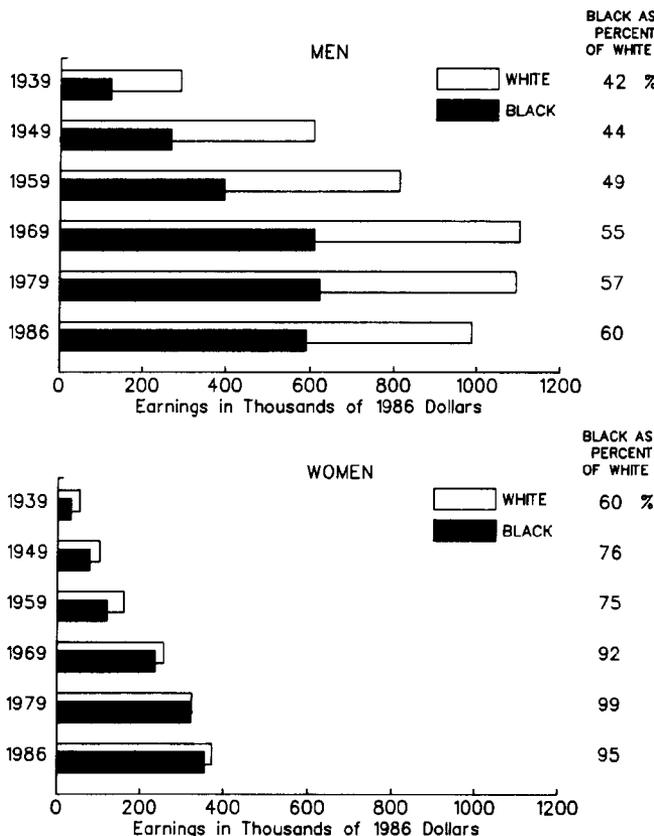


Figure 3. Expected Earnings Between the Ages of 25 and 64, According to the Rates of 1939 to 1986 (earnings amounts in 1986 dollars) (Age-specific per capita earnings were calculated from public use files of the 1940–1980 censuses and the March 1987 Current Population Survey. Persons reporting no earnings in the previous year were included. Amounts shown are the sum of earnings for those 25 to 64 years old.)

different in others. The driving force among black women has been the shift from domestic service and farm labor into, first, blue collar jobs, later, service sector positions, and then, the professions. When we look at the earnings of black women and compare them with those of white women, we find that black women essentially reached earnings parity with white women in the 1970s (Corcoran and Duncan, 1979; Reimers, 1985; Tienda, Smith, and Ortiz, 1987). This relative economic success of black women is not an artifact of comparing career-oriented full-time blacks with white women who work sporadically (Farley and Allen, 1987:339-342). When we model occupational achievement, we also find that black women with a high school education or more are at least as likely as white women to hold jobs in the top-ranked occupational categories.

With regard to labor force participation, the trends for black men and black women are dissimilar (see Fig. 2). Rather than there being a decline in employment and labor force participation, there has been a rise for black women, although in recent years, the increase in employment has been greater among white women. Since black and white women are now similar in rates of employment, and because their hourly pay rates are just about equal, it is not surprising that the expected lifetime earnings of black women approximate those of white women, as shown in Figure 3.

In this election year, those who comment about policy often assume that a few phrases describe the heterogeneous black population of 30 million. A little has been written about social class diversity among blacks in recent years (Landry, 1987; Wilson, 1978:Ch. 6), less about geographic diversity.

The status of blacks varies greatly across the nation. In some locations, income levels are high and poverty rates low. In the San Francisco Bay area, for example, the per capita income of blacks exceeds the national average for whites, whereas blacks in the nonmetropolitan South continue to report low incomes and much poverty.

Figure 4 shows per capita incomes in 1986 for the 12 metropolitan areas that had the largest black populations and for the remaining sections of the four regions. Washington, D.C., and the Bay area stand out for the high incomes received by their black populations. Even in these locations, however, blacks fall behind whites. The relative status of blacks, as indexed by the ratio of black to white per capita income, was best in the two West Coast metropolises and in the Northeast, a finding attributable to the high earnings and low unemployment of New England blacks. Recent declines in the oil industry, no doubt, help account for the meager incomes of blacks in Houston, New Orleans, and the Gulf Coast states—the areas with the largest gaps in income. The geographic differences are great. Along the West Coast, blacks had per capita incomes almost three-quarters those of whites; in Houston and New Orleans, just about one-third as much.

Explanations for Persistent Black Poverty

Having reviewed several dimensions of economic status, we can evaluate the four explanations for persistent black poverty. One can be dismissed rapidly, that is, the explanation of constant, or increasing white racism. To be certain, racism continues to exist, as we know from the ill-informed comments of sports officials and from incidents such as the one in Howard Beach. Honda is the most recent of many large firms to agree to multi-million dollar settlements following charges of employment discrimination. Nevertheless, I take the evidence of higher relative wages and better jobs for employed blacks, the declines in black-white residential segregation that took place in the 1970s in smaller- and middle-sized metropolises (Massey and Denton, 1987; Wilger, 1988), and the apparent willingness of an increasing fraction of whites to vote for Reverend Jackson as indicative of declining racism. As a nation, we faced the dilemma posed by Gunnar Myrdal four decades ago, and we are gradually becoming a less racist society.

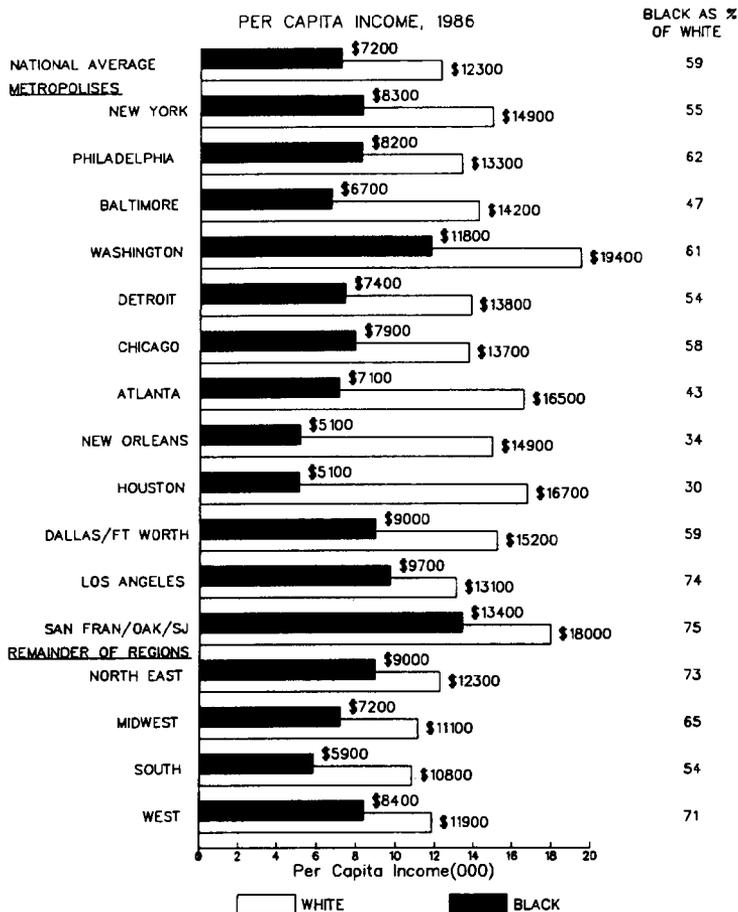


Figure 4. Per Capita Income in 1986 for Blacks and Whites in the Metropolises With the Largest Black Populations and for the Remainder of Regions (These estimates were made by summing the 1986 total income reported by households headed by blacks or whites in an area and then dividing by the number of occupants of such households, using public use files from the March 1987 Current Population Survey.

Caution is appropriate because this survey is not designed to provide estimates for specific metropolises. Assuming a simple random sample, the standard error of the per capita income estimate for blacks in New Orleans is \$350; for whites, \$620. Since this is not a simple random sample, actual standard errors are larger.)

It is not as easy to dismiss the hypothesis that transfer payments keep the poverty rate high, but we have had 15 years of studies of this proposition. Supposedly, welfare programs discourage marginal workers from employment, and thus some people fall below the poverty line who would not be there if they capitalized on their own skills or accepted jobs that they find demeaning. In addition, child support programs may either break up families or encourage irresponsible parenting, thereby increasing the number of women and children below the poverty line.

Negative income tax experiments and similar investigations of transfer programs on male employment suggest that their effects are often in the expected direction; that is, they reduce labor supply and more so for black men than white (Danziger, Haveman, and

Plotnick, 1981; Robins, 1980; Robins and West, 1980). Their effects are modest, with the exception of the Supplemental Security Income and Social Security programs (Parsons, 1980), which account for substantial decreases in the labor supply of older men. Overall, there is no conclusive evidence that the decrease in the labor force participation of black men is primarily due to the availability of transfer payments.

A greater number of studies examined the consequences of welfare for family structure. The National Academy of Sciences' (1987) study of teenage pregnancy concluded that there was no evidence that Aid to Families With Dependent Children (AFDC) or other transfer programs were responsible for premarital fertility, although the benefit programs may have influenced the living arrangements of unmarried mothers and, perhaps, decisions about abortions. (For other summaries, see Garfinkel and McLanahan, 1986:Chs. 4 and 5; Moore and Burt, 1982:Ch. 8).

The value of most welfare payments (in constant dollar amounts) has declined for a decade (Ellwood and Bane, 1984), but we have not seen a corresponding shift in family structure or employment, casting more doubt on the hypothesis that the War on Poverty inadvertently generated persistent poverty (Burtless, 1986; Rodgers and Harrell, 1986:Table 4.3).

The currently most popular explanation centers on the disappearance of manufacturing jobs, that is, the deindustrialization of the United States (Bluestone and Harrison, 1982). Is this the key? Kasarda's (1985) studies show that the number of high-paying, entry-level jobs with minimal educational requirements has declined sharply in some cities; and Wilson's (1987) investigation of Chicago's South Side implies that young black men now find it more difficult to get good blue collar jobs than did their older brothers or their fathers (see also Bensman and Lynch, 1987).

Caution is needed, however, before assuming that declines in manufacturing account for persistent poverty. The common notion that manufacturing employment has collapsed is erroneous, although manufacturing's share of total employment has fallen. The number of jobs in durable goods manufacturing in 1987 was only 3 percent below the number a decade earlier (U.S. Bureau of Labor Statistics, 1978:Table 1, 1988:Table 63). An accurate picture of employment change would stress not only the rapid increase in service and retail trade employment but also the continued rise in governmental jobs at the state and local level. For about a dozen years, state and local government employment, in which blacks are well represented, has exceeded employment in durable goods manufacturing, and total governmental employment is now about as great as employment in retail trade. The total number and proportion of adult blacks employed has increased in recent years and is now greater than ever before. In the late 1960s, 69 percent of the black population 25 to 54 years old was employed; in 1987, 72 percent (U.S. Bureau of Labor Statistics, 1970:Table A-1, 1988:Table 3). The rise in the employment of black women has offset a decline among black men.

Figure 5 shows the proportion below the poverty line in 1969, 1979, and 1986 for large metropolises and the remainder of the four regions. This helps to determine where the hypothesis about deindustrialization may apply and where it cannot fit.

We find great geographic variation in economic status and trends. Poverty rates for blacks in Washington and San Francisco in the mid-1980s were apparently below the national poverty rate for whites. Indeed, the proportion below the poverty line was greater for whites in New York, Los Angeles, and the Midwest than it was for blacks in Washington or San Francisco.

In the 1960s, the South and the Midwest were very different in their poverty rates—low for blacks in the Midwest, but high for those in the South. In the next two decades, poverty increased sharply among blacks in Detroit, Chicago, Cleveland, and the Midwest but fell among blacks in the South.

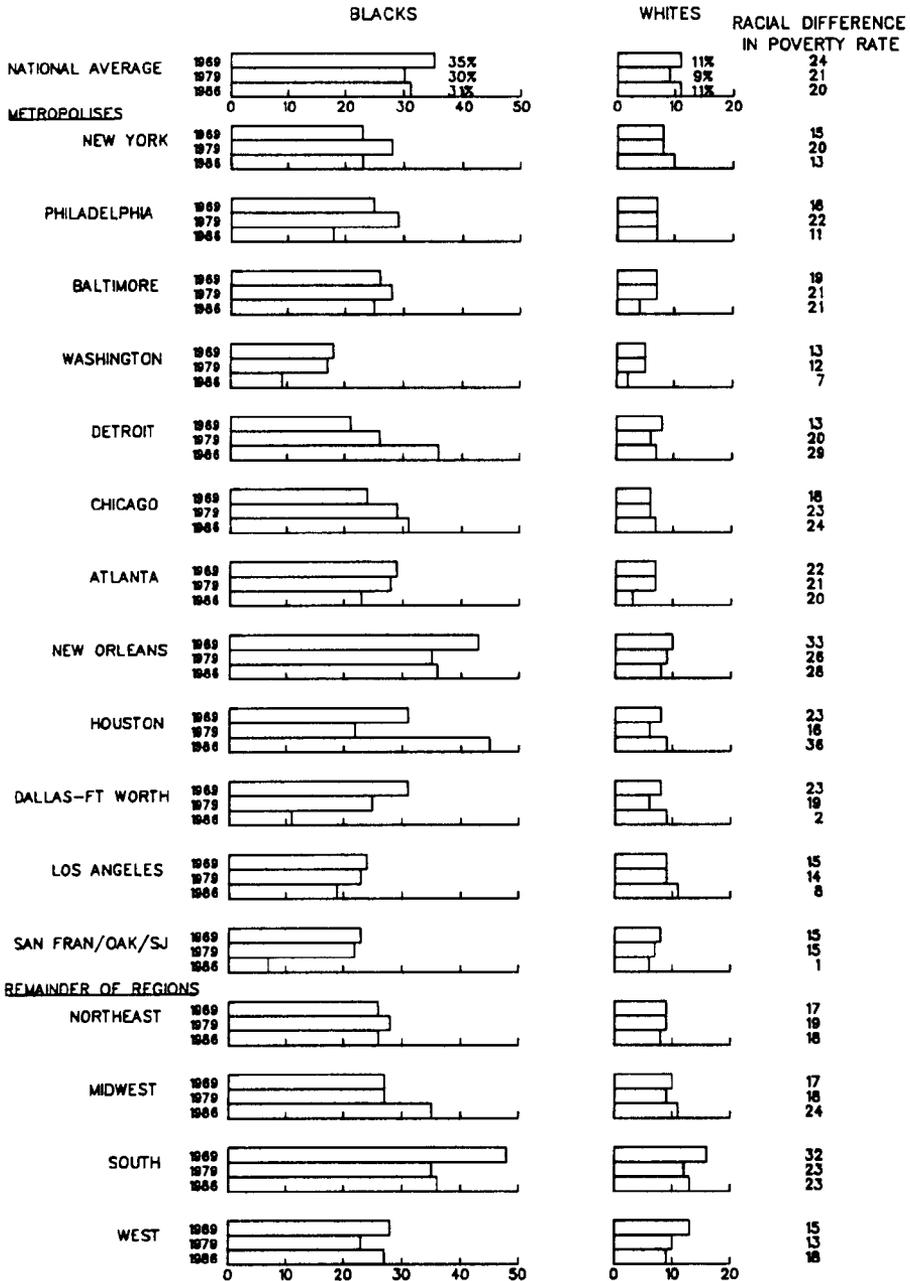


Figure 5. Percentage of the Black and White Populations Below the Poverty Line for the Metropolises With the Largest Black Populations and for the Remainder of Regions [Data refer to approximately constant consolidated metropolitan statistical areas or metropolitan statistical areas, using available data, where appropriate, for primary metropolitan statistical areas. Since the Current Population Survey is not designed to provide estimates for specific metropolises, estimated poverty rates in 1986 may have large standard errors. Assuming a simple random sample, the poverty rate for whites in Houston (9.2 percent) has a standard error of 0.7 percent, whereas that for blacks (44.7 percent) has a standard error of 2.7 percent. *Source:* U.S. Bureau of the Census (1972b:Table 207, 1983:Table 245); public use microdata file from March 1987 Current Population Survey.]

It is likely that the rise in black poverty in the Midwest is an outcome of the shift away from employment in manufacturing. However, the picture is a puzzling one. Detroit and Chicago stand out for their increases in black poverty, but their white poverty rates hardly increased, suggesting an economic polarization of the races in those locations. The change in manufacturing employment cannot, of course, account for the declines in black poverty recorded in much of the South, in Washington, or along the Pacific Coast.

Basically, the idea that blacks were, or still are, concentrated in slow-growing industrial sectors is hardly correct. If we look at the industrial distribution of employed blacks in 1970, we find that employment in their industries grew at a rate of 2.3 percent per annum between that year and 1987 (U.S. Bureau of the Census, 1972:Table 92; U.S. Bureau of Labor Statistics, 1971:Table B-1, 1988:Table 63). Nationally, the growth rate of employment was 2.1 percent per year. The continued growth of employment in the South and of governmental employment throughout the country benefits many blacks, although it cannot offset declines in manufacturing employment in the Midwest.

We turn finally to the hypothesis that changes in family structure play a role in persistent black poverty. They do. Again, caution is needed, because no single factor will explain a major social trend. Since the early 1970s the poverty rate for blacks has been approximately three times that for whites, but in two-parent families, the difference is about two to one. Furthermore, poverty rates have consistently declined among black husband-wife families (Bane, 1986:Fig. 9.3; U.S. Bureau of the Census, 1987a:Tables 1-3).

The shifting distribution of black households by type, that is, the rise of female-headed families, is partially responsible for the persistence of black poverty. Had there been no shift in family structure, the poverty rate for the black population would have fallen in the 1970s and 1980s, with the exception of the recession years. The poverty rate among blacks in 1986 was 31 percent. If the composition of black families in that year had been what it was in 1970, the poverty rate would have been about four points lower, that is, substantially higher than the white rate but lower than it actually was (U.S. Bureau of the Census, 1987b:Table 16).

Economic Change and Family Structure

To understand continuing poverty and to evaluate ameliorative policies, we must analyze trends in the family and fertility. When we examine such changes, we also find a mixed picture. On one important indicator, marital childbearing, there has been a racial convergence. Suppose that a black and a white woman married at age 20 in 1960, remained married to age 45, and bore children according to the fertility rates of that year. The black woman would have borne 5.6 children, or 1.5 children more than the comparable white woman. By 1985 this racial difference was only 0.1 child (U.S. National Center for Health Statistics, 1963:Tables 1-J, 1-W, 2-13, 1987:Tables 2, 3, 18). For a decade, the marital fertility rates of blacks and whites have been almost identical. Thus despite the high—but declining—rates of teenage childbearing, blacks are now a low-fertility population with a net reproduction rate just over one (Bianchi and Spain, 1986:50-59; U.S. National Center for Health Statistics, 1986:Table 2, 1987:Table 4).

On all other important indicators of family composition, racial differences are increasing. The delay in marriage has been greater for blacks than whites; divorce and separation rates remain higher and remarriage rates lower. By 1987 only 40 percent of the black women 20 to 54 years old lived with a husband (U.S. Bureau of the Census, 1988:Table 1). The percentage of adult white women living with a husband fell following the "baby boom," but for black women it was a much greater drop. This is shown in Figure 6.

There has been a substantial shift in the marital status of mothers as the percentage of births delivered to unmarried women has increased sharply, a change also described in

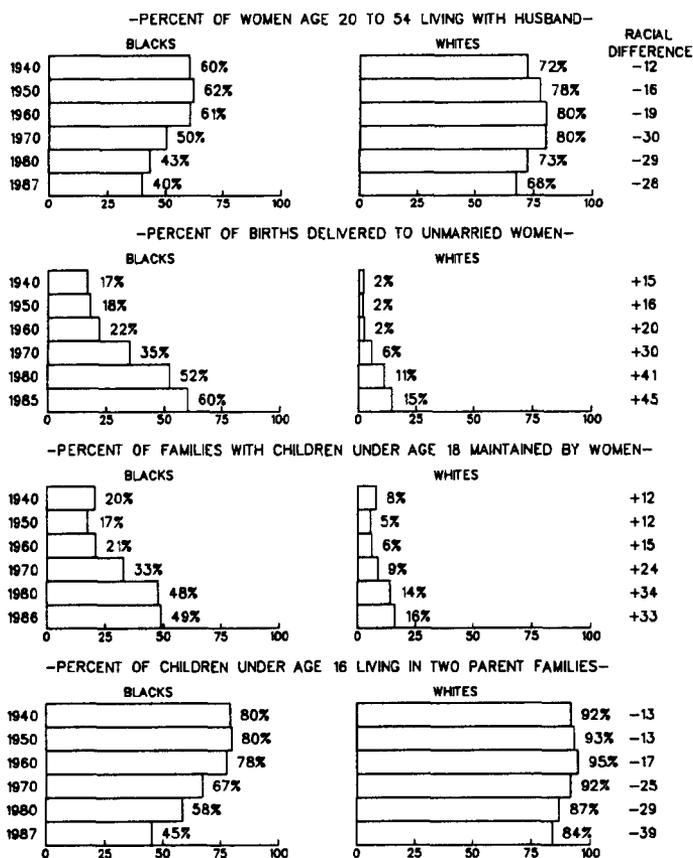


Figure 6. Indicators of Family Status for Blacks and Whites, 1940-mid-1980s [Data for women living with husbands have been standardized for age. The data about births for 1940-1960 and for family status of children for 1950 refer to whites and nonwhites. The procedures for tabulating the relationships within households that contain subfamilies have changed over time. The data for 1940-1980 are from the censuses; for other years, the marital and family status data are from the Current Population Surveys. Source: Public use microdata samples from the censuses of 1940-1980 and from the March 1987 Current Population Survey. Also U.S. Bureau of the Census (1943:Tables 1 and 3, 1955:Tables 4 and 5, 1963:Table 1, 1964:Tables 1 and 4, 1971a:Table 1, 1971b:Table 1, 1981:Table 1, 1987c:Table 1, 1987f:Table 1), U.S. National Center for Health Statistics (1963: Table 1-32, 1974:Table 1-33, 1984:Table 1-34, 1987:Table 18).]

Figure 6. By the mid-1980s about 6 black births in 10 and 1 white birth in 7 were nonmarital (U.S. National Center for Health Statistics, 1987:Table 18). Again, the racial difference on this indicator is now larger than when Moynihan wrote his controversial report. This change comes about not because unmarried women are bearing children more frequently but, rather, because of two demographic trends: marital fertility has fallen and a smaller share of adult women, both black and white, are married, meaning they are at risk of nonmarital fertility for longer spans (National Academy of Sciences, 1987:Table 2-16; Smith and Cutright, 1988; Sweet and Bumpass, 1988:96).

The outcome of these shifts is shown in the lower panels of Figure 6. A growing proportion of families with children are maintained by women rather than by married

couples. At present, one-half of the black and one-sixth of the white families with children are maintained by women (U.S. Bureau of the Census, 1987c:Table 1). By the middle of this decade, only 45 percent of black children under 16 years old lived in husband-wife families compared with 80 percent of white children (U.S. Bureau of the Census, 1988:Table 1). This has major implications for the black-white difference in poverty, since in 1986, the poverty rate for black children was 17 percent in two-parent families compared with 67 percent in mother-only families. Among whites, the poverty rate was 10 percent for those in two-parent families and 46 percent for those in mother-only families (U.S. Bureau of the Census, 1987a:Table 16). In addition, children raised in single-parent households will likely complete fewer years of schooling than those raised by both parents (Duncan and Duncan, 1969; Featherman and Hauser, 1978:329-345; Krein and Beller, 1988).

For three generations after Emancipation, through the abject poverty of the Depression, and during the era of northward migration, black families, even though they differed from white families, survived pretty much intact, with most adult black women living with husbands and most black children raised by two parents. The changes in black family structure have occurred since 1960, and on these indicators, black-white differences are much greater now than 20 or 40 years ago. Identical changes are occurring among whites, but on all measures, whites by the mid-1980s had not reached the levels found among blacks in 1940.

Why has there been this change in family structure? Any explanation must account for the similar trends among both races, but also for the more rapid change among blacks.

Have mores changed? This year we celebrate the 25th anniversary not only of *Demography* but also of Betty Friedan's *Feminine Mystique* (1963). She argued that women could and should find satisfaction outside the home, namely, in the activities traditionally reserved for men. It is certainly possible—even probable in view of trends in western Europe (Lesthaeghe, 1983; Muñoz-Perez, 1986; Sardon, 1986)—that we are seeing a pervasive redefinition of the way women expect to spend their adult lives. Evidence of this appears in the labor force activities of women described in Figure 2, their earnings in Figure 3, and their family status in Figure 6. The greatest social change occurring in the latter half of this century is that involving women—a change that may be the foreseeable outcome of the civil rights revolution that challenged our use of ascriptive characteristics and the contraceptive revolution that allowed women more control over childbearing.

If changing values about how women should spend their lives explain shifts in family structure, why are the changes larger among blacks? Should they not be much the same among both races? Surveys that measure the sex-role attitudes of blacks and whites are sparse, but they provide no convincing evidence of large racial differences (University of Chicago, 1986:questions 198-204, 252-259). There is no reason to believe, for example, that black men have more conservative views of women's roles than white men or that black women subscribe to more liberated values than white women.

A popular explanation for the shift away from the two-parent family among blacks stresses the apparent shortage of men for black women to marry (Guttentag and Secord, 1983: Ch. 8; Spainer and Glick, 1980; Wilson, 1987:72-89). This explanation is hollow. Women born just after World War II found themselves in a marriage squeeze, but this was a short-run phenomenon. Women reaching their 20s in the 1980s could select from a larger pool of prospective husbands than those who reached marriageable age a decade earlier. If we invoke an eligibility criterion, such as educational attainment, which indexes lifetime earnings, we also find that black and white women who entered the marriage market in the last decade were at no disadvantage compared with women who entered earlier (Farley and Bianchi, 1986:Table 3; Goldman, Westoff, and Hammerslough, 1984:Table 5). In addition, if we look at the marriage patterns of the black men who are, presumably, ideal partners—those with extensive educations—we find a shift away from marriage (Sweet and Bumpass,

1988:Table 2.4). Age at first marriage went up during the time when the marriage squeeze was ending.

A third hypothesis for changing family living arrangements focuses on the economic incentives women have to marry, remain married, or remarry. The wage rates of women increased in the 1960s and 1970s. It is important to note that the earnings of black women moved up more rapidly than those of black men, whereas among whites, wage rates changed at rather similar rates for both sexes (Bianchi and Spain, 1986:177–180; Farley and Allen, 1987:335–342; Tienda, Smith, and Ortiz, 1987; U.S. Bureau of the Census, 1987d:Table 1). Black women are approaching economic equity with black men much more rapidly than white women are approaching equity with white men. As Becker (1981:Ch. 3) reminds, such a change could greatly influence marriage.

Figure 7 presents information about the estimated earnings of women and men who worked full time and had similar educational attainments. In 1959 an adult black woman with 12 years of schooling could expect to earn 62 percent as much as a prospective husband who was also a high school graduate. According to the recent survey, the black woman would now earn 76 percent as much.

Trends are different for white women. In 1959 a white woman with a high school education would have earned 60 percent as much as a similarly educated white man; in 1986, only 62 percent as much. When other educational pairings of prospective bridegrooms and wives are considered, we find that the economic incentive to marry has declined more for black women than white, not primarily because the wages of black men are falling but, rather, because the earnings of black women are rising.

It is likely that our mores are changing such that early marriage, permanent marriage, and large families are less highly esteemed and our values no longer abjure nonmarital cohabitation, divorce, and out-of-marriage fertility—a process that influences both races and produces the trends shown in Figure 6. These changes have been given the cognomen of the second demographic transition (van de Kaa, 1987). The ability of a woman to live independently, even if she has children, has increased because of the greater earnings of women and the expansion of transfer benefits. The rise of female earnings, relative to male earnings, is unique to blacks and may partly explain the growing racial disparity in family structure.

Policy Issues: A Demographic Contribution

What is the basic problem regarding the status of blacks and women? As Figure 1 shows, despite economic growth and a War on Poverty that substantially reallocated federal spending from defense to social welfare, one-third of the black population remains impoverished (Burtless, 1986:Table 2.1; Meyer, 1983:Table 2). Largely because of changes in family patterns, many black women and an increasing proportion of white women face spans—often when they have custody of children—during which their incomes put them near or below the poverty line (Duncan and Hoffman, 1985; Levitan and Belous, 1981:Ch. 6; Sweet and Bumpass, 1988:281–291).

There have been lively debates about programs to encourage economic growth and reduce poverty. Demographers should use their acumen to develop and evaluate policy alternatives in three ways. First, there is a need for geographic disaggregation. As Figures 4 and 5 show, economic status varies greatly from one location to another. Many explanations for black poverty, such as the demise of manufacturing jobs or the presumed unwillingness of young blacks to accept minimum wage jobs, fail to take geographic variations into account. If all areas were as prosperous as Washington, we would have almost no poverty among whites and a low rate among blacks.

Demographers can contribute not only by describing these differences but by explaining

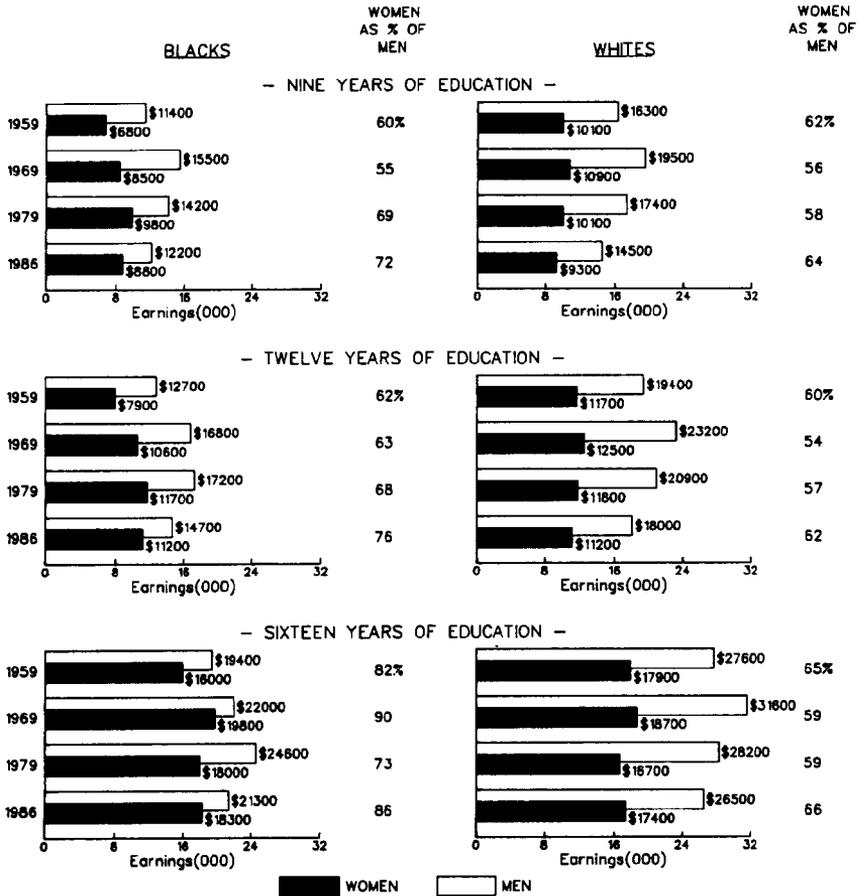


Figure 7. Estimated Annual Earnings for Black and White Men and Women 25 to 64 Years Old Who Worked Full Time, 1959, 1969, 1979, and 1986 (in 1986 dollars) (These estimates are from a model based on persons 25 to 64 years old who worked at some point during the year. The log of the hourly wage rate was a function of the years of elementary and secondary education, the years of postsecondary education, the years of potential labor market experience, the square of the variable, and a dichotomy indicating residence in the South. These estimates assume mean values for all of the variables except educational attainment. Data are from the public use microdata samples of the census of 1960 and the March Current Population Surveys of 1970, 1980, and 1987.)

them. Does the selective migration of blacks with great earnings potential explain the high rates of poverty found in Detroit and the rural South and the relative affluence of blacks on the West Coast? If so, why do not more blacks move from areas of unemployment to those of prosperity? Do we need a national policy to facilitate such migration, similar to those we had in World War II? Should policies be targeted toward improving conditions in areas with high poverty rates? Is it feasible to think that schools can be made effective and crime reduced in such locations? What about the persistence of residential segregation? Does it impede out-migration by constraining the choices of blacks who would leave neighborhoods of concentrated poverty? Would a Supreme Court order calling for metropolitan school

integration contribute to residential integration and the elimination of impoverished ghettos? Are there effective ways to create employment in areas of poverty, or do such programs merely shift jobs from high- to low-wage locations? Are there areas—perhaps in some central cities and the rural South—where the constellation of dilapidated housing, poor schools, no jobs, family disorganization, and crime is so firmly established that encouraging out-migration is the humane and effective policy?

Second, demographers can contribute to the debate, ably fueled by Menken (1985) and Preston (1984), about the future of the family. An assumption is often made that if we create good jobs for men—perhaps by restricting imports—or if we preach traditional values diligently enough, we will see a return to the family system that characterized this country between the end of World War II and the end of the baby boom.

I suggest that the changes in family structure that occurred among blacks are a leading indicator of what may happen among whites. Barriers to the educational and occupational advancement of black women declined recently, and they were strongly motivated to upgrade their position in the labor market because of the restricted economic gains of black men. Improvements in the earnings of black women, however, made marriage less necessary, made it less desirable to stay in an unpleasant marriage, and may have made it easier for husbands to dissolve marriages. If the earnings of white women rise, compared with those of white men, will we find that white families increasingly resemble current black families? Two decades from now, will the majority of white children be born to unmarried women and raised in families headed by their mothers? Will 30 or 40 percent of white children live below the poverty line?

Or is the present situation one that Ogburn (1927) would have called a cultural lag? That is, a man's self-definition is now closely tied to his occupational achievement and earnings. The economic success of women may be challenging to men and threatening to the usual division of labor in which the husband is breadwinner and the wife homemaker. Is the current shift away from marriage and family stability attributable not so much to a desire on the part of women for independence but, rather, to a reluctance on the part of men to marry or remain married to women who are their economic equals or superiors?

We know little about exactly why family changes are occurring. With the careful analysis of demographic data, we may be able to move beyond Lasch's (1979) assertion that we live in an age of narcissism or Ehrenreich's (1983) claim of men's revolt against their traditional obligations. Will we see the emergence of new values that encourage a different division of labor within the family? Will there be a decrease in the labor force activity of men, offsetting increases on the part of women?

Third, demographers can contribute to a debate about affirmative action. In recent years, the term has come to mean rigidly enforced quotas that provide unfair advantages to women and minorities. It has different roots, namely President Kennedy's attempts to end the exclusion of blacks from the skilled construction trades (Kennedy, 1961) and President Johnson's speech at Howard University, in which he argued that it was not fair to liberate a man who had been bound by chains, bring him to the starting line of a race, and believe that he could freely compete (Johnson, 1965).

Issues concerning what level, if any, of affirmative action is appropriate to improve the condition of blacks or what amount, if any, of tax revenue should be spent to reduce black poverty are largely questions about values. They cannot be answered through the analysis of demographic or scientific data because they depend on our beliefs about what is fair or equitable. Demographers can, however, document past trends as well as current differences and then proceed to evaluate the possible consequences of specific programs.

For example, our ethnic histories show that European and Asian groups experienced great hostility and discrimination when they came to the United States (Lieberson, 1963, 1980). Yet the ancestry data from the 1979 Current Population Survey and the 1980 census

show small differences in the present status of the descendants of those who arrived before we closed our borders in the 1920s (Neidert and Farley, 1985). It is impossible to argue that the discrimination directed toward the Irish, Italian, Polish, Jewish, or Asian immigrants severely limits opportunities for their descendants or has a strong influence on their current choice of neighborhoods. With regard to blacks, it has been only two decades since some of the most binding chains have been removed. Nationally, 45 percent of black children less than 15 years old lived in impoverished households in 1986 (U.S. Bureau of the Census, 1987e). In the nation's second largest black metropolis, Chicago, 30 percent of the black men 25 to 54 years old in 1987 were not employed; and in the nonmetropolitan South, the poverty rate for blacks exceeded 45 percent (U.S. Bureau of the Census, 1987e). Using the skills of our craft and the array of readily available data from the Census Bureau's annual surveys, we can assess the probable consequences of affirmative action programs that may be targeted on the basis of both race and geographic location.

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