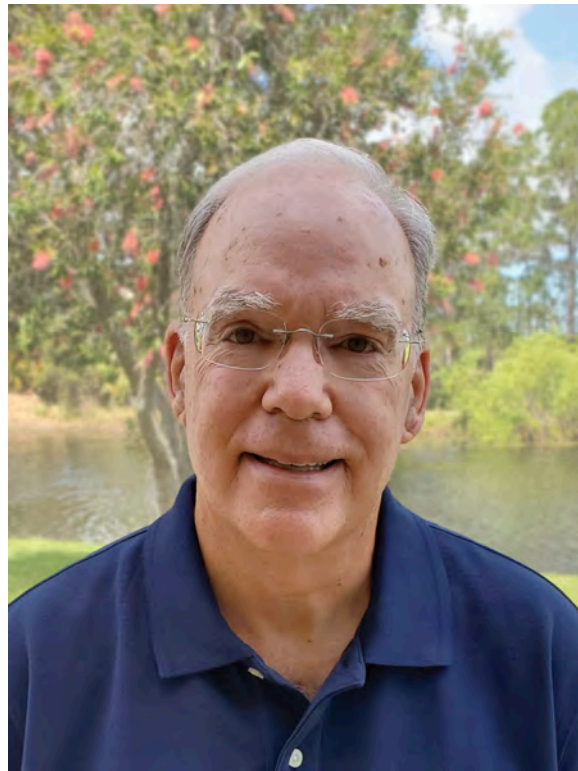


DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Samuel H. Preston PAA President in 1984



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

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

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

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

SAMUEL H. PRESTON

PAA President in 1984 (No. 47). Interview with Jean van der Tak at the Population Studies Center, University of Pennsylvania, Philadelphia, June 14, 1988.

CAREER HIGHLIGHTS: Sam Preston was born in Trenton, New Jersey in 1943, and grew up in Yardley, Pennsylvania (across the Delaware River from Trenton, New Jersey). He received his B.A. in economics from Amherst College in 1965 and Ph.D. in economics from Princeton in 1968. From 1968 to 1972, he was Assistant Professor in the Department of Demography at the University of California at Berkeley. He was at the University of Washington in Seattle from 1972 to 1977 as Associate and then full Professor of Sociology and Director of the Center for Studies in Demography and Ecology. For two years, 1977-79, he was a section chief in the Population Division of the United Nations in New York. Since 1979, he has been at the University of Pennsylvania, where he has been Professor of Sociology, Chair of the Sociology Department (1985-88), Director of the Population Studies Center (1982-89), and Chair of the Graduate Group in Demography (1980-82 and again since 1989). Among his many other professional activities, he has served on the influential Committee on Population and the Committee on Population and Demography and other committees of the National Academy of Sciences and was a Council member of the IUSSP for two terms, 1982-89. His honors include election to the National Academy of Sciences in 1987 and receipt of PAA's Irene Taeuber Award for Excellence in Demographic Research in 1983.

Sam Preston is particularly well known for his research and writing on mortality and demographic measurement techniques and has also worked on urbanization, the economic consequences of population growth, and the implications of aging populations for the well-being of children and the elderly. In addition to a long list of articles and book chapters, his publications include such monographs as Causes of Death: Life Tables for National Populations (with Nathan Keyfitz and Robert Schoen, 1972), Mortality Patterns in National Populations (1976), Patterns of Urban and Rural Population Growth (1980), Vital Rates in India, 1961-1981 (with P.N. Mari Bhat and Tim Dyson, 1984), Population Growth and Economic Development: Policy Questions (with Ronald Lee and Geoffrey Greene, 1986), and Fatal Years: Child Mortality in Late Nineteenth Century America (with Michael Haines, 1990).

VDT: We are speaking at Penn's Population Studies Center on a hot June Tuesday morning, though it's nice and cool in here.

Sam, let's begin at the beginning: How did you become interested in demography? You are a prototype of the small but increasing breed of economic demographers. Do you think the first one was perhaps Joe Spengler [PAA President in 1956-57]? Although Notestein [PAA President in 1947-48] technically had some economics as a graduate student.

PRESTON: I'd say Malthus was the first one.

VDT: Okay! That's certainly beginning at the beginning! How did you become interested in demography?

PRESTON: Well, I went to graduate school in economics at Princeton.

VDT: How did you get from Amherst to Princeton?

PRESTON: Princeton had a good reputation as an economics department and I won a Woodrow

Wilson fellowship coming out of Amherst and had applied to Harvard, Yale, and Princeton. Princeton was closest to home and seemed most attractive.

VDT: Where's home?

PRESTON: Home is Yardley, Pennsylvania--Bucks County--just across the Delaware River from Trenton [New Jersey] and then Princeton.

I remember looking through the Princeton catalogue which listed the economics department and looking at the courses they offered and there was only one course that I was sure I was not interested in taking and that was demography. The only one I could cross out; everything else looked terrific.

Toward the end of my first year at Princeton, I was deciding I was heading into labor economics and somebody told me, "Well, if you're interested in labor economics, a useful secondary course would be demography." So, I signed up to take Ansley Coale's [PAA President in 1967-68] two-semester course in demography, and literally after one lecture, I was sure that I wanted to be a demographer. That abrupt! If I'd gone to an undergraduate school that had sociology I would probably have majored in sociology, but Amherst did not have sociology. I think it might have had political science, although I'm not sure about that. The main social science there was economics. I'm not sorry that I majored in economics; I think it's a good rigorous undergraduate major. But as soon as I could get away from prices and interest rates to birth and death rates, I made the switch. It seemed much more concrete and interesting to me, involving sex and death and all those deeper emotions and processes.

My recruitment to demography was aided by the fact that the Office of Population Research was a congenial environment, which was separate really from the rest of the university and had its own library. I could go in and sit in that library and concentrate on demography readings, whereas all the other economics readings were in this horrible room in the basement of the Firestone Library, where you were competing with scores of other graduate students for access to the readings; it was just really quite unpleasant. So I think the environment probably had a little bit to do with my conversion as well.

VDT: So you were lured by OPR's library, in addition to everything else.

PRESTON: I was lured by Ansley Coale's clear expositional style, his obvious enthusiasm for the subject. And also I think it just struck a responsive chord with me as something that was more pertinent, more engaging probably, than economics, especially labor economics. As economics was taught then, at least at Princeton, it was kind of an old institutional style and didn't really have a lot of rigorous accompaniments; it was kind of throwing ideas together. But you get to demography and you have these very rigid accounting identities, expanding into the stable population model and you know these are truisms--these are not hypotheses. These are exact identities that define processes in human populations. I think the rigor of it attracted me as well as the fact that it was dealing with very interesting materials that economics seemed awfully dry in relation to.

So that's that; it was really a very quick conversion. And I did well in the courses because I was very interested in them. After passing my Ph.D. exams at the end of my second year [at Princeton], I went and talked to Ansley about promising dissertation topics and he said he'd been trying for several years to get somebody to do a dissertation on cigarette smoking and its impact on mortality patterns. That was a fascinating idea to me and I immediately went to work on it. I actually tried to go to work, as I recall, in the summer, probably a month after my exams, and Ansley said, "You really should take a longer time off after your exams; nobody should start a dissertation right after their exams." But I ignored this advice and started to work on a variety of things. It turned out to be an excellent dissertation topic.

VDT: And a book!

PRESTON: And a book, subsequently, by the University of California Press [Older Male Mortality and Cigarette Smoking, Population Monograph No. 7, Berkeley, 1970, and Greenwood Press, 1976].

VDT: You managed it in one calendar year, summer to summer?

PRESTON: Yes, summer to summer, basically. I left for Berkeley [in 1968] without having defended the dissertation; I had a bit more to do in one of the chapters. And I came back at Christmas and defended the dissertation and it was accepted.

VDT: Great. What were your data?

PRESTON: Well, I put the data set together. It was death rates for Western European countries and overseas European countries since the 1880s, when the data set began. I just really went to different libraries and put together these rates. It was an aggregate-level, every-five-years, data set.

VDT: Rates on mortality, but where did the cigarette smoking come in? Did you have something on emphysema, or what?

PRESTON: The Surgeon General's report had come out in 1964, I believe, three years earlier, so that was a good guide to some of the references. And there was suddenly a lot of interest in cigarette smoking as a health hazard. What I needed to do was put together the mortality data with data on cigarette consumption by country and trends therein, which involved some reconstructions--tobacco use converted into cigarettes per adult. Then it was a matter of going to the epidemiological literature and trying to figure out how much risk would be associated with a certain dosage of cigarettes and then converting the national consumption patterns into estimates of excess mortality. And then see at the end whether the excess that would be predicted from the epidemiologic studies was identifiable in the national mortality patterns. The way I tried to do that was to associate older male mortality with mortality of older females, who were not nearly as susceptible to cigarette smoking, and younger males, whose mortality would not have been very much affected by smoking.

So, the idea was to create correlations and then see whether a correlation that typically existed would be disturbed by added cigarette smoking. And the conclusion of the dissertation was that they were obviously being disturbed. Older male mortality wasn't improving anywhere nearly as rapidly as would have been expected based on what was happening to females or younger males.

VDT: You were one of the first to point that out, which became a hot issue?

PRESTON: At the aggregate level, right. I think it probably is one of the only studies that has been on national-level mortality as it would be impacted by cigarette smoking.

VDT: Does that explain your continuing interest in mortality?

PRESTON: Yes, I think it certainly had a lot to do with the fact that I worked extensively in mortality subsequently. You build up capital in a particular area, sources and more studies. And the World Health Organization asked me to do several things after the smoking book came out and that opened up some new areas.

Then I had gone to teach at Berkeley and Nathan Keyfitz [PAA President in 1970-71] was there

and he was interested in multiple-decrement life tables. He came to me and asked if I had any data that I could give him so he could illustrate the use of multiple-decrement life tables in a book he was doing. I said, "Sure, here's some data; there's a lot more where that came from." In fact, nobody had ever systematically investigated cause-of-death patterns in national populations. He and I decided sort of on the spot that that would be a worthwhile activity. So, I set about to acquire, assemble, process all the data I could find on age-sex cause-specific mortality, as far back in time as possible.

VDT: In Western European and North American countries?

PRESTON: Yes, and Japan and Latin America. Latin America started to produce some reasonably good data in the 1960s and 1970s. It's the kind of project that now you know how complicated it is to try to do, but at the time I was very naive and just thought, "I'll just go to some libraries and Xerox some pages and then try to get the data into uniform categories," which turned out, of course, to be a very big job.

The end product I saw, more than the tables that would come out, was some kind of overall assessment of the mortality decline in terms of causes of death, the contribution of various causes to the decline, the role of various causes in sex mortality differentials. So, this ultimately led to two volumes. One was a volume of tables that I did with Nathan Keyfitz and Bob Schoen, who wrote the computer program as a graduate student at Berkeley, and Verne Nelson, another graduate student at Berkeley, who did an awful lot of the detailed tabulation work. That came out as a 770-page book [Causes of Death: Life Tables for National Populations, 1972].

VDT: While you were still at Berkeley?

PRESTON: Yes. Then I took the data and started to do a series of analyses of the data and that came out ultimately, after I'd left Berkeley, as another book [Mortality Patterns in National Populations, New York: Academic Press, 1976].

VDT: So, there you were, not yet 30, and you had produced two books!

PRESTON: Yeah. In fact, I guess three, if you include the cigarette smoking book.

VDT: Of course, and the one with Nathan Keyfitz, and the Mortality Patterns in National Populations. That got on the map.

PRESTON: Yes. I think the mortality patterns book was a good thing for me to try to do at that stage of my career, because it involved me in having to read a very wide range of literature in trying to interpret these patterns that were emerging. For a while, at least, I felt as though I were on top of the mortality literature. But other things subsequently have come to the fore in research; mortality has kind of split recently.

VDT: Then what brought you to the University of Washington, Seattle? What was the job?

PRESTON: Berkeley was a great place.

VDT: Who else was there besides Nathan Keyfitz at that time?

PRESTON: Judith Blake [PAA President in 1981] was the chairman of the Graduate Group in Demography [Chair of the Group in Demography, 1965-67, and of the succeeding Department of

Demography, 1967-72] and at least by my final year there [1971-72], we were the only three members of the department. We were the only three members of the department in my first year there. I think in the second and third year . . . [Etienne van de Walle and Paul Demeny were there for periods as visiting professors.] [Editor's note: Kingsley Davis was heavily involved, but his appointment was in the Sociology Department, not the Department of Demography.]

VDT: You mean what is now Ronald Lee's [PAA President in 1987] group [Graduate Group in Demography] at Berkeley?

PRESTON: Right, but at that time it was actually a department [and subsequently became one again].

VDT: That was the famous undergraduate department too, wasn't it?

PRESTON: We did do undergraduate teaching also, right. I think it was the only department of demography in the country, to date. [Georgetown, at least, now has such a department, established in the mid-1980s.]

It was slated to go to seven faculty members, as I recall, but we never got that high and as a very small department, it was quite vulnerable to personality conflicts, ideological conflicts. Students--this was the Vietnam period of radicalization of the student body and nowhere more than at Berkeley--and the students had certain expectations about what the faculty ought to be doing and those expectations were not universally shared, although they were partially shared, by the faculty.

VDT: They wanted you to be out demonstrating too?

PRESTON: Well, that's exactly right. They thought the faculty should be participating in this important activity, and I agreed with them, and so did Nathan.

VDT: Hmm--and that third member didn't. Okay.

PRESTON: So, it was a very exciting time. We had some terrific students at Berkeley. I didn't know it at the time. You come out of graduate school and you expect everybody to be of the caliber that you knew in graduate school and Princeton had some awfully good students in economics. The students in demography at Berkeley were every bit as good.

VDT: Who were some? You mentioned two who worked on the book.

PRESTON: Bob Schoen. I think Geoff McNicoll is one of the best students I ever had.

VDT: He's the Australian?

PRESTON: Yes, just went back to Australia recently. Griffith Feeney, now at the East-West Center in Hawaii. Bob Gardner, at the East-West Center, was my first dissertation student.

VDT: I know Bob well. He was author of two of my Population Bulletins, on the Asian American population and immigration. Well, it must have been an exciting time. Did you leave with regret?

PRESTON: Yes, I did. But I think the stresses of living in the Bay area probably were pivotal in my deciding to leave. It was just a wild time, it really was, and I think the California life style didn't fit me all that well.

VDT: A product of Pennsylvania and Amherst and Princeton.

PRESTON: Yeah, well . . . Seattle seemed more stable. And I had my own center to run there.

VDT: Were you married by that time?

PRESTON: Yes. I actually married right out of undergraduate school, so I was married to start graduate school and had a child at age 23, while I was in my third year at graduate school. My wife was a nurse and was supporting me through the first two years of graduate school. We had another child in Berkeley and went to Seattle [in 1972] and had our third child.

VDT: Which is the one who is a musician?

PRESTON: Second child.

VDT: I think middle children often are. I told you about my musician son; he's the middle son.

PRESTON: Seattle was a lovely place to live. I grew very attached to the University of Washington as an institution--magnificent campus and there seemed to be a unique environment. The Center for Studies in Demography and Ecology occupies a suite of offices in the basement of an older building. I think my most vivid memory is that they never washed the windows in the basement offices, so you'd be staring up through these muddy windows at a gray sky, invariably spitting rain. The weather in Seattle got me; it got to me in a major way.

VDT: Except that you say it was a beautiful campus.

PRESTON: It was a beautiful campus and on nice days there's no prettier city in the country than Seattle. But there weren't many nice days.

VDT: The only time I've ever been there was during the [April] 1975 PAA meeting. You'd left by that time?

PRESTON: I was there.

VDT: And that was a beautiful week. We all said, "Where's the rain?"

PRESTON: It was spectacular. My wife was very attached to Seattle, and we were certainly going to stay there for good. We'd made that decision, although I don't think I was particularly happy to be there.

VDT: You were quickly made a full professor [1975], by the time you were 30, 32--something like that? That was pretty phenomenal, wasn't it?

PRESTON: Thirty-one, I think. Yeah, it was pretty unusual.

VDT: And simultaneously made director of the center?

PRESTON: Well, I came to Washington at age 28 as director of the center.

VDT: At 28! I think I've heard of that--the youngest director ever of any population center.

PRESTON: I was advantaged by having gotten out of Princeton very quickly.

VDT: I should mention that Ansley Coale holds you up as a prototype--three years from the B.A. to the Ph.D. He doesn't think anyone should ever take longer. And he holds you up as an example--you and Alvaro Lopez, who did it even faster.

PRESTON: Right. Well, Princeton actually had an institutional commitment to trying to get people through in three years.

VDT: You mean from the B.A.?

PRESTON: Yes, not just in economics. At that time. I don't think they are attempting to do that anymore.

VDT: Oh, no. Recently I talked to one graduate student there who was telling me under her breath, "I've been here five years." You're not supposed to say that out loud.

PRESTON: Exactly. But, also my career path was speeded by the Vietnam War, because if I dropped out of graduate school--which, like most graduate students, I seriously contemplated doing--I would have gone to Vietnam. And that prospect kept me with my nose in the books.

VDT: I see, good point. So, you were offered the directorship of the center at Washington. They came to you at Berkeley?

PRESTON: Yes, that's right. At that time, Nathan was pretty much decided to leave for Harvard and Harvard was going to make me an offer too, for an associate professorship without tenure. And Michigan had talked to me about a job and I was very interested in going to Michigan, but they were not ready to make me a tenured offer. And Washington was--in addition to making me director of the center, which was . . . It was a mixed blessing. I don't know why people want to be directors of centers, at this point. It really is a lot of fundraising activity that's required. Now, I'm overstating the case, but being director of the center was not one of the attractions of going to Washington, whereas being a tenured faculty member was, you know, attractive.

VDT: By tenured faculty member you mean you're on a tenure track?

PRESTON: No, I went with tenure.

VDT: Had you been at Berkeley?

PRESTON: No. I think I would have been. They had started tenure proceedings and it was well along and I had been hearing noises from the committee that I was going to get tenure, but I didn't wait around to find out what the verdict was going to be.

Actually, those were the days when tenure was not all that important, it didn't seem to me. I didn't realize. It's true that I went to Seattle with an offer of tenure, which was attractive, but it never occurred to me that that's what the goal was in an academic . . . for an assistant professor. Probably in part because there was no other assistant professor in the department at Berkeley and I just wasn't

socialized to thinking about an academic career. I was just thinking about doing interesting research and teaching students the best I could.

But I got to Washington and there was an obsession at that point in the department, as I recall--which I thought was very anomalous, but it turns out to be almost universal in large departments around the country--an obsession with tenure issues. By that time I had tenure and didn't need to worry about it, but it concerned me that everyone around me was as intrinsically excited about those issues as they were.

But I can understand why now, because what we expect of our assistant professors is almost superhuman--to get tenure here [University of Pennsylvania] --and we've had three people do it in population since I've been here.

VDT: Three in nine years. Is that a lot or a little?

PRESTON: That's actually a lot for a major research university, in one sub-field. All three of the assistant professors we've hired have gotten tenure.

VDT: For tenure they have to compete for a certain number of slots with people in other departments?

PRESTON: Well, if you do very, very well, there's a job there for you no matter what, so you've not competing with other departments. But we're expecting, oh, a couple of books and maybe 15 to 20 articles and other signs of distinction in an assistant professor's career that lasts about six years.

VDT: In other words, what you'd done, almost.

PRESTON: Well, I don't know whether I would have gotten it myself these days, because, as I recall, when I left Berkeley I had six articles, a paltry number by current standards, and then two books, but one of the books was a book of tables, which we wouldn't count as much as we would a book of analysis, and the other one was a dissertation, and that's also discounted . . .

VDT: Even though it became a book? They're not all picked up to be published.

PRESTON: That's right, but still it's assumed that if you're in a distinguished place your dissertation is good enough to have been published. So, I'm glad that I don't have to start out today, because I don't know if I would have made it. But that was a time when there was a lot more demand for academic talent relative to the supply than is the case today, so it was an easier time. And, as I said, I was not too concerned with issues of tenure at that point--naively unconcerned. But it was an attraction to go to Washington with tenure and then I was promoted, I think after two years there, to full professor.

VDT: And you said that the weather got to you after a while. You were teaching at that time. Had you started on your research in urbanization?

PRESTON: No, I hadn't started that at all; I was still mostly doing mortality and some fertility. Actually, the courses I taught at Washington were mainly in fertility, policy-oriented courses, which is what the market demanded. We had a course that was basically taught to all graduate students in sociology. These are very good graduate students at Washington. It was probably the best sociology program at the time on the West coast, certainly the most rigorous, quantitatively. And it would attract very good students who had strong quantitative interests. Tad [Hubert] Blalock was, and still is, there--leading social statistician. I think his name drew in lots of students.

VDT: We were all weaned on his statistics book [Social Statistics, 1960].

PRESTON: Exactly. And I think probably the best of those students in general gravitated toward demography. So, we had some wonderful students in Seattle.

VDT: Can you name some of them?

PRESTON: Yes. I think the best student there was Alberto Palloni [PAA President in 2006], who's now a professor at Wisconsin.

INTERRUPTION

VDT: We're still in Washington. A last question before we leave, out of the rain, and move on to the UN. Cal Schmid was there earlier, wasn't he?

PRESTON: He was the first director of that center.

VDT: He's one of the PAA presidents [1965-66] who is sort of an outlier. He was born in 1901; he's still alive. Henry Shryock [PAA President in 1955-56] went to see him a few years ago. He's retired on an island off the coast [later in a nursing home on the mainland]. Was he around when you were there?

PRESTON: He was a vague presence; his souvenirs in some ways were still around. He had this major graph-drawing operation. He had a full-time draftsman, maybe two. An old-style demographer.

VDT: I've heard he was very graphics-oriented.

PRESTON: Yes, so his graphs were around and we would periodically go through them and pull out interesting examples of graphics. I can remember Pete Guest, a colleague in Seattle, picked out one and put it on his door. The title of the graph was: "Indecent Exposure Arrests in Seattle, 1940." It had clusters of indecent exposure arrests on a map. So, they were doing an unusual variety of things in demography. But Cal was not active by the time I got there; he was coming in every once in a while.

VDT: He was known as a sort of isolated figure there in the far Northwest, who was an early demographer.

PRESTON: That's right, absolutely.

VDT: He had risen up there and stayed there?

PRESTON: Yes, I think he was a native of the Northwest. He had built the center up in a way that emphasized service to the state of Washington, so his graduate student support came from grants given to him by the state of Washington through a variety of applied demographic work, much of it graphics-oriented.

VDT: That sort of tradition has stayed on in Washington, as a state. It had one of the first state demographers, didn't it?

PRESTON: I think that's right. Yes, a very practical tradition. But between Cal Schmid and myself

was Stan Lieberman. Stan had moved the center in a much more academic direction and, of course, he's a brilliant figure and when I arrived I felt very much in his shadow for a while, partly because of his personality, which is the most expansive, humorous personality our field perhaps has to offer. But he had gone to, I suppose, Arizona at that time and left Seattle behind.

VDT: Now, what took you to the UN?

PRESTON: Well, you probably won't believe this story. The UN called me up for a job. I had apparently impressed Bourgeois-Pichat. I organized a seminar while I was at Seattle for CICRED on the effect of infant and child mortality on fertility and Bourgeois-Pichat was the head of CICRED. He came out to Washington, spent several days, and we then had the seminar in Thailand. I'm later told that Bourgeois-Pichat had recommended that the UN hire me, after John Grauman had died, to be head of the section on population trends and structure [within the Population Division, New York], and they called me up and asked me about this job. The last thing in the world at that point that I wanted to do was to leave Seattle for New York City. And I said--this was in the summer or late spring--I said I couldn't possibly leave Seattle; I had all these academic commitments for the next academic year. They said, "Well, what about a year hence?" I hemmed and hawed and said I could consider that, but I couldn't possibly consider doing it in the next year.

So, nothing happened for about eight months. Then a bureaucrat from the UN, Sankar Menon, called up and said, "Mr. Preston, we agreed to your proposal." I'll never forget his language. The proposal he was claiming I had made was that the job be made available to me "a year hence." And I was so intimidated by the UN as an organization and I felt I must have made some commitment to them that I didn't remember, so I gulped and said, "Well, I guess then I'm coming."

I went home and broke the news to my wife, who was in love with Seattle, and she was devastated. But this was only to be a leave of absence. I took a two-year leave of absence from the University of Washington and I decided the UN job was for two years and had every intention of going back. That's how it was palatable for me to leave and palatable for my wife. But we were East only about three months when we realized that this was where we wanted to be.

VDT: In New York?

PRESTON: New York was not where we wanted to be, but the East was back with both of our families and back on more familiar terrain. So, I resigned my job in Seattle and started looking for another job in the East.

VDT: Even though you were in the UN job?

PRESTON: Yes. I decided that the UN was corrupt and I couldn't cast my lot with them for the rest of my life. By the time I left, I was regretful.

VDT: You began to like the work better?

PRESTON: I liked the work from the start--a very interesting set of research reports that had to be written in my section. This work on urbanization . . .

VDT: Began there?

PRESTON: Began and ended there. I did a book while I was there called Patterns of Urban and Rural Population Growth [1980]. That was what I spent most of my time on while I was there. Then I

was responsible for a couple of other projects as well in mortality--the new model life tables.

VDT: Oh, you did those?

PRESTON: I didn't do them, but I supervised the doing of them and hired people, John Hobcraft, initially, and then Larry Heligman, who finished it off.

But it seemed to me to be an organization where there was a very weak association between achievements and rewards, whereas in an academic setting that is a very finely tuned association. There were people in the UN who had been there a long time and who were not productive at all and who were getting very well rewarded. There seemed to be a big random element in everything that was going on; sections were fighting to preserve their own turf or standards. And the conversations at lunch and so on seemed to be largely focused on who was going to get promoted next. It's typical, I suppose, of bureaucracy, but I had just had no exposure to it. And it just didn't seem to me that it had a place that was devoted to science, and at that point that was the chief value in my hierarchy, I suppose. So, in that sense it disappointed me, although I think I had my two most productive years while I was at the UN, without question. I had a good research staff working with me. You're sitting on top of a lot of things that other people don't have and you're in New York, the hub of communication.

I really enjoyed and profited from my years at the UN. By the time I left, I realized why it had to be like it was, why any bureaucracy had to take on some configurations that make it less than totally efficient. But in any event, living in New York was a problem. I had to commute about an hour each way--we lived in Tarrytown--old rickety train ride. It just came down to a choice of where I was going to go.

I had an opportunity to work with the Population Council, and at Princeton and at Penn. I was juggling them. I started to think maybe I should work out a half-time appointment at the UN and a half-time appointment at Princeton, but the UN nixed that. At that time, they didn't want any part-time staff members; I think they have changed their mind about that. Going to the Population Council required me to stay in New York, which I didn't want to do. So, it came down to a choice between Princeton and Penn, and really the pivotal part of that choice was the teaching that I would be doing in the two places. Princeton really only taught two courses in demography and Ansley Coale taught them both--wonderfully well. There was relatively little for me to teach there. They talked about getting a course in medical economics taught at the Woodrow Wilson School, which would have been an interesting possibility. But here at Penn, they'd had retirements--John Durand [PAA President in 1961-62] and Vince Whitney and Ed Hutchinson had just left--and there was just a wide open agenda.

VDT: And you loved teaching?

PRESTON: Well, I think if you're going to be on a faculty, you have to be very concerned with teaching and with what you're teaching. I won't go so far as to say I like it. I teach a course a semester, basically, and do spend quite a bit of time on that course. And here I can teach demographic methods, which I taught at Berkeley, and a course in mortality--mortality is still my major area of interest--and basically anything else I want to teach. So, it really provided an opportunity for me that no other place did, and I decided to come to Penn [in 1979].

VDT: Did you come in as the director of the Population Studies Center?

PRESTON: No, I came in as chairman of the Graduate Group in Demography.

VDT: Then you and Etienne van de Walle [PAA President in 1992] switched, or what happened?

PRESTON: Well, there are two administrative positions here. One is the person in charge of curriculum and the graduate students and the other is the person in charge of the resources of the Population Studies Center. I think it's a very healthy system; it divides the administration up in ways that don't make it too burdensome for any single individual. So, I took over the Graduate Group and then in 1982 took over the Center.

VDT: Is the Center above the Graduate Group?

PRESTON: No, it really sits next to it.

VDT: The Graduate Group is more concerned with the administration of teaching?

PRESTON: Right, that is its exclusive domain.

VDT: And the Population Center with research?

PRESTON: Right--and hiring people, including some people who service the Graduate Group in Demography. What we needed here at that time . . . We had a center grant [from NICHD/NIH], but it was the smallest of all the [population] center grants in the country, by far--I think by about two to three.

VDT: Why was that? In the Caldwells' book on the Ford Foundation contribution to population [Limiting Population Growth and the Ford Foundation Contribution, 1986] they said that this center made an application for a \$1.2 million grant back in 1960. It didn't get nearly that much--it was a lot less [five-year grant of \$200,000], because it wasn't committed to the Third World, wasn't that it?

PRESTON: I don't remember--I wasn't here at the time--but it really has made a difference. When Ford got out of the population field, in the mid-1970s, they didn't get entirely out but they said, "We're going to stop this huge commitment that we've been making, but we've got these universities feeding at the trough and we have to help them dig themselves out before we withdraw all support." So, they gave tie-off grants of a million dollars apiece to Michigan, Princeton, and Brown. For some reason which I never understood, Penn didn't get a tie-off grant and we're still suffering as a result of that. We don't have the income from that discretionary money that these other places have; it makes a difference. But that was made up in part by a grant from the Rockefeller Foundation that Etienne van de Walle had pursued in African demography.

VDT: That was important. Penn had taken a while to have an attachment to one Third World area; that's what the Caldwells pointed out. Michigan had its ties with Taiwan. Penn had its students from developing countries that came here to study in a traditional program, but did not have the ties with any one area overseas. Then Etienne built that up during the 1970s.

PRESTON: Etienne built that up and it's been quite a success, I think. But it still provided mostly stipends and tuition and we didn't have the resources for the staff. So, my first job as director was to try and get the center grant expanded. We put in a supplementary request to NIH and they awarded us a much larger amount in our center grant. This was in 1983. They subsequently expanded the amount in the next round of our five-year application. So, we're now in quite good shape; we're average by center in terms of NIH's support. We can provide resources and facilities for the faculty and students that I think are quite respectable now.

VDT: And you are very LDC-oriented now. The Caldwells said in their book that at the time Vince Whitney applied in 1960 for Ford money to build up the center, Penn had "the oldest and best developed teaching program in population studies in the world" [p. 54]. You had Ed Hutchinson and Dorothy Thomas [PAA President in 1958-59]. However, it concentrated on migration, urbanization, and labor force and it was very U.S.-oriented at that time.

PRESTON: Yes, I think that's right.

VDT: How did it pick up its LDC interests, which you didn't then have and was perhaps one of the reasons Ford didn't give you so much money in the early 1960s? Now, the Caldwells said, you have more less developed country students than any other center.

PRESTON: I think that's right and I think it was largely the students, but also Etienne. And my coming. By that time, I had done a lot of work in Third World demography.

VDT: That came about through your time at the UN?

PRESTON: The time at the UN and it actually started in Seattle. I should mention a significant influence here that I think has been a significant influence on a number of people which was the National Academy of Sciences Committee on Population, under Ansley Coale.

VDT: You were already involved in that in the late 1970s?

PRESTON: I was involved in that from the beginning, which I think was the year I went to the UN [1977]. What it did was to provide an opportunity for very rapid technical advance in indirect estimation. Indirect estimation is an area of demography that sort of was dominated by William Brass and Ansley Coale. Coale and Demeny [Paul Demeny--PAA President in 1986] did the UN manual [Methods of Estimating Basic Demographic Measures from Incomplete Data, 1967] and Brass developed a number of techniques. It was developing, but very slowly until this committee was formed. The committee brought together ten people, including Etienne and myself, who were very interested in developing country demography, especially in the formal aspect thereof--estimates of birth and death rates. This provided a forum for new techniques; UN Manual 10 was produced by that committee. And it did something like 22 monographs on different countries and I was put in charge of the Indian monograph and in India recruited to Penn a student who turned out to be the principal author of that monograph, Mari Bhat, who was a terrifically talented Indian technical demographer.

PRESTON: Being on that committee, does that mean that you did a lot of research on your own that went into committee reports?

PRESTON: Yes. It turns out now that that's not the way National Academy of Sciences committees are supposed to work. You're supposed to be synthesizing other people's work, but you don't undertake original work. However, this committee was much more ambitious than most, maybe more than any other Academy committee, in the number of projects it undertook.

VDT: It came out with the famous report, Population Growth and Economic Development: Policy Questions [by Samuel H. Preston, Ronald Lee, and Geoffrey Greene, 1986].

PRESTON: That was a later document from a different committee. After the technical demography committee stopped functioning, a new Committee on Population and Demography was formed. Gene

Hammel was the chair of that committee. It had two major activities, one of which was the family planning review [Robert J. Lapham and George Simmons, eds., Organizing for Effective Family Planning Programs, 1987] and the other was the review of the economic consequences of population growth.

VDT: I understand that you didn't take time within your other job to do this particular work. Were you expected to do it after hours? Did you get paid for it?

PRESTON: Good question. The India volume was done in the summers when I was being paid to do something else. The Academy really didn't have any money to support that research, but it did expect a volume and we finally came up with a volume. The report on economic development was a different matter. I was asked to chair that committee and I thought they should have a full-fledged economist chairman. I thought it would give the report more credibility. Furthermore, I was so far removed from economics that it would not make sense for me to be in charge of it.

PRESTON: You no longer consider yourself an economist?

PRESTON: Absolutely not. I'm a demographer and if I had to be hyphenated I would be a sociologist-demographer. I went to the department of sociology at the University of Washington, and my only colleagues at Berkeley were Keyfitz, Kingsley Davis, and Judith Blake, who were sociologists. So, I really never was in an environment with economists.

Ron Lee and Gale Johnson agreed to chair the committee and I agreed to write a substantial amount of the first draft of the report. They put me on the staff of the Academy for a semester to do that, although I never physically located in Washington. I agreed to give up teaching for a semester. I sat here in this office and assembled everything I could find that seemed pertinent and read through it and wrote a draft. There had been a committee meeting where some initial papers were presented, but basically the first draft of that Academy report was written without benefit of the commissioned papers. As you know, they came out two years after the report and they simply were not available at the time the report was written, most of them.

VDT: They were called your research, background papers.

PRESTON: I know, I know. One of them was far enough along to make a difference. That was by David Lam [PAA President in 2011], one of the sections on income distribution. The things that we relied upon the most were IUSSP's volumes. Ron Lee had been in charge of a committee on economic consequences of population growth for IUSSP and he had done a terrific job in that role and had sponsored several conferences, papers from which were vitally important for that Academy report.

But, also I attempted to read pretty widely, especially in agricultural economics which it seemed to me was an area that had been neglected by demographer-economists. It had done careful empirical studies of land-use systems as they were influenced by population: household allocation of labor and capital as it's influenced by household size. So, there was an important literature that pertained to rural areas in developing countries. But, of course, the majority of residents of developing countries are rural residents, so I think the report was effective in bringing that literature to light in the context of economic and demographic relationships.

VDT: I think people must feel that was so. I think of Julian Simon growing out of Ester Boserup, an agricultural economist, and of course that was what everybody was uptight about . . .

PRESTON: That's right. The response by agricultural economists to the report was quite good. The

response by demographers was very mixed.

VDT: It was, indeed.

PRESTON: They were expecting something controversial.

VDT: That report is sort of an unread report, although it's extremely readable. I think people can read into it whatever they want to find.

PRESTON: That's absolutely right. Everybody has an opinion on the report. Most of them haven't read much of it. I can remember Lincoln Day getting up--we had a session on this report at the PAA meeting--getting up and denouncing the quality of scholarship in this report and I asked if he'd read it and he said, no, he hadn't had an opportunity to see it but what he'd heard about it indicated that it was very flawed scholarship. Oh my! Then it was denounced by the president of PAA in the next session, Paul Demeny's presidential address ["Population and the Invisible Hand," 1986], which I was very upset about.

VDT: Then there was the press conference in Washington when you introduced it; that wasn't an easy time. But I think there was someone who criticized more the press release, the headline . . . Oh, that was Julian Simon.

PRESTON: Julian Simon thought it dramatized too much the role of population growth. As I see the conclusion of that volume, we argued that population growth was in general--that is to say high fertility more specifically--a negative factor in economic development, but it was not an overriding factor in economic development. One would have to pay a lot more attention to alternative strategies for advancing the rate of economic growth in order to say that population growth limitation programs were the most cost-effective way of stimulating economic development. But we all believed that family planning programs were an important component of a general development strategy, if for no other reason than that it enabled people to better achieve their own personal goals, but also because there were likely to be externalities for other families. Those externalities seemed to me the key to the whole issue of economic consequences and we didn't get very far in pinning them down, nor has anyone else subsequently done so. Ron Lee is now in the process of trying to do that, so I'm hopeful that he'll produce some more definitive results.

VDT: You actually started that report, or the committee started work, in 1983, before the 1984 Mexico City population conference. So, there you were already with what became known as the revisionist view before the shock of it at that conference.

PRESTON: I suppose so. That's interesting. I remember seeing the first draft--I don't know who sent it to us--of the administration's document for Mexico City and reacting in horror and, in fact, writing a letter and going down with a delegation from PAA, three or four people, to meet with people in the White House to complain about this document. I could in no way endorse some of the craziness that was in that document, particularly about abortion and family planning programs. We met with George Bush's chief of staff. We made him look at this press release with this preliminary statement and he looked at it and said, "This is nonsense."

I think in many ways it was an embarrassment that some of the things in the NAS report sounded similar to things that were being said by the administration. Reagan is probably the most unpopular president among academics in the 20th century and if you sound like Ronald Reagan, you're not doing yourself any favor. But we had to go where our noses led us.

VDT: You mean some things in the NAS report, you felt, sounded a bit like the U.S. statement and policy at Mexico City?

PRESTON: Right.

VDT: But it was merely coincidental?

PRESTON: Well, it certainly wasn't because the administration had declared it as American policy.

VDT: Was it a reaction, rising to the bait thrown out by Julian Simon? The Ultimate Resource came out in 1981.

PRESTON: Which I wrote a review on which was quite negative.

VDT: That's right. I read it recently, in Population and Development Review [March 1982].

PRESTON: I think that Julian did stimulate interest in this whole set of issues and did professional service by calling attention to the absence of empirical backing for many of the relationships that demographers had taken for granted. Or maybe--I don't want to charge demographers with ignorance--the layman had taken for granted.

VDT: It had been conventional wisdom for a long time.

PRESTON: I don't think demographer-economists have really changed their tune all that much about these relationships. As David Horlacher pointed out, the report itself sounds very much like the UN Determinants and Consequences [of Population Trends] section on economic consequences of population growth.

VDT: The one of 1973?

PRESTON: The earlier one [of 1953], but also the 1973 one, I think. Simon Kuznets was kind of an intellectual leader in the UN for a number of years and opposition was Kuznetsian in some respects. But the report has not been criticized by mainstream economists; it's been taken as a fairly obvious set of conclusions by economists. It's been the people sort of on the periphery of the population movement, who have seen population growth as a tremendous threat to human advance, that have reacted most violently to what appears to be a more guarded assessment of the role of population growth.

VDT: How do you feel about population growth now? The Population Reference Bureau's 1988 World Population Data Sheet shows that the trend is tending toward the UN high variant; the growth rate has been stuck at 1.7 percent for some years. I know we can't use Data Sheets as trend data, but it's been stuck. Of course, we all know that China's birth rate went up and any blip in China will stick us there. But world growth seems to be faster than we expected.

PRESTON: It worries me for Africa in particular. Most of Latin America is beyond the point where the pressure of population on resources is the key concern in economic processes. Africa is not beyond that point. Parts of Africa are very poor in natural resources. Natural resources in very poor countries play a bigger role in production and the ratio of population to resources is important. I see no evidence

of declining birth rates in Africa. I think they will decline in the next 30 years, but they'd be better off, most countries, if they declined in the next five years.

VDT: Let's jump to your other controversial contribution to the public arena, which was your PAA presidential address of 1984, "Children and the Elderly: Divergent Paths for America's Dependents" [delivered May 2, 1984, published in Demography, November 1984], which appeared in Scientific American a very few months later ["Children and the Elderly in the U.S.," Scientific American, December 1984]. By the way, did Scientific American come to you or did you go to them with that article, which put it into the public arena?

PRESTON: Dick Lincoln [editor of Family Planning Perspectives] was very insistent after my talk that I do something of a more popular version of it in a wider circulation.

VDT: I remember the hushed silence when you gave that speech; I think we knew that we were sitting in on something historic. You were pointing out that children were being shortchanged with federal money vis-a-vis those 65 and over. Of course, we had begun to become aware of how much the elderly were costing this country and would continue to cost, especially when the baby boomers got there in 2015, because with an aging population, low fertility, it was inevitable. However, I don't think before that time people had really thought of the implications of what happened to children. You came by that idea all by yourself?

PRESTON: I did, actually. I was casting around for an idea for a PAA talk and was probably going to do something on mortality in developing countries and thought, well, that's fine and that's what I know best, but most of the audience is probably interested in American issues and if I were able to find an American issue that seemed interesting I would go in that direction. Then a number of things that I was reading--the kind of stuff that comes across your desk heading for the library--sort of formed a pattern and I saw child poverty in particular having deteriorated substantially in the last ten years, without apparently any publicity. I had not seen anybody comment on it.

VDT: You noticed that? Now, of course, it's in the news.

PRESTON: Yes, in the news every week. And at the same time, looking at mortality rates among the elderly, which had gone down rather sharply.

VDT: That was about the time it was realized that the plateau where mortality had been stuck had changed and the decline had resumed?

PRESTON: Right. So, I decided that I'd look at a number of indicators of well-being and see if there was an age pattern of change. And in virtually everything I looked at, the situation for kids had been getting worse and the situation for older people had been getting better. I thought that was an interesting phenomenon and I decided to investigate it and see if I could figure out some factors that were involved in the changes.

I think I made some headway in that. People came away from that address with a message, just as I think you said, that my main claim was that children were getting shortchanged by the federal government and the elderly were arrogating to themselves the larger share. But, in fact, only a couple of pages of that 22- or 23-page report was about political factors or government budget.

VDT: That's what stuck in people's minds.

PRESTON: That's what stuck in people's minds, but the address was mostly about family change for children as an influence on their well-being.

VDT: It derives partly from your own personal situation? You have children--under 18 at that time?

PRESTON: Yes, absolutely. No, I don't think it does arise as much . . . I think the pressure. I did point to the tendency of a greater burden on individuals from the older end of the scale and a lesser burden from the younger end of the scale and work through some of the life-cycle implications of that. That may well have come out of personal circumstances.

VDT: I was talking to Jane Menken yesterday and in her PAA presidential address the following year ["Age and Fertility: How Late Can You Wait?"], she talked about the woman in the middle, the squeeze, with the young and elderly dependents simultaneously.

PRESTON: Exactly.

VDT: In your case, you felt you were pointing out the demographic implications of the changing family and the policy implications?

PRESTON: I also had a long section on industrial change; talking about how the education industry seems to have been failing and I trace that--I don't know how effective or accurate this argument is--to declining youth population, which has led to a reduced demand for schoolteachers, which in turn has led to a horrible reduction in the average quality of schoolteachers. And all of those links, I think, can be documented.

VDT: Well, that must have stuck in the public mind too. I just re-read your address the other day and didn't pick that up, but that's common currency also in the press now. That must have been picked up from your paper.

PRESTON: Well, yes. Albert Shanker, the head of one of the teachers' unions, only about a month later in his column in the New York Times wrote entirely about that section of my talk.

VDT: He picked that up from your PAA speech?

PRESTON: I don't know where he got it; I think it was from the PAA talk. Anyway, Dick Lincoln argued that I should send this to a wider-circulation journal. I sent it to the New Yorker at his suggestion and the woman from the New Yorker said she liked it a lot but I would have to personalize it; I would have to de-statiticize it and try to put in some personal examples. And I didn't feel that I would be effective at doing that. So, I looked for another outlet and sent it to Scientific American, who accepted it quickly. So, it did come out.

VDT: Have you written a lot for the popular press?

PRESTON: No, I haven't. I have been asked to do an awful lot for the popular press following up on that and I've done some of it. I did an editorial--I guess this is not the popular press so much--for the Journal of Pediatrics, a long editorial. I've given congressional testimony two or three times. Senator Moynihan has asked me to testify several times. He actually cited that paper very widely; I've been very flattered by all the attention he's given me. Sent me probably a half dozen personal notes and he's been a supporter.

VDT: He has been a population supporter all along. And there's his book on Nation and the Family.

PRESTON: His book on Nation and the Family starts out, paragraph one, with my paper.

VDT: Wow! That is an accolade.

PRESTON: I will say one more thing about that. All the positive press it's gotten from advocates of children has been matched by negative press from advocates of the elderly.

VDT: Well, I should hope so. Nelly Gray and her Gray Panthers.

PRESTON: Yes, and AARP in particular has taken out after me several times and their journal, Modern Maturity, second largest circulation magazine in the country, called me "America's leading crusader against the elderly" a couple of years ago. Immediately within two days, I had calls both from my parents and my wife's parents.

VDT: Oh, oh! You have all four parents alive?

PRESTON: Yep. "What's going on?" Actually, it turned out that they had hired a new editorial writer who'd picked up a file and hadn't understood the dimensions of what this file represented. And did an editorial--it was his first editorial for Modern Maturity--and got some significant features of it wrong. I wrote to them and they retracted their editorial, basically; published my letter and said they regretted the errors.

VDT: You said your parents had read it in Modern Maturity?

PRESTON: But they heard it from their friends; this is a very widely read magazine. Subsequently, they attacked me again, but this time it was on solid ground. They had a professor of political science say my political demography was full of holes. But at least it wasn't just wild charges. And just a few months ago I defended the article at the Gerontological Society meeting; sort of called on the carpet, put before a jury of my peers.

VDT: But that's fun, isn't it? Or is it? What do you think about that, when you get hauled into the policy arena?

PRESTON: I'm thin-skinned. I do not like to be criticized in anything; I guess most people don't. I would rather they like it than that they didn't like it, and I don't get any pleasure from the combat and I've basically stopped combatting.

VDT: You mean you won't attempt to write anything controversial?

PRESTON: No. I'm going to do whatever I think is correct, but I'm not going to defend anymore. I have done a couple of follow-up things: one on Japan, looking at the same relationships in Japan [with S. Kono, "The Changing Status of Children and the Elderly in Japan"]. That's coming out in a book by the Urban Institute. The Urban Institute held a whole series of seminars around this issue of children and the elderly. They've got a very good book coming out [The Vulnerable, 1988]. Tim Smeeding, John Palmer, and Barbara Torrey are editing it. That'll be out soon. They timed it because they thought it would influence the elections.

VDT: I've heard Barbara speak on it regarding several countries. Why did you pick Japan?

PRESTON: I had done a paper on non-European industrial countries for the Hoover Institution forum on fertility change--Kingsley Davis's [later published: "The Decline of Fertility in Non-European Industrialized Countries," in Kingsley Davis, Mikhail S. Bernstam, and Rita Ricardo-Campbell, eds., Below-Replacement Fertility in Industrial Societies, 1987]. Japan was one of the countries and I got interested in Japan and then Barbara Torrey read that paper and thought I would be an appropriate author for her volume on children and the elderly. So, I acquired Kono as my co-author, whom I knew through the UN, Director-general of the Institute for Population Problems in Japan. It was fun.

VDT: Have you ever lived in a developing country or any other country?

PRESTON: No, I haven't. I have visited something like 40 countries, about 25 of them developing, but I don't believe I've ever spent more than two weeks in any one of them. That will change, but for 20 years I've had children in school and I have another five and it's awkward to move them around for a lengthy period of time. But I can see that changing in the near future.

VDT: You mean apropos of what Jane Menken is now doing, research in developing countries?

PRESTON: Yes.

VDT: I told Jane the story of meeting Marvellous Mhloyi, the Penn graduate, at the University of Zimbabwe two years ago--I was there with a World Bank mission--and she said, "I feel so alone." She had far too many demands on her.

PRESTON: She sure does.

VDT: She had done what sounded like an excellent study of couples in two rural areas which showed that the contraceptive prevalence was probably much lower than the Contraceptive Prevalence Survey of 1984 had found, which has been touted as showing that Zimbabwe was doing best of sub-Saharan countries. Subsequently, she and Ethel Churchill and I had some correspondence about her getting that written up and into Studies in Family Planning. But--no time.

PRESTON: She's become an international superstar.

VDT: That's right. It was very obvious then.

Until you got into policy, I had thought of you as a technical demographer. In your excellent article in Sociological Forum ["The Social Sciences and the Population Problem," Sociological Forum, Special Issue: Demography, Fall 1987], summing up the links between population and economic development, you listed the contributions of technical demographers, things like studying deficient data, momentum, natural fertility--Bongaarts and Louis Henry--biostatistical models, and the timing of births, which makes such a difference in population growth too: The Chinese can have two children if they just time them right. Is there anything more for technical demographers to say? That's one thing I wanted to ask you. Then you felt that beyond that, we now have to draw more on the softer social sciences, even anthropology, to explain some of our links.

PRESTON: That's a good question. Any time you try to take a snapshot of the field and you look at all the interesting things that have been accomplished in the past five years, you begin to think, I

wonder really if there's anything left? Fields go through periods in which they are extremely active. I would say that technical demography has been extremely active in the past ten years. The development of indirect estimation techniques for developing countries, the generalization of stable population equations to Africa or to any population, has had a number of spinoffs. Biostatistical models of conception and birth have been extremely important in understanding the intervening variables of fertility. What is going to happen next? I can't predict that. I can see instances in developed countries where demographic modeling is going to be very important. I think mortality is an area where we really need some modeling, something I'm working on now.

VDT: You're working on something now?

PRESTON: Yes, with Gretchen Condran. We have a project from the National Institute of Aging to study old-age mortality patterns in developed countries. At this point we're in the tedious data-gathering and evaluation stage. It's clear that we're going to need some kind of modeling activities to represent the age pattern of mortality at the oldest ages and to be able to make comparative statements from one country to another. The problem in the United States is that our data are so lousy that we really need a variety of demographer's skills to try and make sense out of it. Some of the techniques that have been used in developing countries to discipline data, to make better sense out of what they're saying, I think at this point we need to be using in the United States, because our demographic data are the poorest of any industrialized country. Twenty percent of young black males are missing from the census; where are they? People are overstating their ages at older ages or misstating their ages very badly. That means we don't have a good picture of old-age mortality. Marriage and divorce statistics don't cover the whole country yet. Illegal immigration means that the demographic accounting identities that are so dependable in many other places can't really be used here very well, because you've got this slippage between the left-hand side and the right-hand side. So, yes, I think there are lots of interesting questions that formal demography can address for the United States.

VDT: So, there's still work there. But you also seemed to be implying that some work like, say, the Caldwells' participant observation in Indian villages and other softer approaches in the larger picture . . .

PRESTON: Right, are very important. For example, they're important in understanding where black males are between ages 20 and 30. Not to say that I'm going to be doing it. I probably have no skills whatever to bring to bear to that kind of activity, but I admire those who can, and especially Caldwell. He's one of my heroes in the field, without any question.

VDT: Do you think there's still room in demography for basic research such as you do? Of course, more and more demographers are getting jobs in the applied field, in business and state and local government.

PRESTON: I hope so. I think, though, our future lies in our ability to bring demography to bear on directly important human issues and questions. And very often that means that we've got to be more than demographers; we've got to be social scientists in a broader sense. I think that the future of our field really depends on how well we not only tend to the core of our own discipline, formal demographic methodology and so forth, but also how conversant we remain with the other major social sciences, economics and sociology in particular.

VDT: Will there be jobs in the academic field?

PRESTON: There will be jobs in economics and sociology departments. There will not be jobs--there are not jobs now--in demography departments, because we don't have any such. So, in order to train students for those jobs, they've got to be acceptable to a department of sociology and that means that they've got to do something probably beyond the core of demography. It's okay if demographic methods are a component of what's done. But there are not going to be many places--there are not many places now--for whom that's sufficient. I think demography has to be connected up with other disciplines and it has to address itself to major social issues, both in the U.S. and developing countries. And I think if we can do that it will certainly remain a vital part of the scene.

It's not guaranteed that that will happen. There's a temptation for programs to get more and more involuted. It's been a temptation everywhere I've been to teach the students more and more demography, to the point where there's not much time that they have left to learn anything else. And I think that temptation has to be resisted.

VDT: What about those who get demographic skills and use them in business and state and local government.

PRESTON: Fine, sure. That's certainly happening, and it's all to the good. I think demography has a lot to offer everywhere, because it has a sort of concreteness that some of the other social sciences lack. It has these accounting identities that are a solid basis from which to spread out and examine the world. And I would hope that it would thrive both in the business community and in the academic community. I don't see any reason why it won't, but we in the universities have to be sure to keep students looking outward instead of inward.

VDT: Who have been some of the leading influences in your career?

PRESTON: There are two key influences, unquestionably. Ansley Coale, who drew me into the field. I was infected by his enthusiasm and his wonderful expositional style.

VDT: That was both in his teaching and . . .

PRESTON: And his personal life. He's a magnetic figure. And then I had the great advantage of going to Berkeley after graduate school where Nathan Keyfitz was. Nathan was a very different kind of character, but totally committed to science and the application of demography to interesting issues and the development of formal demography. And both of them have always been very supportive of me. I've profited a lot from their having dealt with me as a promising young demographer. I couldn't have found two better people to have affiliated with by the time I was 26. So those were unquestionably the biggest influences on my career.

[Preston later added another influence.] Kingsley Davis was a formative influence, as Ansley and Nathan certainly were. He was at Berkeley at the same time.

VDT: You didn't mention that.

PRESTON: It was in a more peripheral role. He ran his own research institute [International Population and Urban Research], which was not part of the department of demography. On the other hand, he was there as a presence and he's a brilliant, insightful man. I really came to appreciate what effect he had on me subsequently, just in his recognition of human societies and how they construct individuals' daily lives. That was something that an economist just never thought about. It took a while, the process of osmosis, in really reading some of his writings after I had left Berkeley, before I came to appreciate the importance of that. So, he certainly deserves to be added to the pair that I listed

as most influential.

VDT: He's had influence on just about everybody I've interviewed so far, whether or not they actually worked with him. How about some leading, outstanding students. You've mentioned a few in passing.

PRESTON: I think at Washington, as I said, Alberto Palloni was a leading student and has had a wonderful career already himself in demography. Here at Penn, I think we trained some excellent technical demographers. They're not well known in this country, because they've gone back to the countries they came from. Mari Bhat I've mentioned.

VDT: Worked with you on the India book. You say you picked him up in India?

PRESTON: Yes, I met him at a National Academy of Sciences seminar in India. He's gone back to Kerala, where he's working for a UN institute in economic development.

Michel Garenne, he's an outstanding French demographer who is working in Senegal now with ORSTOM, managing the ongoing--equivalent to the Cholera Research Lab [of Bangladesh]--activities of field stations of Senegal. It's been in operation for 20 years and it's produced a great deal of informative data about what's happening in mortality and fertility. Those are the two, probably, that stand out the most here at Penn.

I think to the extent that I've been successful in training, it's clearly been in technical demographers rather than substantive demographers. I don't know why, but that seems to be the record.

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

PRESTON: Professional accomplishments.

VDT: You also said you're very pleased that you got Jane Menken here from Princeton. A loss to Princeton. Do you feel guilty about that?

PRESTON: Yeah, I do. I think the loss to them is as great as the gain to us, but I hope the gain to Jane exceeds the loss to Jane so she regards it as a gain.

I think probably the piece of work I'm proudest of is one that doesn't have my name on it and that is the Patterns of Urban and Rural Population Growth of the UN [published without attribution as Population Studies No. 68, United Nations, New York, 1980]. I think it was a valuable summary and analysis of urbanization patterns, about which there was a lot of confusion about the sources of growth and what countries are experiencing the impact of rural to urban migration and so forth.

VDT: And the comparison with the growth of urban population in now developed countries in the late 19th century. That was an important part.

PRESTON: I think that was probably the most satisfying volume I've produced. I had a lot of time to spend on it and a good staff.

The PAA address certainly was gratifying and the response to it. I never anticipated as enthusiastic or as vigorous a response. So that was very gratifying. And I think the points in there were basically correct and were points that probably had some small effect on the national discussion.

VDT: Certainly did.

PRESTON: The National Academy of Sciences report I considered to be a contribution. I thought also that it brought some light on a subject about which there's been mostly heat. But I think the failure of that volume is that we didn't have a clear enough conclusion that people could walk away with a message. As you say, people were able to read into that volume most anything they wanted. And that partly reflects the fact that we agonized over the wording of the conclusion and it was a committee document; the committee spent more time looking at the conclusions than it did any other part of it.

VDT: Being the one most likely to be quoted.

PRESTON: Of course. But we assiduously avoided quotable phrases and paragraphs. And that strategy means people can impose their own views on the document, because they're not going to spend days and days of their lives reading the text.

VDT: I assume that study had something to do with your being elected a member of the National Academy of Sciences [in 1987]. Is that similar to being a member of the French Academy in France?

PRESTON: Yes.

VDT: The top scientific minds in the country, I gather. And is there a top limit in numbers?

PRESTON: Yes.

VDT: Someone has to die off before someone new is elected?

PRESTON: There is a limit to the number elected each year, that is, in the aggregate set by how many vacancies there are created by death. But I think there's some possibility of expansion, small, but expansion from year to year.

But to comment on your comment. That study didn't have anything to do with my being elected to the National Academy of Sciences. I specifically asked that question and service to the Academy in theory has no bearing on election to the Academy, which is by other members of the Academy. Two social and political scientists are elected each year. Not including economics, which has its own category, and there are several economists elected each year. The year I was elected, the other person that was nominated by the section was Samuel Huntington, a political scientist at Harvard, against whom a vendetta was launched by a mathematician at Yale, Serge Lang, and he didn't get elected. So, all the publicity that year went to this guy that didn't get elected.

VDT: Not in demographic circles! Were you the first demographer that's made it?

PRESTON: Oh, no. There are Ron Freedman, Ansley Coale, Bill Brass as a foreign associate, Nathan Keyfitz and also, I believe, Kingsley Davis [yes, plus Dudley Duncan]. So, demographers are actually quite well represented. I guess I'm the first of the younger generation, but I won't be the last. [Jane Menken was elected later in 1988.]

VDT: But that does make you very special. Let's jump into PAA, where you are among those who have been president at a respectably young age--you're among the younger ones--and who have decades and decades to go thereafter--if Kingsley Davis is an example, as he is in many ways. Did you see the photo of him with his baby at the PAA New Orleans meeting [of 1988] in the latest issue of Population Today?

PRESTON: Yes.

VDT: That photo was by my former PRB colleague, Art Haupt, editor of PT. The caption said: "It was noted that the U.S. birth rate had gone up."

PRESTON: It was good to see him there.

VDT: Can you remember your first PAA meeting?

PRESTON: Sure, I attended in 1968, in Boston, and Ansley Coale gave the presidential address.

VDT: That was the first meeting for Jane Menken too. And you remember his address?

PRESTON: I do. It was a very well-done address ["Should the United States Start a Campaign for Fewer Births?"]. In fact, I think he departed from his normal research at that time to talk about an American issue and I think that was kind of an influence on my own choice when I came around to having to give a presidential address.

VDT: That was the issue of did the U.S. need a campaign to bring births down. Of course, in proper demographic style, he discovered the birth rate was coming down anyway.

PRESTON: But his answer was no, we don't, and not just because the birth rate was coming down.

VDT: Then we had the Commission on Population Growth and the American Future which was set up in 1970 to address that high birth rate, which of course had collapsed by the time its report came out.

PRESTON: The year its report came out [1972] the birth rate fell below the replacement level.

VDT: Besides Ansley, can you remember some of the other leading lights at the time of your first meeting?

PRESTON: No, I can't. I don't remember much of anything about that meeting except that address. We went to Jimmie's Harborside for dinner, terrific restaurant. That's all I remember.

VDT: What issues do you think have been important in PAA over the years--outstanding issues?

PRESTON: Well, I think there's been a systematic attention to policy-related issues on the programs at the annual meeting. I think there have been a lot of very good sessions that have addressed the major concerns in developing countries in particular. The Association has become more developing country oriented, or at least it had become more developing country oriented by 1980. A majority of the sessions, I think, and a majority of the distinguished demographers were spending most of their time studying developing country issues, which is very unusual for a professional association. I think probably only the anthropology association would be equivalent to that. It's one of the things that I like about the field, that we are not as navel-gazing a discipline as many others.

VDT: Do you go to other professional association meetings?

PRESTON: Yes, I do. I go to sociology meetings, public health meetings, and then occasionally

gerontological meetings; the statistical meeting I went to last summer. And PAA are the best meetings.

VDT: Why is that?

PRESTON: The sessions are the best attended. The average quality of the papers is highest, I think. And there's this great communal feeling. Demographers know one another; at least they know a significant number of the people who are at the meetings. And you often only see them once a year, so it's a reunion time and the dance card is filled up from breakfast to late-night drinks, year after year. It's a very exciting time.

It comes at a bad time of the academic calendar; that's my only problem with the meetings. Right at the end of the semester and exams are getting under way and so forth. I would rather it were in, say, early June, right about now. But I think it's such a tradition that I'd leave it where it is. It certainly works **very** well as a professional association.

VDT: You're young enough to be able to say that objectively. The oldtimers always hark back to the Princeton Inn days where there were 50 to 100 people at the meetings and everybody attended the same session--there was only one. And that's a big regret, that it's gotten too big. But you still feel there's a feeling of intimacy?

PRESTON: Oh, absolutely, compared to the other larger professional associations. Demographers are in part selected on personality grounds, I think. They tend to be level-headed and yet not nearly as intense in general as economists are, so that there is a little bit more good feeling at these meetings than is typical of many other associations.

VDT: That is a marvelous statement. It's not untypical of what a lot of people have said. Of course, it could be a measure of what it takes to become a PAA president--among all those personalities. What does it take--to rise to the top pretty rapidly, as you did?

PRESTON: I was very, very surprised when Peter Morrison called me up and said that I had been nominated to be president, because I hadn't been first vice-president and I thought that was the normal route. I had run twice for second vice-president and lost the first time to Jane. Then a year hence, I think, we were both nominated to run for first vice-president and I decided that, well, there's no point in my running against Jane again, so I declined the nomination. And then we were both nominated to run for president against each other and she declined. That was in 1983. If she had run she would have won.

VDT: Yes, I think so. All the women who were on the slate were winning in those days.

PRESTON: Yes, but I think she was also a better-known figure. And she'd already beaten me once. So, if she had decided to run, I would have been delayed in my presidency.

VDT: Was that an OPR-mafia decision or something of the sort?

PRESTON: No. I'd already declined to run against her once, so I guess in some ways she thought that it was her turn to decline.

VDT: Ansley Coale was very proud that he was asked to introduce you--three in a row. There was yourself in 1984, Jane in 1985, Paul Demeny in 1986.

PRESTON: Absolutely.

VDT: I asked if he had his speeches in introducing you, because a few such speeches for past presidents have been kept and they're usually very enlightening, usually someone who has known you that well, whom you ask to introduce you. He tells me he speaks from notes and didn't have anything for the record on you.

PRESTON: He had my vitae.

PRESTON: Can you remember what he said about you?

PRESTON: I remember he told a terrible joke.

VDT: He's famous for that, isn't he?

PRESTON: I had suggested that he start off with one of his jokes. So, he told the joke and there was a mild smattering of laughter. Then I said over the podium, "Well, my only regret is that I asked Ansley to tell a joke."

VDT: And then you launched into your famous speech. Do you see any ways that PAA could be changed for the better? You say it still has a fine attendance at sessions in particular.

PRESTON: This past year we expanded the number of simultaneous sessions.

VDT: Eight.

PRESTON: Yeah, it was too many. I think we should go back, because the attendance was down this year, in the sessions--not overall.

VDT: Scattered too thin.

PRESTON: Yes.

VDT: You say fewer overlapping sessions; what are you going to do about all those papers that people want to present?

PRESTON: Well, we could go to participation which is more restricted. I have had two participations on the program every year for the last 15 and that's not necessary. And I think we do need to worry about opening up slots for younger people, people who are not quite as well known. We could restrict participation to one paper, period.

VDT: Whether you're a single or a co-author?

PRESTON: Right, I think that would make a difference. Attendance at New Orleans this year was quite good.

VDT: Over 1,100 [1,115], nearly a record. That's right up there with the Washington meeting in 1981 [1,167] and the New York meeting in 1974 [1,110], which was the first time they went over 1,100.

PRESTON: That could have something to do with there being more sessions, in fact. There is a causal relationship; people have more opportunity to present. So, who knows? Maybe we're in a new era. But I would say that we ought to give that experiment a little longer--look at it very hard.

VDT: I've heard that what's often most missed now by those who have been there a long time is more informal debate in the sessions, as in the past when Kingsley Davis invariably took off after Frank Lorimer and so on. Which is, of course, impossible now. It's been suggested that the panel discussions, which are an attempt at debate, are degenerating into paper sessions too.

PRESTON: I think that's right. And I think it partly reflects the fact that our field has become more normal science--more people who know how to do a paper that is to be presented at the PAA. It is not as speculative as it used to be and it is not drawing people into it who have a more speculative bent of mind.

VDT: Good point. Hadn't heard that.

PRESTON: I think sociology, for example, is still drawing those people. But demography has become a bit more routinized. That may be reflected in the styles at the professional meetings and the style of presentation. If you look at our younger people--I don't want to disparage our younger people, by any means, and I would approve myself of that group--we don't have the kind of broader background that Kingsley Davis or Frank Lorimer had. You're being recruited to the field in part because of our technical skills. And I think that what's happening in large part at the annual meeting. I would like to see an opportunity for people with somewhat broader concerns to be recruited into the field. I don't know how to stimulate that, because our curricula have gotten more and more technical and more and more narrow. It's a concern that we need to address in the future. Maybe the people who are going to be the kind of intellectual leader you're talking about are going to be drawn from other fields and they're going to have gravitated into population studies--much as I think Caldwell has done, for example. I do see him as a current intellectual leader of the field. He's a bit younger than the generation of superstars that we're talking about.

VDT: He is a bit younger?

PRESTON: Yes, he's 61 or so and still very vigorous and he's still out there in the field and I would say over the past decade he's perhaps made his greatest contributions to population studies. I'm a great admirer of him.

VDT: Is he now just on the Council of IUSSP or he's now president elect? No, Livi Bacci will be the next president.

PRESTON: He is not on the Council because he elected to run for president and then he lost to Livi Bacci [at the 1985 IUSSP general meeting].

VDT: Of course, I remember that. Yet he had tremendous backing.

PRESTON: He won the American vote.

VDT: Indeed, and I thought the developing countries too.

PRESTON: Yes, I think that's right. But the meeting was held in Florence and there was a huge European vote and that went to Livi Bacci [as an Italian; versus Caldwell, the Australian].

VDT: That brings us to IUSSP. You are very prominent there, the only American on the Council at the moment, and you have been on several of their committees. I see you have a paper slated for the IUSSP African conference of 1989. What about IUSSP?

PRESTON: It's a very vigorous association also, and I think the most vigorous of the international associations in the social sciences, without any question. With a huge budget. I compared the annual budget of IUSSP to the American Sociological Association and IUSSP is bigger. Isn't that surprising?

VDT: Where's it coming from?

PRESTON: It's coming from donors, European donors, UNFPA, the U.S. to a small extent, but it's coming from governments mostly.

VDT: Quite a bit of that money, though, goes to bring LDC people, move them around to meetings?

PRESTON: Yes, some fraction of it; I would say 10 percent of it, perhaps. A lot of it is for committee meetings.

VDT: Do you find your membership in IUSSP worthwhile?

PRESTON: Yes, I do. I think it's been a very important association. As I mentioned, the seminars that were organized by Ron Lee I think were pivotal in the National Academy of Sciences report. I think the set of volumes from IUSSP over the past 15 years has been a very distinguished set of contributions to the professional literature.

VDT: You include in that the papers that are presented at the meetings as well as their separate volumes?

PRESTON: Yes. I think they stack up against the last ten years of, let's say, Population Studies or Demography. They've made as big a contribution. The problem has been circulation.

VDT: Definitely, people don't know about them.

PRESTON: We tried to solve that problem by going with another press, Oxford University Press.

VDT: Supposed to sell more. The committee reports, for instance, every once in a while I read them in the Bulletin of the IUSSP. They're fascinating material, but where else do they appear?

PRESTON: Right. It's a problem that I don't think we've resolved because Oxford Press is not doing a good job of getting the committee proceedings out; they've been very slow. The other issue there is that IUSSP doesn't have its own journal. Population Studies is sent to all members of IUSSP and we have approached Pop Studies to see whether they would be interested in our being cosponsors of the journal and they have not reacted enthusiastically to that.

VDT: If Grebenik's still around.

PRESTON: Well, he was more enthusiastic than the rest of the Population Investigation Committee. So, I'm in charge of a committee which is at this point enacted to look for alternatives, to see whether IUSSP really should have its own journal.

VDT: Another one in the field?

PRESTON: Yes. It could happen now, because we're trying to put a little pressure on the Population Investigation Committee to get better terms for Population Studies, which is costing us an arm and a leg, whereas--as I have pointed out several times--Demography costs members of PAA nothing. That is to say that production and mailing expenses are almost exactly offset by the subscriptions from libraries. So, it costs the average member of PAA nothing to have that journal; in other words, it's not being subsidized at all by the Population Association of America. So, where there are issues about improving it, I have brought that item up to the Board of Directors several times, that they should not be very niggardly about expenses for Demography, because at this point it's pretty cheap.

VDT: I didn't know that. But not with Population Studies?

PRESTON: Pop Studies is charging \$35 a year I think at this point.

VDT: Which is a large part of your the IUSSP membership fee, which is very high now, and a large part of it is subsidizing Pop Studies.

PRESTON: Yes.

VDT: However, the IUSSP you feel is important in keeping contact with demographers around the world?

PRESTON: Oh, yes.

VDT: Some people get a little nervous about the Third World demographers, but they should be there. There's been a big push to increase their membership.

PRESTON: Well, they've improved their quality a lot as more and more of them have gotten training in the West, especially in the U.S. I have half seriously compared the quality of African demographers to the quality of European demographers outside of France and Belgium. Really there are not very many good European demographers.

VDT: What about the Dutch?

PRESTON: The Dutch have a couple of good ones. Eastern Europeans--it's a wasteland. England has certainly some good demographers.

VDT: And the Africans, many of them trained in the U.S.?

PRESTON: Yes, and they're trained in modern demography and they're quite adept. On the other hand, they go back to situations which are not supportive of their research and, as a result, they have real trouble getting things published that they've done when they go back. We've been talking here for a number of years about trying to get an African demography journal started at Penn that would be an outlet for them.

VDT: At Penn?

PRESTON: Well, it's not going to happen anywhere else, I don't think, and probably shouldn't happen at Penn in the best of all possible worlds, but we have to deal with the world we're living in. Etienne in particular would like to get that started, but I would too. We haven't found anybody that wants to give us the money to do it, yet.

VDT: One final big question--the world population outlook. Are you optimistic or pessimistic? You've said what we all know, that the U.S. is an aging society and the problems that we face there.

PRESTON: I would say a hundred years from now the population problem as we have known it in our generation will have passed from the scene. Meaning that we are not going to be concerned with excess fertility a hundred years from now. Fertility is falling quite rapidly in many developing countries--as demographers have expected, but you're never sure your expectations are right. Latin America, East Asia, certainly, have rapid fertility decline and a number of countries are now below the replacement level. Africa is very far from that stage, but there's every reason to think that if development is successful there . . .

VDT: Huge if.

PRESTON: Yes, a big if--but that eventually, within a century, fertility will fall very sharply. So, if that's being optimistic, I guess I'm optimistic. On the other hand, we all know that population decline, or population aging, which is the counterpart of slowing rates of population growth, creates its own problems. I guess I'm optimistic there too that we will then be able to deal with those problems. But right now, I continue to feel that we are spending too much of our national attention and policy emphasis on the aged.

VDT: Too much? Even though you've just finished saying that this is an aging society.

PRESTON: It's an aging society, but I think on a per capita basis the aged have done extremely well, continue to do extremely well, are going to do even better in the future. In the policy arena it seems to be very difficult for congressmen to stand up to the elderly lobby, to the range of interests represented by older Americans, and call a halt to the expansion, although I guess the Pepper bill that was voted down last week is some sign that they're not going to get everything that they want. On the other hand, they also passed the catastrophic health bill the same day, which is going to be quite expensive [and was repealed--under pressure from the elderly lobby--the following year]. So, in the short term, that's the thing that I think is the great imponderable in the American political arena--where benefits for the elderly are going to come to rest. I don't think they've come to rest yet, because if you look at the public opinion polls the program that Americans want to see expanded--the most popular program for expansion--is medical benefits for the elderly. I think we'll be able to come to grips with this problem, but it hasn't quite happened yet.

VDT: My very last question is a corny one, one of Ansley Coale's: What makes Sammy run? How do you get it all done? You have a tremendous program, so much on your platter.

PRESTON: I don't know. I work steadily and I work quickly. Probably there are 55-hour weeks, which is not that unusual for academics, but I'm pretty steady at it.

VDT: And I understand that you have a fine family life and you're even a supporter of baseball--Little League baseball at one time, at any rate.

PRESTON: That's right. I have a reasonably healthy life outside of the office, I hope. I find that the hours that are sort of demanded of academics at this point are the most problematic feature of my life, but if I can keep it to 50 to 55 hours a week, then things are tolerable. But, again, when I see assistant professors, the pressure that they're under on a daily, weekly, monthly basis, I'm not sure I could do it again if I were just starting out now.

VDT: Well, you've done it brilliantly so far. I hope there'll be many more decades of it. Thank you very much.

PRESTON: Thank you, Jean. It's been very agreeable.

CONTINUED

VDT: Sam has just told me another very interesting thing--that you're songwriting?

PRESTON: You mentioned that Dudley Duncan is now deeply into music and that a number of demographers are, so I added the fact that my current hobby is writing country and Western music.

VDT: For the guitar?

PRESTON: Yes, for guitar.

VDT: Where did this come from? Did you have any musical training?

PRESTON: My only musical training was on the tuba. I was actually a good tuba player, the first chair in the Pennsylvania state band, when I was in high school, and thought briefly about going to musical school and becoming a professional tuba player, but . . . So, then there was nothing for about 20 years.

VDT: You could read music?

PRESTON: I could read music, yeah. I was on my way to China, actually, and read an article in I think it was Esquire about Harland Howard, the famous country songwriter. I had come to enjoy country and Western music and had also thought that if I ever had a second career what I'd really like to be is a songwriter. So, I decided on the spot, on the airplane, I'd launch my second career. I wrote a song and I've written maybe ten at this point and have had three of them performed by amateur groups that I was able to assemble and I have a tape. One of these days I'm going to take the tape to Vanderbilt--sorry, Nashville--and maybe even myself to Vanderbilt--and see if I can find a publisher who's interested in them. I just sent a bunch out in the mail and they mostly came back unopened. You've got to go to Nashville, apparently, to sell them. But mostly it's just a hobby and I enjoy it.

VDT: Interesting. Does your son, the guitar player, play them?

PRESTON: My son plays them; he unfortunately can't sing.

VDT: Do you sing?

PRESTON: No, I don't sing at all. I don't sing and I can't play an instrument that helps me write songs. I just pick them out. I write the lyrics and music, but not the chords; somebody else has to come along and do that.

VDT: I interviewed Paul Demeny in New York last week. He was a topnotch bassoonist--also at the high school level. In those days in Hungary, things were very troubled. He wasn't sure he'd get into university--his father had been a judge--so he concentrated on the bassoon for a while and the local orchestra offered him a job as a bassoonist. Then he was accepted into the university and put away the bassoon and hasn't taken it out since.

And I interviewed Jack Kantner recently in Bedford, Pennsylvania, and he has the trumpet in his background. In fact, he's gone back to it a bit, like Joe Stycos, who is also a jazz musician [on the piano], as you know. Bedford has a music festival each summer and Kantner is the director of it this year.

PRESTON: You're kidding!

VDT: And he has a son who is a flutist and plays in the Grand Rapids, Michigan, orchestra.

PRESTON: You know who is the conductor of the Grand Rapids orchestra? Michael Aiken's wife. Michael Aiken is our provost--also trained as a demographer, by Ron Freedman. So, it's a small world.

VDT: Small world! Then there is my friend Leon Bouvier, who was at the Population Reference Bureau, who started off as a jazz musician, the trumpet. You remember he and Joe Stycos performed at the 50th anniversary meeting in 1981.

PRESTON: I remember.

VDT: There seem to be many demographers who have music in their background.



Annual Review of Sociology

My Life in Words and Numbers

Samuel H. Preston

Department of Sociology and Population Studies Center, University of Pennsylvania,
Philadelphia, Pennsylvania 19104; email: spreston@upenn.edu

Annu. Rev. Sociol. 2020. 46:21.1–21.17

The *Annual Review of Sociology* is online at
soc.annualreviews.org

<https://doi.org/10.1146/annurev-soc-110619-031647>

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Keywords

mortality, life expectancy, urbanization, population growth, formal demography, divorce, African American, inequality, smoking, obesity

Abstract

George Orwell reportedly said that “Autobiography is only to be trusted when it reveals something disgraceful.” I’ve done my best to eliminate any disgraceful episodes from the following account, thereby compromising its trustworthiness. What’s left is a record of someone lucky enough to (a) find a career as a demographer that was ideally suited to his affinity for writing and math and (b) find institutions and individuals who vigorously and uncompromisingly supported the search for a better understanding of our social world. I can only hope that I have been equally supportive of students, colleagues, and institutions whose pathways I have crossed.



INTRODUCTION

I was born in Trenton, New Jersey, on December 2, 1943, into a family with two older sisters that lived in Lower Makefield Township, Pennsylvania. Lower Makefield is a quiet white-collar suburb of Trenton. My father was a life insurance salesman and my mother was a bank teller. Day care was the responsibility of my maternal grandmother, who lived with us until her death when I was age 10.

My father was a warm, intelligent man with a wonderful sense of humor. He had been forced by his father to leave school before finishing high school in order to help support his family. Perhaps for that reason, he placed a heavy emphasis on my success in school. My mother was at least as avid in pressing for good grades because that was seen as the main route to success in life; success for her children was her paramount concern. Their aspirations were aided in 1952 when US Steel located a new plant in the area and brought thousands of new families into the local Pennsbury school district. Enrollment grew quickly by a factor of three or four. The district built new buildings, hired talented new faculty, and began to offer honors programs in major subjects. I was a prime beneficiary of US Steel's arrival.

Elementary school was mild torture for me, sitting in interior confinement when there were important games to be played out of doors. I only enjoyed school when competition was involved, for example, when there was a chance to be first to solve a math problem. I was, however, eager to learn to read so that I could read the sports section of a newspaper, still my first activity of the day. And math skills were valued because they helped me to understand and calculate batting and earned-run averages. They were also useful for running a baseball gambling operation to which my parents were alerted by my third-grade teacher.

The most important event of my elementary school life was the transfer into the school system and into my third-grade class of Winnifred de Witt. We dated in junior and senior high school and married on a date we set in high school, June 19, 1965. We have been married for 54 years and have four children, Sam, Andrew, Ben, and Leah, and nine grandchildren. Winnie has provided fabulous support for both of my extremely gratifying careers as a husband/father and as a demographer.

Some light bulbs turned on in junior high school and grew brighter in high school. I began to enjoy and appreciate American literature, especially that focused on the struggles of working-class families. I read nearly all of the dozen or so novels by John Steinbeck, and much of Dos Passos, Sherwood Anderson, Richard Wright, William Faulkner, and muckrakers like Upton Sinclair. These appealed to and stimulated a social conscience. I wrote short stories for the school newspaper and poems (later, songs) for self-satisfaction. And I appreciated the elegance and certitude of mathematics.

In high school I took something called the Kuder Preference Test, which was designed to show us what we liked to do, a seemingly superfluous function. It asked such questions as whether we would rather be fishing, caring for a sick patient, or sewing. I had a distinctive profile. I finished in the 99th percentile of liking mathematics and the 99th percentile of liking to write. The concluding suggestion was that I write a stock market column. That is remarkably close to what I have done most of my professional life, although I write about numbers of people and rates of change rather than price/earnings ratios. The Kuder preference test has helped to remind me that writing about numbers is not work for me but a form of play. This reminder has been especially useful after reaching an age when I am socially permitted, even encouraged, to stop working.

I did well in high school and was admitted to Harvard, Princeton, Brown, and Amherst. I chose Amherst, which had a gorgeous campus, had a highly demanding academic reputation, and offered a chance to play baseball at the college level. My best friend's father had played catcher for Amherst, and he was instrumental in pushing me in that direction.



Amherst had a grueling curriculum, requiring six freshman courses including physics, calculus, English composition, and history. Two English compositions per week were required in the fall semester and three in the spring. These were less of a chore for me than for many others. When it came time to choose a major, my affinity for math and writing led inevitably to economics. Sociology would have been a likely choice, but Amherst had no courses in the subject at the time. Fortunately, Amherst had a wonderful economics department that sent four or five graduating seniors each year to elite PhD programs. I loved the abstract logic of economics, its applicability to important social issues, and the opportunities it presented for formal solutions. I was especially interested in labor economics because of its direct connection to people. My senior thesis was an update on the Phillips curve, an empirical representation of the time-series relationship between unemployment rates and interest rates.

By my senior year at Amherst, 1964–1965, there was no question that I was headed to graduate school in economics, supported by a Woodrow Wilson fellowship. Among other attractions, graduate school offered a deferment from military service when heavy fire was erupting in Vietnam. My first choice was Princeton, to study with William Bowen, a leading labor economist. As things turned out, he was on sabbatical and then became provost and president of Princeton and never taught while I was there. The remaining opportunities in labor economics were not especially appealing.

I filled in by taking a demography course taught by Ansley Coale. This was the only course in the economics catalog that I had crossed off as uninviting upon entering graduate school. But it was clear after several lectures that my career path had shifted, a true conversion experience. Births and deaths connected to the most basic of human affairs; issues of measurement were paramount in demography, adding a strong flavor of intellectual integrity; the population explosion was high on the agenda of social problems; and population mathematics had at its core a beautiful construction, the stable population model. This equilibrium model revealed the “intrinsic” characteristics of a population, including its age distribution and growth rate, that could be calculated based on its age-specific fertility and mortality rates alone.

Adding to the attraction of demography, Ansley was a marvelously clear and enthusiastic expositor (Preston 2005). His enthusiasm was on display not only in the classroom but throughout the Office of Population Research. He would often wander the halls after his daily noontime bout of tennis or squash, looking for someone to whom he could show his latest hand-drawn graph. I was an eager audience.

My dissertation topic, suggested by Ansley and enthusiastically adopted by me, was on the mortality effects of cigarette smoking as they were manifesting themselves in international mortality patterns. Why this topic was acceptable for a dissertation in economics is a mystery to me, but I’m grateful that it was. I collected a good deal of data on the history of mortality rates in developed countries and was one of the early users of the massive computer on Princeton’s campus, laboriously typing and submitting punch cards and returning hours later to obtain the output. The dissertation showed that the patterns of mortality among older males were divergent from those of other groups in a manner that was closely correlated over time and space with the prevalence of cigarette smoking. And the causes of death responsible for the divergence were those that were most closely associated with smoking. After revision, the dissertation was published in the form of a monograph and an article (Preston 1970a,b).

POSITIONS AND TRANSITIONS

I entered the job market in 1967–1968, my third year of graduate school. My first interview was at Bowdoin College, which did not offer me a job. Late in the cycle, I was fortunate to receive



a job offer from the Department of Demography at Berkeley, the only such department in the United States then or now. In 1968 I became one of three members of the department, together with senior scholars Nathan Keyfitz and Judith Blake. By the time I was 24 years old, I had studied with one of the two leading technical demographers in the world, Ansley Coale, and had become a colleague of the other, Nathan Keyfitz. They set a standard of excellence in research and of professional citizenship that provided a wonderful example for a budding demographer.

Nathan was an elegant, kindly man deeply committed to science. His daily routine at Berkeley was unlike any other. He would go to bed after dinner and awake around 2 a.m. to think and write. The typical product was a short 2–4-page mathematical paper that would be typed up by his wife Beatrice and brought by Nathan to campus the next day around noon. It would be distributed among students, who relished the opportunity to make comments and suggestions on such timely material. Much of this work eventually appeared in *Applied Mathematical Demography* (Keyfitz 1985).

Judith Blake and Nathan Keyfitz were both trained as sociologists, and Judith was married to the prominent and powerful sociologist Kingsley Davis, who cast a long shadow over the small department. So while I taught courses in demographic methods and the demography of the labor force, I gained some exposure to the field of sociology through interaction with my colleagues.

Unfortunately, my colleagues did not interact well with one another. The core of their disagreement was over how faculty should behave with respect to antiwar activities, especially those involving graduate students in the department. The Vietnam War and the Cambodian incursion were roiling college campuses, nowhere more violently than at Berkeley. The building housing the Demography Department was repeatedly evacuated when tear gas entered the ventilation system. Nathan allowed his home to be used for preparing antiwar leaflets and Nathan and I joined students in antiwar demonstrations.

Judith and Kingsley believed that such behavior violated professional standards. As departmental chair, Judith began a campaign to drum Nathan out of the department. Graduate students in Demography went on strike against the war and signed petitions against her leadership. Her anger toward them became extreme. I was largely spared her hostility, possibly because of my youthfulness but maybe also because I represented the balance of power in the tiny department.

Judith initiated proceedings for my promotion to associate professor with tenure in my fourth year, when I was 27. But the department had become so inhospitable that I decided to accept a tenured offer from the Department of Sociology at the University of Washington. I had apparently become a sociologist by osmosis! Stan Lieberman, in the process of leaving Seattle for the University of Chicago, had heard me deliver a dissertation-based paper at a professional meeting and encouraged the department to pursue me.

Nathan left for Harvard at that point, and I was offered a chance to join him. But I was ready to strike out on my own as director of the Center for Studies in Demography and Ecology at the University of Washington, Stan's former position. The University had a strong Sociology Department with high standards and modest resources. The small space allocated to the demography center in the silverfish-lined basement of Savery Hall needed firm defense against an ambitious macrosociology group.

While at the University of Washington, I was invited to organize an international seminar on "Infant Mortality in Relation to Level of Fertility" sponsored by the Committee for International Coordination of National Research in Demography (CICRED). The Committee was headquartered in Paris and presided over by Jean Bourgeois-Pichat, a distinguished demographer. The seminar was held in Bangkok, and the papers were ultimately published (Preston 1978).

Bourgeois-Pichat was a member of the Population Commission, the politically appointed oversight body of the United Nations (UN) Population Division. On the basis of the successful

seminar, he recommended me for an opening in the Population Division. When the position was offered in 1974 or 1975, I told the UN representatives that I had made a commitment to the University of Washington and couldn't leave at that point but mentioned that if a position were to open up later I might be interested.

Two years later, in 1977, I received a call out of the blue from Sankar Menon, an official in the Population Division. He said, and I will never forget the words, "Mr. Preston, we agree to your proposal." I was thunderstruck. The clever way that Menon posed the matter made me feel that I had committed myself to working at the UN, an organization I held in the highest esteem and one that I had no intention of deceiving. I went home that night and told Winnie that there was a good chance that we might be moving to New York, at least for a period of time. The news, and the manner in which it had been transacted, were not well received.

So from 1977 to 1979, I was on leave from the University of Washington as acting chief of the Population Trends and Structure Section of the UN Population Division in New York. While there, I was principal author of the first major study of world urbanization, *Patterns of Rural and Urban Population Growth* (United Nations 1980). At the time, most governments in developing countries were eager to restrain urban growth, and many had taken steps to do so. A common but usually unspoken motivation was that urban populations were politically more unruly than rural populations. Findings in the volume did not support aggressive efforts in this direction. Urban growth was shown to be fastest where economic growth rates and industrial expansion were most robust, while changes in proportions urban among developing regions were not outpacing historical standards. Natural increase, not migration, was the main source of urban growth.

Shortly after arriving, I realized that the UN was not going to serve as a permanent research home despite some powerful advantages. I had spent the previous decade on university faculties where there was a close relation between achievement and reward, a relation that seemed just and appropriate. The UN did its best to obscure that relation with hiring based heavily upon nationality preferences and promotions based on seniority. I was chief of a research section and wanted to hire an Israeli economist for an opening, but after 6 months of delay in the personnel office, it became clear that his nationality was an insurmountable obstacle.

Although I was leaving the UN, Winnie and I decided that we wanted to remain in the Northeast rather than return to Seattle. I accepted a professorship in sociology at the University of Pennsylvania, which has proven to be a wonderfully supportive research home for four decades. Its PhD program in demography has been especially instrumental by providing a stream of outstanding graduate student collaborators, 38 in all. I have taken perhaps my greatest professional satisfaction from supervising 56 doctoral dissertations of students on their way to productive professional careers. A remarkable set of demographic colleagues at Penn have provided support and inspiration in the course of their star-studded careers: Jere Behrman, Irma Elo, Douglas Ewbank, Frank Furstenberg, Michel Guillot, Hans-Peter Kohler, Douglas Massey, Philip Morgan, Jane Menken, Emilio Parrado, Mark Rosenzweig, Herb Smith, Etienne van de Walle, Susan Watkins, and Tukufu Zuberi.

What follows is an effort to summarize some of the major themes that have characterized my research beyond the two studies that I have already described.

Research on Mortality

My dissertation research began a lifelong focus on mortality analysis, which at the time was primarily the province of actuaries. To me, as a social scientist, mortality analysis had many appealing features: Early death is unambiguously a (negative) welfare indicator; death is precisely measured, unlike nearly all other social indicators; and the system for recording deaths reaches into every



household and village on a continuous basis, providing a social indicator with exceptionally uniform and timely coverage.

The dissertation research had identified the tip of an iceberg, the large array of national data on mortality rates by age, sex, and cause of death. These data were capable of shedding a great deal of light on dramatic changes in the way people lived, as reflected in the manner in which they died. Records of deaths by cause, age, and sex with accompanying census counts existed for several European populations starting in the mid-nineteenth century and for several Asian and Latin American populations beginning in the early twentieth century. By 1964, 43 countries supplied national data on deaths by cause, age, and sex.

To convey the patterns lying beneath these data, someone needed to assemble, process, describe, and analyze them. Somewhat recklessly, I undertook these tasks as my first major post-dissertation project. An older and wiser person would have found a dozen reasons to avoid this tedious assignment. The assembly process was aided by several compilations by the World Health Organization, by many trips to the New York Public Library and the Library of Congress (\$0.25 copying charge per page!) and by writing to national statistical agencies for missing data.

Data processing was facilitated by a small National Science Foundation grant of \$29,000 that enabled me to hire a dedicated former doctoral student at Berkeley, Verne Nelson, who aided in data processing. And I acquired two coauthors of the volume presenting the resulting mortality tables: Robert Schoen, then a graduate student at Berkeley with excellent computing skills, who converted the data into life tables and other mortality measures, and Nathan Keyfitz, who had coauthored a volume presenting demographic data on many historic and contemporary populations that served in many ways as a model for the mortality volume (Keyfitz & Flieger 1968).

The volume of mortality tables totaling 787 pages was published by Seminar Press, later acquired by Academic Press (Preston et al. 1972). An analytic volume based on these data was focused on establishing models of how mortality from various causes of death typically contributed to changes in mortality levels from all causes of death combined and how that contribution varied over age, sex, time, and space (Preston 1976a). This approach to characterizing the epidemiologic transition became a basis for later models, including those used in the massive ongoing Global Burden of Disease effort spearheaded by Christopher Murray (Salomon & Murray 2002).

One other analyst at the time was exploiting cause-of-death data. The medical historian Thomas McKeown used data on England and Wales starting in the 1840s to study the factors responsible for improvements in mortality during the next century (McKeown & Record 1962, McKeown 1976). He showed that the mortality decline was primarily attributable to declines in infectious and parasitic diseases, especially tuberculosis, and that, for disease after disease, the bulk of the decline had occurred before any medicine or therapy was available to combat the disease. This was a valuable demonstration. McKeown argued that, if medicine were not responsible for mortality improvements, standards of living must have been. In particular, he attributed the bulk of the mortality decline to improved nutrition. But he presented no direct evidence about nutrition's role, and his process-of-elimination reasoning failed to eliminate some obvious alternatives such as improvements in public health and personal hygiene.

I addressed the factors responsible for mortality improvements in an article entitled "The Changing Relation Between Mortality and Level of Economic Development" (Preston 1975a). My work on international cause-of-death patterns had convinced me that the English history, featuring the role of tuberculosis, was highly atypical, probably because England was by far the most highly urbanized population in the world at the time.

My article used repeated international cross-sections to demonstrate that improvements in standards of living, measured by per capita income, could have contributed only about 20% to worldwide improvements in life expectancy between the 1900s and the 1960s. The remainder



was attributable to shifts in the curve, i.e., to technological and organizational improvements that converted a particular level of per capita income into higher and higher levels of life expectancy as the century advanced. Angus Deaton et al. (2004) dubbed the graphical core of the analysis “the Preston curve,” a term that has caught on without resistance from me.

The paper has been cited 1,900 times and is my most frequently cited paper. It was reprinted with commentary in 2003 as a “Public Health Classic” by the *Bulletin of the World Health Organization* and was also partially reprinted by the *International Journal of Epidemiology* with four commentaries in 2007. David Bloom and David Canning, development economists at Harvard, provided the most flattering commentary and so deserve to be quoted at length. They say that

Samuel H. Preston’s classic paper...remains a cornerstone of both global public health policy and academic discussion of public health... In sum, many discussions and insights that are at the heart of economic and human development would not have arisen, or would have arisen much later, without Samuel Preston’s paper. Indeed, this paper demonstrates well the aptness of Oliver Wendell Holmes’ famous line: ‘One’s mind once stretched by a new idea, never regains its original dimensions.’ (Bloom & Canning 2007, pp. 498–99)

The paper continues to attract attention. According to Google Scholar, the highest number of annual citations since the paper was published occurred in 2016, 2017, and 2018. “Preston curves” are now used to study shifts in relations between many types of variables measured at the country level (e.g., Masters et al. 2018). It is possible that my undergraduate thesis on the Phillips curve burrowed into a mental substratum and laid the foundation for this paper.

The macrolevel assessment of factors in mortality change was followed by a microlevel examination to illuminate the social processes at work. The US Censuses of 1900 and 1910 asked questions on the number of children a woman had borne and the number of those children who were still alive. The responses provided an excellent vehicle for studying child mortality variation at the family and ecological level. They had never been tabulated until colleagues and I produced public use samples from manuscript records from the US Censuses of 1900 and 1910. The 1900 sample was the first such sample drawn from any census.

Michael Haines and I published a monograph analyzing these data (Preston & Haines 1991). Among other findings, we showed that being literate or a teacher or physician produced little advantage for the survival of one’s children at the turn of the twentieth century. Instead, the dominant factor was the size of the city or town in which one lived, indicating the extent to which survival was still in the grips of natural forces. Doug Ewbank and I showed that reductions in child mortality over the next three decades were much faster among professional and urban classes as the new germ theory of disease affected public and private health practices (Ewbank & Preston 1990). These results were consistent with the earlier claim that mortality improvements during this era were primarily attributable to advances in health technology and practice rather than to gains in income or improvements in diet. In the course of this research on social class differences in mortality, I developed two widely used measures of inequality in mortality, the slope index of inequality and the relative index of inequality (Preston et al. 1981).

In studying mortality change over the past 150 years, I feel like an observer of one of the greatest of human achievements, one that is not always fully appreciated. US life expectancy rose from about 49 years in 1895 to 79 years today. In an effort to demonstrate the significance of this achievement, Kevin White and I used counterfactual population projections to estimate that, in the absence of twentieth century mortality improvements, the US population in the year 2000 would have been only half as large as it actually was. One-quarter of the population would have been born and died, and another quarter would never have been born because of a prereproductive death to a parent (White & Preston 1996).



Variable- r Equations

Shortly after I arrived at the UN in 1977, I was appointed to a committee of the US National Academy of Sciences chaired by Ansley Coale. The main purpose of the committee was to prepare estimates of levels and trends in fertility and mortality in developing countries lacking high-quality data, which constituted the large majority of developing countries. This began a period in which I became active in developing estimation methods in collaborations with Alberto Palloni, Ken Hill, Shiro Horiuchi, Neil Bennett, and Ansley Coale. The panel included Bill Brass, a brilliant demographer from the London School of Hygiene and Tropical Medicine. An undercurrent of the panel's work was friendly competition between Brass and his students and Coale's contingent to see which group could produce better methods. The work of the committee represented, in many ways, a high-water mark for advances in technical demography. It produced a sterling methodological volume written by two people involved in the project, Ken Hill and Hania Zlotnik, the former a student of Brass's and the latter a student of Coale's (United Nations 1983). The project produced more than two dozen monographs that reconstructed the demographic parameters of different countries. My assignment was India, which was carried out in conjunction with a talented graduate student at Penn, P.N. Mari Bhat, and Tim Dyson from the London School of Hygiene and Tropical Medicine (Bhat et al. 1984).

Methodologically, I focused on demographic methods for estimating the completeness of death registration and presented an alternative approach to Brass's (Preston & Hill 1980, Preston et al. 1980). Shiro Horiuchi and Neil Bennett, graduate students at Princeton where I was a visiting research associate in 1980, were also working in this area.

In December 1981, I was on an airplane returning from a conference in Manila, musing over an equation developed by Horiuchi and Bennett. In the course of a short derivation, I suddenly realized that the three basic equations characterizing a stable population were a special case of an (almost) equally simple set of equations that characterize any population. All that was necessary was to replace the constant value of the growth rate, r , in the stable equations with growth rates that were a function of age, $r(a)$. This discovery produced a level of excitement that I haven't matched before or since. I still can't shake the idea that I was being rewarded for returning to cold Philadelphia to teach my last class of the semester, while most of my demographer friends stayed in Manila to splash in the hotel pool and eat mahi-mahi.

I invited Ansley Coale, with whom I had been collaborating on death registration methods, to be coauthor of the resulting paper (Preston & Coale 1982). Shiro Horiuchi contributed a valuable footnote to the paper that we later expanded into a paper clarifying exactly how the $r(a)$ function was determined and why it contained so much information (Horiuchi & Preston 1988). The expressions in the Preston/Coale paper have proven useful in estimating demographic parameters in countries with flawed or missing vital statistics data since the $r(a)$ function can be observed in any country with two censuses (e.g., Cai 2008). They also facilitated the introduction of migration into estimates of intrinsic growth rates (Preston & Wang 2007).

The variable- r expressions have also proven useful in understanding population dynamics. For example, all population aging must occur through the $r(a)$ function, which can be decomposed into elements of births, mortality, and migration (Preston & Stokes 2012, Murphy 2017). However, the stable model is still necessary for a full accounting of long-term relations between child mortality and population growth (Lee & Zhou 2017). The use of variable- r methods also demonstrated that population momentum—the tendency of populations to continue growing after reaching a net reproduction rate of 1.00—was a product of population growth in the age interval above age 30 or so rather than of continued growth of the number of births, which had been the previous and much more alarming interpretation (Preston 1986a, Kim & Schoen 1997).



Children and the Elderly

I was elected to serve as president of the Population Association of America (PAA) in 1984. At a plenary session of the annual meeting of the PAA, the president delivers a formal scientific address which is published in *Demography*. I decided to branch out from the areas that I'd mainly been working in and talk about something that was on my mind, the deteriorating conditions of American children, as measured especially by poverty rates, mental health, and academic achievement. At the same time, people over age 65 had been doing much better on economic and health measures. I decided to document these trends and try to provide some insight into the factors producing them (Preston 1984a).

The divergent trends in measures of well-being between children and the elderly were not difficult to document but seemed to come as a surprise to many readers and listeners. Identifying reasons why older people's poverty levels had shrunk from way above average to below average was not difficult in a period when Social Security benefits had been expanding sharply. I attributed the deteriorating position of children in part to changes in family structure over the previous two decades. The disappearance of many fathers and husbands from traditional family structures produced many female-headed households with their characteristic high levels of poverty. School achievement and children's mental health were also impacted.

Rather than coming to the rescue, public policy during the Reagan years was turning its back on children, including sharp cutbacks in the Aid to Families with Dependent Children program. Political support for programs directed at the elderly was much stronger than for programs directed at children. I argued that there were three sources of self-interested support for the elderly: older people themselves, people with older relatives who threatened to require family support, and the working-age population who voted on behalf of themselves in anticipation of reaching old age. Only one of these sources of support is available for children. Children don't vote, and adults don't vote on behalf of their former childhood, which is water over the dam. Changing demography and age differences in voting participation meant that only 38% of voters in the congressional elections of 1982 lived in a family having a child below age 18. I documented huge age differences in support for public expenditure on schools and child care facilities. The paper ended with a call for greater collective responsibility for raising the next generation.

The paper received a good deal of press coverage, and a shortened version that was published in *Scientific American* was also widely cited (Preston 1984b). Reviews were bifurcated along the predictable dimension. The lobby for older people benefitted from the perceptions of widespread elderly poverty and disadvantage, perceptions that I was challenging. An editorial in *Modern Maturity*, the official organ of AARP, called me "America's leading crusader against the elderly." My parents and Winnie's parents called to complain within hours of the editorial's appearance.

However, advocates for children welcomed the message. Albert Shanker, president of the American Federation of Teachers, paid for a column in the *New York Times* reiterating and endorsing the analysis. I was invited to write an editorial for *Pediatrics* (Preston 1986b). I joined the Board of Directors of Child Trends, a research agency headquartered in Washington. The most visible and gratifying supporter was New York Senator and sociologist Daniel Patrick Moynihan, who wrote me several kind notes and cited my work in the first paragraph of the Foreword to his book *Family and Nation* (Moynihan 1986) and on the front page of the *New York Times*. He endorsed the paper in a 1992 letter to incoming First Lady Hillary Rodham Clinton (Moynihan 2010, p. 591).

The paper provided a framework for later research on the comparative position of children and the elderly. A National Academy of Sciences project organized around the relative position of children and the elderly in various countries produced a major volume, *The Vulnerable*, demonstrating the poor standing of US children in international comparisons (Palmer et al. 1988). Shigemi Kono



and I contributed a paper on Japan to the volume (Preston & Kono 1988). In an extensive recent follow-up, Gibson-Davis & Percheski (2018) demonstrate the widening disparities in wealth between households with children and elderly households in the United States between 1989 and 2013.

Family Demography

Like many other demographers, I have developed and applied methods to study the characteristics of the most basic social group, the family. In one paper, a personal favorite, I showed that the average number of children in a family is greater from the point of view of a child than from the point of view of a mother because individuals from large families get more heavily weighted in the former tabulation. The precise relation between the two for a group of mothers and their children is (Preston 1976b):

$$C = F + \frac{\sigma^2}{F}$$

where C is the mean number of children in a family when children are queried, F is the mean number of children per woman when women are queried, and σ^2 is the variance in family sizes among women. The larger the variance in family size, the greater the discrepancy between C and F . The discrepancy is often huge. For example, women aged 45–49 in the United States in 1950 had borne 2.29 children, on average. But their children came from sibships that averaged 4.91 children, more than twice as many. Our views of fertility levels in the past are severely distorted if they are based, as they often are in informal accounts, on reports of individuals about the size of their families of orientation.

The same distinction between types of mean size computations applies to all other social aggregates. For example, the mean number of people in one's household is much larger than the mean size of household. The practice of the US Census Bureau to provide the latter rather than the former figure has given the impression that we are far more isolated in small households than we in fact are (King & Preston 1990). People live in larger cities than the mean size of city, people attend larger classes than the mean class size, and so on. This same relationship was rediscovered to explain why your friends have, on average, more friends than you do (Feld 1991).

Marriages form another social aggregate whose processes are subject to strict demographic rules. Marriages can end by death of husband, death of wife, or divorce. Rates of death and divorce in a particular period can be combined to estimate the probability of divorce for a synthetic marriage cohort subject to those rates. It had been several decades since anyone had calculated that probability when I did so in 1975. The eye-opening result was that 44% of American marriages would end in divorce under the dissolution rates of 1973 (Preston 1975b). In a project supervised by Andy Cherlin and me, James Weed updated the analysis for the National Center for Health Statistics (NCHS) using data for 1976–1977 and found that the probability of divorce had risen to 49.6% (Weed 1980). I later confirmed, using variable- r methods, that the probability of divorce based on rates of 1975–1980 was 52.0% (Preston 1987a). The core result, that half of American marriages would end in divorce under current rates, became part of the national self-identity and is still widely cited today. What has happened since 1980 is not altogether clear, in part because NCHS has stopped producing the requisite data on divorce. Some survey data indicate that the probability of divorce has declined (Stevenson & Wolfers 2011) but others dispute this conclusion (Kennedy & Ruggles 2014).

Families transmit characteristics from one generation to the next. But there is much misunderstanding of this transmission process. Cameron Campbell and I took on the controversial issue



of fertility differentials by IQ score. We used an empirical matrix relating the IQ scores of father and mother to the IQ distribution of their offspring. We combined that with two alternative assumptions about marriage: random marriage by IQ score and total homogamy. In either case, a constant pattern of negative fertility differences by IQ score produced an equilibrium IQ distribution within several generations (Preston & Campbell 1993). So, contrary to intuition and popular belief, negative fertility differentials do not inevitably lead to a declining IQ distribution; they are perfectly consistent with a stable distribution and in fact will produce such a distribution if maintained. In *The Bell Curve*, Herrnstein and Murray concede the point: "The narrowest mathematical implication of their model remains accurate. It is possible to postulate conditions that produce a constant or even rising IQ in the face of negative fertility differentials" (Herrnstein & Murray 1994, p. 735) but proceed to ignore it in the rest of the volume. Our paper has been invoked to help account for the Flynn effect, the systematic rise in population IQ scores in developed countries (Neisser & Am. Psychol. Assoc. 1998).

African American Demography

Throughout the twentieth century, the quality of data on African American demographic patterns was highly inferior to that of Whites. In the first half of the century, births were under-registered, especially in the South, where a majority of African Americans lived. Census counts omitted 10–20% of Black males in the age interval 15–34. Lacking births certificates, many African Americans had their ages misreported on censuses and death certificates. The result was a great deal of uncertainty about the basic parameters of Black demography.

One feature of reported Black mortality was particularly striking: Death rates at older ages 75+ or so were below those of Whites throughout the twentieth century. The cross-over to lower Black mortality was broadly treated as prime example of a survival-of-the-fittest mechanism; vulnerable individuals were being weeded out at younger ages, leaving behind a group of hardy survivors at older ages (Manton et al. 1981). This explanation seemed implausible to me because evidence from other populations strongly suggested that cohorts that were subject to severe health conditions in early life also experienced elevated mortality in later life (Elo & Preston 1992). In any event, determining whether or not African Americans actually had death rates below those of Whites at older ages could not be done until data flaws had been repaired.

With Irma Elo, a valued colleague at Penn and frequent collaborator, I received a grant from the National Institute on Aging to investigate the level of Black mortality at older ages. The core of the research was a study that matched 5,262 death certificates at ages 65+ to manuscript records of US censuses of 1900, 1910, and 1920 when the decedents were children. Only 47% of matched death certificates recorded an age at death that would be expected based on the individual's age on a childhood census record (Preston et al. 1996). We used variable-r methods on the corrected distribution of ages at death to show that Black mortality at older ages was much higher than officially recorded and was also higher than rates for the White population. The cross-over had disappeared when age-misreporting was corrected. The corrected series of age-specific deaths became the point of entry to a wholesale reconstruction of Black demography from 1930 to 1990 (Preston et al. 1998, 2003; Elo 2001).

Given the linkages between death certificates and childhood censuses, we were able to compare the characteristics of African American children who survived to age 85 to those of randomly chosen African American children in our public use samples from censuses of 1900 and 1910. We showed that survival from childhood to age 85 was significantly lower for those born in urban areas, to illiterate parents, and in households with an absent father (Preston et al. 1998).



Internal Exile and Return

I have held many administrative positions, including two terms as chair of the Sociology Department at Penn. I had always been able to combine administration with research and teaching. But from 1998 to 2005, I took a seven-year sabbatical from demography while I served as dean of the School of Arts and Sciences at Penn. Schools at Penn are basically self-governing and fiscally independent, and the job was all-consuming. Beyond preparing multiple decrement life tables of faculty survival (how can we increase the retirement rate?), the only demography I performed during this period was to use off-hours to write a textbook with Patrick Heuveline and Michel Guillot, outstanding demographers and former graduate students at Penn (Preston et al. 2001). It became and remains the leading textbook in demography.

I was successful enough as dean to be offered a higher office, but seven years away from scholarship and teaching was already way too many. But the long hiatus made reentry difficult, like coming out of graduate school all over again but without a dissertation. The cupboard was bare. Whether consciously or not, I responded by returning to the subject of the dissertation, the mortality effects of cigarette smoking. Male mortality had been the focus of my earlier work. Women had taken up the habit later but with equally fatal consequences that were, by 2005, clearly manifesting themselves in national mortality patterns. In my first paper after exile, Haidong Wang and I showed that sex mortality differentials in the United States were arrayed by birth cohort in a way that could be traced back to sex differences in cohort smoking patterns beginning in the 1890s (Preston & Wang 2006). The cohort basis of this analysis produced a set of projections that confidently predicted future reductions in the female advantage in US life expectancy that have so far materialized (Wang & Preston 2009).

For the past several decades, the United States has had one of the lowest life expectancies of any developed country. The National Academy of Sciences formed a panel in 2009 to identify the sources of its poor performance. I cochaired the panel with Eileen Crimmins. Based largely on a background paper that I wrote with Dana Gleit and John Wilmoth (Preston et al. 2011), the panel concluded that the exceptionally heavy history of smoking in the United States was principally responsible for its shortfall in life expectancy at age 50 (Natl. Res. Council. 2011). The study developed a method for attributing deaths to smoking that has become widely used (Preston et al. 2010).

With Andrew Stokes, then a graduate student at Penn, I also prepared a paper related to the panel's work on the role of obesity in the US disadvantage. We concluded that the mortality risks associated with obesity, although apparently large, were simply too uncertain to draw a firm conclusion about its role (Preston & Stokes 2011). This paper led to a five-year National Institutes of Health grant to clarify the mortality risks associated with obesity. Stokes and I have published a string of papers aimed at better identifying those risks. A basic approach in these papers has been to integrate obesity histories into the analysis of mortality hazards, which avoids some of the most serious biases in earlier studies, including reverse causation resulting from weight loss produced by illness (Preston et al. 2013). Our conclusion is that obesity is a more serious threat than assessed by studies using the most common research designs (Stokes & Preston 2016a,b). We found that the growing imprint of obesity on national mortality levels has been offsetting much of the gain that would have otherwise resulted from declines in cigarette smoking and improvements in medical practice (Preston et al. 2018).

Reflections on Demography

I have spent my career comfortably identified as a demographer. Demography is a small field lacking departmental status in the United States except at Berkeley. It competes for territory and



resources with other social sciences and their subfields. In the United States, demography has found its most accommodating home in sociology departments with their relatively permeable boundaries. I am most grateful to the sociology departments at the Universities of Washington and Pennsylvania for their willingness to provide a home for someone without a formal background in the subject.

Partially substituting for conventional academic resources, granting agencies have provided a good deal of support for demography through research grants, center grants, and training grants. The motivations of granting agencies are, in a broad sense, driven by the perception of social problems and the contribution that demography might make to addressing them. These perceptions help to create the channels through which demography and demographic careers flow.

When I finished my dissertation in 1968, demography was dominated by fertility and family planning. Fertility was on center stage because it was believed by most observers to be too high, especially in poor countries and among poor people in richer countries. These concerns were in part a holdover from eugenics, which had been a steady presence at annual meetings of the PAA. Eugenics eventually became discredited by virtue of its association with policies of Nazi Germany, although such policies were not always being discredited when they were being implemented.¹ I narrowly escaped being tarnished by an apparent association with eugenics when my first publication (on social class differentials in mortality) appeared in *Social Biology*, which had only recently changed its name from *Eugenics Quarterly*.

The problem of rapid population growth may have reached its peak in the hierarchy of social problems in 1979, when Robert McNamara, then president of the World Bank, asserted that “short of nuclear war itself, [population growth] is the gravest issue that the world faces in the decades ahead” (McNamara 1979, p. 737). My career was heavily influenced by this construction. The CICRED seminar that landed me a position with the UN was based on the hope that child mortality declines would reduce fertility levels in poor countries. The National Academy of Sciences panel that underwrote my efforts to develop methods for estimating demographic parameters was funded by the US Agency for International Development (USAID) as part of their effort to reduce fertility in poor countries.

Another USAID-funded panel of the National Academy produced a controversial volume of which I was first author, *Population Growth and Economic Development* (Natl. Res. Council 1986). The conclusion of this quite detailed report stated that “On balance, we reach the qualitative conclusion that slower population growth would be beneficial to economic development for most developing countries” (Natl. Res. Council 1986, p. 90). This tepid language disappointed the sponsors, who had expected a stronger denunciation of rapid population growth. Attacks and counterattacks followed. I summarized my own views, and my frustration with the absence of social science from much of the discourse, in an article entitled “The Social Sciences and the Population Problem” (Preston 1987b).

When I received my PhD, mortality studies were barely visible on the landscape of the PAA. Although there was one important contribution to the field in the 1970s (Kitagawa & Hauser 1973), the only demographers systematically working on mortality besides myself were Eileen

¹A 1938 volume entitled *Needed Population Research*, prepared under the auspices of the PAA, provides a flavor of the field’s eugenic past: “What may be the largest attempt to improve the biological makeup of a human population has recently been undertaken in Germany. By means of eugenic sterilization, it is planned to lower the incidence of certain undesirable qualities in the next generation. At the same time, an increase in the proportion of children from superior stock is being sought through the offering of larger economic inducements for additional children to families in certain so-called upper classes than to those in lower classes. The steps taken to carry out the various phases of this plan should be watched carefully by populationists in all parts of the world and such tests of its effectiveness made as are possible” (Whelpton 1938, p. 183).



Crimmins and Gretchen Condran, two Penn PhDs in demography. The annual meetings of the PAA would typically include only one session on mortality. Sometime in the mid- to late 1970s, I was invited to organize a second mortality session, which had to be cancelled because there were not enough submissions.

By 2018 there were 13 sessions at the annual meeting with “mortality” in the title and another 46 with “health” in the title (not including sessions on reproductive health). There are many reasons for this expansion of interest. One is simply that mortality has become more widely recognized as a valuable social indicator. Widening social class disparities in mortality, consistent with widening class differentials in income and wealth, have raised alarm and drawn a good deal of sociological attention (Elo 2009, Hayward et al. 2015). The huge and growing percentage of national income devoted to health expenditures has directed attention to issues of cost-effectiveness and the efficiency of health care services. An aging population has put the spotlight on health issues in later life. And US mortality levels and trends have become problematic, as noted earlier.

The emergence of population health as a central disciplinary concern (with a growing role for international migration as well) testifies to demography’s adaptability. Adaptability is a valuable survival trait for a small discipline lacking a stable academic base.

Two other assets of demography deserve emphasis. It maintains a well-deserved reputation for integrity and intellectual honesty that reflects its highly empirical orientation and its closeness to the process of data production and evaluation. Demographic conversations are brief when assertions are based on flimsy evidence. And second, demography “owns” some ingenious indexes that provide a unique lens on the social world. They convert a huge volume of aggregate events into meaningful implications for individuals. In so doing, they create a micro/macro link that seems more explicit than in other social sciences: “Life expectancy at birth is the average number of years an individual would live if subject for all of his or her life to the set of age-specific death rates prevailing in a population.” What could be more beautiful?

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

I am most grateful to Irma Elo, Michel Guillot, Jerry Jacobs, Emilio Parrado, Jason Schnittker, and Herb Smith for their thoughtful suggestions that improved this article.

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CHILDREN AND THE ELDERLY: DIVERGENT PATHS FOR AMERICA'S DEPENDENTS*

Samuel H. Preston

Department of Sociology and Population Studies Center, University of Pennsylvania, Philadelphia, Pennsylvania 19104

In 1957, the total fertility rate in the United States reached a postwar peak of 3.68 children per woman (U.S. National Center for Health Statistics, 1976:4). In the two decades that followed, it fell to half of its 1957 value, and seems to have reached a temporary plateau at a figure of about 1.8 children per woman. This sharp fertility decline led to a decline in the number of children under age 15 in the United States by about 7 percent between 1960 and 1982 and to a reduction of 28 percent in the proportion of the population under age 15 (U.S. Bureau of the Census, 1984b:33; 1975:15).

Very different forces were at work at the other end of the age scale. The number of people aged 65 and over increased by 54 percent between 1960 and 1980 (U.S. Bureau of the Census, 1982a:25). Reasons for the growth of this age segment are more complex. Somewhat more than half of the growth is attributable to the fact that the cohorts over age 65 in 1980 were already larger in childhood than were the earlier cohorts. Their relative size underwent little change as the cohorts aged into the pre-retirement years and their relation was projected to continue largely unchanged into the elderly years.¹ In 1971, the U.S. Census Bureau projected that the population aged 65 and over would grow by 17.6 percent between 1971 and 1981, only slightly faster than the projected growth (in the intermediate series) of 14.7 percent for the whole population

(U.S. Bureau of the Census, 1971). But in fact the elderly population grew by 28.4 percent during this period, an increase 61 percent greater than expected. Between 1971 and 1981, the elderly population of the U.S. grew faster than the population of India.

What caused this unanticipated growth spurt is, of course, a very rapid decline in old age mortality. The Census Bureau's 1971 projection anticipated a life expectancy of 72.2 years in the year 2000. But already by 1982 life expectancy was 74.5 years, having increased more than twice as much in 10 years as it was expected to increase in 30 (U.S. National Center for Health Statistics, 1983a:15).

So we have passed through several decades of abrupt demographic change. The child population has declined and the elderly population has spurted. Both of these developments were in the main unanticipated.

Most demographers would probably expect such a rapid change in age structure to have favorable consequences for children and troubling ones for the elderly. Fewer children should mean less competition for resources in the home as well as greater availability of social services earmarked for children, especially public schooling. The sharp rise in the number of elderly persons should put enormous pressure on resources directed towards the older ages, such as medical care facilities, nursing homes, and social security funds. At least this view would be characteristic of those who see the world through a Malthusian lens and find the main social drama to be the

* Presented as the Presidential Address at the Annual Meeting of the Population Association of America, Minneapolis, Minnesota, May 3-5, 1984.

pressure of numbers on some kind of inelastic resource.

My thesis is that exactly the opposite trends have occurred in the relative well-being of our two groups of age dependents and that demographic factors have not only failed to prevent this outcome but have, in many ways, encouraged it. Conditions have deteriorated for children and improved dramatically for the elderly and demographic change has been intimately involved in these developments.

EVIDENCE OF CHANGE IN THE RELATIVE STATUS OF DEPENDENTS

First, let's examine some evidence on changes in the relative welfare of children and the elderly. The job is much easier for the elderly because they are routinely included in our data collection systems and are distinguished in most tabulations. We gather very little information on children, however, and only in the last few years have we come to recognize this deficiency.

Probably the indicator of well-being on which different ages are most readily compared over time is the percentage who live in poverty. We obviously cannot compare personal incomes of the two groups but we can compare incomes in the families with whom they reside relative to some standard of minimal need. The basic standard used by the Census Bureau is an income level three times the cost of the Economy Food Plan as determined by the Department of Agriculture. Families with money incomes less than three times this amount are said to be in poverty. Some allowance is made for scale economies in larger families.

Figure 1 shows the percentage living in poverty, by age, in 1982 and 1970, one of the first years in which age breakdowns are available.² Clearly, the relation between poverty and age has changed dramatically. Although it is U-shaped in both years, the right arm dominates in 1970 and the left arm in 1982. The incidence of poverty among the elderly was

double the national incidence in 1970 but by 1982 the proportion of the elderly living in poverty had actually fallen below the national average. The incidence of poverty among children under 14 in 1982 is 56 percent greater than among the elderly, whereas in 1970 it was 37 percent less. It's no mystery that the main factor in the reduction of poverty among the elderly is the expansion of social security benefits. It's been calculated that 56 percent of the elderly would have been in poverty in 1978 had it not been for such income transfers (Danziger and Gottschalk, 1983:746). The rise in child poverty appears all the more remarkable in view of the greatly increased propensity of their mothers to contribute to family income. 48.7 percent of mothers with children under age six in intact families were in the labor force in 1982, versus only 18.6 percent in 1960 (U.S. Bureau of the Census, 1983a:414).

The measure of poverty incidence has been criticized on several grounds. One is that it's not an indicator of welfare or well-being because people can choose to have more children at the same income level, thereby simultaneously increasing their welfare and impoverishing themselves (Pollak and Wales, 1979). While this argument clearly pertains to adults, it has no relevance to children. If they are poor, it's not because they choose to be. A more important objection is that poverty measures include only money income and neglect many in-kind transfers such as food stamps and Medicare. Most of these transfers have increased over the past several decades. But allowance for these would make the disparity in trend even sharper. A recent Census Bureau study estimated that the market value of noncash benefits grew from \$6 billion in 1965 to \$98 billion in 1982 (U.S. Bureau of the Census, 1984a:XI). The large majority of this increase was in the form of medical benefits and the principal beneficiaries were the elderly. Their incidence of poverty for 1982 is 14.6 percent before the allowance for non-

cash benefits at market value but only 3.5 percent after the allowance.

The equivalent reduction for children under 6 is from 23.8 percent to 17.2 percent, a figure still higher than the *unadjusted* national figure (U.S. Bureau of the Census, 1984a). So Figure 1 actually understates the degree to which child poverty has increased relative to that of the elderly. The comparisons also fail to account for tax payments or for the imputed value of owner-occupied housing, factors that several studies have shown to benefit the elderly disproportionately (U.S. Bureau of the Census, 1984b:30; Coe, 1976). The elderly are not oblivious to their improved status. A 1982 Gallup poll found that 71 percent of those aged 65 and over reported themselves as being highly satisfied with their standard of living, far and away the highest satisfaction level of any age group (Gallup, 1983a:18–19).

Figure 2 replaces an economic indicator with a social one, but the story is much the same. Suicide rates in 1960–1961 rose steadily with age, increasing by a factor of about five between ages 15–24 and 65+. By 1981–1982, however, the age gradient is very much weaker. Instead of increasing by five beyond age 15–24, suicide rises by less than one-half.³ The reduced suicide rate among the elderly reflects what is apparently a widescale improvement in their psychological well-being. In 1957, 22 percent of people over 64 scored very high on a scale of psychological anxiety, compared to only 9.5 percent of persons 21–29; but by 1976 scores on the same test among the elderly had fallen and among young adults had risen to a common value of 15.5 percent (Veroff et al., 1981:354).

Suicide among children is very rare, although the trend is upward. But the few other available indicators of children's emotional well-being, collected by Zill and Peterson (1982), generally suggest that some deterioration has occurred. For example, the U.S. Health

Examination Survey has asked parents whether "anything had ever happened to seriously upset or disturb your child." The percentage answering yes rose from 27 percent in 1963–1965 to 37 percent in 1976; the major reason for the deterioration was apparently a rise in family disruption and marital discord. Zill and Peterson conclude from this and the few other available time series that child stress has increased, primarily as a result of the rise in family disruption.

When we turn to public expenditure patterns, recently reviewed by Bane et al. (1983), the trends are less clear cut. They find that, per member of the recipient group, expenditure on the elderly was three times the expenditure per child in 1960 and remained three times greater in 1979. Both grew very rapidly during this period, so that the absolute gain was three times larger for the elderly. (They include public spending on higher education in their calculations; since this was one of the fastest growing components of expenditure on "children," the growth in spending would be less rapid for the child population below age 18.)

However, since 1979 there has been a sharp break with this pattern as many public programs benefitting children have been rolled back while programs targetted to the elderly have been maintained or expanded. One arena in which children and the elderly compete directly is Medicaid, which provides medical services for poor persons. Children's share of Medicaid payments dropped from 14.9 percent in 1979 to 11.9 percent in 1982 despite a rise in the child proportion among the eligible (Children's Defense Fund, 1984a). The Aid to Families with Dependent Children (AFDC) program has been sharply cut back. In 1979, there were 72 children in AFDC for every 100 children in poverty, but there were only 52 per 100 in 1982 (Children's Defense Fund, 1984a). Meanwhile, between those same years expenditures on Medicare and Medicaid rose by \$32 billion, or by 63 percent (Davis, Karen,



Figure 1.—Percentage Living in Poverty by Age, 1970 and 1982.

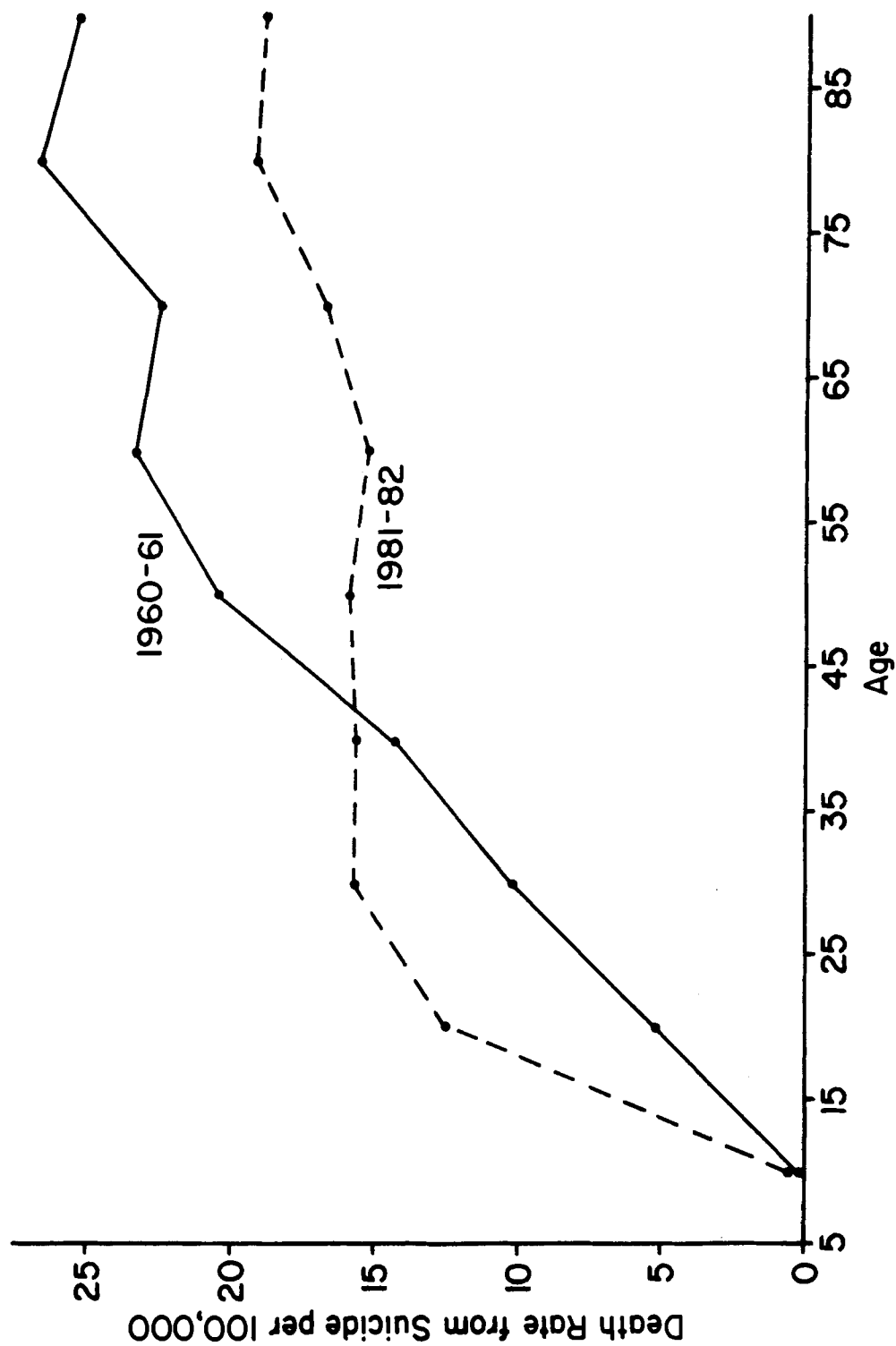


Figure 2.—Age-Specific Death Rates from Suicide in 1960-1961 and 1981-1982.

1983). Medicare outlays alone rose from \$3.4 billion in 1967 to \$57.4 billion in 1983 and are projected to rise to \$112 billion in 1988 (Congress of the United States, 1983).

The Office of Management and Budget recently began estimating the fraction of federal benefits that are directed towards the elderly. The elderly received \$44 billion in federal dollars in 1971 and \$217 billion in 1983, some \$7,700 per capita (U.S. Bureau of the Census, 1983a:376). The benefits were a smaller item than national defense in 1971 and a larger item in 1983 (U.S. Bureau of the Census, 1983a:343). The total federal expenditure on all the major child-oriented programs—AFDC, Head Start, food stamps, child nutrition, child health, and all federal aid to education—is about \$36 billion in 1984, only one-sixth of federal expenditure on the elderly (compiled from Children's Defense Fund, 1984b, Appendices). Per child, federal expenditure on these programs was only 9 percent of per capita expenditure on the elderly. Trends in state and local spending do not appear to have offset the age trends in federal spending (American Federation of State, County, and Municipal Employees, 1984).

The recent changes in public expenditure patterns are not simply some aberrant product of the Reagan administration. The cutback in children's programs began under Carter and has had Congressional support and the support of the American voting public.⁴ Reagan himself proposed large cuts in Social Security benefits in 1981 and 1982 which were soundly defeated in Congress (Chin, 1983). Research funding is a microcosm of national trends. The National Institute of Education's (NIE's) budget in fiscal year 1981 was \$65.6 million, close to that of the National Institute of Aging's (NIA's) \$75.6 million. In fiscal year 1984, the Education budget is down to \$48.2 million, while Aging has risen to \$112.3 million. The Administration's proposal for 1984 called for a cut of at

least 10 percent in extramural funding of NIA but this was overturned by Congress, which provided a 25 percent increase in funds. The Administration's proposed reduction for NIE was accepted (Consortium of Social Science Associations, 1983).

These disparate trends in levels of public expenditure are exaggerated by disparate trends in the apparent effectiveness of public expenditure. The largest portion of public expenditure directed towards children takes the form of public schooling. Reports from commission after commission in the past year have concluded that the quality of our educational products has eroded. The best publicized indicator is the decline in Scholastic Aptitude Test scores. The sum of verbal and mathematics scores declined by 90 points between 1963 and 1980 and seems to have leveled off subsequently (Lerner, 1983). The most authoritative examination of this trend, the Wirtz Commission report of 1977, concluded that most of the decline in earlier years was attributable to compositional factors—different groups taking the test—and most of the decline in later years was real (Advisory Panel on the Scholastic Aptitude Test Score Decline, 1977). Over the period they reviewed, slightly more than half of the decline was real. As evidence, they point to sharp declines since 1970 among all major groups, including high school valedictorians. They also conclude that the decline has been underestimated by 8 to 12 points because the tests have gotten easier. Trends in scores on most other standardized tests, achievement as well as aptitude, are also typically downwards, especially in science and math, and especially among high school students (National Assessment of Educational Progress, 1978, 1981; Lerner, 1983).

At the same time that school performance has been declining, a smaller fraction of children are completing high school. The high school graduation rate dropped from 76.3 percent in 1965 to

73.6 percent in 1980 (U.S. House of Representatives, 1983:22). In contrast, 95 percent of Japanese teenagers now graduate from high school, and because of a longer school day and school years the Japanese graduate will have spent roughly four full years more in school than will an American graduate (Task Force on Education for Economic Growth, 1983). One analyst cited by the National Commission on Excellence in Education (1983:11) asserted that, "For the first time in the history of our country, the educational skills of one generation will not surpass, will not equal, will not even approach those of their parents."

While education is the principal public service provided children, health care is the principal public service provided to the elderly. For other ages, of course, it is not a public service but is primarily privately arranged. However, for the elderly, 69 percent of medical care bills are paid with public monies. The total amount of public outlay for health in 1984 per person above 64 is estimated by the Congressional Budget Office to be \$2,948 (Congress of the United States, 1983:19-20).

One indicator of the success of these expenditures is mortality rates. We have already heard evidence that old age mortality has fallen dramatically. But so, too, has mortality fallen among children, one of the few benign trends for the group. In order to compare the gains of the groups, we need a proper measuring rod. Every well-trained demographer knows that we have such a device in the form of model life tables, which indicate how much change typically occurs in age-specific death rates per unit change in life expectancy at birth. So we can examine recent mortality change in the United States to see how much improvement is implied at each age, using the "West" model (Coale et al., 1983). Figure 3 presents the results for female changes between 1968 and 1980.⁵ If recent changes had been "normal"—that

is, in accord with commonly-observed relationships among age-specific death rates—then the graph would be a horizontal line. All ages would have moved up by the same amount. Obviously, the line is not horizontal. Compared to normal standards of progress, children and young adults improved the least. The four largest gains pertain to the four age groups above 65. (The figure at age 80 should not be taken too seriously since mortality has fallen to such a low level here that it is far outside the range of the models used and extrapolation was necessary). What I find particularly intriguing about the graph is the suggestion of a discontinuity at age 65, the age at which Medicare entitlements begin. Male changes are similar, being smallest among children and young adults and reaching a peak at age 70-75. There is no suggestion of a discontinuity at age 65, however.

The two age groups of dependents even show different trends in their degree of dependency. Older children are more often contributing to their own support by working, while the elderly are contributing less often. Labor force participation rates of persons 16-17 years old rose from 37.9 percent in 1960 to 43.2 percent in 1983, while the participation rate of those 65-69 declined from 32.3 percent to 20.4 percent (U.S. Bureau of Labor Statistics, 1984; Miller, 1984).

Having presented some evidence on the changes in the well-being of our dependents, I'm now going to argue that demographic variables have played an important role in producing these changes through their action in three arenas, which I've labelled the family, politics, and industry.

THE FAMILY

Societies use two major means for transferring resources to dependents: direct public transfers and transfers within the family. The latter is the most important means in virtually all societies. James Morgan (1978) estimates that

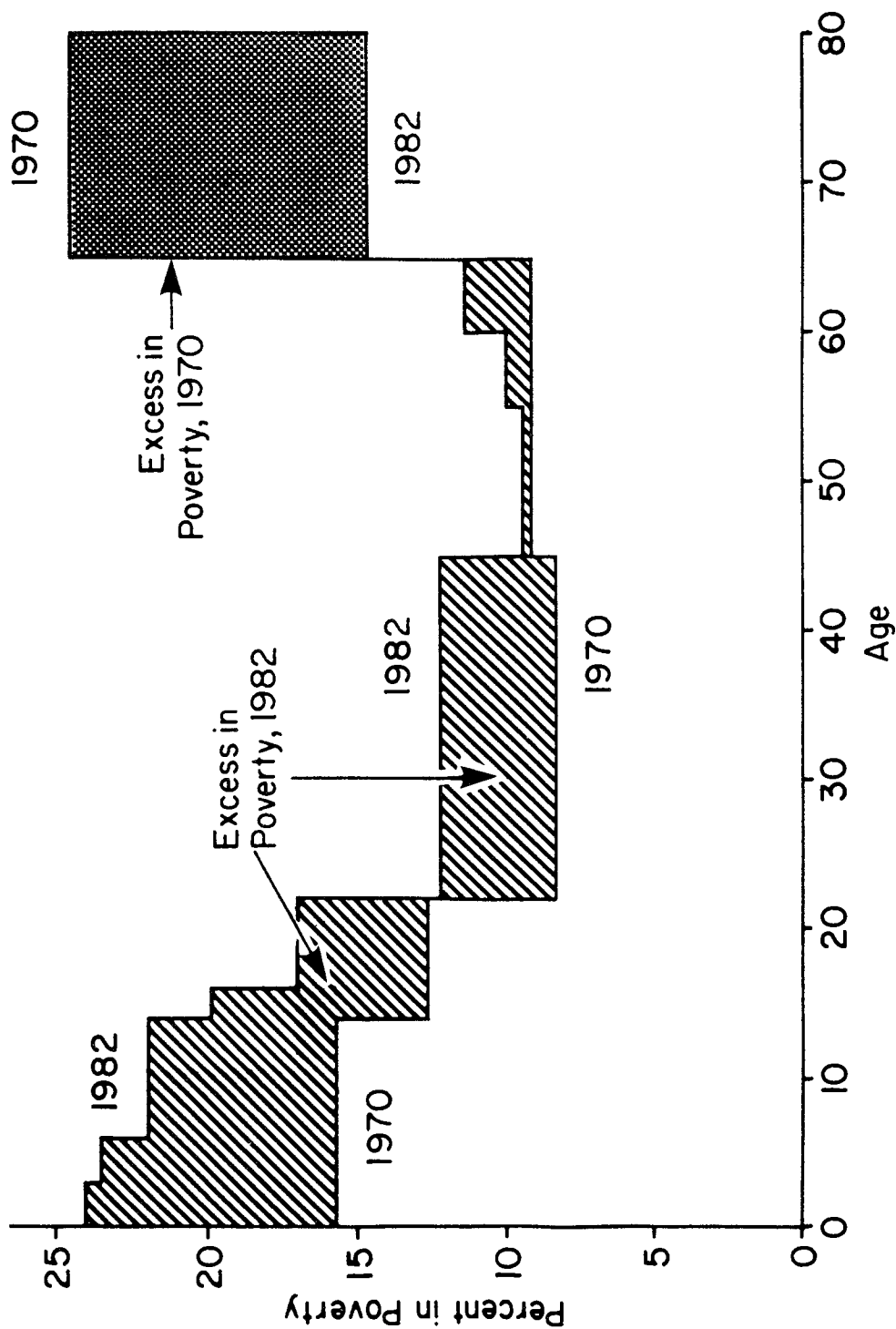


Figure 3.—Mortality Improvements by Age between 1968 and 1980 for U.S. Females.

roughly one-third of GNP in the United States takes the form of transfers from income earners to nonearners in the same coresident family. Over time, families have relinquished more and more responsibility for support of elderly dependents to the state. In terms of residence and income, this process was nearly completed by 1960. As long ago as 1942, Parsons attributed the financial difficulties of the elderly and the political agitation on their behalf to a disappearance of a sense of obligation for their support within the conjugal family.

The situation is clearly very different for children, for whom the family remains the principal source of support. But it's not too far-fetched to argue that during the period since 1960 the conjugal family has begun to divest itself of care for children in much the same way that it did earlier for the elderly. The simplest form of divestiture is not to have children in the first place, and we're doing that in record numbers. But there is also less care-taking for the children that we do have. The main proximate cause here is a disappearing act by fathers. 18.4 percent of births in 1980 were out of wedlock, which in the large majority of cases means that the father takes no enduring responsibility for the child. The figure was only 5.3 percent in 1960 (U.S. Bureau of the Census, 1983a:70; Furstenberg and Talvite, 1980). Of those children both *in* wedlock, according to Bumpass (1984), 43 percent would experience a disruption leading to the divorce of their parents before age 16 under disruption rates of 1977–1979, compared to only 22 percent in 1963–1965. Hofferth (1983) extrapolates recent disruption trends and projects that two-thirds of *in*-wedlock births in 1980 will experience the disruption of their parents' marriage by the time the children reach age 17.

What happens to the father after the divorce? According to Furstenberg and Nord (1982), 52 percent of children with a nonresidential father had not seen him in the past year and an additional 16

percent had seen him less than once per month. Fewer than half of the fathers made child support payments. According to a Census Bureau study for 1978, only 41.4 percent of children from a previous marriage living with the mother received child support payments from their father. A later survey of fathers found them reporting child support payments to about the same number of children, but failing to report the existence of the large majority of the children to whom they were not making payments (Cherlin et al., 1983).

Some of the children abandoned by their natural fathers will of course come to live with other adult males who support them, but this does not happen as often as commonly believed. Bane and Ellwood (1984) use Michigan's Panel Study of Income Dynamics data for 1968–1979 to show that 63 percent of children who enter one-parent spells in childhood are still in them at age 17. The rate of breakup after remarriage is actually higher than the rate of remarriage itself when calculations are based upon child-years of exposure.

The upshot is that economic circumstances usually deteriorate for women and children following divorce and separation. Using data from the Panel Study of Income Dynamics, Duncan and Morgan (1981) found that children whose parents divorced between 1972 and 1978 had a loss of \$6,602 in annual family income between those years; 72 percent of these children had a reduction in the ratio of income to needs. A study of an earlier seven-year period in this data set found that the ratio of income to needs rose 30 percent for men who became divorced or separated and still remained in that state but declined by 7 percent for the women in this category (Hoffman and Holmes, 1976; for a recent review, see Hill, 1983.)

Obviously, these disruption patterns have something to do with the rise in child poverty. The Census Bureau figures on poverty show that 56.0 percent

of children under 19 in 1982 who lived in a female-headed family without husband present lived in poverty, compared to only 13.0 percent of children in other families. The figures were similar but slightly lower in 1970. On a simple decompositional basis, 44 percent of the growth in the percentage of children in poverty between these years is attributable to the growing prevalence of female-headed households. In terms of absolute numbers, 69 percent of the growth in the number of children in poverty occurred in the category of female-headed families.⁶

The rise in marital disruption is also likely to be implicated in deteriorating psychological well-being among children, as we saw earlier. There is certainly little doubt from microlevel studies that the short-run effects are sizeable and significant (Hetherington, 1979; Hetherington et al., 1979; Kellam et al., 1977) and there is mounting evidence of substantial long-term effects, including increases in young adult suicide (Furstenberg and Allison, 1984; Furstenberg and Seltzer, 1983; Fuchs, 1983).

Family instability is also likely to be related to a minor extent to declining school achievement. A recent review of studies by Hetherington et al. (1983) concludes that both short-term and long-term deficits are associated with living in a one-parent household as a child, even after controlling the income effects that are partly produced by disruption itself. They suggest that behavioral problems are a major intervening variable (see also McLanahan, 1983).

It's tempting to push the level of explanation one step farther and try to account for the decline in marital stability itself. That is a subject better suited to encyclopedic treatment than to paragraphs in a presidential address, but I can't resist making some remarks. Some 52 percent of marriages would end in divorce according to disruption rates of 1975-80 (Preston, 1983) and Kingsley Davis (1983) points out that the figure would be much higher if we counted

consensual unions, some of which are reproductive. It seems incredible that we have reached this level of instability when collectively we have better health, more teeth, better odor and more orgasms. And a recent review of public opinion pools by Thornton and Freedman (1983) concludes that we are, in fact, happier with our mates of the moment. The percentage of persons saying that their marriage was either very happy or above average went from 68 percent in 1957 to 80 percent in 1976.

I think that explanations for rising instability occur at two levels. At one level are increased incentives to break up a union. We have already seen that the male's disposable income rises sharply after divorce, and his gains from divorce are greater the higher are general economic levels. Beckerians stress that higher potential earnings for women have made it more costly for them to specialize in home production and to build up marriage-specific capital, especially in the form of children. Higher incomes have also made it easier for both partners to sacrifice the scale economies attached to joint living arrangements. In effect, we can afford to buy more privacy and freedom from others' needs and expectations by establishing separate residences. Low income groups have gained incentives to split up because of expanded public programs—especially Aid to Families with Dependent Children—that penalize couples for staying together. State levels of AFDC payments have repeatedly been shown to influence disruption rates and even remarriage rates (Hutchens, 1979; Hoffman and Holmes, 1976), and evaluations of the guaranteed minimum income experiments reached similar conclusions (Hannan et al., 1977). Finally, exposure to alternative partners has increased with industrial changes that produce less sex segregation at the workplace, larger percentages in metropolitan areas, and more jobs dealing with people instead of dirt or machines.

Viewed in this light and considering

only adult welfare, there is nothing problematic in the upsurge in marital disruption. We simply have greater opportunities now to act on our preferences. The lack of marriage mobility in the past is equivalent to the absence of occupational mobility in a feudal society.

These structural explanations are surely part of the story. But another part, perhaps more important, is the increased prevalence of a world view that legitimizes calculations based upon individual self-interest. Lawrence Stone, the leading family historian, refers to the rise over the past 250 years of affective individualism, the awareness of the self as unique and recognition of the right of that self to pursue selfish goals (Stone, 1982). He argues that individualism is a rare and curious ideology in human history, produced largely by the Protestant revolution and taking its most potent form when nourished by American democracy. Lesthaeghe (1983) provides strong support for the notion that ideas are playing an independent role in family change by showing that changes in marriage and divorce throughout Europe are influenced by the degree of religious secularization, independent of levels of urbanization and industrialization. One might add that it seems unlikely that any purely structural approach could explain why American divorce rates are about double those of our nearest competitors in Europe (United Nations, 1984:692-695).

Within the United States, the independent role of ideational systems for family matters is probably best represented by behavior among Mormons. The crude birth rate in Utah rose from 25.5 in 1970 to 30.1 in 1979 while the nation's was declining from 18.4 to 15.9 (Toney et al., 1983). Its TFR in 1980 was 3.22 (Population Reference Bureau, 1984:8). Utah also has (after Hawaii) the lowest divorce rate in the Pacific and Mountain region (U.S. National Center for Health Statistics, 1984a). But these rates are not a product of social and economic backwardness; Utah also has the highest edu-

cational level in the country and one of the lowest high school dropout rates (Toney et al., 1983). Obviously, we are not simply maximizing some utility function that is shared by the human community throughout time and space.

What these individualistic notions have meant for divorce is obvious. People feel less constrained by others' welfare to remain in what they consider to be a marginal marriage. Opinion polls have asked women whether they agree with the statement, "When there are children in the family, parents should stay together even if they don't get along." The percentage disagreeing with the statement rose from 51 percent in 1962 to 82 percent in 1980 (Thornton and Freedman, 1983:9). Our tendency to count only our own interests and not those of children is vividly illustrated by a recent incident in New York City, which passed a law requiring bars to warn of the dangers to the fetus of alcohol consumption by pregnant women. The local chapter of the National Organization of Women wrote a letter to Mayor Koch protesting that the bill was "protecting the unborn at the expense of women's freedom" (Sandmaier, 1983).

These two tendencies—increased incentives to divorce and increased willingness to act on those incentives in a narrowly self-interested way—are surely together responsible for the sharp rise in divorce. And there is no question that this rise has in turn made life more difficult for children, while its impact on the elderly has been muted by their prior disengagement from the conjugal family.

POLITICS

Besides the family, the state is the other major vehicle for transferring resources to dependents. Here it seems fairly obvious that the changing numbers of young and old have altered the environment for public policy decisions. In a modern democracy, public decisions are obviously influenced by the power of special interest groups, and that power is in turn a function of the size of the

groups, the wealth of the groups, and the degree to which that size and wealth can be mobilized for concerted action. In all of these areas, interests of the elderly have gained relative to those of children.

It's useful to recognize that there are three sources of self-interested support for the elderly: the elderly themselves; the working-age population who are in a general sense "voting" on behalf of elderly persons who might otherwise need family support; and the working age population who are voting on behalf of themselves when they reach old age. The elderly are a very peculiar kind of special interest group, quite unlike Teamsters or Southerners or the National Rifle Association. They are a group that almost all of us can confidently expect to belong to someday. Most programs for the elderly are to some extent perceived as a social contract whereby we transfer resources to ourselves over the life cycle.

Only one of these three sources of support is available to children. Children don't vote; and adults don't vote on behalf of their own childhood, which is water over the dam. I daresay that if we passed through life backwards, adults would insist that conditions in childhood be made far more appealing.

So demographic change can clearly have a multiplier effect on political support for the elderly. The sharp mortality decline at old ages has meant more elderly voters; more working-age people with surviving parents; and an increase in the number of years that a working-age person can expect to live over 65.

The most visible and perhaps most important of these changes is the rise in the number of elderly themselves. This rise has been combined with a high degree of political participation. According to a U.S. Census Bureau (1983b) study of voting patterns in the 1982 congressional election, the highest percentage voting of any age group occurred at ages 65–74. 65 percent of persons in this group voted, more than double the percentage at ages 20–29. In terms of absolute numbers, more people voted at ages 60 and

over than in the swollen baby boom cohorts under 35 or in the prime child-rearing ages from 35 to 49. Once again, this age pattern reverses earlier ones: in the congressional elections of 1966, the voter participation rate above age 65 was lower than for any age between 35 and 64 (U.S. Census Bureau, 1968). The elderly also appear to be politically more knowledgeable. In a 1982 Gallup poll, 56 percent of those aged 65 and over could name their Congressional representative, the highest fraction of any age group, compared to only 30 percent among those under 30 (Gallup, 1983a:175).

The constituency for children, meanwhile, has declined both in numbers and in impact. To demonstrate the changing dependency pressures on the middle aged, I estimate from cohort fertility and life tables that the average 40-year-old couple in 1980 had nearly identical numbers of living parents and children: 2.59 parents and 2.72 children.⁷ But we are still far from where we are headed if present rates of fertility and mortality were to persist: under rates of 1980, a 40-year old couple would have 2.88 living parents and 1.78 living children. It is not until age 52 that the numbers would be equal.⁸ By that age, of course, most children are out of the house, and it turns out that there is *no age* in the life cycle at which the couple is expected to have more children below age 20 than it has surviving parents. The pulls and tugs of dependency concerns on the middle aged are obviously shifting, in numerical terms at least, towards the elderly.

It's not just that we have fewer children these days; parents are also less inclined to live with the ones that they've got. In 1982, only 63 percent of children under 18 were living with both of their natural parents (U.S. House of Representatives, 1983). As a result of declining fertility, residential breakups, and an aging population, only 37.1 percent of American households in 1982 contained a child under age 18 (U.S. House of Representatives, 1983). Tabulations

done at the University of Pennsylvania from the 1980 Census Public Use micro-data show that only 41 percent of the population aged 21 and over lives in a household with a child under 18. Equivalent figures for the 1960 and 1900 Public Use Samples are 50 percent and 59 percent.

In addition to declining numbers of parents potentially representing children among the voting-age population, parents themselves are less likely than average to vote. Among family householders with *no* children present under age 18 in the election of November 1982, 60.5 percent voted; among those whose children were all below age 6, only 38.1 percent voted (U.S. Bureau of the Census, 1983b:18). Of the votes cast in this election, only 38.4 percent belonged to people who lived in a household that had a child under 18.⁹

None of this would matter, of course, if people in different age groups and family circumstances saw public issues the same way. And there are many issues on which age differences appear to be minor. They are not even very large regarding social security, perhaps for reasons that I alluded to earlier. A 1982 Gallup poll asked people how they felt the financing crisis of social security could best be resolved. Age differences in responses were not large, although they were systematic and predictable. Elderly persons were 7 points more in favor than others of increasing current contributions from workers and employees, and 13 points more in favor of increasing the age of eligibility for the retirement cohorts to come (Gallup, 1983a).

Larger age differences seem to pertain to issues involving children. The 1983 Gallup Poll of Public Attitudes toward the Public Schools asked whether people would vote to raise taxes for schools if requested to do so by their local school system. Below age 50 the numbers were evenly split: 45 percent would favor the request and 46 percent would oppose it. At 50 and above the opponents outnumbered

the supporters by 62 percent to 28 percent (Gallup, 1983b). In 1978, HUD commissioned a large Harris poll of 7,074 adults regarding the quality of community life. Respondents were asked to name the public service that they would most like to see improved. 20 percent of those 25–44 but only 6 percent of the elderly named the public schools. People were also asked whether the lack of child care facilities was a problem in the community. 50.0 percent of those aged 25–34 said that it was, compared to only 19.7 percent of the elderly (U.S. Department of Housing and Urban Development, n.d.:255,713–715). While it's clear from other data that the elderly are in frequent touch with their children and grandchildren, it's also clear that they don't automatically assimilate their offspring's perceptions and concerns.

How many issues at the local or national level have turned on the changing age and family status distribution? Unfortunately, this is not a question that admits to simple answers. One possibly informative example is the passage of the Age Discrimination Act of 1975. A very detailed article in the *Yale Law Journal* (Schuck, 1979) documents how little careful thought went into this Act because of legislators' rush to please their powerful elderly constituents. It argues that the Act intensifies age conflict and has been interpreted by the Civil Rights Commission in such a way that the elderly gained not only at the expense of other adults but also of children.

We have talked only about the exercise of self-interest. What about altruistic motives for support of children and the elderly? These are obviously difficult to measure both in intensity and effect. A recent book by Grubb and Lazerson (1982) argues that we have drifted towards a purely self-interested and adversarial form of government and lost along the way notions of community good. Enlightened self-interest has simply become *self* interest—looking out for number one—with particularly devastating effects for children. They argue that

Americans have never had any strong sense of collective responsibility for other people's children, only private responsibility for their own. One suspects that this distinction has been strengthened by the increased availability of effective contraception, so that children are more than ever viewed as the product of a private decision. Without any sense of collective good, the obvious question is why parents shouldn't bear the costs of that voluntary decision. Since we don't choose to have parents, there is no equivalent motive to privatize their costs. A second factor probably helping to blunt any outbreak of altruistic behavior towards other people's children is that they are increasingly drawn from minorities with whom the majority may have trouble identifying. 23.6 percent of children under age 15 are black or Spanish-origin whites, compared to 16.4 percent of persons aged 35-44 and only 10.8 percent of the elderly.¹⁰ Finally, there is the very real concern that whatever public actions are taken may undermine the remaining capacity of the private family to provide for its children, might make matters worse instead of better. But while this argument may have some pertinence to AFDC programs, it seems irrelevant to public schooling, which is already overwhelmingly a public responsibility. And the "moral hazards" arguments simply appear to lack social saliency for the elderly. We appear to worry very little about whether increased benefits for them would undermine their children's willingness to care for them or lead the able-bodied to withdraw prematurely from the labor force (Bane et al., 1983). But we're scandalized by equivalent prospects for those we think of as welfare mothers.

INDUSTRY

The final and least obvious of the demographic mechanisms helping to bring about the trends described is the effect of demographic change on the ma-

jor industries serving the two age groups. Education and health are two of the largest industries in the United States. Education, of course, serves primarily the young and health services are disproportionately directed towards the old. The Congressional Budget Office estimates that an elderly person will spend an average of \$4,680 on health in 1984 (Congress of the United States, 1983:19). This comes to a total of \$131 billion, about 4 percent of GNP.

We have already seen that the quality of products of our educational system is deteriorating and that the system is serving a somewhat smaller fraction of youth. The question is whether this deterioration is in any sense a product of the declining numbers in school. Public elementary school enrollments declined by 11 percent between 1972-1973 and 1982-1983, and secondary enrollments by 18 percent (Feistritz, 1983: Tables 7 and 8). I believe that a persuasive case can be made that these two trends are linked.

At first blush, the evidence is all to the contrary. Expenditure per pupil in real dollars increased by 22.5 percent between 1972-1973 and 1982-1983, faster than the growth of per capita personal income. The average number of students per teacher declined from 22 to 18. The average experience level of teachers increased and a much higher fraction had Master's degrees (Feistritz, 1983: Tables 13, 15, 34). So the quantitative indicators are favorable.

The only problem is that none of these variables has been shown to be related to student performance. Eric Hanushek (1981) has recently published a masterful review article of 130 studies of factors affecting children's performance in schools. He concludes that the only reasonably consistent finding is that smarter teachers do better in terms of evoking student achievement. Teacher effects are very large, although it's hard to say what characteristics—apart from being smart—those effects represent. Another recent review by Murnane (1981) also

suggests that intellectual skills of teachers are the most vital element in student performance. Both reviews conclude that physical resources, expenditures, and class size are immaterial.

So these studies focus our attention on the conditions of public school teachers. One might expect teachers to have shared in the rising pattern of school expenditure. But in fact during the 1973–1983 period, teachers' salaries declined from 49 percent of school expenses to only 38 percent of school expenses. Real incomes of teachers dropped by 12.2 percent during the period. Starting salaries grew more slowly than in 8 out of 9 other large fields with which teaching is routinely compared (Feistritzer, 1983:50,73). The expenditure gap is explained by higher maintenance costs for aging buildings, higher administrative costs and higher energy costs.

Teachers have been faced not only with declining real income in the here and now but also attachment to a declining industry, so that their future earnings prospects are also diminished. These trends are surely implicated in an appalling deterioration in the quality of teachers entering the profession and a rapid outflow from the profession of those best qualified.

You already know of the decline in SAT scores nationally. The decline in SAT scores for those intending to major in education has been even faster. In 1973, education majors scored an average of 59 points below the mean on the combined SAT. By 1982, they scored 80 points lower. The average SAT score in 1982 for those intending to major in education was 394 in verbal and 419 in math (Feistritzer, 1983:88–90). This highly negative selection into the profession has been accentuated by negative selection of those who remain after entering. The 1972 National Longitudinal Survey of high school seniors enables us to compare people who left the profession to those who remained. The mean SAT score is 42 points lower for those

who stayed in teaching than for those who entered and left. Altogether, continuing teachers had SAT scores that were an average of 118 points below those in the cohort who never taught (Vance and Schlechty, 1982:Tables 22–23).

The most obvious interpretation of what's been happening with regard to the teaching profession is that the demand for teachers shifted downwards because of the declining schooled population. This shift led to a lower wage for teachers, which induced a disproportionate number of the better teachers to leave the field or to avoid it altogether.¹¹ It seems likely that this tendency was reinforced by the behavior of teachers' unions, which a Rand study shows to have become increasingly concerned with issues of reduction in force and maximum class size during the 1970s as enrollments plummeted (McDonnell and Pascal, 1979:vi). Some salary increases may have been bargained away for job protection, which is a greater boon to poorer teachers with fewer opportunities elsewhere. It's also likely that greater opportunities for women in other sectors are implicated in the decline in teacher quality, especially since this decline is larger for female entrants to the profession than for male (U.S. National Center for Education Statistics, 1983:222).

If demographic factors are pertinent to teachers' salaries, then this effect ought to appear in state-level data. I've estimated simple OLS regressions to predict the change in average teacher salaries, by state, between 1972–1973 and 1982–1983. The most important factor of those examined is the growth rate in per capita income in the state, with an elasticity of about .4. The growth rate in enrollment has a positive sign in weighted and unweighted regressions and an elasticity of about .12. That is, a decline of 10 percent in enrollment is associated with about a 1.2 percent fall in teacher's salary. The coefficients are larger than their standard

error, but are insignificant. The fact that they are positive and sizeable, however, is quite consistent with an interpretation of the kind that I've offered, as well as with the time series data. It is inconsistent with the Malthusian notion that school districts would translate funds liberated by falling enrollment into a search for better teachers. Quite the opposite effect seems to be working; demographic decline seems to have led to a deterioration in the salaries of teachers, which is surely implicated in their declining quality. It is interesting to note that the growth rate of the proportion over age 65 is negatively associated with the growth rate of teachers' salaries by state, with an elasticity of about $-.25$. States where the elderly have grown more rapidly have had larger declines in teacher salaries. Finally, states where local school districts finance a larger fraction of school expenses have had larger declines in teacher salaries.¹²

While numbers of teachers have been stagnant and salaries and quality declining, quite the opposite trends have been evident in the medical profession. Applicants to American medical schools are so outstanding that choosing among them has been described as a lottery system; even those who don't make it are so talented that we launch foreign invasions to ensure their safety. Tremendous amounts of capital have flowed into the health care industry in the past decade, to be converted into equipment and personnel who embody a never-ending stream of technical advances.¹³ There can be little doubt that the growth in demand for health care services, both in terms of numbers of persons in the ages of prime use and of entitlements that were negotiated from demographic strength, have helped to produce this bloom of health for the health care industry. In turn, the health care successes have helped to generate more health care consumers by reducing mortality, a classic case of supply creating its own demand.

In short, it appears that the predominant industrial response to demographic change has been anything but classically Malthusian. The group with faster growth has been far better served by their specialized industry than the group with declining numbers. The scenario in our schools is not very different from that in certain smokestack industries, except that here the demand reduction has a demographic origin and the product is, for better or worse, the human capital of the next generation.

DISCUSSION

It is not my intention to paint the elderly as the villains of the piece. By prevailing standards, their motives and behavior are certainly no less pure than those of other groups. Their principal role here is instead that of a comparison group, the second of two dependent groups among whom demographic trends have been radically different. Feeble as it is to be dealing with an N of 2, it would be more than twice as bad to have an N of 1. But one can't simply stand on grounds of scientism and wish away the possibility that there is direct competition between the two groups. Indeed, the self-evident public resistance to higher levels of taxation and public expenditure suggests that, in the public sphere at least, gains for one group come partly at the expense of another.

The set of relationships I am proposing might on the surface appear to be exactly the opposite of those proposed by Easterlin (1980). They are not. Easterlin's arguments emphasize above all the manner in which private labor markets react to a cohort of unusual size. I am emphasizing primarily how transfers, either public or private, are related to cohort size for exactly those stages where the cohorts are out of the labor force. The arguments are quite compatible. Taken together, they suggest that the larger the role of transfers relative to earnings, and in particular the larger the role of govern-

ment in the economy, the more advantageous it may be to live in a large cohort.

I have emphasized age to the almost total exclusion of sex, race, and other traditional demographic variables. How, you might ask, can we talk about the neglect of children without mentioning their abandonment by mothers heading into the labor market? The answer is that it's not at all clear that mother's work is a source of disadvantage for children, at least not as a direct determinant. Recent reviews of studies of the effect of working mothers on child development find very few and inconsistent effects, far less clear-cut than those associated with marital disruption (D'Amico et al., 1983; Heynes, 1982). Furthermore, it's obvious that women's work has become a very important contributor to children's living standards, and is the core source of support for the large numbers of children not living with their fathers. But it does seem likely, as I noted earlier, that increased earnings prospects for women have facilitated marital disruption. But so have improved opportunities for men.

With regard to race, let me just say that the main theme here is the changing status of American children, a group that includes all races. I see no particular reason for separating out the races any more than for carrying through a distinction between Northerners and Southerners or other commonly used identifiers. For those who prefer to think of the problems in childhood as being confined to the black population—a group who for most of us constitute “other people's children”—let me just say that there is not a single trend that I've talked about that does not pertain to *both* races. Indeed, for some—declining school achievement and rising illegitimacy, for example—changes have been much faster for the white population.

SUMMARY AND CONCLUSION

Let me summarize briefly. My argument is that we have made a set of

private and public choices that have dramatically altered the age profile of well-being. These choices are in an important sense joint ones involving the number of dependents we have as well as the conditions in which they live. This jointness derives from several sources. One is that the same institution—the conjugal family—remains the principal agent responsible for both childbearing and childrearing. Factors that influence the health of that institution invariably affect both numbers of and conditions for children. There was simply no way to protect children fully from the earthquake that shuddered through the American family in the past 20 years. The factors at work here are not only the objective conditions we face but also the set of values and mental constructs we elect to face them with. At the other end of the age scale, we can obviously affect the number of elderly persons as well as their circumstances by altering health programs, as we have so decisively chosen to do. A final source of jointness is that numbers themselves affect conditions. Some of these effects are largely inadvertent, as I've argued in regard to public schooling, and others seem to be very deliberate outcomes of the political process.

It's useful to step back and ask whether the mixture of numbers and conditions that we've chosen is the one that best serves us. In regard to redistributions from the working-age population to the elderly, the answer is far from obvious. There is surely something to be said for a system in which things get better as we pass through life rather than worse. The great levelling off of age curves of psychological distress, suicide and income in the past two decades might simply reflect the fact that we have decided in some fundamental sense that we don't want to face futures that become continually bleaker. But let's be clear that the transfers from the working-age population to the elderly are also transfers away from children, since the working ages

bear far more responsibility for child-rearing than do the elderly. And let's also recognize that the sums involved are huge. Just the increase in federal expenditures on the elderly between 1977 and 1983, if distributed among the population under age 15, would come to well over \$2,000 per child. The increase in annual benefits for the elderly during this six-year period is almost exactly equal to the total amount of additional annual earnings generated by increased female labor force participation over the entire period from 1960 to 1981.¹⁴

While the redistribution toward the elderly is clearly a decision that a free society should be able to make, the redistributions away from children seem to be less defensible. There is no generally accepted rule in welfare economics for how children's interests ought to be represented in public decisions (d'Arge et al., 1982; Nerlove, 1974; Nerlove et al., 1984). A convenient starting place for work in the area is the assumption that each decision-making adult has children whose utility is folded into the adult's own utility function. That's a very different world from the one that we live in. When only 38 percent of voters are living with a child, the utility that we derive from other people's children would seem to be a far more salient concern than the utility that we derive from our own.

But there is more than consumption value involved. It's clear that public expenditure on children has a different character than expenditure on the elderly. Expenditure on the elderly is almost exclusively consumption expenditure, in the sense that it does not appreciably affect the future productive capacity of the economy. Most types of expenditure on children are both consumption and investment, a logic explicitly recognized in the first school law in America passed by the Massachusetts Bay Colony in 1642, "taking into consideration the great neglect of many parents in bringing up their children in learning which may

be profitable to the commonwealth" (Commager, 1983). The reorientation towards the elderly is thus consistent with the declining share of GNP that is represented by savings and with dramatically rising debt service burdens on future generations.

It seems to me that we are continually faced with two questions. First, do we care about our collective future—the commonwealth—or only about our individual futures? If only our individual futures matter, then our concerns will naturally focus on ourselves as older persons and we will continue down the road we appear to be on.¹⁵ But if we have collective concerns, we face a second and even more difficult decision about what mix of private and public childrearing responsibilities will best serve the needs of future generations. Rather than following the elderly model, at the moment we are attempting to return more and more of these responsibilities to the family. But in view of the manifest erosion in the family's ability to shoulder these responsibilities, this attempt appears to be more an answer to the first question—do we care?—than to the second—how best to proceed? The constituency for children in public decisions simply appears too feeble to fight back. In short, we may be returning responsibilities to families not because they are so strong but because they are so weak.

NOTES

¹ The number of children aged 0–19 in 1915 was 30.6 percent larger than the number of children 0–19 in 1895. These are the cohorts that are aged 64–84 in 1980 and 1960, respectively. (U.S. Bureau of the Census, 1975a:15.)

² Sources: U.S. Bureau of the Census. *Money Income and Poverty Status of Families and Persons in the United States: 1982*. Current Population Reports P-60, No. 140, July 1983; *Characteristics of the Low Income Population 1971*. Current Population Reports P-60, No. 86, December 1972. The data plotted on Figure 1 are:

Age	1970 Percent in Poverty
0-13	15.6
14-21	12.6
22-44	8.3
45-64	9.1
65+	24.6
All ages	12.6

Age	1982 Percent in Poverty
0-2	24.1
3-5	23.5
6-13	21.9
14-15	19.8
16-21	17.0
22-24	12.3
45-54	9.4
55-59	9.9
60-64	11.3
65+	14.6
All ages	15.0

³ Sources: U.S. Department of Health Education and Welfare. Public Health Service. Vital Statistics of the United States, Vol. II, Part A, 1960 and 1961; U.S. National Center for Health Statistics. Monthly Vital Statistics Reports. Vol. 31(13), October 5, 1983. The data plotted on Figure 2 are

Age group	Death Rate from Suicide per 100,000	
	1960-61	1981-82
5-14	0.25	0.50
15-24	5.15	12.50
25-34	10.15	15.85
35-44	14.30	15.60
45-54	20.50	15.90
55-64	23.40	15.25
65-74	22.50	16.80
75-84	26.95	19.25
85+	25.45	18.95

⁴ See especially Grubb and Lazerson (1982, p. 109). A New York Times poll in 1978 found very strong national support for reductions in property taxes, with "welfare and social services" the overwhelmingly preferred targets for service reductions. Respondents consistently overestimated the welfare cost component of their locality's expenses. Poll Tax Cuts Are Widely Backed Around Nation. New York Times: June 28, 1978, p. 1.

⁵ Sources of mortality data: U.S. National Center for Health Statistics. Vital Statistics of the

United States. 1968. Vol. II, Section 5. Life Tables. U.S. Government Printing Office Washington, D.C.; U.S. National Center for Health Statistics. Monthly Vital Statistics Report: 32. Supplement. August, 1983.

⁶ Data are drawn from U.S. Bureau of the Census, Money Income and Poverty Status of Families and Persons in the United States: 1982. Current Population Reports. P-60, No. 140, 1983. Table 17. The percentage of children in poverty rose from 14.9 percent to 21.3 percent between 1970 and 1982. This increase is decomposed by the conventional formula that weights changes in within-category poverty (where the categories are female-headed families and others) by the mean proportion in the category in the two years. Likewise, the change in proportions in the categories is weighted by the mean prevalence of poverty in the category. The absolute number of children in poverty increased by 2.904 million; the increase in the number of children in poverty in female-headed families was 2.007 million.

⁷ To estimate the number of living parents, we begin with the age distribution of fathers and mothers at the birth of their children in 1940 (Grove and Hetzel, 1968). These parents are then survived forward to 1980, one decade at a time. For each decade, mean survival rates from U.S. life tables at the beginning and end of the decade are used (Grove and Hetzel, 1968; U.S. National Center for Health Statistics, n.d.; 1983b). The procedure results in 1.54 living mothers and 1.06 living fathers for the two 40-year-old persons. The procedure assumes that there is no relationship between parents' mortality and their number of surviving offspring.

To estimate the number of children for the couple, we use the number of children ever born to women aged 35-44 in "married couple families" in 1980 (U.S. Bureau of Census, 1982b:59). These children are then survived forward 12.5 years from birth by the U.S. life table of 1970 (U.S. National Center for Health Statistics, n.d.).

⁸ These figures are derived from a stable population corresponding to age-specific fertility and mortality rates of 1980 for males and females. The growth rate of the stable population (-.0050) is derived from female fertility and mortality. Age distributions of parents at childbirth are computed separately for males and females, and life tables are separately applied for fathers and mothers. All persons are assumed to be a member of a "couple" with a same-age spouse. The number of living children is that of the female member of the couple. National Center for Health Statistics, 1982a, 1983b.

⁹ Tabulations from the U.S. Bureau of the Census November, 1982 Current Population Survey Public Use Tape, performed by Nancy Denton at the University of Pennsylvania.

¹⁰ Source: U.S. Bureau of the Census

(1982a:27). 95 percent of Spanish-origin persons are assumed to be white.

¹¹ This reasoning assumes that school districts are not fully capable of adapting salaries to differences in teacher quality and that higher quality teachers have on average, higher earnings opportunities in other occupations.

¹² The regression equations for 50 states are the following:

Weighted by state population:

$$Y = -.0149 + .1439 X_1 + .4770 X_2 \\ (.0060) (.0964) (.1771) \\ - .2553 X_3 + .0085 X_4 \\ (.1630) (.0548)$$

$$R^2 = .208$$

Unweighted:

$$Y = -.0069 + .1011 X_1 + .4770 X_2 \\ (.0067) (.0876) (.1705) \\ - .2862 X_3 - .2291 X_4 \\ (.1640) (.0713)$$

$$R^2 = .449$$

Where Y = Average annual growth rate of teachers salaries, 1972–1973 to 1982–1983, in 1972 dollars

X_1 = Average annual growth rate of enrollments in public elementary and secondary schools, 1972–1973 to 1982–1983

X_2 = Average annual growth rate of per capita personal income, 1970 to 1981, in 1972 dollars

X_3 = Average annual growth rate of the proportion of population aged 65 and over, 1970–1981

X_4 = percentage (divided by 1000) of total school revenue derived from local sources in 1982–1983 (i.e., sources other than state or federal).

Data are drawn from Feistritzer (1983) and U.S. Bureau of Census (1982a).

¹³ A recent U.S. Office of Technology Assessment report on the biotechnology industry shows the United States to be far ahead of its international competition in this rapidly growing area, in large part because of both private and public funding advantages. The huge stock of health care entitlements amassed in the United States is probably a key underpinning of this growth and helps to explain why this is one of the few industries where American growth remains exceptional. See *Science* 223:Feb. 3, 1984:463.

¹⁴ Federal expenditures on the elderly increased from \$95.7 billion in 1977 to \$217.1 billion in 1983 (U.S. Bureau of the Census, 1982a, 1983a), or by \$121.4 billion. There were 51.4 million children below age 15 in 1982 (U.S. Bureau of the Census, 1983a), for a ratio of \$2,364 per child.

The comparison with female earnings, a theme first suggested by Davis and van den Oever (1981), is done in the following way. Women workers in 1981 earned an average of \$8,300 and there were 51.94 million women working, so that they earned a total of \$431.102 billion (U.S. Bureau of the Cen-

sus, 1983c:189). The female labor force participation rate in 1981 (aged 16+) was 52.1 percent and in 1960, 37.7 percent (U.S. Bureau of the Census, 1982a:377). The withdrawal of women back to their participation rate of 1960 would therefore have cost the economy, as a first order approximation,

$$\frac{14.4}{52.1} (\$431.1 \text{ billion}) = \$119.2 \text{ billion}$$

in 1981. So the increase in elderly benefits, in this crude calculation, exceeds slightly the total amount of added earnings of women over a period more than three times as long.

¹⁵ Children's status may still improve when we realize that our social security system is jeopardized by worker shortages, a realization that dominates population policy concerns in Europe (McIntosh, 1983).

ACKNOWLEDGMENTS

Many people contributed their ideas to this paper. Frank Furstenberg and Susan Watkins deserve special thanks for their insights and encouragement from an early state. Other colleagues whose assistance was instrumental in improving the manuscript are Michael Aiken, Jere Behrman, P. N. Mari Bhat, Gretchen Condran, Nancy Denton, Jill Grigsby, Bob Inman, Jerry Jacobs, Doug Massey, Ann Miller, Bob Pollak, Cindi Posner, Steven Taber, Paul Taubman, and Dan Vining. Bill Butz, Don Hernandez, Paul Smith, and Jerry Jennings were helpful in supplying materials.

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