

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Larry Bumpass PAA President in 1990



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
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LARRY L. BUMPASS

PAA President in 1990 (No. 53). Interview with Jean van der Tak during the PAA annual meeting, Omni Shoreham Hotel, Washington, D.C., March 21, 1991.

CAREER HIGHLIGHTS: Larry Bumpass was born in 1942 in Detroit, Michigan, and grew up in a suburb of Detroit. He obtained all three of his degrees in sociology: the B.A. from Wheaton College in 1963, the M.A. from the University of Michigan in 1965, and the Ph.D. from Michigan in 1968. He was research assistant to Ronald Freedman during his graduate student years at Michigan. In 1967 he joined the Office of Population Research at Princeton, where he worked on the final phase of the Princeton Fertility Study and on the National Fertility Study. Since 1970 he has been at the University of Wisconsin, where he is the Norman B. Ryder Professor of Sociology and is associated with the Center for Demography and Ecology, of which he was director from 1977 to 1980. Among other activities, he has been a fellow at the Economic Commission for Europe in Geneva (1974), chair of the Population and Social Science Study Section of the National Institutes of Health (1978-80), chair of the Population Council's International Research Awards Program on the Determinants of Fertility in Developing Countries (1981-85), on the Board of Overseers of the Michigan Panel Study of Income Dynamics (1984-87), and director of the National Survey of Families and Households (since 1986). He was editor of Demography from 1978 to 1981.

Larry Bumpass has been a leader in the study of U.S. fertility since his graduate student years and now in the area of family demography. He is author of a prodigious number of journal articles on these topics and coauthor or coeditor of the monographs, The Later Years of Childbearing (with Charles Westoff, 1970), Social Demography (with Karl Taeuber and James Sweet, 1978), and the 1980 census monograph, American Families and Households (with James Sweet, 1988).

VDT: How did you first become interested in demography and especially in fertility, which was your first interest? We'll get later onto your current interest in the family.

BUMPASS: I think that's an interesting story in how the NICHD training grants can work. It's a system that many of us now still try to use to advantage, in that I knew very little about population before I went to Michigan. I, in fact, only intended to get a master's degree at Michigan.

VDT: Why did you choose Michigan?

BUMPASS: In my undergraduate years at Wheaton College, I took my junior year at the University of Michigan--in part for financial reasons, because of the cost of a private school--and was excited by what I saw there. I took courses with Robert Cooley Angel, who was about 75 then; he was a close relative of Charles Horton Cooley. I took courses in anthropology and sociology. I had a very good experience in that junior year, so I decided to apply for a master's in sociology. At the time, I thought I wanted to go back to a small undergraduate institution and teach, and from the very limited perspective I had then, getting a master's degree was all it would take to achieve that objective.

The Michigan Population Studies Center did as we still do in recruiting. They went through the files and decided my grades were good enough at Wheaton that they could gamble on me, so they offered me a full fellowship if I would take one population course a semester and work on the project I was assigned to.

VDT: This [1963] was just about two years after the Population Studies Center had been formed [1962]?

BUMPASS: That's correct. Obviously, they had just received these training grant positions.

VDT: From NICHD? Population research hadn't started there yet. [The Center for Population Research at the National Institute of Child Health and Human Development, NICHD, was established in August 1968.]

BUMPASS: I'm not sure of the auspices of that fellowship. Here at Wisconsin not too long after that they [NICHD] had a general medical science training grant, providing similar-type fellowships. There was a time later on when I was a Population Council fellow at Michigan. I believe this first position was a federally funded NIH mechanism of one variety or another. From my perspective at the time, all that meant was groceries on the table, my tuition paid, and I was willing to take a population course a semester.

VDT: You were already married by then?

BUMPASS: I was married; I married after my junior year.

VDT: Was your wife working then?

BUMPASS: She was working at the Institute for Survey Research. Well, eventually she did; at that time, she was working for the School of Nursing.

The short story is that I was excited by what I did and by the people, who were obviously very exciting at Michigan at that time. So I didn't drop off the conveyor belt at the master's level, and I actively became a part of that community, working with Ron Freedman and Lolagene Coombs and taking courses from Amos Hawley, Dudley Duncan, David Goldberg, and a number of others.

VDT: At Michigan, I've heard from several sources, you were a member of what Freedman--to quote him exactly--said was "a very unusual cohort over a couple of years, a cohort that reassembled at Wisconsin," including Jim Sweet, who got his doctorate the same year you did, Robert Hauser, David Featherman, and Doris Slesinger. Norman Ryder, who established the Center for Demography and Ecology at Wisconsin in the early 1960s, said he went for Michigan graduates "because they were the best." Freedman said at one point he counted 17 Michigan PhDs in Wisconsin's sociology department. How do you account for that happenstance? Such an unusual cohort--a large proportion of you became leaders in the field.

BUMPASS: There are two parts to the answer, I think. One is the intellectual excitement and the sense of discovery and the growth of the population field at the time we were there. It was a very unique period. Ron Freedman was just in the early field stages of the Taiwan experiments. Dudley Duncan was greatly excited about path analysis and its application to the Occupational Changes in a Generation Study [resulting in The American Occupational Structure, Duncan and Peter Blau, 1967, and other publications]. Amos Hawley's work on human ecology was captivating to many of us. Certainly, the intellectual climate was a very fertile one.

That having been said, I think the most demographically informed answer is that you get random runs and it was just a stroke of good luck that we all happened to share that together and that so many of us ended up close friends at another institution for most of our productive careers. Up until Dave Featherman went off to New York to be president of the Social Science Research Council, the four of us have run daily along the lakefront--Dave Featherman, Jim Sweet, Bob Hauser and I. Not Doris; there's another story about Doris.

When I got to Michigan, Doris was a few years ahead of me. She took some years out later for family reasons. About my second year of graduate student life at Michigan, I was supervised by Doris on a research project under Ron Freedman, so Doris taught me the ropes. And I took over a position that she had held in managing some questions on the Survey of Consumer Finance that Ron Freedman had added. Then there was a spell when Doris was a student of ours at Wisconsin before she finally finished her Ph.D. Now, of course, Doris has been chair of the Department of Rural Sociology at Wisconsin.

VDT: Ron Freedman had a paper at the PAA meeting in 1988 on the "Michigan Model of Graduate School Training of Demographers." I think you must have repeated this at Wisconsin. He made four points. Students who will eventually become creative leaders in the field are largely self-selected. The center can provide them with technical training and role models to inspire them to go beyond technique, but shouldn't interfere too much. That's the first approach, he said.

The second one is that you had the apprenticeship system, hands-on apprenticeship, so you "hit the ground running," after finishing. You worked with a professor in research--and you just mentioned that Doris was working with you on some research with Ron Freedman. Was that the one where you ended up coauthoring a paper with him on fertility expectations ["Fertility Expectations in the United States: 1962-64"] in 1966 already in Population Index?

BUMPASS: That's right. It was the mechanics of seeing the survey coding done and sort of overseeing the field operation at that level that I learned from Doris. I would produce tables and then Ron would have me write comments on what I saw in them, and then we would have a regular meeting where we would discuss what I saw and whether I was right or wrong about that and what else one ought to see in them.

VDT: You were just a first-year graduate student?

BUMPASS: I think this was by the second year that we were doing this. Much of the first year was spent in the real nuts and bolts of calculating tables on these old Marchant calculators, that would grind away forever to create just ratios, and in actually coding data from the Detroit Area Survey. A lot of real nuts and bolts, how-the-work's-done, in the first year, and then more of this professionalization into how to interpret data beginning, I think, in the second year.

VDT: Ron's third point about the Michigan model was the opportunity for interchange between students and faculty and each other; he said graduate students train each other as much as any. There were the twice-daily coffee breaks and students having desks in the same building as the faculty.

And the fourth thing was that Michigan always stressed, was strongly associated with, the use of survey data in population analysis. You obviously got your training early on. You were with the Detroit Area Study?

BUMPASS: First with the Detroit Area Study--this famous study that Arland Thornton and Deborah Freedman continue to publish out of on a regular basis. These are the same women; I was involved in, I believe, the second wave.

VDT: The callbacks. They were first interviewed in 1961, and reinterviewed by phone three times, and then picked up again in 1977.

BUMPASS: That's right. So the second wave, which would have been 1963, was the wave that I started off my graduate career coding and working on with Lolagene Coombs.

The paper that you mentioned that Ron and I did came out of the other work. There were actually papers from both streams of work, but one of the first papers came out of the questions on expected family size that were added to the Survey of Consumer Finances, done by the Survey Research Center. Again, that was one of those things where it was just my great good luck to be in on the early stages of excitement about some things in the field. This was 1963. That was only eight years removed from the first wave of the Growth of American Families surveys. In fact, the second wave [1960] had just been completed and there was the whole excitement or exploration of what can be learned from asking expectations data about fertility. Which, of course, came out of the great embarrassment of demographers at having missed the baby boom and wanting to find other methods besides extrapolation to try to figure out what's going to happen in the future. It was in that historical context that all this work with expected family size was getting under way and I was able to learn the trade in the midst of this.

VDT: What was your master's thesis, or did you have a thesis? And then your doctoral thesis?

BUMPASS: Michigan has had, and still has, a policy that doesn't require a master's thesis. You participate in the Detroit Area Study; you get hands-on experience with survey research of a kind that I think is absolutely invaluable. I think the survey work I do now in helping to oversee a project [National Survey of Families and Households] with 13,000 face-to-face interviews--none of which, of course, I do--is informed constantly by that experience I had as a student of going door to door . . .

VDT: You did?

BUMPASS: Yes, that was part of the required thing, to actually do the interviews.

VDT: Your DAS wasn't the one that had phonebacks?

BUMPASS: No. My DAS--and this, of course, just happens to be where you are in the system, and I can't tell you who was running it; it escapes me at the moment--but it was concerned with international attitudes. That was the topic of the year. And that generated some very bizarre circumstances, like the interview I tried to conduct with the 75-year-old grandmother from Arkansas who had just arrived in Detroit. She had never read a newspaper, never listened to a news program, and I was trying to interview her on her attitudes toward countries she'd never heard of--while she had a son in the room who had been in the military and knew about these places and he wanted to answer the questions. I had to keep telling him that he was not the chosen respondent; I couldn't talk to him; would he please be quiet. And then I kept recording the "don't knows" for this lady for the remainder of the interview.

And there were others, like one with a nearly deaf man, where I would shout the questions, his wife would repeat after me, shouting them, and then he would answer. So I came to a real empathy for what life was like in the field, from those very limited experiences. I fear our students don't get enough of that kind of really nuts-and-bolts experience with what the process is like.

VDT: So they can be skeptical of the data?

BUMPASS: That's right. And appreciate what it takes to get good data. I learned early on a maxim that's still very much a part of our lives: how terribly difficult it can be to get good reporting on income, because of the enormous personal resistance. One respondent I interviewed in those days was from Eastern Europe and was paranoid about state oversight or intrusion and, all attempts on my part notwithstanding, just simply would not divulge any kind of information on income.

VDT: So that was one of your requirements for the master's. What about your doctoral dissertation?

BUMPASS: My dissertation meets a couple of principles--I guess I shouldn't ascribe them too much to the people whom I've heard articulate them, though I will hold Jim Sweet to one, whom I've often heard say: "The best dissertation is one that's finished."

VDT: I have a son whose dissertation is dragging on forever, so I can appreciate that one!

BUMPASS: Dissertations are obviously a very important part of graduate training, but I think it's very important for students to view them as a serious final graduate exercise, rather than as the work that's going to stand the field on edge. Occasionally, students are either smart enough or lucky enough to do that. But in retrospect, I find my dissertation rather pedestrian and perhaps even misguided, but it was carefully done and in the process I learned a lot of the trade. So it wasn't a waste.

VDT: What was the topic?

BUMPASS: It was on the effect of age at marriage on education differentials in fertility.

VDT: From data from where?

BUMPASS: That's where the great experience came, because I worked with data from all three, at the time, national surveys of family growth--the 1955 and 1960 Growth of American Families surveys and the 1965 National Fertility Study. So I gained a familiarity with all three of those data sets. I learned a lot about managing multiple data sets, and I learned first-hand about sampling theory, because you don't get the same results necessarily . . . There's a lesson there that I guess informs everything that I do yet, in that a finding in any survey is only suggestive. I have a compulsion yet to always look for another source of data and try to get the same answer.

VDT: I noticed that in re-reading some of your publications. You said at one point that replication is often recommended in social science but seldom done. You learned to do that early on. Who was your supervisor on the dissertation--Ron Freedman?

BUMPASS: No, Ron went off to the Center for Advanced Study [in the Behavioral Sciences] in Palo Alto, I believe, in my dissertation year. I had worked with Dave Goldberg on a number of related projects over that middle graduate period, so Dave served as the advisor on my dissertation.

VDT: Did you work with the Duncans? You mentioned that you took courses from Dudley and he was there working on path analysis.

BUMPASS: I took several courses from Dudley. I had the opportunity to interact with him on a regular basis, both because of the way the Michigan system was set up and because he and Beverly would have students to their home on a regular basis.

I also learned an enormous amount from Beverly Duncan. Beverly was one who if you would go in and ask her a question that seemed simple enough on the surface, like, "How do I find out this from census or vital statistics data?", she would then march you off to the appropriate source and spend an hour or two hours, or however long it took, to make sure you understood how to do what it was you were trying to do. While she was at Michigan she never occupied a teaching position--she was a research associate--but she was a serious teacher to a great many of us.

VDT: She took on that role on her own initiative?

BUMPASS: Yes, it was just the kind of person she was. It wasn't an assignment; it wasn't official; it wasn't recognized in any official way. It's just the way it was.

VDT: That's interesting. I've heard a lot about the Duncans being "parents" of so many of you Michigan students. You were never associated with Ron Freedman's work in Taiwan?

BUMPASS: Never with the Taiwanese work, other than sitting in his introductory population class at the time when he spoke with such obvious enthusiasm about the work, as he would be just coming back and Tom Sun, director of the work in Taichung, would be there at the time.

My line of work at that time was solely U.S.-directed and I stayed on that trajectory for about ten years at least. And I'm still obviously primarily in U.S.-based work, but over the last ten years I've had an increasing number of non-U.S.-related activities.

VDT: Yes, there has been your work on Korean fertility, and what you're doing with the Japanese colleague you're meeting this Saturday afternoon.

What took you to Princeton? Now, I must quote Ron Freedman again [from his interview of June 12, 1989]. He said Charlie Westoff asked you to work on the National Fertility Study. That he, Freedman, had offered you a job at Michigan, but the Princeton opportunity came and he said, " `You should go there because there's a different tradition there; it's a great place.' I didn't try to keep him or sway him." But then Ron Freedman told Westoff, "Charlie, we must talk baseball here. You got my prize first baseman. You've got to give me somebody in return." And then Al Hermalin went to Michigan and you went to Princeton.

BUMPASS: I think that's a close reconstruction of history, infused with a little bit of affection on Ron's part, but it's close. I do remember when Ron called me into his office and said that Charlie Westoff--of course, I knew who Charlie Westoff was--had this position and he thought it would be ideal for me.

VDT: You mean Charlie didn't phone you direct?

BUMPASS: No, Charlie did not at first call me up directly, as I recall. Ron said that Charlie had this and he thought it would be a good idea, and from there I think probably I told Ron, yes, I thought I'd be interested in looking at it and then Charlie contacted me.

The interesting thing there is that that was really a watershed decision for me, because I hadn't completely abandoned the aspirations that had taken me to Michigan in the first place, to teach in a small undergraduate institution. I had spent four years learning the trade, and yet you've got to understand that the way I got to Michigan, which I've already explained to you, did not involve my seeking out the most high-powered place to attend. I just stumbled into it because I was going back home to go to graduate school.

VDT: Remind me where Wheaton is.

BUMPASS: Wheaton is in Illinois. It's a very small town; very small school. It's a good school, though a very conservative one. I think even by the time I was finishing my graduate work, I had rather little appreciation of how good a place Michigan was. I appreciated what I'd gotten and I found it exciting, but I had no perspective on it--that's the only point.

VDT: Was that perhaps because the Population Studies Center had only been in existence six or seven years?

BUMPASS: Perhaps so. Of course, in the early days when I first went there, we were in some very ramshackle rooms that were above the Michigan theater. My office had a skylight that leaked water on the desk.

So, while I looked at a couple of other positions, including actually interviewing with Omer Galle at Vanderbilt, my major job decision came down between going off to a small school in California called Westmont to be a teacher or to go to Princeton and be a researcher. I wrestled long and hard over that one, because I hadn't fully resolved those early aspirations. And I resolved it basically in a manner to cover my options, that is, I figured I could go to Princeton and if I didn't like that way of life, I could go back and teach, but if I chose to go teach at this small school, I would have a very hard time ever moving back into research life. So I didn't quite make the decision yet; I just said, "Okay, I'll keep my options open by taking the research opportunity while it's here." And, of course, that turned into one of those flukes that opens career doors.

VDT: Do you think perhaps another watershed was the article that you and Charlie Westoff wrote that appeared in Science [September 18, 1970], "The 'Perfect Contraceptive' Population," in which you pointed out the demographic impact of eliminating unwanted fertility? When I asked Charlie Westoff what he considered--off the top of his head--the leading findings from the National Fertility Studies, he said the extent of unwanted fertility, and he cited that article, on which you got a lot of coverage. He said Fred Jaffe [of the Alan Guttmacher Institute] persuaded you to write it. It came out after you'd gone to Wisconsin.

BUMPASS: I think that was a key set of findings, and I think that it's not so much that we found something that people didn't know before, but rather that we were getting into clear focus something we knew and its implications not only for the time, around 1970, but for the baby boom itself. We began to appreciate how much of the fertility during the baby boom was accidental; how close to replacement fertility during the baby boom would have been had there been effective control. And that sort of set the stage intellectually for appreciating the change that then followed very rapidly with respect to sterilization.

VDT: Which you wrote on about that time [e.g., Harriet B. Presser and Larry L. Bumpass, "Demographic and Social Aspects of Contraceptive Sterilization in the United States: 1965-1970," in Commission on Population Growth and the American Future, Research Reports, Vol.1, 1972].

BUMPASS: Where we went intellectually was to that, recognizing that such a high proportion of the population was having the experience of the intrusion of unplanned pregnancy on their lives. Then the diffusion of the pill was under way at just that point in time, so people were experiencing both contraception that was not coitally related and an expectation of complete fertility control in the context of the preceding failure. That just set the stage for the very rapid turnaround of sterilization. The kind of excitement intellectually I felt about sterilization as a topic--actually, I wrote a piece on it not too long ago, still developing those themes--was that it was a more generic case . . . It was very important in its own right, in terms of what was going on in fertility in the U.S.; it was one of the things that was helping to anchor low fertility not just then but for the future in the U.S. But in some sense more importantly--or at a different level, at least--it was important as social science. It represented a behavior that was feared, around which there was a great mythology, around which there was an enormous amount of disapproval, which turned around very quickly because of its utility. So it represented a more generic kind of process of social change, that was just illustrated with respect to

fertility. When faced with a decision between a lifetime of high risk of unwanted fertility, of continuing the Russian roulette of existing methods, having been introduced to the pill and its efficacy--and yet, many women either unable to take the pill for a long time or just insecure about doing so, not knowing what the long-term health implications were--all of a sudden the mythology and fears surrounding sterilization evaporated, because it was a solution to the problem.

VDT: Harriet Presser, who'd already worked on sterilization in Puerto Rico and had done an overview paper on sterilization internationally for the Population Council, told me how you phoned her just at that point and together you wrote the paper for the Commission on Population Growth and the American Future [cited above]. She put it that, "He had his hands on the data," from the 1965 and 1970 National Fertility Studies. I'm glad you put it in this context of the larger picture.

At Princeton, you also worked on the final phase of the longitudinal Princeton Fertility Study, and you were the senior author of The Later Years of Childbearing [1970]. At that time, you ended up most enthusiastic about the longitudinal aspect of that survey. Charlie Westoff in his interview with me said he is no longer very enthusiastic about longitudinal surveys. What do you feel?

BUMPASS: The most succinct answer to that is that I spent most of the last month writing two proposals to fund a longitudinal survey.

VDT: The carry-on of the National Survey of Families and Households?

BUMPASS: That's correct.

VDT: The followup is going to be longitudinal?

BUMPASS: That's correct. We'll follow the exact same people. We will then know, following the first interview, who entered cohabiting relationships, who married, who divorced, who had their first births, when children left home, who had their parents move in with them, who retired.

VDT: When will you go in the field?

BUMPASS: We would hope to be in the field in 1992; it would be five years from the end of interviews. So, I remain very enthusiastic about longitudinal surveys. They are not a panacea in causal analysis. I'm not ascribing that just to the Princeton study. In that period of time in the early 1960s when that study was under way and through the 1960s, longitudinal surveys became one of these methodological fads that the field gets into, which often overstate the value of something. But then you don't want to throw away the good tool when you appreciate its limitations as well as its strengths. Having longitudinal data doesn't solve a lot of your causal questions; it leaves you still with the tangles of interpretation. But among the other things that I gained from the Princeton study was an appreciation for how bad retrospective reports can be for things that are not salient and especially for attitudes. So the great analytic power, as I see it, of our longitudinal design for a family survey is that we have such a wealth of family behavior patterns that we would never expect people to be able to report retrospectively--what they were doing five years ago in terms of household time allocation, time spent with children, parenting practices, time with their parents, and certainly the attitudes they held with respect to a whole range of things, including the stability of their marriage, what they argued over--none of those things you'd be able to get retrospectively. We'll be able to look at the consequences of such variables for subsequent demographic transitions.

VDT: I think Charlie is a bit biased against the Princeton Fertility Study because he thought the social-psychological findings were for the birds.

BUMPASS: Really it was in the tradition of the Indianapolis study and nothing at that level of measurement paid off. Now I have colleagues who are doing very different kinds of psychological modeling, such as Betty Thomson, who is working on motivation with different kinds of measuring instruments, and I believe there is payoff to be had. But that's not a body of work that I'm likely to contribute to with any significance.

VDT: Is your continuing enthusiasm for longitudinal studies perhaps inspired also by your work on the Board of Overseers of the Michigan Panel Study of Income Dynamics, which has followed those 5,000 families for so long, with all their offshoots?

BUMPASS: That study was not something I worked on. I was on the Board of Overseers and I guess I got on that because of my interest in survey data, my experience with the National Fertility Study. In more recent years, especially in my work with Sara McLanahan [who became PAA president in 2004], I have worked with data from the Panel Study of Income Dynamics. And surely there are very important issues that can only be addressed with that kind of data. One of the things that illustrates is very important to me and it's important to put my own study--I should say our study, because Jim Sweet is very much a codirector and partner--to keep our own study in perspective is that there is no single final solution to what we need. Each study design brings with it certain unique strengths and certain weaknesses, and the field desperately needs the kind of mix that we have and not one approach to the exclusion of the others. There are questions I can answer with the Michigan Panel Study of Income Dynamics that I can address with no other data sources. The proposal I've just written for some analytic work on our study involves some methodological issues that I'll be trying to understand.

VDT: Well, you certainly have the most popular data set around. I think the first five papers I heard this morning were based on the National Survey of Families and Households.

What took you to Wisconsin, in 1970? You hadn't stayed very long at Princeton.

BUMPASS: Much as Jim Sweet and I have worked for years on the National Survey on Families and Households, Norm Ryder and Charlie Westoff had been working on the 1965 and then were making plans for the 1970 National Fertility Survey. Norm had been at Princeton for part of the time I was there--I can't recall when exactly--working jointly with Charlie on this and, by one method or another, encouraged Wisconsin to recruit me to a faculty position. It was an opportunity to move from a post-doc to a faculty position--I was a post-doc at Princeton--and to continue working on the 1970 National Fertility Survey at Wisconsin, with Norm.

I recall a particular dinner at a PAA meeting where I sat down with Hal Winsborough, with whom I presented a paper in this meeting today ["The Death of Parents and the Transition to Old Age"], where he, representing Wisconsin, made the initial recruiting effort. I ended up doing a show-and-tell at Wisconsin--on our paper on unwanted fertility for that matter--with Norm Ryder shooting as many holes through it as he possibly could just to see how I would squirm under fire, but I managed to get the job anyhow.

So then I worked at Wisconsin with Norm, under very difficult circumstances, in that those were the years--especially that first semester of 1970--when we were getting tear-gassed and they were marching around with fixed bayonets against the protesters.

VDT: Which led Norm eventually to leave, he said.

BUMPASS: Right.

VDT: When did Jim Sweet come?

BUMPASS: Jim went directly from Michigan to Wisconsin. He was there, I think, in 1967. Bob Hauser spent, I think, no more than a year at Brown and then went to Wisconsin. And Dave Featherman spent what may have been nine months at Princeton and then went to Wisconsin.

VDT: And you've all worked--certainly you and Jim Sweet have worked very closely. But you work also very closely with Ron Rindfuss [1991 PAA president]. How did that come about?

BUMPASS: Ron came to Wisconsin in a post-doc position in the early 1970s to work with Jim Sweet on a book that they did, using the own-children methods from the 1970 census [Postwar Fertility Trends and Differentials in the United States, 1977], and I got to know Ron in the course of that and we began doing some work together and we hit it off. He went to North Carolina somewhere in the 1970s [1976].

VDT: Has Wisconsin become the leading demographic training center, would you say?

BUMPASS: I wouldn't make that claim. I would without embarrassment claim that we're among the leaders. I often interview students who are looking at a number of the major centers around the country and I tell them straight out that I can't bad-mouth my colleagues and friends. Because I think this is a remarkable association and a remarkable community of people. It's truly unusual in the extent to which we feel like a community. I'm talking about demography in general. So that my colleagues at Michigan, Penn, Hopkins, Princeton, Texas, North Carolina are my colleagues, because I work with them. These are all good places and Wisconsin deserves to be amongst that top rank, but I wouldn't want to make any rash claims other than that we're very good.

VDT: You've always been on top of the demographic issues of current interest to U.S. society. You say it was just by chance that you were getting going in the 1960s when there was such tremendous interest in fertility, the baby boom, the surveys were first coming out, and you have obviously moved with the times. I mentioned that this morning I thought almost every paper was based on the National Survey of Families and Households. You really have been on top of the issues that were of current interest. Now, were you conscious of that?

BUMPASS: Again, there's a great measure of good luck. But there's also somewhere working in that--there are two strains of answers I'll try to tie together. Thinking of Ron Freedman as a master mentor, one of the things that I've admired most about Ron all these years is his sense of what matters. I think that came through in the early days, that techniques are terribly important, you've got to do things right, but the first thing is you've got to figure out what's important and then figure out how to know more about it. And so I would hope that I learned a little from Ron on that score. But there's also once again this measure of great good luck. I've been in a career where I've been able to follow what interests me and it turns out that somebody else cares--at least part of the time.

The interest in family was really very early, because as I've come to appreciate more in recent years but I think it's sort of been an evolution, family and fertility are not different problems. Our historical understanding of the demographic transition is really about family sociology. And certainly all the work on nuptiality and the key to that, as divorce became a more visible feature of the landscape in the U.S., trying to understand divorce and its implications for fertility was essential to trying to understand what's going on in fertility. But then, by its very nature, divorce itself became increasingly

an interesting topic, as has now cohabitation and intergenerational relationships. So I'm drifting further afield from just fertility, but I haven't lost my interest in fertility, and I see it really as all a piece of the same cloth. As I tried to argue in my presidential address last year ["What's Happening to the Family? Interactions Between Demographic and Institutional Change," published in Demography, November 1990], the same forces that were changing the relative emphasis on parenting and the motherhood role also affect our patterns of relationships between the elderly and their children. There are social issues at large that Ron Lesthaeghe points to, such as individualization and secularization and commitment versus personal achievement. So I really see them as a working-out of the implications of themes that were there in the very beginning. Jim Sweet and I did our first paper on divorce around 1970, very soon after we had been sharing an office as graduate students.

VDT: You have not had too many single-author papers. How do you and Jim Sweet and you and Ron Rindfuss work together?

BUMPASS: Those are very different working relationships. They are both excellent working relationships. With Jim, it now goes back 25 years, and with Ron, at least 15. These are relationships in which there has been remarkably little stress, despite frequent disagreement. They've been relationships in which we've been able to manage disagreement in a creative way, to argue things out, one tell the other he's being an absolute fool, without it being . . . The underlying respect is so clear and so well understood that Jim can bring something in and say, "This is just absolute nonsense," and I may disagree, I may be mad, but I know he's not calling me a fool, he's just saying something I've done is foolish--and we all have some foolish thoughts a regular basis.

VDT: Does one of you write one section and one another?

BUMPASS: That will vary. Really there is no set pattern to this. On any given paper, usually we'll talk about how the analysis ought to be done, what the issues are, likely one of us on the basis of that discussion will set up the initial calculation, what have you, and we may discuss that and as a result of that that person may go ahead and draft something and then give it to the other person who, depending on how that particular interaction goes, may either expand on it or tear it to pieces and give it back.

VDT: Do you find that working with someone else increases your productivity because you're a team? You publish prodigiously. For instance, in 1989 you had three articles in Demography [on marital disruption, with Teresa Castro Martin, February 1989; on unwanted motherhood, with Sara McLanahan, May 1989; and on cohabitation, with James Sweet, November 1989] and at least one other that I identified ["Children's Experience in Single-Parent Families," with James Sweet, Family Planning Perspectives, November/December 1989]. I don't have your curriculum vitae and I'm sure there were many more. And this was the year that you were PAA president-elect and you had the meeting program to plan, and everything else. I started off by asking if working with someone else keeps you on track, or are you just . . . How do you get it all done?

BUMPASS: There's no easy answer to that. There are some papers where the other person carries the major initiative and my involvement is more in the form of commentary, revision, suggested restructuring, and there are other papers where I play that role, and there are others that are much more of a mixed variety.

I think productivity, just the sheer volume of output, is probably increased by collaborating, but collaborating more by the input of fresh ideas than by the actual production process itself. That is to say, that collaborating probably makes more work than it saves, because you have to deal with this thing of, "You've got it all wrong and you've got to do it over again," whereas you might be able to

find a journal that would accept it as it is. For example, my work with Sara McLanahan, my work with Betty Thomson, involves a set of ideas that we were jointly able to produce something on that I probably wouldn't have worked on without the collaboration. So in that sense, it increased the number of publications.

VDT: Which of your publications do you consider the most important, and why? I know that's a hard question; there are an awful lot.

BUMPASS: [Long pause] I'm not sure I'm going to be able to answer that. I would think it probably would be the ones that start a line of work or bring increased attention to a line of work, which then others may pick up and do much better and carry much further. Charlie pointed to the importance of the paper on unwanted fertility ["The 'Perfect Contraceptive' Population: The Extent and Implications of Unwanted Fertility," *Science*, 1970]. In substance, that was no new discovery at all--David Glass went into it in a paper on the 1954 British family survey--but highlighting it and sort of focusing attention in a way that brought a stream of other work to it.

I think that the work I've done on children's experience with single-parent families has played some of that kind of role, leading both to the demographic explanations and to the kinds of concerns that Sara and a whole large collectivity in the field are addressing in terms of the lifetime implications, both psychological and attainment. I don't want to claim credit for being the only person that did that, but that work helped contribute to that growing attention to the implications for children of marital disruption. More recently, our work on cohabitation--again, we weren't the first people to discover it; there were papers on Canada, papers on Sweden, papers on the U.S. based on other data sets by Arland Thornton--but it has helped to focus attention on the issue.

VDT: The paper in *Demography* got tremendous coverage, in part because Gordon De Jong [editor of *Demography* through 1990] has made certain that more papers in *Demography* are brought to the media's attention. That data, of course, was early data from the National Survey of Families and Households. Did you set up the survey in part specifically to look at these things on which you couldn't get data anywhere else?

BUMPASS: Yes. Certainly that was the case with cohabitation. The story behind the National Survey of Families and Households is, I think, an interesting story of the way in which this profession works as a community. It was not something on which we had this great flash of insight that we then proceeded to share with the world. It grew out of a set of workshops of the national community working on related topics that Jeff Evans was getting together on a regular basis. At the end of those, we'd always ending up complaining in coffeeshops that sooner or later we were going to have to quit trying to squeeze the impossible out of data that were collected for some other purpose and we'd have to take on the task of planning a family survey to find out what we needed to know that wasn't available. It was out of that set of concerns that Jeff Evans [of NICHD] eventually put out a Request for Proposals for design of the study.

We were not the only place that responded to that. We were fortunate enough to get the contract to do it, and then we wrote a proposal based on that contract work. And in the process of designing the study, we would meet on a weekly basis; this was a large collection of my colleagues at Wisconsin. Over the years, there have been at least eight of us involved--Mette Sorenson went off to Harvard, after that Judy Seltzer joined us later--Jim Sweet and myself taking some leadership and referee role. Each of us would take on the obligation to take an area of work for the week's discussion and try to address what had been done, what we knew that we didn't know, and how we might find it out. It was in that context that the questions on cohabitation evolved. In some other areas, we didn't do quite as well; we hope to improve next time.

VDT: Well, it's a tremendous survey--and huge: 13,000 respondents. It's the largest, isn't it? Of course, there's the Current Population Survey.

BUMPASS: Well, as I was saying earlier about the importance of a mix of surveys, no one survey can solve all problems. Part of what motivated that survey was a particular vision--not the only one, by any means--but a particular vision that we needed to be able to relate an array of family domains one to the other. We needed to be able to relate what's going on at work to fertility, to parenting, to relationships with one's own elderly parents, to one's background, schooling. The point is we covered a population of all age groups and we covered a very broad array of topics. Which means that for many important topics our sample size is far too small.

We have collected very good data on childhood history. We know who they were living with at age three; how long they stayed in a single-parent family. Even though there are data we've not analyzed properly yet, we've got these cases where the father goes away and comes back, when he comes back. We've got all this wonderful detail, but if you want to take people whose families broke up when they were under the age of three or between the ages of three and five and analyze it by anything else, you find that the questions are wonderful but you've got too few cases left to do anything.

VDT: You've answered this question as we've gone along, but let me ask: Who have been the leading influences on your career? Starting, obviously, with Ron Freedman.

BUMPASS: I think you've heard the cast of characters. Ron Freedman was a dominant figure. Dudley Duncan was a dominant figure, even though I didn't work directly with him; just in the classroom and then the personal relationship. You've mentioned the coffee breaks at Michigan that Ron had mentioned.

VDT: Not just mentioned; that happened while I was there interviewing him one June. We had to stop religiously at 10 o'clock for that.

BUMPASS: Right. That was something that was resented by a great many folks. But what it meant was that somebody who was as timid and from as sheltered a background as I was could ask Dudley a question any day of the week, because he'd be standing there with his coffee cup, staring into it. And you could walk up and say, "I've got a question."

VDT: You'd have them morning and afternoon?

BUMPASS: Right. So things that I would never enter his door to ask, there would be this time that was perfectly appropriate to raise them. So certainly the whole time with Dudley looms very large.

Certainly, I learned a lot from working with Charlie Westoff. But I named the chair I got at Wisconsin after Norm Ryder; one reason, of course, is that they like people with Wisconsin connections. It is unusual to name a chair after a living person.

VDT: You named it?

BUMPASS: I did it; I had the option. I was given this chair which you were allowed to name. That was within the last couple of years.

VDT: And that will be forever after the Norman Ryder Chair of Sociology?

BUMPASS: Only while I occupy it; that's the way that chair works. I named it after Norm because (a) he had a Wisconsin connection, which pleases the folks who pay the bills there, but far more important, because he had a dominating influence on my research life. Anyone who knows Norm can understand the emphasis on the word "dominant." It takes a while to develop an effective working relationship with Norm. In the early days, I would send him something and I would get back a single-spaced critique that was twice as long as what I'd sent him. This was in the period where he'd gone back to Wisconsin and I was at Princeton. That was the early stage.

VDT: You worked already with him then?

BUMPASS: Exactly. Norm would always think up 47 reasons why a number was wrong. Even when you suspected he agreed with you, he wouldn't admit that. He would tell you all the reasons why what you had found was probably just purely an artifact of methods or date. Several years of that kind of bombardment and it becomes a habit of mind. So this compulsion for cross-checking data, this compulsion of whenever I see a number wanting to know if it makes sense in terms of what else I know about other numbers, is something I attribute very much to Norm's influence. In addition to which I hold him in just enormous personal esteem and affection.

VDT: Very nice. You've named those three. You had mentioned Beverly Duncan.

BUMPASS: Beverly Duncan, sure.

VDT: And what about your students? You do teach, of course.

BUMPASS: Yes. Well, over the last few years, I have spent relatively little time in the formal classroom because of the heavy obligations with the National Survey of Families and Households. I've taught a seminar now and again.

VDT: Do you have any Ph.D. students?

BUMPASS: I have several in process now; I've had several over the years.

VDT: Do you have any leading ones, already making their mark?

BUMPASS: I feel I have an investment in a number of Wisconsin products of whom we're very proud, who weren't my primary students. There are a number of students for whom I was a major advisor of whom I am very proud as well and expect great things from. I would rather not name anyone; I'd probably leave out somebody critical.

VDT: Okay. I told you I was going to be asking you what accomplishments in your career to date have given you the most satisfaction. There are just so many already--to date, of course, because you are midway only.

BUMPASS: Well, that's a much harder question than it seems. The National Survey of Families and Households as a joint product between Jim Sweet and me is obviously a major source of satisfaction. When we set out to do it, we were aware that we not only had the risk, demographically speaking, of making a large splash, we also had the risk of making a very public flop. And it went well. There were some very spectacular failings here and there in the survey, but it's being used by about 120

researchers around the world. We got the data--and this is in very large measure a consequence of Jim's work--the data were available to the public six months after we conducted the last interview. We turned it loose and it had the desired consequence that I will often open a new copy of a journal to find an article on something I'm writing on, based on our data.

VDT: Say that again.

BUMPASS: An article based on our data on a topic that I'm working on at the moment that I didn't know was being written. Having released the data, you never know when you open a journal whether you're being scooped with your own data.

VDT: You have made the tapes freely available?

BUMPASS: We sell them at cost: \$300, or whatever it is, with all the documentation.

VDT: A lot of it has appeared in the media too. You feel it important to communicate with the media?

BUMPASS: I feel an obligation there. There are limits to what one can do. I don't have a real taste for that; I really don't. Something like the cohabitation thing, if you had a taste for it or if you didn't control it, you could do that full time for a week or two, because there are an unbelievable number of small radio stations in the country, all of which only want two minutes of your time. I've learned not to return those phone calls. But I do try to talk to people from the major newspapers and news services, because I do think it's good for the field--to the extent that we can keep these things from being trivialized--it's good to make people aware that there is social science going on that somebody cares about. So I feel an obligation to talk to them.

VDT: In your PAA presidential address last year, you pointed out that your predecessors had all talked on themes that were very relevant to society at the moment: Harriet Presser [1989] on child care, "Can We Make Time for Children?"; Ren Farley [1988] on black-white inequality; Jane Menken [1985] on intergenerational obligations; and Sam Preston [1984] on children and the elderly. You said, "The recurrence of such themes illustrates the relevance of demographic research to much that matters to our society at large." I think you've just said very well that because it matters, you have an obligation to communicate your research. And you speak to policymakers too. For instance, you've just been at a meeting of the PAA Public Affairs Committee. Are you going to do congressional presentations?

BUMPASS: There will be additional presentations of that sort. They vary in nature. I expect they're all at some level useful. I've been involved in congressional briefings where it's largely a matter of providing background information that doesn't have any direct policy link but may help thought about relevant policy to be set in the proper context--the proper understanding of divorce trends, the levels of divorce and children's and women's experience, what have you. And then there are other instances, such as the opportunity I had a year or so ago to meet with a small group with Senator Moynihan where he was focusing much more on thinking very clearly about policy initiatives, where you do get the sense that something you're doing or say could at least run the risk of having some real impact on public policy.

VDT: Which you feel an obligation to do?

BUMPASS: While I have a few obvious political objectives I would like to see accomplished, I really feel the obligation in the spirit as adopted by the Public Affairs Committee, which is to maximize the relevance and accuracy of the information that's available to policymakers. Which means that you clearly run the risk of positions you disagree with more effectively using your information than those that you agree with, but at least you can help them get the facts straight.

VDT: Good. Now we must move on to PAA. Evidently, your first PAA meeting was 1965, in Chicago, when Ron Freedman was president. You asked Ron Freedman to introduce you last year and he pointed that out; it's the custom for presidents to choose someone to introduce them who's had a big impact on them. Ron's address in 1965, as you probably remember, was on "From High to Low Fertility: The Challenge for Demographers." He admitted that he was rather optimistic then. What else do you recollect about that meeting and your early meetings?

BUMPASS: I remember the cast of characters, because I was sort of the wide-eyed new kid on the block, as I suppose graduate students are--hopefully, they're a little more sophisticated now than I was in those days--the sense of being able to see the people that I was reading was very keen. It was quite an experience to see Kingsley Davis and the others. I got the program for that year, I don't remember them all at the moment, but the program was a Who's Who of population at the time. So it was the experience of, "Oh, that's so-and-so," and beginning to associate a face with a name.

As I said in my introductory remarks last year, which didn't belong in the printed version [Demography, November 1990] of the [presidential] address, I formed an opinion of the PAA in those first years that persisted perhaps to some extent despite the change in reality, although I think there's a core of truth in it, that is--PAA really was very small. You could know everyone.

VDT: In 1965 the membership was 1,283. That was just after it took off because of Don Bogue's solicitation of subscriptions to Demography. [Members were 660 in 1962; 802 in 1963; 1,142 in 1964. Demography, with Donald Bogue as editor, began in 1964.] The actual meeting numbers were less [452 at 1967 meeting in Cincinnati, when the records resume after a hiatus since 1935]. The meeting numbers, I believe, were around 400 toward the end of the 1960s [581 in Boston in 1968; 486 in Atlantic City in 1969].

BUMPASS: Which is a third of what we have at this meeting [1991 meeting registered 1,399; 1990 meeting in Toronto, 1,175]. I did form the sense of it being a very small association where everyone knew everyone else and by and large everyone liked everyone else, and was able rather quickly--not in my first year, but in the first years--to begin to participate in that community, the national community. So this persisting sense of the PAA as a national community of colleagues is one that I hold dearly, even though we've grown much larger through the years.

VDT: That's interesting; no one has quite put it that way--the national community. But you must have got into it very quickly, because you got onto the Board in 1974-77. That was only a few years after your first meeting; you were very young. The "old boy network"?

BUMPASS: I guess so; I don't know.

VDT: Tell me a bit about your time as editor of Demography, from 1978 to 1981. I saw that darling picture of you in the photo display [display of "historic" PAA photos to mark 1991 as PAA's 60th anniversary meeting]. I hadn't seen that before, holding in your arms all the issues of Demography until your time.

BUMPASS: Marty O'Connell staged that one. I don't even remember where he did it; he just handed me all of those and shot a picture of it. This was at the time that I was editor. I've no idea what motivated it; I do remember him doing it.

VDT: It could have been for the 50th anniversary meeting photo display that we had ten years ago. You followed ten years after Beverly Duncan took over the editorship from Don Bogue, which of course brought about a radical transformation in Demography, as we all know; it suddenly collapsed into a very learned journal. How did you feel about it when you were editor? It was a lot of work, I expect.

BUMPASS: It was a lot of work. I'm not quite sure what to say about that experience. It was a good experience. It was, again, one of those experiences that knits together--serves an integrative function at the institution you're at, because you can't edit a journal by yourself. Of course, my long-time friend, Jim Sweet, played a major role in helping me with the household and fertility and family-type things; Bob Mare played a major role with respect to the more formal demographic things; and Karl Taeuber with respect to distributional issues--in terms of serving as associate editors and helping me to evaluate the reviews that were coming in.

We were at that point, for whatever reason, in a hand-to-mouth situation. Unlike the recent report at the Board meeting that they [Demography staff] have three issues in advance, we would be facing a press deadline not quite sure that we had the final articles nailed down to make up the next issue.

VDT: I noticed that your last issue, November 1981, was huge--200 pages compared to 100 to 150 before then--but there was one that was less than 100 [May 1980]. You couldn't find enough to fill it, I guess; it wasn't ready. And then your last issue looked like you had thrown in everything.

BUMPASS: That, of course, caused some consternation to the bookkeepers. That was simply a matter of trying to close the books on articles that were outstanding, that is to say, articles that had not yet been resolved, to minimize the cost of transition to Omer Galle--not to be passing along a lot of the decisions. That was a good experience, but it did not play to my strengths.

VDT: You obviously did not have any of your own articles in Demography while you were editor, but I mentioned the year, 1989, when you had three. You publish a lot in Demography. Do you deliberately want to encourage Demography?

BUMPASS: I don't want to over-characterize the bias, but I do feel a primary identification with this collection of colleagues that the PAA represents, and so publishing in Demography is communicating most directly with that community. Now, at the same time, in my last several years, I have had some evolving interest in areas of a different sort, areas in which I'm a real neophyte, which is likely to involve me in some stranger, less clearly designed circles, and I'm likely to be publishing in journals aimed at different communities. But that tendency to publish in Demography reflects both a respect for the journal and the fact that a large part of the colleagues that I most routinely share information with . . . I see publications as part of a dialogue and not as final statements on anything, but rather as the next statement about what we know about this as we continue to think it through, and so Demography speaks to that group of people.

VDT: That was, of course, the rationale for Demography, when Norm Ryder . . . he claims he started it; Don Bogue doesn't always give him credit. Don is the one that picked up the idea and did it. And one of the rationales was that there was a certain community who would see the articles there who

might not otherwise see them, even in the sociological journals--in which you also publish--scattered around; not everybody reads all those.

Now let's talk about your presidential year, which was just last year [1990]. How about planning that meeting program? It jumped to 90 sessions last year and you set the pace for 90 this year. It's just tremendous; it's exploded. What do you feel about that?

BUMPASS: That was simultaneously a very rewarding experience and about four times as hard as I anticipated. I think if anybody ever fully appreciates what's involved in doing that, we will no longer get people who agree to run for president. That cost me a good part of the year, as I think it does everybody. And it was a year that I was going to write a book, which I didn't write because of that. It's not timely now; it would have been then. I don't resent that at all. As you say, my publications are probably too many already. But it was an enormous task.

At the same time, it was an enormous task in which the cooperation and help of others was just remarkable, both in terms of the effort they put in and the quality of that effort. Once again, we're back to this hobbyhorse of mine about the community of people. It was a lot of work, because I would spend a good part of every day on the telephone, talking to people all over the country. Now I hope I didn't just play an old boys' network. When you talk to many hundreds of people, it's stretching it a little bit just to call it an old boys' network, even though when you call them you know almost all of them by first name and they're friends at some level. It was very rewarding.

VDT: What about the graduate students? You don't get their papers in there now. I've heard that in 1989, when my friend Nancy Yinger had a session, she said, "I got 53 excellent papers for this session," and, of course, four only could be chosen. Harriet Presser, who was in charge that year before you, said she saw the need to leave a couple of slots on Saturday afternoon to fill in some of these papers. What's the solution?

BUMPASS: There are at least two issues in your question. There are far more good papers than there is room on the program and there are many years in which PAA has had that. The hardest part of that job really involved the time I spent with almost 200 manuscripts that didn't fit somewhere, trying to read them myself and figure out what to do with them and to get somebody to put together sessions out of existing papers that had already been sent. So that was a lot of extra work. I think maybe a much more rational procedure is to just cut for this year and look somewhere else for next year, because not everything can make the program.

I think the other issue has to do with students in sessions, and it is my sense that PAA probably has higher student participation rates, I would suspect, than most societies. If you go through the program--I haven't done it for this year although I know several students who are on the program. I went through the program for the year before in response to a query from students about a student session and found that there was a very substantial representation of students among persons with papers, some of whom were obviously in an apprentice-collaborative role with the senior investigators and some of whom were presenting just their own work. I thought about it and talked to others and I thought it was not helpful to students to create a play-school section.

I think the notion of having the real program and then a corner for students doesn't give the graduate students credit. In the professionalization process, they may start off in awe, but by the time they are senior graduate students, they're capable of writing the kind of papers that can get on the program, and do get on the program, and be presented as the science that they are, not as a play school. So I really don't favor that.

Now there's another mechanism that students are evolving amongst themselves, so I hear from the grapevine, which I think is just dandy, and that is for the students to organize essentially seminars amongst themselves. They take the initiative. They get together and present papers to one another, so

they learn what other students are doing at other institutions. That's terrific!

VDT: At PAA meetings?

BUMPASS: At PAA. That's starting to happen. For example, at the aging thing yesterday, they at least talked about that sort of thing. I think that is wonderful. But it would be a mistake for PAA to say, "We have the real science and then we have students," because students are quite capable of the real science by the time they're senior students.

VDT: Also last year [1990], of course, a big issue was the shift from the American Statistical Association to the American Sociological Association for handling PAA's business affairs. You sort of came in on the tail end of that, that decision had been made--or forced upon us, shall we say.

BUMPASS: That's right. By the good work of others and a stroke of good luck, I was spared massive embarrassment. Because it was in the fall [1989]--I think in November but the date doesn't matter exactly--before I was to become president in January, that it was announced that we could no longer stay with AStatA. So that then left me with a specter of presiding over the demise of the Association. I could imagine things going badly at AStatA in that intervening period and the arrangements being improperly made or not at all. We could have had a serious disaster in terms of the meeting. But Jean Smith at AStatA worked very hard and her commitment to us as an association and doing that well made it so that we had a very successful meeting and suffered no great trauma on that score. We worried about the transition in the business arrangement, both in terms of being able to do it effectively and in terms of suffering serious financial losses in the transition.

VDT: Where you almost doubled the membership fee [\$45 to \$70]--totally unannounced.

BUMPASS: We were anticipating a deficit of thirty to sixty thousand dollars the year of the transition [1990]. But the transition committee, which Jeff Evans chaired--I may or may not remember all the members at the moment--but that committee with Jeff Evans, Suzanne Bianchi, Tom Merrick, and Larry Suter, and Harriet played a major role looking for organizational alternatives. There were actually several committees that followed one after another there. [A Committee on Organizational Management, formed in spring 1989 with Jeff Evans as chair and including Reynolds Farley, Charles Keely, and Signe Wetrogan, explored "possible alternatives for enhancing the Association's presence in Washington and better satisfying the growing demand for organizational capability in a variety of areas." Following the American Statistical Association's announcement in early October 1989 that it wished to sever ties with PAA, a subcommittee of the Board, the Negotiating Committee, was formed, with Harriet Presser as chair and including Larry Bumpass, Ronald Rindfuss, Suzanne Bianchi, Jeff Evans, and Noreen Goldman, whose work eventually led to the current arrangement with the American Sociological Association. To oversee the transition once this agreement had been made, Larry Bumpass appointed a one-year Transition Committee, chaired by Jeff Evans and including Suzanne Bianchi, Thomas Merrick, and Larry Suter. See, "American Sociological Association to Manage PAA Business Office," PAA Affairs, Winter/Spring 1990.] They worked out this arrangement with ASocA, which I had relatively little to do with, other than kibitzing from the margin, but it was an outstanding arrangement. And Jen Suter [PAA Administrator at the American Sociological Association], in particular, has done such a spectacular job that our giant-deficit year turned to profit. Of course, part of that had to do with the economics of demography, which are very strange indeed, because we raised the price and increased our membership as a consequence, contrary to supply and demand theories. We raised the dues very substantially, from \$45 to \$70.

VDT: Right--and that increased the membership?

BUMPASS: I wouldn't say that increased it, but our expectation of a serious deficit was based on common economic understanding that if you raise the price the demand is going to fall off, and it didn't.

VDT: At the end of 1990, we were at 2,752 members, and we just inched up over the last decade or 15 years [2,618 in 1975; 2,488 in 1980]. What do you feel about PAA getting more members? Should it?

BUMPASS: This is one of these things that can be moderately controversial, but I'll say it anyhow. I am rather indifferent to the size of our membership, as an issue in itself. To borrow a phrase with an unfortunate history, "benign neglect" is the policy I chose with respect to the Membership Committee and membership drives as president, and it was a conscious choice, because--it's been no secret in the course of these discussions--I love this Association and I'm not so keen on turning it into something else so it will be bigger and richer. So seeking new sources of members to increase the revenues, but in the process changing the composition of what it is, is not something I'm keen on doing. If the demography field grows by virtue of expansion of researchers doing population work and the Association grows as a consequence of that, that's just dandy.

VDT: This is a related question: What about the business demographers? What do you see as the outlook for demography and demographers in the U.S.? Are the majority of jobs now for applied demographers--state and local government and business--or is there still room for basic researchers like yourself? I think I sense in there, demographers doing research of the kind that have always made up the core of PAA. What about expanding that core to the business demographers?

BUMPASS: Yes, okay, because I don't want to be misunderstood on that point. I define within the core, by my definition, the work that's being done by the bulk of our colleagues in business demography and in state and local demography; this is the work of the profession. I think some of the boundaries that have from time to time been emphasized are largely artificial, certainly in my sense and obviously in my colleagues' sense at Wisconsin. Paul Voss is the chair of the PAA State and Local Government Group. Paul is a valued colleague in every sense of the word and a leading demographer. The work that that community does is by my definition part of the hard core. That, as I recall, was represented in the work in the program in those early days, in the 1960s.

VDT: Like what?

BUMPASS: Not in terms of there being an interest group of state and local government, but in that a number of the PAA members were active in government statistics programs of all sorts, both at the federal level and at local levels. That has been part of my image of the profession since those early days. I think many of our students, many of our colleagues, will continue to do that. There's obviously an expanding role for them; that's part of the outside world discovering the relevance of what we do to what they do. And there's nothing harmful about being useful.

I'm on the committee now that Rindfuss, our president, has asked me to chair--because I opened my big mouth and raised the issue--to worry about the role of interest groups in the profession [PAA]. And there are serious issues to be worried about in terms of the organizational nature of these relationships.

VDT: Yes, I think they [State and Local Government and Business] didn't like being switched from being committees to interest groups [within PAA].

BUMPASS: Right, and the issues are very complex. They cut across everything from that one to the risk of the Association moving in the direction of sections, that is, taking this larger community and digging trenches and putting up barbed wire. We don't want to do that.

At the same time, some organizational mechanism, such as the interest group provides, is obviously critical, especially, as I've come to appreciate in my conversations with Paul Voss, for the business demographers and the state and local demographers. The sense of identity, of how do we relate to the larger collectivity, is facilitated by the existence of this organizational sub-unit within PAA. We've got to find ways to make that work even better.

VDT: Within PAA; hang onto them.

BUMPASS: Yes--oh, yes. I don't think we'll lose them.

VDT: One last question. You've had consistent funding from NICHD over the years--obviously the survey, Jeff Evans, and so on. The source of the real money for population research is NICHD, but only about 16 percent of the approved grants are getting funded, although yesterday they were saying they were getting up to about 19 percent. Perhaps you've been favored. What do you see as the outlook for basic research with funding getting more and more restricted, even for such a sexy topic--as Jane Menken put it--as population? She felt that population research had been favored over a couple of decades and that might now be changing.

BUMPASS: I'm more optimistic, again for reasons that I outlined in my presidential address, though I didn't draw that line there; that is to say, I didn't develop that point. The reason I'm optimistic is because . . . The efforts of our Population Affairs Committee are in part symptomatic, they're not just causal. They reflect the fact that policymakers as well as the business community are becoming increasingly, extremely, aware that they need to know the demographic trends and understand more about their underlying processes in order to formulate policy that costs billions and billions of dollars. I was talking to the staffer, who is no longer with him, but had been a key staffer for Senator Moynihan. I was just blown away by the way she rattled off alternative policies where she was rounding to the nearest billion. When things cost that much, what it costs to fund research to make the policy informed is a rather minor point. I think that the kind of large increase that NIA [National Institute on Aging] has seen, partly driven just by Alzheimer's and totally out of our thing, but the funding for social science at NIA reflects an increased awareness of how critical it is that policy looking forward to an aging population understands the issues about intergenerational relationships, about frailty, about joint retirement, all of these things that are within what our colleagues do. And the society that faces multi-billion-dollar policies on limited information. I'm optimistic that what we do matters, will continue to serve us well.

VDT: What you are doing matters, because you're really on top of it. Thank you. You're due down at the business meeting to be on show with the directors.

AFTER REMARKS

BUMPASS [removing running shoes]: I'll take off these shoes I put on in the little break before our meeting time, because my other meeting ended early. I buy running shoes for the sake of comfort.

VDT: And do you exercise?

BUMPASS: I run regularly, maybe three times a week, although I'm not a marathon runner like Ren Farley. Ren runs marathon.

VDT: As you know, this is my very last interview for this series, although I regret to say I never got Evelyn Kitagawa, who was one of the rare women who made it right to the top in this exclusive sample. Do you have to be very, very special to make it right to the top of PAA?

BUMPASS: Actually, I think this profession is distinguished by its treatment of women. You go back to Irene Taeuber, Margaret Hagood, Dorothy Thomas, Harriet Presser, Jane Menken.

VDT: But there was the long gap [between Dorothy Thomas, president, 1958-59, to Evelyn Kitagawa, 1977]. You and Jane Menken--well, she was part of that cohort, although you're younger than Jane.

BUMPASS: Jane was part of my Princeton group, when I was at Princeton in that short spell, hanging out at OPR, with Jane Menken, Nancy Howell Lee, Massimo Livi-Bacci, Ron Lesthaeghe--a number of others.

VDT: We've just wondered why the young people are not coming to the business meetings at PAA [technically known as the annual "membership" meeting since the constitutional change of 1974 described here by Larry Bumpass]. Now, in the old days . . .

BUMPASS: That's a reflection of the business meetings of the old days. For all the sense of community and friendship that I described, which was real, the business meetings were often free-for-all, because they were meetings where policy could be decided for the Association. So if something was politically hot, a source of political disagreement--for example, very keen issues on what the U.S. ought to be doing about family planning programs worldwide or other such issues--decisions could be made at those meetings that would affect the course of the Association. So people would argue greatly on both sides of the issues.

I actually served on a constitutional committee with Con Taeuber and others in the early 1970s which set up the current circumstance, because we worried about packing the galleries, about the way in which PAA policy could be decided on a particular issue by organizing to have a lot of people there and vote, that would then determine what would be the course of the PAA. So we set up the business meeting as it is now, as an informational meeting and as a forum for discussion, but any decisions are either made by the Board or by referendum of the Association.

VDT: And people have complained it's become so boring.

BUMPASS: It does that, indeed.

VDT: Well, let's go see how it is today.

What's Happening to the Family? Interactions Between Demographic and Institutional Change

*Population Association of America,
1990 Presidential Address*

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As soon as men and women . . . acquire the habit of weighing the individual advantages and disadvantages of any prospective course of action . . . they cannot fail to become aware of the heavy personal sacrifices that family ties and especially parenthood entail under modern conditions. (Schumpeter 1988/1942, pp. 501–502)

As I argued several years ago (Bumpass 1986), the perspectives of demographic transition theory are relevant to contemporary family change (Davis 1967; Freedman 1975). Because fertility cannot be isolated theoretically from the institutional context in which it is embedded, theories about fertility decline are intrinsically also theories about changes in the family as an institution.

Hence the question, “What’s Happening to the Family?” is one that demographers have been asking for a long time, and our perspective has been both historical and dynamic. The institution of the family is not seen as a fixed form against which we can judge current behavior. Rather, it is the collective representation of our changing family experience, as that experience interacts with its environment. Normative expectations play a major role in structuring family patterns, but they also tend to lag behind changing behavior, accommodating in time to behavioral changes.

Duncan and Schnore’s (1959) POET formulation draws attention to the reciprocal nature of causation and the interplay between social organization and technological change as technology redefines environment. Certainly the evolution of our technological culture has radically altered the social context of family relationships.

I believe that the theoretical perspectives of a half-century ago were essentially correct: from Ogburn’s emphasis on declining family functions (Ogburn & Tibbits 1933)¹ to Lorimer’s description of increasing individualism at the expense of moral obligation (Lorimer, Winston, & Kiser 1940). Goode’s (1963) *World Revolution and Family Patterns* was on target with respect to many of the linkages between industrial economies and the reduction and reshaping of family roles, though I doubt that the conjugal family system should be seen as the end point of this process.²

At the same time, the changing structural and economic bases are not the whole story. Lesthaeghe (1983; Lesthaeghe & Surkyn 1988; Lesthaeghe & Wilson 1985) argued that individuation and secularization are independent causal forces in family change. These cultural factors exist in symbiotic relationship with modern economies, but they have deep historical roots of their own (Bellah, Madsen, Swidler, Sullivan, & Tipton 1985; Shorter

1973; Stone 1982). When needs and interests conflict, as they do in any collectivity, how much weight must the individual give to the interests of others in contrast to his or her own interests and how large a circle of others must be considered? The shrinkage of this circle from a larger community toward the individual alone is nowhere better illustrated than by the level of marital disruption, to which I will return momentarily.

I am aware that the stance taken here may appear to run counter to historical work discrediting various links between modernization and family change. I think the differences are more apparent, however, than real. This perspective does not require close temporal association between economic or structural shifts and family changes. Underlying ecological contexts of organizational change may create tensions, much like those at a fault line, the effects of which are not seen until the confluence of forces overcomes inertia. The precipitating factors may be economic, cultural, or political events, or they may be no more than the accumulated weight of past change.³ The most dramatic recent example of this is the collapse in the total fertility rate in Spain, over a single decade, from one of the highest in Europe to one of the lowest (Munoz-Perez 1987; Sardon 1986).

Our confidence that we understood low fertility was shattered by the baby boom, so we retreated and tried to comprehend this embarrassing disconfirmation. As Ryder put it, demographers were "victims of a failure of nerve: they should have stuck to their theoretical guns" (1979, p. 359).

Although the baby boom is long over, the demographic transition in the West most likely is *not*. We tend to think of the transition in the relative value of childbearing as a *past* event for today's low-fertility societies and implicitly assume that we have reached a final equilibrium. We have no theoretical basis, however, for this assumption. Indeed, a growing number of authors are suggesting that the factors supporting marriage and parenthood are likely to erode further under the pressure of competing opportunities (e.g., Bernard 1972; Butz & Ward 1979; Davis 1983; Demeny 1986; Huber 1980; Keller 1971; Popenoe 1988; Ryder 1979; Westoff 1978; Westoff & Ryder 1977).

Our consideration of family change profits greatly from research on the baby boom and the subsequent fertility decline. We recognize, as emphasized by both Blake (1972) and Davis (1972), that a broad array of pronatalist influences persist, particularly in the different socialization of males and females and in discrimination against women in the workplace. We also see that some pronatalist forces may occur on a cyclical basis, as emphasized by Easterlin (1978) and Butz and Ward (1979).

However, we too often consider one explanatory scheme at a time. We must expand our view to include simultaneously both profamilial and antifamilial factors and to regard family and fertility behavior as a dynamic outcome of these opposing forces. This puts the question in historical perspective, drawing our attention to the trend line around which fluctuations may occur; and it raises the question of which forces are likely to strengthen and which to weaken in the future.

In spite of the great inertia of pronatalist forces, subreplacement fertility is the norm in Western industrial societies, and the processes underlying the long-term decline seem far from exhausted.

Hence in my perspective on the question in this talk's title, what is happening to the family is a continuation of what has been happening for at least several centuries. The subtitle draws attention to potential feedback loops. Changes in one family domain may contribute to further changes in that domain or in others. We have experienced major alterations in marriage, divorce, childbearing, women's employment, parenting, and adult intergenerational relationships—and these have mutually reinforcing effects as well as impacts on normative expectations about family life. I now turn to these issues. The following discussion draws heavily on the 1987–1988 National Survey of Families and Households (Sweet, Bumpass, & Call 1988), and results not otherwise referenced are based on our tabulations with these data.

Marital Disruption

I will begin by focusing on marital disruption, because changes in this area have the most profound implications. Surely other linkages are important, and I will discuss several along the way, but I believe that no other changes have so significantly altered family life.

The deep historical, cultural, and structural roots of changing family behavior are illustrated well by the trend in cohort divorce rates (Cherlin 1981; Preston & McDonald 1979). Although annual rates of divorce have fluctuated around the trend line, the underlying rate of increase in the level of lifetime divorce has been virtually constant for more than 100 years, generating the accelerating curve from 7% for marriages in 1860 to the current expectation of well over one-half. It is too soon to conclude that the plateau in the divorce rate of the last decade represents the end of this trend—we must remember that there was a similar 15-year plateau before the takeoff of the late 1960s.

In any event, the current level of marital disruption is very high. Castro Martin and Bumpass (1989) recently estimated that almost two-thirds of recent first marriages would be likely to disrupt if current levels persist. Further work leads us to suspect that 60% may be closer to the mark.⁴ The *exact* level of marital disruption is much less important, however, than the social fact that the majority of recent first marriages will not last a lifetime.

The pervasiveness of this experience argues against hypotheses tied to specific locations in the social structure, certainly for the underlying trend. Though there are important differentials—in particular by race, education, and age at marriage (Castro Martin & Bumpass 1989)—no group is immune. For example, rates of the early 1980s imply that more than half of first marriages are likely to disrupt among whites, women who attended college, and those who married after the age of 23.

Further, the strength of the forces affecting marital stability is illustrated by the experience of Catholics and others who disapprove of divorce. Despite the strong position of the Catholic church and its buttressing through religious sanction and exhortation, Catholics are no less likely to divorce than are non-Catholics (Bumpass, Castro Martin, & Sweet in press). Similarly, Thornton (1989) found in his longitudinal analyses that disapproval of divorce is unrelated to subsequent divorce rates, even though attitudes tend to accommodate to having been divorced.⁵

This high level of marital instability dominates core aspects of family life for the majority of the population and challenges family norms. The most important impact is on the family contexts of children.

About half of today's young children in the United States will spend some time in a single-parent family, most as a consequence of divorce (Castro Martin & Bumpass 1989). Furthermore, this is not simply a transitional phase between first and second families: the majority will remain in a mother-only family for the remainder of their childhood (Bumpass & Sweet 1989a).

We have heard about the problems this creates for children so often that we are in danger of being numbed to them. In addition to psychological distress (Heatherington, Camara, & Featherman 1983; Wallerstein & Blakeslee 1989; Wallerstein & Kelly 1980), consequences include a marked increase in poverty for women and children (Garfinkel & McLanahan 1986; McLanahan, Astone, & Marks in press), effects on parenting practices and adult time available for children (Heatherington et al. 1983; Thomson, McLanahan, & Curtin 1990), and substantial negative impacts on children's educational attainment and their own family and fertility histories (McLanahan 1985; McLanahan & Bumpass 1988).⁶

Divorce illustrates the force of secular individualism more clearly than changes in any other family domain. Although there are obviously many cases, such as abusive families, in which divorce is essential for the well-being of children, the interests of parents and children in the preservation of a marriage often differ (e.g., McLanahan 1989; Wallerstein & Kelly 1980). Despite the widely recognized impacts on children, we increasingly ac-

knowledge parents' self-interest as a legitimate criterion for the resolution of this conflict. The proportion saying that a couple should not stay together for the sake of the children increased from 51% to 81% between 1962 and 1982 (Thornton & Freedman 1983). As McLanahan (1989) pointed out, this is a conflict of interest in which children are virtually powerless.

Traditional notions of the family cannot long survive such massive disconfirmation. Of course many families are stable. More than a third of first marriages are likely to remain intact for life, and more than half of the children who begin life in a two-parent family will have intact families throughout childhood. Nonetheless, widespread awareness of how common disruption is leaves little basis, a priori, for confidence in marital survival. Insecurity about the viability of marital relationships may lead to reduced investments in some marriages, which, in turn, lower the prospects that they will last (Becker 1981). Hence there may be feedback from demographic behavior to the institution of marriage itself.

A major case in point is the attachment of young women to the labor force. This long-term trend may be reinforced by the high level of marital disruption. Because marriage provides such a shaky lifetime guarantee, adolescent girls may increasingly recognize that they must be able to support themselves if necessary. In turn, increases in women's economic independence may reduce the probability that couples will stay in bad marriages (Goode 1963; Hannan, Tuma, & Groeneveld 1978) or, perhaps, even stick out the tough times in good marriages.

Of course, few actually plan for divorce; prenuptial agreements are news because they are rare and violate our normative expectations. Stable marriage remains the ideal: three-quarters of people surveyed agree that a couple should remain together for a lifetime except under extreme circumstances, and only 12% explicitly disagree. Nonetheless, norms against divorce have weakened greatly: only a third disapprove of divorce even for couples who have a preschool child.

Cohabitation and Marriage

Although the disruption of marriages has the most profound implications for changing family life, the *formation* of unions has also undergone revolutionary change (Bumpass & Sweet 1989b). The proportion of first marriages that were preceded by cohabitation increased from 8% for marriages in the late 1960s to 49% among those in 1985–1986. The accelerating increase in cohabitation has not likely attenuated. Consequently, it is likely that more than half of persons in their 30s have lived in a cohabiting relationship, and more than half of recent marriages were preceded by cohabitation.

How has it happened that what was once morally reprehensible has become the majority experience in just two decades? We are still working to understand the meaning and implications of this trend, but I would guess that two major factors are at issue. The first is the erosion of normative objections. "Shacking up" was offensive, after all, not because couples were sharing cooking and the laundry, but because they were sharing a bed. The revolution in the sexual experience of unmarried persons over the same period has seriously weakened this basis for disapproving of cohabitation. Only a fifth of young adults now disapprove of premarital sex, even for 18-year-olds; and only one-sixth explicitly disapprove of cohabitation under any circumstances.

All else being equal, it seems likely that sharing a household with an intimate friend is more rewarding than maintaining the same relationship at a distance. So with reduced normative constraints, we should expect increases in cohabitation, even if nothing else changes.

Something else has changed, though; in particular, we are much less confident about marital stability. As the fragility of marital relationships is increasingly recognized, couples

may sense a need to "try out" marriage by living together before making a long-term commitment.

In a series of questions about reasons for cohabitation, that "couples can make sure they are compatible before getting married" was indicated far more than any of the other options offered. Of singles who expect to cohabit in the future, more than 80% say that they think this is an important reason for cohabitation. Consistent with this view, at least one partner expects marriage in 90% of cohabitations.

What are the implications of cohabitation for family life more generally? Cohabitation is clearly symptomatic of our changing values about what is unique to marriage. Nonetheless, because the changes are at the boundary of marriage, I believe that cohabitation has not changed the meaning of marriage much for the married population. There are, however, several implications worth noting:

1. First, it is the meaning of "single" that has changed markedly with cohabitation. The reduced number of years spent married during young adulthood should not be viewed as simply a period of unattached living. Most of the rapid decline in marriage has been offset by increasing cohabitation; young persons are setting up joint housekeeping almost as early in their lives as before marriage rates declined. It is also important that almost half of the cohabiting couples have children present ((Bumpass, Sweet, & Cherlin 1989).

2. Associated with this, the event of marriage is a less specific marker of other transitions. Sex, living arrangements, and parenting depend less on marriage. This, of course, is a nuisance for demographic modeling, which assumes such linkages. We have to rethink how we ought to model the dependencies between marriage and related demographic events (e.g., Rindfuss & Parnell 1989). At the same time, the effect on most duration-dependent measures is probably not great because the average duration of cohabitation is very short (median of 1.5 years).

3. There is not likely to be a single answer to whether unions should be clocked from cohabitation or marriage. Since marriage is not a necessary condition for sharing a household, it may be delayed simply until a more convenient time for the ceremony or until the couple is more certain of their commitment. Hence for some couples "marriage" essentially began when they started living together, whereas others avoid an unstable marriage by splitting before they reach the altar.

4. It seems most likely that these "premarital divorces," as Sweet calls them, have helped keep the divorce rate from going even higher. If many couples are using cohabitation to test their relationship, and if 40% split up without marrying,⁷ then we expect those who do marry to have more stable marriages than would have been the case in the absence of cohabitation.

Despite all the attention given the results on the higher divorce rate of marriages preceded by cohabitation (Bumpass & Sweet 1989b; "Doubt on" 1989), this finding is not contrary evidence on this point. Cohabitation is selective and includes a lower proportion of couples who hold traditional family attitudes (Booth & Johnson 1988; Bumpass et al. 1989) and a higher proportion of those who are uncertain about their relationship.

5. Further, the data suggest that not all cohabitations may be part of the marriage process. Some may be better characterized as relationships of convenience in which marriage is not at issue. Although three-quarters of cohabitators expect to marry their partner, a quarter do not. The other partner anticipates marriage in most of these latter cases, and the process may be moving either towards or away from marriage. In 10% of cohabiting couples, however, neither partner expects marriage.

The planned follow-up to the National Survey of Families and Households will allow us to evaluate the extent to which cohabitators with different marriage expectations subsequently marry. At present, there is some relevant preliminary evidence based on responses to a

question on the perceived chances of eventual separation. Cohabitations in which both partners expect marriage resemble recently married couples more than they do other cohabitators.⁸

6. Finally, both marital instability and cohabitation are part of, and contribute to, the reduction in the perceived necessity of marriage. If marriage is not a prerequisite for many of the benefits traditionally associated with it, and if it is only a weak guarantee of those benefits, awareness of these facts may be eroding marriage's normative prescription. Only a third of young adults under 25 agree that "it is better to be married than to go through life single," and one-quarter disagree (Bumpass et al. 1989). As Thornton (1989) recently emphasized, marriage has become much more discretionary as an adult role.

Now, virtually all persons plan to marry, and almost 90% are likely to do so if current age-specific rates persist.⁹ Nonetheless, both marriage and remarriage rates have continued their downward slide; and for the most recent experience in the National Survey of Families and Households (NSFH), these declines were not fully offset by cohabitation. Marriage is not going out of style, but it is being progressively delayed. It seems likely that the proportion never marrying, and especially the proportion not remarrying after divorce, will continue to increase.

Childbearing and Parenting

Parenthood is also losing its ascribed status (Bumpass 1973). Indeed, an item similar to the one on marriage, whether "it is better to have children than to go through life being childless," gets similar responses. This is exactly what we would expect from both the theoretical perspective on long-term fertility decline and the progressive legitimization of individual self-interest.

As we all know, the total fertility rate has been stable in the United States over most of the two decades when marriage behavior has been changing so radically. This stability, however, conceals a great deal of change. The proportion of children born to an unmarried mother has doubled since 1970 to more than a quarter in 1987.

One aspect of this trend is nonmarital childbearing's now becoming part of the life experience of a significant proportion of women. Life table estimates suggest that 17% of white women and 70% of black women will have a child while unmarried if recent levels persist. The rapid increase in the prevalence of nonmarital childbearing is evidence of, and contributes to, the erosion of norms against behavior traditionally described by such terms as "illegitimacy" and "bastardy." It is illuminating on this point that one-third of our unmarried respondents under 35 agreed to the item, "It would be all right for *me* to have a child while unmarried if I planned on marrying," and that only half explicitly disagreed.

This is not to say that unmarried childbearing has become desirable for many, only that the normative constraints against it have weakened greatly. Why might this be so? I think the answer again lies largely in the level of marital instability. If marriage assures neither a stable two-parent family for the child nor lifetime economic security for the woman, the importance of marrying to "legitimate" a birth is much less compelling (O'Connell & Moore 1980). Further, as with cohabitation, it is decreasingly relevant that the birth provides evidence of nonmarital sex.

More than two-thirds of nonmarital births are reported by their mothers as unplanned.¹⁰ Hence I argue that the increase has been driven far more by changing orientations toward marriage, and the reduced stigma of nonmarital childbearing, than by desires to have children out of wedlock.¹¹ Falling rates of marriage and remarriage and increased rates of marital disruption have simply lengthened the unmarried period of risk for unplanned childbearing.¹²

It is an interesting twist in all of this that a quarter of all out-of-wedlock births are to cohabiting couples and hence constitute two-parent unmarried families. But in the present

context, it is best to regard the vast majority of these as simply unplanned childbearing that happens to occur during cohabitation, rather than as a form of planned "marital" fertility. Most of these result from unplanned pregnancies, and a third of the parents do not marry each other (Bumpass & Sweet 1989a).

The unusually high birth rate among teenagers is one important component of unplanned fertility (Trussell 1988). In the current "stable" fertility setting, almost two-thirds of teenage births are now premarital, and 80% are either premarital or premaritally conceived (U.S. Bureau of the Census 1988).

Overall, nearly two-fifths of births in the early 1980s were either timing or number failures (Pratt & Horn 1985). Thus there is a substantial component of recent fertility that could be drastically reduced in the future. I think we will continue to move toward a "perfect" fertility-control society of the sort Westoff and I reflected on 20 years ago (Bumpass & Westoff 1970). RU486 (the French abortion pill) and future such developments are likely to move us a bit farther along that path.

To the extent that we reduce unplanned fertility, I expect a substantial effect on completed family size—in part because delayed fertility tends to lead to births foregone (Rindfuss & Bumpass 1978). I expect further declines in *intended* fertility for other reasons as well.

First, once again, is our high level of marital instability. As Weiss and Willis (1985) pointed out, the very substantial prospect of single parenting is likely to be increasingly recognized as among the potential costs of motherhood. For men, the argument runs that the potential benefits are likely reduced, given that after divorce most children stay with their mothers and have very reduced contact with their fathers.

The second reason for expecting further fertility decline is the difficulty of juggling family and work obligations. One aspect of this is the theme developed so well by Presser (1989). With the increasingly equal involvement of women in the labor force, arranging for child care has become something of a nightmare, which recent legislation has only begun to remedy.

Quite apart from child care, though, complex work schedules often compete with potential family time. For example, questions on work schedules in the NSFH revealed that among two-earner couples with children at home, three-fifths have some work hours between 5:00 and 10:00 in the evening and half have weekend hours; about one-sixth work most of the hours during these periods.¹³

The employment of mothers of young children is very instructive with respect to our relative values. When there is a conflict between our values concerning the parenting needs of children and economic roles, our priorities are demonstrated by our behavior.¹⁴ The importance of a mother's full-time care for an infant has been built into our value system and reinforced by major sources of advice on child care. Three-quarters of U.S. women in 1970 said that they thought that a mother's working was harmful to a preschool child (Mason & Bumpass 1975). Yet in the years since, the employment of mothers of young children has doubled to more than one-half.¹⁵ Only a third now agree that a mother's working is harmful, but more instructive, almost another third are uncertain and neither agree nor disagree. Even among mothers of preschoolers who are working full time, only about half disagree that such work is harmful for children. It is important that I not be misunderstood on this point. I am not asserting that mothers' working is harmful; the evidence seems mixed at best. My point is that this major change in parenting behavior has occurred *despite the belief* that it is or may be harmful.

This change is often cast as being driven by economic necessity. Surely many mothers of young children may work against their preferences because they have to. We must also remember, though, that economic need is a highly amorphous concept, always seeming to outstrip what we have.¹⁶ It seems more appropriate to recognize women's employment as yet another process with deep historical roots, driven in large part by the extension to women

of individual opportunities that have long been available to men.¹⁷ As Davis (1984) emphasized, it has been a continuous process over the century, with the increases among mothers of young children the last to occur because of our strong cultural values against such employment.

Thus even in the context of stable marriage, when parenting competes with other adult interests and roles, self-interest is a legitimate basis for decision. By "self-interest" I do not intend to denote selfishness in a narrow hedonistic sense but, rather, concern with what one *ought* to do for oneself: for example, the need for a woman to be capable of financial independence. There is also a clear normative component to individuation that is illustrated in the declining legitimacy of "housewife" as a full-time adult identification for women¹⁸ or, indeed, in expected life styles and consumption patterns.

Our low birth rate and increasing nonparenthood (Bloom & Trussell 1984) are the outcomes of the struggle between parental roles as prescribed by our culture and other adult commitments, in which family roles often lose out. Low fertility exists throughout Western societies in the face of persisting gender inequality. I think we will move inexorably toward true equality,¹⁹ and the full implications of *that* for fertility are yet to be seen.

Marital Relationships

Turning to marital relationships, increasing women's employment may be altering the mate selection process (Oppenheimer 1988), the nature of marriage bargains (Becker 1981), and marriage relationships and marital stability (Cherlin 1981; Hannan et al. 1978). Indeed several authors (Bennett, Bloom, & Craig 1989; Schoen & Owens 1990; Thornton 1989a) have suggested that what men and women want out of marriage may be diverging, making it harder for bargains to be struck in the marketplace and contributing to lower marriage rates. Husbands and wives may increasingly have different expectations about the division of household labor, and some may even disagree about whether the wife should be employed.

We find that one-third of husbands in recent marriages prefer that their wife not be employed. Nonetheless, males are not the only source of disagreement on this issue. One-sixth of wives would rather not be working, including 10% of those employed full time; and of women who prefer to work, the vast majority want less than full-time employment.²⁰ The difference in the amount of hours men and women say they would prefer to work undoubtedly reflects both differential socialization and the realities, for women, of having to combine household and labor market obligations.

In the context of the attention given wives' employment, we find surprisingly little relationship in the aggregate between female employment and measures of marital quality. On the other hand, effects do appear when the joint preferences of spouses are taken into account. Furthermore, which combinations affect marital quality depends on whether you are talking to the husband or the wife. For example, in marriages in which the wife wants to work and is working despite her husband's preference that she stay home, trouble in the marriage²¹ is associated with her employment when the husband is the respondent but not when the wife is. On the other hand, when the wife does not want to work but her husband wants her to, her employment is associated with trouble in the marriage only when she is the respondent. In other words, when there is a discrepancy between spouse preferences, if the wife works in violation of the traditional power relationship, husbands see it as disruptive of the marriage; whereas if the wife does not work, in deference to the traditional power relationship, she sees it as disruptive.

Thus there are important issues of spouse disagreement over the wife's employment. At the same time, we must remember that employment is not a point of conflict in the majority of marriages. About three-quarters agree about the wife's employment—most that she should be working. Among these couples, the wife's employment has no effect one way or the other on marital quality.

We find that employment beyond the hours regarded as normal in our society seem related to marital quality. Trouble in the marriage is reported more often if the wife is working more than 45 hours or if either spouse works on a job where the shift varies (White & Keith 1990).

A major issue in the effect of wives' employment on couples' relationships is the extra task load that working women carry because of persisting traditional expectations.²² The good news is that male household efforts are responsive to the level of the wife's employment. In our data the actual hours contributed by the husband (and not just the proportion of total effort) increase by 30% between couples in which the wife is not working and those in which she is working full time (Dutchin-Eglash 1988); the time fathers spend in care and feeding of young children similarly increases with the mother's hours of employment. The bad news, though it is little "news," is that husbands carry only about a third of the household task load even if their wife is working full time.²³

Husbands and wives agree that household task allocation is unfair to the wife.²⁴ What is striking is the marked difference between male and female respondents in the association between the perceived fairness of household task allocation and reports that the marriage may be in trouble. There is only a modest association among males, but females who regard the division as unfair are much more likely to report trouble in their marriage. Further, this effect increases with the number of hours a wife works. Wives working full time are twice as likely to report having trouble in their marriage if they regard the division of labor as unfair to them (40% compared with 18% among those who regard it as fair).

Hence the "second shift" (Hochschild 1989) of working women affects the potential stability of many marriages—at least as seen by women. The gender difference in recognizing this effect is likely a major reason why among persons with separations in the last 10 years, women are more than twice as likely as men to report that they wanted the separation (McLanahan 1989).

In the midst of all this disruption and changing role behavior, why hasn't the family disappeared? Why does anyone bother to form families with children? Although the answer seems obvious because of our profamilial cultural values, it is worth reiterating that I view family relationships as being in a dynamic tension, with many profamilial factors involved in this balance.

Once again, I am drawn back to the classroom in Ann Arbor, where Freedman passionately argued that the *relational* aspects of family were not available in the marketplace. Intimacy with a partner in the context of commitment provides unique aspects of meaning and social support. The data make it clear that parenting creates many strains, but raising children also provides many rewards of more intimate and holistic relationships than are to be found in work roles. The status our society ascribes to marital and parenting roles is eroding in comparison with those of the workplace, but the majority of us continue to find family roles intrinsically and uniquely rewarding.

Intergenerational Relationships

A part of this is clearly found in intergenerational relationships in mid-life and later years, as these provide a major source of social connectedness and support—perhaps all the more important (at least in relative terms) because of the greater volatility of marital relationships. The ascribed aspect of this relationship makes parents and children more permanent members of one's "convoy" of social support (Kahn & Antonucci 1981) over the life course. Friends and even spouses may come and go, but it is harder to trade in one's parents—and for women at least, one's children. Most parents and children live near one another and continue to interact on a regular basis throughout life (Hoyert 1990; Shanas 1982). Help with household tasks, baby-sitting, emotional support, and financial exchanges

(and most important, the potential for these if needed) weave a fabric of interdependency linking the generations together.

We may not fully appreciate the importance of some aspects of intergenerational support because they are specific to certain life stages. Our data on family help with first-home purchase illustrate this point nicely. Among those who have purchased a home since 1980, about a quarter reported help from relatives (mostly parents). For those who received such help, about half was gifts and half loans, and the median amount was almost \$5,000.

Similarly, although independent living on the part of the elderly has become increasingly common—reflecting both our values and improved economic ability to implement them (Michael, Fuchs, & Scott 1980)—coresidence is far from rare even today. Of the elderly with living children, one-sixth are presently sharing a household with an adult child, and one-quarter of the elderly in ill health are doing so. From the middle generation's point of view, over a quarter of persons aged 55–64 report that a parent has lived with them since they have had their own household. We miss much of this in cross-section because the period of coresidence tends to be short—a period of reduced independence preceding death or institutionalization.

At the same time that these critical intergenerational linkages persist, they too are likely being altered. The proportion of the elderly seeing a child at least once a week declined by 25% between 1962 and 1984, and there has been a marked decline in the functional interactions of help with household repairs by men and help with housework by women. For example, the proportion of women over 65, with living children, who reported that they had helped a child with housework during the last month declined from 52%, to 39% (Shanas 1982), to 8% over the 1962–1987 period. Many factors may be involved, including the marked increased employment of their daughters and the high level of marital instability.

The full effect of marital instability on intergenerational relations will not be seen until the cohorts that have experienced the high instability of the last two decades approach retirement. Given that most fathers become rather marginal in the lives of their children within a few years of marital disruption (Furstenberg & Nord 1985), what does this imply about future intergenerational relationships as these fathers enter old age? Among persons with both parents living, those separated from their father during childhood are much less likely to see him regularly: 50% see their father more than once a year, compared with about 90% among those whose parents are still married at interview.²⁵ As we would expect, the differences are not as dramatic for mothers; but even so, the proportion having little contact with their mothers is twice as high among those whose parents are no longer living together. Lower levels of exchange are also associated with divorce and remarriage in the middle generation.

Conclusion

Thus it seems that profamilial normative pressures have eroded in all areas of the life course. The nature of these value changes was dramatically illustrated in a recent piece by Easterlin and Crimmins (1988). They noted that there has been almost a doubling since 1970 in the proportion of young people who set financial success as a major life goal, with an associated decline in the proportion concerned with finding meaning in life.²⁶ Not coincidentally, there was also a marked decline in the proportion who think parents should spend more time with their children.

Reporters often want me to say whether recent family change is “good” or “bad.” I have to respond that there is no single answer because we hold values that are incompatible. From the perspective of adult individual freedom, much is being gained. The steady erosion of the ideological baggage of patriarchy is good for both men and women. The increasing opportunity to choose rewarding occupations and rewarding intimate relationships is not something many of us would wish to reverse. At the same time, we continue to care about

the well-being of children, about those without adequate social and economic support, and about the human capital of the next generation.

As Popenoe (1988) argued,²⁷ asking whether the family is disappearing misses the point. What is at issue is not the persistence of the institution of the family but, rather, the nature of family patterns in the relevant future—and the opportunities and costs of those patterns. Understanding the long-term character of institutional change should direct social policy toward the amelioration of negative consequences, rather than toward attempts to reverse the tide. The underlying causes are not in events or policies of the last several decades, and it is most unlikely that social policy can significantly alter the course of these trends.

I agree with Teitlebaum and Winter (1985) that the arguments against outweigh those in favor of pronatalist policies in Western societies. Nonetheless, with continuing subreplacement fertility, I expect to see an increase in pronatalist concern—motivated by variations on the 1930s themes of “national decline.” Experience to date suggests that even very expensive programs are unlikely to have much effect. On the other hand, attempts in this direction could inadvertently improve the well-being of families to the extent that they include factors such as family allowances, parental leave, and child care.

I am cautiously optimistic about the prospects for constructive legislation, though I arrive at this optimism through a perverse form of cynicism. Although I clearly prefer that policies be implemented out of a concern for children and the less privileged, our record in this regard is checkered at best. On the other hand, our society may be willing to commit *major* resources to prevent further deterioration of our economic competitiveness; and we may have the foresight to understand the importance for this goal of our investment in children. Adequate health insurance, quality day care, and a family income floor for all children seem essential ingredients for bolstering the human capital of the generation that must pay the taxes when we are old.

In addition, we just may be able to achieve adequate provision for the long-term care of the elderly through the conjunction of the self-interest and political power of the baby boom. We tend to focus on the consequences for health care when the baby boom reaches older ages. As recognized in a recent series of articles in the *New York Times*, however, this generation's influence will first be felt as they seek help with the long-term care of their own parents. The baby boom represents an increased voting block in middle age, when a higher proportion than ever before will have surviving parents and parents living to be very old.

So what is happening to the family? Family relationships occupy an important but ever shrinking space in our lives (Espenshade 1985). This is the continuation of a long-term process and is not confined to one country. Trends in cohabitation, marriage, fertility, and marital disruption are widely shared across Western industrial societies. To my mind, major causes include the individualizing tendency of participation in our economy and cultural values of individualization that both facilitate this participation and are reinforced by it.²⁸ There is no reason to think that these processes are exhausted or are likely to reverse. Hence the demographic transition in the West is likely to continue for the foreseeable future, with implications for family change all across the life course.

Notes

¹ The perspective taken here sees family change as a constant interplay between past adaptations and new demands. This interplay recognizes mutual causation, and there is no implicit functionalist teleology (Sober 1984). Adaptations that occur are not the only ones possible. Some changes may be adaptively neutral, and some may be adaptive in the short run but dysfunctional in the long run. Indeed, it is elementary that populations that persistently undervalue parenthood will be progressively less represented by their descendants in successive generations.

² One might attribute this conclusion to either another variety of the ethnocentrism seen in assumptions about the Victorian family in the work of the early sociologists or a functionalist teleology

that survival requires limits to individuation. There is a good deal of ambivalence, however, in his discussion over whether the process is *toward* the conjugal family form or more simply the working out of individualizing forces.

³ We must distinguish theories about particular family forms from the more general argument about industrialization and reduced family functions. Goode (1963) was careful to make this point; but as Thornton (1989b) indicated in a recent presentation at Madison, the debate over the role of extended families in Europe has often tended to discard the general proposition on the basis of evidence on very specific subhypotheses.

⁴ My thanks to Jim Weed for pointing out our error in adjusting upward for the difference between the divorce registration area and all divorces, since divorce totals are used in the National Center for Health Statistics procedures.

⁵ Based on duration-specific rates for 1980–1984, adjusted for underreporting (see Castro Martin & Bumpass 1989).

⁶ Some argue (Emery 1982) that it is the conflict signaled by divorce, rather than divorce itself, that is relevant and that children in unhappy intact families are no better off than those in single-parent families. Although it is surely likely that children in unhappy families are less well off than those in happy families, the evidence is increasing against this simple selection hypothesis (McLanahan et al. in press).

⁷ Schoen and Owens (1990) estimated that the rate of marriage after cohabitation is declining.

⁸ Among unions of less than 10 years, the proportion indicating that the likelihood of their separating is even or higher is 14% among married couples who cohabited with each other; among cohabiting couples, it is 18% if they both expect marriage, 33% if one partner expects marriage, and 55% if neither expects marriage.

⁹ Bennett (1990) estimated that 11% of women will never marry, based on an application of the Coale–McNeil model to the National Survey of Families and Households data. We get almost identical results using a period life table, but this contrasts sharply with the much higher levels never marrying implied by the total first marriage rates that are commonly reported by the National Center for Health Statistics (1990)—699/1,000 in 1987—and commonly found in the literature (Sardon 1986). What seems to be ignored in the use of these rates is the extent to which period distortion by timing shifts is exaggerated in such total rates for a nonrepeatable event such as a *first* birth or *first* marriage than it is for a total fertility rate or total marriage rate or for period life table measures. This is likely so because the combination of age segments of experience from different cohorts (as if there were no dependence within a cohort) violates reality much more seriously in the case of nonrepeatable events. Obviously, the population at risk for a nonrepeatable event at any given age is the proportion surviving from preceding ages. Since the same age-specific data are required to calculate either the total first marriage rate or a life table survival rate, there seems to be no reason for the continued use of the former.

¹⁰ Based on calculations with the 1982 National Survey of Family Growth (Bachrach, Horn, Mosher, & Shimizu 1985).

¹¹ Bennett et al.'s (1989) finding of the marked negative effect on marriage rates of nonmarital fertility reflects this disinclination to marry among those who chose not to marry despite the pregnancy and birth, as well as a reduction in marriage chances because of the birth.

¹² A very high proportion of such conceptions are terminated by abortion, but those that are not are the major portion of births to unmarried women.

¹³ Among those with such hours, about two-fifths of this potential family time is spent with one or both parents at work.

¹⁴ Uhlenberg and Eggebeen (1986) argued that reduced investment in parenting is reflected directly in increased problem behavior of teens.

¹⁵ In the longitudinal 1970–1975 National Fertility Survey, decreasing agreement that working was harmful to children occurred primarily among those who worked in the intervening years despite original beliefs that it was harmful (Bumpass 1982). This suggests another instance of attitudes accommodating to behavior.

¹⁶ A woman's desire to work is largely unrelated to her economic circumstances. For example, the proportion wanting to work is the same (two-thirds) irrespective of a need index based on husband's income, and this is also true among mothers of preschool children. Freedman and Thornton (1990) suggested that consumption aspirations are fueled for many by the experience of having substantial discretionary income from part-time work while living in the parents' household during adolescence.

They note that the consumption aspirations of today's youth seem very unrealistic and are likely to result in a disillusionment that could well affect family behavior.

¹⁷ Goode made this point in 1963, and of course the trend has been facilitated by both equality of education and shifts in the structure of the economy (Oppenheimer 1970).

¹⁸ My thanks to Hal Winsborough for reminding me of the normative aspects of these processes. Many in our society may feel driven by social expectations for behavior that they are at best uncertain about. It could also be argued that even consumption aspirations are less directly hedonistic than they are normative compliance.

¹⁹ Labor force needs associated with low fertility (Bourgeois-Pichat 1981) are likely to reinforce changes in this direction.

²⁰ Wives prefer to be employed an average of 26 hours per week, compared with the 38 hours husbands prefer to work.

²¹ Based on the question: "During the past year, have you ever thought that your marriage might be in trouble?"

²² Waite and Goldscheider (1990) pointed out that this is exacerbated by a decline in the amount of housework performed by children—offsetting somewhat even the increased help from husbands.

²³ The proportion is only a fifth of the traditional "female" tasks, but outdoor and household maintenance ought to be included in such comparisons concerned with equity rather than simply gender roles.

²⁴ They agree that this is most unfair when she is working part time, which is consistent with males' likely increasing their household work more when their wife works full time rather than part time (Dutchin-Eglash 1988).

²⁵ Those whose parents have separated since childhood are intermediate at 29%.

²⁶ Among college freshmen "being very well off financially" was ranked as essential or very important by less than 40% in 1970 and by about 70% in 1985; "developing a meaningful philosophy of life" declined from three-quarters to about 45% over the same period.

²⁷ There is a nice overview of related issues in the chapter "Family Decline: The Career of an Idea" (Popenoe 1988).

²⁸ Not surprisingly, I find that I must keep returning to the themes of my predecessors: from Preston (1984) on children, to Menken (1985) on intergenerational obligations, to Farley (1988) on inequality, and to Presser (1989) on the competition between family and work. These addresses provide a welcome opportunity to engage in broader speculation and opinion than is customary in the daily practice of our profession. Obviously, such opinions represent the individual biases of presidents rather than any official stance of the Population Association of America. Nonetheless, the recurrence of such themes illustrates the relevance of demographic research to much that matters to our society at large.

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