

# **DEMOGRAPHIC DESTINIES**

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

### **Interview with Jane Menken PAA President in 1985**



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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Merchant (2016 to present), and Win Brown (2018 to present)

## JANE A. MENKEN

PAA President in 1985 (No. 48). Interview with Jean van der Tak at the Population Studies Center, University of Pennsylvania, Philadelphia, June 13, 1988.

**CAREER HIGHLIGHTS:** Jane Menken was born and grew up in Philadelphia. She received the B.A. in mathematics from the University of Pennsylvania in 1960, an M.S. in biostatistics from Harvard in 1962, and the Ph.D. in sociology and demography from Princeton in 1975. She was an assistant in biostatistics at the Harvard School of Public Health from 1962 to 1964, mathematical statistician at the National Institute of Mental Health in Bethesda, Maryland, in 1964-66, and research associate in biostatistics at Columbia's School of Public Health in 1966-69. From 1969 to 1988, she was at Princeton where, among other positions, she was Assistant and then Associate Director of the Office of Population Research and Professor of Sociology and Public Affairs. From 1988, she has been at the University of Pennsylvania as Professor of Sociology and Demography and Director of the Population Studies Center (since 1989). She has consulted widely and as of 1991 is consultant to the International Centre for Diarrhoeal Disease Research in Bangladesh, chair of the National Research Council Committee on AIDS Research, and adviser for the Rockefeller Foundation Population Program and for the Demographic and Health Surveys. She received the PAA Mindel Sheps Award in Mathematical Demography and Demographic Methodology in 1982, was elected to the National Academy of Sciences in 1989, and had been a Council member of the International Union for the Scientific Study of Population since 1989.

Jane Menken is well known for her research and publications on the biological determinants of fertility and in the general area of mathematical demography, as well as teenage childbearing in the U.S., population and health in less developed countries, and population policy, among other topics. She is coauthor or editor of the monographs, Mathematical Models of Conception and Birth (with Mindel Sheps, 1973), Natural Fertility (1979), Teenage Sexuality, Pregnancy, and Childbearing (1981), and World Population and U.S. Policy: The Choices Ahead (1986), and author or coauthor of some 80 articles and book chapters.

**VDT:** Jane is just now [June 1988] in the process of shifting from Princeton to the Population Studies Center here at the University of Pennsylvania. However, in three weeks she's going off to Bangladesh for six months and you must tell us what you'll be doing there.

But let's begin at the beginning: How did you become interested in demography? You're one of the rare demographers who, like Raymond Pearl and Lowell Reed, moved into demography from biostatistics.

**MENKEN:** An even more interesting coincidence is that Bob Reed, the son of Lowell Reed [PAA President in 1942-45], was one of my advisers at Harvard in the biostatistics department. He's best known for his involvement with the Harvard Longitudinal Growth Study. He was one of the statisticians who worked very closely with that project for many years, but he was teaching at Harvard for a long time.

My own involvement began when I was an undergraduate here at Penn and realized as a math major that I was not interested in pure mathematics but rather wanted something that was more related to people. At that time, a friend was a Harvard medical school student, and at that time second-year medical students did courses in biostatistics, and his instructor was Mindel Sheps. He asked her what opportunities there were for people who were interested in applying mathematics in other kinds of ways, particularly to health. And Mindel, being the kind of person she was, not knowing me in any way, invited me to her home one weekend when I was in Boston and told me about the School of Public Health at Harvard. There I was, 20 years old and completely ignorant of any of these things.

She sent me off to the department of biostatistics. It became clear very rapidly that that was where I wanted to go to graduate school and I applied for admission to the master's program. That was my senior year at Penn.

I went to the School, which at that time was geared toward mid-career students, by and large. More than half of the student body came from outside the U.S. and most of them from the developing world. I was age 20 by that time and even with me, the mean age in our class was 37. It was a real education. There were people who had been working in developing countries. One of my classmates was a nun who was a physician and had worked in Latin America and Africa. Another had worked in family planning in Argentina and Chile for many years. Others were involved in trying to develop trachoma vaccine in Saudi Arabia. So, it was an education in and of itself.

After I received my master's degree, Reed hired me to work in the biostatistics department, which I did for two years. One thing I became involved in was helping in the analysis of the Khanna study, which John Wyon and John Gordon and then Bob Potter were analyzing. This is the long-term study in the Punjab in India which introduced family planning and health programs in a number of villages. It was their work in population, combined with the work that the people who had been working in Latin America involved me in, that drew me into population research.

At the same time--before I got there, Mindel and Cecil Sheps had left Boston and gone to Pittsburgh--but I began reading the early work that she was doing and that she and Ed Perrin were doing on mathematical modeling of human reproduction and very clearly felt that I'd found something that I was both interested in and suited all my ideas about what I was most interested in doing.

The period at the National Institute of Mental Health [1964-66] was a diversion. For two years, my former husband was a clinical associate at the National Institute of Mental Health. It was an interesting experience.

**VDT:** You'd been married before you went to Washington?

**MENKEN:** Yes. I was married at 20.

**VDT:** You came as a married woman at age 20 to Harvard--child bride! It didn't seem to truncate your education.

**MENKEN:** No, not quite. I went to NIMH as a mathematical statistician. It was a fascinating experience educationally, because what they did was to hire people and put you in a room and say, "Think." I had nothing to think about at that stage of the game, without experience. But it made me realize how important research apprenticeship is for most of us. I was sitting there at my desk with pencil and paper; I even had access to a computer--I was a good computernik. I could have done other kinds of things. But I didn't have the ability to formulate the kinds of questions that were answerable or approachable by me, at that stage. I finally did get involved in other kinds of projects, but the first few months at least made it very clear to me that our educational strategy needs to be involving people in research projects and not assuming that people can automatically generate projects on their own. If you have somebody like that--the first few graduate students I was adviser for included Doug Massey [PAA President in 1996], who even at that early stage was somebody for whom all you needed to do was step aside and let him do what he planned to do. But that's not the case with a lot of people.

**VDT:** Were you an apprentice at the time of your master's in biostatistics from Harvard? You said you became involved later in the Khanna study; did your master's thesis have something to do with that?

**MENKEN:** No. I was always involved in working on other people's projects, but mainly as a

consultant on the statistical part rather than carrying through on a project. When I left, I didn't have access to the data that were there at that time.

**VDT:** But at NIMH you were given carte blanche at the beginning?

**MENKEN:** Right.

**VDT:** And you hadn't gotten your doctorate at that time.

**MENKEN:** It was a different era. We did get involved in a number of projects there and it was clear that although there were issues that I thought were important, they were not ones I was particularly interested in working on and that population studies were where my heart was.

Then at the time that we were leaving, I talked to people at Columbia University; I was interested at that point in going into a Ph.D. program in biostatistics. But the most important piece of information I got from the conversation was that Mindel was moving from Pittsburgh to Columbia. So I wrote and asked her whether or not she had positions available in her projects.

**VDT:** You were going to New York anyway?

**MENKEN:** We were exploring possibilities and that made New York infinitely more appealing. We're talking about a different era, in the mid-1960s. When people look for two jobs now, it's a very different situation.

**VDT:** It was so unusual then to have two people looking for two professional jobs?

**MENKEN:** We had no problem. It was clear that at most of the places we were interested in going it was possible for both of us to do that.

**VDT:** Your former husband was a medical researcher?

**MENKEN:** He's a neurologist. When we were ready to leave New York City after he'd completed his residency at the neurological institute, we went looking for university towns that needed neurologists and had good demography. The two that were at the top of our list were Chapel Hill and Princeton, and it was possible either place. As I said, it was a very different era.

It was a very different era also for women with children--in many ways simpler than today. What I was able to do from the time that our first child was born was to work part-time, to name my hours. It was unusual enough for professional women to be doing this and there was sufficient need for people to do those jobs that there were no problems in saying that one wanted to work part-time.

So, it's an interesting shift that there were many more barriers for women, but once you were within the barrier, there was more flexibility in some ways than there is today, when I think it's much harder to have an academic or research position where you're working, as I originally did, three days a week, six hours a day. I did that for nine months.

**VDT:** This was at Columbia?

**MENKEN:** No, that was first at NIMH.

**VDT:** Already in the government in Washington, they would accommodate you?

**MENKEN:** Absolutely. At Columbia I worked four days a week, the same kinds of limited hours, and I did that at Princeton for the first two years there. It's an interesting commentary.

**VDT:** It is indeed.

**MENKEN:** That while women's opportunities to reach higher levels of professions have expanded, the ways in which one has to do it have been through what were accepted as the male model. Rather than changing work, what we've done is change women to accept that kind of work pace. I think it's now very much more difficult to juggle children and work.

**VDT:** I think you're absolutely right. Washington right now is having a notorious case of a woman lawyer who has been ordered back to work four months after her baby was born, full-time, or else.

**MENKEN:** It's a major issue. I know how much my own career has depended on that kind of flexibility.

**VDT:** I didn't have this until page 2 of my interview schedule, but I'm glad you brought it up. Now back to Columbia.

**MENKEN:** I was a research associate, which was again flexibility; research associate was a Ph.D. title.

**VDT:** Although you hadn't yet started on a Ph.D.

**MENKEN:** I hadn't gone on to a Ph.D. at Harvard primarily because Harvard exams in statistics--I would have switched to the statistics department--were only oral exams. And as many people in the field know, I had a long history of being the shyest person on earth and knew that I couldn't face doing oral exams at that point.

**VDT:** So that's the reason you didn't do the Ph.D. at Harvard! Did you actually take the coursework?

**MENKEN:** I was getting a master's degree in biostatistics and they would take this as equivalent to the beginning years of the Ph.D. program in statistics. So, I could have gone on the next fall and taken the prelims at Harvard. It was not the coursework that kept me from doing that.

**VDT:** Has that now changed at Harvard or do they still have oral exams?

**MENKEN:** I don't know.

**VDT:** Isn't it rare in the U.S. to have oral exams? I know it's common in Europe.

**MENKEN:** It's still common in mathematics. And if you can't talk, you're in trouble. At Columbia what we started doing was the work that led to the book that Mindel Sheps and I wrote on Mathematical Models of Conception and Birth [1973]. We were working quite closely with Jeanne Clare Ridley and Joan Lingner, who were in Pittsburgh; we had a long-distance collaboration on that project.

**VDT:** Were Jeanne Clare Ridley and Joan Lingner involved in that book? I don't remember seeing their names . . .

**MENKEN:** No, they weren't involved in the more mathematical part. But there was a whole series of papers that we wrote together, looking at various aspects of mortality and fertility change. Jeanne was much more involved in writing the simulation model REPSIM, which was the basis of much of the work that we were doing in collaboration, whereas Mindel and I did things that were different, using mathematical analysis rather than simulation, and that's what led to the book. But they're very closely linked.

When Mindel and Cecil Sheps left New York to go to Chapel Hill early in 1969 and we left a few months later and went to Princeton, I intended to work at home and complete the book and had gone to see Charlie Westoff to ask if I could have access to the OPR library. When he heard what I was working on, he said, "Why do that sitting at home? Come and work here and work on your project." That's how I first went to the Office of Population Research.

Two years later, we had completed the book, or most of it, and I finally had decided that it was time to go into a doctoral program. I thought very carefully about whether or not I wanted to do it in statistics and decided instead to go through the sociology department, because I was much more interested in learning something about the determinants of fertility and about social science. So, I applied for admission to the department at Princeton.

An interesting sociological point about that: Norm Ryder and I were walking back from lunch one day and I told him I had applied and been accepted. He looked at me, and true sociologist that he is, said, "This will change our relationship!" He knew something about roles: colleague/colleague versus student/professor--a different relationship. I went through the program at Princeton.

**VDT:** You switched to being a full-time student or were you still assisting in some project?

**MENKEN:** No. Princeton only has full-time students; you can't be a part-time student there. Or, if you're a full-time student there, you can't have a job at Princeton as well; they can't stop people from having jobs elsewhere, I suppose.

I resigned and went into the program full time, and at the end applied for a job. There were no guarantees that there would be positions, but if they would want me back, I would want to stay there. But I immediately went back [after finishing Ph.D.] to be a research demographer. That was in 1975. Again, there was a great deal of flexibility in having a research position that made it possible to juggle different kinds of responsibilities. That made it far easier for me, so it was a flexible and good position for me to be in. My research interests continued in mathematical modeling.

**VDT:** You'd done your dissertation on Estimating Fecundability, one of the shortest dissertation titles I've ever seen.

**MENKEN:** Well, that's what it was on. What I was interested in doing was looking more closely at a piece of the reproductive process. Mindel and I had done a lot of work on models of conception rates and the time to conception in the book and I was concerned about looking at real data to try and see what we knew about fecundability from populations. I was interested in looking at both what kinds of data were available and what kinds of methods had been used and whether or not the methods yielded different kinds of results. So, the first part of my thesis was an extensive review, an attempt to really understand what kinds of methods had been used on data from as many places as I could find. Then, to apply the methods that were an extension of work we had done on methods to estimate it.

In looking for data, I was introduced to Henry Mosley, who was then--as he is again--at Johns Hopkins University. Henry had been at the Cholera Research Laboratory in Bangladesh, which is now the International Centre for Diarrhoeal Disease Research, and he and Lincoln Chen, who is now at Harvard, and several other people had done a study of birth interval dynamics that basically took the

same kind of framework that we were working with mathematically and attempted to collect data by following women, visiting them every two weeks and asking them whether or not they were pregnant, breastfeeding, menstruating, getting all kinds of detailed information. They had data from a small study of just over 200 women, who had been followed for two years, from 1969 to 1971, when the war broke out in Bangladesh and the data were curtailed. Those data were available and they had done some analyses, but they made them available to me. That was my first contact with the work that was going on in Bangladesh.

**VDT:** Was that the Matlab project?

**MENKEN:** Part of it. From that point on I was involved with some of the planning for projects there and worked with a number of people who are actually in Bangladesh. But it wasn't until four years ago that I felt I could leave and go for a month; that was the summer of 1984. I spent a month there in conjunction with a project that was basically an outgrowth of these studies.

So, my interest in Bangladesh and the reason I'm going there actually grew out of a very general interest in the reproductive process. It moved much more toward a continuation of that interest, but also an interest in developing societies in general. Some of the work that we'll be doing in Bangladesh I hope will lead to a project looking at women and development.

**VDT:** You've certainly chosen the country with about as many population problems as there could possibly be.

**MENKEN:** It's a far cry from looking at mathematical models to doing this particular research project. That leaves me, really, at the beginning of returning to Princeton [on the faculty], where the research we did covered a wide range of issues. It was, and has been, a wonderful place to work in collaboration with a number of different people and very fine students.

**VDT:** When did you start teaching? You said you were taken on as a research associate in 1975 when you got your Ph.D.

**MENKEN:** Charlie Westoff and I taught an undergraduate course every other year; as a member of the research staff, one can teach. We began it in either 1975 or 1976.

**VDT:** He would take one fall and you would take the next?

**MENKEN:** No, we did it together. We were always there together; one of us taught and one of us sat.

**VDT:** How interesting; I've never heard of that.

**MENKEN:** Team teaching like that? They teach courses like that. James Trussell and I for five years taught a statistics course like that. This was the basic introductory graduate stat course for students in the Woodrow Wilson School--public and international affairs and sociology students. We traded off. Team teaching is interesting. It was interesting in the small seminar class that Charlie and I taught, because when we came to a topic, he and I would have a dialogue as well as having one with the students, and they got quite different perspectives.

**VDT:** One would do the talking one week and the other the next week?

**MENKEN:** We talked on our own topics; we took what we were interested in or had more expertise

in and traded that back and forth. James and I did that in teaching statistics, in part to have discussions which showed that statistics was not something so cut and dried that there were no opinions and judgments that went into it. Sometimes we argued fairly seriously, so much so that one Asian student asked the then director of graduate studies why they made people who hated each other teach together! It also made for some funny moments, so much so that an economist who was teaching next to us one year complained bitterly about how could he teach macroeconomics when people were laughing so loudly next door. I think it was a very good way to teach and something that we enjoyed. Etienne van de Walle and I have team-taught in a research seminar here at Penn and I think it's been very good.

**VDT:** You think that Princeton was innovative in this system, or where does it come from?

**MENKEN:** Oh, I think lots of people do it. East-West Center [Population Institute, Hawaii] does it all the time in their summer seminars, by having usually two people who are the coordinators for their workshops and then resource people who are part of the seminar. A number of years ago, I was a resource person along with Ron Rindfuss [PAA President in 1991] and Larry Bumpass [PAA President in 1990]; Griff Feeney and Jay Palmore were the two East-West Center people in charge of the seminar itself.

**VDT:** A high-powered group. Was that over a period of several weeks?

**MENKEN:** Yes.

**VDT:** Lucky students!

**MENKEN:** It was a mixture of students, as the usual East-West summer seminars are. It was a mixture of people from Asian countries, most of whom are practitioners, and graduate students, both Asian and American, so you would have people from a wide range of backgrounds. I think in many of those situations, it encourages people to participate if you are already having people who are having interchanges as part of normal operating procedure. That was quite good.

Where do we go from here?

**VDT:** I'd like to ask a bit about your work. Of course, you are most famous for mathematical models and the famous work that you did with Mindel Sheps. Being at Princeton broadened your horizon--not that it needed to be broadened; you have many other interests in demography. I was intrigued by a statement--I suppose you've said it in other ways--you had in your recent article, "Proximate Determinants of Fertility and Mortality: A Review of Recent Findings," in Sociological Forum [Special Issue: Demography as an Interdiscipline, Fall 1987]. You said: "Although gaps in the knowledge of proximate determinants remain and continuing periodic measurement is necessary to monitor their levels and change, the primary need now is to improve understanding of the causal chain that determines fertility." You said that at the very beginning, you were interested in determinants of fertility. I interpret from this line that you feel still more needs to be done on the socioeconomic background to fertility change and family planning programs. Do you feel that's the work that still needs to be done, or do you feel there's still something to be said on proximate determinants?

**MENKEN:** I think there's always something more to be said on almost any research issue. But if you're talking about priorities, I think what we've done is come a very long way in understanding the proximate determinants. When we were working in the late 1960s on models of fertility, the common wisdom among physicians was that breastfeeding didn't matter, that it was an old wives' tale that there was any relation between breastfeeding and conception. So, we were working in a very different kind

of situation, even at that stage. By the time we were completing the book, my very strong feeling was that to go on and develop more and more elaborate models was certainly feasible but it was wasteful until we had some knowledge of what went on in the real world--that theoretical work guides the kinds of questions one must ask of the real world, and then you can go back and fill in your models again. And I think that we've done that on proximate determinants.

**VDT:** Using, in part, the World Fertility Survey?

**MENKEN:** Parts of the World Fertility Survey, yes. I would say that the work in Bangladesh has been extremely informative because they were able to do much more detailed prospective studies. I think we can get a great deal from World Fertility Surveys; we're never going to get very good data on fetal losses, because people just don't report them.

**VDT:** Unless you have a prospective study like the work in Bangladesh, which went back every two months?

**MENKEN:** The first one went back every two weeks and did pregnancy tests every two weeks, so they had good data.

**VDT:** In the same issue of Sociological Forum, Rindfuss, Palmore, and Bumpass ["Analyzing Birth Intervals: Implications for Demographic Theory and Data Collection"] say that despite looking at the now classic proximate determinants which John Bongaarts pointed out--age at marriage, breastfeeding, contraception and abortion--there were still variations by women's education, so something had to be missing, and the something had to be coital frequency.

**MENKEN:** I think that is one of the major unknowns. There's still no satisfactory explanation for why such a high proportion of conceptions in Bangladesh occur in a very few months of the year--I can't remember the figure. It's a very strong seasonal variation. There are people who've explored the nutrition hypothesis and that doesn't offer much explanatory power. In my own thesis, I explored separation of spouses, because there's a pattern where men are either working in fields farming or they're fishermen who are away. Even if you adjust for the number of days away per month, that very strong seasonal pattern remains, although slightly attenuated. I can think of no explanation but that of differences in coital frequency that are related to life style changes through the course of the year. But, again, that's very difficult to obtain data on. I tried for a long time to push for good studies of that. I can't think of any other explanatory factor at this point.

**VDT:** Except for sexually transmitted disease, perhaps?

**MENKEN:** I think there's much more disease that people have really gotten; there's more variation in sterility and in age at sterility than is commonly accepted. I would have said that we knew enough about sterility until a graduate student, Ulla Larsen, who was at Princeton and is now a postdoctoral student at Berkeley, worked on a procedure for estimating proportions sterile at each age that could be applied to survey data. She applied that to World Fertility Surveys from sub-Saharan Africa. And it seems to me that sterility in a number of populations is occurring at earlier ages and is more variable than I had thought in looking at data from Western countries.

So, I think, while I would stand by what I said--I would put higher priority on looking at what causes populations to change their fertility behavior, what determines the levels of fertility-related data--I still think that there are issues in proximate determinants that are worthy of study.

**VDT:** You yourself, of course, have obviously been interested in wider fields too, drawn in, for instance, by your work on family planning in the U.S. You've written a lot in Family Planning Perspectives, for instance, on teenage pregnancy.

**MENKEN:** You can't help that, being a demographer. A friend of mine who is a physician, who got tired and burnt out by years of practicing, became interested in legal issues in medicine. He's now, at age 50, a second-year law student. We were talking about what we were both doing and I told him about going to Bangladesh and about my interest in AIDS and in a number of different areas, and he said, "You know, after all these years, I finally know what being a demographer means. It's a license to go and do what you want to do." He said, "If I want to change my interest slightly, I have to go off to school for three years to become a lawyer."

I think that my interests do fit together in a weird kind of way. The interest in teenage childbearing came about really in interest in what the timing of fertility meant in more sociological terms. Contraceptive effectiveness, that's a natural outgrowth of my basic biostatistical interest in measurement and my demographic interest in measurement. I think we can't understand causes until we can measure what happens. I think one of the major problems with all of the discussion about AIDS today and about HIV prevalence comes down to a real need to get better measurement systems so we can talk about differences--measurement of AIDS cases and of the prevalence of the virus itself in different sub-groups of the population in different geographic areas. It seems to me we don't have any good idea of trends in incidence. Most of the information we have on sero-positivity comes from very non-random samples, to put it politely. They're convenience samples and it's very difficult to try and decide whether or not these are comparable measures and what it is that they actually do measure, if they're interpretable in any way.

**VDT:** Does AIDS come into the work you're doing in Bangladesh?

**MENKEN:** No.

**VDT:** I didn't think it could; I thought Bangladesh had the lowest prevalence of AIDS. But everybody else is interested.

**MENKEN:** Oh yes. I sit on a committee of the National Academy of Sciences that's looking at research needs in social and behavioral sciences with respect to AIDS. So I've spent a lot of this past year not doing my own research but reading a great deal and hearing a lot of presentations and evaluating a lot of studies. That's what led to the remarks I made at PAA [1988 meeting in New Orleans]. This year there was a panel on AIDS and I was trying to think through what demographers could contribute to research on AIDS.

**VDT:** You've certainly always been at the cutting edge of "hot" issues. There was your famous PAA presidential address ["Age and Fertility: How Late Can You Wait?", delivered at the 1985 PAA meeting in Boston, published in Demography, November 1985]. It was right on the button with "How late can women wait?" which is an increasing issue with U.S. women who are delaying childbearing to the ages when infecundity seems to set in. You set to rest some alarmist views, particularly of that French study that came out about that time--you become terribly sterile at about the age of 30 or 35.

**MENKEN:** Fall over the cliff.

**VDT:** Yes, something like the Haylick thing of longevity; at 85 everything falls off. Also, the thing I particularly liked about that speech was the woman in the middle--women caught in the squeeze. You

showed so ingeniously, demographically, the women who have children under 18 at the same time that they have parents 65 and over and how increasingly there are those women caught in that squeeze. I haven't heard too much on that since then; perhaps I'm not reading the right things.

**MENKEN:** Once again . . . What I was doing in that project and what Susan Watkins and John Bongaarts and I did in a later article that was in the American Sociological Review ["Demographic Foundations of Family Change," ASR, June 1987] was to do this based on models, because what we were saying was there are no data on families. There were data on the components--on marriage, divorce, mortality, fertility. That's what we put together to generate a picture of what would be happening on average. But we didn't have any data. There were no studies we could find that asked people of different ages, "Are your parents alive?" What we have done in this country was to collect data on households and not on families, and we felt very strongly that it was family ties that we were interested in.

The data collection by Larry Bumpass and Jim Sweet in the new [1987-88] National Survey of Families and Households is the first data set--becoming available this summer [1988], supposedly--that will contain sufficient information. The meeting I have after this is with some of my colleagues here; we're writing a proposal to begin to look at some of the issues of intergenerational responsibilities and to try and quantify who is in what kind of family situation. We plan to use that data source and a number of other kinds of things. But, once again, there's a limit to how much talking one should do before one really can go and look at what is happening in an area.

**VDT:** Were you yourself in that situation? Did you have elderly parents as well as your children growing up?

**MENKEN:** Yes.

**VDT:** Like most people do. I think it takes a woman to feel that that's a demographically researchable topic. Your interest in the "woman in the middle" grew out of your own experience?

**MENKEN:** Very definitely. I think it was experience of just talking with people, and when people of a certain age gather together and hadn't seen one another in a while, being struck by the fact that we were as likely to be talking about our parents, or possibly more likely to be talking about our parents than we were to be talking about our children. That the experience of facing problems of aging was not just a problem of a person who was aging him or herself but really was a changed family situation.

And we began to ask the question: How much has this increased over time and how many people are in this situation? Is it that we're hearing much more about this because there are so many more people who are having the experience of caring for or being responsible to parents at ages where in the past they would be the senior generation? We have to be very careful--we try hard to say that it's not just caring for, but that it's a changing social framework. We all know senior colleagues who show no signs of having fallen over a cliff at age 65, but who remain active and involved for many years after that.

**VDT:** They might get a little piled up. As you know, I've just been to Princeton to interview your former colleagues there. I'm interested that Ansley Coale [PAA President in 1967-68], though technically retired, is still very much involved, still in his office.

**MENKEN:** Of course.

**VDT:** And Norman Ryder [PAA President in 1972-73] expects to stay in his office when he retires

next year, because it has more wall space for his books. Things get a little piled up. Where are you going to put the next generation of professors?

**MENKEN:** I'm not worried about the offices. One reason Ansley retired before the mandated age was his own strong feeling that there should be turnover, that there should be room for people to move up. And he quite happily moved over to become professor emeritus, but with no diminution in his activity. Nathan Keyfitz [PAA President in 1970-71] is another example of someone in our field who has done more in retirement . . . I remember laughing that when Nathan retired from Harvard, he remained on the faculty half-time and at the same time he accepted a professorship at Ohio State which was two-thirds time; that was "retirement"! He has been returning to Indonesia, where he'd been 20 years earlier, and he's also in Vienna with IIASA [International Institute for Applied Systems Analysis].

**VDT:** Here at Penn you have Ann Miller, Ed Hutchinson, and Vincent Whitney, all still on the faculty as professors emeritus. They don't teach; do they occasionally come in?

**MENKEN:** Ann comes in all the time and Ed comes in on quite a regular basis; he's in his eighties and he's working on a book on vital statistics. [Dr. Hutchinson died in December 1990.]

**VDT:** Well, as I say, you're certainly into a hot topic there.

**MENKEN:** I think it's a very important one and I think it has both demographic and policy implications, many of them positive, some of them raising problems.

**VDT:** I'll be talking to Sam Preston [PAA President in 1984] tomorrow. I think he's putting into public view really for the first time the idea that the necessarily more attention to an older population means we're shortchanging our children. But that's not your topic . . . caught between the two.

**MENKEN:** I had a sabbatical for a semester when I was beginning the work that led to the PAA address and I was actually here at Penn a couple of days a week. Sam and I traded off. He spent a sabbatical at Princeton; I spent a sabbatical here. We were down the hall from one another working on both of these things at the same time and not knowing what the other was doing.

I think the two policy issues of major importance are what happens to people. The elderly population seems to be split into groups that are very well off and those who are poverty-stricken. Children seem to be more and more concentrated in a disadvantaged category, and unless we pay attention to that, we're risking the future, as the title [Children at Risk] of the National Academy of Sciences report on adolescent pregnancy stated; I think we're risking the future of our children.

**VDT:** Those are two policy issues in developed countries. You've also been involved in the policy issues of developing countries, where there continues to be rapid population growth. You were director of the American Assembly symposium on it [and editor of the resulting publication, World Population and U.S. Policy: The Choices Ahead, 1986], which was a reaction, I presume, to the U.S. policy turnaround at the 1984 Mexico City population conference: Population growth has now become a neutral phenomenon to be solved with a free-market approach.

**MENKEN:** Yes.

**VDT:** What do you think about that issue now? Has it simmered down? Of course, it [the 1984 U.S. turnaround] was followed by the National Academy of Sciences study [Population Growth and

Economic Development: Policy Questions, 1986], which didn't end up by saying population was a neutral phenomenon, but it did take a less alarmist stance.

**MENKEN:** I think the most thoughtful people always were of that view, that the population issue was blown up to far greater import than it merited. I believe it was Etienne van de Walle who responded to the comment in President Johnson's speech [1965] about dollars for development and population [roughly, \$5 spent on family planning is worth \$100 spent on economic development], which Etienne amended to what I think is a very good statement: that if there were \$20 to be spent on development programs, the program would be improved if one of those dollars went for population issues. That looking at population alone, unless there are other changes going on in society, you're not necessarily going to improve the lot of people; you're not going to improve the economic position simply by changing population growth without any other changes in society. It's always been appropriate, to my mind, to have this as a part of development effort, but not as the only effort.

**VDT:** You've answered my next question: What do you see as leading issues in demography over the years you've been involved? You've said that in this country, at least currently, it's the issue of distribution of the population by age, and in developing countries, you'd see it as . . .

**MENKEN:** Rapid population growth, still.

**VDT:** What are you going to do in Bangladesh this time? This will be the first time you've had six months there?

**MENKEN:** Yes, a long stretch there. In Bangladesh what you find is that, as in many other developing countries, there's a lot of data collection that goes on and much of the data sits unanalyzed. The attempt to increase expertise in this area has generally involved sending people for graduate studies either to institutions that are set up specifically, like the UN demographic centers in different regions, or to Western research institutions. So, people go away for three or four years at a time and when they come back to their own institutions in their own countries, they have loaded on their shoulders all of the responsibilities of policymaking and administration and then attempt to do some research with facilities that are not up to the standard they were led to expect in the major demographic centers of the world. Our response to this was to ask the question whether or not we could do the training within countries, working with people on research projects--an extension of the apprenticeship-in-research kind of approach.

In the project that I'll be working on, there'll be two research workshops this time. There was one a year ago for two months at the International Centre for Diarrhoeal Disease Research in Dhaka, in which we worked with people on the staff there, not choosing the research project, saying the research project had to fit within the center's research plans and that we would work with people; that we were not coming there to get research done for ourselves, we weren't going to be writing papers, we were going to be working with them on their research. Instead of normal classroom teaching--most of these people have some background in statistics or epidemiology--as questions came up, we would set up lectures on specific topics that seemed to be of general interest because they were appropriate for several of the projects at the same time.

But we began trying to work with people in actually carrying out research and in trying to set up collaboration. So, while I'm in Bangladesh this time, we'll be running one workshop this summer and another late in the fall. The one late in the fall will bring people from other research places in Bangladesh to the center. We do this as a pilot study for the possibility of doing a different kind of teaching and training.

**VDT:** Interesting! John and Pat Caldwell in their book on the Ford Foundation role in population [Limiting Population Growth and the Ford Foundation Contribution, 1986] mention that Penn formerly had less of a close tie than, say, Michigan did with a particular developing country, although you've had Etienne van de Walle in Africa in recent years, and that the relationship was mainly sending Third World students here. But now you're going there; work on research there.

**MENKEN:** Yes. I think it's very difficult to go there until there is an institution of some sort with which to work. There hasn't been that kind of thing in Africa yet.

**VDT:** I visited Marvellous Mhyloyi, who had studied at Penn, at the University of Zimbabwe in Harare. She was overwhelmed. Too many demands on her there; couldn't get on with her own research.

**MENKEN:** Absolutely.

**VDT:** Boy, she could have used some help from "home."

**MENKEN:** That's one of the things we hope we'll be able to do more here. What Penn has done is to bring people in from different African countries, and I think it's getting to the point where there's going to be a network of well-trained African demographers--unfortunately, not more than one or two in each country. I've been on the Rockefeller Foundation Population Program Advisory Committee, along with David Bell and Ron Freedman [PAA President in 1964-65]. The three of us have said over and over again that we can't simply train people and send them back with no support. What happens is what's happening to Marvellous, who is incredibly competent, and she's overwhelmed. She was at a meeting at Hopkins several months ago. We had a marvelous conversation; I had not known her well before. And I think that that's in general true.

**VDT:** She said to me, "I feel so alone." She needs support.

**MENKEN:** That's right, and I think what we need to do is to have programs that will allow people like Marvellous time to come back for a few months so they can bring their research and do it, or programs that allow people to go there.

**VDT:** She had done a survey of 120 couples in two different regions of Zimbabwe which showed, probably, far lower contraceptive prevalence than had been found in the Contraceptive Prevalence Survey [of 1984] which was high for Africa, but she had no time to analyze it.

**MENKEN:** That happens over and over again and I do feel that we educators in the population field need to pay attention to that.

**VDT:** Who have been some of the leading influences in your career? Obviously, Mindel Sheps. Tell me something about her. She was a biostatistician . . .

**MENKEN:** And a physician. I don't find it easy to talk about Mindel. She had a very strong social conscience. Right after World War II, she and Cecil went back to their native Canada, to Saskatchewan, and were involved in writing the first socialized medicine act for a province in Canada. They had very strong feelings about the plight of the poor; were very much involved in civil rights. So, she was a very strong influence in all aspects of life. She was somebody who was a wonderful friend, besides having a sharp and incisive mind, and a willingness to work and to encourage work in

many, many people. When she died, there was a memorial service in Chapel Hill, which turned out to be a gathering of her friends and relatives and I found out then how many lives she had influenced and how many people had benefited from her support. I mentioned earlier what she did for me when she didn't know me! Just simply by her interest in encouraging science and encouraging people in whatever way they could best use their talents.

**VDT:** She died in 1973, which was much too young.

**MENKEN:** Yes.

**VDT:** Did you have anything to do with setting up the PAA Mindel Sheps Award in Mathematical Demography, which you yourself won, naturally, in 1982? This has become such a heartwarming part of our meeting every other year.

**MENKEN:** It was her husband Cecil and her son Sam and his family who decided that they wanted to set up this award. Bernie Greenberg, who was then dean of the School of Public Health at Chapel Hill and who had been chairman of biostatistics and was a PAA member, did most of the negotiating with the Association to establish the award.

**VDT:** How about some of your other influences? Ansley Coale was your professor at Princeton.

**MENKEN:** Long before that, at Harvard, there was a microbiology professor, Roger Nichols, who was working in Saudi Arabia on the vaccine trials that I mentioned--Harvard had these people who'd go out to do field work and come back to analyze data. He came looking for someone to help with the statistics and help design some of the vaccine trials. I was a newly hatched master's degree recipient. I was doing some of the consulting there and became very interested in the project, and he encouraged me every step of the way, and remained for many years after he came back to Harvard somebody with whom I always talked about what I was doing, what I was hoping to do. He left Harvard a number of years ago, because his own interest was science education and he felt that science in this country really was not being taught well at early levels and became director of the Boston Museum of Science. And the morning of my PAA speech, which was in Boston, he came over and we had breakfast together and talked about it, and he said, "You've come a long way, baby!"

**VDT:** Lovely.

**MENKEN:** He was a very strong influence in my life. Even harder, he died a couple of months ago, very suddenly.

Yes, Ansley--and Charlie Westoff PAA President in 1974-75]--have been enormous influences, in quite different ways. Charlie has a great deal of concern for his younger colleagues and very frequently involves them in projects in ways that open up career opportunities for them. He was the one who came wandering into my office one day and said--it was during the days of the Commission on Population Growth and the American Future--he said, "I don't like what I see about teenage childbearing and health. Are you interested? Do you want to go find out about it?"

**VDT:** You were pretty early in your career when you wrote that important paper for the Commission report ["Teenage Childbearing: Its Medical Aspects and Implications for the United States Population," in Charles F. Westoff and Robert Parke, Jr., eds., Commission on Population Growth and the American Future Research Reports, Vol. 1, Demographic and Social Aspects of Population Growth, 1972].

**MENKEN:** I said, "Sure." He said, "You're a public health person; go find out about these things." So off I went to do that. And he has done that for a number of people and does it consistently. Ansley, by his example, his enthusiasm all the time for his research. Everyone laughs about Ansley, running around with these little scraps of paper on which he has a graph, saying, "Look at this!" The joke around OPR was he was even known to be found talking to the cleaning lady when there was nobody else available to share his latest finding. I think the quality of his intellect is only one of the facets of the man that makes him such a great person.

**VDT:** He has great physical bounce. He said he'd be in for our interview at 9:30 on his bike and he arrived within minutes of 9:30, on his bike, and it was pouring rain.

**MENKEN:** His wife has finally persuaded him to wear a helmet, but Ansley, in all kinds of weather, rides his bike.

**VDT:** And plays tennis, every day.

**MENKEN:** Absolutely, both he and Charlie. And Jim Trussell has been extremely important. We have shared a very good research partnership. One of the difficulties of the move I'm making [from Princeton to the University of Pennsylvania] is the distance that has been put on that kind of relationship.

**VDT:** Why are you moving?

**MENKEN:** I'm moving because I did not want to continue to live in a small town; I wanted to live in a city. And I faced incompatibility between my work life and life in a small town.

**VDT:** Protected environment?

**MENKEN:** Precious.

**VDT:** Ah, good way to put it. Ansley Coale simply explodes at the traffic in Princeton; it's horrendous compared to some 60 years ago when he was an undergraduate.

**MENKEN:** None up until four years ago. You could just watch the traffic around Princeton increasing month by month. It was a decision that was a long time in coming and a very difficult one to make. I thought about it for a very long time and finally had to come to the decision that I had to move and try something else, so that's a very tough decision. However, given that I was moving, I'm delighted at where I've landed.

**VDT:** You'd worked already, of course, with people here, at least with Sam Preston--you've been coauthors of many studies and articles--and with Frank Furstenberg.

**MENKEN:** Yes, we've all worked together in various combinations. So, there are people with whom I already work well and others with whom it's quite clear that there will be increased collaboration.

One of the special appeals for me of coming to Penn is the presence of a medical school and a greater health component to the university. Doug Ewbank has been teaching a course in clinical epidemiology in the medical school in conjunction with a program there and I've been teaching in that program and we plan to continue. There's a project being set up on AIDS in Philadelphia and we'll be

involved in trying to estimate . . . I've already been working with a young medical student trying to design a study to look at newborns, where they take blood samples for testing for metabolic diseases.

**VDT:** Any other colleagues who've been outstanding influences?

**MENKEN:** Ron and Deborah Freedman, just in their general enthusiasm for things that they do. John Bongaarts and I have collaborated.

**VDT:** You've never been in the same university with the Freedmans.

**MENKEN:** Nor with John. And, until now, not with Sam or with Frank Furstenberg. I think that's one of the beauties of the field, that we can do things across universities.

**VDT:** It doesn't require living in the same place. It's a small enough field still?

**MENKEN:** I think so. John Knodel and I talk a lot about our shared interests in working in developing countries and that's been important. Henry Mosley, with his work on health and population, has been an enormous influence on my own interests and work. People at CDC, Centers for Disease Control, who are working overseas. Those are really the major ones, I think, within the field. Nathan Keyfitz, who is part family. His daughter is married to my brother.

**VDT:** Interesting! How did that come about?

**MENKEN:** Not through anything we did; they met in New York. The funny story about that one is that my brother, Marty Golubitsky, wrote a paper on mathematical demography that appeared in Theoretical Population Biology when Nathan was one of the editors. One morning I had walked into my office, picked up my mail, and found a request from Nathan to review a paper. I looked at it and burst out laughing; it was my brother's paper. So, I wrote back to Nathan, saying as soon as I saw the title page I knew it was a marvelous contribution, but I thought he needed another referee, and signed it Jane Golubitsky Menken.

**VDT:** Now your students. You have a wonderful reputation for mothering your students. Who have been some of your students that you're proudest of so far?

**MENKEN:** I have wonderful students and I hate to . . . Okay, my first student was Jim McCarthy.

**VDT:** At Princeton--the first student whose dissertation you monitored?

**MENKEN:** Yes, that I supervised. The second student was Doug Massey. There was a little group at that point that was Jim McCarthy, Doug Massey, Susan Watkins.

**VDT:** Was she your student?

**MENKEN:** No, she and I had become friends when she first came to Princeton as a visitor, before she became a graduate student, and it would have been inappropriate for me to supervise her, so I stayed away from that. Those two, Jim and Doug, were incredibly self-starting and bright and interested; they were wonderful to work with. But I've had a long series of extremely good students. Jill Grigsby, who is at Pomona College. They've gone off to do quite different kinds of things--some to work for

foundations, some to work in developing countries, some to combine doing all of those. Most recently, Carolyn Makinson finished and she went to work for the Demographic and Health Surveys. She wrote a wonderful thesis on sex differences in mortality in Egypt. She spent a year living in Cairo and then in a village, living with families in both places, and locating data sources, collecting data, and then coming back and writing an excellent thesis.

**VDT:** This is an obvious question to ask any woman professor. Do you think that women really have an advantage in their relationship with their students, in part, the maternal instinct? Your predecessor here at Penn, Dorothy Thomas, had a reputation for being particularly a "mother" to her students. Of course, Sid Goldstein [PAA President in 1975-76] at Brown has a reputation for his "paternal" hand with his students. What do you think?

**MENKEN:** I really don't think that's true. I think there are differences in the way one handles relationships with students. I think of it as being much more personality rather than gender.

**VDT:** You're probably right.

**MENKEN:** I should add--when you ask about students--one of the things that was wonderful about Princeton is that the occasional undergraduate would find his or her way to the Office of Population Research--Princeton undergraduates all write a senior thesis--and we've had some wonderful ones. Michael Stoto, who is now at the National Academy of Sciences and had been at Harvard for a long time--got a Ph.D. in statistics there--was an undergraduate at Princeton and that's where his interest in population developed. Judith Seltzer, who's an assistant professor at Wisconsin [and later became PAA President in 2016], was an undergraduate in the program. There've been a number of people. Some of these go off and do other things, they don't all go into demography, but they've been wonderful to work with.

I think that one of the real rewards of being in a major research institution is the kinds of students one gets. One of the joys here at Penn is that by having a Ph.D. program in demography you can fit in many more people from developing countries, with many different kinds of backgrounds, than has been possible in the traditional programs that have been either through sociology or economics, where people may not have either the background or the interests to go through the standard programs.

I've been so impressed this year with the students here at the Population Studies Center, many of whom, like Marvellous, will go back and be swallowed up in their countries and we don't hear about them as major figures in demography, but they are going to be major figures doing the work of beginning to get better, or continuing to improve, demographic data collection and analysis in their own countries. And they are plenty smart and devoted!

**VDT:** Let me ask you, again as a woman in the field. You've obviously never felt any discrimination because you were a woman. You followed in the wonderful tradition of Mindel Sheps in your own biostatistics field. And, of course, in PAA and demography, there were the early women leaders, Irene Taeuber, Dorothy Thomas, Margaret Hagood. As people always point out, the three awards in PAA are all named for women, although now there will be a fourth one, for Robert Lapham. So, as a woman, you have not felt any discrimination? Or, on the other hand, was it an advantage?

**MENKEN:** I'm very aware of what other women have felt they faced in demography and in other fields. I think early exposure in such a masculine field as mathematics must have immunized me--and the experience of working with outstanding women.

**VDT:** Meaning Mindel Sheps?

**MENKEN:** Mindel Sheps and Jane Worcester, who was one of my biostatistics professors at Harvard, who was at that time a woman university professor. And there was just no question in her mind that she was doing what she wanted to do. I have never really felt that I was discriminated against. And I know I've been fortunate in that regard.

**VDT:** Do you think perhaps there was an advantage?

**MENKEN:** Yes. I think there was an advantage in the sense that men would not have been allowed to have the weird kind of career pattern I had; they would have been excluded from the academy long before. That women, perhaps because they weren't taken as seriously, were also given special options. That's possibly inappropriate, but it certainly was useful for me.

I really don't think about me as a woman in those situations, so that in many cases when I'm talking to women about the problems they face, I sometimes feel like I'm not a native of the same country. I think that I continue to behave as if I'm not going to be discriminated against. I think that there are men who have trouble accepting women as determined, aggressive, as all of us are who have chosen this kind of work. But by and large that's their problem, not mine.

**VDT:** Which accomplishments in your life, so far--because there are going to be still many more--have given you the most satisfaction?

**MENKEN:** I can't answer that. I don't think like that. I don't count my life in chalking up . . .

**VDT:** Okay, you've already talked about accomplishments. One last thing before turning to PAA. In the PAA meeting just past [1988] in the Kingsley Davis session ["Two Centuries after Malthus: The History of Demography"], there was an interesting paper [by Jay Teachman and Kathleen Paasch] on a content analysis of Demography over 25 years. Most of your publications have appeared elsewhere and they pointed out that often women publish in fields, like family planning, that are better covered elsewhere. One thing they pointed out was that women seldom are single authors, and you have nearly always appeared as a coauthor. Is that because you're just very generous to others, or you genuinely do work as a joint author?

**MENKEN:** I prefer joint work and, therefore, joint authorship. I suspect that, if you count James Trussell's publications in the same way, his were equally coauthored. That's the way we work. I think that demography is a field in which many of us do coauthor. Phil Morgan [PAA President in 2003], who just stuck his head in here, has just been promoted to a tenured position here at Penn and one of the issues that came up about Phil Morgan was how many of his publications were coauthored. The same issue came up with Ron Rindfuss a number of years ago--his publications were mostly coauthored. I think we are a coauthoring field.

Now, it may be disproportionate--I didn't hear that paper and I don't know the statistics on that--but I think that for every example of women who coauthor . . . I think our field is built with men who coauthor; we tend to work that way.

**VDT:** Now on PAA. Can you remember which was the first meeting you attended?

**MENKEN:** Sure, it was 1967 in Cincinnati.

**VDT:** What was so outstanding about that particular meeting that you remember it so quickly?

**MENKEN:** Birdwatching.

**VDT:** Most unusual answer I've gotten from any of my interviewees!

**MENKEN:** Putting faces to all these names. I felt I should be wandering around with binoculars, looking at nametags.

**VDT:** Where were you by that time?

**MENKEN:** I was at Columbia. It was the first year after I started working there. I started working in the fall of 1966; this was the spring of 1967.

**VDT:** By then you were really absorbed in . . .

**MENKEN:** In population, yes. And so, to go off and begin to meet, or at least see and listen to, some of these people whose work I'd been reading, it was a wonderful experience. The following year the meeting was in Boston and Ansley was president and gave a talk on should the U.S. begin a campaign for fewer births. That was what really made me think about going to Princeton. I think the best PAA presidential addresses have come when people have really tried to pull themselves out of their normal research focus and to try and think of broader applications of work. And Ansley was asking a question, "Does what we know mean that we should be beginning programs that would have specific focus?"

**VDT:** "Should the United States Start a Campaign for Fewer Births" [published in Population Index, October/December 1968]. That was, of course, about the baby boom, though fertility had started coming down by then.

**MENKEN:** That's what he was saying. It was 1968--not all the data were available; we're always several years behind--but he was saying that everything was indicating that a decline had already begun and there seemed to be no need for intervention to change motivation in the U.S. It was such a reasoned approach to an important problem. And here was this person who was known for his esoteric mathematical kinds of work or his work on economic change and population change. I found it a memorable experience to sit there and listen to that one.

**VDT:** Can you remember other memorable PAA addresses or events over the years that stand out in your mind?

**MENKEN:** What I remember most after those early meetings is the combination that PAA has offered of meetings where people really go to sessions and listen to what's going on, that we view this as a real means of communication in the field, unlike meetings of other associations where you wander in and the meeting halls are empty and you're not getting that kind of intellectual stimulation or intellectual exchange that goes on at PAA meetings. So, I find the meetings themselves good. They have always provided very useful ways of doing our work. And then, they're just plain fun. Basically, I think demographers are interesting people to be around.

**VDT:** Though you weren't in on the early Princeton Inn days, that everybody regrets have passed, because there was one session at a time that everybody attended, "around the table"? But it's still the same flavor, you feel?

**MENKEN:** It's getting larger; I think it's getting more difficult to retain that kind of flavor. But I think it's there, more than with most other associations.

**VDT:** Did you, when you were president in 1985 and responsible for the program, make a concerted effort to see that some of that flavor was retained?

**MENKEN:** What we were focusing on mostly when planning the program was to cover the broad range of interests of people in the Association and to take into account some of the changes in interests, like state and local demography and business demography. I was, and am, a strong supporter of planning programs that meet the needs of people who are in those areas. Unlike economic demography or a field of substantive interest that fits within the traditional framework of fertility, mortality, migration, many of the issues that people in the applied fields face are not the subject matter of standard research papers. We needed to have different kinds of approaches that would allow them a forum. And I think the increase in the breakfast meetings, the introduction of Applied Demography, the newsletter, and the support now given by the Association to the extent of allowing, in the membership mailings, a checkoff for a subscription to Applied Demography are all good innovations.

**VDT:** Did that happen in your year?

**MENKEN:** It was being worked on. I can't remember whether it happened then or the next year, but it was certainly part of the discussion. I spent a lot of time with the committees on state and local demography and business demography.

**VDT:** Do you see that as the way that PAA will need to change? I suppose it always has to accommodate new interests and needs.

**MENKEN:** Of course, sure. It may mean that at some point there will be a need for a different organization. I hope that doesn't happen. I would much rather see the changing of applications or interests accommodated within the framework of one association.

**VDT:** In other words, you wouldn't like to see what's happening to the American Psychological Association, now it's about to split up into several different groups? You think it's all right having those meetings that do occur at the breakfast meetings, as you say, and on the day before, on the Wednesday?

**MENKEN:** Absolutely. And one of the things we did start was the Thursday evening sessions, where people who wanted special topics--what we did was give them room--could organize something. I think the Association should be open and receptive to the changing interests of the membership. And I think that can be done without diluting standards of the organization.

**VDT:** That sort of answers my question on whether there are changes you'd like to see in the program structure. You have said, to accommodate these changing interests. Some of my interviewees, going back to the early days at Princeton when it was possible to have informal debate in the sessions, have lamented the fact that there's now not time or seemingly the place to have more informal debate, that they have panel sessions now that have ended up being paper sessions. What about that?

**MENKEN:** I think that the roundtables were a good innovation, in that they allow for smaller discussion. I think that what we're stuck with is that the field has grown. If they wanted to stay with 50 people around a small table, then none of them should have had more than one student in their

lifetime!

**VDT:** Sam Preston has just come in for his mid-afternoon snack of M&Ms, sitting in a lovely glass jar on Jane's desk. That's a measure of the lovely atmosphere around here. Obviously, there's lots of informality and fun back and forth, certainly in a place like Penn's Population Studies Center.

One last big question. What do you see as the outlook for demography in the U.S.? You've just put your finger on one question I had: With applied demographers coming up, not just in state and local government but also in business, is there still room for the basic demographic researcher like yourself? Of course, there are still going to have to be professors, but you've just pointed out that all your professors emeritus are living on and on, spry and active.

**MENKEN:** I think the answer is yes and no. I think that funding for demographic research is going to be more difficult to obtain. I think we have benefited for many years by being a sexy topic and sexy topics have a way of declining in appeal, whether or not the problems go away or the importance of issues goes away. I was at an NICHD Advisory Council meeting just last week and for NIH grants from that institute, where most of our funding comes from, 19 percent of approved grant proposals will be funded this year.

**VDT:** That's even worse than I heard from Art Campbell [PAA President in 1973-74] a few months ago.

**MENKEN:** Yes. And I think that we are going to have to, in many cases, draw back. It's going to be much more difficult. I don't see an unending population establishment. I do see that more universities, more colleges, will be increasing their interest in having a faculty member with demographic interests. I mentioned earlier Jill Grigsby, who teaches at Pomona College, a small, very good college. I think that there will be opportunities for people who want to do undergraduate teaching. But I think, realistically, we face a situation in which demographers will have more difficulty in finding the traditional kinds of academic positions.

I do see that there's a greater interest in interdisciplinary kinds of endeavors, that there are more demographers in places like medical schools, and that may be a way for the future. I think we have to realize when we're training people that we're frequently training people who will be applied demographers in a variety of situations, whether they're working for government or for business or working in overseas agencies or a variety of different kinds of work situations.

**VDT:** For that will they need a Ph.D.? Will a master's do in some cases?

**MENKEN:** In some cases. But I think . . . I look at science policy and the way it's formulated, or people who are making decisions, and in many cases their knowledge of the area in which they're supposed to be making decisions is abysmal. I think that in the field of population, people who are in many of these other kinds of positions do need a Ph.D. I also think that, even if they move off into other kinds of areas, the knowledge they carry with them and the ability to ask questions and to work to find answers and the ability to ask questions of people who are trying to offer them solutions is sharpened by more years of academic training--twice the master's level training. I know that Bowling Green State now has an applied demography master's program and I think that there are places for that kind of technical training. I would say there's a place for more applied undergraduate training. But I still think there's a very strong place for a Ph.D.

**VDT:** Thank you very much, Jane. You must go to your meeting--and on to Bangladesh.

## AGE AND FERTILITY: HOW LATE CAN YOU WAIT?\*

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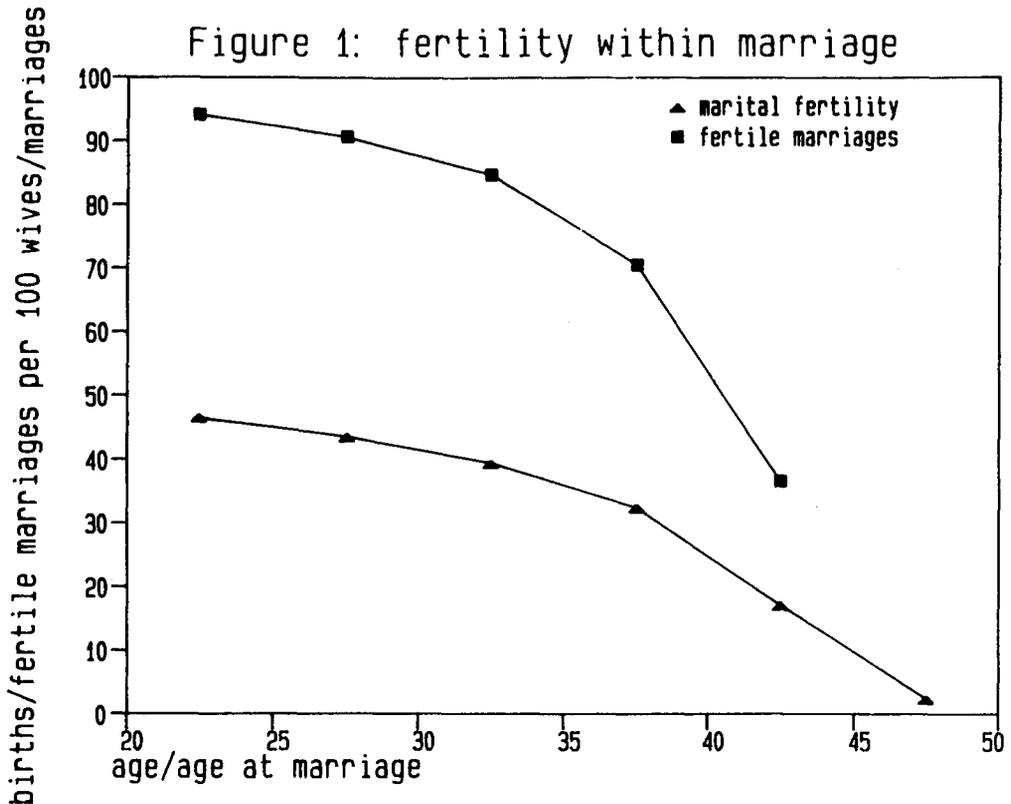
Today I would like to talk about two demographic issues, both of which may have far-reaching effects in the United States and neither of which is well-understood. The first of these is infertility, which has received extensive but often ill-considered attention in the press and other forms of popular media. The second issue is related, although the connections are not immediately apparent. This issue is the changing size of successive generations of mothers, daughters, and granddaughters, and how such change affects the family. This subject has thus far received little attention, but is also likely to have far-reaching consequences because demographic change has altered the boundaries of the expectations and obligations that parents and children have for one another.

Let me begin with the issue of infertility and what is said about it in the popular media. I use the term infertility to mean reduced ability to conceive and bear a live child. Popular presentations are often inaccurate, but they play a role beyond the province of the experts in defining what is seen to be a social problem. Perhaps the most common topic for articles has been the new and innovative techniques that can be called into use when old-fashioned copulation has failed to produce a wanted child—techniques like artificial insemination, surrogate motherhood, in vitro fertilization. Few articles have attempted to describe the facts about the extent of infertility, and the one that received the most attention, published in the *New England Journal of Medicine*, concluded that fecundity declines more rapidly with age than had previously been thought (Federation CECOS, Schwartz and Mayaux, 1982). This article was accompanied by an editorial suggesting that the shift to later ages of childbearing would at best have to be reevaluated if not reversed, and that women who cared to have children would have to revise their plans and have their children earlier if they were to have them at all (DeCherney and Berkowitz, 1982). Many later magazine and newspaper articles reported the sad stories of women, often high-powered career women, who postponed childbearing until they thought themselves ready to take on the joy and responsibilities of motherhood, and then found themselves far less successful in the bedroom than they had been in the boardroom.

Given this attention, it is useful to begin by describing what we *know* and, equally importantly, what we do *not* know about infertility. We know that in record numbers, women are remaining childless in the prime reproductive years. Let's talk about two age groups, women in their late twenties and those in their early thirties. In 1981 nearly 40 percent of the younger group were childless, as were over 20 percent of women in their early thirties (U.S. Bureau of the Census, 1983). In the previous twenty years, the proportion childless nearly doubled for the younger ones and increased by over 50 percent for the older women, but still has not reached the levels observed in the early 1940s (Heuser, 1976). Some of these women are surely

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\* The Presidential Address presented at the Annual Meeting of the Population Association of America, Boston, Massachusetts, March 28–30, 1985



postponing the birth of their first child, but how many will remain permanently childless, whether by choice or by chance?

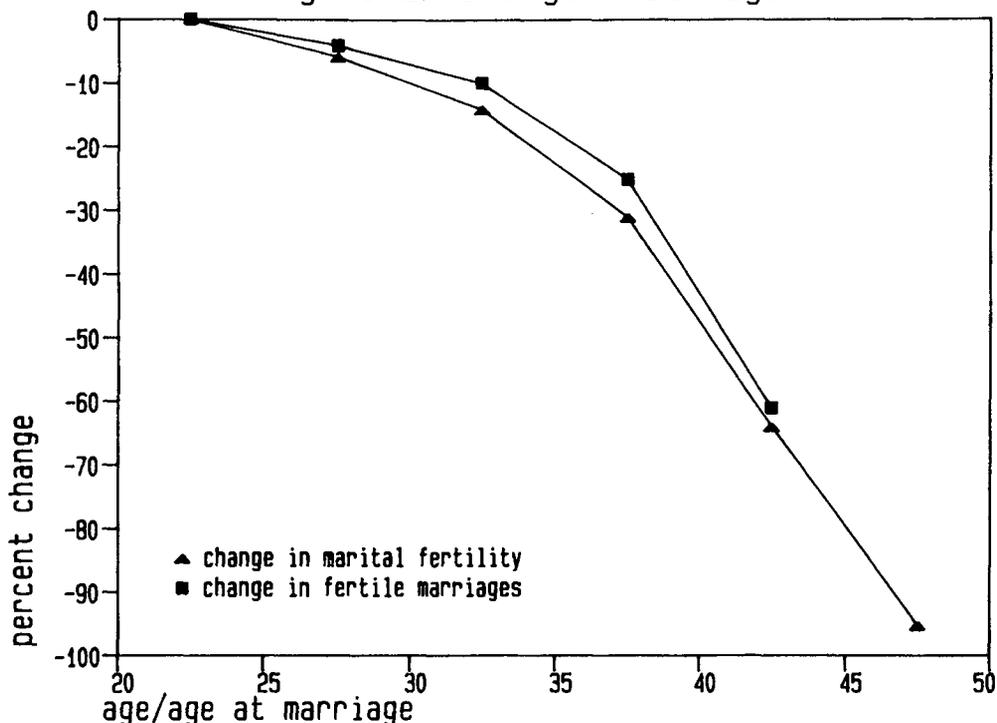
What do the women themselves say? Only 11 percent of women between the ages of 25 and 29 and 13 percent of those in their early thirties expect to have no children (U.S. Bureau of the Census, 1983). It is likely that more will remain childless than expect to do so. Some of those who do expect to have a child eventually will surely change their intentions, either voluntarily or involuntarily because divorce precedes the time they planned to have a first birth or because their hopes for establishing a family unit are not met. Still others will face infertility problems encountered because they chose not to become mothers at younger ages.

#### AGE AND INFERTILITY

What can be said about the decline in reproductive capacity with age? The study reported in the *New England Journal* by the Federation CECOS et al. (1982) was based on women undergoing artificial insemination and reported that the change in conception rates from the late twenties to the early thirties was slight, but significant. The accompanying editorial described the change as "striking" from age thirty on (DeCherney and Berkowitz, 1982). It is important to note that the resulting furor was based far more upon the editorial interpretation than on the scientific conclusions.

For an alternative evaluation of fecundity change with age, we cannot look to contemporary populations in developed countries for evidence because women who regularly have unprotected intercourse today are almost certainly selected for low fecundity. John Bongaarts (1982) and, later, Ulla Larsen and I (Menken and Larsen,

Figure 2: changes with age



forthcoming) turned to historical data for populations with little evidence of deliberate fertility control and reviewed the changes in their fertility with age. A summary of the results is given in Figures 1 and 2.<sup>1</sup>

To look at postponers, we examined data from populations in which women frequently married at later ages, a rather usual phenomenon in Northwestern Europe at least by the seventeenth century (Hajnal, 1965; Flinn, 1981). These women may be thought of as postponing childbearing as well as marriage. The upper curve in Figure 1 shows the typical proportion ever having at least one child, by age at marriage. For example, about 88 percent of women married when they were 30 to 34 had one or more children. The lower curve is typical of marital fertility, as obtained from populations Louis Henry (1961) described as having natural fertility. Marital fertility is reported as annual births per 100 married women in an age group. There is a consistent pattern of decline in both measures, though the levels of course are different. In Figure 2 the change relative to the early twenties is shown. The change by age at marriage in the proportions having at least one child is modest: four percent lower when the woman married in her late twenties, ten percent lower in her early thirties. Sharp declines begin only after age 35. The change is slightly steeper for marital fertility, as might be expected in the past, when conditions surrounding childbirth were at best septic and at worst brutal, and thus more likely to lead to gynecological impairment. The evidence, therefore, is persuasive that biological infertility rises, but only moderately, with age, at least until the late thirties and early forties.

The historical evidence thus sets upper bounds to the biological clock, since even in the natural fertility populations described here some childlessness could have

been voluntary. Moreover, some medical conditions contributing to subfecundity that could not be corrected in the past are now treatable. It is worth noting that the best evidence regarding this very modern issue came from an area frequently viewed as totally impractical and without application to the real world: historical demography.

As long as conditions now are no worse than they were in the past, women today who postpone childbearing are indeed taking some risk that secondary sterility, either theirs or their partners, will prevent them from having children. But that risk may, on balance, be reasonable in the context of the many life decisions they face, some of which will be discussed later.

This argument is incomplete, however, unless we can rule out the possibility that age-specific infertility has increased over time. We need to ask if the reproductive capacity of women who postpone childbearing until older ages is likely to differ from that of similar delayers in the past. Do they face hazards unknown to their ancestors?

Sexually transmitted infections were not uncommon in the past, as they are not in the present. But in the past the link between childbearing and sex was closer than it is now, at least for women. The woman who delayed marriage until thirty in 1800 was unlikely to have long experienced the joys of sex—or the risks of its attendant disease; today, the woman who delays marriage until thirty is highly likely to have been sexually active for some time, and many couples, whether married before or after thirty, delay having children. Thus, considerable sexual activity may precede the attempt to bear a first child, and those years of sexual activity carry some risk of reproductive impairment and that risk increases with the number of partners which either the man or the woman has had (Sherris and Fox, 1983; Cates, 1984). Moreover, there is persistent suspicion that women who have undergone abortions may have problems in later pregnancies, although recent studies do not support this contention (Hogue et al., 1982, 1983). IUD use, however, does appear to be associated with increased risk of pelvic inflammatory disease (PID), while pill use seems to lower the risk (Senanayake and Kramer, 1980; Westrom, 1980; Cramer et al., 1985). In fact, the main indirect evidence that infertility may be rising is the enormous increase in reported cases of PID in the United States (cf. Aral and Cates, 1983). Accurate data on the extent of PID and especially of PID-induced infertility however, are difficult to obtain and do not now exist. There is as yet no evidence that PID is especially high among those women who are deliberately postponing childbearing, especially if they are partners in monogamous unions.

Clearly, PID is a very real problem, one which deserves increased attention. Yet it is important to distinguish carefully between the effects of *disease per se*, and the effects of *postponing childbearing*. PID is treatable; aging is not. For most women who want to postpone childbearing, and do not care to abstain in the meantime—and it is not my intention here to recommend whether or not they do so—there is some risk of PID, but the diagnosis and treatment and even the prevention of these diseases is certainly feasible.

The concerns of women who are postponing childbearing are legitimate. Yet I think they are also easily exaggerated. There are at least three reasons why we might overestimate the extent of their infertility. First, over the past quarter-century the major question about fertility has been how to control *unwanted* childbearing, so that infertility problems have been overshadowed. Second, there is a compositional explanation: among the smaller group of women who would like to have children now, a higher proportion may indeed have infertility problems. Third, current medical practices tend to lead to exaggerated diagnoses of infertility.

First, consider how the issue of unwanted childbearing has overshadowed that of

infertility. Infertility in the past was surely a problem for individuals, as it is today. Yet a woman's sense of shame and inadequacy may have kept her quiet; her private tragedy could be overlooked in the concern over unwanted fertility, and adoption was a far more easily available solution then than now. Before the revolution in fertility control, it was all too easy to have a child when you *didn't* want one. In 1965, over twenty percent of all births in the previous five years were unwanted, and another 45 percent were mistimed (Pratt et al., 1984). When the urgent concern for so many was how to prevent *unwanted* births, there is little wonder that what I would call a myth of superfecundity gained currency. Mothers warned their daughters that unless they were protected at all times they would become pregnant immediately—once was enough, we were told. This was not always true, of course, but for many women, it does require strenuous efforts to prevent a birth, efforts which often were unsuccessful, efforts which overshadowed the public awareness of infertility problems. Now, fertility control is highly effective. Less than seven percent of births reported for the five years prior to the 1982 National Survey of Family Growth were unwanted, though the NSFG did find that over 22 percent were still reported as mistimed (Pratt et al., 1984). Unwanted fertility has virtually disappeared in much of the population, with significant exceptions, sexually active teenagers being the most troubling. Of course, all problems of fertility control have not been solved; satisfactory, effective, and safe methods are not always available; the large numbers of abortions are a measure of failure to control fertility in other ways; many may have chosen sterilization as the best among alternatives, none of which was fully satisfactory.

Yet, a subtle consequence of the success in preventing unwanted births is that fertility now appears to be more within individual control. Women, or couples, have, perhaps with great effort, been able to turn off fertility; they had come to believe that was the real problem and to expect that they could turn fertility on easily. It is hardly surprising, then, that couples who decide to have a baby frequently believe there are problems if conception does not take place within a few months. And women and men are no longer as willing to keep their disappointment to themselves.

Now I'd like to turn to the second reason, a compositional explanation, for an increased perception of infertility as a problem. Even if there had been no change in the age-specific prevalence of infertility, I will argue that other kinds of demographic change made an increase in infertility inevitable among the women who want to have a child. There is a selection process. Women who want no more children have chosen overwhelmingly to remove themselves from risk of accidental pregnancy by surgical sterilization, either of themselves or their partners. By 1982, nearly forty percent of all married women were protected in this way, with the proportions rising steeply with age—44 percent of all women in their early thirties, 58 percent of those 35–39, over two-thirds of women in their early forties (Mosher and Pratt, 1985). Women who are not sterilized fall into three groups: those who are trying to have children but have not achieved the number they want, those who are postponing childbearing, of either a first or later child, and those who do not want more children and have chosen to use imperfect, though generally highly effective, contraception or none at all. All three groups may be selected so that women at risk of childbearing are less fecund than women in a similar population where fertility is higher and surgical sterilization is far less common.<sup>2</sup>

In fact, the infecund comprise an ever-larger proportion of the older women in this pool. Thus, a large part of the public perception of problems of infertility may be compositional, due to successful fertility control by those who have the number of children they want.

Finally, under current medical practice couples are considered infertile if they have tried for a year without conceiving (c.f. Kleinman and Senanayake, 1979; Hatcher et al., 1982). A year is too short for this diagnosis. Even in high fertility populations a sizeable group will take more than a year to conceive. Analyses of data from historical populations and from modern infertility clinics demonstrate that among couples who meet the one-year criterion for the diagnosis of infertility, high proportions go on to have a child, even when their infertility problems are not treated (Trussell and Wilson, 1985; Collins et al., 1983).

An additional complicating factor is that the number of physicians interested in infertility has increased dramatically. While this increased supply most likely resulted from increased demand for infertility services, a likely effect was to stimulate increased consultation with patients about perceived or potential fertility problems (Aral and Cates, 1983). In addition, obstetricians are or believe themselves to be at high risk of malpractice accusation, and the high cost of malpractice insurance bears out their fears. What would be more natural than to issue warnings about potential fertility problems that would have been ignored in a less litigious age?

Therefore, it would not be surprising that a high proportion of postponers suspects or believes that at least one of the partners is infertile or subfecund. This is certainly the case for married childless women who have not been surgically sterilized. In 1982, of women in this group who were 25 to 34, nearly a quarter believed their fecundity was impaired (Mosher and Pratt, 1985). We cannot tell from survey data the extent to which these women suffer from actual, as opposed to perceived, fertility problems. But the increasing concern with infertility by the public and doctors alike leads me to suspect that the proportion of these couples who actually are sterile or even infecund is less than their own self-estimate indicates.

We may now be in danger of substituting a myth of infecundity for the earlier myth of superfecundity. To paraphrase W. I. Thomas, the husband of a previous PAA president, Dorothy Thomas, "If people define situations as real, they are real in their consequences" (Levy, 1966). If couples have become concerned that postponing the birth of their first child may make that birth difficult or impossible, we may see a reversal in the recent rises in age at first birth, and a return to at least slightly earlier childbearing. It would be disastrous if the myth of infecundity were to be interpreted as indicating a diminished need for family planning services: if anything, for teenagers the need for new and innovative programs to prevent early motherhood is urgent. The remarkable reduction in unwanted childbearing and the success of the contraceptive revolution in this country have been achieved only with use of contraceptives that are not completely satisfactory for all women and with high rates of abortion and sterilization. The need for continuing efforts to ensure and increase availability of the means of fertility control and to develop new methods is clear. At the same time, the successful use of currently available methods has also led to increased visibility and perhaps exaggeration of infertility.

Although the risks of postponement for individual women or couples do not appear to be high, there is an increased need for services, both medical and supportive, to help those who do experience infertility problems or who believe they do. It would seem only prudent to recommend that young people should be advised of the importance of assessment of their reproductive capacity while they are young enough either to seek treatment or to decide to have children earlier than originally intended. Last, it may be helpful for couples who have postponed childbearing to begin monitoring menstrual cycles regularly when attempts to become pregnant first begin, so that treatment can start sooner if problems exist and reassurance will be available if they do not.

I have tried to argue that infertility is a problem that may become far more visible when fertility is low and is more likely then to be perceived as a public problem. But why is fertility so low—at below intrinsic replacement levels for the past decade? And what are the implications of low fertility, voluntary or involuntary, for families? To turn attention to this question, I will need to bring onto the stage that other major force in demographic change, mortality, and I have chosen to tell the story mainly from the viewpoint of mothers and daughters.

#### THE EFFECTS OF DEMOGRAPHIC CHANGE ON THE FAMILY

Demographic change is assuredly not the only reason that the family of today differs from the family of the past, but it is certainly one of the reasons. Membership in a family depends on the kinds of events we as demographers study, and thus it is reasonable to try to estimate the impact of demographic change on family membership. But families are more than aggregates of individuals. Family membership carries with it a set of connections, of obligations, and of expectations that these obligations will be met. Every society defines, by custom as well as by law, what parents should do for their children and what they can expect from them, and what children can expect from their parents. Every society similarly spells out the obligations husband and wife have to one another. There is a private as well as a public dependency burden, and its locus is the family. As Norman Ryder (1983), John Caldwell (cf. 1982) and others have long suggested, many societies are in the process of redefining these expectations and obligations, and the contemporary United States is no exception. It is not unreasonable to speculate, as Kingsley Davis has done in the related area of sex roles, that one of the sources of these changes in the family is demographic (Davis and van den Oever, 1982; Davis, 1984).

How can we estimate what the decline in fertility and in mortality have meant for the family? Some of the information which I will use to suggest answers to this question comes from a study undertaken with Susan Watkins and John Bongaarts (cf. Watkins, Menken, and Bongaarts, 1984). Like others who have attempted similar tasks, we found that most data refer to households, not families. To describe the past and predict the future, we turned to simulation, using a model of the family developed by Bongaarts (1984a, 1984b). In developing these simulations, we were indebted to three groups of demographers: those who began the examination of kin availability through mathematical models (cf. Lotka, 1931; Goodman, Keyfitz, and Pullum, 1974, 1977; Keyfitz, 1977; Pullum, 1982; Goldman, 1984), those who have begun to reformulate existing data into a form appropriate for analysis of kinship networks (cf. Day, 1984; Kobrin, 1976; Wolf, 1984; Schoen, 1983; Schoen et al., 1985), and those who have attempted to describe more precisely the content of family ties (cf. Bianchi and Spain, forthcoming; Hagestad, 1984). If, as Pete Seeger (1963) said about song-writing, the best compliment is to plagiarize, I hope all of these people will accept the next-best compliment: we have borrowed liberally of their ideas and results.

I should express some trepidation about relying on computer simulations in this endeavor, which I will do with definitions from Joel Cohen (1985), who said, "A demographer is somebody who guesses wrong about the future of populations. A mathematical demographer is somebody who uses mathematics and computers to guess wrong about the future of populations." We may be using mathematics and computers to guess wrong about the past as well. Despite my trepidation, I believe the implications of certain types of demographic change are described well by these simulations and were not always obvious in advance.

There are five simulations, each using rates from a particular time period: rates for

Table 1.—Parameters <sup>a</sup> used for simulations

Parameter	Year				
	1800	1900	1940	1960	1980
1. Expectation of life					
Females at birth	40.0	50.0	65.4	73.1	78.1
Females at age 15	42.7	47.9	55.7	60.2	68.9
Males at birth	38.0	48.0	61.3	66.8	70.7
2. Marriage (females)					
Mean age	20.0	22.5	22.3	21.0	24.0
Percent ever marrying	95	90	96	97	95
Age difference between spouses	4.0	2.5	2.5	2.0	2.0
3. Index of					
Divorce	.10	.30	.39	.44	1.10
Remarriage					
from widowhood	1.00	1.40	.70	1.20	1.10
from divorce	1.00	.90	.90	1.20	1.10
4. Fertility					
Total fertility rate	8.00	3.70	2.40	3.60	1.80
Mean age of childbearing	30.0	29.4	28.3	26.7	27.0
Percent becoming mothers <sup>b</sup>	91.0	82.0	82.1	90.0	74.9

<sup>a</sup> The parameters are estimated from vital statistics, census data, and historical sources (see Watkins, Menken and Bongaarts, 1984, for details) and are used in conjunction with the following models:

1. West model life tables (Coale, Demeny and Vaughan, 1983)
2. Coale and McNeil (1972) model marriage distributions
3. Schoen (1983) estimates of period age-specific divorce and remarriage rates for 1975, multiplied by the index
4. Bongaarts' (1984a,b) adaptation of Coale and Trussell (1975) model fertility schedules

<sup>b</sup> The percent becoming mothers refers to women who survive to age 50 and is derived from the simulations

1940 and 1960 were used to represent the recent past. Estimates for 1800 and 1900 give a longer historical perspective, and rates for 1980 are a first guess for the future. An abbreviated description of the assumptions may be found in Table 1; details are

given in Watkins et al. (1984). The simulations spell out the implications of a set of rates; they do not tell the story of any real population.

We looked at whether or not a woman in a cohort was alive at each age and at the status she occupied in her family of origin and in adulthood. Was she a daughter? a mother? a wife? and was she more than one of these simultaneously? We also looked at a measure familiar to demographers—expectation of life at age 15, but in a modified form. We looked at the expectation of life as a daughter of living parents, as a daughter of elderly parents, and as a mother. In other words, we looked at a 15-year old girl and asked the following question: In her remaining lifetime, how many years will she spend, on average, as a mother or as a daughter.

Today I will not discuss the vast array of complex changes in family networks that result from marriage dissolution and remarriage. They do not affect survival of parents and seem to have little direct effect on childbearing, and it is women as mothers and daughters that I am describing.

To illustrate with a calculation I find particularly illuminating, Figure 3 shows the percent of women, by age, with a surviving mother. The changes at the older ages are remarkable. Considering only the shift from 1940 to 1980 rates, the fraction of fifty year old women whose mother would be alive jumps from 37 to 65 percent, an increase of three quarters. It is indeed new to human experience that a large majority of fifty year olds would still have living mothers. Although we did not do the requisite calculations, we can expect that a lot of them would still have mothers-in-law as well—or, given divorce rates, ex-mothers-in-law. One of the sad facts of contemporary life is that they are far less likely to have a surviving father. Figure 4 makes this

Figure 3: survival of mothers

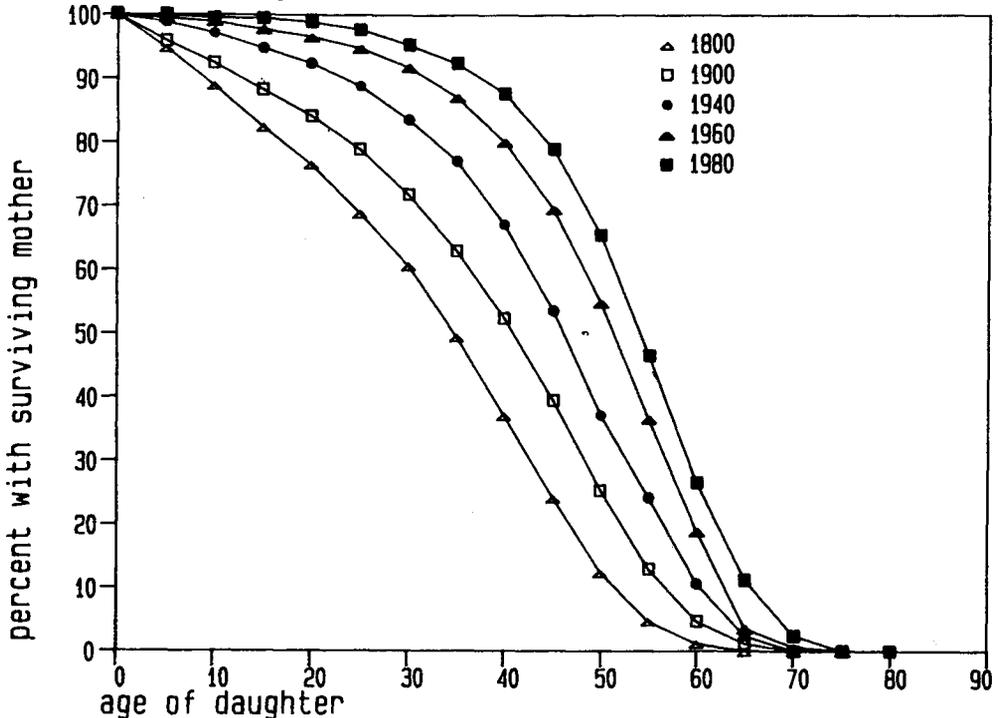
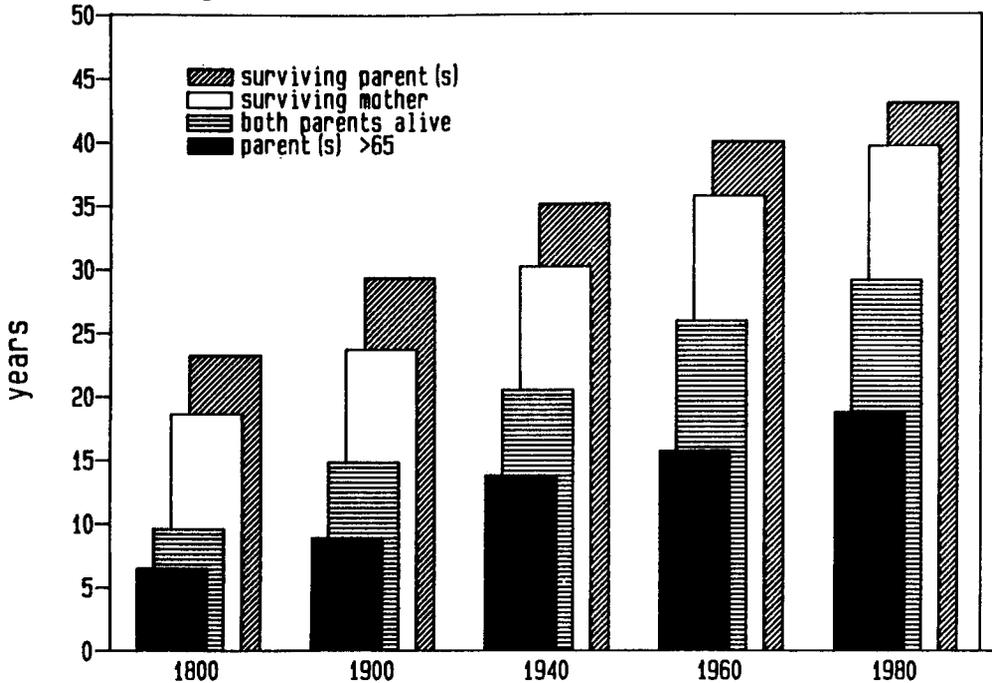


Figure 4: adult years as a daughter



point in another way, by showing for cohort members who survive at least until age 15, their remaining expectation of life with at least one surviving parent, with a living mother, with both parents, and with at least one parent who is over 65. The bulk of the difference between the expectation of life with both parents and with at least one is due to the premature deaths of men. The time spent with at least one parent over 65 increased from 13 years under 1940 rates to 19 years under 1980 conditions.

What do these kinds of figures mean in more human terms? First, we are the children of living parents for much longer, with all the implications of the kinds of connections that follow. The families we grew up in remain available to us well into our own middle age. Daughters can postpone childbearing and still expect that their children will grow up with lively and active grandparents. We experience the deaths of our parents at later and later ages: they are with us longer, for better or for worse, just as our own children will have us with them longer, for better or for worse. Although age 65 is an inadequate proxy for the starting point of the dependencies associated with biological aging, most of us can expect to be in a situation where our parents will depend on us in their old age, if not financially, then emotionally. Although we have not done the calculations here, we may expect our mothers to be widows for rather long times. If, as some speculate, better health care has actually increased the length of time during which an older person experiences increasing disability and greater need for assistance, the burden on adult children has increased, and the children may well be much older themselves when this obligation to assist their parents comes due. The empirical work describing the ways in which families are adjusting to the new demographic situation has only begun (cf. Shanas, 1968; Crystal, 1982; Soldo, 1980; Brody, 1985).

These changes in the duration of family ties are influenced almost entirely by lower

mortality. We can also look at the effect of fertility and mortality together on mothers and daughters. Again for cohort members who survive to age 15, Figure 5 shows the average number of years, or expectation of life, as mothers with children of different ages. Figure 6 gives the expected years of responsibility to the preceding or subsequent generation of dependents and to both simultaneously. I do not want to give the impression that "responsibility" is always a negative concept. These figures can and should also be interpreted as years with the special kind of connections and opportunities we have only with our parents and our children.

In these figures, children under 18 and parents over 65 are defined as the dependent groups; the responsibility measure given takes into account only whether or not there is at least one dependent, but not the *number* of dependents. The years with responsibilities to both older parents and young children simultaneously, we have called the years of overload.

In Figure 6, we can look first at the expected years with responsibility either to parents or to children. According to our simulations, the average decreased from 1800 to 1900, rose again between 1900 and 1960, and then fell slightly by 1980. Between 1800 and 1900, the fertility decline was sharp enough to counter mortality improvements that kept the women alive, as well as improving survival of their children and their parents. After 1900, the continued fertility decline (which was interrupted only by the baby boom), was outweighed by mortality improvements. This combination led to an increase in the total number of years of responsibility, although with a very different distribution between parents and children.

Let us look more closely at the implications of the 1960 rates, when the years of responsibility to both ends of the age scale are the greatest. These patterns may characterize real women, rather than just a synthetic cohort. These women are the

Figure 5: adult years as a parent

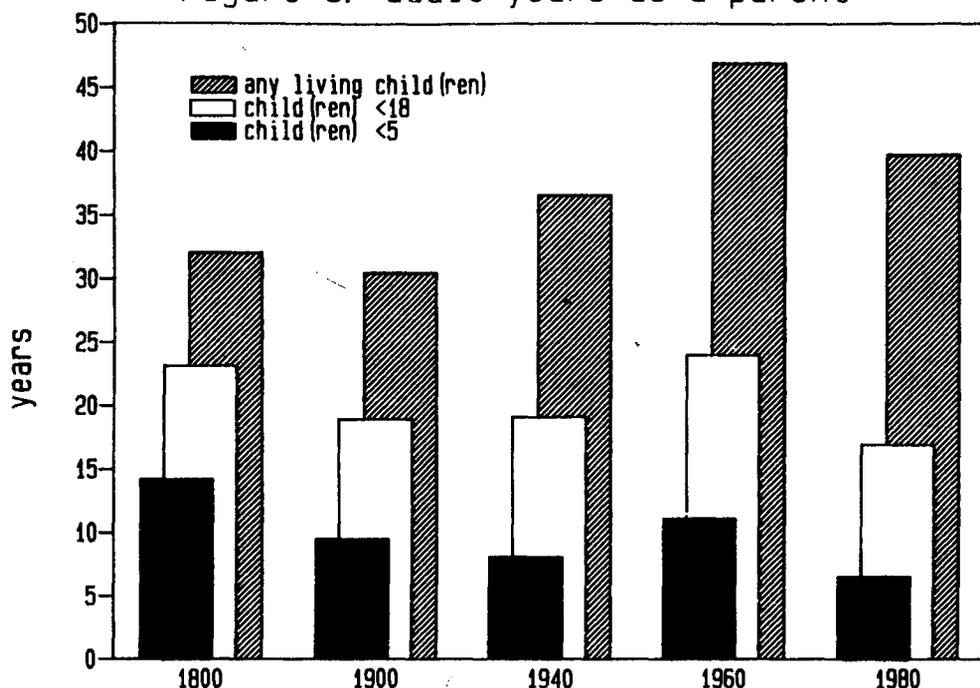
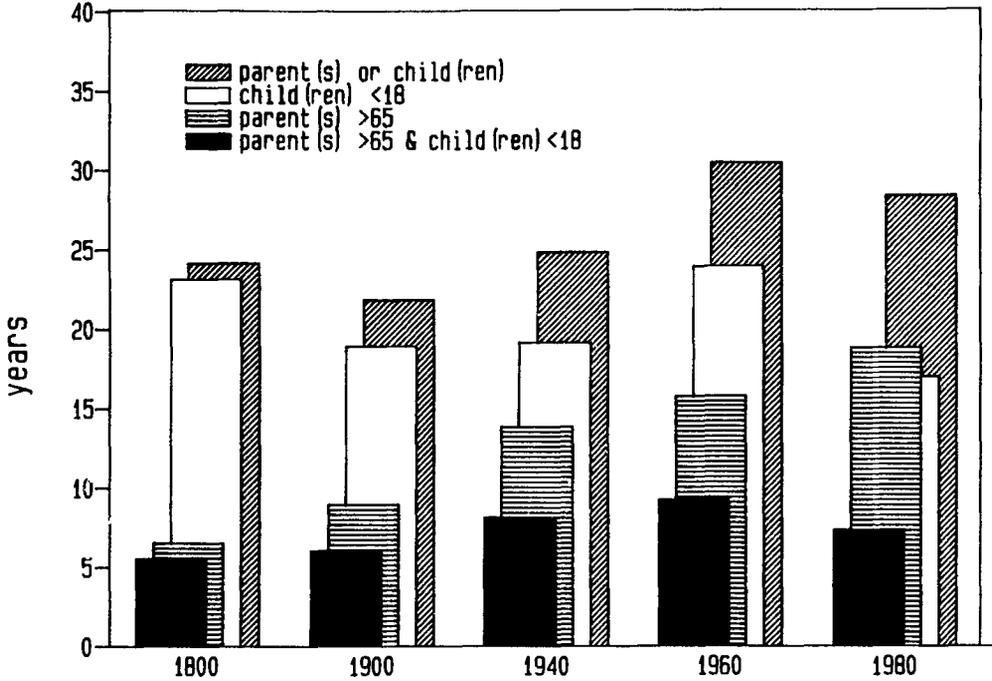


Figure 6: years of responsibility to dependents



ones who became mothers during the baby boom—many of them have, simultaneously, children under 18 and surviving mothers or fathers, and perhaps even grandmothers. The graphs do not tell the whole story, however, because the number of people who at any one time depended on these women had also increased. The women themselves are also children of the thirties, so that they had few siblings with whom to share their responsibilities to their parents. These are also the women who reentered the labor force in record numbers, so that for many of them the years of overload were magnified not only by duration and people, but also by moving outside the home and into the labor market. I don't know whether they were motivated by need to help support their large families, their children in college or beyond, or their aging parents, or by the need to provide for themselves and their children as the stability of marriages fell and as they faced their own old age with the realization that their mothers were economically vulnerable because of the inadequate provisions for women whose careers had been those of wife, mother, and homemaker, or if they were moved to enter the labor force by the need to break away from the feminine mystique. For whatever reasons, they have entered the labor force and, if the traditionally defined structure of obligations was in effect, they had an unusually high dose of responsibilities. Where could they look for help?

Last year Samuel Preston (1984) documented persuasively the shift in financial responsibility for the elderly from the private institution of the family to the public institutions of the state. The claims of women on the public sector have surely not been exhausted or satisfied, with the appeal for publicly funded day care being perhaps one of the most obvious of these unsatisfied claims. Some burdens, however, have not been shifted—family members, not state bureaucrats, are still next of kin—and other cares—such as the obligation to respond compassionately to a

mother's call of distress—cannot be shifted. What, then, can be done? Within the family, the evidence does not show husbands or brothers leaping into the breach. Working women still take primary responsibility for child and household care; women tend to be the primary caretakers for elderly relatives, including, in many cases, the husbands themselves (cf. Bianchi and Spain, forthcoming).

The next generation, as modeled in the 1980 figures, may be thought of as having tried one solution, whether or not they perceived it as such, to the dilemma of changing sets of responsibilities, or to the advantages of being part of the family unit established by their parents for far longer. They become mothers later, if at all, and have fewer children. The overall level of responsibility decreased between 1960 and 1980 because of postponement of the first birth and the increase in childlessness. I mention both here because, for an individual, they have quite different effects on the overlap. Reducing the number of children from one to none eliminates 18 years of responsibility and wipes out any overlap. Postponing childbearing increases the duration of simultaneous responsibility to young children and elderly parents. Given the longevity of parents, the number of years a person has living elderly parents still is greater in 1980 than in 1960, but the number of dependents is smaller because of low fertility. When only women who have children are considered, the overlap *increases* by more than three years between 1960 and 1980, from 9.6 to 12.8 years. What, then, are future cohorts likely to do?

They are unlikely, of course, to diminish support for attempts to reduce mortality even further, especially for men, and I see little likelihood that women will exit from the labor force. Thus they will continue pressure on the public sector for assistance, especially for publicly supported and well-run day care. They are also, I think, unlikely to increase their fertility much, at least in terms of the number of children per *mother*. It is possible that fear of infertility or other timing considerations may move childbearing back to somewhat younger ages and that childlessness may decrease. I would not rule out completely the possibility that fertility could decrease even further.

The dilemma expressed in concern for infertility may be a reflection of a more general uneasiness over how to deal with the burdens of responsibility associated with contemporary family life: the question "can I have a child?" may disguise another: "should I have a child?" Low fertility and voluntary childlessness have sometimes been presented as evidence of selfishness, even hedonism, among young people today. They may, however, be reasonable responses to the demographic changes that have altered the structure of family dependencies and led to questioning and perhaps redefining family obligations. We understand little about the causes of the sweeping changes in the contemporary family but have not looked seriously to demographic change and demographic constraints for many of the answers. Might it not be appropriate to interpret recent fertility change in part as representing an awareness of the lasting nature of families of origin and of the private dependency burden, the extent of responsibilities that we owe to our mothers, to our daughters, and to ourselves?

#### NOTES

<sup>1</sup> Details of the derivation of the curves in Figure 1 are contained in Menken and Larsen (forthcoming).

<sup>2</sup> In a low fertility society in which sterilization is common, as compared to a higher fertility population, women at any given age who are trying to have children are more likely to have fertility problems because the more fertile will have completed their families; the postponers are going to be older at the time they begin attempting to have children and therefore are likely to be less fecund than if they had decided to become parents earlier; and those who want no more children and believe themselves highly fecund are more likely to have chosen sterilization, leaving as preventers, whether or not they are contraceptors, women who may on average be less fecund.

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