

# **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)  
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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 in 1939 in Philadelphia, where she grew up. 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.

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# On Becoming a Mathematical Demographer—And the Career in Problem-Focused Inquiry that Followed

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### Abstract

I greatly appreciate this opportunity to reflect on my career. Looking back over five decades of involvement in demographic and sociological scholarship, I have tried to say a bit about my personal life and my work—from developing mathematical models of fertility early on, to applying lessons from those models to empirical work in the United States, Bangladesh, and elsewhere in the developing world, to involvement in evaluations of health and population interventions. Equally important to me have been the building of research capacity and involvement in program and policy development. So much remains for new generations of scholars to do, but my hope is that, in choosing their own directions, they—and sociology as a whole—will take as their mission examining issues of societal importance around the world.

## INTRODUCTION

It all started at the Harvard School of Public Health (HSPH) with the simple question: Why did fertility differ dramatically in populations that did not practice deliberate fertility control? This was the puzzle that led me to mathematical demography and determined my lifelong research path.

I had gone to HSPH in 1960, right after graduating from the University of Pennsylvania with a degree in mathematics and with a strong sense that my interests lay in applications more closely connected to social issues rather than in pure mathematics. As a biostatistics master's degree student there, I was asked to work with a group of epidemiologists who were conducting an experiment in introducing family planning and improved medical care in a group of villages in India, the Khanna Study (Taylor 1997). In the Indian subcontinent, women who survived to the end of the reproductive span (say age 50) had, on average, just under 7 children. At the same time, I learned that Mindel Sheps, who later became my mentor and dear colleague and friend, was studying fertility among the Hutterites who, in the same reproductive span, averaged over a dozen children (Sheps 1965). So was born the driving question: Why, when societies practiced little in the way of deliberate family planning, did fertility differ so dramatically? And what, then, were the consequences of such differences for women and for populations?

Sheps, with Edward Perrin, had begun developing mathematical models to study fertility. I joined her project at Columbia University in 1966. The theoretical model we explicated takes an individual longitudinal—or family building—perspective based on an interacting set of social and biological determinants. It is orthogonal to the long tradition of formal demography of population growth, which takes group level characteristics as its building blocks: population rates of fertility, mortality, and in more recent approaches, migration (Bourgeois-Pichat 1971, Coale 1972, Keyfitz 1968, Rogers 1975). Instead, we built on both sociological and sociodemographic work on the components of fertility—but more about the specifics of this modeling effort later.

## SOME PERSONAL BACKGROUND

Growing up in Philadelphia, I was a shy, tongue-tied, awkward kid. My childhood was colored by our family situation. Dad, a Russian Jewish immigrant, reached the United States in 1924 at age 13, after a difficult childhood of poverty, World War I, and the Russian Revolution. He brought with him the kidney disease that compromised his health for the rest of his life. For him, he said many times, life began when he arrived in Philadelphia. He was able to attend college intermittently and finally graduate, but in 1932, the best job he could get was as a truck driver for a dry-cleaning factory. Despite his chronic illness, he worked at this job and then added a small dry-cleaning store of his own. His patriotism evidenced itself in my life—as a child and preteen, I am sure we visited every historic site in Philadelphia and its surrounds. His positive example was one of courage and persistence. It also came in his support for my interests in mathematics. He never thought about sex differences in the brain. But he also firmly believed girls should not go to college and married women should not work outside the home. This stance, combined with his illness that at any time could bring total disability or death, made little sense to me. It forced me, early in high school, to think about women's roles in family and society in general and in my own life. I gradually realized I wanted not only to have a family and children but also to further my education and have a career that enabled me to use my intellect and support myself and my family. I am grateful to my father for his love, for his example of dealing with adversity, and for making me a feminist in the 1950s, well before the momentum of the movement in the 1960s.

Mom quietly supported my education. The Philadelphia inner city schools I attended were far from challenging, so I became a regular in the local library. One day when I was 10 or 11, I came

home and told her that the librarian refused to let me check out an adult book. Back she marched me. When the librarian said that the book was beyond my understanding, Mom asked her to pick out some paragraphs and let me tell her the meaning. That was the last time my choice of books was challenged. When my brother and I were cleaning out our family home after our parents died, I found a rubber-banded packet of my library cards (in those days, cards were stamped with the return due dates for the books) that Mom had saved. Her heart attack at age 40—when I was 15 and my brother, Marty Golubitsky, was 10—left her invalided for many years. Marty and I raised one another and that special bond continues to this day. Marty and his wife, Barbara Keyfitz, are mathematics professors, now at The Ohio State University.

My dreams would never have been realized without outside support, both financial and personal. A National Merit Scholarship enabled me to attend the University of Pennsylvania (Penn) while living at home. At Harvard, I had a National Institutes of Health (NIH) traineeship. My PhD studies in sociology and demography at Princeton University were supported by a Population Council fellowship. Unsurprisingly, I am a fervent supporter of efforts to create opportunities for the financially disadvantaged and to make higher education affordable.

I was fortunate to meet Matthew Menken, my first husband, at Penn. He went on to Harvard Medical School and I followed him to Boston. Matt and I divorced when our two children were teenagers and I remain grateful to him for our continued shared parenting. Ken (Yaakov) and Kate and their spouses are men and women of principle who are leading contributory lives of service. They are wonderful parents to their seven children and perfectly illustrate variation in childbearing—Ken and Tova Menken have six children and Kate Menken and Stephen Hanmer have one.

As a second-year medical student, Matt was required to take a biostatistics class. He asked the professor about opportunities for his then-fiancée (me), who was interested in statistics. It was my great good luck that Mindel Sheps was his professor. She invited us to her home, told me about HSPH, and introduced me to the chair of the Department of Biostatistics! Six years later, when we were considering a move to New York City, I learned that Mindel was also moving to Columbia University and wrote to her, asking for a job on her projects. And so began mentorship, collaboration, and deepest friendship.

Mindel's and my interests coincided; we thought in the same way about fertility and mathematical models. We spent three years together at Columbia and continued our work long-distance after leaving New York City. The experience of bouncing ideas across a table and then solving the mathematical problems we posed was exhilarating. We were able to live in a world of ideas based on the shared set of questions we wanted to answer and also a shared sense of humor—part of the acknowledgment in our book, *Mathematical Models of Conception and Birth*, reads “We wish that, like many other authors, we could acknowledge hours spent by our spouses in typing and checking the material. But Cecil Sheps and Matthew Menken did not read a page of technical matter. They were, however, unfailing in their interest and encouragement” (Sheps & Menken 1973, p. xxiii).

Mindel died of cancer at age 59 in 1973, before the publication of our book. In dedicating it to her, I wrote “She combined excellence in scholarship with warmth, integrity, a capacity to inspire close associates to explore their talents to the fullest, and a deep concern for social problems.” In a 1971 convocation address at the University of Manitoba, Mindel had suggested that

it would be well to approach all human problems with humility, with a strong sense of the limitations of our knowledge and of the existence of large areas of ignorance, and with readiness to admit the errors we may make. . . . The only hope of solving the problems of this planet lies in the application of scientific understanding and skills in the service of human dignity, freedom and welfare. Neither science nor high ideals can do the job alone. (Sheps & Menken 1973, p. v)

I have taken her words as the creed that guides my own work and life.

Charlie Westoff also played an important role in my career. As Matt and I readied to leave New York City in 1969, we looked for smaller towns with opportunities for a neurologist and a demographer. I planned to continue research with Mindel, who had moved to the University of North Carolina. I first met Charlie when we shared a panel at an annual meeting of the Population Association of America. I visited him to ask whether, if I were to move to Princeton, I could have access to the Office of Population Research (OPR) library while working at home. My recollection is that he looked at me and said “That’s the stupidest thing I ever heard. You’re coming here to *work*.” And that’s what I did.

I have been fortunate in all of the institutions in which I worked—HSPH, NIH, Columbia, Princeton, Penn, and now the University of Colorado Boulder (CU), in that order. NIH, Columbia, and Princeton all accepted part-time employment while my children were young. I knew I was following a risky path, but my disagreement with the philosophy of the women’s movement of the 1960s and 1970s was its emphasis primarily on women’s careers, with little attention to the needs of families and children. My mantra was and is: “I don’t want women to have men’s jobs [as traditionally defined]; I want people to have human jobs.” While enormous progress has been made in the more than 50 years since publication of *The Feminine Mystique* (Friedan 1963), much remains to be done to promote more symbiotic work-family relationships, whether or not family includes children.

Years before going to OPR, I had considered applying for the PhD program in statistics at Harvard, but I decided the time was not right. I had seen too many graduate students who were working on dissertations that were chores rather than passions. I did not yet know how to fit my own passions into a dissertation, so I decided to take a job as a better next step. After two years at OPR, I entered the Princeton sociology graduate program with a dissertation topic decided, having realized that no matter how good my research, my career choices were limited without a PhD. I returned to OPR in 1975, degree in hand, in a research position that allowed flexibility for me to be an active parent at a time when high-quality child care was even more difficult to find than it is today. By early 1980, I felt it was time to move to a full faculty position. With outside offers in prospect for me, Charlie Westoff, Ansley Coale, and then-chair of the Sociology Department Marvin Bressler persuaded the university to offer me a “target of opportunity” full professorship as the tenth tenured woman at Princeton.

OPR was an intellectually crackling place to be in the 1970s and 1980s. Ansley, Charlie, and Norm Ryder set the highest standards of research. Graduate students in those days included James Heckman, Doug Massey, John Knodel, Susan Watkins, James Trussell, Myron Gutmann, Hilary Page, and a list of others too long to give here. OPR was a center of the demographic world, with visitors coming through regularly and ongoing discussions on research and its relevance for policies and programs, and conviction that programs were better the greater the evidence base for their design and implementation. Collaborations with James Trussell and Susan Watkins, which continued over many years and a range of subjects, began during this period. Ansley and Charlie, then director and associate director of OPR, asked me to become assistant director. I discovered that I like research administration—meeting needs to establish and maintain an environment that fosters high-quality interdisciplinary research and mentoring.

My next position was equally fulfilling. Sam Preston and Frank Furstenberg enticed me to Penn in 1987, where I was director of the Population Studies Center for five years. A major attraction was Penn’s focus on African demography and on training young African scholars. In the mid-1990s, Etienne van de Walle claimed that more Africans with PhD training in population studies had received their degrees at Penn than all other institutions in the world combined. Sam and Frank, Etienne, Susan Watkins, Doug Massey, Phil Morgan, Doug Ewbank, Herb Smith,

Tukufu Zuberi, Irma Elo, Andy Foster, Jere Behrman—who could ask for a more talented and diverse group of population scientists than those at the Population Studies Center? The graduate students were also outstanding, and I continue to collaborate with Randall Kuhn, Sam Clark, Enid Schatz and Sangeetha Madhavan.

I was introduced to Richard Jessor in 1989 on my 50th birthday by Frank Furstenberg and his wife Nina Segre. It still amazes me that the love of one's life can show up at ages 50 and 65! His children and grandson are part of my family. Dick, who has been a CU faculty member since 1951, persuaded me to move from Penn to Boulder in 1997.

In 2001, I became Director of the CU Institute of Behavioral Science (IBS) and served in that position until becoming a research professor in 2015. Administration took much of my time for those 15 years; I am especially grateful to colleagues without whom my research during that time would have been impossible. IBS is an interdisciplinary social and behavioral science institute that attracts faculty and graduate students from all the social and behavioral sciences who work on problems of societal importance and carry out basic and translational research and outreach. Through the efforts of its outstanding faculty, IBS now has the National Institute of Child Health and Human Development–funded CU Population Center, the new US Census Bureau–associated Rocky Mountain Research Data Center, and the long-standing Natural Hazards Center. IBS-associated faculty who recently were or currently are Sociology Department professors include Del Elliott, Fred Pampel, Rick Rogers, Stefanie Mollborn, Jason Boardman, David Pyrooz, Ryan Masters, Lori Hunter, Lori Peek, Kathleen Tierney, Amanda Stevenson, and Sanyu Mojola. CU recognized our achievements and our potential for future research by funding the new IBS Building that opened in 2010.

## RESEARCH INVOLVEMENTS

My research career began in 1966 with my collaboration with Mindel Sheps. Its focus was on developing theoretical foundations for understanding fertility and family building. We learned that plausible but quite different sets of basic building blocks led to near-indistinguishable fertility patterns. For this reason, while I continued to create mathematical models to address specific questions, my interests turned to methodology and population-based studies of the determinants of fertility. These interests, in turn, led to my work in Bangladesh. Bangladesh in the early 1970s was characterized not only by high fertility but also by the unusual situation that women had higher mortality than men, so that understanding the position of women and its impact on them and their society became part of my research agenda. Finally, and also because of my experience in Bangladesh, the latter part of my career has focused on evaluation of interventions intended to improve the lives of people in developing countries.

## Theoretical Models of Family Building

When I first entered graduate school in 1960, the rapid rates of population growth in recent years were unprecedented and led to intense public policy concern. Arguments for policies to reduce growth go back, of course, to Malthus and notions that the world was going to run out of sustenance. The mid-twentieth century formulation went further—that population control would lead to social and economic benefits for countries and the world (Natl. Acad. Sci. 1971). Mortality decline was occurring nearly worldwide, so fertility, increasingly seen as the primary determinant of population growth and structure, became the policy focus. The view of fertility decline for the good of the world shared uneasy space with what I will call the Margaret Sanger view, which comes from concern for health and wellbeing of women and babies. Its claim was that health and

life chances of mothers and children would improve if there were better child health programs, increased intervals between births, and fewer children in a family. Becker combined these views to some extent in his quantity-quality tradeoff theory of the family: that having fewer children would permit greater investment in their quality, e.g., human capital, health, etc. (Becker 1981). A third view, aligned closely with the Margaret Sanger position, is one of reproductive rights and reproductive choice: that women and couples have the right to have children when and if they chose to do so (Cohen & Richards 1994). In some cases, population control was unfortunately associated with the eugenics movement and with coercion.

A somewhat startling gap in early population policy was failure to recognize that successful efforts to reduce mortality made reducing the growth rate more difficult. Fertility had to drop sufficiently to overcome any decline in mortality.

For Mindel and me, our professional work was firmly guided by a desire for scientific understanding. Implicit in our research was also the search for evidence that could inform public policy. In that regard, I served on the Guttmacher Institute Board of Directors for more than 15 years, beginning in the early 1980s. I shared fully its institutional values, which are “Commitment to rigor in . . . research and analysis and to publication of . . . findings, whatever the ramifications for policies and programs may be; prioritizing the needs of disadvantaged groups and addressing inequities in access to and use of information and services; addressing emerging questions to promote understanding and shape the public debate; collaborating with others to maximize the reach and impact of our research and analysis” (<https://www.guttmacher.org/about>).

In the 1960s, scientists in many fields were motivated to understand dynamics of population change. These dynamics, like most social processes, are difficult to study. There are no laboratory approaches; analyzing actual life situations, as in countries with intensive family planning programs, is difficult because of the inevitable presence of a large number of uncontrolled variables. Such realities account, at least in part, for interest in mathematical and simulation models as a way to investigate population dynamics. They also influenced my later empirical work in developing countries, work which has sought and implemented ways to measure fertility and its determinants and to address questions of whether, and how, reduced fertility indeed benefits individuals over their life course.

Mindel and my family building models followed the pioneering work of Henry (1972a,b), Vincent (1961), Perrin (Perrin & Sheps 1964), Potter (1963), Blake and Davis (Blake & Davis 1964, Davis & Blake 1956), Gini (1924), Pearl (1933), Dandekar (1955), Brass (1958), Tietze (1956), and others. In explaining human fertility and its variation, we, like others, assumed that social, economic, and psychological factors are the principal determinants, but they can only operate within biological structures and limitations.

Our models were based on a framework that included a few events of known importance and the intervals between them. The reproductive span is an interval that begins with the later of biological capacity to conceive (menarche) and initiation of sexual relations and ends with the earlier of sterility and cessation of sexual relations. Within that span, there are successive live births and the intervals between them. A birth is followed by a postpartum period that ends when both ovulation and sexual relations have resumed. Next, there is the interval to conception, often referred to as the waiting time to conception. This interval depends on fecundability, the monthly probability of conceiving. Next is a pregnancy interval that ends either with a live birth, stillbirth, or abortion (spontaneous or induced). In the case of a nonlive birth, there is another waiting time to conception and pregnancy period. There may be several of these waiting times until the live birth that ends the birth interval occurs. In most cases, both pregnancy and postpartum periods associated with a live birth are much longer than when pregnancy ends without a live birth.

Based on this framework, Mindel and I developed probability theory models using Markov renewal process theory and simulation models that allowed us to relax the strict assumptions we had to accept for the mathematical models (for example, fertility unchanging with age or over time). Conclusions—sometimes startling ones—that remain relevant today, both for basic understanding of family building and for family planning policy, follow from these models. They include:

- A highly effective contraceptive used by a fairly small proportion of women reduces birth rates more than a less effective contraceptive used by a much higher proportion of women (Sheps & Perrin 1963). So if one believes a highly effective contraception produces a better outcome for a contraceptive user who wants to postpone pregnancy, then both individual and population goals are better served by access to highly effective contraception.
- Accidental pregnancies are common even when quite effective contraception is used. Even when fecundability is reduced to 0.0125, a monthly risk of just over 1 in 100, the probability of an accidental pregnancy is 40% within 5 years, 67% within 10 years, and 82% within 15 years. The reproductive health implications are clear: It is highly likely that during her lifetime, a woman will have to make the decision to either carry on with an unintended pregnancy or choose induced abortion. Because the risk of unintended pregnancy is so high, some will choose sterilization, feeling that that is the only sure pregnancy prevention method.
- Abortion alone is a poor way to control fertility. Abortions almost always are performed early in pregnancy and the subsequent postpartum period is very short. One abortion may only postpone the next birth by a short time. When induced abortion is the main means of fertility control, a woman may have many abortions over her lifetime. A much greater effect on births is achieved when abortion is a backup measure when contraception fails.
- Long postpartum periods serve as a substantial brake on fertility. Later empirical research demonstrated that breastfeeding practices are the major determinant of the length of this period (Howie & McNeilly 1982). Improved survival of breastfeeding infants can also lengthen this period.

An especially informative modeling study we undertook was to change one factor abruptly—in this case the induced abortion rate. The study was motivated by actual experience in Romania where induced abortion was the main means of fertility control until 1966. The government became concerned about low fertility and abruptly ended legal abortion. Six months later, the monthly birthrate shot up but then began to decline. We were able to show that the observed pattern was to be expected after a sudden shock like this. There would be no simple change in the birth rate but rather a series of fluctuations before a new constant birth rate was reached. This birth rate, while higher than the preexisting one, was not nearly as high as the one that initially resulted from the change in availability of abortion. Cohen (1974, p. 1046), in his review of our book, said these results “would warn effectively against believing that the early results of programs designed to affect the birth rate will be the same as the long-term results.”

We also studied selection effects. Some of the few existing studies of waiting time to conception showed monthly conception rates declining with time. We found that it was usually not possible to determine whether the cause was fecundability varying among women (heterogeneity) or fecundability truly declining for all women. With heterogeneity, there is a selection effect: The most fecund conceive sooner, leaving a less fecund pool of women to conceive the next month. Years later, Heckman & Walker (1990) delineated special cases in which heterogeneity could be distinguished from homogeneous declining fecundability.

With the publication of our book in 1973, I felt that we understood the roles that late biological maturation and late sexual initiation, early sterility and early end to sexual relations, contraception

that prolonged time to conception, increased frequency of spontaneous or induced abortion, and longer postpartum periods could play in reducing fertility. We had noted that populations could end up with very similar fertility patterns through quite different mechanisms. I decided a shift to empirical studies was my next step.

Along the way, however, there were several more modeling studies—of family structure with Susan Watkins and John Bongaarts (Watkins et al. 1987), of the effects of lengthening birth intervals through long-term breastfeeding with Randall Kuhn (Menken & Kuhn 1996), and of the effects of famine with Watkins and Cameron Campbell (Menken & Campbell 1992, Watkins & Menken 1985). I also want to note the pathbreaking work of Bongaarts, who based new and widely used measures of fertility determinants on models like ours (e.g., Bongaarts 1978).

## **Fertility Determinants and Reproductive Health**

My first foray into empirical research was my doctoral dissertation on methods for estimating fecundability. I had no data of my own and no opportunity to carry out a study of my own design, but again, research generosity entered the picture. Henry Mosley and others had carried out a study in Matlab, Bangladesh, that collected detailed data on birth interval segments. Their paper is a classic (Chen et al. 1974). Henry shared their data with me and introduced me to the International Centre for Diarrhoeal Disease Research, Bangladesh (now icddr,b) and its Matlab Health and Demographic Surveillance System (HDSS). My main substantive dissertation finding is that the monthly probability of conception even in the absence of birth control was quite low, far lower than had previously been estimated or asserted for European historical populations. Causal mechanisms could not be addressed in this study, but both low frequency of intercourse and the effect of intense breastfeeding after ovulation had resumed have been suggested.

In 1975, James Trussell and I began a long collaboration on fertility determinants in the United States. We were among the first to study contraceptive effectiveness by applying life table approaches to survey data on monthly method use (Menken et al. 1979; Pratt et al. 1979; Vaughan et al. 1977, 1980). This approach yielded plausible estimates of effectiveness when a method was actually used by married couples. As might be expected, the most effective methods were those, like contraceptive pills and intrauterine devices (IUDs), that were applied furthest from intercourse itself. These ordinary use estimates were considerably lower than manufacturer estimates—the so-called laboratory or perfect use effectiveness. We were invited to a US Food and Drug Administration (FDA) Advisory Committee meeting in the 1980s where information to be included in packets of contraceptive pills and IUDs was discussed. We learned very quickly that researchers and activists may take very different stances on this issue. Our view was that ordinary use measures should be presented. A vocal set of activists argued that women should only be given information on outcomes under perfect use. Social science arguments that women and couples frequently are imperfect users failed to sway them in the least.

We were also deeply concerned with evaluating the impact of changes in reproductive health policy. So when the Hyde Amendment, passed in 1976, severely restricted use of federal funds for abortion, we, with the Guttmacher Institute, implemented a study of the impact of this restriction on women's ability to obtain legal abortions (Trussell et al. 1980). Federally funded abortions under Medicaid fell dramatically from 295,000 in fiscal 1977 to approximately 2,000 in 1978, but some states continued to subsidize abortions with nonfederal funds. We analyzed state abortion data and conducted surveys in abortion provider units in several states, including two that cut off abortion funding and others that did not. We concluded that approximately 20% of women who would have obtained abortions were unable to do so in 1978. The other 80% paid for abortions.

Because Medicaid funding was restricted to women on welfare in the states we studied, it is unlikely that these women had private insurance for abortion services. Our conclusions remain eerily relevant nearly 40 years later (Trussell et al. 1980, p. 130):

Americans are of many minds about abortions and about the desirability of using public funds to pay for them. It seems unlikely, however, that very many people can gain satisfaction from the effects of the Hyde amendment in [states we studied].

Those deeply opposed to abortion on religious or moral grounds who hoped that stopping federal and state funds for abortions would bring about dramatic decreases in these procedures must be disappointed. Most poor women in need of abortions managed to obtain them.

Those, on the other hand, who believe that abortion is a procedure which should be available as part of good medical care, and should not be discriminatorily deleted from the range of services offered to poor women under our system of publicly financed health care, will be angered that poor women are forced to dig into their exiguous welfare budgets to pay for a needed abortion. And they will be discouraged that a substantial number of indigent women were forced to bear unwanted children and that a few (no matter how few) were driven to risk their lives and health from self-induced or clandestine abortions. . . .

Those who believe that women should have the right to make their own conscientious decisions about abortion, but who do not support public funding of abortions, will be sobered at the realization that one can't have it both ways.

James and I were equally concerned with understanding infertility, especially at a time when women were increasingly postponing childbearing to later ages. In 1982, an incendiary editorial in the *New England Journal of Medicine*, based on an article in that issue, claimed that women's conception rates fell strikingly from their late twenties to early thirties (Schwartz & Mayaux 1982). It suggested that women who cared to have children would have to revise their plans and have children earlier if they were to have them at all (DeCherney & Berkowitz 1982). James, Ulla Larsen, and I turned to data on historical European populations in which late marriage was common (Flinn 1981, Hajnal 1965), as did Bongaarts (1982). We found that the decline in the proportions having at least one child was only 10% when women who married in their early thirties were compared with those who married in their early twenties. Sharp declines began only after age 35. The evidence was persuasive that biological infertility rises only moderately with age, at least until the late thirties and early forties (Menken 1985, Menken & Larsen 1986, Menken et al. 1986). Most evidence suggests that age-specific infertility has likely declined over time—people are healthier and have access to innovative medical technologies. Yet whether infertility is rising continues to be debated (Levine et al. 2017). What would I, if asked, tell my granddaughters? Let increased risk of infertility after age 35 be one, but only one, of the many factors you take into account as you plan your life course.

## Health and Population Change in Bangladesh—The Matlab Studies

Henry Mosley's work, which introduced me to the Matlab HDSS, jump-started my long and continuing research on Bangladesh. Bangladesh, once described as the world's "basket case" by Henry Kissinger, who thought the country would need continual assistance after obtaining its independence from Pakistan, has seen astonishing change. Between 1970 and 2010, the infant mortality rate plummeted from 173/1,000 births to 37/1,000, the total fertility rate went from 6.9 children to 2.2, and girls' school enrollment rose from 34% to 93%. Changes like these and the existence

of the Matlab HDSS created an unusual opportunity for research on determinants of health and population change and the impact of health and social interventions on individuals and families.

The Matlab HDSS was launched in 1966 by icddr,b as part of a program to test cholera vaccines. Accurate measures of population at risk for the disease were essential. Basic demographic information (births, deaths, migrations, marriages, divorces) is collected through regular (at most bimonthly) visits to each household in a defined geographic area (Alam et al. 2017). Population censuses in 1966 and at 8–10 year intervals thereafter permit calculation of accurate measures of population size and structure at any point in time. In mid-2014, the population under surveillance was over 230,000. In 1978, icddr,b introduced the quasi-randomized Matlab Maternal and Child Health and Family Planning Program (MCH/FP) in approximately half the area; the other half serves as a comparison area that receives government services as provided nationally (Phillips et al. 1982). Data produced by this system are valuable for their accurate demographic measures over time and for vital statistics-based research. But their unique value is in enhancing more detailed longitudinal studies. They can provide the sampling frame for selection into the study, longitudinal information on community context, data collected prior to the study on respondents, and follow-up until migration or death. I refer to the HDSS as the skeleton that supports the bodies of specific studies.

Omar Rahman and I decided to initiate the Matlab Health and Sociodemographic Survey (MHSS) with the Matlab HDSS as our sampling frame. And so I entered the complex realm of survey design and implementation. MHSS1 took place in 1996–1997 (Rahman et al. 2001). MHSS2, in 2012–13, followed MHSS1 households. Omar, Randall Kuhn, Andrew Foster, Nizam Khan, and Abdur Razzaque led MHSS1, and Randall, Tania Barham, Andrew, and Razzaque led MHSS2. Elisabeth Root, Sajal Saha, Nobuko Mizoguchi, Gisella Kagy, Chris Jochem, Sveta Milosheva, and Patrick Turner were essential to the success of MHSS2. The team linked survey respondents and households to HDSS and other data on household members collected since 1974 (including MHSS1/2) and created the 35-year longitudinal Matlab Historical Record (MHR). Importantly, icddr,b has agreed to the release of the MHR as a public use data set.

MHSS2 was designed to ask whether the MCH/FP intervention positively affected the long-term life trajectories of women, children, and their families. In order to avoid selection bias, it was essential to minimize attrition of MHSS1 respondents. Indeed, we found that nearly 60% of males born shortly after the intervention began left Matlab before MHSS2 and would have been missed if MHSS2 included only Matlab residents. Migrant location methods developed by Kuhn and Barham that took advantage of near-universal cell phone availability were extraordinarily successful: More than 92% of those alive in each targeted age/sex group responded to our survey.

This is not the place for a full list of publications, but I mention a few studies of determinants of elder's survival (Kuhn et al. 2006, Rahman et al. 2004), childbearing and women's survival (Menken et al. 2003), arsenic exposure and health (Jochem et al. 2016), and long-term effects of exposure to famine (Kagy 2015). Early evaluations of the MCH/FP intervention show it was associated with higher cognitive functioning (Barham 2012), less outmigration (Barham & Kuhn 2014 title their paper "Staying for Benefits"), lower family size and increased consumption and child education (Foster & Milosheva 2015), and better labor market outcomes (Barham et al. 2016). We are working both on our own analyses and on preparing the MHR for release.

## **Health and Demographic Surveillance Systems in Low and Middle Income Countries**

Influenced by my experience with the Matlab HDSS, I became interested in methods for obtaining population and health information worldwide. Periodic cross-sectional systems such as the

Demographic and Health Surveys provide valuable information but do not follow individuals; longitudinal studies such as the Health and Retirement Survey and the National Longitudinal Study of Adolescent to Adult Health are highly valued but struggle to measure the context in which respondents live. HDSS systems are somewhat of a hybrid in that cross-sectional studies can overlay a larger longitudinal structure. However, their coverage of only a small geographic area and outmigrant selection may make them less advantageous.

At least 30 European countries have national population registers that link administrative documents for an individual (birth, death, marriage, divorce, migration) to form longitudinal records (e.g., Poulain & Herm 2013). The US Census Bureau recently established a system of Federal Statistical Research Data Centers in which restricted-use information can be linked to generate longitudinal records.

Comparable national systems do not exist in low and middle income countries. However, as of 2017, 47 HDSS sites (including Matlab) in 18 countries comprised the INDEPTH network (<http://www.indepth-network.org/>). Together, they collect longitudinal information on health and life events of nearly four million people. I have worked with the University of the Witwatersrand (Wits) Agincourt HDSS in South Africa (Kahn et al. 2012) and with the African Population and Health Research Center (APHRC) Nairobi Urban HDSS, and I chaired the INDEPTH Scientific Advisory Committee for five years, 2002–2007. There are other data collection and analysis networks, e.g., the ALPHA (Analyzing Longitudinal Population-based HIV/AIDS data on Africa) network on HIV/AIDS in 10 sites, which includes some INDEPTH members (Reniers et al. 2016). I will focus on INDEPTH because of my association with the network and several member sites.

INDEPTH and its member sites seek to inform health and population policy by providing data through which pressing health and population needs can be identified and by providing settings for longitudinal research, especially related to interventions. I believe this promise has yet to be fully realized for several reasons. First, ongoing data collection is complicated and expensive, and few funders are willing to sustain essential core infrastructure. We in the United States need only refer to the difficulties of having a fully funded Census to recognize this issue. Second, place-based studies have been criticized as inadequately generalizable to other areas, even within the same country. INDEPTH is addressing this problem by sponsoring multi-site studies. Third, analysis of data has been slow. Owing to financial constraints, staffing tends to emphasize data collection. Few sites involve researchers with sufficient training in longitudinal data analysis and sufficient time dedicated to analysis. Unlike the Demographic and Health Surveys and the studies listed above, sites do not share data with the world research community to an extent that demonstrates their value convincingly and generates continuing support for them. To address these problems, INDEPTH sponsors short training courses and a repository for public use data, iShare (<http://www.indepth-ishare.org>). Its multisite studies now usually use a common data platform so that analyses can be carried out much more quickly. The website contains full descriptions of ongoing studies. And, finally, there is a more recent concern. As migration has increased, an HDSS population is increasingly limited to so-called stayers, who may be quite different in important ways from those who leave.

Despite their positive contributions, many HDSS sites are in financial straits and in danger of closing. A more general discussion in our scientific communities is needed on the contributions of this type of population laboratory and the extent to which they, as sources of accurate and timely demographic and health data and platforms for basic research and intervention studies, merit support in science policy and funding.

## Evaluation of Health- and Population-Related Interventions

My work for the past several decades has been devoted to evaluation of population health interventions, not only in Bangladesh but also in South Africa, Honduras, and closer to home in Colorado. These projects are all interdisciplinary and go across institutions, as has become increasingly possible in our interconnected world. All are based on quasi-experimental designs that permit some degree of causal inference.

In South Africa, we are concerned with the extent of HIV/AIDS, the rollout of treatment, and impacts of HIV/AIDS on health and wellbeing of individuals and families. Our research is inextricably tied to capacity building for population research as well and is discussed below.

Honduras, like many countries, embarked on decentralization of health services with the goal of improving health outcomes. Decentralization, begun in 2005, was interrupted by a change of government, thus establishing a natural experiment: A set of *municipios* (comparable to US counties) was decentralized by 2011, and a comparison set remained under the Ministry of Health. A team of political scientists (Krister Andersson, Alan Zarychta, and Tara Grillos), geographers (Elisabeth Root), and sociologists/demographers (Jane Menken, Bertha Bermudez Tapia) is investigating health and health system impacts of this widely implemented but rarely evaluated intervention.

The Colorado Family Planning Initiative (CFPI), beginning in 2009, provided free or dramatically reduced-cost long-acting reversible contraceptive devices to Title X clinics in Colorado (but not surrounding states). Both fertility and abortion rates fell substantially in Colorado compared with surrounding states (Ricketts et al. 2014, Lindo & Packham 2017). CFPI thus presents a unique opportunity to assess long-term life course impacts of expanded access to these highly effective methods at a time when access to adequate data for this purpose has also improved dramatically. An interdisciplinary team of sociologists/demographers (Amanda Stevensen, Sara Yeatman, Stefanie Mollborn, Jane Menken) and an economist (Katie Genadek) is collaborating with the US Bureau of the Census. It will link American Community Survey and other administrative records to create data sets to investigate impacts on women's subsequent poverty status and economic stability. This project harks back to James Trussell's and my long-ago conclusion that contraceptives that free women from worry about accidental pregnancy are the most effective and may have the greatest impact on their life trajectories. In earlier work, however, causal analysis of the long-term impact of early parenthood was not possible (Furstenberg et al. 1981, McCarthy & Menken 1979, Trussell & Menken 1978).

## CAPACITY BUILDING IN THE UNITED STATES AND THE DEVELOPING WORLD

In addition to my committed involvement in research over the decades, I have had an equivalent commitment to building research and training capacity at home and in the developing world.

### Teaching and Mentoring

Even more than formal teaching, I love working directly with students, both undergraduate and graduate, and postdoctoral fellows, whether involving them in ongoing projects or on their own theses and later research. I am proud of the many who are professors and equally proud of those in nonacademic settings. At the risk of omission after these many decades, professors (some of whom have already retired) include Jim McCarthy, Doug Massey, Maxine Weinstein, Andrew Foster, Judy Seltzer, Zeng Yi, Sam Clark, Randall Kuhn, Irma Elo, the late Silvia Llera Lomeli, Omar Rahman, Jill Grigsby, Enid Schatz, Carrie Foote, Jane Miller, Patrick Heuveline, Andrew London, Sangeetha Madhavan, Nicole Angotti, Georges Reniers, and Christie Sennott.

Of those who followed nonacademic careers, Carolyn Makinson recently retired as director of the International Rescue Committee (IRC) UK. For her promotion of major new programs in refugee studies and her leadership of IRC, she was appointed to the Order of the British Empire, CBE. Alex Ezeh served as director of the APHRC from 2001–2017 and led it to become a successful research and policy institution in sub-Saharan Africa. Eliya Msiyaphazi Zulu is the founding executive director of the African Institute for Development Policy. Others, including Barbara Mensch, Sajeda Amin, Laura Nyblade, Nizam Khan, Daniel Goodkind, Jill Williams, Casey Blalock, and Pradip Muhuri, went on to the Census Bureau, the Population Council, the Demographic and Health Surveys Program, and other organizations.

## **Institutional Collaborations**

Working in Bangladesh, I became fully aware of the dearth of Bangladeshis who had the training needed to contribute to national development by analyzing policy-relevant data from and for their own country. I strongly dissent from the notion that people in the global south should collect data to be analyzed by people in the global north. To every extent possible, my colleagues and I have tried in our international research to be true collaborators with individuals and institutions.

By the late 1990s, several colleagues and I wanted to go beyond this approach. The Andrew W. Mellon Foundation under then–population officer Carolyn Makinson had recently decided to promote development of population studies in Africa and, in particular, in South Africa. Under apartheid, serious population studies programs did not exist. With Mellon support, IBS established the African Population Research and Training Center in 2001. In close collaboration with African institutions, our goals were to build the following: an international pool of talented social science researchers in the United States and in sub-Saharan Africa with the skills needed to address population issues, advanced international-level graduate training capacity in the subcontinent to serve needs of the region on a sustained basis, and strong collaborative research linkages with these institutions focused on crucial areas of population health and migration. We began by developing strong ties with Wits in Johannesburg, South Africa, and with the APHRC in Nairobi, Kenya. I served as a member of the APHRC Board of Directors for ten years, including three years as its chair. Our principles for creating institutional ties were, first, members at all levels of each institution must be involved and committed to the collaboration; second, collaboration had to be mutually beneficial to participants and all decisions made in collaboration, what a South African colleague termed mutuality; and third, training needed to include research experience through which students learn by doing research with established scholars.

With these principles in mind, we sought additional funding. The William and Flora Hewlett Foundation became our primary funder in 2005 and provided over 10 years of uninterrupted support. NIH was also a major funder through a grant from the Fogarty International Center for training in Africa of African graduate students and postdoctoral fellows. The National Institute on Aging provided infrastructure support to upgrade facilities at the Wits Agincourt research site in northeast South Africa and support for research emphasizing HIV/AIDS. I am grateful to Carolyn Makinson, Sara Seims, and the late Richard Suzman, who made this funding possible.

Under the leadership of Stephen Tollman and Kathleen Kahn, Wits decided to initiate graduate programs in population studies and demography in the School of Social Sciences and the School of Public Health. To address the dearth of people who could develop and teach in this new training effort, we established a 3-year postdoctoral program at CU in which fellows would spend two years at Wits, teaching and carrying out collaborative research, and then return to the United States for a year of intensive analysis and publication preparation before moving on. The plan succeeded beyond our greatest expectations. Fellows include Sam Clark, Enid Schatz, Sangeetha Madhavan,

Jill Williams, George Reniers, and Nicole Angotti; all but one are now faculty members at research universities. They contributed immensely to the development of thriving graduate programs in population studies at Wits. With encouragement (and sometimes financial support) from our team, several Wits staff members completed doctoral degrees. Our group and its graduates continue research collaborations with both Wits and APHRC (e.g., Clark et al. 2015, 2017; Gómez-Olivé et al. 2013; Madhavan et al. 2017; Mojola et al. 2015; Ralston et al. 2015; Schatz et al. 2012; Sennott et al. 2016).

Under this program, the Wits/Brown/Colorado/APHRC Colloquium on Population Issues met annually for a decade, rotating among institutions. Graduate students and postdoctoral fellows from each unit participated in the Colloquia and in short courses that were held in Johannesburg and Boulder. Partly through these meetings, APHRC and Wits conceived CARTA (the Consortium for Advanced Research Training in Africa). CARTA is a south-north collaboration based at APHRC that is working with eight universities and several African research institutions to develop “world class multidisciplinary research that impacts positively on public and population health” (<http://cartafrica.org/about-cartaf/>).

## **POLICY INVOLVEMENTS**

As I indicated earlier, I have had a career-long commitment to furthering the application of research to inform public policy. The National Academy of Sciences, through its National Research Council (NRC), afforded me the opportunity to work as an invited volunteer on issues for which agencies, primarily of the federal government, requested advice. I was an NRC Committee or Panel member or chair continuously from 1978 to 2006 and sporadically since then. I benefitted as much as I gave to these enterprises. In particular, my research on African demography, HIV/AIDS, and aging in the developing world grew from to my participation in the Committee on Population and its predecessor Committee on Population and Demography, the Committee on National Statistics, and the Committee on AIDS Research and the Behavioral, Social, and Statistical Sciences. I also was a member of the Advisory Committee to the director of NIH for three years.

## **CONCLUDING THOUGHTS**

In retrospect, I have been unusually fortunate to have had such a rewarding life in science—from where it all started, with a young student who discovered a problem of social significance that shaped the contours of her life, to theoretical models of fertility, to empirical studies and evaluations of interventions, to participation in capacity building and policy formulation. It has been both intellectually challenging and personally satisfying.

I am enormously grateful to the many students and colleagues—senior and junior, in the United States and around the world—with whom I have had the great good fortune to share this lifelong adventure. I hope that all the work over the years has made at least a small contribution to scientific understanding in the service of human welfare.

Now, as I approach the end of my eighth decade, I continue to have a full and fulfilling research agenda, but as I look to the future, I am well aware how much remains to be done. Therein lies the challenge for the young scholars who have taken the place of my generation. I wish for them the joy that social inquiry can bring.

## **DISCLOSURE STATEMENT**

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

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## LITERATURE CITED

- Alam N, Ali T, Razzaque A, Rahman M, Zahirul Haq M, et al. 2017. Health and Demographic Surveillance System (HDSS) in Matlab, Bangladesh. *Int. J. Epidemiol.* 46:809–16
- Barham T. 2012. Enhancing cognitive functioning: medium-term effects of a health and family planning program in Matlab. *Am. Econ. J. Appl. Econ.* 4:245–73
- Barham T, Kuhn R. 2014. Staying for benefits: the effect of a health and family planning program on out-migration patterns in Bangladesh. *J. Hum. Resour.* 49:982–1013
- Barham T, Kuhn R, Turner P. 2016. *Long-term effects of early childhood interventions on migration and labor market outcomes: evidence from a quasi-random child health and family planning program in Bangladesh*. Work. Pap., Sloan Sch. Manag., MIT
- Becker G. 1981. *A Treatise on the Family*. Cambridge, MA: Harvard Univ. Press
- Blake J, Davis K. 1964. Norms, values, and sanctions. *Handb. Mod. Sociol.* 101:456–84
- Bongaarts J. 1978. A framework for analyzing the proximate determinants of fertility. *Popul. Dev. Rev.* 4:105–32
- Bongaarts J. 1982. Infertility after age 30: a false alarm. *Fam. Plan. Perspect.* 14:75–78
- Bourgeois-Pichat J. 1971. Stable, semi-stable populations and growth potential. *Popul. Stud.* 25:235–54
- Brass W. 1958. The distribution of births in human populations in rural Taiwan. *Popul. Stud.* 12:51–72
- Chen LC, Ahmed S, Gesche M, Henry Mosley W. 1974. Prospective study of birth interval dynamics in rural Bangladesh. *Popul. Stud.* 28:277–97
- Clark S, Madhavan S, Cotton C, Beguy D, Kabiru C. 2017. Who helps single mothers in Nairobi? The role of kin support. *J. Marriage Fam.* 79:1186–204
- Clark SJ, Gomez-Olive FX, Houle B, Thorogood M, Klipstein-Grobusch K, et al. 2015. Cardiometabolic disease risk and HIV status in rural South Africa: establishing a baseline. *BMC Public Health* 15:135
- Coale AJ. 1972. *Growth and Structure of Human Populations: A Mathematical Investigation*. Princeton, NJ: Princeton Univ. Press
- Cohen J. 1974. Review: mathematical models of conception and birth. *J. Am. Stat. Assoc.* 69:1046–47
- Cohen SA, Richards CL. 1994. The Cairo consensus: population, development and women. *Fam. Plan. Perspect.* 26:272–77
- Dandekar V. 1955. Certain modified forms of binomial and Poisson distributions. *Sankhyā Indian J. Stat. (1933–1960)* 15:237–50
- Davis K, Blake J. 1956. Social structure and fertility: an analytic framework. *Econ. Dev. Cult. Change* 4:211–35
- DeCherney AH, Berkowitz GS. 1982. Female fecundity and age. *N. Engl. J. Med.* 306:424–26
- Flinn MW. 1981. *The European Demographic System 1500–1820*. Johns Hopkins Symp. Comp. Hist. Book 12. Baltimore, MD: Johns Hopkins Univ. Press
- Foster A, Milusheva S. 2015. *Household recombination, retrospective evaluation, and the effects of a health and family planning intervention*. Work. Pap. id:7183, eSocialSciences
- Friedan B. 1963. *The Feminine Mystique*. New York: WW Norton
- Furstenberg FFJ, Lincoln R, Menken J, eds. 1981. *Teenage Sexuality, Pregnancy, and Childbearing*. Philadelphia: Univ. Pa. Press
- Gini C. 1924. Premières recherches sur la fécondabilité de la femme. *Proc. Int. Math. Congr.* 2:889–92
- Gómez-Olivé FX, Angotti N, Houle B, Klipstein-Grobusch K, Kabudula C, et al. 2013. Prevalence of HIV among those 15 and older in rural South Africa. *AIDS Care* 25:1122–28
- Hajnal J. 1965. European marriage patterns in perspective. In *Population in History*, ed. DV Glass, DEC Eversley, pp. 101–46. New Brunswick, NJ: Transaction Publ.
- Heckman JJ, Walker JR. 1990. Estimating fecundability from data on waiting times to first conception. *J. Am. Stat. Assoc.* 85:283–94
- Henry L. 1972a. *Démographie. Analyse et modèles*. Paris: Larousse
- Henry L. 1972b. *On the Measurement of Human Fertility: Selected Writings of Louis Henry*. Amsterdam: Elsevier

- Howe PW, McNeilly A. 1982. Effect of breast-feeding patterns on human birth intervals. *J. Reprod. Fertil.* 65:545–57
- Jochem WC, Razzaque A, Root ED. 2016. Effects of health intervention programs and arsenic exposure on child mortality from acute lower respiratory infections in rural Bangladesh. *Int. J. Health Geogr.* 15:32
- Kagy G. 2015. *Effects of labor market opportunities and environmental conditions on health and human capital development*. PhD Thesis, Univ. Colo., Boulder
- Kahn K, Collinson MA, Gómez-Olivé FX, Mokoena O, Twine R, et al. 2012. Profile: Agincourt health and socio-demographic surveillance system. *Int. J. Epidemiol.* 41:988–1001
- Keyfitz N. 1968. *Introduction to the Mathematics of Population*. Reading, MA: Addison-Wesley
- Kuhn R, Rahman O, Menken J. 2006. Survey measures of health: How well do self-reported and observed indicators measure health and predict mortality? In *Aging in Sub-Saharan Africa: Recommendations for Furthering Research*, National Research Council, ed. B Cohen, J Menken, pp. 314–42. Washington: Natl. Acad. Press
- Levine H, Jørgensen N, Martino-Andrade A, Mendiola J, Weksler-Derri D, et al. 2017. Temporal trends in sperm count: a systematic review and meta-regression analysis. *Hum. Reprod. Update* 23:646–59
- Lindo JM, Packham A. 2017. How much can expanding access to long-acting reversible contraceptives reduce teen birth rates? *Am. Econ. J. Econ. Policy* 9(3):348–76
- Madhavan S, Clark S, Beguy D, Kabiru CW, Gross M. 2017. Moving beyond the household: innovations in data collection on kinship. *Popul. Stud.* 71:117–32
- McCarthy J, Menken J. 1979. Marriage, remarriage, marital disruption and age at first birth. *Fam. Plan. Perspect.* 11:21–30
- Menken J. 1985. Age and fertility: How late can you wait? *Demography* 22:469–83
- Menken J, Campbell C. 1992. Age-patterns of famine-related mortality increase: implications for long-term population growth. *Health Transit. Rev.* 2:91–101
- Menken J, Duffy L, Kuhn R. 2003. Childbearing and women’s survival: new evidence from rural Bangladesh. *Popul. Dev. Rev.* 29:405–26
- Menken J, Kuhn R. 1996. Demographic effects of breastfeeding: fertility, mortality, and population growth. *Food Nutr. Bull. U. N. Univ.* 17:349–61
- Menken J, Larsen U. 1986. Fertility rates and aging. In *Aging, Reproduction, and the Climacteric*, ed. L Mastroianni Jr., CA Paulsen, pp. 147–66. New York: Plenum
- Menken J, Trussell J, Ford K, Pratt WF. 1979. Experience with contraceptive use in developed countries. In *Contraception: Science, Technology and Application: Proceedings of a Symposium*, pp. 24–44. Washington, DC: Natl. Acad. Press
- Menken J, Trussell J, Larsen U. 1986. Age and infertility. *Science* 233:1389–94
- Mojola SA, Williams J, Angotti N, Gómez-Olivé FX. 2015. HIV after 40 in rural South Africa: a life course approach to HIV vulnerability among middle aged and older adults. *Soc. Sci. Med.* 143:204–12
- Natl. Acad. Sci. 1971. *Rapid Population Growth: Consequences and Policy Implications*. Baltimore, MD: Johns Hopkins Univ. Press
- Pearl R. 1933. Factors in human fertility and their statistical evaluation. *Lancet* 222:607–11
- Perrin EB, Sheps MC. 1964. Human reproduction: a stochastic process. *Biometrics* 20:28–45
- Phillips JF, Stinson WS, Bhatia S, Rahman M, Chakraborty J. 1982. The demographic impact of the family planning–health services project in Matlab, Bangladesh. *Stud. Fam. Plan.* 13:131–40
- Potter RG Jr. 1963. Birth intervals: structure and change. *Popul. Stud.* 17:155–66
- Poulain M, Herm A. 2013. Central population registers as a source of demographic statistics in Europe. *Population* 68:183–212
- Pratt WF, Grady WR, Menken JA, Trussell J. 1979. An overview of experience with vaginal contraceptives in the United States. In *Vaginal Contraceptives: New Developments*, ed. GJ Zatuchni, pp. 82–99. Hagerstown, MD: Harper and Row
- Rahman O, Menken J, Foster A, Gertler P. 2001. *Matlab [Bangladesh] Health and Socioeconomic Survey (MHSS), 1996. 5th ICPSR version*. Santa Monica, CA: RAND
- Rahman O, Menken J, Kuhn R. 2004. The impact of family members on the self-reported health of older men and women in a rural area of Bangladesh. *Ageing Soc.* 24:903–20

- Ralston M, Schatz E, Menken J, Gómez-Olivé FX, Tollman S. 2015. Who benefits—or does not—from South Africa's old age pension? Evidence from characteristics of rural pensioners and non-pensioners. *Int. J. Environ. Res. Public Health* 13:85
- Reniers G, Wamukoya M, Urassa M, Nyaguara A, Nakiyingi-Miiró J, et al. 2016. Data resource profile: Network for Analysing Longitudinal Population-based HIV/AIDS data on Africa (ALPHA Network). *Int. J. Epidemiol.* 45:83–93
- Ricketts S, Klinger G, Schwalberg R. 2014. Game change in Colorado: widespread use of long-acting reversible contraceptives and rapid decline in births among young, low-income women. *Perspect. Sex. Reprod. Health* 46: 125–32
- Rogers A. 1975. *Introduction to Multiregional Mathematical Demography*. New York: Wiley
- Schatz E, Gómez-Olivé X, Ralston M, Menken J, Tollman S. 2012. The impact of pensions on health and wellbeing in rural South Africa: Does gender matter? *Soc. Sci. Med.* 75:1864–73
- Schwartz D, Mayaux M. 1982. Female fecundity as a function of age. *N. Engl. J. Med.* 306:404–6
- Sennott C, Reniers G, Gomez-Olive FX, Menken J. 2016. Premarital births and union formation in rural South Africa. *Int. Perspect. Sex. Reprod. Health* 42:187–96
- Sheps MC. 1965. An analysis of reproductive patterns in an American isolate. *Popul. Stud.* 19:65–80
- Sheps MC, Menken JA. 1973. *Mathematical Models of Conception and Birth*. Chicago: Univ. Chicago Press
- Sheps MC, Perrin EB. 1963. Changes in birth rates as a function of contraceptive effectiveness: some applications of a stochastic model. *Am. J. Public Health Nations Health* 53:1031–46
- Taylor CE. 1997. Origins of longitudinal community-based studies. In *Prospective Community Studies in Developing Countries*, ed. M Das Gupta, P Aaby, M Garenne, G Pison, pp. 19–27. Oxford, UK: Oxford Univ. Press
- Tietze C. 1956. Statistical contributions to the study of human fertility. *Fertil. Steril.* 7:88–95
- Trussell J, Menken J. 1978. Early childbearing and subsequent fertility. *Fam. Plan. Perspect.* 10:209–18
- Trussell J, Menken J, Lindheim BL, Vaughan B. 1980. The impact of restricting Medicaid financing for abortion. *Fam. Plan. Perspect.* 12:120–30
- Vaughan B, Trussell J, Menken J, Jones EF. 1977. Contraceptive failure among married women in the United States, 1970–1973. *Fam. Plan. Perspect.* 9:251–58
- Vaughan B, Trussell J, Menken J, Jones EF, Grady W. 1980. Contraceptive efficacy among married women aged 15–44 years. *Vital Health Stat.* 23:1–62
- Vincent P. 1961. Recherches sur la fécondité biologique. Étude d'un groupe de familles nombreuses. *Population (Fr. Ed.)* 16:105–12
- Watkins SC, Menken J. 1985. Famines in historical perspective. *Popul. Dev. Rev.* 11:647–75
- Watkins SC, Menken JA, Bongaarts J. 1987. Demographic foundations of family change. *Am. Sociol. Rev.* 52:346–58

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

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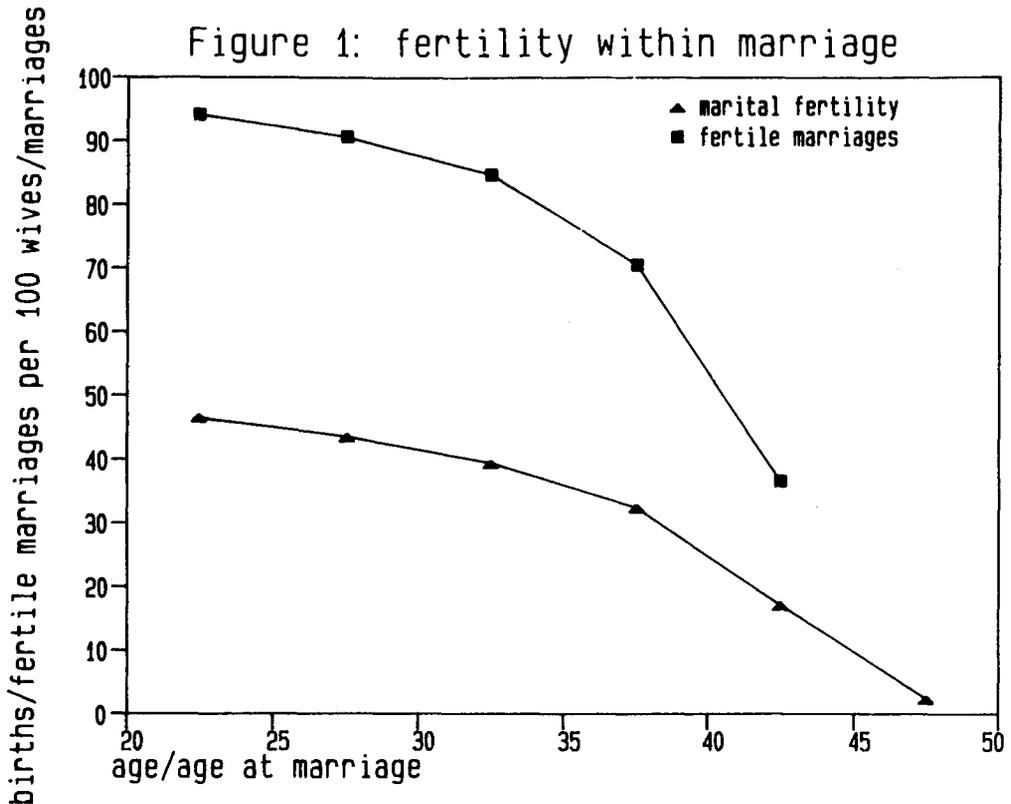
Today I would like to talk about two demographic issues, both of which may have far-reaching effects in the United States and neither of which is well-understood. The first of these is infertility, which has received extensive but often ill-considered attention in the press and other forms of popular 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?

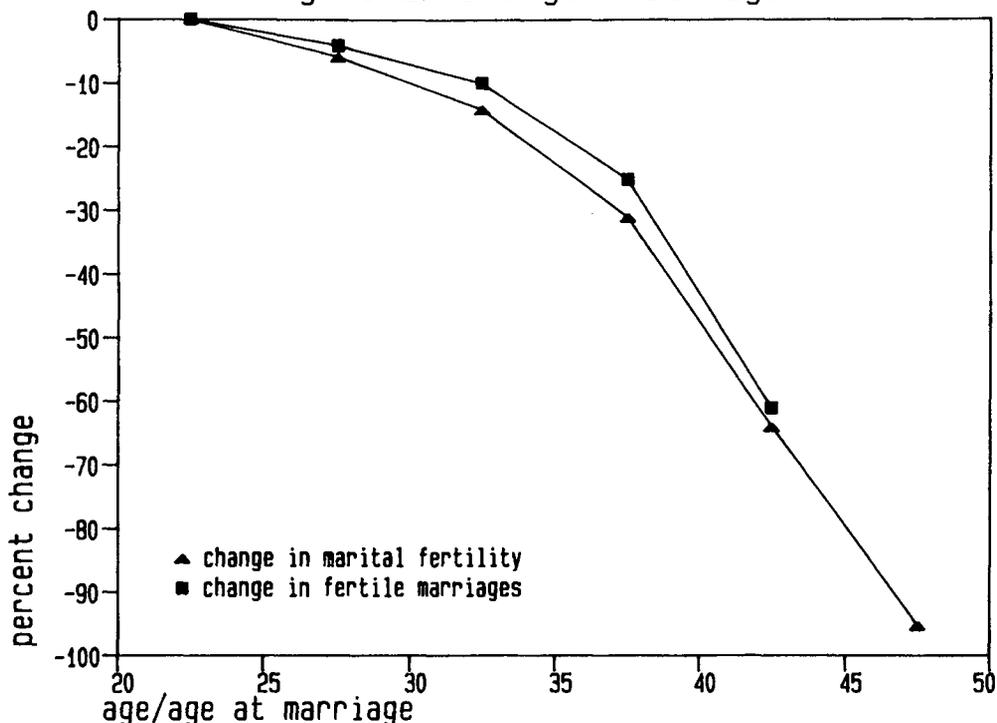
What do the women themselves say? Only 11 percent of women between the ages of 25 and 29 and 13 percent of those in their early thirties expect to have no children (U.S. Bureau of the Census, 1983). It is likely that more will remain childless than expect to do so. Some of those who do expect to have 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

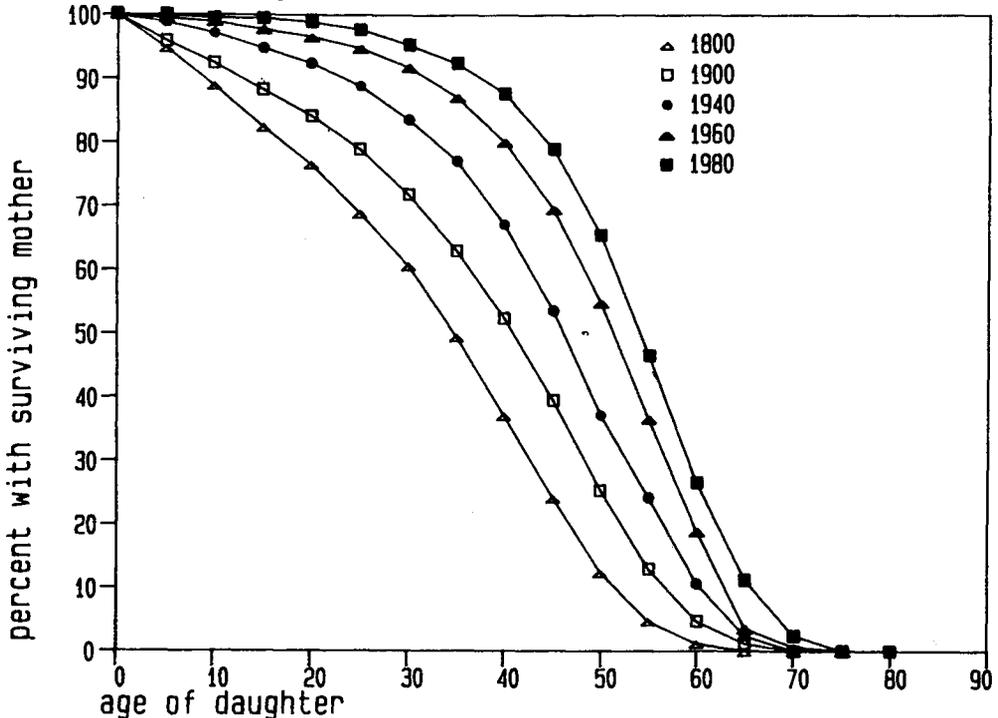
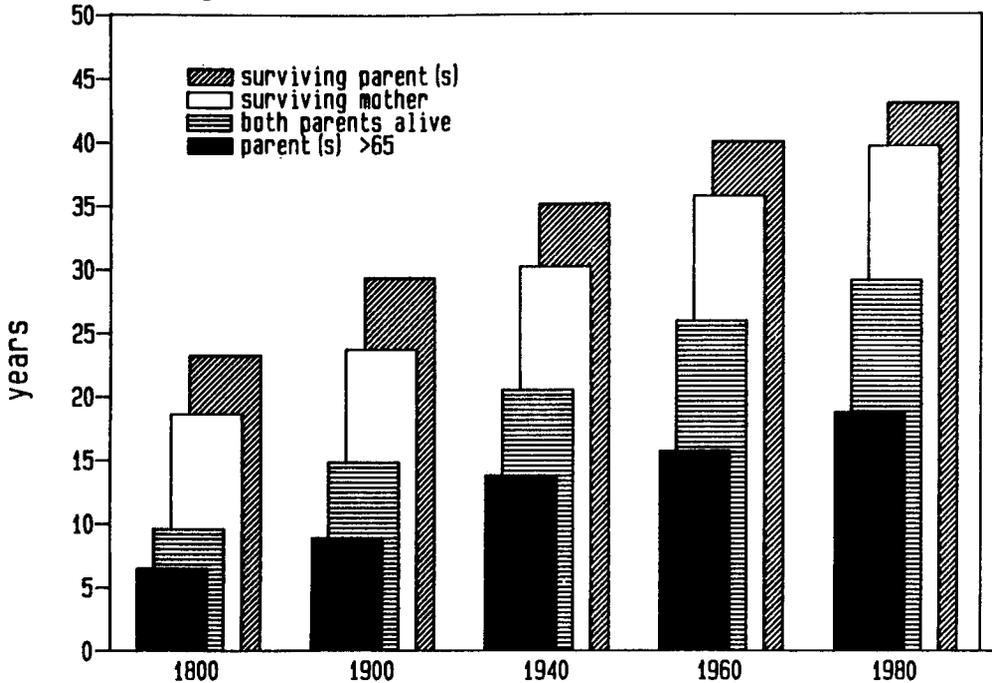


Figure 4: adult years as a daughter



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

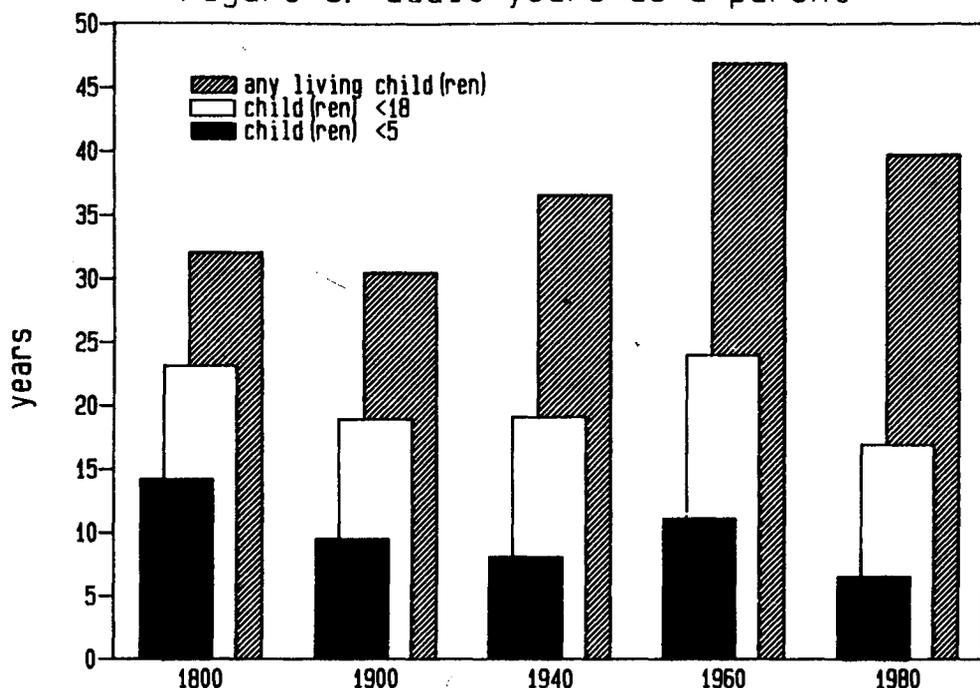
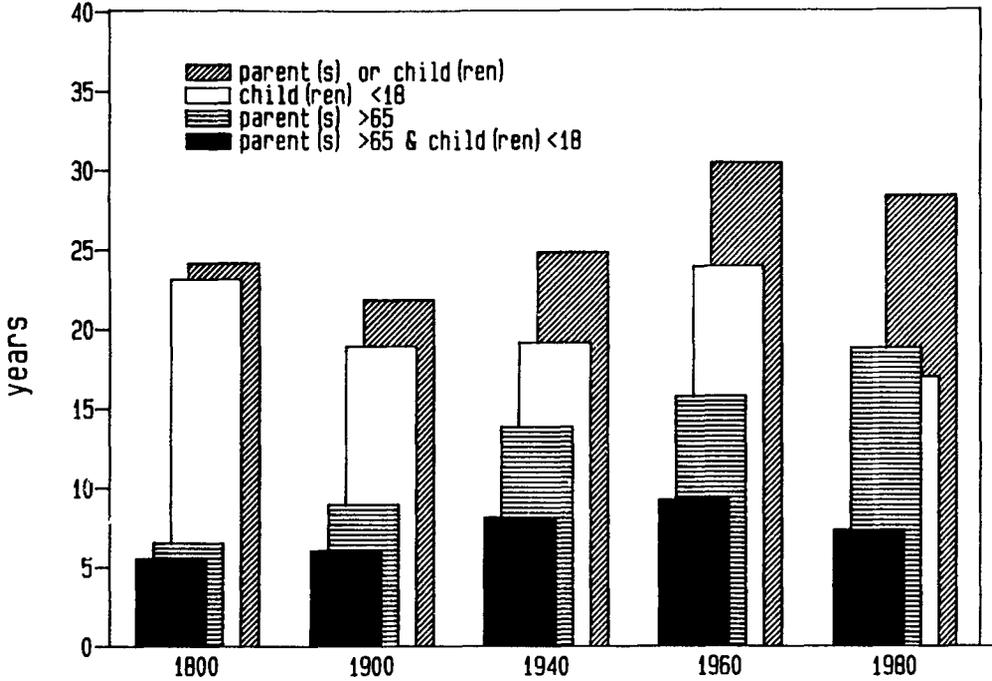


Figure 6: years of responsibility to dependents



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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## REFERENCES

- Aral, S. and W. Cates, Jr. 1983. The increasing concern with infertility: Why now? *Journal of the American Medical Association* 250:2327-2331.
- Bianchi, S. and D. Spain. Forthcoming. *American Women, 1980 U.S. Census Monograph Series*.
- Bongaarts, J. 1982. Infertility after age 30: A false alarm. *Family Planning Perspectives* 14:75-78.
- . 1984a. The Projection of Family Composition Over the Life Course with Family Status Life Tables. Paper prepared for the Workshop on Family Demography: Methods and Their Application, cosponsored by the International Union for the Scientific Study of Population and the Population Council.
- . 1984b. The Demographic Determinants of the Duration and Incidence of Widowhood. Paper prepared for the Seminar on The Demography of the Later Phases of the Family Life Cycle cosponsored by the International Union for the Scientific Study of Population and the German Association for the Scientific Study of Population, 3-7 September.
- Brody, E.M. 1985. Parent care as a normative family stress. *The Gerontologist* 25:19-29.
- Caldwell, J.C. 1982. *The Theory of Fertility Decline*. New York: Academic Press.
- Cates, W. Jr. 1984. Sexually transmitted organisms and infertility: The proof of the pudding. *Sexually Transmitted Diseases* 11:113-116.
- Coale, A.J. and P. Demeny with B. Vaughan. 1983. *Regional Model Life Tables and Stable Populations* (2nd. ed.). New York: Academic Press.
- Coale, A.J. and D.R. McNeil. 1972. The distribution by age of the frequency of first marriage in a female cohort. *Journal of the American Statistical Association* 67:743-749.
- Coale, A.J. and T.J. Trussell. 1975. Model fertility schedules: Variation in the age structure of childbearing in human populations. *Population Index* 40:185-201.
- Cohen, J. 1985. *Mathematical Demography: Recent Developments in Population Projections*. Pp. 179-189 in *International Population Congress 1985, Vol. 4*. Liege: International Union for the Scientific Study of Population.
- Collins, J.A., W. Wrixon, L.B. Janex, and E.H. Wilson. 1983. Treatment independent pregnancy among infertile couples. *The New England Journal of Medicine* 309:1201-1206.
- Cramer, D.W., I. Schiff, S. Schoenbaum, M. Gibson, S. Belisle, B. Albrecht, R. Stillman, M. Berger, E. Wilson, B. Stadel, and M. Siebel. 1985. Tubal infertility and the intrauterine device. *The New England Journal of Medicine* 312:941-947.
- Crystal, S. 1982. *America's Old Age Crisis*. New York: Basic Books, Inc.
- Davis, K. and P. van den Oever. 1982. Demographic foundations of new sex roles. *Population and Development Review* 8:495-512.
- Davis, K. 1984. Wives and work: Consequences of the sex role revolution. *Population and Development Review* 10:397-418.
- Day, A. 1984. Kinship Networks and Informal Support in the Later Years: Demographic and Life Course Perspectives. Paper prepared for the Seminar on the Demography of the Later Phases of the Family Life Cycle, sponsored by the International Union for the Scientific Study of Population and the German Association for the Scientific Study of Population, 3-7 September.
- DeCherney, A.H. and G.S. Berkowitz. 1982. Female fecundity and age. *The New England Journal of Medicine* 306:424-426.
- Federation CECOS, D. Schwartz, and M.J. Mayaux. 1982. Female fecundity as a function of age. *The New England Journal of Medicine* 306:404-406.

- Flinn, M. 1981. *The European Demographic System, 1500–1820*. Baltimore: The Johns Hopkins Press.
- Goldman, N. 1984. Changes in widowhood and divorce and expected durations of marriage. *Demography* 21:297–308.
- Goodman, L., N. Keyfitz, and T. Pullum. 1974. Family formation and the frequency of various kinship relationships. *Theoretical Population Biology* 5:1–27.
- . 1977. Addendum, family formation and the frequency of various kinship relationships. *Theoretical Population Biology* 8:376–381.
- Hagestad, G.O. 1984. Families in an aging society, unpublished manuscript.
- Hajnal, J. 1965. European Marriage Patterns in Perspective. In D.V. Glass and D.E.C. Eversley (eds.), *Population in History*. London: Edward Arnold.
- Hatcher, R., G.K. Stewart, F. Stewart, M. Josephs and J. Dale. 1982. *Contraceptive Technology 1982–1983* (11th ed.). New York: Irvington Inc.
- Henry, L. 1961. Some data on natural fertility. *Eugenics Quarterly* 8:81–91.
- Heuser, R.L. 1976. *Fertility Tables for Birth Cohorts by Color*. DHEW Publications No. (HRA) 76-1152. Washington D.C.: U.S. Government Printing Office.
- Hogue, C.J.R., W. Cates Jr., and C. Tietze. 1982. The effects of induced abortion on subsequent reproduction. *Epidemiological Reviews* 4:66–71.
- . 1983. The impact of vacuum aspiration abortion on future childbearing: a review. *Family Planning Perspectives* 15:119–126.
- Keyfitz, N. 1977. *Applied Mathematical Demography*. New York: John Wiley and Sons.
- Kleinman, R. and P. Senanayake. 1979. *Handbook on Infertility*. London: International Planned Parenthood Federation.
- Kobrin, F. 1976. The fall in household size and the rise of the primary individual in the United States. *Demography* 13:127–138.
- Levy, M.J., Jr. 1966. *Modernization and the Structure of Societies*. Princeton: Princeton University Press.
- Lotka, A.J. 1931. Orphanhood in relation to demographic factors. *Metron* 9:37–109.
- Menken, J. and U. Larsen. Forthcoming. Fertility Rates and Aging. In L. Mastroianni and A. Paulsen (eds.), *Aging, Reproduction, and the Climacteric*. New York: Plenum Press.
- Mosher, W.D. and W.F. Pratt. 1985. Fecundity and Infertility in the United States, 1965–1982. NCHS Advance Data, Number 104.
- Pratt, W.F., W.D. Mosher, C.A. Bachrach, and M.C. Horn. 1984. Understanding U.S. fertility: Findings from the National Survey of Family Growth, Cycle III. *Population Bulletin* 39(5).
- Preston, S.H. 1984. Children and the elderly: divergent paths for America's dependents. *Demography* 21:435–458.
- Pullum, T. 1982. The eventual frequencies of kin in a stable population. *Demography* 19:549–565.
- Ryder, N.B. 1983. Fertility and family structure. *Population Bulletin of the United Nations* 15:15–34.
- Schoen, R. 1983. *United States Marital Status Life Tables for Periods 1910–1975 and Cohorts 1880–1945*. Manuscript.
- Schoen, R., W. Urton, K. Woodrow and J. Baj. 1985. Marriage and divorce in twentieth century American cohorts. *Demography* 22:101–114.
- Seeger, P. 1963. *We Shall Overcome*. Columbia Records: CL2101.
- Shanas, E. 1968. *Old People in Three Industrial Societies*. New York: Atherton Press.
- Sherris, J. and G. Fox. 1983. Infertility and sexually transmitted disease: a public health challenge. *Population Reports* 11(3), Series L, No. 4: L113–L151.
- Senanayake P. and D.G. Kramer. 1980. Contraception and the etiology of pelvic inflammatory disease: new perspectives. *American Journal of Obstetrics and Gynecology* 138:852–860.
- Soldo, B. 1980. The Dependency Squeeze on Middle-aged Women. Presented at a meeting of the Secretary's Advisory Committee on Rights and Responsibilities of Women, Department of Health and Human Services.
- Trussell, J. and C. Wilson. 1985. Sterility in a Population with Natural Fertility. *Population Studies* 39: 269–286.
- U.S. Bureau of the Census. 1983. Fertility of American Women: June 1981. *Current Population Reports, Series P-20, Number 378*.
- Watkins, S.C., J. Menken and J. Bongaarts. 1984. Continuities and Changes in the American Family. Paper presented at the 1984 Annual Meeting of the Social Science History Association.
- Westrom, L. 1980. Incidence, prevalence and trends of acute pelvic inflammatory disease and its consequences in industrialized countries. *American Journal of Obstetrics and Gynecology* 138:880–892.
- Wolf, D. 1984. Kin availability and the living arrangements of older women. *Social Science Research* 13:72–89.