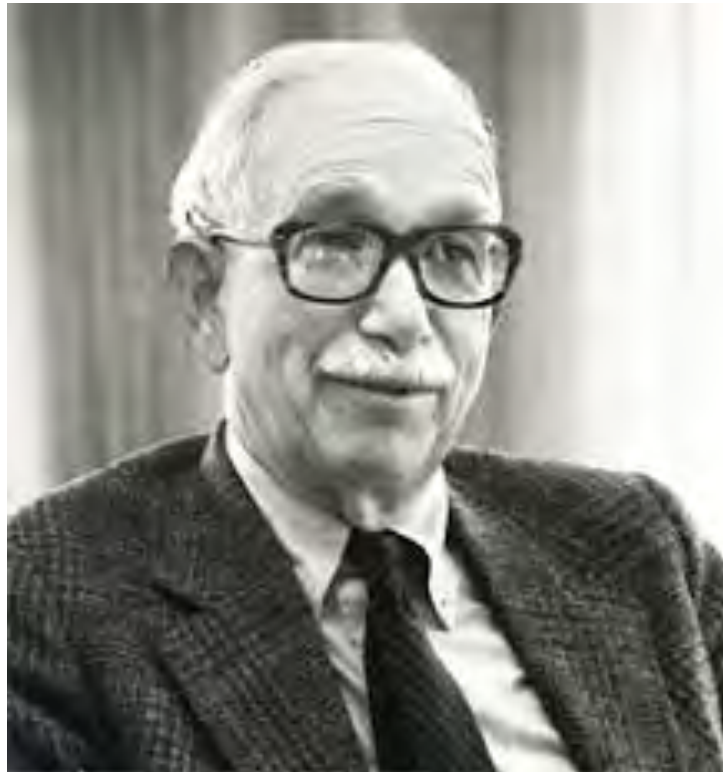


# **DEMOGRAPHIC DESTINIES**

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

### **Interview with Ronald Freedman PAA President in 1964-65**



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

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

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

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

## RONALD FREEDMAN

PAA President in 1964-65 (No. 28). Interview with Jean van der Tak at the Population Studies Center, University of Michigan, Ann Arbor, June 12, 1989, with excerpts from Dr. Freedman's interview with Anders Lunde at the PAA meeting in Philadelphia, April 26, 1979.

Dr. Freedman requested (in a letter of April 25, 1990, to Jean van der Tak) that the following statement appear at the beginning of this text:

"On reading the transcribed interview, Ron Freedman decided that the 'stream of consciousness' flow should stand, complete with grammatical errors and incomplete and repetitious sentences. Reader, beware!" (Subsequently, however, this transcript, like all those in the series, has been lightly edited.)

**CAREER HIGHLIGHTS:** Ronald Freedman was born in 1917 in Winnipeg, Canada, which makes him one of PAA's three Canadian-born presidents, along with Nathan Keyfitz (president in 1970-71) and Norman Ryder (1972-73). He came to the U.S. as a child and grew up in Waukegan, Illinois, a suburb of Chicago. He received his B.A. in history and economics from the University of Michigan in 1939 and the M.A. in sociology from Michigan in 1940. He then went to the University of Chicago and completed the prelims for his Ph.D. in sociology before joining the Navy in 1942. In 1946 he returned to Chicago to work on his dissertation and received the Ph.D. in 1947. He also joined the faculty of the department of sociology at the University of Michigan in 1946 and has been associated with Michigan ever since, variously, as Professor of Sociology, Research Associate with Michigan's Survey Research Center, and first Director, from 1962 to 1971, and then Associate Director of the Population Studies Center, which he founded. He has been adviser and consultant to many national and international institutions in the U.S. and Asia. He was Vice-President of IUSSP in 1965-69. Among his awards, he was elected to the National Academy of Sciences in 1974 and received PAA's Irene B. Taeuber Award for Excellence in Demographic Research in 1981.

Ronald Freedman is famous in the field of demography for his work and publications on fertility from the sociological perspective, beginning in the 1940s with analysis of the Indianapolis Fertility Study. He was co-director of the first U.S. national fertility survey, the Growth of American Families survey of 1955, and originator of Michigan's ongoing Detroit Area Study. He is best known, perhaps, for his work in the Third World, especially Taiwan. In the early 1960s, he set up the Taichung experiment which led to Taiwan's very successful family planning program and inspired other programs in Asia. He was co-director of Taiwan's Population Studies Center in 1962-64 and has been consultant to the Taiwan government ever since. He has been family planning research consultant to several other Asian governments, including, most recently, China. Throughout his career, he has produced a large number of influential books and articles on family planning programs, fertility surveys, and the interface between social science research and human fertility. He died in Ann Arbor, Michigan, in 2007.

**VDT:** When did you first become interested in demography?

**FREEDMAN** [This combines Dr. Freedman's replies in 1979 and 1989]: That happened in 1939 quite by accident. I'd had one course in sociology as an undergraduate and, for various reasons, decided that I wanted to be a group social worker and that the way to do that was to get a graduate degree in sociology. So I entered the sociology department at the University of Michigan. Almost by chance, I registered for two courses, one in population, one in human ecology. I had expected these courses to be taught by the late R.D. McKenzie, but when I got to class, I found that he was ill and a young

teaching fellow named Amos Hawley was giving those courses. It took about two weeks before I became entranced with what was going on, and that changed my life. At the same time, I was taking my first course in quantitative research methods with Clark Tibbits and the two things together opened my eyes. The idea that one could quantitatively study social problems was new to me. I was fascinated by that from the methodological point of view and then fascinated by the subject matter. By the end of that term, I knew that somehow I was going to work in this general field.

Previously, I had really been going into sociology with the view of being a director of one of the Hillel foundations that Jewish students have on many campuses. As a student at Michigan, I was the student director of the Hillel foundation. My salary of \$5 a week and my room there sustained me during most of my student years at the University of Michigan.

**VDT:** Then you decided to go to the University of Chicago, as the mecca of sociology at that time, after getting your master's in sociology at Michigan?

**FREEDMAN:** Right. I had received fellowship offers from Michigan and Chicago. Michigan's department was then small and my friends here felt that I had learned what they had to give and encouraged me to go to Chicago. I must say that at the end of the first semester there, I came back and said I wanted to come back to Michigan because I didn't feel I was learning enough and felt I knew more about human ecology than they did there, on the basis of what I'd learned here. Fortunately, at the end of the second semester things opened up. At that time, there were something like 300 graduate students at Chicago; about half the Ph.D.s in sociology in the United States were being produced there and it was an anomic situation; you were a wanderer in the wilderness.

**VDT:** Three hundred in the department of sociology alone?

**FREEDMAN:** Yes. Well, many of them were part-time, on the fringe. What turned it all around for me was that I became a member of a much smaller, inner group of people who communicated with each other, used the famous Phil Hauser notes for studies; we had private study sessions. I was research assistant first to William Ogburn and then to Louis Wirth. A high point of the year was that Phil Hauser, then working in the Census Bureau, came back to Chicago and gave us some lectures on the introduction of the new idea of sampling into the data collection of the federal government. I got caught up with fascinating, interesting people like Sam Stouffer, who was a marvelous teacher and inspiring figure, and I never thought of leaving after that.

**LUNDE** [from the 1979 interview]: Could you tell us something about Stouffer and W.F. Ogburn.

**FREEDMAN:** Stouffer was a very exciting, disorganized teacher. He would get a bright idea, rush into the room, rush to the blackboard, and before the period was over, his coat and trousers were full of chalk. It frequently had nothing to do with what we were studying at the time. He got all of us excited. Kingsley Davis, who happened to be a visiting postdoc, was in that class. We learned a great deal about population statistics and sampling, the two courses I took with Sam. His style of teaching was disorganized but inspirational.

W.F. Ogburn was a Southern gentleman of the old school and a very kindly man. He was a man with nobility of character, clarity of mind, but a very dull teacher. He spoke very slowly from 3 by 5 index cards. I took my first course in multivariate analysis with Ogburn. The course consisted of algebraic derivations of the various formulae for multivariate analysis. We had a classroom that looked out on the Medway and I remember vividly one day--this was before the day of Xerox--Ogburn told us he would have the notes he had on cards reproduced and give them to us, but he put them on the blackboard and this day he had filled about four blackboards. Then he stopped and looked

pensively out the window, then looked back and said, "I guess that's a mistake," and he erased a few blackboards and started over again.

**VDT:** You completed your prelims before going into the Air Force in 1942 and came back to Chicago in 1946 to work on your dissertation on recent migration to Chicago. You found that Professors Ernest Burgess and Louis Wirth had obtained some 1940 census data for Chicago from the question, "Where did you live five years ago?", the first time that question was asked in the census, and you were going to use that to test the Burgess zonal hypothesis.

**FREEDMAN:** That's right. That was an interesting story, because it turned out that the simple version of the Burgess hypothesis didn't work. That would have been that mobility and social disorganization decreased together with distance from the center of the city. I told my wife this is going to be a textbook thesis. I had a hypothesis; it was obviously going to be right. We should be able to organize the data in three months, I'd write it up, and we'd go home. Well, it took six or seven months of prodigious work. We had to combine the 900 and some census tabs into the 75 areas into which Chicago was divided on about 18 characteristics by age. This was before the day of the computer. Deborah and I did this on nine-key adding machines, working days and days and days. It turned out that the Burgess hypothesis didn't work.

When I found that out I was deeply distressed, because I had assumed it was going to work because much of the Chicago school of work on social disorganization was based on the idea that mobility produced social disorganization and since high rates of social disorganization decreased with distance from the center of the city that ought to be true about migration as an index of mobility as well. What I subsequently decided, changed the whole thrust of the dissertation. That was that one had to take into account the social framework of the migration and that migration in itself would not be disorganizing if one moved between similar social environments. For example, I found that migrants who came from farm areas into the city were concentrated in the center of the city and that was a source of disorganization, along with immigrants from other places who were also moving between dissimilar environments. On the other hand, far out in Hyde Park, for example, that was an upper middle-class professional area, with a lot of migrants and a lot of mobility, but those moving there from out of Chicago were people who moved with sophistication from one area to another. So I recast what I had and it came out very well.

When things first didn't work out, I went to see Professor Burgess. He was not on my committee, which consisted of W.F. Ogburn and Louis Wirth. I said, "Professor Burgess, I've been working for six months with the data that you know about, testing the Burgess zonal hypothesis, and the data don't seem to fit the hypothesis." To which he replied, "Young man, something is wrong with the data."

**VDT:** I think your resolution of that lived up beautifully to a remark you made in your 1988 PAA paper on the Michigan model of graduate training ["Graduate School Training of Demographers: The Michigan Model"]. You said that in the Michigan apprenticeship system, "We believed that learning to do research should involve learning to meet the unexpected," and as an example of that, you mentioned "relationships contrary to the initial hypothesis." So you recast your original hypothesis and the data then did fit.

**FREEDMAN:** Yes. By the way, my dissertation was published. Philip Hauser, who was at the Census Bureau most of the time I was at Chicago and came back occasionally, came back in 1947 when I came to Michigan to set up what became the Population Research and Training Center. He began a publication series in demography and population at the University of Chicago Press and I was fortunate enough to have my dissertation selected as the first monograph, Recent Migration to Chicago

[1950], which was the first book publication I ever had. It was a nice thing for an instructor--in those days, one was instructor for four years before becoming assistant professor--to have in his second year at the university; gave me a boost there.

**VDT:** You described in your 1979 interview how you switched from migration to fertility, specifically from migration to the analysis of the Indianapolis Fertility Study.

**FREEDMAN** [combines answers in 1979 and 1989]: I had always had some interest in fertility but had never done any research on it. My first five or six publications were all in migration, most of them jointly with Amos Hawley. I gave a migration paper, probably in 1948, at the American Statistical Association in a session arranged by people from PAA, and Clyde Kiser and Pat Whelpton were in the audience. They were the central figures in the Indianapolis Fertility Study. They invited me to have dinner with them and propositioned me: "We've got this marvelous body of data, collected just before the war. We haven't gotten it to the analysis. It's too big for any small group. We'd like to know if you're interested in that." I'd heard about the study and said, "I'm sure I'll be interested in looking at it, send me some of the materials and I'll get back to you." They did that almost immediately.

Those were the days before the big grants and before people had to be paid for doing research. I was teaching four courses a term at the time and we talked over the logistics of it. All this was on punched cards at the Milbank Memorial Fund [where Clyde Kiser was, in New York City] and they promised that if I would send tabulation requests to Clyde Kiser, he had a 101 tabulator and would produce tables for me. I think they also gave me about \$500 for clerical help. I had learned at an early age to be a bargainer, so I said I'd be glad to do it, but I'd be much more interested and we could do more work at Michigan if they would find fellowships for graduate students who would also work on it. So, in addition to the work I did, which involved three pieces jointly authored with Pat Whelpton, two graduate students got dissertations out of it. The person who is still known in the field was my first Ph.D. dissertation student, Jack Kantner, who has recently retired from Johns Hopkins.

**VDT:** I interviewed Jack a year ago in Bedford, Pennsylvania, and he raved about what a wonderful experience it was. He mentioned, for instance, that once he and some other graduate students drove down with you to see Whelpton at the Scripps Foundation in Oxford, Ohio. It was a wild ride in a car that you'd borrowed from your brother-in-law. Your mind was on the fertility survey and not on the road, but you got there all right.

The Indianapolis Fertility Study set the pace for the series of national fertility surveys that followed. But one thing that is always pointed to in it is that it was looking at the social and psychological variables in fertility but there was little yield from the psychological variables. Nor was there in the longitudinal Princeton Fertility Study which followed it. I think you too felt at the time that there wasn't much yield from psychological variables. Do you still think psychological variables can't tell you much?

**FREEDMAN:** I would say that in the sense that we worked on them then that not much came of it. In a much broader social-psychological sense, I think there still may be a great deal in it. For example, the ideational hypothesis of fertility change has re-emerged in a kind of classic recent article in Population Studies by Cleland and Wilson and work by Ron Lesthaeghe that emphasizes the role of ideas in this field. That's something that's gone up and down. Demographers in general have been rather skeptical about these things. I was trained as a human ecologist to be skeptical about it; ecology does not in general involve those concepts. But I've always felt that one should keep an open mind on matters. I think that's an important hypothesis now. I think of what emerged from the Princeton European Fertility Study, looking back at the demographic transition in Europe; ideational elements were very important then. And that's especially important in this age of instantaneous communication

of information around the world.

I've never felt that individual psychological variables yielded very much and I think they still don't. The longitudinal Princeton Fertility Study [of women in the U.S.'s eight largest metropolitan areas who had their second child in September 1956] was an offshoot of the Indianapolis study. Some people were involved in both. Charlie Westoff [first director of the Princeton study] was a research associate on the Indianapolis study [at the Milbank Fund]. Two psychologists who were on the Indianapolis study board, Dan Katz, who is still here at Michigan, an emeritus, and Lowell Kelly, who was also a Michigan person, and Donald Marquis, who built up Michigan's very important psychology department after World War II, were all members of a group that met prior to the Princeton study to talk about what was going to be done. One of the basic thrusts of the Princeton study design, which involved Bob Potter, Philip Sagi, Charlie Westoff, and a psychologist named Elliot Mishler, who was also a Michigan person at that time, was the exploration of a whole series of psychological variables. I think the record shows that those didn't work out very well either.

**VDT:** In the 1979 interview, you also told how P.K. Whelpton got you involved, along with Michigan's Survey Research Center, in the 1955 Growth of American Families study. You also described that very well in the videotaped interview you had in Barbara Wilson's series on national fertility surveys [described by Barbara Wilson in "Videotaped Interviews about American Fertility Surveys," *Vignettes of PAA/U.S. Fertility History, PAA Affairs*, Winter 1985].

**FREEDMAN** [from the 1979 interview]: After the war, Pat Whelpton led in putting out population forecasts for the Census Bureau and he had the unfortunate experience that comes to demographers: those forecasts couldn't be printed fast enough to be right. By the time they got to the Government Printing Office, they were wrong. Pat was trying to use cohort fertility approaches and looking at historical data to make projections. Around 1954 he came to see me in Ann Arbor; by that time, I had become involved with the Survey Research Center at Michigan. He said it might be worthwhile if we could produce a survey in which we would get women's fertility history and ask them how many additional children they expected to have. I got enthusiastic. We got a grant from the Rockefeller Foundation and that began the series of national surveys. We did it in 1955 and Pat and Art Campbell did it in 1960. In 1965 and 1970, it moved to Princeton and then--the goal which we had worked toward for a long time--it was taken over by the federal government in the National Center for Health Statistics [National Survey of Family Growth, beginning in 1973].

I worked very closely with Pat Whelpton. He was a good man to work with. We had a lot of arguments. Pat was a stubborn fellow; he got an idea and he wanted to stick to it. I'm not entirely flexible either. We argued a lot, but we were never angry; we convinced each other of one thing or another.

**VDT:** Are you pleased that the GAF led to the series of U.S. national fertility surveys and the World Fertility Survey and its successors?

**FREEDMAN:** Yes. I thought it was very important at the time, and I think in retrospect it's very important. I might say something here that is relevant to my own evaluation of my career. I reflected on it some over the weekend in preparation for meeting with you. I think that overall whatever I may have contributed to the field is not so much in the substantive research I have done; most of that will die, much of it is dead now, some of it that's still there will die--the field changes. I think that what I have contributed lies in two areas. One is that I have been fortunate enough to be in a position to take some initiatives which I think had a lasting impact in that they influenced what other people were doing; they opened a whole area of work. The Growth of American Families study was certainly one of those areas; perhaps, in many respects, the most important. The other area is through my students. I

think that's more important, probably, than the first thing. That is, all of these other things pass away. People forget about them, and they will forget about me. What effect I'll have in the future, I think, will be through my students and their students. In my last two years of teaching, I had the privilege of having in my class three students whose fathers or mothers were students in that course 25 years earlier.

**VDT:** Wonderful! I was going to get onto your students, because that is indeed a tremendous contribution, going on through your career. On the GAF, out of that you wrote the splendid book, with Whelpton and Art Campbell, Family Planning, Sterility and Population Growth [1959]. The 1955 survey took place almost at the peak of the baby boom [1957] and, of course, your projections for U.S. population were off, though not as much as some of Whelpton's earlier projections. Your medium projection for the U.S. population in 1990 was 273 million and in 1989, this year, the estimate is actually about 249 million. You were off about 24 million; that isn't so bad. Why did you not continue with the Growth of American Families study?

**FREEDMAN:** I got involved in working in the Third World. I had arranged for the second study to be done here at Michigan and then the question was what could be done later. Charlie Westoff and Norm Ryder were interested in taking up the task and I was glad to have them do it. In 1960 the Population Council subverted me by sending me to the Third World for the first time. They sent me to India for two months and on the way back I stopped in Thailand, Hong Kong, Japan, and I became interested in what was going on there. The Population Council then asked me if I wanted to be involved in the Taiwan work. And at the same time, I founded the Population Studies Center. So, the question was how to handle all of this. I felt that the GAF was well launched; there were good people who were going to take it up, and I went on to other things.

**VDT:** Before we get into the Population Studies Center and Taiwan, could we finish up on your domestic side? About 1954 you included fertility questions in the Detroit Area Study. Did you start the Detroit Area Study?

**FREEDMAN:** Yes, I started that with the inspiration of the late Angus Campbell, who was director of the Survey Research Center of the Institute of Social Science--a remarkable man. He really introduced me to survey research, which is relevant to my whole career. GAF would not have happened without this. Angus and I had become friends and he knew that I had worked on the Indianapolis study but had never done a survey and didn't know a lot about survey research. He gave me a faculty fellowship for a year, gave me half-time to wander around the Survey Research Center and look at the data sets there and I found I'd published a few things based on those data. I learned a lot from Leslie Kish, who was head of the sampling section, and Charlie Cannell, who was head of the field section. These people are still at Ann Arbor, still friends of mine. And I became interested in survey work.

Then Angus and I had the idea--I think he really was the originator--of starting what we then called a laboratory for sociologists and other social scientists, political science and economics, by doing an annual survey in Detroit, using good methods, doing a real project in the sense that we always had a faculty investigator who came in with a problem and the students would be involved for a semester working on the substantive problem. They would simultaneously get didactic training on interviewing, sampling, what have you, and participate in every phase of the work, from helping to draw up the questionnaire; they all had to interview and code. And that lasted. As director in the first few years, I took the privilege of including some questions on desired and expected family size. Pat Whelpton knew that I had done that and knew that I had spent time at the Survey Research Center. So that was the background for my getting into survey work.

I've used the more standard, traditional demographic data sources: census and vital statistics

data. At that time, very few demographers used surveys; the Indianapolis survey was a unique thing. But most of my career has been involved with the application of survey methodology to demographic and related social science problems. And it came about in that way. It was a fortunate thing for me that I was at the University of Michigan which had, and has, what I think is the world's leading survey research center and that I became a friend and colleague of people like Angus Campbell and Leslie Kish. Our careers have intertwined ever since. When we come to China, I can tell you about involving Leslie Kish in survey sampling in China.

**VDT:** Did you have the idea of the longitudinal study that began with the Detroit Area Study of 1962?

**FREEDMAN:** That was the joint brainchild of David Goldberg and myself. We were the principal investigators for the Detroit Area Study that year. We thought it would be a good idea to make this longitudinal and we added something that at that time people didn't do. It was a survey on family life and fertility and at the end of the interview we told the people we were likely to want to come back and talk to them again, because we were interested in how their families were going to fare. We asked them for the names and addresses and telephone numbers of three friends or relatives who would know where they were if they moved. We've used that and asked those questions again ever since.

David went on to other things and I brought into the Population Studies Center Lolagene Coombs--she was W.F. Ogburn's assistant at the University of Chicago as Lolagene Convis--and she worked with me on that longitudinal study. One thing we did was we began using the telephone for the re-interviews. That's very common now; it was not common at all at that time.

After a number of years and quite a few publications in which Lolagene was involved with me, we put that to bed when I got heavily involved in Taiwan. Then some years later, my good wife Deborah and my former student and colleague, Arland Thornton, picked it up and it's still going strong and major analysis on that is still going out.

**VDT:** At the same time, you started the Population Studies Center and you said the Population Council got you involved in Taiwan. Jack and Pat Caldwell in their book on Limiting Population Growth and the Ford Foundation Contribution [1986] described how Michigan's center began. You went to the Population Council with a rather modest funding proposal and Dudley Kirk and Parker Mauldin took you to Oscar Harkavy at Ford. In 1961 Ford gave the center more than you asked for, while Penn, who were applying for their center at the same time, got less than they asked for. How did all this come together? It was your idea to set up the Population Studies Center and you said your interest in Asia was aroused by your Population Council trip to India. How did it all fit together?

**FREEDMAN:** I think one reason the Pop Council and Bud Harkavy were interested in Michigan was that I had already indicated my interest in the Third World and had begun negotiations to work in Taiwan. And that was a major objective of the Ford Foundation; they were interested in Third World fertility and family planning programs. My work on the Indianapolis study and GAF were preparations for applying these methods overseas. I had indicated that I thought they should be, and was beginning to work in those directions.

Now, when I went to Ford for that first interview with Bud, I didn't have the concept of the Population Studies Center. I just wanted to expand a very small internal program we had. I didn't know that anybody was going to provide money for overseas activity, although we had begun to talk about Taiwan on a small scale. I was quick to jump, however, when Bud outlined these larger opportunities; he wanted me to create an all-university institute, to found a big empire, involving public health, psychology, anthropology, and everything else.

I talked to the people back here and went back and talked to Bud. I said that I didn't want to be an empire-builder and I didn't want to be an administrator; I wanted to remain a scholar. In any case, I



thought that we could do better work if we had a reasonable size group which had their base in specific social science activity and we would do it in sociology and economics. We didn't have an economic base at that time; I said we would develop that. But I also told him I would be happy to help foster other things. What I did was to work with Myron Wegman, who was dean of the School of Public Health, and Moye Freymann, who was out in India, North Carolina later, was brought here for a time and I helped found the Population Planning Center, but as an independent enterprise.

**VDT:** Michigan's population program, set up in 1965, was to have three branches?

**FREEDMAN:** Right. We had at that time also an activity in the medical school which died out, but population planning is thriving. It is a full department of the School of Public Health and we have many interrelations with them.

**VDT:** Although there were strains for a time, weren't there?

**FREEDMAN:** Yes, the second dean of the School of Public Health abolished the population planning center. The current dean, June Osborn--great authority on AIDS--revived it. She had decided either to kill it as a program or make a department of it and she made it a department, the Department of Population Planning in the School of Public Health.

**VDT:** About Taiwan, did you conceive of the Taichung experiment? The Caldwells call that and your later work with the Taiwan family planning program a landmark in the demographic study of developing countries.

**FREEDMAN:** The background of this is that the Population Council told me they had been visiting people in Taiwan in the rural health division of what was called the Joint Commission of Rural Reconstruction. That was an organization that did a tremendous job and laid the basis for the current development of Taiwan, beginning with rural land reform. The rural health division was working with the provincial department of health. At that time, Taiwan was one province and then and still is only one province of the Republic of China, which claims sovereignty over what they consider all of China. Taiwan now has Taipei and Taichung as separate entities.

The rural health people had some contact with the provincial health people who had begun some family planning activity through the maternal and child health division and they were interested in some statistical and social science background. The Council asked if I would be interested in working on that. I was just starting the center and told them I didn't want to get involved unless I had somebody who was competent and would stay there for three years. I would spend as much time there as possible. Deborah will tell you that I spent a lot of time. I took it out of my hide, if I may say so. I spent all my vacations there, would try to have my courses collapsed into shorter periods and so forth.

But I did not go for an exploratory visit until Yuzuru Takeshita became available. He was then known as John Takeshita. He spoke Japanese fluently. He was born in the U.S. but spent his grade school years in Japan in his family's home village, came back to the U.S. a month before Pearl Harbor and was incarcerated in one of the Japanese camps. He had finished his dissertation at Michigan, which was based on a survey in Osaka, Japan, similar to the Detroit Area Study, and was willing to go to Taiwan. When I found that he was available, I told the Council, "Now I'm ready to go. I'll go and we'll see if we and the Taiwanese like each other; if we can work together." I went with the late Marshall Balfour, who knew East Asia and the Taiwanese; he'd been in Asia in 1948 with Frank Notestein and Irene Taeuber. We went and I fell in love with the Chinese. They were my kind of people. They were clearly pragmatic and empirical in orientation.

The people in Taichung were medical people and they were doing surveys in a very rough kind

of way. When I told them how we did surveys and solved problems, I could see their eyes light up with interest. I also found that unlike many observers, including some professors in Taiwan I talked to, they weren't afraid of the problems. I had talked to a prominent professor at the National Taiwan University who had some demographic training and he said, "A family planning program will never work here and you can't interview people about how many children they want because people who've had more children than they want would never say so because that would imply that they wanted those children dead and the spirits of the dead would come after them. You couldn't do it." The public health medical people had a completely different idea. They said, "We're talking to people all the time about pregnancy and birth and the beginnings of the use of contraception and we find they're very interested in those things." These were the people in maternal and child health in the Taiwan provincial institute. The director, J.Y. Pong, now retired, was my counterpart. We agreed that we were going to do some work together.

Before I went to Taiwan, I had been in interaction with the late Bernard Berelson at the Population Council, who was a major influence on my career and life; close friend, close collaborator. Barney had the idea that we ought to do a large social experiment someplace, applying the best we knew from social science methodology, to test whether people in Third World countries would talk to you about these things, whether if you offered contraception to them, it would work. When I came back from that first trip, I told Barney, "I think I have the place. I think this is manageable, they're ready, they'll cooperate with us."

We had agreed to do a pretest and we went back and interviewed about 300 women who were below 30 years of age and had at least two children and at least one son. We interviewed those 300 women and if they weren't using contraception--most of them weren't--we asked if they'd like somebody to visit them, and we pretested whether they would accept. J.Y. Pong did that: "We Chinese did this," you understand. By that time, Yuzuru Takeshita was there with his family.

And the experiment worked! We had something like a 98 percent response rate. Something like 25 percent of the women who said they wanted contraception began right away.

I called Barney from Taiwan and said, "This is the place," and Barney flew out there and we sat with our Chinese friends for three or four days and sketched out the design for the Taichung study. He left and we went to work on that. That was the beginning of the Taichung study. We got heavily involved in that, of course. We published a good many things before the book, Family Planning in Taiwan: An Experiment in Social Change, which didn't come out until 1969.

Everything we did was oriented to the idea that the research was not to interfere with the public policy action that was going on. So we would first analyze what was needed to get quick feedback for the people who were running the program. For example, in the Taichung study--at that time we were using IBM cards--and we had a program card first on which we put the background things that we thought were of greatest importance--social and demographic things but also the program things with reference to what people wanted. We coded and ran those things first. That was always the pattern of our work in Taiwan, because we felt we had to persuade them that the scientific work we were going to be doing was going to be useful to them as well as relevant for our publications.

**VDT:** Wonderful way to work together. The Caldwells point out that the Taiwan program, started with the Taichung experiment, was an inspiration for other Asian countries--particularly South Korea, wasn't it?

**FREEDMAN:** Yuzuru and I consulted there. They asked us to come after we got the Taichung study under way. And Marshall Balfour asked us to go there again. He went with me and introduced me.

**VDT:** Did you have a similar experiment there?

**FREEDMAN:** No, there we were purely consultants. We brought a couple of Koreans here for training. Dr. Sook Bang, who was an important figure in those early days, got his Ph.D. with Population Planning here. He was an M.D., so that was appropriate, but actually he sat in our center when he got his dissertation done.

**VDT:** So there was this contact with you Michigan people and then they spread through Southeast Asia?

**FREEDMAN:** An example was John Ross. He's been an important figure over the years in fertility and family planning research. He was teaching sociology at a Michigan college when he heard a talk I gave on the Taichung experiment at a meeting of the Michigan sociological society and he came and asked what he'd have to do to get to be a demographer. We got a postgraduate fellowship for him at Michigan. He subsequently worked in Korea and did a good many other things for which I don't claim any credit--John's a very bright guy. But that's some idea of the linkage.

**VDT:** You've written about the networking that went on among your colleagues and former students, especially in Asia with the Organization of Demographic Associates in the 1970s. You have explained that you yourself didn't stay too long in the developing countries where you worked and the Caldwells said that faculty members set up the projects that were carried out by your collaborators, in this case, Takeshita in Taichung.

**FREEDMAN:** Takeshita was a staff member here. He was there for three years, but the work all the time was done by the Chinese. There were very few Americans involved.

**VDT:** Who actually wrote the book and the articles, you or the collaborators? Of course, all your names were on them.

**FREEDMAN:** The Chinese wrote some of the articles, but basically we did most of the writing, but not me personally. Many people were involved in that book. Yuzuru was a primary coauthor, but we had James Palmore, Bob Potter, and L.P. Chow, director of the Taiwan work for a number of years and now a professor at Johns Hopkins, was active in this. The Chinese weren't prepared for scientific social science writing at that time. Over the years, they gradually became adept and we became consultants. They have very competent people like Dr. T.H. Sun, one of my Ph.D. students who was director for many years, and the current director, Dr. Ming-Cheng Chang, at the Taiwan Provincial Institute for Family Planning, which is the successor to the Taiwan Population Studies Center. They've got other PhDs and other people we've trained. We helped a lot in the early years, maybe five years, but these Chinese became increasingly independent in doing the work. I've been there as a consultant at least once a year every year since then, except the year I had my heart attack and the year I had heart surgery. I was there last year and I'll be going again this fall. Currently, we're writing two books about the whole Taiwan experience jointly, a group at Michigan under Al Hermalin and Arland Thornton--not my leadership--on this side and Dr. Sun and Dr. Chang on that side.

**VDT:** Why two books?

**FREEDMAN:** We're writing one on the changing Taiwan family and another on the demographic transition in Taiwan. There's so much on the family and Arland is interested in that, so we're writing two books.

**VDT:** Is the family book the one Deborah is involved in?

**FREEDMAN:** Yes, she's involved in that also. While we were writing the first book, I brought Al Hermlin in from Princeton. A little story on that. Larry Bumpass did his doctoral dissertation here and had been my research assistant. A very distinguished person now in the field; I just got through reading his latest article. Charlie Westoff hired Larry to work with him on the continuation of the GAF study and I called Charlie and said, "Charlie, we must talk baseball here, you got my prize first baseman. You've got to give me somebody in return. I need somebody." I had offered Larry a job but the opportunity came at Princeton and I said, "You should go because there's a different tradition there; it's a great place." I didn't try to keep him; didn't sway him at all. Then Al came and he had a hand in the finishing of that book, but also became a leader and gradually became one of the important figures in the continuing work on Taiwan. Then he branched out, of course, in many other things. He's very distinguished and won PAA's [first] Robert Lapham Award [for "distinguished contributions to the application of demographic knowledge"] last year [1988].

## **BREAK HERE**

**VDT:** We've just taken a break and I've been introduced to the famous coffee break that takes place every morning and afternoon at Michigan's Population Center, with interesting visitors as well as all the staff and students gathered here. One question I haven't asked is did you ever learn Chinese?

**FREEDMAN:** No. I thought about it. I talked about it with my friend Alex Eckstein, a specialist on Chinese economics, who spent 18 months in Taiwan trying to perfect his Chinese. I was on a five-ring circus and the question was should I take a couple of years to learn Chinese or should I proceed with the Taiwan work and the Michigan center and everything else. I decided that I might very well stop and not learn Chinese very well anyway. It looked as if I'd be involved in other countries; I went to Korea, India, and people wanted me to go to Indonesia. I decided that, yes, it would be much better to know Chinese, but that just wasn't going to work; there was too much to do.

When I went to India in 1960 on that first trip for the Population Council, I had begun to practice interviewing people with the aid of an interpreter and I think I got to be pretty good at it. It's not the same as interviewing yourself but you can do a pretty good job if you use some tricks that you have to know. For example, always look at the person you're talking to, never at the interpreter. The interpreter is in the background interpreting; a good interpreter will be as inconspicuous as possible. But the fact that I can't read Chinese still gripes me. I've been fortunate enough most of the time in having good people to translate for me. But it was a question of what to do with my time. I regret that, but there it is.

**VDT:** The Caldwells say the Michigan program had more measurable impact on the Third World situation than any other U.S. university population program. We've talked about the connections with Korea. You sent some people to, or worked in, Hong Kong, Malaysia, Thailand?

**FREEDMAN:** Yes. Let me say first that the program in Taiwan had a great deal of influence because, in the first place, it was a pioneer and, in the second place, it was well documented and we had the Taichung survey instruments, which were modeled and adapted from things we had been using in the Detroit Area Study and GAF. We had more influence, I think, than Korea did for two reasons. One was that we published things right away and we made all the instruments available. Secondly, at an early point our friends in Taiwan were very entrepreneurial and set up an international training center to which people came and were briefed in what was going on. An example is Dr. Harjono Sujono, who has been head of the Indonesian family planning program for many years. After he got his Ph.D. at Chicago, he stopped on the way home in Taiwan and spent some time looking at what was going on there. Then the Indonesians went on to do things their own way, which is the reason the

Indonesian program is a success. Hundreds of people passed through that training center. It was a whole apparatus which had Population Council and other funding support for a time, in addition to what was going on in the program. So that had quite a lot of influence.

Plus the fact that I was on the hustings talking about Taiwan in various places. Yuzuru and I had a paper at the famous Geneva meeting in 1965, which was the first really big international meeting on fertility and family planning. Out of it came a big book edited by Berelson et al. [Family Planning and Population Programs: A Review of World Developments, 1965]. I think the influence fanned out from that. Also, of course, while this was going on we trained a lot of students here.

Now, all of this began to have an impact in other places. Jay Palmore, who is a major figure in the field, very important person at the East-West Population Institute, was an assistant professor here at Michigan when we were writing the Taiwan book and helped write one of the chapters, although he hadn't been to Taiwan at that time, but he was brought up in the Philippines so had had a background in the Third World. We were asked along with the people in the Taiwan Population Studies Center to do a benchmark survey for the Malaysian family planning program. I organized a team consisting of Jay Palmore with two of our graduate students, Allan Schnaiberg and Chris Langford, who is now a professor at the London School of Economics. We did the survey, which was the background for the beginning of the family planning program.

At the same time, our friends in Michigan's population planning center were advising about the family planning program. They wanted a field man and I got my friend, Dr. J.Y. Peng, whom I worked with in Taiwan, to go to Malaysia; they have a Chinese as well as Malaysian population. I also got a young Ph.D., recently graduated from the University of Chicago, by the name of Lee-Jay Cho, and gave him his first postdoctoral job as adviser in demographics in the Malaysian department of statistics. Lee-Jay honors me as his guru because of that. Another network connection.

Other offshoots. I've mentioned Korea. At an early point, I became consultant to the Hong Kong Family Planning Association. Hong Kong's birth certificate at that time did not have the age of mother on it, so they didn't have any age-specific birth rates. I had done some preliminary work using indirect estimation methods. I'd published an article with a couple of our Indian students here which demonstrated that the decline in fertility that had been trumpeted in Hong Kong between 1960 and 1965 was really a result of age and nuptiality factors and not a decline in marital fertility. We set up a program at the Hong Kong Family Planning Association to estimate age-specific fertility rates by drawing a sample of 10,000 birth records from the nursing homes and hospitals where most of Hong Kong's births occurred; almost all the births at that time were occurring in institutions. We did that study for three years and published a number of things in which we were able to chart what was happening. I have had a continuing association with Hong Kong. In 1987 they did their fifth KAP survey and while I was at Hebrew University I was consulting with them. And I was there in 1988, when we had a press conference about what had happened there. Their total fertility rate is down to about 1.4 and they've got an 82 percent contraceptive prevalence rate, and so on.

**VDT:** Talking about KAP surveys, did you set up the series of ongoing KAP surveys in Taiwan?

**FREEDMAN:** We get involved with most of those; more heavily with some than with others. They're quite capable of doing their own surveys. They do many surveys without us, but we collaborate. For many years after each of the KAP surveys, we have been publishing an article on trends in Studies in Family Planning. The one on the basis of the 1985 survey was published in 1987, I think.

**VDT:** That's right, you've kept it absolutely up to date. You also have connections in Thailand?

**FREEDMAN:** Yes. They started their first work in family planning with a pilot project somewhat

modeled after Taichung.

**VDT:** I was going to ask, were there any pilot projects other than Taichung?

**FREEDMAN:** Yes, there were the ones in Korea. The one in Thailand, the Potharam project, was the one that had a heavy Michigan involvement. They approached me about doing it and I was too busy and I talked to Amos Hawley and he went as the principal demographic consultant. He needed a field man and I recommended Dr. J.Y. Peng again and he went there. That was a great success.

After that I was in Bangkok occasionally as a consultant, but other people did much more; Allan Rosenfield and Warren Robinson were there. We used to visit to consult, but I won't say we had a heavy role in that. More recently, my colleague John Knodel has become, I think, the leading non-Thai demographer working on Thailand, so he has a continuing association there. We've had Thai students. One of our prize students was Dr. Napaporn Chavoyan, who is at Chulalongkorn and doing wonderful work. She has done a lot of work with John Knodel. Brown University has had a long connection there and has had many Thai students as well.

I guess I should insert that in this short period in which I'm talking about myself, I may sound megalomaniac and it may give you the impression that I think Michigan did it all.

**VDT:** Some people think so!

**FREEDMAN:** I want to emphasize the fact that there were a lot of other centers involved who were doing important things. We crossed paths with Brown many places. They had a much more important role in Thailand, for example, than we did, until John got involved. After John came to us, I think we became an important element there. The fact that John spends half his time there is important. He knows Thai and . . .

**VDT:** Has done a lot of innovative work, such as his focus groups.

**FREEDMAN:** Yes.

**VDT:** Let me throw this at you. Peter Donaldson and Charles Keely had an article in the Family Planning Perspectives 20th anniversary issue of November/December last year, 1988, on the international population scene ["Population and Family Planning: An International Perspective"]. They wrote that centers like Michigan and Chicago which trained so many Asian family planning leaders--they mentioned, for instance, Harjono--have declined in importance in that respect and that those programs need to be revived or alternatives developed if both family planning programs and population studies are to become stronger in countries where they are not yet well established, especially Africa. Do you believe that Third World people still need U.S. training in demography?

**FREEDMAN:** Yes, I think so. They have their own training institutions, but I would say that at the Ph.D. level, they still need to go other places. There are many more resources in the countries than existed at the time we were doing this earlier work. I think what's needed now is more networking, more strong relationships of the kind that we had with Taiwan and that Brown has had with Thailand, in which you had people going back and forth and there's a great deal of collaborative work going on. And in which the people who go back home know that they've got technical backing and the opportunity to come back to the place where they were trained and a great deal of collaborative work can be done, so that they've don't get isolated from what's going on in the outside. That I think is certainly true with respect to Africa, which is a much more difficult place in terms of creating institutions and so forth. There, of course, Pennsylvania has taken . . .

**VDT:** A lead. Etienne van de Walle.

**FREEDMAN:** Right.

**VDT:** Now on a related topic. In the 1960s, there was some strain between demographers like yourself who were becoming involved in international population programs and the more traditional demographers. Donald Bogue in his interview in this series said that he, you, and Joe Stycos were leaders in the demographic group who had faith that something could be done about the population explosion and you were opposed by others who, as you yourself put it in your 1979 interview and you also mentioned it in your 1965 PAA presidential address ["The Transition from High to Low Fertility: Challenge to Demographers," Population Index, October 1965]: these others felt that involvement in family planning programs was not quite scientific, just social work. Could you say something about that controversy?

**FREEDMAN:** I never regarded that as a real dichotomy; I didn't think one had to choose. My point of view always was that one of the facts of social life in the Third World were these programs and they were to be studied. They were elements of the demographic situation. I felt, still feel, always felt, that they needed to be studied, as objectively as possible.

I was not as great an enthusiast and optimist as Don Bogue was. I always felt that one had to have a proper amount of skepticism about what was going on and not go overboard. I think I did go overboard myself. I not long ago re-read my PAA presidential address and while there are hedges in there about what was going to happen in India and Pakistan, for example, I thought there were signs that things were going to change much more rapidly than they have.

**VDT:** That's right. You said that within five years, there was to be a significant fertility decline in India and Pakistan.

**FREEDMAN:** That's one of the many ways in which I was wrong. On that score, by the way, I don't have any apologies, because most of the prominent demographers in the field have been wrong whenever they make projections. One of the principles I've learned is that social scientists greatly under- or over-estimate the rate of social change and, in any case, the projections are very often wrong. But I think that's part of the scientific process. I remember thinking about these things and what I should say in my presidential address and I thought I should say what I thought was going to happen, the possibility, perhaps colored by my experience. I tried to be as objective as possible and I tried to instill that in the organizations that I was working with.

On the other hand, I must say that I felt that in order to be credible to the people I was working with, to get access to study what was going on, I also had to help them with the problems they had. They would not have been very satisfied if I had said, "Well, I'm here just to study these things and I'm not going to talk to you about the program."

At first I was reluctant to say anything about policy and the program, because I felt I really didn't have any training in that. I thought public health people knew the answers to those questions. I came back and started to talk to public health people and decided that despite all their merits, they didn't really know much more than I did. They were trying to find out what they knew; they didn't know very much. They hadn't really researched this sort of thing. So I felt that I should just give the best advice possible.

I tried to maintain a balance between these things and to express proper skepticism. For example, I think in our 1969 book on the Taichung experiment there is some extrapolation with skepticism. Usually while I was there, I would say I spent two-thirds of my time on what I would say

was social scientific work and perhaps a third of my time finding out what was going on in the program. And I usually left them a four- or five-page memorandum about--given what I knew from the social science point of view--what I thought was happening on the program side and suggestions about what was feasible and might be done. And I feel that there's nothing wrong with that. I feel that those of us who did these things got information about what was going on in the program that many of our critics did not have. People like Kingsley Davis, for example, more recently Don Hernandez, and other people rarely studied--Kingsley, so far as I know, not at all--what was happening in the program. They had a kind of macro view of the situation and didn't have what I think are essentials to assess the role of these things.

In any case, it seemed to me--and still seems to me--that the way these questions get resolved is that people publish as openly as possible what's been done, open themselves to criticisms and corrections.

**VDT:** You and Barney Berelson certainly did that with your January 1976 Studies in Family Planning article on "The Record of Family Planning Programs." You were certainly then speaking to policymakers--I would say U.S. international policymakers, not just the program people in the countries. Presumably you set out to do that to refute the Bucharest "development is the best contraceptive" theory that had been espoused for a year or two before that. You showed that family planning programs had had a measurable, independent effect on fertility decline. But you hedged your conclusions there: the record was mixed, you said at that time.

**FREEDMAN:** That's still true, of course. It's much more complicated than we thought it was. About the other thing I've learned over the years. When I began in the field, it was easy to concoct theories because we had few facts; it was much easier to deal with the theories when we had few facts. We've learned a lot since then, which has made simple theories much more difficult.

**VDT:** That's right. I was going to ask you about your contribution to modifying the simplistic demographic transition theory. You've just said you felt it was important to speak to policymakers--I'm also talking about policymakers in the U.S.--and you also must have talked to the press. In your 1965 PAA presidential address, you added a paragraph before it was printed in Population Index because apparently it had been covered by the press and they had interpreted you as predicting rapid fertility decline within the near future all over the world. Have you always felt it important to talk to the press as well as policymakers?

**FREEDMAN:** Yes.

**VDT:** Was there press coverage of that speech?

**FREEDMAN:** Yes, people called me afterward. I had quite a lot to do with the press on the Growth of American Families study, because that was very unusual. There were columns written about it and I learned the hard way that once you speak to the press, you have no control over what goes forth. Yes, I've spoken to the press. I've testified before congressional committees. I suppose half a dozen or more times, I've briefed various AID officials. When I was coming back from China after the one-in-a-thousand study [of 1982] that we worked on there, the cultural affairs and economic attaches in the U.S. embassy came to a seminar we had in Beijing and asked if I'd do something with the State Department [in Washington]. They called me in together with Bill Lavelly, my young collaborator on the China work. I've had a fair amount of that in the past, speaking to the press, interviews, things of that sort. I've always felt that was a responsibility.

I've been very happy with the growth of two institutions which I think are important. One is



the Population Reference Bureau, the other is the Alan Guttmacher Institute, which I think have played a very important role in providing data and information--people who are expert at dealing with the media. I was chairman of one of the first committees that the PAA had on public affairs, before I was president of the PAA. [From the 1979 interview: At the meeting before the 1960 census, an ad hoc subcommittee of the PAA was formed, consisting of Dudley Duncan, Dorothy Thomas, Frank Notestein, Phil Hauser, Con Taeuber and myself. It was before COPS (Committee on Population Statistics) was formed, but we were trying to perform a similar function. That committee decided that it would be a good idea if the Census Bureau had a one-in-a-thousand sample and Con told me later that that was the shove that produced the one-in-a-thousand sample of the 1960 census. After that I became chairman of COPS; Dudley Duncan took it over when I became president. There were a number of things that the committee did at that time in connection with both the Census Bureau and Vital Statistics. We had an important role in connection with what I think is still going on, the questionnaire of followback studies of samples of mothers taken from birth statistics. We had a very important role in pushing and shaping that.]

I was in Hong Kong in 1987 for a press briefing on their KAP study and appeared on the front page of three Hong Kong newspapers, because they interviewed me about Hong Kong and then, of course, they wanted to know about Mainland China. Yes, I've done a fair amount of that over the years.

**VDT:** Your family planning research, which raised some controversy among mainstream demographers in the 1960s, would you say it's now respectable among mainstream demographers? If so, you helped to make it so.

**FREEDMAN:** Well, it's moderately respectable. There's still a lot of controversy. But I think the legitimacy of doing things in this field is there. It depends what one does. I think I'm one of those people who have kept their foot in social science and in demography. I think people like Jack Caldwell, for example, and many others recognize that I'm a reasonable person on these things; that I listen to what they say and read what they say. Some people get swallowed up entirely in the internal dynamics of the family planning programs and they have lost credibility. I wouldn't say it's a standard part of the field, although there still are a lot of publications in the standard journals, Demography, Population Studies, and so forth, that deal with these issues. I think that some of us have helped to make that more respectable.

**VDT:** What are your views now on population growth in the less developed countries? As you know, the population growth rate decline has slowed. In the Population Reference Bureau World Population Data Sheets--of course, you're not supposed to use them as a time series--but the world's population growth rate this year, 1989, is estimated to be 1.8 percent, up from 1.7 percent in last year's data sheet. And the UN has revised its projections upward. Are you depressed?

**FREEDMAN:** No. A large part of that recent increase is because of the little upswing in China. I think that we ought to remember one thing, that is that the fact that population growth rates haven't come down so fast is largely the result of the fact that the fertility decline has been matched--and in some cases exceeded--by the mortality downturn, and that's all to the good. It's still the case that, with ups and downs, in an increasing number of countries contraceptive prevalence rates are going up. About China, people are saying terrible things are happening, the birth rate is going up and there's a very high birth rate there. I think that's sort of a ridiculous view, that is, the total fertility rate has come down from 6 to 2.0 and it's bounced up to about 2.4. The thing to focus on is 6.0 to 2.4, in some places a range close to 2. It's just a remarkable change. It goes with fits and starts. Many people were pessimistic about Latin America. Many Latin American countries have had very substantial increases in contraceptive prevalence that were not expected at the time.

There are still many points of difficulty. I would say there's a long way to go in Africa. Change is still very slow in the Indian subcontinent. There has been some fertility decline in India, but not as much as some had initially expected. I might say that after that 1965 PAA speech in which I was so wrong, when I had an opportunity to observe the Indian family planning program and its difficulties, I became much more pessimistic. And nothing has happened in Pakistan; some progress in Bangladesh. And, of course, in the Muslim world, by and large, where you have Muslim fundamentalists not much has happened. But even there, there are exceptions. There are parts of Indonesia which are very strong Muslim areas in which the TFR is down, the contraceptive prevalence rate is up. The latest data from Egypt and Tunisia show contraceptive prevalence up, birth rates down.

No, I'm not pessimistic. But when I talk about demography, I always apply what I learned as a weather forecaster: that is, don't look out the window when you're making a weather forecast. Short-run trends are not the significant thing. I still feel that if one looks ahead for 25 or 30 years, there are going to be significant declines in fertility. But there are going to be a lot of difficulties in the period--no question about that.

Another way to look at this is to ask how many people back in 1960 thought the places that have had the fertility declines were going to have them. I would say very few. I was told by very distinguished demographers when I began to work in Taiwan that any program there would just skim the cream off the top and nothing much would happen. I remember that in the early 1960s there was a Milbank Memorial Fund meeting and Clyde Kiser edited the proceedings, Research in Family Planning [1962]. Taiwan and Korea are not mentioned in that book. And very few people expected that Thailand would have gone as far as it has. So we have to be very careful, I think, in making these projections.

**VDT:** Could you tell something of your view of "revisionism"--the idea that rapid population growth is not necessarily a barrier to economic growth. You and Bernard Berelson were already saying that in your lead article in the Scientific American September 1974 issue on "The Human Population." This was long before that idea became fashionable, with Julian Simon and the U.S. policy turnaround at the 1984 Mexico International Population Conference. Obviously, revisionism was not a new idea with the 1980s. Do you deplore, for example, the cuts in U.S. population aid and the shift to justifying family planning aid on the grounds of health: not being able to come right out and say it's on the grounds of limiting population growth?

**FREEDMAN:** I think it should be on both. Under Ray Ravenholt, the USAID policy was a very bad policy. Here is where I think that extremism did a very great disservice to the field: the idea that access to family planning would immediately lead to large fertility declines. I never held that point of view and I would say anybody with a real socio-demographic background would understand that that wouldn't happen. I think they greatly oversold what was possible.

I have never believed that you're going to convince anybody that they ought to be using family planning by citing figures about the GNP or macro growth rates. In Taiwan we were emphasizing from the beginning the health and welfare of the families that were involved. So I think that both of those have to be elements in the situation.

There's enough evidence for the revisionist view of the demographic transition. The Princeton European Fertility Study and other work have indicated that simple theories with respect to the role of economic and social development in fertility decline simply don't work. There are too many exceptions to that. But it's a long, complicated subject.

**VDT:** Right. But I'm glad we've touched on it and how you are among those who have revised the idea of the demographic transition. Now that we're on your theoretical work, I'd like to talk a bit of your theoretical work on the sociology of fertility. Along with everything else you were doing in these

years, you put out your first 1962 piece on "The Sociology of Human Fertility" [:A Trend Report and Bibliography," Current Sociology, Vol. X/XI, No. 2, 1961-62]. Was that the first time that you laid out your funnel model?

**FREEDMAN:** That was the first time I published it; I had been using that in my teaching for quite a long time. That is a very simple model, but it seems to have had a lot of effect.

**VDT:** A marvelous organizing framework. I used it in my first term paper when I was an M.A. student at Georgetown in the late 1960s, a paper I did for Murray Gendell on 300 years of French-Canadian fertility, from the 1600s, and got an A on it: 300 years of fertility laid out and what would explain it. That was just a wonderful organizing system and has been celebrated ever since. Were you conscious of the need to organize?

**FREEDMAN:** Yes. I did something there that nobody can do again, that I can't do again. I read everything in that long bibliography of 300 and some items that was attached to it. My essay was simply trying to relate to that bibliography, which I organized as far as I could in terms of that framework, and I read everything at that time. You can't do that anymore. I discovered that when I revised. My revision [The Sociology of Human Fertility: An Annotated Bibliography, 1975] was not as good as the original, I think, because I quickly discovered that I didn't know a lot of the literature and couldn't possibly cover it. I had it under my personal command when I did that first work.

I think I helped in a small way to pave the way for the important concept of the proximate determinants. Not because I invented it; I used the Davis-Blake intermediate variables framework and that had had some influence. But I think that I helped to popularize that and give it international currency, because I made it an essential element. I thought it was very important. Then John Bongaarts later picked it up and did what I didn't do and it's now quantitative and can be made part of models; there are various ways of using it.

**VDT:** He just worked on the proximate variables.

**FREEDMAN:** Yes. The proximate variables, which is simply another name for the intermediate variables, is, I think, one of the important empirical and theoretical contributions in the last 15 years of fertility study. That's being used all over the world. I don't want to detract in any way from the credit that John Bongaarts gets for really putting this front and center.

**VDT:** Yes, but his was only that one box and yours was the whole picture.

**FREEDMAN:** Yes, I had the framework going behind that. In general, when I did this diagram, the box farthest over to the left was the social and economic setting and a sub-part of that box was family planning programs. I don't remember whether that's clear in the 1962 version; I've had other versions since then. I've always put the family planning programs as a small part of the big box. The family planning program is part of the social and economic environment, part of the background. And I always made it small, as part of this larger thing, to indicate what I thought was its proper role; that is, it is not the determining role, it is part of the social and economic environment.

**VDT:** You summed up much of your years of experience and your philosophy, your approach to research, in your March/April 1987 Studies in Family Planning article, "The Contribution of Social Science Research to Population Policy and Family Planning Program Effectiveness." I wish all leaders in the field would write such articles. Back to Michigan and the Population Studies Center. Did you ever consider setting up a separate department of demography?

**FREEDMAN:** No, we never had any idea of doing that. In our initial proposal to the Ford Foundation and in every report of the Population Studies Center, we always had the idea that work in demography and on population issues should be grounded in the social sciences.

Now, there are other ways, as I indicated in my little message on graduate training ["Graduate School Training of Demographers: The Michigan Model, PAA meeting 1988]. There are programs which are demography and you can seat them in various departments in various ways; there are other ways of making that connection. I always felt it desirable that our people should be trained as sociologists, with a major emphasis on population issues. My colleagues here also felt very strongly in that way. That was one point I emphasized to Bud Harkavy when we set up the center. I said, "We are not going to be a demographic center; we're going to be a population study center." As I conceive population study, it is demography in its social and economic setting. So, no, we never had that idea.

When the department of demography was set up at Berkeley, I was offered a chair there. The year before, I was there on a sabbatical, in fact finishing the book on Taiwan, and Deborah was finishing her doctoral dissertation. Judith Blake, who was setting that up along with Kingsley, arranged for me to be offered a chair, be part of that program. I thought it over and on a number of grounds decided that I didn't want to do this. One thing that influenced me was the fact that it was going to be completely separate.

**VDT:** Draw from many different disciplines, right. You, of course, have your bias but do you think sociology is the best background for a demographer?

**FREEDMAN:** No, I won't say that. Yes . . . I like sociology. I think it's a broader field. We've trained a lot of economic demographers here, about a third of our graduates are economic demographers.

**VDT:** More and more. And you now have several on the staff.

**FREEDMAN:** We've had somebody on staff for a long time. Paul Demeny was our first economic demographer; I brought him here. When he left, I brought Ron Lee here; Ron began his academic career here. Then we had a series of other people. Eva Mueller was here during this whole period. And my good wife got her degree and joined the group as well.

I think sociology is a very good basis for this connection. And most demographic training in the United States has been in sociology departments. I think economic departments are not in general as hospitable. Insofar as they tolerate population people, they have to be people who will demonstrate first of all that they are *echte* [real] economists; otherwise they're suspicious of them. But I think that's a good base. I think there are many disciplines that have substantial contributions to make--for example, anthropology, very important, public health, other disciplines. But we can't be in all of these places.

There are some models of centers that involve all of those people. The North Carolina center is an example; they've done a very good job of that. It's a difficult thing to do. That hasn't been my view and I think the one we followed here was the one I was comfortable with. I decided I wanted a model I could work with. If I was going to lead this, it was going to be something I could work in. I wanted to continue to be a scholar; I wanted to continue to be identified with the discipline I was in. And I got a group of people who were comfortable in doing that.

Early on, we brought on Dudley and Beverly Duncan, who were for ten years very important elements of the center. They helped set up our apprenticeship program. They had the emphasis on sociology, as I did. We brought Ren Farley in. Dave Goldberg was with us at that time as well. We brought Paul Siegel in, various other people. We grew. And then, as quickly as we could, we started

population work in economics, and that has been with us for a long, long time.

**VDT:** You also had history: John Knodel. You mentioned that in your PAA paper, bringing in history.

**FREEDMAN:** Yes. I was an undergraduate major in history. Yes, demographic historical work; John helped bring that here. And we've had some other connections with Charles Tilly, who was here; he had a joint appointment in sociology and history. There are many disciplines that are relevant. The question is how these can be organized. And I'm a great believer in diversity. There's the Pennsylvania model, different from ours; North Carolina is another model. They all seemed to have worked very well.

**VDT:** Yours in particular worked very well. The Caldwells say in their book that Chicago staffed Michigan and Michigan staffed Wisconsin. Norman Ryder, of course, set up Wisconsin's population center and he said in his interview that Michigan graduates were the best. You pointed out in your PAA paper on the Michigan model that you stress the apprenticeship system, so your graduates "hit the ground running" and need little in-service training when they're launched.

**FREEDMAN:** The UN likes our people for that reason; we have a number of people there. Yes, all the leading demographic people at Wisconsin, with one or two exceptions, were our students. They were members of a very unusual cohort over a couple of years: Larry Bumpass, Jim Sweet, Robert Hauser, David Featherman, who has just become president of the Social Science Research Council, Doris Slesinger. These are all Michigan people who reassembled at Wisconsin. Quite a few years ago, I gave a couple of lectures at Wisconsin and at that time I counted 17 Michigan PhDs in the sociology department there.

Those people have gone on to make their own careers. They're very distinguished, very able. If they had gone someplace else, they--Bob Hauser, Larry Bumpass, people like that, had such ability that--well, the way I would put this is that with people like that we couldn't do them any serious harm. They would have made it anyway. But we had the facility and I think we had a significant influence in that way. That's what I was talking about when I talked about our former students.

**VDT:** This is not quite a fair question--you've mentioned many of them--but who have been some of your leading students? I think your list would go on and on.

**FREEDMAN:** When I knew you were coming, I wrote down the names of a few others I would mention. One of them is Krishnan Namboodiri, who is at Ohio State now with a fine chair, was chairman at North Carolina. Krishnan was another indication of outreach. Amos Hawley and I, at the request of the Population Council, set up a population center at the University of Kerala [India]. I told them I didn't want to do it until I had some people to send back. Krishnan was finishing his Ph.D., one of the best students we ever had. He went back along with a man named Pillai, who is still there, to organize that center. He stayed for five years; we backstopped him. It didn't work out very well, for reasons I won't go into now, not as a result of Krishnan's difficulties. We put a lot of emphasis on our Third World students going home and we felt we could do that because we were going to support them. Gerhard Lenski, who had been a colleague of mine at Michigan, was chairman at North Carolina and about five years after Krishnan was back in India, he called me and said, "I know how you feel about students going back to the Third World, but we need somebody in demography and Krishnan is my number one candidate." And I said, "Well, he's paid his dues; call him." He went to North Carolina and had a distinguished career and is still having a distinguished career. I was delighted to be able to select him, along with Jane Menken, as the two people for lead papers in a

session which I've organized for the IUSSP meeting in New Delhi [September 1989].

Anrudh Jain I consider one of our distinguished students; has had a distinguished career since he left here. I went to India with Anrudh when he graduated to make sure he would get a decent start there. I had to do a lot of table-pounding in many of these places to make sure these students were given an opportunity to do the work they were capable of doing. That was one of the things I thought was important.

Dr. T.H. Sun, who directed for many years the family planning institute in Taichung. He is now the vice-director of the research and evaluation division of the executive office of the president of Taiwan, very important position.

Another of our students who I think is doing extremely well--publishes all the time; a consultant--is John Casterline at Brown; one of the students I work with and have a lot of regard for. Jack Kantner, one of my first students. I've mentioned Napaporn Chavoyan. And then finally--I could go on for a long time--but I'll mention Jim Phillips, who's at the Pop Council now and who is the key figure in making the Matlab project [Bangladesh] so well known in family planning and demography. So those are students that . . . There's a long list of others.

**VDT:** And you mentioned right at the beginning of the interview that if you're remembered in history it will be through the ongoing influence of your students. Who have been some of the leading influences on your career? You've mentioned Hawley.

**FREEDMAN:** I mentioned Amos Hawley, Clark Tibbits, and Angus Campbell.

**VDT:** Remind me, Angus Campbell was . . .

**FREEDMAN:** Angus Campbell was director of the Survey Research Center. He not only helped conceive the Detroit Area Study, but when we started GAF, I went to him and said, "We're going to do this study." That was a very controversial thing I did when I did that study--now everybody takes all this for granted--the idea of interviewing people in a cross-sectional sample of the United States about the use of contraception and miscarriages and how many children they wanted, things like that. The newspapers did not deal with those issues in those days. And I was warned by a number of people that this was going to blow up: the Bishop of so-and-so and the Catholics would be against this; the fundamentalists would be against this, and so forth. So then I had a talk with Angus and he said, "Let's try it; let's do it." The whole budget for that study was \$75,000, from the Rockefeller Foundation. But because of what I knew might be the problems, we proceeded very carefully. First of all, I organized two sponsoring committees. One was a medical committee, distinguished ob/gyn's and heads of medical schools, who wrote a letter and went on our letterhead. Then I organized a list of distinguished lay people; I had Judge Learned Hand and other people on that.

**VDT:** And you say Angus Campbell was such an influence?

**FREEDMAN:** He was an influence because he backed the idea that the Survey Research Center would put its influence on the line. When the grant from the Rockefeller Foundation went through--at that time there weren't that many grants in a university like this--when it went through our Board of Regents, I got a call from the Secretary of the university saying the Board of Regents were concerned about what the backlash might be from a study like this. I spoke with a Catholic physician who was one of the regents and he said they didn't want to infringe on my academic liberty in any way, but they were just concerned about public reaction. I said, "I think it will work all right. We're going to do this very carefully, we're going to pretest it. But I can't guarantee there won't be any problem." He asked me if I'd be willing to speak with Monsigneur Somebody from the Archdiocese of Detroit and I said,

"Sure." I went to Detroit and showed the Monsigneur the schedule and he looked at it and said, "Well, there's no reason why any good Catholic can't answer your questions. You're asking questions of fact. But if you use these in a biased way, such as Mr. Kinsey has got in his book, we will feel free to attack you." And I said, "Of course."

Well, to indicate how careful we were, we did elaborate pretests. First of all, we had some problems in that the interviewing staff of the Survey Research Center indicated that they were very worried about asking these questions. So we picked a number of counties for a pretest. We went to an area of Brooklyn that was heavily Roman Catholic. We went to Jeff Davis County, Georgia, which was then the poorest county in the United States. We went to a rural county in Iowa and a rural Catholic county. We tried it out. And the interviewers who had this experience said, "This is easy." This is one of the important things about GAF that people don't know now, because they don't remember how difficult the subject was then. So one of the primary things that GAF showed was that you could do studies like this. We had the highest response rate that the Survey Research Center has ever had on a survey with a non-captive population.

Now, we had some backlash. In one area in the Southwest, two women in a neighborhood who had been interviewed talked to each other about the interview and said, "They shouldn't ask us these questions," so they went to the chief of police. At that time, the supervisor of the interviewers always registered with the local police department to tell them what we were going to do. And he called me and I told what we had done and he said, "Well, there's nothing illegal about asking people any of those questions. They don't have to answer, do they?" I said, "No." And it went very well.

So I would say it is not simply the substance of what we did, but this broke the idea that you couldn't do these things. You remember that the Indianapolis Fertility Study did not interview Catholics; they interviewed only white Protestants. That was because they were worried about what would happen.

Now, when I began to work in Taiwan--wherever I began to do these things, I would get the same thing, "Oh, you could do that in the United States but you can't do it here." And my response was always, "Well, let's try it out." And in general our experience was that this was a subject that was close to the hearts of the people that we were talking with. The interviewers in the end said--now everybody takes this for granted--that it was much easier to interview them about their family's fertility and family planning than it was to interview them about inflation, prices, things like that.

**VDT:** Right. Well, we were on the list of who had been influences on your career.

**FREEDMAN:** I've mentioned P.K. Whelpton--not so much from an intellectual point of view, although his cohort analysis had a great influence on me--as because he gave me the opportunity to do the Indianapolis study and without Pat I don't know whether I would ever have done the GAF study. So that was an important influence.

I've mentioned Barney Berelson. He had an influence on me in many, many ways. We worked together. We did the first systematic evaluation of population work in the World Bank. We did a number of research things together. We were constantly on the phone with each other, exchanging manuscripts, and we had a great deal of influence on each other.

I have to mention my wife, Deborah, with whom I've had a long professional interaction. She got into this field after our kids were in junior high school and we've collaborated. She reads most of what I write; I read most of what she has written. For a while, we were on separate tracks; we've collaborated in recent years, and that was important.

There's a long list of other people; I can't give you all of these. Irene Taeuber was somebody with whom I communicated a lot and I think she was a major influence on my views on what was possible in East Asia. There are some important, charismatic people in the Third World. I mention just one, Dr. S.C. Hsu, who was head of the rural health department in Taiwan, who really made all

that possible and was a great influence on my view of what could be done there. I could name other people like that. But these are a representative, good sample of leading influences.

**VDT:** Good. How do you get everything done? In the 1960s, for example, you've mentioned it was a five-ring circus. At that time, you produced "The Sociology of Human Fertility." You were setting up the Population Studies Center and the Taiwan program simultaneously. You were teaching. You edited Population--The Vital Revolution [1964], which was a series of Voice of America radio broadcasts. Did you set those up?

**FREEDMAN:** Yes.

**VDT:** You organized the 1967 Fertility and Family Planning Conference here on the Michigan campus and coedited the book that came of it. You were PAA's first vice-president in 1963-64 and had to plan the annual meeting in 1964. You were president in 1964-65; there was your address for the 1965 meeting. The presidency of PAA involved more work than you'd expected; in your 1979 interview, you mentioned the difficulties with Don Bogue and the startup of Demography. There was the Detroit Area Study. How did you get all that done?

**FREEDMAN:** I've often wondered. I get tired when I think about it. Well, when I was a college student and had to work my way through college--I had a 40-hour-a-week job while I was going to school--I learned to get up at five o'clock in the morning. And I've done that most of my life. I've reverted. I gave that up a while ago, but I find in my old age I don't need as much sleep. But I worked a lot. I worked long hours.

But I'd say that the main thing that made this possible was that I have always had very strong, good collaborators. I think I've had a knack for finding good people and turning things over to them. I ran the Detroit Area Study for just two or three years and then I turned it over to a series of my students and then it went on by itself. I've had no contact with it since then. I started the longitudinal study in Detroit and worked with it; I got Lolagene to work on it with me and she did a lot of work on that. I got Yuzuru Takeshita, Al Hermalin, Alden Speare, and other people to work with me on Taiwan and I gradually phased that work out. When I did some work on Indonesia, I had Sieu-Can Khoo and Boudon Supratilla to work with. I had Indian students working with me on the Hong Kong material.

More recently, I've been working in Mainland China--we haven't talked about that--and I have a young collaborator who is now the principal figure; I'm the junior author now for work we're doing on China. This is William Lavelly, who is a real China scholar. He has lived both in Taiwan and Mainland China, speaks and writes Chinese, is trained as a sociologist and a demographer. We've published jointly, but now he is the main person doing that work.

All of these things. I think that goes back to something I said earlier. I started something; I was an institution builder in a sense and got other people involved. When I brought the Duncans here, Beverly Duncan for many years did not want an appointment in the sociology department. She could have had it any time she wanted; she got it in the last few years of her ten-year stay here. But for about seven years, she was my righthand person in the center. The reason I could be abroad doing all these other things was that I knew that Beverly was handling administrative problems, in addition to doing her own research. So the idea has always been to get others involved.

I needed them for other reasons as well, namely, that I became obsolete from the statistical point of view. I would try periodically to keep up, but I could only keep up so far. When Bill Mason, who is our center director, came here and began teaching our statistics course, I took the course to upgrade myself.

The answer is my colleagues, partners, and coworkers.



**VDT:** You chose them.

**FREEDMAN:** Well, they chose me and I chose them. I was lucky. I also had some that didn't work out so well. And there were some things in which I didn't work out so well.

**VDT:** One thing I forgot to mention about those years was that you were vice-president and on the executive council of the IUSSP in 1965-73.

**FREEDMAN:** That didn't take much time.

**VDT:** And now you're going to the IUSSP meeting in Delhi.

**FREEDMAN:** No, I'm not going. I've organized a session but I'm not planning to go.

**VDT:** Do you feel the IUSSP is important?

**FREEDMAN:** Yes, it's an important organization. It is now a much bigger organization than it ever was before; having many seminars. As a matter of fact, on the 23rd of this month [June 1989], Deborah and I are going to Tunis. We have the leadoff paper in a conference that Jim Phillips and John Ross have organized for IUSSP on essentially the relationship between family planning programs and their effect on fertility. Another replay on that theme.

**VDT:** What is your current work on China--somewhat interrupted now because of the troubles there? [A planned trip to China had just been derailed by the bloody events in Tiananmen Square and other parts of China.]

**FREEDMAN:** When I think about things I've done in the last ten years or so, I would emphasize something that happened there. I went there in the early 1980s on a WHO mission with Ed Wright, chairman of the mission, and Srinivasan to advise the State Family Planning Commission about evaluation of their work. At that time, China was very closed; it was hard to get figures on anything.

**VDT:** This was before the 1982 census?

**FREEDMAN:** It was before the 1982 census and the 1982 one-in-a-thousand survey. The last week of that mission, I learned that they were going to do what is now the famous one-in-a-thousand fertility survey--one-in-a-thousand meaning a sample of a million. Mr. Jow, the person who was traveling with us, didn't tell me about this until the last ten days. I was about to go to Beijing to participate in a training workshop on evaluation for all the statisticians of the provincial family planning programs. I was doing the stuff on surveys. And I was appalled by the fact that he hadn't told me they were doing this big survey.

This was in Sichuan. He came at ten o'clock one night to tell me about this. I was outraged that he hadn't told me before, but I said, "Okay, tell me about it." And he told me that within a month they were going to do this big survey. I went over the plans with him and I told him I thought they were not ready. The sampling wasn't well designed; the questionnaire wasn't good; they could not in the period of time they were planning to do it train the survey people to do all of this. I told him they ought to postpone it for a year. He didn't know if they could do that. We stayed up all night.

We went on back to Beijing and I gave this seminar and got his permission to use this as a case sample. I indicated what I felt the problems were and told them if they conducted the survey as it was,

China would lose face because a survey this big was going to become internationally known. And in the report which we wrote to them and then gave to WHO, in my section we recommended that they wait a year. But before we wrote our report, the day after I gave this talk, we met with the Chinese leaders and they agreed to postpone it for a year.

I arranged for them to come to Michigan and to Princeton for consultation on the questionnaire and consultation with Leslie Kish on sampling. I arranged that Leslie would go to China--his first trip to China--to help them design the sampling. He gave a workshop on sampling and began to train people in sampling. After the survey, Ansley Coale did a publication for the National Academy in which he indicated that it was an excellent survey. My role in persuading the Chinese to postpone it may be the most important thing I've done in the last ten years. Some of the important things you do don't result in a publication.

Now Bill Lavelly and I have two publications based on our analysis of the survey ["Local Area Variations in Reproductive Behaviour in the People's Republic of China, 1973-1982," also with Xiao Zhenyu and Li Bohua, Population Studies, March 1988; and "Education and Fertility in Two Chinese Provinces: 1967-70 to 1979-82," also with Xiao Zhenyu and Li Bohua, Asia-Pacific Population Journal, March 1988]. We have a couple of other publications in progress on that work.

Last November, I was in China with Shelly Segal for the Rockefeller Foundation talking to the State Family Planning Commission about setting up some Taichung-type studies in which they would study the introduction of new contraceptives and analyzing the interface between the clients and the provision of services. I told them at an earlier session and again in the session with the Minister that if they were going to do these studies, they had to be prepared for bad news; that any systematic evaluation was going to find out the problems. Were they ready for that? I said, "There's no point in doing this if you're not ready to get at the problems." And both at an earlier session with a lower-ranking person and with the Minister, they agreed they were ready. They said, "We're meeting a lot of problems; we need to know the facts. Deng Xiaoping says, 'Truth from facts.'" That seems a little ironic in view of what's happened recently, but that was the case. Everybody was citing Deng Xiaoping: "Truth from facts."

The two-week trip that has just been postponed--the middle of it would have been the time when the bloody happenings in Tiananmen Square took place--was to follow up on that. I was going with Parker Mauldin and Joan Kaufman to carry this one step further. We had developed some suggestions for experimental design and were going to try to develop this. Temporarily that's been postponed till next October, but I think it's going to be much later. We'll see what happens.

**VDT:** That's great. Perhaps you can add at the end some other things that are not on the record that you want to put on record.

**FREEDMAN:** I think I've talked enough. Any of us who have been presidents of the PAA, after 35 or 40 years in the field there's a lot to talk about. All we can do in an oral history is to get the high points. I think you've gotten on the record some things that I think are the important turning points of my career; places where maybe there's a little signal that got on the network that might have some repercussions afterwards.

**VDT:** What do you consider your leading publications, and why? Things that are on the record.

**FREEDMAN:** I think those are the two books, the GAF book [Family Planning, Sterility and Population Growth, 1959] and the Taiwan book [Family Planning in Taiwan: An Experiment in Social Change, 1969], and the 1961 sociological piece, with the bibliography and essay, sponsored by UNESCO ["The Sociology of Human Fertility: A Trend Report and Bibliography," Current Sociology, Vol. X/XI, No. 2, 1961-62]. I think perhaps those have had the most influence and are the most widely

cited.

I guess also the joint piece with Barney Berelson on family planning programs ["The Record of Family Planning Programs," Studies in Family Planning, January 1976], which was rather crude at the time. A much more sophisticated, important work that followed was, first, Parker Mauldin's piece with Barney ["Conditions of Fertility Decline in Developing Countries, 1965-75," Studies in Family Planning, May 1978] and then the Mauldin-Lapham work [e.g., Robert J. Lapham and W. Parker Mauldin, "Contraceptive Prevalence: The Influence of Organized Family Planning Programs," Studies in Family Planning, May/June 1985]. In terms of influence on what happened later, those were more important.

You know, I think in the course of this discussion I've mentioned Parker Mauldin as much as anybody else and that would be certainly a person who had a lot of influence on my life. That goes way back, because Parker and I were graduate students together at the University of Chicago. That was a notable time, by the way, at the University of Chicago before World War II. Kingsley Davis was a postdoc when I was there with Parker.

**VDT:** I've interviewed Kingsley recently and he recalled that. You write so well. You lay out all your points so neatly--one, two, three, four--easy for reading and remembering. That organizing showed also in your funnel model. What explains that?

**FREEDMAN:** That goes back to my high school days as a debater, where one did things like that. And at an early point, I developed what I subsequently talked to students about when they couldn't organize their doctoral dissertations. That is, first tell people what it is you're going to say, then say it, and then give them a review of what you've said.

When I had the first draft of my doctoral dissertation, W.F. Ogburn at that time was giving a seminar. This was after World War II when I came back to do my doctoral dissertation in the spring of 1946; he had a big seminar. There were still a lot of students at Chicago and I remember there were about 75 students there. We had a seminar on dissertation writing and Ogburn had seen a draft of a chapter of my dissertation. He asked me if I would talk in the seminar about the way I did this. If you would look at my doctoral dissertation, you would see that it was all done this way. That is, I had a first chapter that said what I was going to say and each chapter began with an outline of what was to come, and I said it, and then I had a summary of what happened.

It doesn't make for elegant writing. It makes for clarity and it leads people to follow what you're saying. But it's not the kind of writing that literary people do.

**VDT:** Well, it's excellent. Now on PAA. You covered some of your PAA recollections in your 1979 interview with Andy Lunde: for example, on your first PAA meeting and the people you met at the early meetings.

**FREEDMAN** [from the 1979 interview]: I think I joined the PAA in either 1947 or 1948. Amos Hawley suggested that I join. I can't remember if I went to the meeting in the spring of 1946 or the spring of 1947. It had to be at least that early because I got involved in research and was reporting research to the Pop Association at a fairly early point.

At the time, the Association was very small but wonderful. I went to my first meeting and the great giants were there: Warren Thompson, P.K. Whelpton, Clyde Kiser, Lowell Reed, Frank Notestein. And the thing that was so marvelous was these were not only the intellectual giants in the field, but they talked to me and became my friends while I was an instructor.

The thing that thrilled me apart from the fact that the giants spoke to us young ones was that there was a continuity from one meeting to the next, that is, an argument that had been taken up at the 1948 meeting continued in 1949. And if Dorothy Thomas was speaking, I knew the assumptions she

was making. If I had any doubts on that score--and there were times in my teaching career when I had taken different research and teaching routes--they were erased by the enthusiasm. That Association was extremely important to me and I think all the other young people who were coming into the field at that time. There weren't very many people to talk to and those meetings were important for stimulation.

**VDT:** You obviously still enjoy PAA meetings; you come and you participate. You have a core group of people whom you know and see every year at PAA. Do you think PAA meetings are getting a little out of hand? We've just had a record turnout of close to 1,200 [1,193] at our Baltimore meeting [1989], 84 sessions, eight overlapping at a time, many spinoffs.

**FREEDMAN:** It's not as cozy as it was when there were three or four hundred of us, but I think that's inevitable. When I went to my first PAA meeting, I got on a first-name basis with all the leading figures by the time of the second meeting. I knew Warren Thompson, Frank Notestein, Frank Lorimer, Irene Taeuber, Dorothy Thomas, people like this, and that was very nice. Now those days are past; that can't be anymore. I still think that the PAA is much more manageable than the sociological or economics or statistical meetings. But it can't be as cozy as it was.

[On membership size, from the 1979 interview.] We're sometimes nostalgic for the days when we were small enough to meet at Princeton, but I think the growth has been good. I never have favored a special policy to bring in all of the people on what I regard as the margins of the field who have some sort of interest in this area. I think it is important that the organization continue to have a central core of activities and people who are concerned with the scientific study of population. Now, we have M.D.s, we've got public health people, geographers, and others. Many of them didn't get professional training in demography and are doing the work and that's fine. I'm against any kind of rigid exclusion policy. I think it's working pretty well in the sense that if you're not interested in the scientific aspect of population the meetings won't interest you. So I have no objection to somebody whose main interest is the family planning clinic in Fayetteville, Arkansas. If they want to join, fine, and if they become interested in the scientific aspect, fine; if not, I think they will fall away. We are growing, I think, at a reasonable rate.

One aspect of that question is better answered by newcomers, because those of us who've been in the organization for a long time don't find increasing membership a terrible burden because we've always known a core and each year we meet new people. Somebody who comes in this year, for example, will have a harder time, simply because there are more sessions, there are more people. I would have to say that they're at a disadvantage as compared with those of us who began when the organization was smaller. It's inevitable that we're larger, but I don't think it's out of hand.

[On Lowell Reed, PAA president in 1942-45; from the 1979 interview.] He was a courtly man. He was my picture of a Southern or an English gentleman; big, courtly man. He had a wonderful capacity for listening. I always felt he was genuinely interested in what I was doing. Sometimes I feel when people ask me what I'm doing, it's really an introduction so they can talk about what they're doing. Lowell was very good at bringing out young people. He also did a great deal in helping to organize activity in the Indianapolis Fertility Study, which I was involved in. Lowell was involved in a series of meetings of people who got involved in that study after World War II and I always found him very helpful in that respect. I had a different entree to him in another sense, because his son, Robert Reed, was a classmate of mine at the University of Chicago and I met Lowell through the son at an earlier point than I would otherwise. I always think of him as a man who was very important on the organizational side of the field. By the time I got to know him, he was an administrator; he was not doing research in population. But he was probably more impressive than anybody doing research in

facilitating the work of other people.

**VDT:** Your first paper at a PAA meeting, I believe, was in 1949. You had a paper on "Some Aspects of Research in Differential Fertility"--it must have been the Indianapolis study--but that was read by Wilbert Moore, in a session chaired by Clyde Kiser.

**FREEDMAN:** Yes, I couldn't go to that meeting.

**VDT:** Was that the only one that you ever missed? In 1979 you said that you'd probably missed only one meeting.

**FREEDMAN:** I was not present at the meeting when I got the Irene Taeuber Award in 1981. I was in China. Al Hermalin accepted for me and I wrote a few words and said that Irene Taeuber would understand that being in China was an excuse for not being present when I got the award. I think I missed a meeting with one of my heart episodes and the Minneapolis meeting in 1984; I think I was abroad for that one. But I've certainly been at the large majority of these meetings. I don't think I've missed very many.

**VDT:** And you still participate.

**FREEDMAN:** I think there are very few PAA meetings in which I haven't participated in one way or another on the program, giving a paper or being a discussant or running a roundtable or something. A story on that. When I had my first heart attack in 1981 and was recovering, Frank Notestein wrote me a note and said, "Ron, it's time to get off the center of the stage; the young actors are waiting eagerly in the wings." I've always remembered that.

**VDT:** Well, thank goodness you're not too much off the center of the stage--for China.

**FREEDMAN** [on the "big" issue of his PAA presidency, 1964-65; from the 1979 interview]: The big issue of my term of office was the foundation of Demography. It was a rough road. That involved Donald Bogue as first editor of Demography. In those days the budget of the Population Association was a very small number, maybe a couple of thousand dollars and here we were going to start a journal. It was clear that the costs for such a journal were much larger than the total funds we had and if we published for five years, we couldn't cover it. I don't know what would have happened if Don Bogue hadn't been as stubborn as he was. He laid out a set of rules of what should happen with Demography in order to be fiscally conservative. Don is a wonderful person, fine scholar, and he has a steel-like determination to do whatever he thinks is necessary to do. Every several weeks I had either a meeting or a telephone conversation with Don. I'd say, "Don, you're going beyond the guidelines. You're spending more money than we can possibly raise." And Don always said, "You're right; gotta change it." And he always went ahead and did exactly what he had done before.

One of the things that Don was doing was to print much bigger editions of Demography than we needed. And they were expensive. We felt we should print enough for the membership and a couple of hundred extra. Don was printing maybe a thousand or so extra on the argument that when this became established, the libraries and other institutions would want to buy back copies. But we didn't have the money to pay for them. In the end, we had a big deficit. Don told us that if we allowed him to take responsibility for selling those back issues, he'd cover the deficit. And he did. It was successful and Demography, of course, is well established and it's a fine journal. But if you talk to Paul Glick, he'll verify that that was a troubled period.

**LUNDE** [from the 1979 interview]: Not only that but something else happened. When we made a study of the membership of the Association, we found that it increased dramatically after the publication of Demography. There was a long period from 1931 when the membership remained small--gets up to about 300 or so, and even that we're not sure of. There were many lapsed members carried on year after year; nobody really knew how many members there were. After the publication of Demography, there was a jump within a year to about 875. [Membership was 660 in 1962, 802 in 1963, 1,142 in 1964. Demography began publication in 1964.]

**FREEDMAN**: Well, it was a good thing. I don't want to give the impression that I or any members of the Board at that time were against it; we were all for it. I suppose there had to be a whacky period there, but I think that it was an important event. It's obviously a well-respected international journal now. I'm interested in what you tell me about the membership, because I never followed up on that, but that was one of the arguments, that, first of all, Demography would call attention to PAA and, secondly, it would be a specific benefit to people who joined. I'm happy about the way that story turned out.

Between one thing and another, the presidency took quite a lot of time. I had thought the presidency would not be very time-consuming, because the year before as first vice-president I had arranged the meeting program. At that time we didn't have a president-elect and the first vice-president served as the program chairman. It was a big job and I had done that the year before and I thought I could rest on my laurels. But Demography, especially, turned out to be time-consuming, but interesting and a worry.

Sometimes I got angry, like people do, but the thing that made all of this all right was the people were good people. I'm very fond of the central core whom I've known all these years. They're all decent people. I found that, with a few exceptions, if they said they were going to do a job they did it. It's not just a cold professional matter; there are deep personal loyalties involved and I feel a warm affection for the people involved, including some with whom I've never been in agreement on professional issues.

**VDT**: What do you see as the outlook for demography and demographers in the U.S.? Is there still room for the basic research and involvement in Third World programs, as you've done? Or does the future lie with business demography and state and local government; is that where the jobs are?

**FREEDMAN**: I can't foresee a time in the next decades--we can't talk about what's going to happen a century from now--when demographers and population study will not be important. The reason people are getting into business demography is because it's becoming evident there, as it is with so many other fields, that there is nothing in the world of social and economic arrangements in which there is not a demographic element. People have to come back to us even if they don't want us. That is, they've got to come back if you're dealing with teenage pregnancy, the problems of the aged.

One of the things I've done in the last few years is to help our friends in Taiwan to answer the question: What do you do with the use for the family planning and the apparatus it has for surveys and for action work after you have saturation use of contraception and a TFR of 1.6? What they are doing is using their family planning workers and their survey apparatus, (a), to study chronic disease, and (b), Al Hermalin is going out there soon, because they are one of five countries in which they're working on problems of aging, because they all have significant problems of aging.

Let me go back to what I said earlier about AID and I feel this way about the Population Crisis Committee and others who feel we're always going off the edge of the cliff--we're about to go into catastrophe. That all hinges on the idea that there is a population problem. There is no such thing. There are a series of population problems that keep changing. And therefore there's a great future for people in the population field. In some respects, many places, that will involve fertility and family

planning. In others, it involves mortality, migration, problems in aging, the demography of AIDS, and all kinds of things. So on that score, I'm a great optimist.

There may be periods of time when funding is difficult and so forth. But so far as I can see, we're flourishing. Those people who are coming to the PAA meetings, who're overwhelming us and so forth, very few of them are unemployed. They're working.

**VDT:** Are you going to write your autobiography.

**FREEDMAN:** No.

**VDT:** What about your collected works? You're summing up Taiwan, in a sense.

**FREEDMAN:** I'm working on some of those. I did that piece in Studies in which there's quite a lot ["The Contribution of Social Science Research to Population Policy and Family Planning Program Effectiveness," Studies in Family Planning, March/April 1987]. Apart from everything else, I don't think I'm important enough to write an autobiography. I've written pieces and people will be able to draw on them if they like.

**VDT:** You've had a wonderful career and you must have enjoyed it.

**FREEDMAN:** Yes.

## **INTERVIEW CONTINUES**

**VDT:** Ron said a number of things during lunch that I'd like to capture. Right now he is talking about the fact that he is not going to the IUSSP meeting in September, although his name appears on the program as having organized a session. You explained that it costs quite a bit of money and that IUSSP meetings are . . .

**FREEDMAN:** Somewhat stuffy at present. And I've been to New Delhi many times and September in New Delhi can be very hot. Since my open-heart surgery, I find heat a little trying. Also, this is one of those several areas in which I've paid my dues. So, I'm not going to do that again.

**VDT:** You were saying more on what the Chinese work was and what's now going to be interrupted by this turmoil in China.

**FREEDMAN:** What the Rockefeller Foundation had been discussing with the State Family Planning Commission was the idea of doing some studies that would involve benchmark surveys and observations on the introduction of new contraceptives. What happens to the women who adopt the contraceptives, that is, what is their subsequent fertility and family history, what complaints do they have, what are the continuation rates, how do they perceive the services they receive, how is what they are doing affected by the government program. That's from the side of the women. And at the same time, there was going to be a study of the providers: how skilled are they, to what extent do they know about the side effects, to what extent are they providing a quality service.

We were going about three weeks ago to meet with our Chinese colleagues at the State Family Planning Commission to discuss study design, how to study these things. We called that off in view of the disorder and concern about other issues of more pressing immediate importance.

**VDT:** Did you say there was a Michigan graduate who was going to be put in charge of the program?

**FREEDMAN:** No, that was another program. The Luce Foundation has given the Michigan group in our sociology department, under the leadership of Martin White and Barbara Anderson, a grant for a three-year program in which our group, together with the sociology department of Beijing University, would cooperatively run a program modeled after our Detroit Area Study in the city of Baoding, in which graduate students from Beijing University would receive formal training in survey research methodology and then participate in interviewing and coding on two studies, to begin with, one with respect to the changing family and one with respect to aging patterns. We had been given some assurances that the Chinese side would be led by Dr. Wang Feng, who got his degree with us about 18 months ago. It now appears that he won't be going back for a while. In any case, this is not a propitious time for doing work of this sort, sending students out to interview peasants. The time will come. We'll have to wait. That will be another chapter to come.

**VDT:** It seems a pity. This seemed to culminate your work--the networking and influence in Asia. Let's hope it's only temporarily interrupted.

**FREEDMAN:** I haven't told you about all the problems we had. We had many, many problems. It's not unusual if you're working in Third World countries that you run into difficulties. I won't go into any detail now, but as we were about to go into the field after testing the protocols and questionnaires and so forth for the Taichung study, the whole thing was about to be called off. I went through a rather difficult month of negotiation before it got back on track. That's not unusual.

And when I told you about Malaysia, I didn't tell you about all the problems we had. There's a whole series of things. It's difficult to work in Third World countries. Afterwards you think about the pleasant part of it and what you accomplished and so forth. But . . .

**VDT:** Yet you persevered and got a tremendous amount done.

**FREEDMAN:** Oh, you have to be persistent.

**VDT:** You made an interesting remark about Nathan Keyfitz, who is now in Jakarta, Indonesia.

**FREEDMAN:** I simply said that I think Nathan has the capacity of taking almost any subject that he's involved with, demography or other aspects of social science, and putting a new light on things by looking at them from a different angle of vision than most people have. I shared that view most recently with my friend Leslie Kish, who had just been reading something of Nathan's, and other people, I think, have this same kind of view. He's a remarkable man.

I have to say also that a large number of the people you are interviewing, present company excepted, are very interesting people. We've had some really outstanding people in our population community, extraordinary people. Quite different. Dorothy Thomas is one kind and Irene Taeuber is another and Norm Ryder has got a different personality and a different set of interests. A lot of creative, dedicated, hardworking, interesting people.

**VDT:** Present company totally included!

**FREEDMAN:** It's been fascinating to have these encounters. And I think that the younger generation is having the same experience, with people like Ron Rindfuss, Larry Bumpass, and all these other young stars.

**VDT:** That's right. May I ask you frankly--I asked this recently of one of the other interviewees--who among the younger generation do you think might assume your mantle, your broad vision of what the



field of demography encompasses?

**FREEDMAN:** Oh, I think the names I've mentioned are all extraordinary people. Larry has a very broad view. I'm very impressed with Ron Rindfuss of North Carolina [who became PAA President in 1991].

In an intermediate stage, we've got a whole . . . I'm a member of the older generation, but there are people who are senior in the field who are still very young who are really quite extraordinary. I think right now there are three people who I think are very good in this respect. First of all, Sam Preston [PAA President in 1984], who always comes up with interesting work, is a modest fellow but does extraordinary things. Jane Menken [PAA President in 1985], a dear friend, who is very impressive.

**VDT:** I just heard that she was just elected to the National Academy of Sciences. Are you a member too?

**FREEDMAN:** Yes, I'm a member.

**VDT:** Who's the third one you're impressed with?

**FREEDMAN:** The third one I've forgotten, but there are others. Ron Lesthaeghe, a Belgian, and his wife Hilary Page are a terrific team in what I think of as a younger category.

**VDT:** I told Ron that often people in these interviews have come up with interesting afterthoughts and I've discovered several times that people, these demographers, have been musicians. And he said-- well, tell us what you do.

**FREEDMAN:** I said I'm not really a musician, but as a boy I learned how to chant at the synagogue; my father was in a choir. And each year on Rosh Hashanah, which is the Jewish New Year, I chant the evening service on the first night. The last time I did that was the 50th time I've done that in Ann Arbor. I began to do this when I was here as an undergraduate student.

Dr. Ronald Freedman, a preeminent international demographer, the U-M Roderick McKenzie Distinguished Professor Emeritus of Sociology, and founder of the Population Studies Center, died November 21, 2007 in Ann Arbor. He was 90.

Freedman helped to shape the field of demography in the late 1940s when he advocated bringing a broader sociological perspective to the study of fertility and family planning. One of the first demographers to use sample surveys in his research, he was also among the first to ask women direct questions about their childbearing intentions and preferences, and to recognize that preferences and intentions often differed from actual behavior.



During the 1950s, Freedman was instrumental in developing the Growth of American Families survey – the precursor to the current U.S. National Survey of Family Growth – data from which were key in developing population projections during a time of concern about a declining U.S. population.

In the early 1960s, Freedman shifted his interest to the developing world. His landmark experimental study of fertility in Taiwan greatly influenced family planning movements there and in other developing countries. The Taichung Experiment provided insights on both the diffusion and acceptance of information on family planning. Freedman subsequently worked with other Asian nations, including Hong Kong, Malaysia, Indonesia, Bangladesh, and the People's Republic of China, on fertility and family planning research and programs. He also made important contributions to the World Fertility Survey, the first centralized international survey research program and the largest social science research project in history.

As in the U.S., Freedman's work in Asia underlined the importance of viewing childbearing and contraception behavior within a broad sociological framework, influenced by the values of society and the social and economic conditions faced by each family.

In 1961, with support from the Ford Foundation, Freedman founded the Population Studies Center (PSC) at the University of Michigan. Under his leadership, PSC established an

apprenticeship system for training graduate students in demography and population study methods.

During Freedman's 50-year career, he was the recipient of many honors and awards. He was a Guggenheim Fellow, a Fulbright Fellow, President of the Population Association of America, a member of the National Academy of Sciences, winner of the Irene B. Taeuber Award from the Population Association of America and the Office of Population Research, and a Laureate of the International Union for the Scientific Study of Population (IUSSP).

"Ronald Freedman was one of the leading social scientists of the last half of the 20th century," said sociologist Arland Thornton, director of the Population Studies Center.

Born in Winnipeg, Canada, Freedman grew up in Waukegan, Illinois. He received a BA in history and economics from the U-M in 1939, and a master's degree in sociology from U-M in 1940. At the University of Chicago, he completed prelims for his PhD in sociology before joining the U.S. Army in 1942 to serve in the Air Corps Weather Service. Four decades later, he remarked, "When I talk about demography, I always apply what I learned as a weather forecaster – that is, don't look out the window when you're making a weather forecast. Short-run trends are not the significant thing."

In a 1989 interview, Freedman said: "I think that, overall, whatever I may have contributed to the field is not so much in the substantive research I have done. The field changes... I have been fortunate enough to be in a position to take some initiatives that I think will have a lasting impact in that they influenced what other people were doing and opened whole areas of work."

Ultimately, Freedman viewed his role in educating future generations as his most important contribution. "All of these other things pass away. What effect I'll have in the future, I think, will be through my students and their students."

The Ronald and Deborah Freedman Fund for International Population Activities has been established at the Population Studies Center with the intent of enriching demographic research on fertility and promoting ties between PSC and overseas scholars and institutions.

Freedman's wife Deborah and his daughter Jane Davidson predeceased him. He is survived by his loving companion Virginia Selin, his son Joseph, daughter-in-law Maria, and son-in-law Jeff Davidson, and by grandchildren Lily Davidson, Michael Davidson and Anna Freedman, as well as by his brother Chuck (Lila), and many nieces and nephews.

Services for Freedman were held November 25, 2008 at Beth Israel Congregation at 2000 Washtenaw in Ann Arbor. A memorial service in celebration of Ron Freedman was held Friday, May 2, 2008.



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## The Transition From High to Low Fertility: Challenge to Demographers

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## C U R R E N T I T E M S

THE TRANSITION FROM  
HIGH TO LOW FERTILITY:  
CHALLENGE TO  
DEMOGRAPHERS

Demographers have a great opportunity to study the demographic transition now beginning in some high fertility countries. In the next five to fifteen years fertility is likely

to decline sharply in many more countries, where the process of change can be studied and even accelerated under planned experimental conditions. The more quickly demographers gain new knowledge about the whole process of change, the greater the probability that they can make practical contributions to social and economic development. The opportunity is unique. None of the previous major fertility declines in the West or in Japan was studied in detail as it occurred, and much of their demographic and social history is forever lost.<sup>/1</sup> Moreover, previously there was not the opportunity to observe the effects of large-scale planned programs to bring family planning to masses of the population and, thus, to accelerate the decline of the birth rate.

The evidence that fertility is declining in at least some high fertility countries now is varied and partial, but convincing. I believe the present declines in a few countries are precursors of things to come in other high fertility areas, as favorable conditions develop. The decline is probably best documented for Taiwan where birth rates have fallen 17 per cent from 42 in 1958 to 35 in 1964.<sup>/2</sup> In Hong Kong, birth rates fell below 30 for the first time in 1964 from 32 in 1963 and 37 in 1958.

We cannot document a fertility decline in Korea directly from the vital statistics, which are sadly deficient. Nevertheless, on the basis of evidence about family limitation practices, it is highly probable that the birth rate has begun to decline there. In Seoul, which has about ten per cent of South Korea's population, it is likely that more than 30 per cent of all pregnancies were intentionally aborted in 1963, an increase from perhaps 16 per cent 10 years earlier.<sup>/3</sup> Among women aged 35-39 the induced abortion rate was probably 58 per 100 pregnancies in 1959-64, as compared with 14 in the preceding five-year period. While induced abortion rates probably are not so high in other parts of Korea, rates in two rural districts indicate that about 20 per cent of all pregnancies were ended by induced abortion in the period 1962-64 as compared with about 5 per cent in 1960-61.<sup>/4</sup> In South Korea as a whole the proportion who have begun recently to limit family size by contraception, sterilization, and abortion is, probably, enough to have started a fertility decline, although the size of the decline is still unknown.<sup>/5</sup> In Singapore birth rates have fallen from 44 in 1956 to 32 in 1964. A recorded decline in Puerto Rican birth rates from 41 in 1945-49 to 30 in 1964 is difficult to interpret because of the effects of migration, but it is likely that a genuine fertility decline has occurred there, too.

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Editor's Note.--This is the text of the address delivered by Ronald Freedman, Population Studies Center, University of Michigan, President of the Population Association of America, at the banquet on the evening of April 23, 1965, at Chicago, as part of the annual meeting of the Association.

For several of the areas in which the fertility decline has begun already I venture to predict acceleration of the decline within the next five years to levels of 20-25 births per thousand.

### The Conditions for Fertility Decline

Before discussing the specific basis for these audacious predictions, I want to review the broad assumptions on which they are based, not so much because I am certain of their truth, as because testing the assumptions themselves is an important part of the needed research agenda.

Let me say in summary that I expect fertility to decline first and most rapidly under the following conditions:<sup>6</sup> (1) where significant social development has already occurred; (2) where mortality has been relatively low for some time; (3) where there is evidence that many people, wanting moderate-sized families, are beginning to try to limit family size; (4) where there are effective social networks transcending local communities through which family-planning ideas and services and other modernizing influences can be disseminated; (5) where there are large-scale, effective organized efforts to disseminate family-planning ideas and information; (6) where such new contraceptives as the intrauterine devices or contraceptive pills are effectively available.

I do not assert that all of these are necessary preconditions for any fertility decline. I do not know what the necessary mix is for the beginning or for accelerations of fertility decline. That is precisely one of the important general questions on which we need research. Obviously, the first four conditions are relevant both to the past and to current situations. The last two conditions, organized programs and the new contraceptives, introduce new elements for which history provides no specific guide-lines. I want to comment briefly on each of these six conditions that I assume are favorable for fertility decline.

First, fertility is likely to decline most rapidly where there has been a significant beginning of the kind of social development that decreases dependence on local kinship and community ties. As long as getting the things worth having in a society depends mainly on local kinship-based institutions, rapid fertility decline is unlikely. More generally, declining fertility has been associated in the past with various types of modernization or development. The important questions are: why should these changes affect fertility? how much change in which development variables is necessary for one or another rate of fertility decline?

The United Nations has recently completed an impressive comparative review<sup>7</sup> of how current fertility rates are related to various development measures. The report indicates that almost all countries with adequate data have either high to very high fertility or quite low fertility. There are essentially no cases in the middle. The U.N. report also finds that a variety of measures of development sharply distinguish the high from the low fertility countries, but that there is practically no correlation between these development measures and fertility within each of these two groups of countries. The United Nations publication offers the interpretation that, under historically observed conditions, a combination of development measures must pass a minimum threshold to make

low fertility possible. I find this plausible and would expect the threshold to be that combination of development changes that will produce both low mortality and a sufficient erosion of the primacy of kinship to make important aspirations attainable in ways for which high fertility is irrelevant or burdensome. The need for research to replace this speculation with data is obvious.

My second assumption specifies that mortality decline may be an important precondition for fertility decline. Since in all societies some minimum number of children is regarded as essential for married couples, it is unlikely that fertility will fall much until parents are reasonably confident that this minimum number will survive and that excess children are unnecessary. The survival of almost all children imposes increasing pressure on traditional housing, familial, and other arrangements developed over a long time-period as adaptations to high child mortality.

Clearly, I disagree with the final conclusions of those who lament that if mortality and fertility declines do not occur simultaneously there is a serious population problem. Of course, there is. This is precisely one of the "population problems" which induces, and is solved by, the fertility decline. Probably, it is desirable from a policy point of view to begin family-planning programs early in the development process. Nevertheless, while I think the lag between mortality and fertility decline can be reduced substantially by appropriate action, I doubt that such programs will become effective until married couples feel reasonably certain that the number of living children they want will survive, if born, and until the parents observe about them and experience, themselves, the problems of excess fertility, when mortality is low. Certainly I may be wrong in this view, and the stakes are high.<sup>/8</sup> Why not run experimental tests trying to use the best family-planning programs in field organizations under varying mortality conditions that exist now? This will serve both science and social policy.

The third condition I specify as a sign of incipient fertility decline is that married couples should want only a moderate number of children and that an increasing minority of them should have made some efforts to limit family size, if only crudely and ineffectively. In sample surveys in many high fertility countries women have stated preferences for only moderate numbers of children.<sup>/9</sup> Such statements of fertility desires are at best approximate, and may or may not discount the effects of child mortality. We know that in many places where such statements are made the opportunity to obtain contraceptives does not evoke any significant response. However, if the populations making such verbal responses also have begun to make more use of abortion, contraception, or other means to limit family size, we may take this as an indication of the existence of a family-size problem great enough to move some groups of people to seek a solution. Such groups should be especially relevant for family-planning programs.

The fourth assumption I make is that the idea and the means of family planning will spread most rapidly in a society in which there are effective social networks transcending the local community for interaction and communication. Such networks were crucial in the decline of fertility in the West and in Japan, even without organized family-planning programs. It is likely that information and social validation for

family-limitation practices developed over time through interactions in informal networks of friends, relatives, and neighbors, linked in turn to sources of information and supplies by the mass media and commercial networks, and rather later by the private health network.

This again is, at best, a plausible speculation. We need research now in the developing societies on just how information, social support, and validation for new family-limitation ideas and practices are disseminated and how they affect particular components of fertility.

My fifth assumption is that when the other conditions are favorable, a well organized program to bring family planning to a population will accelerate fertility decline by speeding up the diffusion of family-planning practices. Such programs do appear to be accelerating this process, at least in Taiwan, Hong Kong, and Korea.<sup>/10</sup> I believe that they do so by providing better information to circulate in the widening social networks, by rationalizing service and supplies, and by providing important social legitimation for the new ideas.

There is a vast area for important research on whether and how various kinds of family-planning programs affect family-limitation norms and practices and, through them, fertility. Time permits only one example. In the fertility declines of the West and Japan the most modernized strata of the population adopted family limitation practices first, thus producing the well known differential fertility patterns. But what will happen if there is a well organized large-scale family-planning program, absent in these earlier situations? In Taiwan, and probably in Korea, for example, the situation before any organized program was like that of the West earlier.<sup>/11</sup> Fertility had begun to decline; it was lowest and family-planning practices were most common in the modernized sectors of the population. With large-scale programs, however, a surprising number of those in the less modernized strata have adopted family planning—such groups as illiterates, farmers, members of joint families, etc.

My interpretation of this situation is that during the early stages of the transition the problem which family planning solves is rather widespread in all parts of the population, but it is those in the modern advanced strata who are able to define their problem in relation to available solutions, to seek out the solution, and to use it with minimal assistance or support. Those in the less modern strata need a large-scale, well organized program to accelerate their adoption of the solution, because they need help in defining their problem, in knowing that a solution is available, and in receiving social reassurance that this new activity is safe and socially acceptable. This is, of course, interpretative speculation. Comparative research is needed on whether the facts observed in Taiwan and Korea are general and what they mean.

My final assumption is that the availability of such modern contraceptives as the intrauterine devices or the contraceptive pills will accelerate the adoption of family planning and the fertility decline. Until recently I was among those who believed that improvement of contraceptive technology is irrelevant for fertility decline, since the major declines in countries like England and France occurred mainly through the use of coitus interruptus. It now seems to me that when minimal favorable conditions exist, fertility decline will be accelerated, if there are



available contraceptives which are inexpensive, effective, not connected with the sexual act, trouble-free, and requiring little forethought or care, once use is initiated. At any given level of motivation such contraceptives should increase the probability that those under cross-pressures between old and new values will adopt the new values and persist in them. I still believe, however, that type of method is largely irrelevant until there have been certain minimal changes in mortality and social organization, but that view, too, needs testing.

Observations in at least three countries make it appear that the introduction of the intrauterine devices in large scale programs does accelerate acceptance greatly. Information about these new methods appears to be particularly suitable for diffusion by word of mouth or mass media in informal networks.<sup>/12</sup> Yet, I must admit that we don't know whether under the same social conditions the same large-scale effort could be effective with traditional contraceptive methods. Probably, there will be opportunities for experimental tests.

### Prospects for Fertility Declines

I return now to my predictions of large fertility declines soon, at least in Taiwan, Hong Kong, and Korea. I base my predictions on the fact that in these places the six favorable conditions I have specified are present in reasonable measure.

In Taiwan, which I know best, there is low mortality and considerable social development of the type relevant to fertility decline. While fertility decline is general throughout the island, it is significant that fertility does vary systematically in relation to social and economic development and inversely to mortality in the 362 local areas of the island.<sup>/13</sup> The majority of the married couples want three or four children, and most have that number by the time the wife is 30. Given rising aspirations for self and children, the result is increasing action to limit family size after age 30. At ages 35-39 fertility declined by 30 per cent between 1958 and 1963. This fertility decline occurred before the beginning of large-scale organized programs, now underway. It resulted from the efforts of individual couples to do something in various ways about family size after having the desired number of children. People learned about what to do from friends, neighbors, and relatives, from the drug stores and private doctors, and to some extent from the mass media. Significant minorities used induced abortion or sterilization.

All of these conditions would have led me to expect a significant decline of fertility in Taiwan even without an organized program, but a decline likely to be rather slow. My prediction of a rapid decline is based on the expectation that the organized program recently begun will greatly accelerate the decline already underway. We know that a major experimental effort in one city, Taichung, in 1963 was followed in 1964 by a 6 per cent decline in its birth rate as compared with an average decline of less than 3 per cent in the four other large cities.<sup>/14</sup> In 1964 the large-scale organized program was extended to many other parts of the island. In that year 50,000 intrauterine devices were inserted as part of this planned effort, although the program was not yet fully staffed, was unable to use the mass media, and did not yet operate in some of the major

population centers. The program goals for 1965 are for 100,000 IUD insertions and the five-year goal is for 600,000 insertions, estimated to reduce the birth rate to about 24 by 1970. On the basis of the favorable conditions and the success to date, I have little doubt that the 1965 goals will be met. The five-year goal of 600,000 insertions may be high, but its attainment is not out of the question. A report for March 1965 indicates that more than 10,000 IUD insertions were made, although the average monthly goal is only about 8,500.

In South Korea although some aspects of the situation are not so favorable, there is high literacy and use of the mass media, a market orientation, and mobility. Here, too, there is considerable evidence that desired family size is moderate and that recent individual efforts to achieve this goal have been considerable. The organized effort here, more recent than in Taiwan, is strongly supported by the government and is systematically utilizing the national mass media in a substantial way for the first time in the history of any country./15

The opening months of the large scale program last year were very successful. More than 100,000 intrauterine devices were inserted under the official program, almost entirely in the last six months of 1964. An experimental program in one part of Seoul is demonstrating that more intensive efforts can be even more successful, especially with the use of the mass media./16

While the specific conditions in Hong Kong are somewhat different, especially in the fact that the organized program is under a voluntary agency, the basic conditions making for fertility decline are similar to those previously cited, in my judgment.

In at least these three places, Hong Kong, South Korea, and Taiwan, a demographic event of great importance is occurring now—a fertility decline of major proportions in a short time, accelerated by organized social effort. The great challenge to demographers is to study the conditions and processes of the decline with all the tools of modern social science.

So far, I have purposely used as examples a few small countries in which the conditions are especially favorable for fertility decline. What about the rest of the high fertility areas where most of the world's population lives? Here, the relevant conditions are much less favorable. Nevertheless, I venture to predict that a majority of the world's population will be living in countries with declining fertility within the next five to ten years. I do not mean to say that the declines will occur without organized and vigorous efforts, and I do not predict that the "population problem" will be solved, but I am predicting that the fundamental social conditions will bring forth the necessary effort to initiate and accelerate fertility declines. Where the declines will occur, and how rapidly, will depend on many factors, only some of which we can specify now. One of our urgent research tasks is to undertake to keep track of the relevant social, economic, and demographic variables in as many countries as possible, so as to make possible both regularly revised predictions and sound ex-post-facto studies. With a humble acknowledgment of inadequacy for the task, I want now to canvass the other major-high fertility areas in a quick, presumptuous, global survey.

In Africa as a whole the prospects for fertility decline seem small. Apart from the theoretical availability of modern contraceptives, none of the favorable conditions I have specified seems present for any substantial parts of the indigenous populations.

In Latin America the conditions do seem favorable for major fertility declines in some places, so that we may expect at least moderate declines for the area as a whole. Significant parts of the Latin American population have relatively low mortality and considerable social and economic development. Recent surveys indicate strong preferences for small families in parts of the population.<sup>/17</sup> Evidence of high illegal induced abortion rates in some places<sup>/18</sup> indicates that many couples are looking for solutions to pressing family problems. In many areas the necessary health and other social networks exist. It seems likely that access to these networks by organized programs and a more permissive attitude may be developing with the deepening interpretation of the Catholic position now underway. After all, even without such a reinterpretation low birth rates were achieved in such Catholic countries as Argentina and Italy.

In Asia, where most of the world's population lives, the situation is complex. Japan has already gone through the fertility decline, and it is underway at least in Hong Kong, Korea, Taiwan, and Singapore. Even my audacity this evening does not extend to systematic, country-by-country predictions for Asia. Almost the whole range of possibilities can be found in Asia, because the necessary conditions are so variable between and within countries. However, I do believe that in three of the major populations—India, Pakistan, and mainland China—conditions are sufficiently favorable to make fertility decline likely in the next ten years, probably not as quickly or as uniformly as in Taiwan or South Korea, but of vast significance nevertheless.

In India and Pakistan significant mortality declines already underway are likely to continue. In parts of these populations and in particular places there has been significant social and economic development and a vast expansion of the mass media influence and of links to the wider world. The desire for families of moderate size appears in both urban and rural sample surveys.<sup>/19</sup> However, evidence that people are doing something to limit family size is found mainly in urban areas. The necessary social networks to transcend local and familial dependence exist in many places, but many other places remain isolated. In both countries official action recently has made intrauterine devices available for use on a mass scale. In short, my hurried estimate is that there are significant parts of the Indian and Pakistani populations in which the pertinent conditions are favorable but others in which they are not. Therefore, I expect fairly rapid fertility decline in some parts of these populations, which on balance will produce only moderate declines in the national rates in the near future.

If I were asked for advice about the Indian family-planning program I would recommend concentrating first the limited personnel resources available on those tens of millions of couples who are in the areas and strata with the favorable conditions. However, at the same time, I would select for the most careful immediate study a number of less promising areas varying in respect to the conditions I have specified. In these

areas I would utilize the best workers and services possible to try to discover the threshold levels at which unfavorable conditions can be overcome by efficient organization, services, and supplies. Many will disagree with my diagnosis of this particular situation. Some feel that such populations always have been ready for family planning, so that it is only necessary to bring them information and supplies in effective ways. Others agree that at least minimal changes in basic conditions are necessary, but argue that these have already taken place all over countries like India and Pakistan./20

Both for science and social policy, these positions should be tested by experimental studies now. We need to know particularly whether changes in how people perceive themselves and their world are a sufficient basis for fertility decline prior to substantial changes in their objective social and economic situation. For example, suppose the members of a peasant population are linked to the wider world through the mass media or education. Suppose that they begin to identify with institutions and ideas and social roles outside of the local, the familial, and traditional. Is this a basis for fertility decline before the institutions in which they actually live are changed? I don't think so, but this certainly needs study. We need to know, too, whether such changes in self-perception and identification do in fact precede other objective changes for any substantial number.

Speculation about mainland China is perilous, foolhardy, and necessary. After all we are talking about twenty to twenty-five per cent of the world's population. It would be foolish to allow disagreements with communist ideology and political practice to prevent us from making the best assessment possible of the demographic trends and possibilities. Admittedly, even the China experts must speculate about important trends, and I am not even that kind of expert on China. Nevertheless, I think we can say something meaningful about the prospects for fertility decline. First of all, there is evidence that the communists have created a considerable medical and public-health network which probably has reduced mortality to levels as low as those of India and Pakistan, and perhaps lower./21 This health network, while staffed with many people with insufficient training by best Western standards, is, probably, adequate as a potential means for bringing family planning to the masses. How much economic and social development of various kinds has taken place is very debatable. However, there seems to be little doubt that literacy has been increased considerably and that there is a considerable capability for reaching masses of people with communications. There have been many changes which probably are eroding further the strength of the extended family, whose earlier universality is in serious question anyway. Recently, we have had some evidence that there is not only considerable government interest in family limitation but that there is already significant provision for contraception and induced abortion, at least in the cities./22 While there is no evidence that such newer contraceptives as the intrauterine devices are being used extensively, such devices are easily copied, and the Chinese certainly can make them in quantity and distribute them to millions if they wish to do so. I expect that they will and that fertility will decline.

I want to emphasize that I am not predicting that major fertility declines are inevitable in the large populations of Latin America and Asia.

I have been predicting only moderate declines in the next 10-15 years for these populations as a result of large declines in some parts of them balanced by much more modest declines in the larger rural populations. How large the declines will be and when they will occur will depend to a considerable extent on the size and effectiveness of the organized effort to bring family planning to the masses and to legitimate it in the society as a whole. But, I doubt that any organized effort concentrating on fertility alone can produce large fertility declines where the other necessary conditions are missing./23

### The Tasks for Demographers

One of the perils and glories of demographic work is that our projections can be checked against the facts. I'm not sure that the quick surveys I have made should be dignified by the terms "prediction" or "projection." I certainly expect that I will be proved wrong in many particulars. This would be true of even the most careful and painstaking projections at present. Nevertheless I issue a call for making such projections much more carefully, so that we can learn from careful diagnosis of both errors and successes about the basic forces at work.

Whether I am right or wrong about particular places, it seems safe to say that fertility changes will be occurring at various rates and under varying conditions in different countries in the next five to twenty-five years. Probably, we can learn most about the social and demographic processes in fertility change if we observe them under such varying conditions, before, during, and after the major changes.

The tools of modern science and demography and the computer make it possible for us to study the coming fertility declines in ways that were never possible before. Despite ingenious and energetic efforts by those working with historical data we know very little about the fertility decline of the West and Japan. What we know does not permit us to link together directly the major variables at work. For example, in his brilliant address two years ago, Kingsley Davis discussed the importance of abortion in the fertility declines of the West and Japan./24 Unfortunately, he had to rely on fragmentary evidence about the incidence of abortion. He could not link precisely even the evidence available to specific social strata and to specific effects on fertility. We can collect such detailed and systematic data today.

Demographers generally believe that the fertility declines in England and France resulted from the adoption of contraceptive practices. But what is the evidence? For England, we have a sample survey based on a rather unsatisfactory hospital sample of women reporting retrospectively for periods as long as 50 years./25 Our French colleagues have ingeniously used diaries, letters, and other historical documents to buttress the plausible argument that the fertility decline began early in France and resulted mainly from the use of coitus interruptus./26 For neither England or France is the prevalence and effectiveness of the practices well documented nor are there data on the links to the rest of the social-demographic structure. Even for the most recent dramatic fertility decline in Japan we do not have much detailed knowledge of the change process.

What I am suggesting is that we plan boldly to study the whole array of pertinent social and demographic variables so that we can study their variations over time and link them into chains of influence for individuals, groups, regions, and countries. The programs of research which I am suggesting must be viewed in their broadest context. As I see it, we need comparative research on a time-series basis on four broad groups of variables.

1. As our end point we need data on fertility itself to include, as far as possible, both period and cohort measures of the number and timing of births in major population strata.
2. We need measures of what Kingsley and Judith Davis/27 call the "intermediate variables"—those through which any social variables must work to affect fertility—for example, age at marriage or first sexual union, periods of separation, contraception, fecundity, voluntary or involuntary fetal deaths, etc. We need to have measures of these for significant strata of the population and to know how each affects the number and spacing of children.
3. We need to know what the social norms are about these intermediate variables as well as about family size and child spacing. We also need to know how these norms about what ought to be done influence the intermediate variables and through them fertility itself.
4. Finally, we need to know which elements of social and economic organization affect these norms, these intermediate variables, and finally fertility itself. A new crucial element in the set of causal social variables is the operation of the new programs designed specifically to modify family size.

Presumably, lines of causation run from the social and economic organization to the intermediate variables either directly or through the social norms and then from the intermediate variables to the fertility variables themselves. Of course, biological variability in fecundity also enters into the final result.

This total system of variables is complex. No single investigator or institution can study the whole. Even collectively we may not fill in the outline of the whole structure in our life time. Others may conceptualize the basic framework in radically different ways. But, I hope that many of us, individually or in working groups, will try to do our research in such a way as to gather cumulative and comparative data on whole clusters of these variables in relation to some view of the whole process. It is a large task, but not impossible, if we think and work boldly and if we can bring in a larger number of young first-rate workers who see the task as an exciting scientific challenge.

Fortunately, the task is made simpler by the opportunity for large-scale experimental studies in connection with family-planning programs. Such experimentation is both feasible and ethical in a situation in which the mass of the population wants help in limiting family size and the indigenous leadership sees this as a desirable goal. The possibilities of disentangling cause and effect relationships in experiments conducted under almost classical conditions is a great opportunity for imaginative researchers. Unfortunately, many of the action programs underway do not take advantage of these unique opportunities.

Social demographers who are willing to assist in the practical evaluation of these family-planning action programs have a unique opportunity

to collect simultaneously demographic and social data in detail for the periods preceding, during, and following the action programs. The demographer who helps to establish ways of measuring vital rates where registration is imperfect, helps to evaluate an existing program, while developing the data needed for basic demographic analysis.

Sometimes, demographers look at those working on the action side of family-planning programs as far away from basic demographic concerns, more emotional than scientific, more concerned with operational details than general knowledge. On the other side, the family-planning activists sometimes see the demographers as unable to contribute to the solution of immediate problems, diverting energy and resources from these problems. On balance, both views are incorrect, in my opinion. The demographer must recognize that the family-planning action programs on the scale now under way are going to be a significant element in the set of social forces determining fertility and other demographic trends. They are one of the facts of life today. They are properly a subject of research and provide unique opportunities for basic demographic studies. On the other hand, the family-planning "activists" will be short-sighted if they do not encourage basic demographic research as part of the evaluation of specific programs. The demographers who can be attracted to this work are uniquely equipped to assist in the evaluation studies. Figures of distribution of supplies or intrauterine device insertions are important, but they are, after all, only measures of some intervening variables. What is wanted ultimately is data on how these affect birth rates and family-building patterns. Then, too, what we learn today in basic research activities may be useful in guiding programs later in Latin America or in Africa.

There is also a responsibility to the scientific community and to posterity to learn everything we can about the important demographic changes now underway. We cannot always precisely specify how what we learn can be applied now, but the growth of the central body of scientific knowledge is of value for its own sake, and its useful applications are difficult to foresee in advance.

Finally, none of us should be under the illusion that if all countries achieve low fertility, practical "population problems" will be solved forever and there will be no scientific problems about human fertility. The problems will be rather different, to be sure, but I cannot imagine a society in which the relations of man to man and to his environment do not have demographic aspects involving the patterns of reproduction. After all, not too long ago both demographers and activists were concerned with the problems of declining and aging populations. What we learn in this generation about the transition from high to low fertility will help the next generation to work on the new demographic problems—whatever those may be.

#### FOOTNOTES

- 1/ Of course, a great deal of important work has been done on Japanese demography. These include the work on family planning by Dr. Y. Koya, many important publications of the Institute of Population Problems of the Ministry of Health and Welfare in Japan, the public opinion surveys of the Mainichi Press, and Irene Taeuber's

monumental The Population of Japan. However, we do not have detailed studies on many aspects of the Japanese fertility decline. For example, we do not know for either the past or present the proportion of the child-bearing population who have had abortions in different social strata, at what stage of married life, and with what specific effects.

- 2/ Data on vital rates for Taiwan are from the Taiwan Provincial Department of Civil Affairs. All other vital data cited are from the United Nations Demographic Yearbooks or official publications of the countries concerned.
- 3/ The data on abortion in Seoul are from an unpublished study by Dr. Sung-Bong Hong of Sudo University, sponsored by the Population Council. They are from one district of Seoul, but it is unlikely to differ greatly from Seoul as a whole.
- 4/ Data from the Koyang study being conducted by Dr. Jae Mo Yang and Dr. Sook Bang of Yonsei University with support from the Population Council. The rates for the earlier period may represent more retrospective "forgetting," but there is little reason to doubt a major increase.
- 5/ See T. I. Kim, F. H. Choe, K. S. Lee, D. R. Koh, The Early Stage of Family Planning in Korea, Seoul, 1964.
- 6/ Some of the ideas presented briefly in this address are developed more fully in "The Sociology of Human Fertility," Current Sociology 10/11(2):35-38. 1961-62; and in "Norms for Family Size in Underdeveloped Areas." Proceedings of the Royal Society, B, 159(974): 220-245. March 17, 1964.
- 7/ Population Bulletin of the United Nations, No. 7 - 1963 (St/SOA/ Series N/7). Sales No: 64. XIII. 2.
- 8/ After this address was delivered Ansley Coale reminded me that fertility apparently began to decline in a number of European countries prior to or simultaneously with the mortality decline. It is probably unnecessary for a mortality decline to precede a fertility decline if some of the other necessary conditions change; e.g. changes in the economic situation or social organization may reduce the value of marriage or of a certain number of children, without any change in mortality. Then, the desired number of children might be attained with lower fertility and without a reduction in mortality.
- 9/ For a review of some of these findings see Parker Mauldin, "Application of Survey Techniques to Fertility Studies" to be published in a forthcoming volume of the papers presented at a symposium on Research Issues on Public Health and Population Change, University of Pittsburgh, June, 1964.
- 10/ For a summary view of the programs in Taiwan and Korea, see S. M. Keeney. "Korea and Taiwan: Two National Programs." Studies in Family Planning, No. 6, March 1965, pp. 1-6.
- 11/ For Korea, see Kim et. al. op.cit. For Taiwan see R. Freedman, J. Takeshita, and T. H. Sun. "Fertility and Family Planning in Taiwan: A Case Study of the Demographic Transition." American Journal of Sociology 70(1):16-27. July 1964; and B. Berelson and R. Freedman. "A Study in Fertility Control." Scientific American 210(5):29-37. May 1964.
- 12/ In experiments in both Korea and Taiwan a majority of women coming to clinics for intrauterine devices have heard about them through mass media or through informal communications, although in each



- country there were substantial programs for direct communication by health workers.
- 13/ From an unpublished M. A. paper in economics at the University of Michigan by Paul K. Liu.
  - 14/ These data and plans are from unpublished reports of the Taiwan Population Studies Center of the Provincial Health Department of Taiwan and the University of Michigan Population Studies Center. For a partial report see pp. 430-435, this issue.
  - 15/ The data and plans for Korea are from unpublished reports by the Health Ministry of Korea, and from conversations with responsible Korean officials.
  - 16/ From unpublished reports of the Sun Dong Gu Action Research Project of the school of Public Health of Seoul National University, directed by Dr. E. Hyock Kwon.
  - 17/ For examples, see Carmen Miró and Ferdinand Rath. "Preliminary Findings of Comparative Fertility Surveys in Three Latin American Countries." Paper No. 2 of the 1965 Milbank Memorial Fund Conference. To be published.
  - 18/ For example, see Rolando Armijo and Tegualda Monreal. "The Problem of Induced Abortion in Chile." Paper No. 11 of the 1965 Milbank Memorial Fund Conference.
  - 19/ For examples, see Mauldin, *op. cit.*, and S. N. Agarwala. "A Family Planning Survey in Four Delhi Villages." Population Studies 15(2): 110-120. Nov. 1961.
  - 20/ For example, see Donald Bogue's 1964 Presidential Address to the Population Association of America, "The Demographic Breakthrough: From Projection to Control." Population Index 30(4):449-453. Oct. 1964.
  - 21/ E.g., Lee Robert Worth. "Health Trends in China Since the Great Leap Forward." American Journal of Hygiene 78(3):349-357. Nov. 1963.
  - 22/ See "Family Planning in China." Japan Economy News (Nippon Kerzai Shinbun), 20 Jan. 1965. Also for a report of the observations of a Japanese medical team on their observations of family-planning clinics, abortions, etc., in mainland China see Dr. Kan Majima. "Chugoke de no sanji seigen ronso [Discussions on Birth Control in China]." Bungei Shunjei 42(2):144-148. Feb. 1965. Edgar Snow reports the following comment by Chou En Lai, "We are encouraging family planning, especially in schools, factories, and government offices, we have had pretty good results. Young people at these places know the advantages of late marriage and after marriage they have the desire to plan their families. Thus, family planning can be spread, if we try, but since it does require proper propaganda and education, it will take time. I feel that we should be satisfied if China's rate of population growth can be brought down to below 1 per cent within this century." P. 16 in "She On-lai shusho kaiken [An interview with Premier Chou En Lai]." Asahi Jyanaru, Vol. 7, No. 10, March 1965.
  - 23/ This paragraph was added after the address was delivered, because so many newspapers and even some scholars interpreted the address as a flat prediction of major fertility declines all over the world in the near future. Apparently the eagerness for such a decline and its potential news value makes it necessary to emphasize again what I thought was clear in the address itself.

- 24/ "The Theory of Change and Response in Modern Demographic History." *Population Index* 29(4):345-366. Oct. 1963.
- 25/ Lewis-Fanning, E. *Report on an Enquiry into Family Limitation and Its Influence on Human Fertility During the Past Fifty Years.* Papers of the Royal Commission on Population, Vol. I. London, H. M. Stationery Office, 1949.
- 26/ Bergues, H., et al. *La prévention des naissances dans la famille.* Institut National d'Etudes Démographiques, Cahiers de Travaux et Documents, 35. Paris: Presses Universitaires de France, 1959.
- 27/ Kingsley Davis and Judith Blake. "Social Structure and Fertility: An Analytic Framework." *Economic Development and Cultural Change* 4(3):211-235. April 1956.

### SUPPLEMENTARY NOTE

#### The Accelerating Fertility Decline in Taiwan\*

The rate of fertility decline, previously described for Taiwan,<sup>1</sup> was even greater in 1964 than in previous years. The general and total fertility rates, whether for all women or for married women, decreased much more rapidly between 1963 and 1964 than between 1962 and 1963 (see Tables 1 and 2). In each of the age groups in which fertility has been decreasing (15-19 and 30 or over) the 1963-64 decline was much greater than that in 1962-63.

Given this recent accentuation of previous trends, it is not surprising that the rates of decline for the last five years (1959-1964) are quite considerable. Total fertility fell 15 per cent for all women and 12 per cent for married women. For women over 30, the decline in fertility rates was largest and increased with age, ranging from 21 to 43 per cent among all women and from 22 to 44 per cent among married women. For women 20-24 and 25-29, fertility rates did not change significantly in the five-year period either for all women or for married women. For all women 15-19 the 20 per cent decline in fertility over the five-year period was in large part due to the decline in per cent married at ages 15-19 in this period. This is evident, first of all, in the fact that for married women the decline in fertility at ages 15-19 is only 4 per cent during this period (compared with 20 per cent for all women). The percentage married among all women, 15-19, declined by 17 per cent between 1959 and 1964.

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\*The basic demographic data for this report are made available through the co-operation of Mr. Yan-fu Liu, Chief of the 3rd Section, Department of Civil Affairs, Taiwan Provincial Government. They are processed at the University of Michigan Population Studies Center as part of its collaborative work with the Taiwan Population Studies Center of the Taiwan Provincial Department of Health under the scientific direction of Dr. L. P. Chow, Associate Director. The demographic work in all of these co-operating agencies is supported by grants from the Population Council. The work at the University of Michigan is also supported by a grant from the Ford Foundation. An important aid in all of the population and family planning work in Taiwan is the general leadership of Dr. S. C. Hsu of the Joint Commission on Rural Reconstruction.