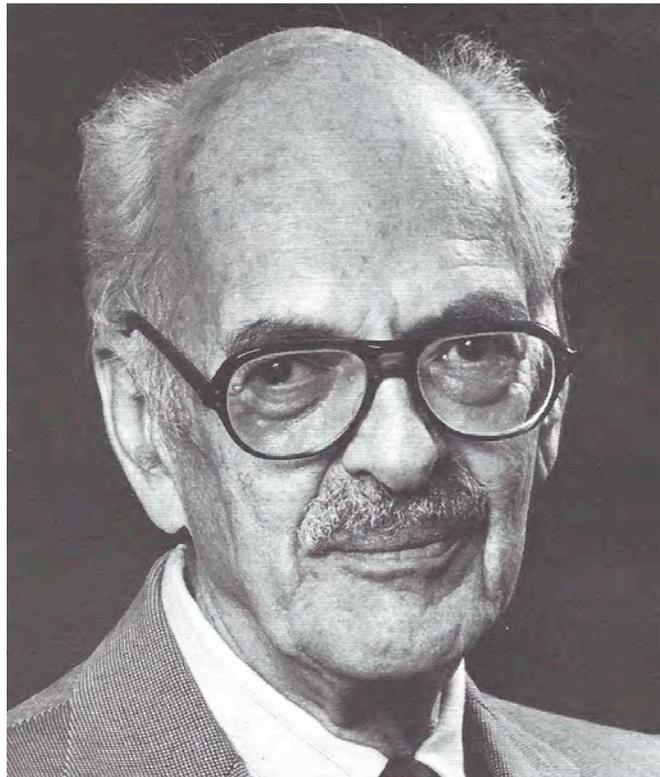


# **DEMOGRAPHIC DESTINIES**

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

### **Interview with Nathan Keyfitz PAA President in 1970-71**



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)

## NATHAN KEYFITZ

PAA President in 1970-71 (No. 34). "Self-interview" following a questionnaire supplied by Jean van der Tak, taped at Dr. Keyfitz's home in Jakarta, Indonesia, December 31, 1988.

Nathan Keyfitz, famous for his perceptive and original work in many aspects of demography, especially in the application of mathematical techniques, has spent several years since 1984 partly in Austria with the International Institute for Applied Systems Analysis and partly in Jakarta as adviser to the Indonesian government. He kindly offered to tape an interview on his own for this series.

**CAREER HIGHLIGHTS** [in Dr. Keyfitz's own words, with some additions]: I was born on June 29, 1913, in Montreal. [Ronald Freedman and Norman Ryder are PAA's other two distinguished Canadian-born presidents.] I received the B.S.A. in arts and science from McGill University in 1934 and the Ph.D. in sociology from the University of Chicago in 1952. From 1936 to 1959, I was with the Dominion Bureau of Statistics in Ottawa, Canada, as research statistician, mathematical adviser, and, finally, as senior statistical adviser. Also during that time, I lectured in statistics at McGill. Thereafter, I taught and did research at the University of Toronto, 1959 to 1963; the University of Chicago, 1963 to 1968; the University of California at Berkeley, from 1968 to 1972; and then at Harvard, from 1972 to 1983, and Ohio State, from 1981 to 1983. From December 1983 to the present time [and still as of spring 1991], I have been director of the population program at the International Institute for Applied Systems Analysis in Laxenburg, Austria [outside Vienna]. Currently [end 1988 and again in winter 1989-90], I am with the Harvard Institute of International Development in Jakarta, Indonesia, working for the Republic of Indonesia in the effort to improve its system of higher education.

At various times, I have also consulted, taught, or done research in Sri Lanka, India, Argentina, Chile, Germany, Hawaii at the East-West Center, China, Indonesia, and in some other countries as well.

[Dr. Keyfitz's many awards include several honorary degrees, election to the National Academy of Sciences, and the Mindel C. Sheps Award in Mathematical Demography of the PAA.]

My publications include half a dozen books and a large number of articles or book chapters, scattered over the years from about 1938 to the present time, and, I hope, not yet ending. [These "half dozen books" include, for example, Introduction to the Mathematics of Population, 1968, second edition 1978; Applied Mathematical Demography, 1977; World Population: An Analysis of Vital Data, with Wilhelm Flieger, 1968; and Population Change and Social Policy, 1982.]

[Dr. Keyfitz died in Cambridge, Massachusetts in 2010.]

The first questions I'm asked to answer in this self-interview are: **What led to your interest in demography and mathematical demography in particular?** and **Tell something of your career in Canada before coming to the United States.**

After I graduated [from McGill] in 1934, there was really not an awful lot to do. I worked for an insurance company for a little while and then landed in Ottawa in the Dominion Bureau of Statistics. At that time, the best that the Dominion Bureau of Statistics could offer me was a menial job, advising as it was called, editing as it would now be called, the census schedules in the 1936 census of the prairie provinces. This was rather uninspiring work, now entirely taken over by computer. So after a few months, I began to look at the census results. There was also the 1931 [Canadian] census for comparison with the 1931 census of England and Wales and the 1930 census of the United States and I compared the occupational distributions in these three countries. There were many difficulties in doing this-- occupations were differently defined--but some results stood out. I remember, for

example, that there were considerably more dentists per million population in Canada than in Britain and very many more dentists per million population in the United States than in Canada.

My work in this attracted the attention of Dr. Robert H. Coates, who was a very distinguished Canadian and the Dominion Statistician, as the post was called at the time, and my boss, but several layers up the hierarchy. He ultimately got me off the revision of the census schedules and I set to work doing one of a series of monographs commissioned to be done in-house on the 1931 census. I was assigned mostly to the monograph on unemployment--the general preoccupation of the time. My immediate superior was Murdoch McLean, a Scot who was far more interested in Gaelic than he was in statistics or in unemployment, but I was given a free hand and not responsible really for day-to-day work to anyone but myself. This privilege I used to learn some demography to write several chapters of the monograph.

I had been at this for a few years when a man named Dr. John H. Robbins, who was in charge of education statistics in the Dominion Bureau of Statistics and who was rather well known throughout the country for interest in the Canadian Social Science Research Council and who had contacts in the United States as well, told me that there were possibilities for study in the United States. The University of Chicago had indicated that it might have a fellowship open for someone interested in the study of fertility in particular and would I write a page or two, a proposal, to Professor William Ogburn, who was the chairman of the department of sociology at the University of Chicago and a very distinguished demographer. I wrote a page or two and they seemed to have made a hit, because almost by return mail, Ogburn offered me a fellowship at the University of Chicago that was intended to pay my tuition as well as living expenses. And it did so quite lavishly, actually, on the amount of \$2,500.

So I spent a very good ten months--in the first instance--at the University of Chicago. In those days, the residence requirements for a degree were much less stringent than they are now. It was possible for me to get a Ph.D. from the University of Chicago ultimately [in 1952] based on that ten months--three quarters--of residence [in the early 1940s].

Ogburn, Ernest Burgess, Louis Wirth, and Herbert Blumer were the stars of the department of sociology at the time. I got to know all of them well. I got to know various fellow students, some of whom have just disappeared, others of whom have gone on to great careers. I remember Reinhard Bendix as a fellow student, who had come from Germany and took his degree well before I did; he ultimately became the authority on Max Weber. There was a man by the name of Roy, who studied occupations, and Weinlein, who studied pharmacists under Everett Hughes.

Everett Hughes had come there from McGill. He had been at McGill when I was a student there in the early 1930s and had gone on to the University of Chicago and was very much present during the time of my stay there. I learned a great deal from Hughes and was in touch with him through most of the rest of his life. He went from Chicago just before I joined the faculty there [in 1963] and became a teacher at Brandeis and lived in Cambridge [Massachusetts]. Lived very close to us [during Keyfitz's time at Harvard, 1972-83], so we knew him and Helen well. We [Nathan and Beatrice Keyfitz] just had a Christmas card this year [1988] from Helen, who is in a nursing home but otherwise of good cheer and still brisk.

I guess that's enough to cover Jean van der Tak's first question. Except that she goes on to say that Phil Hauser, whom she has just interviewed [November 12, 1988], points out that I already had an international reputation in demography before going to Chicago for my Ph.D. in the early 1950s [receiving degree in 1952]. I think that's exaggerated. I thank Phil for making that observation. She says, quoting again Phil Hauser [not actually quoting him], that I had articles on Canada's population and on Ontario fertility in Population Studies in 1950 and 1951 and I was elected to the IUSSP [International Union for the Scientific Study of Population] in 1950, just two years after IUSSP membership was reconstituted on an individual basis. Jean asks me whether I was the first Canadian member; it's conceivable that I was.

I should say that Phil had a very crucial part in my development; this is years later and I'll get to it again in due course. In 1962 I was at the University of Toronto, having gotten my degree in sociology, and was teaching general sociology, not demography. At that time, there wasn't enough interest in population at the very conservative University of Toronto for me to offer a course in it, so I taught an introductory sociology course and some other courses. It was quite instructive to do so for the two or three years that I was at it. Phil had offered me a post at the University of Chicago once or twice earlier and I said, "Well, I have obligations to the University of Toronto. They got me out of the Dominion Bureau of Statistics," which at that time seemed a blessing and I got the enormous freedom of an academic post as against a public service post. So I did not go. But then in 1962-63, I took a year away from Toronto on leave as a member of the faculty of the Université de Montréal. My object in this was to learn French and I certainly learned French. To give a lecture, let us say, at 10 o'clock tomorrow morning in French means that you're scrambling through the dictionary all night and it does wonders for your French. I don't know how much it does for the students. But in any case, I had a very good year at the University of Montréal. They must have appreciated it; they did offer me a continuing post. But during that year, Phil was on the telephone again and I went along with his idea of coming to the University of Chicago. I got a visa without much trouble and showed up in the fall of 1963.

That was when my research in demography really got started. Under Phil's encouragement, I did quite a lot of things. I got into computing, which was new at the time. There weren't many demographers outside the Bureau of the Census who knew anything at all about computing. I practically lived at the computer center of the University of Chicago; programmed very extensively in FORTRAN; had students who were interested not only in demography but also interested in doing mathematical computations. Among others, there was Wilhelm Flieger, who was a priest from Bremen who turned out to be very talented in FORTRAN programming as well as an extremely able demographer. There was Michael Murphy, who went on to become a very senior official in the Canadian government, first in Statistics Canada, as the Dominion Bureau of Statistics had been renamed, and then in the Department of Health and Welfare, where he is to this day. He's a very important figure in Canadian demography--an American, who I think has become a naturalized Canadian. And his wife Mary, quite independently, has become a very senior official in the Department of National Revenue.

Well, that tells a little about my shift from Canada to the United States and how I got started doing real--I hope, real--demographic research.

### **Why did you decide to obtain a Ph.D. and why did you come to Chicago?**

I think I've pretty well covered that.

### **How did you manage to fulfill the requirements, commuting from Ottawa, and what was the topic of your Ph.D. dissertation?**

That I can answer, even after most of 40 years. I decided to come to Chicago because Will Ogburn offered me a fellowship there. How I managed to fulfill the requirements commuting from Ottawa is that the requirements were at that time very mild--three quarters in residence--and I got leave from Ottawa to do that.

The topic of my Ph.D. dissertation was fertility--the fertility of the Canadian population as given by the 1941 census. I took a sample--I was fortunate to be on the inside to do this, because there is a matter of confidentiality of the original census documents--I was able to use my insider position to take a sample of something like one thousand women in the province of Quebec and one thousand women in the province of Ontario. In the province of Quebec, they were all French-speaking, French

mother tongue, Roman Catholic religion and so on, so I had a pretty homogeneous sample. I was able to set up a factorial design by which these thousand women were used for a number of different contrasts: the contrast between those with much schooling and little schooling; the contrast between those of higher and lower income; I just used two categories in each of these. And, in particular, those who lived near a city--they were all farm women--and those who lived far away from a city, and those contrasts proved to be statistically significant. The whole thing was done quite properly, with random sampling and all. Each of the observations contributed to something like five different contrasts, as is possible in a two by five factorial study. Between those five contrasts and the restrictions on the sample as a whole, I was able to keep about 15 variables constant when I made the comparisons. As I say, I was especially interested in the contrast between those living near the city and those living far away from the city, because it seemed to me that there was a process of diffusion of the small-family pattern, and that showed very conspicuously, in fact. The diffusion of the pattern was clear in that with everything held constant--everything that could conceivably affect fertility that was measurable in the census--there was a difference of about one and a half children between the average for the far-away-from-the-city and the near-to-the-city. The diffusion of the small-family pattern was clearly indicated.

By the way, the striking diminution of fertility in Quebec took place subsequent to 1941, just after the war. The average number of children per woman in 1941 was something like eight and a half and it was not until many years later that average Quebec fertility dropped down to something like two children per woman. Now fertility in Quebec is the lowest of all the Canadian provinces, if I remember the recent numbers. So this matter caught the population just as fertility was starting to decline, just as the influence of the Catholic church was weakening. I thought there were some bad aspects of that weakening as well as good ones. But one of the features that was very conspicuous was the decline in fertility.

So that was my dissertation and it was sent to the University of Chicago. I remember the occasion well. I had to make a trip on behalf of the Bureau of Statistics to Australia and I contrived things so that I dropped off the dissertation on my way to Australia, gave the committee a month to read it, and arranged to have the hearing [defense] on my way back from Australia; I economized a trip. It did indeed work out that way. I had the oral. It was a bit awkward in one sense, in that the people who were examining me were mostly younger than I was. There was Leo Goodwin, with whom I later joined in research when I was on the faculty of Chicago, and there was Dudley Duncan, of whom I've been a lifelong admirer. These men were both considerably younger than myself, but they managed to look respectable enough at the meeting and we got through that all right. [See Dudley Duncan's and Philip Hauser's descriptions of this event in their respective interviews.]

**What impressed you most about Chicago, the department of sociology, the Population Research Center, and your colleagues and fellow students there during your student years in the early 1950s and as a faculty member in the 1960s?**

My student years were actually in the early 1940s, not the early 1950s, because I took most of ten years, while I was in the Bureau of Statistics, between when I did my residence and when I got the degree and I didn't have any great amount of personal contact with the department during those ten years. So my fellow students during my student years were pretty much a non-overlapping group with the people I subsequently knew. In particular, Phil Hauser, Dudley Duncan, Evelyn Kitagawa, who were later to become my respected colleagues--none of them were there during my student time. Among students who were there, as mentioned, were Reinhard Bendix, later the authority on Max Weber, who had recently come from Germany and was the star scholar in the department in the early 1940s, Roy, who studied occupations, and Weinlein, who was working on pharmacists under Everett Hughes. The faculty people that I listened to most and thought most of when I was a student included Everett Hughes, Herbert Blumer, Louis Wirth, Ernest Burgess, and, most especially, Will Ogburn, who

had been responsible for getting me to Chicago to begin with.

Ogburn I saw most of and most admired. He was a true aristocrat. I remember once at a tea he gave at his house that a lady visitor asked him where he came from; he came from Georgia. What did his family do there? He said they planted. She asked him what did they plant. He was surprised at the question; it caught him, of course. He had this natural aristocracy about him that never left him whatever he was doing, whatever he was interested in. His concern for the use of empirical data in research, I suppose, exceeded considerably his sophistication in mathematical manipulation, but he tried his best. I remember in one of his classes there were only two of us students; the other was Josephine Williams, who subsequently had a career in demography and sociology and was, like myself, trained in mathematics at least up to the undergraduate level; she had an M.A. in mathematics, in fact. We found his struggles to explain multiple correlation somewhat humorous, shall I say. Ultimately he discontinued the class; we would be better off to read it up in a book. He was a man of very great self-confidence. It didn't embarrass him in the slightest when he got stuck expounding some particular calculation.

### **What led you to leave Canada for the United States, also after a stint at the University of Toronto?**

That's a hard thing to say, but obviously the greatest influence in that direction was Phil Hauser, who had asked me more than once, who made an offer of degree of freedom of research in demography. The University of Toronto was not very interested in demography at that time. Phil was offering research facilities, a computer and all. Computers had not been heard of in Toronto [in 1963]. He was offering research facilities that were really unmatched in Canada. Canada was altogether far less specialized than the United States, so if I'd stayed in Canada, I guess I would have continued for many years at least--until the last decade or two--I would have continued to teach sociology, introductory courses. Perhaps I would get a chance to do one or two courses every few years in demography.

By the way, I did know Norman Ryder at a very early stage. [Ryder, like Keyfitz, came to the U.S. for his Ph.D.--Princeton, in his case--and returned to Canada to work, also at the Dominion Bureau of Statistics and the University of Toronto, before being lured back to the U.S. Ronald Freedman, PAA's other Canadian-born president, migrated to the U.S. with his family as a child.] I remember his coming to the Dominion Bureau of Statistics [in the early 1950s]. At that time, there was a question in the Canadian census called "racial origin," where race was defined as whether you were Irish, French, etc. I suppose it was to a considerable extent owing to Norman's very strong and continued argument--Norman is nothing if not persistent--it was owing to him more than anyone else, I think, that that question ultimately got modified. I won't say it was dropped, but it was certainly put into a more neutral form and the word "race" was dropped in favor of "ethnic origin." It still isn't very precise and it still certainly does not satisfy Norman's sense of what is useful demography and useful social science. But, as I say, he had a great influence in making the question at least respectable during the time when race was a very nasty subject.

### **Now I'm asked about the Berkeley department of demography at a time when it was the first--and only, as far as I know--department of demography per se in a United States university.**

The people that I knew there [1968-72] were especially the Davises [Kingsley Davis and Judith Blake] and Ronald Lee was there as a student; he later went on to Harvard. And we had other good students. I remember Roger Avery and quite a number of people of whom you subsequently heard the names.

The department flourished during its brief existence [1967-72]. But we didn't all get along very brilliantly with Judith Davis, who had very strong views as to what was the right thing to do. For example, I was asked by John Noonan to share a course with him. John Noonan's views on birth

control and abortion did not by any means coincide with those of Judith Davis. And from the time when I gave this joint course--by the way, I enjoyed it very much; Noonan is a brilliant man--my relations with Judith were strained. They were strained sufficiently that on getting a proposal from Harvard--from George Homans, who was chairman of the sociology department of Harvard at the time--I accepted with alacrity. I might well have accepted the Harvard invitation even if I'd been getting along swimmingly with Judith, but her antipathy to my activities helped to make the decision very easy.

At the same time as I made that decision, Sam Preston, who was on the faculty, and Etienne van de Walle, who was there as a visitor and whom we'd hoped to attract as a member of the faculty, both left, for Pennsylvania. [Preston went to the University of Washington at that time.] So it looked as though Judith was the only member of the department. The university did not want to have her alone vote in a new faculty, so it proposed that a committee be set up, including non-members of the department, of course, that would appoint a new faculty. As I remember it, Judith didn't want this and she carried on a war with the administration. Her carrying on a war with me didn't really cause her any trouble at all. But the administration of the university, right up to the president, was too much for her and ultimately the department was disbanded and she found herself down at the University of California at Los Angeles.

But, you know, the department was disbanded and yet in a way it continued. There is a [Graduate] Group in Demography now that is doing remarkable work under Ronald Lee. It has in it Eugene Hammel, who is a very distinguished anthropologist whose main interest is population, and it has other very good people. It has managed to collect together some brilliant students--Andrew Foster and others whose names I've forgotten. I've had little contact with it, but I have been enormously impressed with the quality of students and faculty that have been assembled there under Hammel and Lee, and then Kenneth Wachter, who is one of the stars of the department. In a way, there is continuity between what Judith Blake started--well, what Kingsley Davis and Judith started--and the present activities. I've always admired Kingsley Davis enormously and I'm still in contact with him. Indeed, I'm working to present a paper to a conference that he's holding in Palo Alto in February [1989; papers published in K. Davis and M.S. Bernstam, eds., Resources, Environment, Population: Present Knowledge, Future Options, supplement to Population and Development Review, forthcoming July 1991]. Well, so much for the Berkeley department of demography.

**Do you think that U.S. demographic training has suffered from a lack of specific departments of demography other than population research centers?**

I've always held it that demography by itself is a methodology that is invaluable, of course--it uses masses of data; it has its own techniques--but that the substance of demography is not really self-contained. It has to go along with sociology or anthropology or economics or biology and, by itself, it really doesn't have the weight of substance that a discipline needs. So I'm not averse to demography being done in departments of sociology and other disciplines and I would say that probably demographic training has benefited from close association with sociology and other disciplines.

**Jean asks: What are your brief recollections of Harvard and Ohio State?**

I guess she wants it brief, because I spent some eleven years at Harvard [1972-83] and found it just a great place to be. I had outstanding colleagues, partly in demography--Roger Revelle, Howard Hyatt--but in sociology especially, including Talcott Parsons, George Homans, Daniel Bell, Orlando Patterson, Nathan Glazer, David Riesman, who retired during my time there, and many others. Everett Hughes was not a part of the department but he was very close to the department and we saw a great deal of him. So Harvard and the Cambridge community were just great. We had eleven good years

there.

In due course, about 1981, Bill Petersen recruited me, somewhat indirectly but in effect, to replace him in a chair that I think had been set up for him at Ohio State University--the Lazarus chair for population research and teaching. Lazarus is a major department store in Columbus. I guess Ohio State was the largest single campus in the country, about 55,000 students and an enormous faculty, among whom there were some very, very good people. We had good associations there. My wife and I spent three summers there, as I recall. We bought an apartment and made ourselves a part of the community in Columbus, Ohio. We still own the apartment.

I subsequently was made emeritus of Ohio State University as well as of Harvard, so I was really on the faculty of Harvard simultaneously with Ohio State. In my declining years, that is between 65 and 69, Harvard's rules permitted me only a half-time appointment, only a one-semester appointment. So it fitted well with my work as Lazarus Professor at Ohio State. Mind you, work was exactly the same whether I was at Harvard, Chicago, or whatever. I was doing the same demographic research. I just moved my peanut stand, as it were, to a new location, but I was selling the same product. [Jane Menken, in her interview, cites Keyfitz as an example of "someone in our field" who has perhaps been more active than ever as a professor emeritus: "I remember laughing that when Nathan retired from Harvard, he remained on the faculty half-time and at the same time accepted a professorship at Ohio State which was two-thirds time. That was `retirement.'"]

### **Who were the leading influences on your career and why?**

Well, I suppose John Robbins, who back in 1939 or so was in touch with Will Ogburn. John Robbins was a Canadian social scientist, a member of the Canadian Social Science Research Council, who knew his way around the funding situation. He was a colleague of mine in the Dominion Bureau of Statistics, head of education statistics. He told me that there was a fellowship available at the University of Chicago for someone who had worked on fertility. I proposed to Ogburn that I come down there and work on fertility and he accepted the proposal and I started my Ph.D. studies. That was one factor, working through Robbins and Will Ogburn.

I suppose that the biggest element in my getting going on research and being reasonably productive was Phil Hauser, who had this great American spirit of generosity. It came as a surprise to me; Canadians aren't quite that open and quite that generous. He got me funding, showed me how to get further funding myself, got me connected up with the computer at the University of Chicago, got me some very good students, and in general launched me in the research career. I wasn't that young at the time. Since this was 1963, I must have been fully 50 years old when I got started on my real research work. I had done a few papers before, but my real research work started in 1963.

I worked for some five years to produce this book. Jean is very generous in her remarks, but I can't say that I was publishing two books a year. It happened that between 1963 and 1968 I was working on two books and they both appeared in 1968 [Introduction to the Mathematics of Population and World Population: An Analysis of Vital Data, with Wilhelm Flieger].

I was enormously aided by one of the students that Phil Hauser recruited for me, Wilhelm Flieger; we did this World Population together. I did a lot of programming for it, but then Bill really re-programmed all of what I had done and got it much better, much simpler. Ultimately we got the book out, using the very primitive technology of that time, using the mainframe of the University of Chicago.

I should say that there was another book of the same kind, also I believe called World Population, put out by Freeman a few years later [Keyfitz and Flieger, Population: Facts and Methods of Demography, 1971]. Most recently, we've been in contact with the University of Chicago Press. Bill is very active in the Philippines in the Catholic University of San Carlos in Cebu. He and I have been in touch and have agreed that there's room for another issue of World Population now, using the

data subsequent to 1965. What is more important is that the University of Chicago Press also thinks there's room for a new book. We hope that it will be out in the next year or two, using very modern technology, all done on microcomputers, much of it in Bill's place. Although I've never been there, I can imagine that's not a world center of computing or demography, so it will mostly be done on micros in his little population research center at San Carlos University in Cebu.

Among other influences on my career, besides Phil Hauser and John Robbins and Will Ogburn, there was Leo Goodman. Leo Goodman impressed me from an early point; his way of thinking and thinking about a problem until he got down deeper into the essence of it than anybody else had done and coming up with a simple answer to matters that had puzzled the rest of us. That he did, for example, on the kinship question that we published jointly [in, e.g., L.A. Goodman, N. Keyfitz, and T.W. Pullum, "Family Formation and the Frequency of Various Kinship Relationships," Theoretical Population Biology, Vol. 5, 1974, pp. 1-27].

Then there was Dudley Duncan, whom I didn't get to know personally so much but whose work always inspired me--his ability to learn a brand-new field, his ability to get at the empirical aspect of a subject, his ability to cut through nonsense.

I haven't mentioned Don Bogue. I saw a good deal of him at Chicago; he got me involved in the journal Demography. I thought there was a good deal of no-nonsense in Don. He didn't get deep into mathematics. He had an incredible capacity for work; he produced books the way other people produce articles. His books are very solid, very well organized. I must say I learned a great deal from Don.

Then, of course, there were many people [at Chicago] not in demography. I took classes with Richard McKeon, a classical scholar and philosopher, and I was influenced by his sharp way of organizing his philosophical argument. I learnt something even from Robert Hutchins, who was president of Chicago and very much in view at the time I was a student. He wasn't especially liked by the sociology department, and yet I thought he had a great deal to say. I later made contact with him at the Center for the Study of Democratic Institutions in Santa Barbara. He was then getting on in years but his same self.

There were people at Harvard when I was on the faculty there, in my first years there. I saw a great deal of Talcott Parsons and of George Homans. They didn't get along well with one another. They had very, very different perspectives on the world: George, down to earth and empirical and practical and with his psychological view of society; Talcott, rather high up in the stratosphere often, but nonetheless a scholar in his knowledge of the literature, in his willingness to discuss matters with someone like myself who really hasn't ever gotten very deep into the great works of Weber and Durkheim.

At Ohio State, the man who impressed me most and with whom I had many, many discussions was Saad Nagy, who became chairman of the department during my time there and who did excellent studies of rehabilitation and other matters, rather on the margin of medical sociology.

Also in medical sociology, I ran into David Mechanic at one stage, at Wisconsin, where I was for a semester, and I saw a lot of Paul Starr, who wrote that great book, The Social Transformation of American Medicine.

**What took you to Austria and the International Institute for Applied Systems Analysis? Tell something about your work there.**

At the end of 1983, I was at the age when neither Harvard nor Ohio State really had any further use for me. Under rules then operating, nobody, but nobody, could be a member of a university faculty after the age of 70, so I was, in effect, put out to pasture. These rules no longer apply, by the way; circumstances are quite different in the last five years. But at that time, I was effectively put out--as everybody else was--at the age of 70.

The International Institute for Applied Systems Analysis had an opening. Andrei Rogers, whom I'd seen on previous occasions as a visitor to IIASA and who'd been a very effective head of the population program there, doing great work on migration using his multistate model, was leaving IIASA and going to the University of Colorado at Boulder. There was a post there and I was invited to come over and fill it by C.S. Holling, the well-known ecologist who was the director of IIASA for the first couple of years of my tenure in that post. I changed rather considerably the direction of the IIASA population work, moving it toward the study of aging. I was able to get good material from member countries and from the United Nations. We were able to get very good computer facilities. IIASA had excellent software and very skilled computer operators. So I found IIASA a fine place to work. I repeat that I really only do one thing and it turned out that IIASA was a good place to do it.

While I was there, I had contact with many people--some better than others. Ake Andersen, who hails from the University of Umea in Sweden, closer to the Arctic Circle than any other institution I've been associated with or known about, an economist and statistician, was a great inspiration to me for the three or so years that we overlapped.

I'll be going back to IIASA in May [1989]. This is the fifth consecutive winter that I've spent in Indonesia; spending six months in Indonesia and six months in Austria. Choosing the best part of the year in each one has been very satisfactory. But just how long I'll be able to keep up this shuttle is not at all clear. My present contract with IIASA extends to the end of 1989 and whether I'll be able to continue beyond that remains to be seen. [Dr. Keyfitz was still at IIASA as of spring 1991.]

Shortly after I got to IIASA, Mr. Reagan decided that we were giving valuable material to the Russians and cut off the United States contribution. That was a very serious financial blow, of course. In a way it was flattering to us to think that we were providing such valuable material to anybody. I had thought that we were working in systems analysis in a rather abstract fashion, making models of various kinds. The Russians had very good mathematicians; they're probably better than the American or other mathematicians. And it seemed just so unlikely that the kinds of things I was doing, or other Americans were doing, would have been of great military--or any other--value to the Russians, more than they were of value to any other member country. In any case, Mr. Reagan cut us off and gradually we've been cutting back on a project basis; American aid to IIASA has tended to be on a project basis rather than the unconditional annual contributions that we get from the 16 other countries.

**Who were your most outstanding students? Tell something about your love of and expertise in teaching.**

I was fortunate in having many, many good students. I've mentioned some of them--Mike Murphy, Ronald Lee. McFarland comes to mind; Andrea Tyree comes to mind. I think that Bill Hodge was still a student when I first came to Chicago in 1963. [See list of students, toward end of the interview.]

I learned a great deal from students and I learned a great deal in attempting to present matter so that they would understand it. If my writing has any clarity, it's due to students saying again and again, "We don't understand that argument." Students are a great discipline to an academic. It seems to me of just fundamental importance that teaching and research be combined. If you have research only, you get narrower and narrower; if you have teaching only, you get broader and broader, but more and more superficial. It seems to me that the two are absolutely essential.

I conceive something of the way in which teaching and research stimulate one another in the United States by noting the absence of the same here in Indonesia. I'm going to tell more later about my work in Indonesia. But now I can say that that peculiar combination that it seems to me the United States academy presents in its best and most developed form, that peculiar combination of teaching and research in graduate school, is designed to produce the most worthwhile results in research and produce a new generation of scholars that will ultimately replace the teacher.

The absence of it in Indonesia makes me clearer than ever on how important it is in the

American academy. In Indonesia, research is not all that important. There isn't any sense that it's possible to gain new knowledge. There's some sense that really everything has already been discovered and it's only a matter of reading about it in books and finding out about it and then teaching it.

That sense that there are plenty of things to be learned, that much of what is thought to be known really isn't true--that is what you get in graduate school, and, of course, through the process of submitting your work to refereeing in journals. That is something that has been developed over the long history of academic work in the United States. It's come about gradually; it's had a great emphasis in the postwar period. And you see how difficult it is to obtain it when you experience a less developed country and you find that it just does not exist.

One of the advantages of the American academy over the Indonesian one is that an academic in the United States really has no place to go except to better research and attracting better students. That's the only place he has to go. In Indonesia, the academy is part of the civil service; being a professor is just one stage in rising through the civil service. You may well move from being a professor to being a dean to being a director of a division in the public service, not necessarily with respect to education at all; it might be in trade or somewhere else. You then go up to be director-general; you may ultimately become a minister.

This fact that the academy is just a part of the promotional system in the civil service makes it that nobody takes the work of the academy seriously. It's something that you're going to do for three or four or ten or twelve years, depending on how lucky you are and how quickly you get promoted out of it. And in those circumstances, nobody does very serious research--or very serious teaching, for that matter.

We're trying to get that changed here. Whether there's any prospect, we don't know; it's very strongly ingrained. That attitude toward knowledge is something that stands out so clearly in the American university. In other parts of the world, in less developed countries, it's going to take a long, long time to develop.

### **What do you see as the leading issues in demography over the years that you have been involved?**

I guess that is something that I have thought a little about. The issue in demography at one stage--this goes back to my student days--was whether one should use models or whether one should just talk about the subject. I suppose Dudley Duncan had more influence than any other single person. Again, Phil Hauser--although he himself was not a master in manipulating models--still had a sense that the way Dudley was doing it was right and he pushed me in that direction.

It's very interesting to me that when I travel in Czechoslovakia or the Soviet Union, for instance, and talk to demographers there, I find the demography of, say, the United States in the 1930s, when talking about the subject, writing about the subject, was pretty well the style. There's some sense that one really has to do it a little differently and getting themselves modernized is what the Eastern countries are very anxious to do now.

Another issue was whether demographers should study the demographic situation of the world and their own country and own locality or should try to do something about it. I have tended to be on the side of those who wanted to study it. I always took the view that doing something about it could be carrying you in the wrong direction if you didn't know what the problems really were. It seemed to me that there's room for a division of labor, of course, between those who are going to do and those who are going to study and think. And I tried as much as I could to be on the side of the studying and thinking.

In the 1930s, there was a lot of writing about how the birth rate was falling catastrophically. I remember that Enid Charles, a British demographer whose name, I imagine, is not very well

remembered now, wrote a book called The Twilight of Parenthood, complaining about the fall of the birth rate in the 1930s and how the race was going to decline and disappear. That phase of bemoaning the low birth rate, saying something should be done about it, gave way--I guess as soon as the war was over; well, let's say in the 1950s anyhow--to a sense that there were just too many people; the birth rate was much too high. Some said even in the developed countries it was too high on account of the baby boom, and certainly was too high in the less developed countries. There was quite a controversy at that time between those in the advanced countries who said that the less developed countries would grow much faster economically if they would grow more slowly demographically. The less developed countries at that time didn't believe it and there were strong voices among them that said, "We need more people; people are our only strength; we don't want to hear those in the advanced countries who are trying to deprive us of our one source of strength."

It's curious that in recent years that situation has been reversed. The less developed countries have learned that they don't need all those many people, that they will advance far more quickly and have far fewer problems--economic, political, ecological, and others--if they cut their birth rate drastically. That applies conspicuously to Indonesia. It applies practically to all the other countries that I know, except perhaps Malaysia and Singapore. In any case, that's the side that is now taken by the less developed countries.

The advanced countries, especially in the United States, especially in neoclassical economics, have changed their side and they are now where the LDCs were in the 1960s, saying that a rapidly increasing population doesn't really constitute a great disadvantage to economic growth and, in effect, saying, "Sure, people should have choice as to whether they're going to have children or not have children; the great argument for birth control is essentially an ethical one--people have the right to decide. The economic argument for checking growth is not a strong one." That's a neoclassical argument.

I think it's fairly wrong. I think the less developed countries know their own interest by now and their efforts to control population are based on what they see. That is unemployment and what they see as the difficulty of getting capital, because capital to them is not a matter of something that you make at home. Capital is something you buy from the United States or Japan; capital is equipment that can't possibly be made in the less developed country and there's only so much possibility of buying it. They want to hold down population to what will be able to work with the capital that they can secure.

## **Experiences in Burma**

My first contact with the less developed countries was in Burma around 1951 when I spent three months in Rangoon helping with a kind of trial census that was being done there. The government of Burma then, as now, had only a very tenuous hold on the country. It was firmly in command in Rangoon, but once you got out of Rangoon the various kinds of minority groups, ethnic groups, brigands, and others were fighting the government and there was not the slightest chance of taking a census for the whole country. We did our best in respect to Rangoon; developed procedures for editing the schedules, procedures for punching cards, making tabulations and such.

I worked then with U Kyaw Khine and U Soe Liang. U Soe Liang was a young government worker official with ambitions to become a demographer and I got along extremely well with him and his family, to the point where I actually moved into their house during the last month and a half of my stay. I lived with this couple and their children and learned a lot about the less developed world. They were formally Buddhist, of course, and Buddhism was their religion if anybody asked. In the front living-room, there was a shrine to the Buddha and the lady of the house, who was probably the more pious member at that time, would do her meditation in front of the Buddha regularly every morning, starting about 4 o'clock. But I discovered in the course of my living with them that there was also a

shrine in one of the back rooms of the house to the tree spirits. These tree spirits are regarded as very, very powerful. They are effectively a pre-Buddhist religion; they go back to very ancient times. And they were available as a religion for the practical contingencies of life. If you wanted to overcome an enemy, you wanted to deal with the contingencies of ordinary life, the tree spirits, who had a very elaborate shrine, would give you the right kind of advice, would actually do some of your work for you. This to me was very interesting, this combining of Buddhism and the pre-Buddhist, very primitive--well, primitive is not a fair word, that's the word that came to mind--primitive religion.

U Soe Liang died a few years ago, but we've kept in touch with his wife and son Nathan, named for me.

I found Burma just about the most romantic place I had ever been and I still don't know of any place that shows more dramatically the contrast between East and West, the nature of an alien--I have to say as an alien--culture and civilization. I've had no contact with Burma since, to my regret. Phil Hauser went there. He must have spent quite a bit longer time than I did [a year, 1951-52], and he got to know Soe Liang and the other people. Of course, Phil got around far more than I did. I think he traveled a good deal through the country; the government provided him with suitable protection. All in all, I think he had a great experience.

## **In Indonesia**

I had another opportunity in Indonesia. I took a year in 1953 with the very young Republic of Indonesia. A Roundtable Agreement, in which the Dutch had signed a treaty giving Indonesia its independence, had occurred only four years before, in 1949. So everything was very new. There was a great sense of euphoria: "Now that we've got the Dutch rulers out, we're going to take over and we will make ourselves a proud and independent country." I got this sense of euphoria from a Dr. Sumitro, who was the Minister of Finance at the time and for whom I worked, and from Dr. Djuandra, who was head of the Planning Bureau and subsequently became Prime Minister, perhaps Indonesia's most distinguished Prime Minister ever. Unfortunately, he died; I believe he died in office. Very conscientious, intelligent man. And I did some demography at the time.

I also found that the Planning Bureau, for which I was supposed to work, didn't really know what it was going to plan, and the amount of activity there seemed to me not such as to encourage my making great contributions. So I spent some of what would otherwise have been idle time, I guess, living in a village in East Java with some young people, including a Professor Widjojo, who subsequently became the architect of the Indonesian economy, with Dr. Permadi, who subsequently became a director of the Bank Rakyat Indonesia, a rural banking system, and with others. We went and lived in this village. These were students who had been assigned to me as counterparts and with whom I worked. I was very fortunate in the selection of my counterparts, all of whom proved how good they were by becoming truly important in the subsequent development of the Republic.

Living in that village in East Java in 1953 taught me a great deal and also taught my student collaborators a great deal. They were city-bred; they didn't know all that much about the way the rural population lives. We ended up by all of us knowing quite a lot. I took 150 pages or so of notes at that time. I would interview somebody and then rush off to the room in which I lived and type up these notes. I never made any use of them; they were kicking around.

I rediscovered them in my files around 1983 and it seemed to me that it would be a great opportunity to examine that same village and find out how things had changed. That was the experience that was described in my article that Jean refers to, in the December 1985 Population and Development Review ["An East Javanese Village in 1953 and 1985: Observations on Development"].

I went back in 1984, I guess it was, during my first of five [recent] trips to Indonesia, and lived there for a while and saw the changes. And the changes were tremendous, extraordinary.

Here there were two schools in the village, where in 1953 there were no schools. Nobody

could read and write, virtually, aside from the headman. No one could speak Indonesian among the adults; they all spoke the local language of Javanese that I never got to understand. I could communicate mostly through the headman, who did read and write and did know Indonesian, as did very few of the senior members of the village. By 1984, everyone under 40 was able to read and write. They'd all been through primary school and many had gone on to a secondary school in a neighboring village.

They had paved the main village road. Electricity was coming to the village and there were already several television sets. The primitive, home-made houses of 1953 had given way to professionally designed and built houses by 1984. The physical aspect of the village, the amount of education and knowledge of the world on the part of the people who lived in it, the level of living, were all just very, very different.

In 1953 there was a very rough time for the three or four months before the big harvest. The local language calls it a paceklik; that means just not enough food. People who had started after the harvest eating rice dropped down to corn a very few months later and by the time of the paceklik were eating nothing but cassava, which is not really at all nutritious. These people of 1953 had given way to villagers who were unquestionably eating rice the year round. There was no food problem at all. The pounding of rice to hull and polish it, which was done by the village women from prehistoric times and certainly in 1953, was now done by a hulling machine, and for a trifling sum, the farmer could get his whole crop hulled, saving the village women an enormous amount of arduous work.

Those were the sorts of changes that I reported, including, by the way, the changes in the attitude toward the family, so that the small family was well on its way in this peasant village by 1984.

I certainly agree that micro-level demography, the way John Caldwell practices it, is very much a major approach in our discipline, even though my own study was not extensive enough or deep enough that I could produce results that were specifically demographic. My time there was short--it was a matter of months rather than years--so I didn't really get into attitudes on population and practices in relation to childbearing, causes of high mortality, and such. In retrospect, I would have liked my two visits to have concentrated a little more on the demographic aspects of life in that village, rather than being of a general ethnographic character.

## **Publications**

Jean has a question about my work on policy and indeed I have written many pieces that profess, at least, to deal with the policy consequences of demographic research. I don't have my publications in front of me, so I'm not in a position to say what is important and what is less important in my work, as she asks me to do, in any detail.

I certainly spent the first half of my life on the mathematical and technical demography that she describes. There were two books on that subject. Introduction to the Mathematics of Population came out in 1968 [revised second edition, 1978] and dealt with what the title says, the mathematics of population, rather than demography as such. The book has been quite widely used, but when I saw it in print I was a little dissatisfied with its somewhat abstract character and wrote another book, also mathematical, called Applied Mathematical Demography, published by Wiley in 1977. This had no overlap at all with Introduction to the Mathematics of Population and concerned, as again it says, applied mathematical demography. In that I had some original pieces, but mostly it was an attempt to round up what was known on the subject regarding everything from the effect of contraception, the making of life tables, on a great number of questions that could be dealt with mathematically and that involved techniques needed by demographers.

As I've mentioned, it's considerably exaggerated and I'm flattered that Jean thinks this, but it's not true that I wrote two books every year. There's only one year, 1968, in which I published two books [Introduction to the Mathematics of Population and World Population: An Analysis of Vital

Data, with Wilhelm Flieger] and I'd written them in the preceding five years. Subsequently, I have written maybe half a dozen books and they were spread over a considerable length of time.

## **Population and development**

The policy questions have indeed been interesting me more and more and right now, as mentioned, I'm preparing a paper for a conference that Kingsley Davis--who is one of my heroes--is holding in Palo Alto [at Stanford's Hoover Institution] in the next couple of months [proceedings cited above]. I don't know whether I'll be able to get there or not, but I'm very anxious to contribute a paper, in which he has asked me to deal with the question of population and development. As I see it, that question breaks down into four pieces.

The first has to do with the Malthusian notion of why it is that increasing population is a handicap [constraints on land and resources]. According to John Stuart Mill and the whole English economic tradition of the 19th century--up to World War II really--when you have more people you have to use marginal land, marginal sources of energy, and you just push further out on a curve, a line of diminishing returns. There are some increasing returns to scale in manufacturing industry, but sooner or later you bump up against the limits of the environment. That's the resources argument.

That has been greatly altered by the process of invention; the process of almost automatic invention of substitutes in the 20th century. With copper still being used in conducting electricity but with high tension much less of it is needed; copper for transmission of signals being made really quite unnecessary through the advent of glass fiber and many other ways of communicating--satellites and such. In respect of tin, for instance, we've learned how to put the tin on the tin can more thinly and save about half of the tin. In respect to rubber, we've learned how to produce rubber in factories, which is every bit as good for most purposes as natural rubber. We've learned how to make fibers that are more satisfactory than jute and sisal. We've learned how to use cellulose sponges, artificial rubber or acrylic in place of kapok, so that many of these tropical products, especially, have been substituted.

It seems to me no coincidence that the process of substitution came simultaneously with the ejection of the white colonials from the tropics. Once the Europeans lost their colonies, they really had the strongest incentive to work hard in the laboratory and that is what they did. They replaced the colonies in effect--Holland is an outstanding example--by clean, highly productive, capital-intensive factories in the former metropolitan country.

Germany had really set an example of just that in the 19th century. Not having colonies, having been disunited and hence late in the race to grab off Africa, Asia, and any other part of the world that was to be had for the conquest, being late in the process vis-a-vis France and Britain especially, she was far behind. She turned to her laboratories and made among other things--I think the first article that she made was a substitute for indigo, for dye-stuffs, and the German dye industry very quickly replaced thousands of hectares of indigo plantations in India and elsewhere.

The process was picked up by the former colonial powers after World War II. It meant that the tropical populations, which had really built up on the need for tropical products--one example is sugarcane, which was very important in the development of Indonesia in the 19th century and which was responsible for the growth of population--that is to say, the law and order established by the Dutch and the minimum of medical care and perhaps the somewhat better nutrition that the sugar incomes made possible all went to increase population quite rapidly. Then the populations of India, what is now Pakistan, Indonesia, and Asia generally, built up really on the strength of these colonial products, were left high and dry. That is part of their population problem here.

In any case, because of the process of invention, the world is not going to suffer starvation for lack of food. Africa, in this as in many other things, is an exception. But the Green Revolution as I see it in Indonesia and as I read about it in India ensures that lack of food--the original Malthusian threat--no longer sets a limit on population.

The economists got rid of resource shortage shortly after World War II. The [Ansley] Coale and [Edgar] Hoover book [Population Growth and Economic Development in Low-Income Countries], published in 1958, dismissed the possibility that India could ever have serious food difficulties as its population increased. That was fairly radical for its time, but it has since proved to be correct.

But Coale and Hoover did find another reason why population growth was disadvantageous in the shortage of capital. If the population is growing rapidly, then each new generation has to be schooled, has to be clothed, has to be fed, and has to be equipped for industry, and this could go on at only the level that the previous generation had and only the level of equipment that the previous generation had and still use a fair part of the limited national savings that were available. So Coale and Hoover concluded that because of shortage of capital, not the absolute population growth but its rate of growth was disadvantageous to development.

Subsequently, economists--neoclassical economists in particular--have also got rid of that. They show that capital has not been all that important in the more developed countries, that it only explains a small part of growth. Much more is explained by improved education added to human capital, and the suggestion is that human capital effectively supplants physical capital. Of course, if people are all that are needed in production and neither land nor capital, [the other two features] of the classical trinity, are required, then the population can grow just as large as it wants and get ever richer. I may be unfair in describing it so, but that seems to me essentially the model that is now dominant in neoclassical economics.

I have to say that in so far as I'm an economist at all, I'm an economist of the older school, the one that at least believes that capital is important and that capital is not available in indefinite quantities to less developed countries. Moreover, that capital is something that has to be imported from abroad to a considerable measure. The old idea that capital is made in the form of a loom by a village carpenter that is then used to give employment to the village women in weaving cloth--that kind of capital is not really all that important. It can't compete with automated capital, which is beyond the capacity of the less developed country to produce itself right at the outset. So you have the whole phenomenon of indebtedness.

The countries do their utmost to buy capital in order to employ their young people. And this employment problem is what continues to preoccupy them and it preoccupies them especially with the stringency of the availability of capital, the fact that those loans have in effect gone into reverse, so that the flow of funds is now from the less developed countries to the more developed in the form of service on the preceding loans being greater than the amount of new loans and the merchandise trade surplus. So the preoccupation with giving the younger generation, now relatively well educated, employment is the third reason for restricting population.

Classical economics says that all this would be overcome if the country had a free labor market. There's no reason why a free, unrestricted exchange won't clear labor markets just as completely as it clears capital markets or commodity markets. And yet nowhere do people allow a free market to work in the case of labor. Free labor markets don't exist anywhere. United States probably comes closest. Europe has innumerable restrictions in the form of minimum wages, restrictions on hours, restrictions on who can work, extensive payments to those who are out of work, great restrictions on dismissals of employees, so that an employer, for instance in Italy, will think very carefully before he takes anyone on, knowing that he's either got him for life or has to pay very heavy severance penalties to get rid of him. All these restrictions on the labor market, which are very familiar and found everywhere in the less developed countries--perhaps even more than in the developed countries--make it that the young people can't get into the circulation of jobs and goods and such. The more fixed the people that are already there are and the higher the wages are set above the equilibrium level, the more impossible it is for the younger generation to get jobs, and especially to get jobs that accord with their education and expectations. So the employment problem is thought of everywhere in the less developed world as a reason for restricting population.

The fourth reason for population planning--I've mentioned land and other resources as a first constraint on population growth; capital as the second; and employment as the third--is ecological. The environment won't stand it, especially in an era of hectic development, with the middle classes of the world--I've written extensively on that; I had an article in Scientific American some ten years back--increasing perhaps at the rate of four and a half percent per year, as measured by automobiles in use, television sets in use, and so forth. The resultant increase of carbon dioxide in the atmosphere, accentuated by the cutting down of forests, especially in the Amazon but everywhere in the world in fact--that is itself partly caused by the increase in population--the process is making it that we're undergoing an irreversible increase of carbon dioxide in the atmosphere. That is regarded as virtually sure. This isn't my view alone; it's the view of the people who know something about it. A virtually inevitable warming of the biosphere. And that's going to have dreadful results. The results will be much more awkward for the less developed countries than for the more developed countries. The people who live in the deltas of the Ganges, for instance, the Irrawady and other rivers--Mekong in Vietnam, for instance--are going to find that the sea level is rising. Something of that kind has been seen in the floods that we've had right here in Java in this particular year [1988]. Not only deforestation has been responsible for some of the flooding but also the permeation of ocean water in the low-lying coastal areas.

In any case, you have a real damage to the atmosphere, to the ozone layer, brought about essentially by the mode of living of the developed countries, and whose bad effects are going to be most keenly felt by the less developed countries. And it's going to be very, very difficult to get international cooperation to deal with this question.

Beyond that there are other difficulties facing the less developed countries. This indebtedness problem that involves a paradox--that American, German, and Japanese banks want the money they loan returned with interest, they want up-to-date servicing on those loans at the same time that their governments, under pressure from their manufacturing industries are doing their utmost to keep out manufactured goods from the less developed countries. And without knowing any economics, it's very clear that those loans can only be repaid by goods. They can only be repaid by the more developed countries standing for a negative balance in their commercial trade. They have to receive that money back in the form of goods; they can't possibly get it in the form of cash. Indonesia now has a debt of some \$40 billion; it's far from the worst. It has a treasury with four or five billion dollars in the form of assets. There's no way that it's ever going to be able to repay the \$40 billion unless the developed countries will accept its textiles, its other labor-intensive manufactures. And the more developed countries are putting barriers against the acceptance of these goods.

The policy consequences of all this very much involve population. They don't involve population alone, but they much involve population. And that is the subject of my present researches.

## **On PAA**

I'm now asked whether I remember the first PAA meeting I attended and Jean was good enough to provide a list of all the meetings that ever were. Despite that, my memory is not very sharp. I think it would have been certainly in the early 1930s. I'd come down from Canada for the meeting. I believe Leo Schnore was there and certainly Paul Glick and Henry Shryock. The thing that struck me most, I remember, was there was just a tremendous enthusiasm, a tremendous loyalty to the PAA. These people were determined to put together an association that would last. And it has not only lasted, but has expanded from the 38 people who were present at that [organization] meeting in 1931 to--as I see it on this list--well over a thousand at all the recent meetings, including meetings out on the West coast and in New Orleans that are not all that easy to get to. The success of the Association, I think, has been due to a continuance of that enthusiasm and devotion on the part of a certain number of people.

Now I was president in 1971. I indeed presided over a meeting at the time when the Women's

Caucus proposed three resolutions. [Presented at the Board meeting and at the business meeting during the April 1971 PAA meetings in Washington, D.C., these resolutions proposed: (1) elimination of discrimination on the basis of sex in graduate admissions and professional opportunities in population studies, and special recruitment programs to increase the proportion of women in the field; (2) removal of legal and financial obstacles to access to contraception, sterilization, and abortion; and (3) development of non-familial roles for women. For details, see Harriet Presser, PAA history vignette on the Women's Caucus, PAA Affairs, Winter 1981.]

The truth is, I was a little baffled at the people who spoke against them, because the existence of discrimination in demography--I don't think we're in any way different from sociology or economics or engineering or anything else--the history of discrimination was rather clear and these were attempts somehow to get around that discrimination. That had somewhat of an aura of affirmative action that seemed not to appeal to the membership. In any case, all three were turned down.

All I can remember now, is my feeling of puzzlement that they were regarded as apparently dangerous by the majority of the membership. [Presser points out that the Board rejected all three resolutions, but the membership at the following day's business meeting, while rejecting the second and third resolutions, passed "a modification of the first resolution which added references to race as well as sex and additionally called for a committee to be appointed to study the extent of sex and race discrimination in the population field."] My guess is that there was a subsequent considerable liberalization on the part of the membership.

My own view, by the way, of the women's liberation movement, that is no longer the last word, no longer in fashion, is that it left a permanent residue in American society. It isn't the residue that the proponents of ERA and such would have liked. And yet it is a permanent and very clear residue, as noted in the recruitment of women by the medical, engineering, and other professions and the increase of women in the academy. It is not enough to satisfy the more enthusiastic liberated women, but still a definite, clear advance that is not likely to be abandoned.

### **What changes do you see in PAA meetings?**

The much increased number of sessions. And I don't think there's any doubt that there have been substantial improvements. We've become an important national society now, where in the 1930s we were a small clique, a small sect if you like, of people who had a special scientific interest and where there was no thought of masses of recruits.

### **What do you see as the outlook for demography and demographers?**

I am certain that there is room for new theoretical and technical contributions. You can read Demography and Population Studies and other journals. And the number of journals is increasing. I think that prior to around 1963--I may be wrong on this--but I think the only journals devoted to population were Population Studies and Population of INED, in French and Population Index. Now it's at least half a dozen that I know of, and some other national journals in various languages--in German, Hungarian, Spanish, Indonesian for that matter--are flourishing. There's an Australian journal, a Canadian journal. So the expansion of the field and the large number of journals and the fact that they continue to publish worthwhile material seem to be the answer to Jean's question of whether the old-timers of my generation said it all. I think it's quite clear that there's plenty to be said still.

### **What do you see as the outlook for world population? Are you discouraged by the slower than projected decline in the population growth rate, which has been stuck at 1.7-1.8 percent for several years?**

I am, somewhat. But I don't look at the growth rate; I look at the absolute number of people. The growth rate, the birth rate, has really nothing directly to do with what I now see as the constraint on population. It is the absolute number of people that is important. Those who point to the growth rate and say it has been declining and so the population problem is solved have to look at the fact that the absolute number of births has been increasing, will be increasing rapidly by the end of the century and till about 2030, if I remember right, and projecting far into the future. It will not be until the middle of the 21st century that the annual number of births will come down to the number that we have today. In other words, the absolute increment to the world population is going to be as great or greater than that of today until the middle of the 21st century. And when you think of the effect on the environment of this rapidly increasing number of people and especially think of the increasing fraction of them that will be driving automobiles and otherwise in high consumption and contributing to the carbon dioxide in the atmosphere, then it seems to me you really have to worry about the problems ahead. And it seems to me also that the sooner people take into account the effect of all this on future generations, the better it's going to be for the long run of the world.

It isn't fashionable at the moment to think of future generations. You see that in the low savings rate in the United States. There's no point in saving, people say, we've got insurance against sickness, we've got old-age pensions--no point in saving. Our children are independent and hopefully professionals of one kind or another and well established. Saving is out. The statistics show that. And when saving is out that means that nobody is concerned about the long-term future of their family or of the country. And even less are they concerned with the long-term future of the world as a whole.

So we have a real problem in this population growth that I think is more acute, perhaps, than it's been at any time in history. I'm not even referring to Africa, where I think chronic starvation is ahead. But just thinking of the world as a whole, the expansion of the middle class--which in itself is a very good thing--the kind of consumption, the prospects for unemployment of the younger generation in less developed countries as well, that is going to have tremendous implications for social stability. It seems to me that the demographic component of the world's future troubles is by no means trifling.

### **What are you doing now in Indonesia?**

I got into Indonesia in the first place, five years ago, in the interest of carrying on my demographic studies, and I did that for a couple of years. But then my Indonesian superiors felt that I would be more useful in a study of the higher education system in Indonesia. And it does indeed involve problems.

Indonesia has gotten to have something like a million students in higher education. Well over 200,000 come in each year, either to the public or the private institutions. The institutions got started at a time that really was not very favorable; it was the late 1950s. Whatever may be said about the Dutch colonial regime, the Dutch faculty members who were sent out here were absolutely first-class scholars and they maintained an extremely small but extremely high-quality university system that consisted of four or five small but excellent institutions. That obviously was not good enough for the new republic, which needed masses of highly trained people. And in a populist move, Sukarno in the late 1950s, when he had no money and there were no faculty from abroad left in the country, established most of the 44 public universities that now exist.

Some of these are still fairly good and in the best ones there are some very good faculty. The best teachers in the best schools are doing a wonderful job. But the mass of graduates who are being turned out really don't know enough to be very useful to the private sector, to the developing industry. Indonesia, with the disappointment of oil prices, is turning to manufactured exports, and relatively successfully, despite all the difficulties. And it needs large numbers of trained people. The people that come out of the universities are not highly trained. The engineers are not skilled engineers; the managers are not skilled managers, with some exceptions. And so they don't get jobs. This means

great unemployment at the same time that there is a great need for skilled people.

And that is the problem of the universities: how to up-grade themselves; how to make themselves high-quality, without diminishing the number of students. And without greatly increasing the budget; there is no large amount of money available for education. And without stirring up any political difficulties. Education has become a rather political issue here because it is the means for social mobility and anything that you do in regard to the universities is being watched by many people. These things are politically dynamic--much more dynamic than the government likes. The whole situation is very touchy.

I'm trying to make proposals for the improvement of quality in the universities. And that's quite incidental to my demographic interests.

### **What are your plans for the future?**

I'm not sure how long this shuttle between Vienna and Jakarta is going to be able to continue. My health is fairly good, but my endurance is not as unlimited as I once thought it to be. So I just can't say how long this shuttle is going to continue. Then I will settle down and stay at home--wear slippers all day. I just don't know when that is going to start, but it could be moderately soon.

### **Additional comments:**

#### **On students**

One respect in which the preceding [self-interview so far] is particularly weak is the recollection of my students. I don't have any list here by which I can systematically recall them. But quite a number do come to mind among many others.

Lee-Jay Cho at the University of Chicago. Lee-Jay is Korean. He really did very well in our work there and subsequently got to be head of a very important center, the East-West Population Institute. Jay Palmore, Bob Gardner, and Bob Retherford, students of mine at Berkeley, are also in the Population Institute in Hawaii. Also at Berkeley were Frank Oechsli and Bob Lundy.

Reynolds Farley, who of course is well known to PAA members and subsequently became president of PAA, was a student of mine in the first year when I joined the University of Chicago in 1963. Judah Matras was a student at the University of Chicago.

Among the people who studied with me at Harvard, Noreen Goldman has subsequently done some brilliant work. And Nick Eberstadt is a very distinguished writer, somewhere on the boundary between demography and journalism, but perhaps better known to a wider audience than most of the rest of us. Also at Harvard, I had an Indonesian student, Mayling Oey-Gardiner, who subsequently has been teaching at the University of Indonesia in Jakarta and been doing important research here.

There was a Brazilian couple, Juan Carlos and Maria Elena Lerda, who both were students of mine, first at Berkeley and then at Harvard. They subsequently divorced; subsequently had good careers, both of them, down in Brazil.

### **Some of my students [list added April 25, 1989]:**

Chicago:        Wilhelm Flieger  
                  Michael Murphy  
                  Andrea Tyree  
                  Tom and Starling Pullum  
                  Dhruva Nagnur

Berkeley: Griffith Feeny  
Jay Palmore  
Roger Avery  
Robert Lundy  
Robert Retherford  
Frank Oechsli  
Robert Schoen  
Robert Gardner

Harvard: Noreen Goldman  
Mayling Oey-Gardiner  
Juan Carlos and Maria Elena Lerda  
Jose Gomez de Leon  
Robert Semiring

Ohio State: Susan Mott

### **On colleagues**

Among colleagues who have influenced me greatly must be mentioned Sam Preston, whom I had the good fortune to have as a colleague at Berkeley. I guess it was just after he got his degree at Princeton, he was very young, he came to Berkeley and we saw a great deal of Preston and Winnie.

Paul Demeny--we were both at the University of California in Berkeley for a period of time. He was one of the people that we'd hoped to attract permanently to that program in demography.

Evelyn Kitagawa was a colleague and we were very close to one another from the first days of my coming [in 1963] to the University of Chicago.

Jane Menken I've known for a long, long time and she is indeed a relative by marriage of mine. [Her brother is married to the daughter of the Keyfitz's.]

### **On children**

I didn't say anything about my family. My daughter Barbara got a Ph.D. in mathematics at NYU and subsequently has climbed the academic ladder. She taught at Princeton, Columbia, and now at Houston [University of Texas] and she has finally got to the full professorship [of mathematics]. And is becoming fairly well known in a field that I don't read in and I doubt that many members of the Population Association are familiar with--partial differential equations as applied in aerodynamics and hydrodynamics.

My son Robert went on to get a doctorate in economics at the London School of Economics and is now a valued member, in fact he is the manager for forecasting of a big consulting firm in Toronto.

### **On Beatrice**

The most serious omission in what precedes is my wife Beatrice, who really is responsible for a very large part of what I have done, if it's of any value at all. When we first met, back in 1936, I was an extremely junior employee of the Dominion Bureau of Statistics and I spent a good deal of my time outside of the office in such desultory activities as amateur photography, taking the car apart, playing chess now and again, listening to gramophone records I collected--all these sorts of things that really are more appropriate to a man who is retired than one who is trying to get going in some kind of

career. Well, Beatrice really changed all that. She decided to make something of me.

Her first move was to see that I wore a clean white shirt every day. She herself personally washed and ironed me a shirt each morning. And that white shirt actually did rather well by me; it ultimately got me to a middle-level position in the Bureau of Statistics. But, of course, that would only go so far. She needed to do something a little more basic than that. And so she got me into this business of writing. She typed my work; went through innumerable drafts. She rather pushed me to take an academic position when the opportunity arose. I turned down a couple of opportunities first and she wasn't going to see me turning all of them down and getting older and older. So Beatrice really helped at every stage.

Not only that, she doesn't have any formal schooling to speak of, but is extremely knowledgeable. She has an incredible memory. Any book that she has ever read--and that is quite a large number--she simply knows. I can ask her the plot of any English novel, of many French novels, even Russian novels, although she doesn't read Russian in the original, and she will have it at her fingertips. Her knowledge of biology, of nutrition, of medicine far exceeds mine. I just constantly refer to her as to an encyclopedia.

She is able to entertain me, in the swimming pool--we do half an hour regularly swimming up and down the pool each morning--or after dinner she will entertain me with a literary criticism, with an account of what James Joyce had in mind in some of his what is to me more obscure writing.

So her part in making me whatever I am is really fundamental.

You know, various people have spoken about how an individual is formed. Freud thought the individual was formed in his first few years of life. Marx thought the individual was formed essentially by his work. I think the individual is formed to a considerable extent by his or her spouse. I don't know what effect I've had on Beatrice, but the effect that she has had on me is enormous and entirely positive.

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## Nathan Keyfitz

### Past Honored Member

Nathan Keyfitz far exceeded his cohort's average life expectancy having passed away in 2010 at the age of 96.

He had three challenging careers, during which he profoundly influenced the demographic profession. His first career as a statistician began at the Canadian Dominion Bureau of Statistics. At age 46 he began his academic career at the University of Toronto. And his unusually creative third career began after he retired from academia at age 70.

In 1934, Keyfitz graduated with a B.Sc. in Mathematics from McGill University in the depths of the Great Depression when jobs were scarce in Canada. He took his first job at the Canadian Dominion Bureau of Statistics where he spent 23 years, first as a research statistician and later a senior statistical advisor, using his mathematical skills to analyze unemployment statistics and demographic trends of the Canadian population. According to Keyfitz's fascinating memoir (see <http://keyfitz.org/nathan/memoir>), the Bureau's water cooler is the site of his first encounter with Beatrice Orkin; Beatrice later became Keyfitz's wife for 70 years of marriage and the mother of their two children, Barbara and Robert. In 1951, Keyfitz obtained his PhD in Sociology from the University of Chicago, with a dissertation on the fertility of the Canadian population. This launched his second career.

At age 46 Keyfitz accepted his first academic post at the University of Toronto. This was followed by academic appointments at the Universities of Montreal, Chicago, U.C. Berkeley, Harvard and Ohio State. Near the end of his Chicago stay, he published his widely influential *Introduction to the Mathematics of Population*. In 1977, he published *Applied Mathematical Demography*, an innovative book that illuminates the many ways that simple mathematical tools provide insights into complex population issues; the third edition was published in 2005, 28 years after the first!

Keyfitz's academic career formally ended with his retirement at age 70 from two institutions – Harvard and Ohio State – which marked the beginning of his third career. In "retirement" he became a consultant to Harvard's Institute for International Development and the head of the population program at the International Institute for Applied Systems Analysis in Austria. He continued to be in constant demand as a consultant and advisor both domestically and internationally, building upon his earlier worldwide travels. Keyfitz took his first international trip at age 38 to Burma to help with their census. Subsequent trips included Indonesia, India, Sri Lanka, Argentina, Russia, China and many other countries. Along the way, he taught himself numerous languages, typically by reading foreign newspapers during his commuting time. The issues he confronted in his travels informed many of the research questions he subsequently addressed. His interests and publications increasingly focused on issues of environmental and food security, population and sustainable development, the ethics of consumption, climate change, and poverty. But he continued his academic pursuits in aging, social security and pension systems both domestic and international, still with a strong interest in Canada.

With his creativity and keen understanding of the interplay of population models and data, Keyfitz is often considered to be the founder of the field of mathematical demography. However, his interests were so broad, his analytical abilities so formidable, and his publications so vast that he has influenced the intellectual history of many disparate fields, both methodological and substantive. He stood at the forefront of population research for more than half of a century. He is sorely missed by his friends, colleagues, and former students.



## MODELS\*

Nathan Keyfitz

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Demography has made some progress in developing and applying models. I shall trace the peculiar way in which the models of demography have come into existence, suggest what they are good for, and say where they fall short. My main question will be whether they can be broadened to help resolve some stormy issues that have broken over the study of population and its habitat in the last two or three years. All this within the half hour allowed by custom on this occasion.

Our discipline has been greatly influenced by demand for prediction of future population and by efforts on the part of our members to satisfy that demand. The public expects predictions. When I meet someone who asks me what I do, and I reply that I work at the mathematics of population, the reaction is something like: "Great! Then you're the man who can tell me what the population of the world will be in the year 2000," or even: "When, according to your figures, will there be too many people in the United States?" To protest that I and others practicing the profession of demography are interested in facts by no means escapes the question; the most interesting facts are those relating to the future.

If the questioner is experienced enough to know that prediction is difficult, he at least supposes, when he hears about demographic models, that the accuracy of prediction is a test of the models. The demographer, he might say, has every right to look into many aspects of past

and present population, but he validates this right, and distinguishes himself from the ordinary run of men, by now and again doing a prediction. Like the Pharisees in the Bible who asked Jesus for a miracle, our public wants a sign from heaven that we are authorized, that we are not mere guessers like themselves. Our calculations may be esoteric, but everyone can understand a prediction that the census of 1980 will count 230,000,000 persons in the United States, and any Pharisee can check its accuracy when the census total is released about August 1980. By doing this small miracle of prediction we not only help people in their practical affairs between now and 1980, but when that date comes around we will have proved our competence by an unmistakable sign. The desire to be useful, and even more the desire to prove competence, are strong. We do not reply as Jesus did to the Pharisees, "There shall be no sign given to this generation."

Rather we attempt a miracle, and the miracle fails miserably. But instead of being compromised thereby, we are strengthened. This strange and seemingly contradictory condition needs explaining.

The work of P. K. Whelpton, described in his own words (1963), will lead to the explanation and also to my main propositions concerning models. Whelpton sometimes refers to his work as projections, sometimes as forecasts, sometimes as calculations, sometimes as estimates. I will follow Harold Dorn (1950) in treating these as equivalent. Whelpton was not so innocent as to be unaware that people used his projections for pre-

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\* Revised text of the Presidential Address delivered April 23, 1971, at Washington, D. C., as part of the annual meeting of the Population Association of America.

diction, and calling them projections is a legitimate way of removing some of the burden of responsibility implied by prediction. The main point is that Whelpton cared a great deal how his projections compared with the population counted in subsequent censuses.

"My first attempt to make population projections," he says, "began in 1926. It soon became evident that it would be desirable to project age-specific rates for births, deaths, immigration and emigration, and to compute the resulting population." In this very beginning of his work 45 years ago he invented the components method, while his contemporaries were still extrapolating with logistic and other curves. Implicit in his work-sheets was the now well-known discrete population model, since expressed in matrix form (Leslie, 1945), given a stochastic interpretation, and otherwise elaborated. Before the idea was appropriated by mathematicians, Whelpton was applying what we now know as non-stationary Markov chains. Whelpton's demographic intuition anticipated some higher mathematics, but he paid no attention to that and went on to other population problems.

For his projection did not turn out to correspond very closely to the subsequent performance, and so Whelpton was "led to a desire for birth rates that were specific for parity as well as age, in order to compute better gross and net reproduction rates." The recognition of parity has also led to valuable models. But again more was needed, for the parity-specific rates were not effective predictors either.

The third improvement was "putting together the birth rates for successive ages in *successive* years as well as those for successive ages in the *same* year." And so the cohort approach was born, which has meant other major changes in our discipline and outside it.

Whelpton's story has now reached 1945; he has gone from the extrapolation

by curve fitting used until his time to the components method, to parity, to cohorts—and still the predictions are off the mark. In fact, the results were so bad that Whelpton said he was glad the published report did not mention that cohorts were used, for this might have prejudiced further use of cohort tables. "Cohort tables showed what had happened in the past but not what would happen in the future." Twenty years of labor had not brought him much closer to accurate prediction. Continued discrepancies suggested a fourth approach, which was to be his last.

This was "to question a representative national sample of young married couples regarding the number of children they expected to have," an idea that led to the 1955 and 1960 studies of Growth of American Families. Logical enough. If we want to know how many children women will have, let us ask them. Extensive and important results were subsequently produced by surveys based on such direct questioning. "Now," Westoff, Ryder, and others may well say, "the demographer need not depend solely on censuses and vital registrations; these multi-purpose operations serve population analysis only incidentally to many other objectives; sampling permits the demographer to collect his own data."

But has this helped the accuracy of prediction? Ryder and Westoff (1967) are the first to say no. When you ask presently married women their childbearing intentions you cover fairly well the births of the next five years or so, but beyond that time the presently married will for the most part have completed their families. And to ask a girl not yet married, say ten years old, how many children she would have if and when she married is a hypothetical question that can only elicit an arbitrary answer. So this fourth and last phase of Whelpton's search for a dependable method of prediction turns out little better than the first three. Is the whole long effort, forty

years' work of one of the most creative of demographers, a failure?

Chekhov says "one must be a god to tell success from failure." Think of the inventors who worked to build perpetual motion machines. No such machine resulted from their efforts, but they and their critics established the first and second laws of thermodynamics; those basic laws of physics emerged from efforts of inventors and mechanics to get something for nothing. The search for a gambling system, destined to failure as surely as perpetual motion and equally animated by the desire to get something for nothing, was a stimulus to probability theory. Optimists tried to trisect an angle in a finite number of operations with ruler and compasses from about the fifth century B.C. Mathematicians of the nineteenth century A.D. proved that it can't be done. A failure? Not at all—one of the outcomes was group theory, an enduring part of both pure and applied mathematics.

Population prediction is our perpetual motion, our trisection. Except when the trends are smooth it is likely to fail. But the attempt to predict has inspired many of the demographic models that we now possess. Such models permit experiments out of which we obtain causal knowledge; they explain data; they focus research by identifying theoretical and practical issues; they systematize comparative study across space and time; they reveal formal analogies between problems that on their surface are quite different; they even help assemble data. I shall give brief examples of these claims for models and then go on to show how prediction in the face of ecological problems that now challenge us can produce even more important models.

One immediate use of Whelpton's projection is to explain why the presently underdeveloped countries have much younger age distributions, with higher dependency ratios, than the countries of

Europe; indeed they have much younger age distributions than the countries of Europe when Europe was in the condition of underdevelopment in 1800. Is the difference between the high dependency ratio of Venezuela now and the much lower one of Sweden in 1800 mostly due to Venezuela's birth rates being higher or to its death rates being lower? The answer is found by projecting Venezuela with its own rates of birth and death, and then with its own birth rates and Swedish death rates; the difference in the proportion of children under 15 is due to the difference in death rates. Trying this and other other combinations shows that the difference as observed between Sweden and Venezuela is about three quarters due to birth rates and only about one quarter due to death rates (Coale, 1956; Keyfitz and Flieger, 1971). Whelpton's projection model has permitted a genuine experiment.

Other such experiments come quickly to mind. What is the probability that an American boy will ultimately die of heart disease as against some other cause, on the mortality rates of the United States at the present time? Again a model, this time the multiple decrement life table, an extension of the ordinary life table used for survivorship in population projections, tells us that the overall probability is well over one half. All other causes of death together account for less than half the probability.

Then does it follow that the elimination of heart disease would greatly step up the rate of increase of population? It does not, as we easily find by projecting with a life table in which the deaths from heart disease have been eliminated and all other rates of death and of birth left as they stand. But it greatly changes the age distribution, especially at the older ages: the number of men 85 years of age and older would increase twelve-fold as a result of heart disease being eliminated, supposing all other causes to persist at present rates. This is decidedly

different from the effect of eliminating deaths from cancer alone, which would increase the number of men over 85 by only about 50 per cent when the age distribution had shaken down.

These are conditional statements, and as such they are true beyond debate, given their assumptions that death rates by age from all other causes and birth rates by age of mother will remain as they are. Because other things will in fact change, they are not the concrete or unconditional predictions so much sought after, and which seem forever out of our reach. Yet when a particular decision is to be taken, do we not want to know what the result of that decision will be by itself, in abstraction from all other changes? There may be circumstances in which the conditional prediction is more helpful than the concrete prediction of what will *really* happen. The main technical difficulty here arises from effects associated with the proposed change—for example, the effect that the elimination of heart disease would itself have on death rates from other causes.

What about the effect of emigration on subsequent population numbers? The authorities in Indonesia have attempted to relieve Java's population problem by outmigration to Sumatra. They know that for this purpose the departure of young couples is more effective than the departure of old men, but how much effect does the departure of young couples have on Java's rate of increase? Again the Whelpton model tells numerically the conditional effect of emigration at one age as against another (Widjojo, 1970). The continuous form of the population projection, pioneered by Lotka (1907, 1939), permits some compact general formulas (Keyfitz, 1971). Arranging that all emigrants were 17 years of age would secure the maximum effect in holding down the rate of increase, but even if this were administratively feasible, well over 1,000,000 emigrants per

year would be required to hold Java's population constant.

In the economic development field, how much of a lift will it give to India if she reduces her birth rate by one half over 25 or 30 years? Coale and Hoover (1958) constructed models of what would happen, demographically and economically, on unchanged birth rates and on reduced birth rates. A better answer might be found than the one they give, but only by whoever can devise a more realistic model, and in the fifteen years since they did their work no serious rival to it has appeared.

Models explain data on population characteristics such as age. Coale (1957) shows how a human age distribution is determined by a fertility and mortality pattern. There are five women for every three men at ages 85 and over in the United States. The large and rapidly increasing mortality difference between the sexes obviously has something to do with the matter, but exactly how much of the difference between the number of men and of women enumerated does it explain? Amos Hawley (1950) shows how to use the life table to answer such questions. In their systematic study of population characteristics in the United States, the Taeubers (1958, 1971) again and again ask questions of their cross-sectional census data that they can answer only in terms of models. They apply models retrospectively to show how things got the way they are. Even more explicitly, Freedman (1969) explains the falling birth rate in Taiwan by models of conception and birth developed by Potter and others. These are in turn based on mathematical analysis due to Sheps (1967).

Models concentrate the interest of the profession and prevent the scattering to which empirical materials by themselves often lead. Frank Notestein's (1945) demographic transition, a verbal model, has enabled us to concentrate on a host of problems: why was the transition in

England different from that in France, and why was the Japanese transition different from both? Merely setting down the ideal form of transition, giving it a name and an existence, has provided a frame in which the demographic process of an individual country can be placed, so that it may be studied in a genuinely comparative fashion. Kingsley Davis (1963) made the transition dynamic—the engine that drives down the birth rate is the fall in death rates.

A model systematizes comparisons across time as well as over space. Abbott Ferriss (1971) tells me that work on social indicators at the Russell Sage Foundation leads to the conclusion that a time series becomes of interest when it corresponds to a variable somehow linked into a model (Sheldon and Moore, 1968; Land, 1971).

Note that such examples imply a liberal view of models: they may be algebraic, arithmetical, computer simulation, or verbal. Mathematical models have the advantage of applying a longer and tighter argument. Simulation (Mindel C. Sheps and others, 1966; H. Hyrenius and others, 1964–70) gives numerical answers where the mathematics is intractable. Verbal models often grasp most effectively the essentials of the action.

Models are in competition with one another and survive or are eliminated in a process of natural selection. The PAA annual meeting is a great marketplace in which models, along with data, are put on public display and bought and sold; the demographic journals have the same function of facilitating exchange. The selection is the more effective the greater the knowledge and objectivity of the buyers in the market, the more intense their concern with real problems and with goodness of fit to real data.

Models help research by revealing unexpected similarities between problems in different fields. The probability of marriage at successive ages is formally

analogous to the probability of dying, and the life-table technique applies to both. In fact the same life-table technique applies to anything that can happen only once to an individual—it formally unifies first marriage, having a first child, a contraceptive failure that results in the next child (Tietze, 1967; Potter, 1967), the probability that a given marriage will be broken by divorce (Jacobson, 1959).

Another example is migration among regions, on which Everett Lee (1957, 1966) has produced monumental work. Internal migration turns out to be formally identical with mutations among genetic characters at a given locus. Again, from the viewpoint of the number of individuals in the next generation, a person's being sterilized, leaving the country, or dying amount to the same thing: the effect of any one of these on the ultimate rate of increase of a population is exactly the same. Paul Glick et al. (1963) have analyzed how in the wake of the baby boom girls seeking husbands somewhat older than themselves encounter a shortage, what he called the marriage squeeze. Following the down-slope of the baby boom about 1980, men will experience a shortage of girls of the ages they usually marry. How do ages of brides and grooms adjust to meet such changes in the availability of potential spouses? The problem is analogous to the matching of applicants and jobs; the marriage market and the labor market are formally similar. Since both present difficult problems, it is a help to be able to transfer results in one field to the other.

Models are even useful in the assembly of data. Death statistics for a country may look reasonable but, when made into a life table, show an expectation of life of 90 years; clearly something is wrong. When we see that the male deaths of Spain 1967 imply an expectation of life at birth two and a quarter years higher than the United States for the

same year, we at least question the statistics. The life table calculation permits a critical assessment of official data—population and deaths by age—that look reasonable on the surface. In other instances, theoretical relations between birth and death statistics and age distributions enable us to compare births inferred from ages with births as registered. For many countries the comparison shows the registrations to be low and not usable for serious demographic work. Assessment of this kind has become possible on a large scale with the advent of the computer.

And finally, despite what I said earlier, models help prediction. Projections by the United States Bureau of the Census, the Dominion Bureau of Statistics, and other national agencies, as well as the United Nations, based on alternative sets of assumptions, appear conspicuously among official statistics—for example at the very beginning of the United States Statistical Abstract. They wisely leave it to the user to decide which set of assumptions he prefers and hence which future population.

In a similar category are what I call counter-predictions, showing that something cannot happen. Mexico cannot continue with its present rate of increase for as long as the lifetimes of children now born. For if it did, the population would double four times, that is, multiply by 16, so that there would be 800 million people in Mexico. Since this is virtually impossible, the argument shows that opposition to birth control cannot be a matter of principle (except for those willing to advocate higher death rates), but that differences of opinion can arise only on whether birth control is to be applied a little sooner or a little later.

Every professional forecast is conditional on a set of assumptions, preferably stated explicitly and in large type. Judgment in selecting the assumptions is crucial, and unfortunately cannot always be objectively assessed, even after the

event. Because reality is complex, we must always seek means to test the model underlying the prediction more directly than by whether it predicts what subsequently happens. Suppose, for example, that someone proposes to project using the stable population model; it is his responsibility to check the applicability of this model in advance by seeing whether the observed age distribution conforms to the stable age distribution. The agreement of a prediction with the subsequent record is both too late and too unconvincing a test of the model on which the prediction was made.

As long as a series changes uniformly, extrapolation will give a perfectly good forecast, and this was how reputations for forecasting population could be gained during a long period ending in the 1930's. Since then we have had to live with turning points. To forecast a turning point requires a model incorporating the mechanism that causes the turning point. The line between extrapolation and a model is not always sharp, and an example from the education system may help us draw that line.

The difficulty that recent Ph.D.'s in some fields have had in getting jobs could not have been foretold by extrapolation. Up to two or three years ago the number of universities granting higher degrees was increasing, individual universities were steadily increasing their output of graduates, and everyone with a doctorate was immediately employed. Extrapolation could lead only to the conclusion that every young person in the country would attain the Ph.D. and be hired as a college teacher.

Now consider a model that divides the educational system into just two levels, say college and graduate school. A certain fraction of those graduating from the lower level enter the upper level as students. Teacher-student ratios at both levels are fixed. For each level the supply of teachers in any year is equal to the teachers surviving from the year be-

fore, plus a certain fraction of the current graduates of the upper level. This primitive model is sufficient to show that the *absolute demand* for new Ph.D.'s is closely related to the *rate of increase* of college students, and that a levelling off in the number of entering college students results in drastic declines in the demand for Ph.D.'s for teaching. Such a model could have foretold today's turning point, in which Ph.D.'s in physics, for example, are having to revise considerably their employment expectations.

Extrapolation is bound to miss the turning point, while an appropriate model can reveal it in advance. Thus when a turning point comes along it has the use of telling us whether what we have been doing up to then was extrapolation or the application of an appropriate model. Demographers can be grateful for at least one turning point per decade since the 1930's, for though distinctly unappreciated at the time it was these that compelled and inspired Whelpton to devise his successive models. One turning point showed that the demographic calculation was not being correctly made—that it should be arranged in cohorts rather than in periods, a point that Norman Ryder (1964) has developed much further. Joseph Spengler (1966), thinking of other turning points, has insisted that the birth rate is a function of price and income as well as of social values. To predict the demographic future before the future happens requires equations that include more than purely demographic variables.

On a broader front, advanced countries are today up against a far more general turning point, of the puzzling sort that fills one with respect and awe in the face of the future and forces a rethinking of the fundamentals of a discipline. At least four signs of a turning point are described in current literature: deterioration of the environment, the interaction of population and income in causing this deterioration, instability in

the ecosystem, and new social attitudes to life and work. Each deserves at least a few words here.

The increasing scale of human activities has begun to overwhelm nature, which heretofore has mostly looked on man's work with immutable indifference. Since the Middle Ages men of European descent have multiplied by ten, and in a sudden acceleration at the very end of this second millenium of our era, the total energy at their disposal is multiplying by ten each generation. The exploitation of lands and mines, the output of solid, liquid, and gaseous waste, everything that comes under the heading of "conquest of nature", have reached a new level of intensity. Automobile scrap heaps are conspicuous in America and will become so in Europe, just as cathedrals were conspicuous in the Middle Ages. The commercial and technical advantage of disposable containers and disposable vehicles (perhaps in disposable cities) leaves its mark not only on the earth but on the moon, where biodegradation is even less capable of restoring the environment. Technology provides untold benefit; the question is how to secure the goods it produces without what Boulding (1966) calls the bads.

The fault lies both with the number of people and the technical means at their command. Just how much of the trouble is population numbers, and how much the kind and quantity of income? Economists long ago perceived this problem of imputation in respect to the output of goods, and they devised joint production functions by which the effects of a separate increment of capital or of labor can be calculated. We need the same kind of joint model for the production of nuisances. The N.D.E.—Net Damage to the Environment—is as important in a negative way as the G.N.P. is positively; the same effort to measure it is required, along with demographic-economic models to explain it in terms of population and income. With such

models, incorporating as well our growing disposition to concentrate in the rings of metropolitan areas, pencil-and-paper experiments can be made that will precede action and hopefully guide it. The destructive aspects of the interaction of population and income are no more to be neglected than the positive aspects.

The food ceiling that seemed to limit population in Malthus's day had at least the advantage of stability. Agricultural and other production was so constrained that there was little danger of population rising through the ceiling and then crashing down; I do not recall that Malthus anywhere foretold sudden *ecological* disaster through excess population. For him nature was constant and exercised a severe but steady control over human numbers.

The achievements of modern agriculture, culminating in the green revolution, have by-passed the ancient fixity of land and food, and bought a precious 10 or 20 years. But soil destruction through over-use, water contamination by fertilizers, exhaustion of minerals, also set ceilings on population, and these lack the one benign feature of the Malthusian food ceiling; they can let population rise temporarily well above its long-term upper limit.

So also can the widespread use of a pesticide. It raises food production by keeping down insects so that population can increase. But then the insects adapt genetically, and the pesticide works its way up food chains and into human fat cells. Once it has become harmless to insects and dangerous to man, the pesticide has to be promptly replaced. The replacement may be another pesticide that goes through the same cycle, and population continues to increase. Highly selected cereal strains that may become susceptible to rust after population builds on them are another source of potential instability, especially critical if the original genetic material from which they were evolved has been lost. Whether we

will continue to be capable of meeting successive emergencies is a technical matter on which a demographer as such can have no opinion. We are told that adjustments in the face of such instabilities could be violent, a hazard for human existence on the planet, as well as for demographic prediction.

Economic models are centered on the equilibrium in the productive relations of men. Systems employing negative feedback are well thought of in economics—if shoes are in short supply because a major factory has burned down, the price will go up, this will bring more shoes into the market, and then prices will come down again. The benefits of negative feedback are also appreciated in technology, as in the form of a governor that keeps a steam engine going at a constant speed, or a thermostat controlling a heating system.

But the same men who recommend and design equilibrium systems for the regulation of the economy and of machines are less aware of the need for them in natural systems. In the pursuit of short-range goals they may impatiently tear out those thermostats and governors that nature has installed. The challenge is to devise indexes and construct models that will describe and predict what is happening under such circumstances.

For two or three years now a fresh wind has been blowing over our field of study. A new debate has been initiated, with such redoubtable protagonists as Ansley J. Coale (1970) and Paul R. Ehrlich (1970). Unfamiliar names appear over articles written in journals and newspapers that before gave no attention to population. Radical differences of values underlie the several expositions.

One side wants exponential increase, if not for population then at least for the economy. Keeping the economy growing "at a sustained rate of at least 5 or 6 per cent per year, year after year (with no relapses)" (Jaffe, 1966) is a common

recommendation for avoiding unemployment and similar evils. The other side applies an S-shaped growth curve—with its implication that there is a ceiling for everything and that the ceiling is lower than one thinks. Malthus was so unimaginative that he could see only a food ceiling; the modern view sees ceilings everywhere—on minerals, water, air and elbow room. Moreover, the low ceiling will be reached very soon. One book cover reads, “The 1970’s—last chance for a future that makes ecological sense.” Time as well as space is short.

Tied to the phases of the S-shaped growth curve are personal attitudes and systems of morality. When the population was small and death came early, those who did survive to adulthood had to raise large families if the tribe was to continue, and religion and morality had to encourage childbearing. Once death rates have fallen and correspondingly low birth rates are required, morality has to reverse itself and now discourage childbearing, if equilibrium is to be attained in the new conditions.

The same reversal may well occur with respect to income. In the rising phase of industrial development, when there is much hard work to be done and only primitive capital equipment with which to do it, the prime virtues are diligence and abstinence. Today, we are told, young people are lazy and improvident; they do not work and save as we were taught to. But suppose that excessive diligence scars and bruises the earth’s surface, and that excessive abstinence now goes beyond the community’s demand for capital, as Keynes (1936) saw that it could. A morality of less diligence may well be functional for the part of the curve in which we are pushing against a resources ceiling. Dudley Duncan (1969) foresees a time when “our assessment of men will not depend so heavily on their ability to ‘get ahead.’” Perhaps excessive ambition and income will even come to seem immoral.

Many will not agree with the assessment of the physical and social changes that I have summarized in four groups here, but every observer says in his own way that we face a critical turning point. The prospect of a turning point further stimulates public curiosity about the future. Among other signs and portents, a whole industry of futurologists has arisen. Their procedures will not foretell man’s numbers and distribution; what is needed is more of the creativeness by which Whelpton devised methods for dealing with the problems of an earlier and simpler time. Our age and our discipline have their four horsemen: deterioration of the habitat, rapid population growth and its destructive interaction with income, instability of ecosystems, and changing attitudes to life and work. The more closely their coming can be foretold the less likely they are to prove apocalyptic.

The vitality of our discipline is shown in its undiscourageable effort to gather data on the past, much of it aimed at all-but-impossible prediction of the future, whose byproduct has been models through which population processes can be understood. Now is no time to stop just because some new variables have to be put into the equations.

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