

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Kingsley Davis PAA President in 1962-63



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)

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KINGSLEY DAVIS

PAA President in 1962-63 (No. 26). Interview with Jean van der Tak in Dr. Davis's office at the Hoover Institution, Stanford University, California, May 1, 1989, supplemented by corrections and additions to the original interview transcript and other materials supplied by Dr. Davis in May 1990.

CAREER HIGHLIGHTS: (Sections in quotes come from "An Attempt to Clarify Moves in Early Career," Kingsley Davis, May 1990.) Kingsley Davis was born in Tuxedo, Texas in 1908 and he grew up in Texas. He received an A.B. in English in 1930 and an M.A. in philosophy in 1932 from the University of Texas, Austin. He then went to Harvard, where he received an M.A. in sociology in 1933 and the Ph.D. in sociology in 1936. He taught sociology at Smith College in 1934-36 and at Clark University in 1936-37. From 1937 to 1944, he was Chairman of the Department of Sociology at Pennsylvania State University, although he was on leave in 1940-41 and in 1942-44.

"I came to Harvard in 1932 with a fellowship in hand (the Bromfield Rogers Memorial Fellowship). By going to meetings and seminars, I quickly got acquainted in the new setting. In my second summer in Cambridge [Massachusetts], I was an assistant to Howard Becker, a young luminary in sociology who was regularly at Smith College. When Frank Hankins, chairman of the department at Smith, decided to take a year's sabbatical, Becker knew about me and asked if I would be interested in coming to Smith for a year. I agreed to come. The next year, Howard Becker took a leave and I substituted for him, giving me another year at Smith.

"When Becker was about to return to Smith, the Great Depression had reached its nadir. Nevertheless, I found a job at Clark University--a job I took because it was close to Cambridge and therefore would allow me to keep up my contacts at Harvard. However, before the end of the year [1936-37], I accepted a position as Head of the Division [Department?] of Sociology at Penn State. I got this job because the current incumbent, Willard Waller (well-known at the time), was a good friend who believed I could do a good job at Penn State.

"I was in residence at Penn State for three years, 1937-40. I had, however, become interested in population studies and so I applied for a postdoctoral fellowship from the Social Science Research Council for further study in that field.

"During that year [1940-41], I was on leave from Penn State. [The year was spent at the University of Chicago, studying demography with Samuel Stouffer; in Puerto Rico, conducting a fertility survey; and at the Census Bureau in Washington.] The next year [1941-42], I returned to my regular job at Penn State.

"In the following year, as a result of coming to know Frank Notestein and others at Princeton, I was invited to come to Princeton for a year as a visiting research associate in the Office of Population Research, on leave from Penn State. This arrangement lasted for two years, at which time I was given an appointment to the Princeton faculty as an Associate Professor of Public Affairs, while retaining my affiliation with OPR. After these two years on leave, in 1944 I resigned from my position at Penn State. [At Princeton, after 1944, he was Research Associate at OPR from 1944 to 1948, and from 1945 to 1948, Associate Professor of Anthropology and Sociology, a department which he started.]

"I accepted a position in the Graduate Faculty of Political Science at Columbia in 1948. [At Columbia, he was Associate Professor of Sociology, 1948-52; Professor, 1952-55; and also Associate Director, 1948-49, and Director, 1949-52, of the Bureau of Applied Social Research.] I went to [the University of California at Berkeley in 1955 as Professor of Sociology. Subsequently, in 1970, my title was changed to Ford Professor of Sociology and Comparative Studies. In 1977 this position was given to me emeritus. [Also at Berkeley, he chaired the Department of Sociology, 1961-63; founded and chaired International Population and Urban Research, 1956-77; and helped establish the Department of Demography.]

"In 1977 I went to the University of Southern California [Los Angeles] as Distinguished

Professor of Sociology, a title I still hold. In 1980-81, I was a fellow, for the second time, at the Center for Advanced Study in the Behavioral Sciences [Stanford]. In 1981 I accepted a part-time appointment at the Hoover Institution [Stanford] as a Senior Research Fellow. I still hold this post as well as the position at USC."

Among Kingsley Davis's many other posts and awards relevant to demography, he was the second U.S. Representative to the United Nations Population Commission, 1954-61; he was president of the American Sociological Association in 1959 and has received ASA's Distinguished Career Award; he received the Irene Taeuber Award for Distinguished Research in Demography from PAA in 1979; and he was the first sociologist or demographer elected to the National Academy of Sciences, in 1966.

Throughout his career, starting in the mid-1930s, Kingsley Davis has produced a prodigious number of influential books, articles, book chapters, and conference papers on fertility, migration, urbanization, and practically all areas of demography. William Petersen has said that he was "a pioneer" in establishing the field of social demography (Petersen, "Kingsley Davis," International Encyclopedia of the Social Sciences, Biographical Supplement, Vol. 18, 1979, p. 139).

Kingsley Davis died in Stanford, California, in 1997.

VDT: How did you first become interested in demography?

DAVIS: I got my Ph.D. in sociology at Harvard, studying mainly with Talcott Parsons. My dissertation was on comparative kinship systems. I did it under Lloyd Warner, who was at Harvard then. Just before I got my Ph.D., he left and went to Chicago, but I ended up writing the dissertation on the subject that he and I had agreed on. I published an article with Lloyd Warner in the American Anthropologist on my dissertation ["Structural Analysis of Kinship," American Anthropologist, April-June 1937]. So I was interested in comparative research, especially on social structure, before I got interested in population.

As you know, at the time there were interesting population policies, especially pronatalist policies, in Europe. My feeling was that these policies were naive when it came to the sociology of the family; they didn't know what they were doing. So I decided to publish an article and set them right. I wrote a critique, which was published in a British journal ["Reproductive Institutions and the Pressure for Population," Sociological Review, July 1937].

In doing that study, I read a lot of demography, especially on fertility, and I thought, "Well, this is an interesting field." It was concrete; one could get his hands on the phenomenon; it wasn't all verbal theory. I continued to read in the field and to get acquainted with what was going on. When I went to Penn State as chairman of sociology, I could put in any course I wanted. I put in a course on population so I could learn the subject. And I did more reading and gradually became a demographer.

In 1940 I received a Social Science Research Council fellowship for postdoctoral study in the field so I could learn demographic techniques that I didn't know before. I spent that year partly at the University of Chicago with Samuel Stouffer. It was somewhat a mutual education, because Stouffer didn't really know much demography. He was more of a statistician; very interested in methodology.

Then I went to Puerto Rico because I wanted to get to know demography in a nonindustrial country. I spent that time interviewing people in the countryside [the region surrounding the mountain town of Lares]. I did a survey, but like other people I found that my cases became too few when I put the characteristics in.

VDT: When you wanted to analyze the data?

DAVIS: Yes. I was looking at fertility and why their fertility was so high, as it was at that time. But it was mostly practice; I was learning in the process.

VDT: Did you set that survey up on your own?

DAVIS: Yes. At first I had a very capable woman, a Puerto Rican nurse, helping me, but I got to where I could do the interviewing too. I had started learning Spanish some time before I went to Puerto Rico, knowing that I was going there. I came back from Puerto Rico and went to the Census Bureau for four or five months to see how they were doing on the 1940 census.

VDT: That was the time that the "class of 1940," as they called them, came in--Paul Glick, Henry Shryock . . .

DAVIS: Right, they were all there.

VDT: So, all that really plugged you into demography?

DAVIS: Yes.

VDT: Then you went back to Penn State for a year, and then what took you to Princeton?

DAVIS: The Office of Population Research. At a meeting of the PAA in Chapel Hill [1940], I got acquainted with Frank Notestein. He thought I looked like pretty good material. He had a contract with the State Department at the time to do special studies of particular areas. He and I talked and he seemed interested in my thoughts and ideas on Latin America. So he gave me a position at the Office of Population Research, mainly to do research on Latin America.

VDT: It was not thought at that time that you would do India?

DAVIS: No, although it wasn't long before India was included in my portfolio. [Davis's OPR research on India, culminating in The Population of India and Pakistan, 1951, was funded by OPR's contract with the Office of the Geographer of the Department of State, which began about 1945, after World War II ended and after OPR had produced four important books on the population of Europe for the League of Nations. The State Department contract was "to extend its (OPR's) work to Asia." Frank Notestein, "Demography in the United States: A Partial Account of the Development of the Field," Population and Development Review, December 1982, pp. 664-665.]

VDT: How did you first become interested in India?

DAVIS: I was always teaching sociology and I was also working with this SSRC fellowship. I'd been interested in social stratification, a very important topic in American sociology. I did some work with Wilbert Moore on the theory of stratification. We did a much-debated article on that ["Some Principles of Stratification," American Sociological Review, April 1945].

When you're dealing with social stratification empirically, you can hardly ignore India, so I studied the caste system. And when it was time at the Office of Population Research to assign different areas to different people, I more or less staked a claim on India. I'm glad I did, because it proved to be a very interesting case, not only of stratification but of population as well.

VDT: So that's how you came to write the famous book, The Population of India and Pakistan [Princeton University Press, 1951]. Did you go to India?

DAVIS: Only after the book was published. I wanted to go see whether what I said was correct.

VDT: But how did you collect all the data; was it shipped over to you?

DAVIS: That's one of the interesting things about India. For its stage of development, it has better census data than any other country in the world. I wore out the census volumes borrowed from the Harvard library, especially the 1931 census; I practically wore it out.

VDT: But you also put in all the sociological insights and perspectives that nobody had done before.

DAVIS: Yes, it wasn't just all statistics.

VDT: Indeed, that was the wonderful thing about it. That book, as Ansley Coale among others has said, was a monumental book. It's still being reprinted; still a seller as a reference on the population of India. That book influenced the start of the Indian family planning program in 1952.

DAVIS: Yes. When I finally went to India, in 1951 or 52, I visited the Commission or Office on Economic Planning. They were planning a lot of things at that time, with a new government coming in after Independence [in 1947]. The Gandhi party had been concerned about population, believed in population policy. When I went into the office they took me into the seminar room, a big room with a big oval table, and I saw two or three copies of my book, dog-eared; they'd obviously been read. And we talked about population policy. They asked me, very seriously, if they started birth control in India--that was the term they used; "family planning" hadn't come in--would the population show the same immorality that Kinsey showed in American women?

VDT: They had seen the Kinsey report?

DAVIS: Yes, they're a literate people; they read a lot. So they were really concerned. I had to think fast. I told them that surely they did not think that the conduct of Indian women was based on ignorance.

VDT: Very good! In your book, you recommended that India should have a sustained and vigorous birth control program, along with emigration and rapid industrialization. You didn't really believe at that time that the government would take you up on it, but they did.

DAVIS: Right.

VDT: Not too vigorously at first.

DAVIS: Not vigorously. For years they worked on only one birth control means--rhythm.

VDT: Also in the book, you pointed out that India's population at the time of Independence in 1947 was 420 million and growing at 1.2 percent a year. If that rate continued, the population would double to 840 million in 2005, which you said would be a catastrophe. According to the Population Reference Bureau's World Population Data Sheet, India's population is estimated at 835 million this year, 1989, growing at 2.2 percent a year. Do you think that's a catastrophe?

DAVIS: Catastrophe all the way round.

VDT: They didn't listen hard enough to you.

DAVIS: No. I made the contrast with South Korea, Hong Kong, Singapore, Japan . . .

VDT: And China.

DAVIS: Yes, they've done a lot.

VDT: At Princeton, simultaneously with the India-Pakistan book, you also wrote Human Society [1949].

DAVIS: Yes. Well, I went to Princeton to do research at the Office of Population Research. Then they appointed me to the faculty at Princeton, in the Woodrow Wilson School of Public Affairs, made me an associate professor and asked me to do some teaching. I was to start the sociology and anthropology department.

VDT: I had the impression that Notestein really brought you on, as a sociologist, for that reason; that you could start the department.

DAVIS: It was a possibility. I got Wilbert Moore there, who had been at Penn State with me, Paul Hatt, and Marion Levy. [Also Melvin Tumin, Edward Devereaux, and Harry Bredemeier--all "men who soon made the department one of America's best." Petersen, op. cit., p. 140.]

VDT: All top names. About Human Society, John Weeks, one of your Berkeley students whom I happened to talk to recently, considers it your greatest book.

DAVIS: Really! I'll tell a bit of the background of that. There was one course in population at Princeton and Notestein taught that, so all my teaching was in other aspects of sociology. We had a pretty big introductory course in sociology. The students rated the courses--I guess they still do--and we got a mediocre rating. We decided we were going to get that course up in rating. Harry Bredemeier, an instructor, and I carefully prepared every lecture, sometimes dramatizing it and pretending it was a radio broadcast play or whatnot. We had precepts at Princeton, breaking down the courses into discussion groups. The students became so enthusiastic that they wanted to have more precept hours than were scheduled. The course came out at the end with a very high rating.

We discovered that the best material was the material we wrote ourselves. Human Society was written to improve that course at Princeton. That was completely in addition to the research at the Office of Population Research.

VDT: You were doing the two simultaneously. Human Society inspired others and it has remained a textbook in the field. Besides those we've mentioned, what other colleagues do you recall at Princeton?

DAVIS: Dudley Kirk was there and Clyde Kiser. Kiser lived in Princeton and worked simultaneously in Princeton and at the Milbank Memorial Fund in New York. He worked mainly on American demography, but I didn't follow his work too well, so I didn't know too much about it. I also remember Ansley Coale.

VDT: Also at the time you were at Princeton, in the mid-1940s, you helped frame the demographic transition theory, which, of course, several people were working on at that time. Were you one of the

first to use that expression, the "demographic transition"?

DAVIS: I edited a volume of The Annals of the American Academy of Political and Social Science on world population [Vol. 237, January 1945]. I called it "World Population in Transition." And the title of my paper, which I put first in the volume because it covered the theory, was "The World Demographic Transition."

VDT: So probably you were one of the first to use that term--well, everybody knows that.

DAVIS: I was a young man who got acquainted quickly in the field. I went to all the meetings; published a lot.

VDT: You did. Well, you've been publishing a lot ever since.

DAVIS: Had a lot of fun. So, they wanted to do an issue of the Annals on population and they thought of me, although I was not at the time a senior person.

VDT: No, you weren't. That was quite an accolade, to ask you to edit that volume.

DAVIS: I think Notestein was a little miffed. I had gotten well acquainted with the editor of the Annals, Thornton Sellin, a criminologist, so he thought of me to edit the issue on population. Later I edited another issue for him, this one on Latin America ["A Crowding Hemisphere: Population Change in the Americas," Annals, Vol. 316, March 1958].

VDT: In that first Annals article, you were espousing the demographic transition theory in its classic form--that socioeconomic change is necessary for fertility to decline, following mortality decline. However, I was interested in following your change in view in an article by Dennis Hodgson, "Demography as Social Science and Policy Science," in Population and Development Review, March 1983. He pointed out that by the mid-1950s, you were urging a direct family planning program for India, without waiting for socioeconomic change.

DAVIS: Yes.

VDT: You wrote: "The planned diffusion of fertility control in peasant populations prior to, and for the benefit of, the urban-industrial transition is a viable (and humane) policy for a country like India."

Then you shifted back, in 1967, in your provocative article in Science [November 10, 1967], "Population Policy: Will Current Programs Succeed?" You argued that family planning programs as they were then structured couldn't do the job alone because they stressed fertility desires and those desires were too high--four children and more--to bring fertility down to replacement level, and that there had to be socioeconomic change to change motivation before there could be a deliberate reduction in fertility. You went on to outline some policies to change motivation: control illegitimacy and encourage late marriage and low fertility within marriage.

As I understand it, you started off first with classic demographic transition: socioeconomic change has to be first. Then you were saying in the mid-1950s--probably wanting to push India into more direct family planning programs--that fertility could be changed without waiting for socioeconomic change. Then by the mid-1960s, you were back to saying there had to be socioeconomic change first. Do you still stand by that?

DAVIS: I think maybe there's some misconception.* [See Davis footnote of May 1990 on the

Demographic Transition, below.] It was never my thesis that you could change reproductive patterns simply by offering devices. The very emphasis on the institutional side of things was to show that reproductive motivation is intertwined with the rest of the social system. You can't ignore these intertwinings.

Now, in policy you've got two things always. You've got what's necessary from a scientific point of view, which is cause and effect. And you've got what's not necessary but may be desired nevertheless. Economic development is too comprehensive to be thought of as an instrument for population control. All along, I have consistently argued that population policy should try to change those aspects of the institutional structure that support high fertility. The U.S. has never taken that suggestion seriously, but Singapore, Taiwan, and Korea have.

VDT: You mean things like encouraging later marriage?

DAVIS: Yes. To be sure, the age at marriage has been going up [in India], but it's a slow process. What has been happening is difficult to ascertain, because India doesn't have a vital statistics system and depends on censuses for data on marriage.

It's a false antithesis to say that population control depends on total development or that it can just be done by family planning. It doesn't make sense either way. Obviously, if one is going to bring fertility down, one must have the means to do so, but having the means will not do it alone. There's no opposition. I've never been against distributing contraceptives, and I've never talked about transforming fertility by simply furnishing them contraceptives. And I think the Indian case illustrates the truth of that.

VDT: That they've stressed just distributing the devices without trying to change the institutions?

DAVIS: Yes, they didn't do much. Fertility has dropped a little in India, but amazingly little since 1952. The fact is they have added hundreds of millions of people while carrying on what they considered to be a population policy. In those terms, the policy has to be judged an utter failure. To characterize it by the perspective of the world, the failure to control population is the greatest tragedy that ever hit humanity.

VDT: You mean in general the countries that have failed to do so?

DAVIS: Yes.

VDT: Of which India is the largest. But think of China, the means they have used to bring down their fertility rate.

DAVIS: They have taken seriously the motivation side.

VDT: Motivation? Well, coercion, perhaps.

DAVIS: You can't have control unless you use incentives and disincentives. And keep records.

VDT: Keep records? In this country?

DAVIS: Yes. It isn't backward countries alone that have a hard time dealing with the population problem.

VDT: You really have said both approaches are necessary. An article of yours that I like very much was your review of the World Fertility Survey in Sociological Forum in 1987 ["The World's Most Expensive Survey," Sociological Forum, Fall 1987]. You criticized the WFS as a waste of money. But aside from that, you charged that family planners were still avoiding the issue of socioeconomic change, or as you said, changing the institutions that make for high fertility. You wrote that "the use of birth control is a necessary condition for fertility decline, but so is social modernization." So, you've really made that point well.

***Davis footnote on the Demographic Transition, May 1990.**

The essence of the Demographic Transition [theory] is that it is a dynamic model, a model of a process of change. This process has changed over the decades, as the name implies, but it has always accompanied the Industrial Revolution, and has never not accompanied that change.

I was not responsible for the term, but I believe I was instrumental in its becoming a standard part of the demographic vocabulary. A competing term was Vital Revolution, recommended by Norman Himes, but it never caught on, whereas "The Demographic Transition" did catch on.

At first the term was applied to the "classic" cases--the proven cases of European countries and European countries overseas--because these were the only countries that had completed the fertility change. Japan was the first non-European country to go through the transition, but Korea, Singapore, Hong Kong, and Taiwan have followed closely. "Progress" had been a term used long in advance of the Demographic Transition.

The demographic transition is not a scientific law, but an extremely useful empirical regularity. It is useful precisely because it relates several variables in a system and because it is not an equilibrium model. It can be expected that as latecomers enter the process, the exact pattern will be altered. I have emphasized the greater speed of the transition now compared with the past (see Kingsley Davis, "Population and Resources: Fact and Interpretation," forthcoming, August 1990, in a special supplement to Population and Development Review). This has meant a faster growth of population during the transition.

One problem is that people view the demographic transition in a mechanical way, as if it were a scientific law. If there were such a law, it could not tolerate an exception. The whole thing would have to be thrown out. An empirical generalization, however, can tolerate some exceptions and still be valuable in analysis.

One way of avoiding the mechanical approach is to look at it in motivational terms. This is what I tried to do in the paper on "Change and Response" ("The Theory of Change and Response in Modern Demographic History," PAA presidential address, Population Index, October 1963). What did it mean to the individual to be in a situation where families were larger than they had been in the past? Most of the people were farmers. What did it mean on the farm to have more children to support on limited land? How did people respond? My belief is that the new regime viewed the demographic change as catastrophic. All responses were utilized to some extent (this is where the "intermediate variables" come in). Celibacy, late marriage, abortion, infanticide, migration, birth control within marriage--all were used as escapes from large families. The value of children tended to be less than their cost. The birth rate, and especially the replacement rate, began to fall.

Thus, the demographic transition was not something spontaneous, having nothing to do with motives. As I see it, and as I tried to stress when writing about it, it had to do with efforts to preserve or enhance social status in a newly emerging industrial society. It was the product of a very human process of change. It is not a predictive instrument. One cannot say that country A is two-thirds into the demographic transition and thus its population must be growing a certain percent each year.

VDT: Let's get back to your career. What took you to Columbia in 1948?

DAVIS: Mainly a chance to teach demography. Notestein was giving the only course at Princeton; there I was teaching something else besides population. By going to Columbia, I could teach the graduate course in population and would have more students--which I did; a lot more students. There was also more independence to do what I wanted to do. But the trouble was, I hated New York City.

VDT: New York City--even then?

DAVIS: Even then.

VDT: Some of your students at Columbia--I've heard or you've mentioned--were Andy Lunde, William Petersen, Art Campbell, Sam Baum, Gwendolyn Johnson. Can you remember others? You've said that when you left Columbia [in 1955] you gave Lazarsfeld a list of 14 or 15 students that you had at that time.

DAVIS: I can't remember all of them, but others included Judith Blake, Paul Jacobson, Monroe Lerner, Joe Stycos, Lincoln Day, Robert Parke, Jeanne Clare Ridley, and Eduardo Arriaga [Arriaga was Davis's student later at Berkeley, see below]. Lincoln Day and Alice Day met in my course.

VDT: A bit of romance! I know about that; I've interviewed Link for this series too. Some of the students you had may have been more mature, such as Andy Lunde and Art Campbell, who came back from the war.

DAVIS: Yes, some of them were a little older.

VDT: That department of sociology at Columbia, postwar, was very influential, very special. Why was that?

DAVIS: It wasn't a big department, fairly small. It was forward-thinking. They had Paul Lazarsfeld emphasizing methods, Robert Merton emphasizing theory, and Bob Lynd to carry the radical left. It was a pretty good setup.

VDT: It must have been a stimulating department; many people have talked about it.

DAVIS: I learned to do applied social research at the Bureau of Applied Social Research [at Columbia]. I came to the office one morning, talked to one of my aides about how things stood, and found we had 80 some people doing research.

VDT: All sociologists?

DAVIS: No, they were from economics and other fields too--doing contract research of one kind or another. I said to myself, "Why am I doing this?" And straightway decided to get Charles Glock to take over.

VDT: You didn't care for the administrative work?

DAVIS: I don't like contract research, especially survey research.

VDT: Have you avoided that in your career?

DAVIS: Oh yes, pretty much.

VDT: Why don't you like contract research?

DAVIS: I like the kind of problems you set yourself rather than somebody else's. That's the definition of applied research: you take somebody else's goal and try to figure out how to reach it. I did not want to do that.

VDT: Have you always been successful in getting funding to do the research you defined?

DAVIS: I've not had much trouble getting adequate support over the years, in one way or another.

VDT: You left Columbia. What took you to Berkeley?

DAVIS: More money, California, climate.

VDT: How did you come to establish International Population and Urban Research?

DAVIS: I wanted to do comparative research on urbanization and applied for the finances to do that from the Ford Foundation and they gave me most of what I asked for. We had to have an office, so we got one in the Institute of International Studies at the university. I made other proposals and got more money for other aspects of research and started publishing. A lot of the people who worked in the place were graduate students and also were doing their dissertations with me. After they got their degrees, they often published a book. There were 18 volumes in that series.

VDT: The famous series of orange Population Monographs, that went all over the world.

DAVIS: That's right.

VDT: I recently happened to pick up one by Jogindar Kumar on Population and Land in World Agriculture [1973]; obviously it had been his dissertation. He was very effusive in his thanks to you for the extra encouragement you gave him. You must have done that with all your students.

DAVIS: I thought that was a good problem. I said, "It certainly pays you to work on it; get it done." I did that with Andy Collver. He didn't get his dissertation done as fast as I thought he could, so I told him one day that I'd give him only so much more time; he'd have to get the thing done. And so he did. He published a very good book in the series. It was on birth rates in Latin American countries, using mainly censuses.

VDT: Which came first, the dissertation topics and then the promise to put them in the series, or did you suggest the topics to them?

DAVIS: Nobody had a promise to get into the series.

VDT: But you did see the series as in part a vehicle for these dissertations?

DAVIS: Not everybody at Berkeley wrote their dissertation with me. Kumar was a very good student;

very well trained in statistics.

VDT: Eduardo Arriago was in the series. You have mentioned that you'd met him at a Milbank Memorial Fund meeting.

DAVIS: Yes, I forget what the meeting was on; it wasn't a very big one. I'd been looking for somebody to cover the Latin American field, because I had some money for that. I asked somebody who had been working on Latin America and he told me, "There's this fellow named Arriago from Argentina; smartest guy." He'd worked in Argentina some; got his first degree in Argentina. So he was at this meeting. I talked with him in Spanish; he didn't know English. You could see right off the bat that he was a very smart guy. I think he was working for the Pan American Union. When I described the job at Berkeley, he decided to come.

VDT: Had he done his Ph.D. by that time?

DAVIS: No, he did it under me. It was in the series--two volumes on mortality.

VDT: Right, those books are well known. Just after you arrived at Berkeley, you and Judith Blake published your landmark article, "Social Structure and Fertility: An Analytic Framework" [Economic Development and Cultural Change, April 1956], on intermediate variables, which has set the direction of fertility research ever since. John Bongaarts honed it into his proximate determinants. It really was the framework for the World Fertility Survey--much as you criticize the World Fertility Survey--and for the National Survey of Family Growth; they collect their data to fit into that framework. Are you pleased about that?

DAVIS: Oh yes, sure.

VDT: It's one of the great pieces in demography. How did you come to write that?

DAVIS: I guess as a method to get things organized. You see, much of social science is published when you don't see clearly what the problem is. We published it; Joe Stycos, I think, was about to publish something similar. He was one of my students both at Princeton and Columbia.

VDT: Some people have criticized the WFS--you did it too in part--for just collecting the intermediate variable data, forgetting the sociocultural background, which you had meant to emphasize also in that article.

DAVIS: Yes. Well, presumably it helped in sorting things out.

VDT: It did indeed. Now let's talk about Berkeley's department of demography which you helped to establish--the first and still almost the only department of demography in the U.S. It lasted from 1967 to 1972. Is it important to have a separate department of demography?

DAVIS: It turned out to be a very good basis for graduate education, because if you take the various elements in demography and push the analysis, you get into economics, sociology, and other disciplines. Take something like epidemiology. A person getting a Ph.D. in sociology would not think of taking epidemiology, but in the department of demography it's required. The epidemiology graduate studies causes of death; demography studies mortality, including causes of death. So, as a department, I think it was very successful. If you want, I can try to find in the files a copy of the proposal for the

department which we presented to the administration of the university. A very good idea.

VDT: And California did buy the idea?

DAVIS: Oh yes.

VDT: Way ahead of its time?

DAVIS: Yes. And we were the victim of the student revolt.

VDT: In the late 1960s? You feel that was the problem that happened?

DAVIS: There was so much anger; things were just terrible. Never knew what was going to happen next. Students could go on a rampage and break \$250,000 worth of windows in 30 minutes.

VDT: You feel that brought down the department?

DAVIS: Well, funds were very scarce. Bowker came in as chancellor and this new department of demography was vulnerable. He could do away with that and use the money somewhere else. And, of course, like many innovations, some of the people you'd expect to be most for it were against it.

VDT: Like whom?

DAVIS: Like a lot of people in the PAA.

VDT: Why?

DAVIS: It wasn't the way they'd done things. They all had PhDs in some other field.

VDT: And felt that was the way things had to be done?

DAVIS: I think there was some fear among people who would be trained in the field [of demography] that their degrees would be devalued. In a department, you can get a sequence of courses. If you need more faculty, you can appoint them. We had a Group in Demography [prior to forming the department] and we had a terrible task of persuading departments to make appointments that were relevant to demography but also relevant to the department. We tried to get Paul Demeny, who was at Michigan, but nobody in the economics department had ever heard of Paul Demeny. Everything he'd published had been in demography; he hadn't done anything the economists would recognize as economics. So, we didn't get him. Things are difficult when you don't have a separate department of demography.

VDT: To get established and accepted by others in other disciplines?

DAVIS: Yes. And you get good training when you have a department, because you can organize the courses so as to make one thing dependent upon the next. When you get a Ph.D. in some other field, maybe you get two courses in demography, but not much more. So, I think there's quite a rationale for a separate department. It's coming; it's on its way. Princeton now has a Ph.D. in demography.

VDT: I guess you were a little ahead of your time. It's taken people a long time to come around to

your way of thinking. A department of demography seems logical.

DAVIS: Better late than never.

VDT: Can you tell me a bit about your time with the UN Population Commission from 1954 to 1961. You were the second U.S. Representative, after Phil Hauser and before Ansley Coale. Later they had people like General Draper and Phil Claxton who were not demographers. Does that time stand out in your memory?

DAVIS: It was a busy time.

VDT: How often did you meet? Did you have to go to New York several times a year?

DAVIS: Once a year.

VDT: Did you find that the Commission was rather politicized? It was set up to advise on the program of research of the Population Division. Family planning was pretty much a taboo topic then. Did you find that?

DAVIS: No, not really. We didn't call it family planning; we called it birth control-- Margaret Sanger's old term. And there was remarkably little politicization. It was the usual mumbo-jumbo about population being a function of economic development. But on the whole, the Commission stuck relatively well to scientific study until the family planning movement took over.

VDT: And that was after your time. The U.S. got into funding of family planning only in the late 1960s, after your time. Could you tell something about your work at the University of Southern California and at the Hoover Institution. Do you still divide your time between the two?

DAVIS: Yes.

VDT: What happens? You teach down at the University of Southern California [in Los Angeles] for a semester?

DAVIS: I have an appointment as Distinguished Professor there. It's only part-time. It was full-time at first, but I couldn't take the climate. My bronchitis was much worse there.

VDT: From the smog?

DAVIS: Yes. A whole generation of children are affected; getting worse all the time. I had an invitation to return here [Stanford] for a year [1980-81] at the Center for Advanced Study in the Behavioral Sciences, so I came. I had been there for a year before [1956-57], just after I came to Berkeley. So, I cut down the time I spent at USC to, at most, a graduate seminar in the fall semester.

VDT: Then here at Hoover is your headquarters as a researcher? In recent years, you have organized conferences and edited the proceedings. There was the one on marriage, in 1982; Contemporary Marriage was the report that came out of that, in 1985. There was low fertility, in 1985, and the wonderful book that came out of that, Below-Replacement Fertility in Industrial Societies, in 1987. And you're going to do the same with the latest conference you've organized, in January this year [1989], on population, resources, and the environment?

DAVIS: I already have a reader, a book we got out to serve as a background.

VDT: Is that the Teitelbaum-edited one? No, it's still another set of papers [Kingsley Davis, Mikhail S. Bernstam and Helen M. Sellers, eds., Population and Resources in a Changing World: Current Readings, 1989].

DAVIS: I have a paper on migration in the Teitelbaum volume ["Social Science Approaches to International Migration", in Michael S. Teitelbaum and Jay M. Winter, eds., Population and Resources in Western Intellectual Traditions, 1989].

VDT: These publications--how do you manage to accomplish all these things? It seems a good field for you to be in, collecting together these experts on different topics.

DAVIS: On marriage, I had some ideas on it and thought I'd get some others interested. Somehow the proposal I wrote wound up in the hands of Gardner Lindzey and he said, "Come up to the Center [for Advanced Study in the Behavioral Sciences] to work on that." So I did that. Shortly after I got here [Stanford], Hoover asked if I'd be interested in being a research fellow after my year at the Center. I said yes, so I've been here ever since.

The subject of below-replacement fertility seemed to cry out for some kind of investigation, so Mikhail Bernstam and I brought together a conference. I've always been interested in population and resources, but never published much on it. That's a good field too. There's a lot happening, but the writings on it are scarce. A lot of natural scientists have peculiar notions about human demography. So I organized a conference.

VDT: This is all an example of how you've always moved with the times. We've talked of how--well, you've explained that your views on the demographic transition never really did shift. You have shifted from concerns about rapid population growth in less developed countries to concerns about low fertility in developed countries and now to population, resources, and the environment. So you have really moved with the times, through the years. [Petersen in "Kingsley Davis," op. cit., p. 140, writes: "Nearly two decades before ecology became a fad, Davis was devoting a tenth of the assigned reading in his basic course to a highly sophisticated treatment of ecological relations as these affect the quality of life."]

And you have been flexible in your views--for instance, on the family. In your 1967 article, "Population Policy: Will Current Programs Succeed?", and in the article you wrote for the Commission on Population Growth and the American Future, in 1973 ["The American Family in Relation to Demographic Change," Research Reports, Vol. I, Demographic and Social Aspects of Population Growth], you suggested downgrading the family as one means to reducing fertility, because the familial institution can be a block to reducing fertility. Then in a chapter on "A Theory of Teenage Pregnancy in the U.S.," published in the 1980 monograph by Chilman, Adolescent Pregnancy and Childbearing, you urged a rehabilitation of the family to reduce teenage fertility in the U.S. So, I think you shifted your views there: first the family should be downgraded in importance and then it should be upgraded, certainly in the U.S. Were you the first to point out how much higher teenage fertility is in the U.S. than in other industrial countries? Of course, Charlie Westoff went on to point that out and then the Alan Guttmacher Institute researchers, under Elise Jones, did that excellent project and book on that. You suddenly noticed that U.S. teenage fertility was so much higher than that of other developed countries?

DAVIS: I can't remember.

VDT: Well, you were early on with that.

DAVIS: It's true, I think. It depends a bit on how you measure fertility.

VDT: The U.S. has very high teenage abortion rates, added to fertility. It was actually pregnancy rates that the Alan Guttmacher people were looking at. Who have been the major influences in your career? You've mentioned already Notestein, and in your 1979 interview [with Anders Lunde and Abbott Ferriss for the PAA oral history project; the interview covered little of Davis's career and the tape is defective], you mentioned Lotka, who gave the presidential address at the 1939 PAA meeting, the first you attended.

DAVIS: Made a powerful impression on me.

VDT: You already were into demography; you went to that PAA meeting [in Washington, DC] because of that.

DAVIS: I was getting into it.

VDT: Can you think back on some other of your early influences?

DAVIS: Well, on the sociological side, Talcott Parsons. And Lloyd Warner, in anthropology. I was never much of a disciple. There are a number of schools of thought. In sociology, there's quite an interest now in the origins of functionalism and about the history of functionalism, interpretation. This doesn't please me much, because it means you get tagged and then everything is fitted into that, regardless of whether it is relevant or not. But I learned a good bit from Parsons on structural functionalism. And I learned a lot from the readings that these people gave me to do. They started me off and that continued to be a long interest. Some theories I was quite interested in but never published much about them. I've got filing cabinets full of things I never published.

VDT: Can you give me an example of some field that's interested you but in which you never published?

DAVIS: When I went to Harvard there was a course organized by William Morton Wheeler, a great expert on ant society and insect society generally. Fascinating. I continued to read in this area but never published anything on it, except in a recent symposium. I wish I had had a chance to work more in the field of human evolution. Washburn at Berkeley has been an inspiration; he was with me at the Center for Advanced Study in the Behavioral Sciences.

VDT: Perhaps that will be your next topic. Of course, you are cited by practically everyone I have interviewed in this series--and this is the 26th interview--as a leading influence on them, whether or not they were your students or colleagues.

DAVIS: I appreciate that.

VDT: Nathan Keyfitz, for instance; he taped an interview on his own in Jakarta. He said: "Davis is one of my heroes." And some people who merely read your publications were greatly influenced by them. And, of course, many outstanding U.S. demographers have been your students. It's probably not fair to ask but who do you think of as some of your leading students? You've already mentioned some at Columbia and Arriaga and Jogindar Kumar at Berkeley.

DAVIS: It's hard to say one is more outstanding than another; they do different things. Some of them have a lot of publications; others are administrators. If you look at the occupational structure of the people who've studied with me, it's not characteristic of the whole field. Some of them have gone into administrative work, like Andy Lunde, some into research. It's somewhat unpredictable what they're going to do. But some other names, not yet mentioned, include Harriet Presser, Ruth Dixon-Mueller, Jerry Rale, Woody Carlson, Don Hernandez, Nelly van den Oever.

VDT: You must have used your graduate students and assistants very well to have produced as much as you have.

DAVIS: Well, I employed my graduate students.

VDT: And you've always given them credit.

DAVIS: Yes, my intention was always to give them credit for what they've done. I don't like publications in which the first name on the list is the guy who's never seen the manuscript at all. That gets into some very difficult situations. We have many cases like that--a lot of fraud and bitterness over allocation of credits. That's worked out pretty well. Someone like Jogindar Kumar, for example. He put an awful lot of work into that dissertation; I put a little bit.

VDT: You didn't put your name on that book.

DAVIS: No.

VDT: Let's talk about your publications. You say you don't know how many you have, but here's a list. It runs on page after page.

DAVIS: There's some repetition there.

VDT: What do you consider among your most important publications and why? Well, The Population of India and Pakistan, to begin with.

DAVIS: And Human Society.

VDT: What stands out in your mind?

DAVIS: I feel that in some cases I was sufficiently ahead of events. Those publications deserve credit; they changed views. I think I might have been more courageous than some demographers.

VDT: Indeed, you have been.

DAVIS: I didn't go along with family planning ideology.

VDT: So you would regard the 1967 Science article, "Population Policy: Will Current Programs Succeed?", as a sort of landmark?

DAVIS: It was only an example of being first with something or other--in this case, with a critique. There have been other critiques that confirm mine.

VDT: It was very controversial at the time.

DAVIS: Yes, it was, but I went ahead and did it. And my PAA presidential address, "The Theory of Change and Response in Modern Demographic History" [1963]. It is one of the things I'm most proud of, as a theory.

VDT: Multiphasic response theory. I mentioned recently to Harriet Presser that I'd be interviewing you and she said, "Oh, I always have his classic article on multiphasic response on the course agenda for my students, except it should have been `multi-faceted.'"

DAVIS: Right.

VDT: That was an important article, indeed. My own copy from my student days is so marked up. That and, of course, the intermediate variables article you would put up there?

DAVIS: Yes. And the demographic transition article [in "World Population in Transition," Annals, 1945]. Actually, the "Change and Response" article can be viewed as a further discussion of the demographic transition, although in "Change and Response" I didn't use the term.

VDT: No, you didn't.

DAVIS: What I did in the original treatment of the demographic transition was to try to understand why the different elements behaved as they did. I think the demographic transition has often been treated as a kind of statistical pattern, but I tried to think why there is a pattern. In the "Change and Response" paper I stressed the range of possible means. I tried to avoid the extreme concentration on family planning and birth control. My sociological training had taught me that so far as goals are concerned, collective goals are different from individual goals. Family planners confuse individual motivation with goals for the society at large, but there's no society with a brain. The one brain, the only goals, are individual goals. It's a very interesting question, how human beings reach collective goals. There's no collectivity that thinks; just individuals who think. This is the basic problem of human organization: how the two get together.

VDT: How you get from individual goals to goals for the collectivity?

DAVIS: Yes. What Margaret Sanger was talking about in birth control was not population control. She had no conception of a national population policy. She was thinking in terms of individuals: Give them the means, contraception, and they'd have the number of children they want. But what people want for themselves is not necessarily what the society should have.

VDT: That's what you were bringing out in the 1967 article.

DAVIS: Trying to.

VDT: You write so well; what explains that? You had English as your undergraduate major and philosophy as your next choice [for the first master's degree].

DAVIS: Perhaps it's partly biological. In the third grade, we had to do essays, had to do some original writing. The teacher wouldn't believe that I'd written my story. I don't think it's purely a matter of

training, although almost anyone can learn to write if he tries hard enough.

VDT: You were born with the gift of expressing yourself?

DAVIS: I have several relatives who are good writers. And I did a major in English, which was reinforcement. Because they thought I wrote pretty well, people encouraged me to write more. And so I got a good deal of practice and that encouraged me to think that English was what I should study in college.

When I was in junior high, I had a teacher, Peter Madrey, who gave us 31 rules of composition. We had to memorize these rules and use them in writing stories, and had to give the numbers for every punctuation mark. We had to say why that punctuation was used. It was wonderful training. Peter Madrey didn't stay long in junior high teaching; he became a salesman for a big publishing firm in Dallas.

VDT: He must have been a wonderful teacher, since you still remember his name. You've always written for the popular press, for instance, your New York Times op-ed piece last October [October 18, 1988], "Our Idle Retirees Drag Down the Economy." The demographic establishment has sometimes criticized you for that.

DAVIS: Really? I hadn't realized that this was a basis for criticism.

VDT: In some ways--perhaps not now, but in the early days. Do you think it important that demographers, sociologists, academics talk to policymakers? Obviously you do, because you've written such articles as the 1967 population policy piece.

DAVIS: I think demographers should write as clearly as possible to get things out and in circulation. Why do research in demography to hide it in technical journals only? I think demographers have an obligation to get their findings as widely known as possible. If they get findings, the public should know about it.

VDT: Is that, by the way, one of the reasons that you supported the Population Reference Bureau at a time when it wasn't so respectable? [PRB vice president, 1952-55; trustee, 1952-70.]

DAVIS: Yes, sure--in terms of maximum publicity for new findings.

VDT: You've certainly contributed to that, having such interesting things to say, such things as your contribution to the September 1974 issue of Scientific American ["The Migrations of Human Populations"]. That was a great issue [The Human Population]. And you've always been something of an action demographer. In Hodgson's 1983 Population and Development Review article, he described how you shifted from being a social scientist to an action demographer, like Notestein, in the 1950s. But I think you've always had something of an action demographer in you.

DAVIS: That's a peculiar way of describing it.

VDT: In your 1979 interview, you said you felt--just as you say now--that academics and demographers have an obligation to get their research out. But how about the activists?

DAVIS: If they have funds for research, they have an obligation to defend their results, get them out. Give people back their money's worth.

VDT: That's a good way to put it. But what about trying to influence policy, as you did in your 1967 article? In your 1979 interview, you said you felt that the academic demographer-sociologist should not try to influence policy goals, but if there are goals, their role is to point out how to achieve them, demographically.*

***Davis footnote on "Influencing Policy," May 1990.**

There seems to be some confusion on the subject of demography and its influence on policy. I have said that scientists, no matter what their field, have an obligation to bring their scientific findings to bear on important issues, if the findings are relevant. What I dislike is a scientist making pronouncements in other fields than his own, simply because he is a scientist, as when Nobel laureates in theoretical physics make unsupported statements about the drug problem. In other words, a demographer influences policy by providing authoritative evidence of a demographic character, not by simply proclaiming his unsupported opinions or preferences. The whole subject is complex and difficult to deal with in a short passage.

VDT: Following along that line and to repeat a bit of what you said in your 1979 interview about your philosophy of research in demography, your sociological approach. Incidentally, Ansley Coale in his interview said of you that "Davis is an original and insightful social theorist." You've said just now, again, that it's important to understand that demographic behavior takes place in a framework of human society and to analyze why demographic behavior happens, not just describe it.

What do you see as leading issues in demography over the years you've been involved? We've talked about your concerns with rapid population growth in developing countries, to which you first called attention with the India book. Now you've shifted your interest to low fertility, aging populations, in developed countries. But what do you see as leading issues in demography over all the years of your career?

DAVIS: I don't guess I've got any useful thoughts on that. It seems to me the main interests are abiding interests. One interesting thing now is that to judge by attendance at PAA meetings at any rate, the profession is expanding more in the applied field than in teaching and research. That will give rise to almost unlimited prospects. We are beginning to develop a higher morale for those who are in applied demography.

VDT: Those who are in state and local government and in business? Not publishing, perhaps?

DAVIS: They have a difficult time trying to get enough time to publish. Also, the job doesn't pay them to do research specifically for publication.

VDT: Do you think that they will, for instance, continue to come to PAA meetings?

DAVIS: Yes, especially if provision is made for discussion of their interests. I think that probably we ought to give them more attention.

VDT: Interesting point. Jay Siegel gave a paper at a PAA meeting back in the 1950s ["The Teaching of Demography," 1951 PAA meeting] where he pointed out that there were far more people teaching

some population in universities than there were members of PAA. Sociologists had to teach a course on population every five years or so, for instance, but were not really interested in it. But that's part of what I wanted to ask you in our talk on PAA. One final question on your career: What accomplishments in your career have given you the most satisfaction?

DAVIS: [Laughter]

VDT: You've had so many.

DAVIS: I suppose one accomplishment has been to point out that family planning as the sole approach to population control is a monumental mistake. Also, I was pleased to be one of the first to draw attention to the rapidly declining mortality in underdeveloped countries. I wrote an article ["The Amazing Decline of Mortality in Underdeveloped Areas," American Economic Review, May 1956], which was one of the first to call attention to the fact of unprecedented speed in mortality decline.

VDT: You were ahead of George Stolnitz?

DAVIS: George and I were running neck and neck. George followed me at the Office of Population Research, took over a lot of the files I had on mortality. We were both interested in comparative mortality trends and gathered data on them.

VDT: He had already done some thinking about it?

DAVIS: Yes. We came out about the same time with articles on the speed of mortality decline.

VDT: Well, that's an example of how many things you've been into. Frankly, I don't associate you with that. That's one of George Stolnitz's main claims to fame, but just one of your several claims to fame. Presumably you are pleased with the impact you had on India's family planning program?

DAVIS: Yes--with reservations.

VDT: What about International Population and Urban Research, the institute at Berkeley, does it still exist?

DAVIS: Berkeley now has a Graduate Group in Demography.

VDT: That was a successor of both the department and IPUR? I interviewed Ron Lee there last Friday.

DAVIS: Ron Lee was a very good student of the separate department, but he went to Harvard for a Ph.D. in economics. The question again?

VDT: The accomplishments that have given you the most satisfaction.

DAVIS: I was pleased with the [1967] paper on family planning. I hoped it would stir up people in family planning and it did that. It started people thinking in relatively new channels about population policy. I guess my satisfaction is measured by the interest shown.

VDT: Which was a lot.

DAVIS: Whether the interest was favorable or not is another question. I would rather be discussed unfavorably than not discussed at all. And the intermediate variables article, I'm satisfied with that. I still use it in my graduate seminar.

VDT: The copy in the Population Reference Bureau library is all written over--some people criticizing you in the middle of that. And I've got a dog-eared copy of Below-Replacement Fertility in Industrial Societies.

DAVIS: I'm glad you found that useful.

VDT: Excellent. And right on target now, when we're wondering whether we have to be concerned about low fertility and an answer to Ben Wattenberg, who is concerned.

Let's jump to PAA. You said in your 1979 interview that the first meeting you attended was in Washington, in 1939, when Lotka made his presidential address. Dudley Kirk--it happened to be his first meeting too--said he recalled that it was in the Hay-Adams Hotel. Lotka's presidential address really made an impact on you and encouraged you in your demographic career.

DAVIS: I am not sure about the date and place [of my first meeting]. For instance, I remember the place as Philadelphia, but it could have been Washington. [There was a "fall meeting" of PAA, under the auspices of the American Philosophical Association, in Philadelphia, November 18-19, 1938. On Friday evening, November 18, Alfred Lotka, who was PAA president at the time, having been elected at the sixth annual meeting of May 1938 in Princeton to hold office through the seventh annual meeting of May 12-13, 1939, in Washington, spoke on "Contacts of Population Study with Related Branches of Science," according to the announcement of the Philadelphia meeting in Population Index.]

VDT: What other highlights and people do you remember from the early meetings? I want to mention that a number of people have mentioned the wonderful debates there were between you and Frank Lorimer in the 1950s and early 1960s, in every fertility session. Apparently, you two sort of lit into each other; it almost came to a physical fight at one point, I'm told. You took the socioeconomic approach to understanding fertility decline and his approach was cultural. I've never understood why those two should be different.

DAVIS: Essentially there aren't any differences.

VDT: Yes, but somehow you seemed to approach it differently enough to cause interesting fireworks in those meetings.

DAVIS: I guess it would be a long story to go into. I never thought of Lorimer as a rival, but he evidently thought of me as being one for him.

VDT: You never thought of him as important?

DAVIS: In my career, no. His material was too unsystematic to be worth serious consideration. I didn't think much about it.

VDT: I see. Well, you had some lively debates in those meetings. Charlie Nam, in particular, said everybody looked forward to the fertility session every year, because invariably you two would have lively discussions.

DAVIS: I guess that's an exaggeration.

VDT: What else do you remember from some of the early meetings?

DAVIS: Pat Whelpton. He was well known at the time; very effective demographer. He was an associate of Warren Thompson at the Scripps Foundation [for the Study of Population Problems]. He was always gifted in interpretation. I always enjoyed talking to him. In demography, he was a very talented technician and yet a master interpreter. He worked well with Thompson, and like Thompson, he was easy to talk to. One of the good things about the early meetings was that they were small enough to allow you to talk to most everybody you wanted to.

VDT: That's what many people say they found outstanding about the early meetings. Do you miss that?--although you have come faithfully to all the meetings.

DAVIS: Oh, yes. I liked the meetings better when they were smaller.

VDT: But you're one of the few members who's been coming that long who still presents papers and roundtable sessions.

DAVIS: Well, might as well.

VDT: Okay! Obviously, you still enjoy the meetings, because you come and you take part--surrounded by admirers. Do you remember any outstanding meetings over the years? Here's a list of the meetings that Andy Lunde prepared. It includes the attendance numbers at the earliest meetings [through 1935] and from 1967 on. Your meeting, where you gave your "Change and Response" address, was in Philadelphia, in 1963. That was one of the few where they had an interesting setting for the dinner; it was in the museum of anthropology of the University of Pennsylvania.

DAVIS: I didn't see much of it. I was sitting in the hotel room finishing up the paper.

VDT: Were you! And did you change it at all before it was published in Population Index [October 1963]?

DAVIS: I went back over it, but I didn't do much changing. When I was in my hotel room, I'd known I wanted to use the Irish as an example, but I hadn't quite finished the analysis of the Irish situation. The presidential cocktail hour was going on and I got there just as everybody was being seated after the cocktail, ready to receive the presidential address. I got there just in time.

VDT: You mean just off the top of your head you were doing the analysis of the Irish situation! You had some data in front of you?

DAVIS: I'd already looked at some data.

VDT: Well, you are a quick study. Let's talk about applied demography, where you say you feel the future is--that's where the jobs are.

DAVIS: Well, that's where a lot of the expansion is occurring.

VDT: Do you think there's still room in demography for basic researchers, overall theorists, multi-interested demographers like yourself?

DAVIS: Of course. I would not want to exclude anyone.

VDT: PAA is still pretty small--2,600 [2,679 at the end of 1989]. It's been fluctuating about that number since the mid-1970s. That's small in comparison to the American Sociological Association or the economists.

DAVIS: Yes.

VDT: But you feel that anyone who wants to come in should come in?

DAVIS: I don't hold any fear of imposters. As long as people are willing to pay their dues, I guess they show special interest in the field.

VDT: Who among the younger demographers do you think could assume your role, your mantle, as the grand over-arching theorist, who sees the whole picture?

DAVIS: [Much laughter] Well, I don't know.

VDT: Okay. Are you discouraged by the outlook for world population trends?

DAVIS: Yes.

VDT: Indeed. Here is the Population Reference Bureau 1989 World Population Data Sheet. The world population growth rate this year--though you're not supposed to use the Data Sheets for time series--has gone up from 1.7 to 1.8 percent. World population is at 5.2 billion and the projection for the year 2000 has gone up. Are you still pessimistic?

DAVIS: I'm not optimistic.

VDT: And what about aging populations in developed countries?

DAVIS: Well, what about them?

VDT: You're not too concerned about them?

DAVIS: Nothing you can do about them except modify their circumstances.

VDT: And what are your plans; still no retirement? You've deplored non-working retirees, in that New York Times op-ed article and in the article you wrote with Nelly van den Oever in the March 1981 Population and Development Review, "Age Relations and Public Policy in Industrial Societies."

DAVIS: Well, it gives demographers a lot of work to do.

DAVIS: Are you going to take time to write your autobiography?

DAVIS: I don't think so.

VDT: I hope someone will write of you. At least you should supervise the collection of your publications. Is somebody doing that?

DAVIS: That's in the cards.

VDT: Good. And I think you should write an article like Frank Notestein's last article, published in Population and Development Review [December 1982], "Demography in the United States: A Partial Account of the Development of the Field." From your perspective, you've got an important story to tell, that only you can tell.

DAVIS: I'd be interested in doing it if I get the time.

VDT: Well, you've had a wonderful life, on all fronts, including your new small son, Austin. Did you name him for Austin, Texas?

DAVIS: Yes.

VDT: And your wife Marta and your various contributions, as I say, on all fronts. To end up, I just have to tell you a story that Lincoln Day told me.

DAVIS: He's a great storyteller.

VDT: He is. He said you and he were both stuck in the Dallas airport after you'd attended a seminar at the University of Texas. You had been impressed by the caliber of the students you had met there. And you said that you probably wouldn't have made it into the University of Texas today, because you had not been in the top 25 percent of your high school graduating class. Now, is that true? In any case, you did get into Texas and look what a wonderful career you've had! Is that an apocryphal story or not?

DAVIS: [Laughter]

VDT: And I notice that you graduated cum laude from Texas, in English.

DAVIS: Yes.

VDT: But is it true that you weren't in the top 25 percent of your graduating class at high school?

DAVIS: That's apocryphal; I don't remember that. [Laughter]

VDT: Well, anyway, Lincoln used that as a wonderful example of universities being exclusive, making it so tough for people to get into universities; you might knock out the geniuses and late-bloomers. Though you were no late-bloomer; you were a very early bloomer.

DAVIS: It never occurred to me that they might not accept me at Texas. One of my satisfactions, several years back, was to get a letter from the editor of the campus literary magazine. I was editor when I was there. He referred to a particular editorial I wrote, wanted to reprint it. Gave me a lot of satisfaction.

VDT: You were saying things back then still worth saying?

DAVIS: I had an editorial each month; the magazine came out monthly. But I was operating under a faculty committee that supervised student publications. At that time, of course, matters of freedom were differently conceived from what they are now. The printer was instructed always to hold up publication of questionable material until the committee could see it. So, the printer called me over one day and showed me my editorial, all set in type. It was on campus morality. I'd said campus morals were pretty bad but not bad enough.

The committee called me over and said, "Kingsley, you can print this if you want to, but you can't remain a student if you do." So, I said, "Okay, I'll continue my education." So, I ran a big box with a black margin around it and the words: "Editorial this month has been censored." They said they weren't censoring my material, but if I published it, I couldn't remain in school. So, I said, "Editorial this month censored." I circulated it, mimeographed, throughout the campus. And I always remember, "Leads to industrial democracy"--that was a socialist line. I ran a radio program under socialist auspices while I was a student.

VDT: You ran a radio program too! You were into many things. And this editor, who wrote you a few years ago, did he find the original editorial, and it finally ran in the student magazine?

DAVIS: Yes.

Two Davis after-remarks:

Re running for PAA office: "I was always pitted against Irene Taeuber and she always won."

Re times away on consultancies, etc.: "I've never been more than six weeks away from a university campus. You don't need that long to understand a country" [re his time in India].



Kingsley Davis

1908–1997

BIOGRAPHICAL

Memoirs

*A Biographical Memoir by
Geoffrey McNicoll*

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NATIONAL ACADEMY OF SCIENCES

KINGSLEY DAVIS

August 20, 1908–February 27, 1997

Elected to the NAS, 1966

Kingsley Davis was a significant figure in American sociology of the mid-twentieth century and by many estimations the leading social demographer of his generation. He made influential contributions to social stratification theory and to the study of family and kinship. He was an early theorist of demographic transition—the emergence of low-mortality, low-fertility regimes in industrializing societies—and a protagonist in major and continuing debates on policy responses to rapid population growth in low-income countries.

Davis held faculty appointments at a succession of leading universities, for longest at the University of California, Berkeley. He was honored by his peers in both sociology and demography: president of the American Sociological Association in 1959 and of the Population Association of America in 1962-'63. He was elected to the National Academy of Sciences in 1966, the first sociologist to be a member.



Kingsley Davis

By Geoffrey McNicoll

Kingsley Davis was born in Tuxedo, Texas, a small community near Abilene, in 1908. His parents were Joseph Dyer Davis, a physician, and Winifred Kingsley Davis; he was a collateral descendant of Jefferson Davis. He took his baccalaureate from the University of Texas in 1930. (Revisiting the campus decades later as a distinguished professor, he allowed that he would not have qualified for admission under later, more stringent criteria.) Two years later at the same institution he received an MA in philosophy with a thesis on the moral philosophy of Bertrand Russell. Viewing English and philosophy as having scant bearing on the Depression-era conditions of the time, he turned toward the social sciences as a field for further graduate study.

He was awarded a doctoral fellowship by Harvard's sociology department, then newly established under the chairmanship of the distinguished Russian émigré scholar Pitirim Sorokin. (Sociology at Harvard had previously fallen under a Department of Social Ethics.)

In his years at Harvard, fellow students included George C. Homans, Robert Merton, and Matilda White Riley, all, like Davis, future presidents of the American Sociological Association. Another student contemporary, but in the Law School and apparently unknown to Davis, was David Riesman, who only later shifted his sights to sociology. Important influences on Davis's sociological thinking, aside from Sorokin, were Talcott Parsons, then an instructor in the department, and the anthropologists W. Lloyd Warner, his principal thesis adviser, and (from a distance) Ralph Linton. Davis's Ph.D. dissertation, titled *A Structural Analysis of Kinship: Prolegomena to the Sociology of Kinship*, was completed in 1936.

Davis's subsequent academic affiliations can be simply listed: Clark University (1936-'37), Pennsylvania State University (1937-'42), Princeton (1942-'48), Columbia (1948-'55), UC-Berkeley (1955-'76), University of Southern California (1977-'91), and the Hoover Institution (1981-'92).

Davis as Sociologist

In his early post-doctoral years Davis wrote on a variety of sociological subjects, principally in the areas of kinship, family, and stratification. His dissertation largely informed the article "Structural analysis of kinship," with W. Lloyd Warner (*Amer. Anthropol.*, 1937), an elaborate comparative study that fell between disciplines and perhaps as a result was rarely cited. However, the understanding of family structure and processes that underlay it bore fruit in Davis's later papers on caste societies, social stratification, family conflict, adolescence, marriage, "sexual property," and prostitution that he published in the 1930s and 1940s.

These were pioneering and sometimes controversial treatments of their subjects. In the summation of the *New York Times* obituarist, Davis took on issues "previously considered too intimate to explore," such as male sexual jealousy as a foundation of social organization or the case for prostitution as a benefit to society. An especially notable cause of controversy was "Some principles of stratification," which he wrote jointly with Wilbert E. Moore (*Amer. Sociol. Rev.*, April 1945). Premised on the "functional necessity" of stratification, it argued that social inequality was "an unconsciously evolved device by which societies insure that the most important positions are filled by the most qualified persons" through the allocation of prestige and tangible economic returns. By implication, egalitarian ideals were unachievable—a conclusion, not surprisingly, many have resisted. "The Davis-Moore theory of stratification," or simply "the Davis-Moore hypothesis," is chiefly how today's sociology students would likely encounter Davis's name.

Davis's main longer-form work in sociology was the book *Human Society* (1949). This was an ambitious, crisply organized analytical overview based on the sociology course he taught at Princeton. The "impertinence" (his word) of its title was qualified in the preface: He dealt with basic structural-functional questions, such as "What enables human social systems to operate as going concerns? What do they require for their existence, and what major structures do they evolve which enable them to meet these requirements?" Social evolution, though barely mentioned, was a thematic undercurrent. The level of generality was high: a reviewer (in *Social Forces*) remarked that "the carefully reasoned text is unbroken by provocative and diverting illustrations." Yet it was not any issue of readability but, rather, competition from another quarter that counted for more in the marketplace of ideas. Writing much later, the sociologist John Finley Scott, a former student of Davis's, remarked on *Human Society's* muted impact, qua theory, on the discipline. "It was not Davis but Talcott Parsons, impenetrable in style and utterly devoid of parsimony, who gave sociologists what they wanted in heavy theory."

Over time, disciplinary interests and approaches change. Before long, Parsons, too, receded into the little-read historical background of sociology. Increasingly, structural-functionalism was disdained, becoming almost a pejorative term. Davis protested. His vigorous defense, though even then to some a voice from the past, came in his 1959 presidential address to the American Sociological Association, "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology." Functionalism, he asserted, was not so much dated as ubiquitous and thus no longer noticed. What was not functionalist in sociology was either reductionist or mere description. Sociologists, he might have said, were like Molière's Monsieur Jourdain, unknowingly speaking prose.

Davis's legacy in sociology lies in the clarity of ideas and analytical rigor that make *Human Society* still well worth reading. Yet that insistence on objectivity and rigorous methodology, Seymour Martin Lipset observed, put him at odds with those who called for relevance to social problems. "He saw sociology as a basic science, like the biological disciplines, which contribute to dealing with cancer by research on cells, not by surgery."

A typology of the field proposed by a later Berkeley sociologist, Michael Burawoy, identifies four varieties of sociology: professional, critical, policy, and public. Davis's oeuvre lies mostly in the first camp, "studying the world as though it were an external object, accumulating a body of knowledge and theory that interprets and explains." To a degree he also had a public role, addressing a readership beyond academic boundaries. In contrast, critical sociology, in the 1960s and 1970s increasingly embraced by sociology students and not a

few faculty, held no place in Davis's purview. As for social policy, when he treated it at all it, too, was as a subject for dispassionate analysis. Policy issues were to become central in his later work in demography, but his major disputes were with those he saw as lacking that detachment.

Demographic Theory and Analysis

Davis's introduction to formal demographic analysis came in 1940, during a postdoctoral stint at the University of Chicago with the statistician Samuel Stouffer and at the Census Bureau in Washington with Philip Hauser. In 1942 he joined Princeton's Office of Population Research (OPR) at the invitation of its founding director, Frank Notestein. (OPR fell under the School of Public and International Affairs; an appointment for Davis in sociology was arranged subsequently.)

Davis and Notestein, in separate publications in 1945, are generally credited with the first statements of what is termed the theory of demographic transition. (Dudley Kirk, another OPR researcher, could justifiably share the credit.) In its original formulation, this theory asserted that the industrialization of agrarian societies would be accompanied by a shift from a regime of high death and birth rates to one of low rates, and that death rates would fall earlier than birth rates, resulting in a period of population growth.

These propositions of course had a prehistory. From the late nineteenth century on, death rates had been steadily dropping in many Western countries, not least the United States. Social scientists proffered overlapping reasons, economic, social structural, and cultural, but essentially tying the trends to the emergence of industrial society. The best of these accounts, for example by the sociologists Edward Alsworth Ross and Charles Horton Cooley in the first decade of the twentieth century, hold up well today. In the 1930s, Adolph Landry in France and Warren S. Thompson in the United States envisaged similar demographic trends worldwide. As to the sequence of the changes, it was reasonable to expect that fertility, tied into many societal institutions and traditions, would be more resistant to change than mortality. This argument Davis had spelled out as early as 1937 in a seminal essay criticizing pre-World War II European *pronatalist* efforts ("Reproductive institutions and the pressure for population"). Although often characterized as more generalization or stylized fact than theory, demographic transition is the basic lens through which most people broadly see the course of population change in the modern world.

Davis returned to the subject in a later contribution, “The theory of change and response in modern demographic history,” his 1963 presidential address to the Population Association of America (PAA). In this, he offered a fuller account of the range of individual and societal responses to the challenge posed by sustained natural increase, encompassing migration as well as delayed marriage and birth control.

Davis’s most cited article, written jointly with Judith Blake (his wife and an important collaborator in his Berkeley years), was a contribution both to demographic analysis and to clear thinking about population policy. Titled “Social structure and fertility: an analytic framework” (*Economic Development and Cultural Change*, 1956), this article introduced what the authors termed intermediate fertility variables (now usually called proximate determinants of fertility). These are the factors immediately influencing fertility and through which—and, when fully listed, *only* through which—prevailing economic and social conditions and individual behavior can affect fertility outcomes. The Davis-Blake intermediate variables were 11 in all, such as age of entry into a union, contraceptive use, and induced abortion. Given quantitative substance in a later contribution by John Bongaarts (who condensed the number and added a newly-recognized proximate determinant: reduced fecundability due to breastfeeding), this simple framework is now routinely used in fertility analysis and policy design. Not least, it emphasizes the distinction—often blurred in the family planning literature—between motivation to use contraception and actual use.

Population Studies

Although Davis’s scholarly reputation in demography is mostly as theorist, in his early career he was at least as well known for a contribution to classical population studies: *The Population of India and Pakistan* (1951). The writing of this elaborate treatise occupied a lot of his research time at Princeton, spilling over to Columbia. The choice of subject lay in OPR’s divvying up of the world under contracts from the League of Nations and the State Department to study likely post-World War II demographic situations in major regions. The plausible relevance of Davis’s prior writings on social stratification to caste in India gave him a claim on the subcontinent. (The partition of British India occurred during the writing.) The oversize volume that resulted was an exhaustive study, not least remarkable given Davis’s resolutely U.S.-based scholarly background and personal unfamiliarity with the gritty realities of South Asian life: he took his first trip to India only after its publication.

The content draws heavily on India's long sequence of censuses from the 1860s onward, preserved in meticulous reports of successive colonial census commissioners. But the book's ambition went far beyond a sort of cumulative census report. It was intended, Davis wrote in his introduction, as "a contribution to the sociology and economics, as well as the demography, of India and Pakistan." On policy, he had a simple message: population growth urgently needed to be curtailed, and this should be achieved through "a sustained and vigorous birth control campaign, and a scheme of rapid industrialization."

The book was well received both professionally and in the region. Alfred Sauvy wrote a favorable notice in *Population* (April-June, 1951), though muttering about the Malthusianism of "l'école démographique américaine." In Delhi's *Economic and Political Weekly* of December 1, 1951, the reviewer's sole reservation, it could be argued, turned out to show Davis's prescience: "There is seemingly a shade of insolence and tenacity in his criticism of caste demography. He not only seems to exaggerate the influence of caste, but also believes that it is adapting itself to new conditions and would therefore stay with us for a long time to come." The book's policy advice was well timed: India in the early 1950s was initiating its family planning program.

Just as in sociology, in population studies Davis ranged widely. Beyond India, he published articles on contemporary population problems in the United States, on Latin American and Caribbean (Puerto Rican and Jamaican) demography, on human migration over the millennia, and on the world population crisis. He wrote not just for academic journals but also for *Foreign Affairs*, *Scientific American*, and *The New York Times Magazine*. A continuing interest of his was urbanization, reflected in the name of the institute he set up at Berkeley: International Population and Urban Research (IPUR). There are recurring articles on the subject in his bibliography, and several monographs. IPUR published two lengthy reports by Davis that assembled worldwide statistics on city sizes—to observers a somewhat pedestrian activity for an eminent scholar. Most of IPUR's research output was similarly workmanlike, heavily empirical and low-key. An exception, the source of the greatest publicity it garnered, not altogether welcomed, was a government-funded 1963 report it sponsored, prepared by David Heer, estimating the scale and geographic distribution of U.S. mortality in a nuclear war. It was published as *After Nuclear Attack: A Demographic Inquiry* (Praeger, 1965).

While Davis was wholly comfortable with numbers, as the India study made decisively apparent, he had little liking for survey research. Yet that is the direction social demography took. There had been important family surveys in the United States, one

even based at Princeton, but beginning in the 1960s demographic surveys proliferated worldwide as advances in computing technology removed the laboriousness from data analysis. The most prominent example was the World Fertility Survey (WFS), carried out through 40 surveys in an elaborately detailed uniform format in numerous third-world countries between 1972 and 1984. In “The world’s most expensive survey,” a 1987 review essay on the WFS summary volume, Davis complained of its paucity of theoretical underpinnings beyond what he characterized as “thinly disguised rationalizations” of family planning programs. His blunt conclusion: “In sum, I think the WFS was misconceived. It assumed that, from the standpoint of policy, the great need was for more and better data, but actually the need was for clearer and broader thinking.” The criticism was for naught. The WFS was succeeded by an even larger and more expensive program along the same lines, the largely U.S. government-funded Demographic and Health Survey, which became (and remains) the main depiction of changing demographic conditions around the world—as well as a major feedstock for graduate study and publication in demography.

Population Policy

According to transition theory the spread of industrialization around the world should bring about a fall in fertility to match that of mortality. Yet that expectation was soon challenged. The years following World War II witnessed, in the title of one of Davis’s articles, “the amazing decline in mortality in underdeveloped areas” induced by interventions such as malaria control and other public health measures. It was the pace of this decline that was unexpected, unleashing unprecedentedly rapid population growth in regions still economically backward. This growth threatened to impede economic development and sustain the existing high levels of fertility. A direct approach to reducing fertility seemed called for.

Dennis Hodgson (1983) has documented the changing view among American demographers in the early 1950s “from a strictly social scientific stance to an explicitly policy-oriented one,” with Notestein and Davis in the van. Alarm over rapid population growth spread widely. Survey evidence seemed to suggest the existence of a pent-up demand for birth control, one that family planning campaigns could supply. Agrarian societies could thereby lower their fertility prior to industrialization, with benefits both for individual well being and for economic development. Davis’s writings at the time, especially on India, were supportive.

In the 1960s the development of the IUD and oral contraceptives gave further impetus to this approach. A major organizational effort was mounted to spread the use of modern contraception in poor countries, led initially by private U.S.-based organizations such as the Population Council (of which Notestein was now president) and the Ford Foundation, but soon joined by U.S. and other government foreign assistance agencies and, eventually, the United Nations. Davis, now at Berkeley, had become increasingly skeptical of program interventions that promised a demographic outcome without dealing with the motivational basis and societal supports for high fertility. This was the setting in which his 1967 article “Population policy: will current programs succeed?” was published—prominently, in the journal *Science*. In essence, Davis was criticizing “the designation of population control as a medical or public health task” and reasserting, in a more sophisticated form, the earlier social-science-centered stance of transition theory. He did not pull any punches: “The world’s population problem,” he wrote, “cannot be solved by pretense and wishful thinking.”

The *Science* article took aim at the proponents of family planning, a group that encompassed many well-known demographers at Princeton, Chicago, and Michigan, among whom it created some amount of consternation and a lot of anger. One response came in the form of a letter to the editor of *Science* signed by the ten members of the National Academy of Sciences’ Committee on Population, attempting to stake out a middle ground. Davis’s reply in the same issue flatly rejected the terms of compromise. To the plea that contraception had to be promoted “within the framework of existing values,” he responded: “‘existing values’ are not the means for solving the problem; they are the problem.” Moreover, he accused the signers of trying to enlist the Academy to back their position. “Scientific controversy is ordinarily conducted in terms of logic and evidence, but the committee has chosen to assert a presumed authority.”

Aside from the asperity, what is remarkable about this debate is its longevity. Fertility declined substantially throughout the world in the subsequent decades except for some regions of Sub-Saharan Africa, but the counterfactual of what would have happened in the absence of family planning campaigns remains contested. Blurry field experiments with treatment and control areas have not resolved the matter, nor have innumerable applications of multivariate analysis. Social scientists by and large (and economists especially) take Davis’s side; public health practitioners and program managers take the other. Demographers are split.

There was a more radical follow-up to Davis’s critique. The “Population Establishment” (Davis’s term for the foundations, non-governmental organizations, and agencies promoting

and funding family planning programs) distanced their policy objective from population limitation, preferring the more politically acceptable goal of reducing unwanted births and benefiting women's health. Davis saw this as disingenuous. Returning to the fray in a 1973 paper in *Daedalus*, "Zero population growth: the goal and the means," he spelled out the more drastic kinds of measures that might indeed bring about zero population growth (ZPG) or negative growth: amounting, essentially, to curtailing reproductive freedom. His summary: "People want families and children... They do not want runaway population growth either, but they want to avoid it painlessly... In short, they want a miracle."

One can read into the ZPG article, carefully couched though it was, Davis's hankering for a less populous, less hemmed-in society of the kind he grew up in—both for the United States and, a fortiori, the world. "The human species," he wrote, "is now in the preposterous situation of using an extremely advanced technology to maintain nearly four billion people at a low average level of living while stripping the world of its resources, contaminating its water, soil and air, and driving most other species into extinction, parasitism, or domestication." Shorn of sociology, the sentiments mirror those of the ecologists Garrett Hardin and Paul Ehrlich. In a 1989 interview for the PAA Oral History Project, Davis put it starkly: "[T]he failure to control population is the greatest tragedy that ever hit humanity."

The issues that occupied Davis in the last phase of his professional life, based mainly at the Hoover Institution, were those facing the family and society in the contemporary West. He wrote on intergenerational conflict and social support for the elderly, the "sex role revolution" and changes in work-life balance, the causes and consequences of very low fertility, and the rural exodus of population. He organized a succession of conferences on contemporary marriage, below-replacement fertility, and resources and the environment, contributing introductory essays and editing or co-editing the ensuing volumes. The sociological imagination was intact; the sharp edges less evident.

People and Places

Davis had a role in the development of several sociology programs—at Penn State, Princeton, Columbia, and Berkeley. He chaired a new department of anthropology and sociology at Princeton, recruiting such figures as Marion Levy, Wilbert Moore, and Melvin Tumin and building what was to become one of the country's leading centers of sociological research. At Columbia he joined Paul Lazarsfeld and Robert Merton in an already strong group, briefly (until he could disengage) directing a program on applied social research.

His Berkeley period was more complicated. In 1955, when Davis joined it, Berkeley's Sociology Department already had some prominent members—notably Herbert Blumer, Seymour Martin Lipset, and Reinhard Bendix. Ambitious expansion plans brought in a wave of new faculty recruited from the more established programs at Harvard, Chicago, and Columbia. The results were striking: in the national ranking of sociology departments, Berkeley moved up from eighth in 1959 to first in 1966. The dominance was not to last. The Free Speech Movement in 1964 and the anti-war and other protests later in the decade, some with significant vandalism, seriously politicized the department—as it did the university generally—dividing the faculty into supporters and opponents of an increasingly radicalized student body. (The 1969-70 campus demonstrations were quelled by California National Guard troops under then Governor Ronald Reagan, with repeated use of tear gas—on one occasion spread by helicopter.) A number of senior members of the department chose to leave. Davis had chaired the department in the early 1960s but withdrew from most of the subsequent departmental politics—literally so, by moving up the hill to IPUR's base on Piedmont Avenue. His position vehemently opposed to the radicals was not in question.

The year before he came to Berkeley Davis had married Judith Blake, then a graduate student at Columbia (and 18 years his junior), who was to become a noted social demographer herself. Together, she from a post in the School of Public Health, they were the driving force in setting up Demography as a formal academic department at Berkeley in 1967, with Blake as chair. This innovative enterprise (few such departments then existed anywhere, and not many do now) assembled a distinguished faculty: Nathan Keyfitz, Paul Demeny, and, in his first academic position, Samuel H. Preston—each of them a future president of the PAA—as well as drawing on associated faculty in neighboring departments, such as the economists Carlo M. Cipolla and Albert Fishlow, and Davis himself. Its coterie of Ph.D. students—myself included—found this a rich learning environment, even during these turbulent years. The department foundered in the early 1970s, weakened by faculty departures and eventually closed in a cost-cutting move by the university's chancellor, only to be revived in a new administration and under new leadership—that of Ronald D. Lee—later in the decade. It took over the old IPUR building as its base.

Age-based retirement at Berkeley led Davis to a distinguished professorship at the University of Southern California. Los Angeles smog drove him north again—first to a period at the Center for Advanced Study in the Behavioral Sciences, then to a fellowship at the Hoover Institution—politically a congenial setting. He finally retired in 1992.

Like most senior academics, Davis took on numerous responsibilities outside the university, many of them requiring time in Washington or New York. He chaired or was a member of a diverse array of National Academy of Sciences committees and was on the NAS Council. He served on advisory committees to the Census Bureau, the National Institutes of Health, NASA, and the California state government. For several terms he was the U.S. representative to the UN Population Commission. He held leadership posts in the American Association for the Advancement of Science, the American Statistical Association, and the American Association of University Professors. He was a one-time board member of the American Eugenics Society and a long-time trustee of the Population Reference Bureau.

Davis the Man

John Finley Scott offers perhaps the best succinct summary of Davis the scholar: “unsentimental and iconoclastic, sharp-witted and combative.” His friendships were many and long lasting, both with colleagues like Merton and Lipset and with former graduate students. Belying a reputation as somewhat of a womanizer, writes Ruth Dixon-Mueller, “he was ultimately a strong mentor for his female students.” David Heer, his biographer and a longtime friend and colleague, has written of Davis’s “famous rages” but says that these blew over quickly. Some academic conflicts were longer lasting. He fell out early with Sorokin, in a rift that Heer argues was “bound to occur” between two very strong personalities with such divergent worldviews. It began, he speculates, with Sorokin taking exception to Davis’s stance on the role of organized religion in the control of sexuality, evidenced in his early work on sexual property and prostitution. His dispute with Notestein was rooted in the major policy differences discussed above.

For Davis the person, more should be added. He took pride in proclaiming his rural Texan origins, displayed, when he chose, in a distinctive Southern drawl. At the same time this made him a something of an outsider in Ivy League precincts, which may have encouraged his move to California—and his later expressions of disdain for the mainly East-Coast population establishment. (A dislike for large cities, New York in particular, was another push factor.)

Physically, Davis was trim and fit throughout his life. His one-time student at Penn State and friend for six decades, William J. Goode, writes of Davis’s “sheer animal joy of living.” At Hoover, then in his late 70s, “he continued to ride his bike to school, to climb in the Stanford hills for an hour a day, and to spend at least two nights a week square dancing.” Only in his final years did this pace of life slow down, as the ravages of Parkinson’s took over.

Davis married four times and had four children. His first marriage was short-lived, before he left Texas. His second, to Jane Quinn in 1936, lasted 18 years; they had two children: Jo Anne (b. 1945) and Jefferson (b. 1950). They were divorced in 1954. The same year he married Judith Blake, with whom he had a daughter Laura (b. 1959). This marriage too ended in divorce, in 1976. In 1985 he married Marta Seoane, who survived him. His youngest child, Alexander, was born in 1987. Davis died on February 27, 1997, at the age of 88.

Davis's papers are deposited in the Hoover Institution Archives. A full inventory is at the Online Archive of California website.

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The Theory of Change and Response in Modern Demographic History

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CURRENT ITEMS

THE THEORY OF CHANGE AND RESPONSE IN MODERN DEMOGRAPHIC HISTORY

The process of demographic change and response is not only continuous but also reflexive and behavioral—reflexive in the sense that a change in one component is eventually altered by the change it has induced in other components; behavioral in the sense that the process involves human decisions in the pursuit of goals with varying means and conditions. As a consequence, the subject has a frightening complexity—so much so that the temptation is great to escape from its intricacies. One method of escape is to eschew any comprehensive theory, simply describing computations or working on a single hypothesis at a time. Another is to adopt some convenient oversimplification, such as the assumption that population is simply a matter of two capacities—a “reproductive urge” on the one side and “means of subsistence” on the other—or, at an opposite extreme, that demographic behavior is a function of a “traditional culture” or “value system.”

My purpose here is to try to encompass some of the complexities in an overall analysis of demographic change in the industrialized countries. To do this, I prefer to start with Japan. Not only does Japan, the sole fully industrialized non-Western country, furnish a perspective that no other country can furnish, but some phases of its population change are statistically better documented.

Abortion as a Demographic Response

The phenomenon most discussed—and one commonly regarded as peculiarly Japanese—is the rapid rise of the registered abortion rate from 11.8 per 1000 women aged 15-49 in 1949 to a peak of 50.2 per 1000 in 1955,¹ although at the latter date the registration of abortions is estimated to have been only 50 to 75 per cent complete.² The resort to abortion has been the leading cause of probably the fastest drop in the birth rate ever exhibited by an entire nation, births per 1000 women aged 10-49 falling by 41 per cent between 1950 and 1957. Westerners profess to be astonished by this phenomenon, but they should not be. The behavior of the Japanese is essentially the same in kind as the behavior of West Europeans at a similar time in their social and demographic history. The main difference is that Japanese tolerance permits the abortion rate to be reasonably well known, whereas in the past of Europe the abortion rate has never been known and, for this reason, is usually ignored in population theory.

Yet there is indirect and approximate evidence that in the late nineteenth and early twentieth centuries in Western Europe abortion played a great role. David Glass, who in 1940 summarized the findings for eight

Editor's Note.—This is the text of the address delivered by Kingsley Davis, International Population and Urban Research, University of California, President of the Population Association, at the banquet on the evening of April 26, 1963, at Philadelphia, as part of the annual meeting of the Association.

northwest European countries, cited the records of women under a German sickness benefit fund which show a gradual climb in abortions from 38 per 100 births in 1908 to 113 per 100 in 1932./³ In Belgium "there were many books explaining how to induce abortion and any woman could buy, for 60 centimes, a uterine syringe and use this to induce an abortion."/⁴ In both France and Germany advertisements by abortionists were freely published. In fact, one gets the impression that the attitude toward abortion in West European society was much less intolerant between 1900 and 1935 than it is today. A study of maternity cases in Israel in 1958 showed that, for women born in Europe, America, and Australia-New Zealand, 32 per cent of those having a third birth admitted having resorted to induced abortion./⁵

Finally, in five of the People's Republics in Eastern Europe, which have legalized abortion, the subsequent history of the rise of registered abortions, as summarized by Tietze,⁶ is amazingly like that of Japan. In Hungary, for example, medical boards were established about 1953 for authorizing therapeutic abortions. "That these boards progressively liberalized their policies is reflected in the growing numbers of legal abortions from 1953 onward." After the decree of 1956 permitting "the interruption of pregnancy on request, the number of legal abortions increased rapidly until in 1959 it exceeded the number of live births."/⁷ Not only did the legal abortion rate rise rapidly in all four countries but also, as in Japan again, there was a substantial non-legal rate. The number of abortions per 100 births in 1961 was in Hungary, 145; Czechoslovakia, 55; Poland, 35; and Yugoslavia (1960), 34.

If, then, abortion was once a widespread practice in the most advanced countries of Western Europe, if it is now widespread in Eastern Europe, where it is legal and subject to record, and where economic development is behind that of Western Europe, there is no reason to regard the resort to abortion as peculiarly Japanese. It is not an outgrowth of ancient tradition in Tokugawa times; not an outgrowth of the absence of Christian ideology. It is a response to social and economic conditions arising in country after country at a particular time in the process of modernization. The fact that abortion was not safe earlier in the century shows how determined the people of northwest Europe were in their reproductive control. Now that it is reasonably safe when legalized,⁸ it is an effective means of family limitation for Hungary and Poland as well as for Japan.

If Western prudery and Oriental realism have led to an exaggeration of the role of abortions in Japan, this tendency has been helped by a statistical illusion. Not only have abortions increased as births have fallen, but the sum of births and registered abortions for each year yields a combined rate per 1000 population that has changed little during the big fertility drop (Table 1)./⁹ This seems to say that an abortion was responsible for each birth saved. Actually, of course, of abortions can and do occur much more frequently than births can./¹⁰ Other factors must therefore have played a role in Japan's falling birth rate.

Other Responses in Japan

One such factor was contraception. Irene Taeuber points out that this practice increased rapidly after 1950 although abortions were available,

Table. 1. Births and Abortions in Japan.

	Annual totals (000's)			Sum per 1000 Population
	Births	Abortions	Sum	
1949	2,697	102	2,798	34.4
1950	2,338	320	2,658	32.1
1951	2,138	459	2,596	30.8
1952	2,005	798	2,803	32.8
1953	1,868	1,067	2,935	33.9
1954	1,770	1,143	2,913	33.1
1955	1,727	1,170	2,897	32.6
1956	1,665	1,159	2,825	31.4
1957	1,563	1,122	2,686	29.6
1958	1,653	1,128	2,781	30.4
1959	1,626	1,099	2,725	29.5

Sources: Kimura, Masabumi. "A Review of Induced Abortion Surveys in Japan." Paper No. 43 in mimeographed proceedings of the 1961 conference of the International Union for the Scientific Study of Population. P. 1; United Nations Demographic Yearbook 1960.

relatively safe, and cheap./¹¹ Use prior to that time is shown by a 1950 national survey which found that a fifth of all couples were currently practicing contraception and that nearly a third had done so at some time. Furthermore, the age-pattern of change in marital fertility shows that, before the great rise in reported abortions began, couples were increasingly controlling their births, especially at the older ages./¹²

Of late, further control has been achieved by sterilization. Reported operations, totaling 5,695 in 1949, averaged 42,843 per year during 1955-59, at which time they equalled 3.8 per cent of the reported abortions. There is even some indication of a small amount of infanticide./¹³

In addition, the Japanese migrated from their homeland in sizable numbers. The proportion of Japanese persons aged 15-59 outside to those inside the home islands was 2.8 per cent in 1920; 3.2 per cent in 1930; and 5.6 per cent in 1940./¹⁴

Finally, the Japanese have exhibited still another adjustment—postponement of marriage. The proportion ever married among girls aged 15-19 fell from 17.7 in 1920 to 1.8 per cent in 1955, and for women 20-24 it fell from 68.6 to 33.9 (Table 2). The shift for men was also drastic. Indeed, it may be that the age at marriage rose faster in Japan than in any other country in history. By 1959 the nation had a marital age higher than that of most Western countries (Table 3). In the United States in that year nearly half the brides in first marriages were under 20, but in Japan only one-nineteenth of them were that young. However, the Japanese concentrate their marriages more heavily in the modal ages—20-24 for brides and 25-29 for grooms—than Western countries do, as Table 3 shows.

Table 2. Japan: Changing Proportion Ever Married, by Age.

Age	Percentage Ever Married					
	Women			Men		
	1920	1940	1955	1920	1940	1955
15-19	17.7	4.3	1.8	2.8	0.4	0.1
20-24	68.6	46.5	33.9	29.1	10.0	9.8
25-29	90.8	86.5	79.8	74.3	58.0	59.3
30-34	95.9	94.7	92.0	91.8	89.7	90.8
35-39	97.3	97.1	96.0	95.9	95.6	97.0
40-44	97.9	98.0	97.6	97.2	97.3	98.3
45-49	98.1	98.4	98.3	97.7	98.0	98.8

Source of data: Taeuber, Irene B. The Population of Japan. Princeton, Princeton University Press, 1958. P. 211.

Table 3. Age at First Marriage, Selected Countries.

Country and Date	All Marriages with Age Known	Percentage Marrying at Following Ages					
		Under					
		20	20-24	25-29	30-34	35-39	40+
Brides							
Japan, 1959	100	5.3	63.8	27.5	2.9	0.3	0.1
U. S. A., 1959	100	48.6	37.8	7.4	2.8	1.5	2.0
Sweden, 1950	100	13.5	45.0	24.6	8.3	4.1	4.6
Italy, 1951	100	14.1	46.0	27.0	6.9	3.2	2.8
Grooms							
Japan	100	0.4	23.7	61.8	12.7	1.2	0.3
U.S.A.	100	16.1	53.5	18.5	6.1	2.6	3.2
Sweden	100	1.9	31.4	38.4	15.7	6.7	5.9
Italy	100	1.7	24.6	42.3	18.3	8.1	5.0

Sources of data: for Japan, United Nations Demographic Yearbook 1961, Table 28; for the United States (29 states only), U. S. National Office of Vital Statistics. Vital statistics of the United States 1959, Vol. 1, p. 61; for Sweden and Italy, United Nations Demographic Yearbook 1958, Table 22.

The one adjustment the Japanese have not adopted is celibacy. In 1955 the proportion of women aged 40-44 who had never married was only 2.4 per cent, whereas in the United States in 1950 it was 8.1, and in Italy in 1951 it was 15.7 per cent (see Table 4). It looks as though the age at marriage is flexible in Japan, but not the decision to marry or not to

Table 4. Proportion Never Married among Women Aged 40-44.

	Per cent
Japan, 1955	2.4
U. S. A., 1950	8.1
New Zealand, 1951	11.3
England and Wales, 1951	14.2
Austria, 1951	14.3
Italy, 1951	15.7
Sweden, 1950	15.8
Finland, 1950	17.8
Portugal, 1950	18.5
North Ireland, 1951	23.0
Ireland, 1951	26.7

marry. However, even this may change. The women who in 1955 were aged 40-44 represent a generation whose marriages, occurring mainly in 1930-40, were still almost wholly arranged by parents. As the age at marriage gets later, and as mating becomes more a matter of individual selection, a rising contingent of women may never succeed in attracting a man they are willing to marry.

The Theory of the Multiphasic Response

What, then, is the picture that Japan presents? It is the picture of a people responding in almost every demographic manner then known to some powerful stimulus. Within a brief period they quickly postponed marriage, embraced contraception, began sterilization, utilized abortions, and migrated outward. It was a determined, multiphasic response, and it was extremely effective with respect to fertility. It brought down the gross reproduction rate, with only a brief wartime interruption, from 2.7 in 1920 to 0.99 in 1959./15 A change that took at least 60 years in the United States required only 40 years in Japan.

What was the stimulus that caused such a massive response? In my view, the demographic stimulus was the decline in mortality and the sustained natural increase to which it gave rise. The data prior to 1920, though not entirely trustworthy, do at least suggest a declining death rate./16 This is consistent with the better established trend after 1920, when, in not quite 30 years, mortality dropped to an extent that had required, starting at the same level, 76 years in Sweden and 37 years in Germany. The resulting natural increase climbed above 10 per 1000 around the turn of the century and averaged 12.8 from 1900 to 1959. When, as in Fig. 1, these rates are plotted on the same chart as those for three Scandinavian countries averaged together (Denmark, Norway, and Sweden), with Japan lagged 50 years, the latter appears to be re-enacting the history of natural increase in northwestern Europe, but more abruptly./17

But why the multiphasic reaction to sustained natural increase? Were the Japanese experiencing increased poverty? Were their "means of subsistence" disappearing under the impact of increased millions? No,

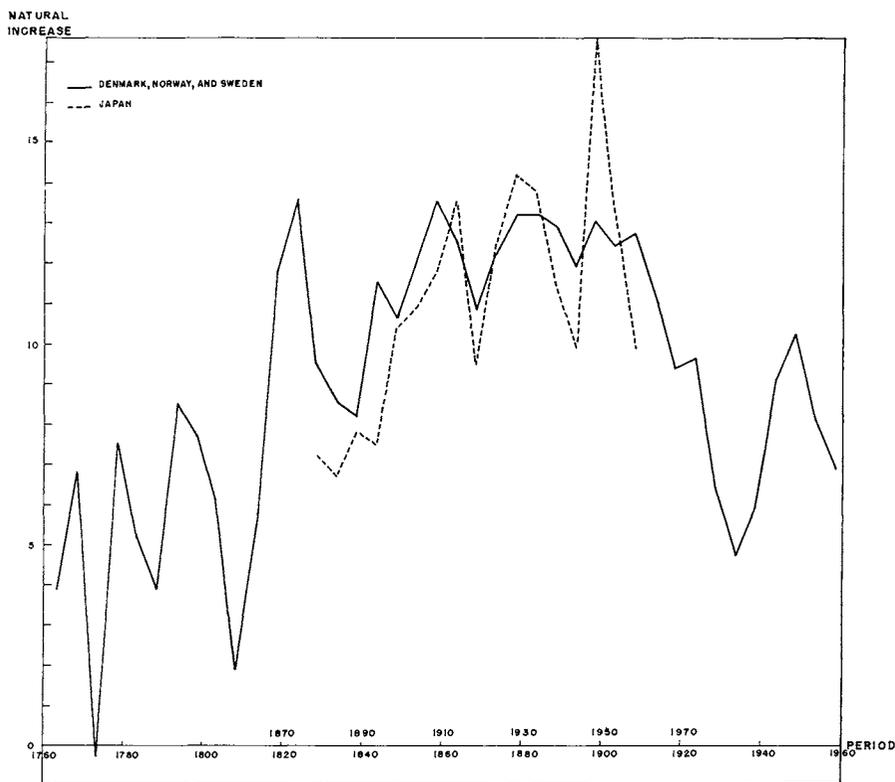


Figure 1. Rates of natural increase in Denmark-Norway-Sweden (averaged) and Japan, with Japan lagged 50 years.

such an explanation—of a type often called upon in demographic theory—has no relation to the facts. During the 45 years from 1913 to 1958 the average rate of growth of industrial output in Japan rose by 5.4 per cent per year, thus exceeding the 5-per-cent rate of Germany, Italy, and the U. S. A. from 1880 to 1913, and greatly exceeding the performance of the United Kingdom and France in any sustained period.¹⁸ Obviously the demographic response of the Japanese is not to be explained in terms of spreading poverty or diminishing resources. Nor were the people influenced in their behavior by concern about national “overpopulation,” for they let their government proclaim a policy of population expansion during the “Co-prosperity” era. In short, an explanation of the vigorous Japanese response to sustained natural increase must account for the antagonism between such increase and prosperity, in terms of behavior prompted by personal rather than national goals.

Was the Northwest European Response Similar? Since the northwest European countries, years ahead of Japan, also had a sustained natural increase, did they manifest a similar multiphasic response? The answer is undeniably yes. Although generally overlooked because of our preoccupation with the contraceptive issue, the fact is that every country in northwest Europe reacted to its persistent excess of births over deaths

with virtually the entire range of possible responses. Regardless of nationality, language, and religion, each industrializing nation tended to postpone marriage, to increase celibacy, to resort to abortion, to practice contraception in some form, and to emigrate overseas. The timing and relative importance of the reactions were not identical in the various countries, and of course methods could not be used that were not then technically feasible for the public at large (e.g., harmless sterilization); but the remarkable thing is that all of the northwest European countries reacted, that they did so in each case with the reappearance of the whole range of responses, and that virtually the entire panorama was later repeated in Japan.

That the stimulus was also similar to that in Japan is clear. Our three Scandinavian countries in Fig. 1 reached a high plateau of natural increase around 1815 and sustained it for more than a hundred years. Since the plateau was reached long before a significant drop in the birth rate occurred, there were about six decades of what was then an unprecedented rate of human multiplication—sufficient to double the population every 61 years in the absence of emigration—before the birth rate began visibly to fall around 1870, and it took another 30 years or so before the drop in fertility could move fast enough to gain on the steadily falling mortality. Periods of substantial increase had of course been known before, but they were brief and virtually self-correcting, since each time the death rate would soon rise again and wipe out the gain. What was unprecedented in northwest Europe was that self-correction was avoided so long over such a wide region. Local catastrophes did occur—as in the Irish potato famine of the 1840's—but it was characteristic of Europe at the time that these were accepted not as inescapable acts of God but as examples of what must be avoided at all costs by collective effort. Northwest Europe was winning the fight against death to a degree never before accomplished, and its success, with the resulting natural increase, explains the desperateness of the subsequent demographic response.

The Theory of How the Stimulus Produces the Response. But how were the stimulus and the response connected? It was not true in Europe, any more than in Japan, that the connecting link was poverty. From 1860 to 1900, the gross domestic product grew on the average at almost 3 per cent per year in Denmark and Sweden, and almost 2 per cent in Norway.¹⁹ When interpreting the effects of sustained population growth, most observers seem to assume that the question concerns the level of living. Was the population growth too fast, they ask, to maintain the general level? If the answer is “no,” interest tends to vanish, because there is no “problem.” If the answer is “yes,” then all sorts of further consequences supposedly follow, because, with growing poverty, human beings must bestir themselves. But, as we have seen, the northwest Europeans and Japanese bestirred themselves in the face of prolonged natural increase without being goaded to do so by rising poverty. The answer to the central question about modern demographic history cannot be posed, then, in the framework of ordinary population theory, which assumes the sole “population factor” to be some relation between the population-resources ratio and the collective level of living. It is doubtful that any question about demographic behavior can be satisfactorily posed in such terms, because human beings are not motivated by the population-resources ratio even when they know about it (which is seldom).

My own view is that no society has been geared to a sustained high rate of natural increase except by conquest. Under a prolonged drop in mortality with industrialization, people in northwest Europe and Japan found that their accustomed demographic behavior was handicapping them in their effort to take advantage of the opportunities being provided by the emerging economy. They accordingly began changing their behavior. Thus it was in a sense the rising prosperity itself, viewed from the standpoint of the individual's desire to get ahead and appear respectable, that forced a modification of his reproductive behavior.

Mortality decline impinged on the individual by enlarging his family. Unless something were done to offset this effect, it gave him, as a child, more siblings with whom to share whatever derived from his parents as well as more likelihood of reckoning with his parents for a longer period of life; and, as an adult, it gave him a more fragmented and more delayed share of the patrimony with which to get married and found his own family, while at the same time it saddled him, in founding that family, with the task of providing for more children—for rearing them, educating them, endowing their marriages, etc.,—in a manner assuring them a status no lower than his. The obligations of marriage and expanded parenthood were not easy, as Banks has shown so convincingly for nineteenth century Britain,²⁰ in a changing society where one's position was threatened from every side and where one's children had to acquire new and costly forms of education. The parent needed to conserve some means for himself, because of longer life-expectancy and because of the importance of capital for seizing opportunities or staving off disaster in the fluid situation of the times.

The inappropriateness of the old demographic behavior was not confined to one segment of society, such as the "middle class" or the towns and cities. Nor was it characteristic of some societies and not others. Whenever and wherever mortality declined on a sustained basis, there the continuation of old demographic patterns brought a train of disadvantages.

Readjustments in the Agricultural Areas

Our view receives an acid test, for example, with respect to the peasantry, because a central tenet of population theory is that farmers lag behind other classes in altering their demographic behavior. We note, however, that the explanations given for this alleged fact are mutually contradictory. On the one hand, it is commonly taken for granted that no adjustment is made by farmers because none is needed: agrarian societies can assimilate natural increase indefinitely, because "children are an asset on the farm." This makes the farmer's unchanging reproductive behavior purely rational. However, it is hard to avoid seeing that a sustained natural increase in a delimited farming area will eventually mean "too many people for the land." This much granted, the theorist may explain rural demographic slowness by saying that farmers feel children to be an asset on the farm. Now, however, the farmer is no longer rational but irrational, and one must find an explanation for his stupidity. This is easy if one assumes that peasants are "traditional in their attitudes." By this route we are led to feel it is natural for modern attitudes and practices to begin in the cities and "diffuse" gradually to the countryside.

Such thinking appears to be a case of a non-existent fact being "explained" by a plethora of unsubstantiated reasons. In Japan and north-west Europe, population increase was especially hard on the peasantry, with the consequence that their reaction was especially drastic. The structure of the rural societies was such that they could accommodate permanently growing populations only on one assumption—territorial expansion. Technological improvement provided no accommodation, because it called for fewer rather than more workers. As capital was increasingly applied to agriculture and the optimum size of farm unit rose, a young man found it more difficult, rather than less, to acquire what was necessary in agriculture to guarantee a satisfactory social status.

Prolonged Natural Increase, Inheritance, and Agriculture. In the absence of long-run natural increase, there is no general problem of rural inheritance. The few parents with numerous surviving offspring are fortunate, for they have not only the child labor but also the eventual old-age security that children can furnish. Their children can receive enough land or substance to marry at a normal age, because each large family is matched by families that have died out entirely or have had only one child survive. Naturally, land and goods flow from the dead to the living in several ways—by purchase prior to death, by collateral relatives in the absence of true heirs, by remarriage of widows—and so large families acquire the means to endow their children for marriage. Without population growth, then, the demographic inequalities of one generation are ironed out in the next. There is no general problem of inheritance but only a problem for an occasional family that has lost out by ineptitude or has no heirs because of misfortune.

When, however, there is a sustained high rate of natural increase, inheritance becomes a chronic problem. Since the proportion of families with numerous surviving children is now much higher, these families are not matched by others that have land but few or no survivors. As a consequence, if their children are given land to marry with, the size of the farm will be reduced; if they are given cash or goods, its capital will be denuded. The parents are reluctant to do either, because they also have to live and, given their now greater life-expectancy, they hang on to the land until much later in the life of the offspring. Young people are forced to postpone marriage, and some to forego it altogether. Thus the strictly agrarian system has very little capacity to absorb population increase.

This inability, be it noted, has nothing to do with "the inheritance system." The latter is concerned solely with the matter of discrimination among potential heirs, whereas our concern is with the growth of the total number of potential heirs (all with their social expectations) in relation to the resources available in agriculture. If there are more heirs than can be accommodated at the expected standard of living with the land available, no inheritance system can itself alter this fact. It can at best decide who gets hurt and who does not. In other words, if there is no sustained natural increase in a settled agrarian area, any system of inheritance will work. If the opposite is the case, then no inheritance system will work, unless, of course, there is some real solution available. Despite the vogue of inheritance systems in population theory, it is doubtful that they play any determinative role in demographic change. Rather, they simply reflect whatever demographic solutions are developed in the society. This view is strengthened when one realizes that

fixed and rigid inheritance systems are figments of the social scientist's imagination. They are not something "laid on," which the people follow in the fashion of automatons; rather, they are fashioned and modified as changing conditions and interests demand.

"Traditionalism" and Agrarian Demographic Response. If historically the peasant communities of Japan and western Europe experienced a sustained natural increase, did they fail to respond successfully because "the peasant was wedded to his traditional value system"? To say so is to commit not only a factual error, as we shall see in a moment, but also a tautology. An explanation in terms of "tradition" has no value in social science, because "tradition" is merely a name for absence of change. A type of social behavior is like the momentum of a moving body: it will not change unless something forces it to change. If the absence of a contrary force is itself not explained, we have no real theory of the persistence but merely another name for it. As for the so-called values, they should be recognized as being a part, or aspect, of the behavior itself and, accordingly, as requiring to be explained rather than being used as the explanation. The fact that people migrate is not explained by their favorable evaluation of migration. By definition, nobody does anything voluntary without some purpose, however vague, in mind. The question of change or persistence is therefore a question of what did or did not act upon the total action (motive-plus-conduct). In other words, to say that fertility continues to be high in some group because of the group's "high-fertility values" is like saying that birds fly because they have wings.

In the case of the European peasantry, however, the alleged fact to be explained—a lack of demographic response—is itself not true. The demographic behavior of the rural population did change, and it changed drastically, because it had to. The common assumption to the contrary seems to arise from our parochial tendency to ignore all changes except the reduction of marital fertility by contraception. If contraception was not at first adopted on a major scale in most of the agrarian sectors, it was because ready alternatives were available. One of these was migration. As the economic revolution advanced, the rural sections found in the rising cities an ever expanding outlet for their excess natural increase—an outlet that helped them to capitalize on the opportunities offered by continued industrialization.

Indeed, as we know, in all of the industrializing countries rural-urban migration removed not only the farmers' natural increase but also a substantial portion of the base population as well. In Japan, for example, Irene Taeuber estimates that, without migration, the rural population of 45.9 million in 1920 would have reached 62.6 million by 1940 instead of the actual 45.5 million.^{/21} The significance of rural-urban migration is that it involves a shift of occupation as well as residence. In fact, members of a farm family can leave agriculture, either part-time or fulltime, without ever leaving home.^{/22} The best indicator of rural migratory adjustment is therefore the diminution of the agricultural labor force. In Great Britain the greatest number of men employed in agriculture was 1.8 million in 1851. A hundred years later, when the total population was nearly 2-1/2 times as great as in 1851, the agricultural male labor force was down to 1.1 million.^{/23} In Japan the population employed in agriculture, given as 15.7 million in 1876,^{/24} was 13.7 million

in 1958/25 when the total population was more than 2-1/2 times greater. Daughters often left the countryside in greater abundance than sons. Village girls in Japan went to work in cities as maids or in factories and shops, typically remaining away for six years, often saving enough to get married either upon returning home or while remaining in distant towns and cities./26

Thus it can hardly be said that rural population in industrializing countries made no demographic response. They responded to sustained natural increase by the drastic process of removing it. Their failure to feature contraception and abortion was not due to "traditional attitudes" (mass migration out of agriculture was not "traditional" either) but to the availability of an alternative which fitted the interests and structure of peasant families in the evolving economy.

The critical moment in the peasant family-cycle, especially in north-west Europe, was the time when the surviving young people were to get married. Up until then their labor was useful on the farms and their consumption limited; but, if they were to marry, they had to have the means (i.e., adequate land) to support a family in a fully adult, independent, and respectable manner. The common process by which reproduction was brought into equilibrium with the agrarian economy was the postponement or hastening of marriage according to the socially defined scarcity or abundance of land. With a prolonged decline in mortality, there were more claimants to land for marriage and a greater reluctance on the part of elders to give it up; but the same progressive forces in the society which were bringing the mortality decline were also opening up opportunities for employment in non-agricultural sectors. The decision to stay in agriculture or to seize these new opportunities was made in the young person's life at about the same time as the marital decision. Indeed, the two decisions—whether to postpone marriage and whether to leave agriculture—were doubtless often made jointly. Leaving agriculture might be the only hope for getting married—as in the case of Japanese girls who had to have a dowry. Migration out of agriculture was thus an adjustment that was congruent with the response-pattern already built into the rural social structure.

This adjustment would not have been available, however, if it had not fitted into and aided the trend of the larger economy. Since industrialization by its very nature requires an exodus from agriculture,27 the fact that economic development was occurring is proof enough that rural-urban migration was being rewarded. Many a farm got desperately needed capital, many a farm-boy or farm-girl achieved matrimony, because of receipts from the city. The adjustment of Japanese and European peasants was clearly not a descent into grim poverty and senseless subdivision; it was not a "resistance to the forces of modernization" in the name of a "traditional value system." It was, on the contrary, a utilization of the new opportunities of the economic revolution.

Delayed Marriage, a Continued Rural Response. The rural populations of industrializing nations did not respond to sustained natural increase by one means alone. In addition to out-migration, they adopted their old mechanism—postponement of marriage—to the new exigences, particularly in regions remote from urban centers. They did this, of course, not as a deliberate effort to reduce fertility or to solve the population problem, but as a response to the complexity and insecurity of the

new requirements for respectable adult status under changing circumstances. In Japan, as noted already, a dowry was required for a girl's marriage. Her farm family, short on land and long on surviving members, needed cash more than it needed girl-power. Japanese factories and offices, on the other hand, needed cheap labor. It was therefore advantageous all around for rural girls to work under supervision, have their salaries returned home, and delay marriage for several years./²⁸ As a consequence, the age at marriage rose almost as fast in the rural areas of Japan as it did in the urban./²⁹ In neither sector was postponement a response to deepening poverty. Agriculture, as well as the total economy, was increasing in productivity./³⁰ In all sectors of the economy, then, families had to grasp the new opportunities of the evolving society or else face relative loss of social status and consumption. Their chances were not improved by demographic behavior that permitted the large family size made possible by declining mortality.

Rural marital postponement was particularly important in the eighteenth and early nineteenth centuries in northwest Europe, because outside opportunities were then too few to make out-migration work as the sole adjustment. Even in England and Wales, the country most conducive to rural-urban migration around 1800, less than 17 per cent of the population resided in places of more than 20,000 inhabitants. If within a decade the natural increase of the rest had migrated to cities, the urban population would have risen by approximately two-thirds instead of the actual one-fourth. In the United States, with plausible assumptions as to differential natural increase, one finds that, had all the natural increase of the farm population and one-half of that of the rural nonfarm population between 1840 and 1850, gone to the urban places, the latter would have increased by approximately 275 per cent during the decade, or three times as fast as they actually did. Apparently, the earlier in economic development the downward trend in mortality occurs, the more difficult it is, other things equal, to avoid solely by out-migration an increase of people on farms./³¹

Even with both marital postponement and rural-urban migration, a decline in farm-size often occurred in areas of northern Europe. A study of twenty villages in southern Poland finds, for example, that the average size fell from 7.24 hectares in 1787 to 3.17 in 1931, "although the whole area owned by peasants increased from 16,966 to 21,558 ha."/ ³² In Ireland there was evidently a similar shift, with the result that by 1841 more than half the holdings were of less than five acres./³³ Even in the United States, in the Southeastern region, the improved acreage per farm fell from 103.6 in 1860 to a low point of 37.9 in 1925./³⁴

Forgetting the possibility of increased yields,/ ³⁵ one tends to view such declines as the consequence of some "inheritance system" or as simply an indication of population pressure and deepening poverty; but they can more properly be viewed, in my opinion, partly as the maintenance of the same product per family with less land and partly as the consequence of a one- or two-generation lag of the adjustment mechanism behind the lowered mortality. That the adjustment mechanisms were there is evident in the twenty Polish villages. As can be seen in line 3 of Table 5, the number of children born per mother, during roughly the period 1872 to 1914, was almost twice on the largest farms what it was on the smallest. This positive association between completed fertility

Table 5. Children Born and Surviving, and Age at Marriage, for Polish Mothers Born between 1855 and 1880, by Size of Farm.

	Land-less	Size of Farm (Hectares)			
		0-1	1-4	4-7	7+
Number of Mothers	9	36	110	31	15
Average Year of Birth for Mothers	1872	1875	1875	1874	1874
Number of Births per Mother	3.9	5.4	6.4	7.7	9.1
Surviving Children per Mother/a	2.9	4.1	5.0	5.9	8.0
Age at Marriage of Mother	31	25	24	22	20
Births per Year from Marriage to Age 45	0.28	0.26	0.30	0.35	0.37
Births per Year from Marriage to Birth of Last Child	0.43	0.35	0.36	0.39	0.41

a/Evidently these were the children who survived to get married.

Source: Stys, W. "The Influence of Economic Conditions on the Fertility of Peasant Women." *Population Studies* 11(2): 136-148. Nov. 1957.

and size of farm has been reported often for peasants.³⁶ In the Polish case, differential mortality adds to the inequality in surviving children, but only slightly. The main factor in the differential fertility and in the number of surviving children alike is the age at marriage (line 5). That there is little limitation within marriage is shown by the sixth line—births per year between a woman's marriage and her 45th year. Comparison of the last two lines suggests, however, that the poorer peasant couples stopped their reproduction earlier (perhaps by abstinence and abortion), or suffered more impaired fecundity; for the births per year between marriage and the last child (last line) show smaller class differences than those between marriage and the woman's 45th birthday (previous line).

The European peasants' response to sustained natural increase clearly reflected a social structure that held married couples responsible for their children. This feature—along with its corollary, postponement of marriage for those incapable of supporting children—was part of the independence and separateness accorded the nuclear family, as opposed to the joint household, in west European society. As such, it went back to medieval and post-medieval times;³⁷ and it tended to yield a later age at marriage than is found in most joint household systems. It did not necessarily produce a late marital age, however, because, with high mortality, individuals so unfortunate as to have to marry late were balanced

by those lucky enough to marry early. When, in the late eighteenth and the nineteenth centuries, the rural areas were faced with a natural increase unprecedented in its size and duration, postponement of marriage appeared as one of the adjustments. This was by no means the only adjustment that enabled the peasants to avoid subdividing land to the point of severe poverty and resurgent mortality. In addition, the peasants maximized migration off the farm, increased permanent celibacy, and curtailed reproduction in the later years of marriage (probably by abortion, folk-contraception, and abstinence). Since, owing to the accelerating economic transformation, rural-urban migration became increasingly available, the forces tending to depress fertility, especially marital fertility, did not need to act so strongly as they did in towns and cities. In the latter places, migration out of agriculture was obviously not a possible alternative. The city-dweller's "migration" into a more lucrative occupation was mainly by acquiring education, skill, experience, and contacts—none of which was helped by an improvident marriage or a high marital fertility. His solution lay more in the direction of contraception and abortion, to which he had better access than the peasant.

Ireland as a Test Case

If correct, our analysis should hold not only for the different social classes but also for the various countries of northwest Europe, even in cases that are commonly regarded as demographically unique. Ireland, for example, is habitually cited as a country having in modern times a population history unlike that of any other nation. Not only did she experience a pronounced decline in population while her neighbors were all showing an unprecedented increase, but she exhibited a tendency toward late marriage and celibacy that strikes many observers as peculiar. On the assumption of uniqueness, particularistic explanations of her demographic history have been given—e.g. that it is a result of the Irish famine, the "land" situation, or extreme religious zeal./38

But how unique is Ireland? It is certainly not unique in having a marital age that was comparatively late to begin with and which grew later in the last half of the nineteenth century. In 1830-40 Irish women married reasonably early for Europeans: the proportion of brides who were under 21 was 28.1 per cent; under 26, it was 66.5 per cent—both proportions similar to those in England and Wales./39 The Irish age at marriage evidently rose after that, reaching its highest point about 1911, at which time it started gradually down, as Table 6 shows. By 1957 the average age at marriage for Irish women was 27.6, only two years above the figure of 25.6 years for women in England and Wales./40

If the late age at marriage in Ireland is to be explained, it must therefore be explained in terms applicable to northwest Europe as a whole. In seventeen countries of that region around 1950, the proportion of brides at first marriage who were age 25 or older was 39.6 per cent, compared to 24.9 per cent in three East European nations and 21.5 per cent in five overseas industrial countries of European origin. The 1959 Irish figure of 53.1 per cent seems abnormally high until we realize that in 1950 the Spanish percentage was 50.4, the Norwegian 50.8, and the Swiss 46.7.

Table 6. Percentage of Women in Young Age Groups Ever Married: Ireland and Sweden.

Ireland				Sweden			
Date	Women			Date	Women		
	15-19	20-24	25-29		15-19	20-24	25-29
-	-	-	-	1750	4.4	27.3	56.6
-	-	-	-	1800	2.7	22.4	51.8
1851/a	0.0	10.0	40.0	1850	0.8	16.5	49.4
1861	2.2	25.3	36.0	-	-	-	-
1871	1.9	21.9	51.0	1870	1.0	15.7	46.2
1891	0.8	14.0	40.9	-	-	-	-
1901	0.6	12.0	37.8/b	1900	1.1	19.6	48.5
1911	0.5	11.6	34.4/b	1910	1.1	19.8	48.6
1926	0.7	13.0	38.2	1920	1.1	20.3	49.3
1936	0.9	13.6	35.9	1930	1.0	19.6	48.3
1946	1.6	17.5	42.4	1945	3.3	36.1	69.7
1951	1.1	17.7	45.4/b	1950	3.7	40.3	73.6

a/ These percentages relate to age groups "under 17," "17-25," and "25-35."

b/ For 1901, 1911, and 1951, data were available only for the age group 25-34. The figures here are our estimates derived by interpolation from earlier and later censuses giving the five age classifications.

Sources: Ireland, *Census of Population, 1946*, Vol. 5, Part 1, p. 34; U.N. *Demographic Yearbook, 1958*, p. 187; *British Sessional Papers, 1856*, Vol. 31, pp. 0-99; *1863*, Vol. 61, p. 616; *1874*, Vol. 74, Part 2, Table 18; *Historisk Statistik för Sverige, Vol. 1, 1720-1950*, (Stockholm; 1955).

Granted that Ireland is part of the late-marrying wing of northwest Europe, one may explain this fact as due to her Roman Catholicism. But for five Catholic countries of the region,⁴¹ the average percentage of first-brides aged 25-plus was 40.4, as compared to 39.1 for ten non-Catholic countries. Even if Catholicism were involved in Irish marital postponement, how would it be? It would certainly not be because the church has an injunction against early marriage. The usual interpretation is that the church defines marriage as second best, and hence gives no powerful encouragement to early marriage; but Belgium, an eminently Catholic country, has an earlier age at first marriage for females than does Norway, Sweden, or Scotland.

The way to understand Ireland's demographic career is hardly in such particularistic terms. Fascination with her late marriage should not blind us to the fact that she responded to long-continued natural increase by other means as well. She responded by permanent celibacy, for example, and here again Ireland was not an isolated case but rather an extreme exemplification of the northwest European pattern, itself extreme. In 1951 some 24.7 per cent of the Irish women aged 45 or more had never

been married. In 1950 the Icelandic figure was 21.5 per cent; the Norwegian and the Scottish, 20.9. The degree to which Europe stands out can be seen from the following:

Table 7. Regional Comparison of Average Percentage Single of Women Aged 45 or Over.

	Number of Countries	Average Percentage Single among Women Aged 45 or Over
European/a	30	12.6
Northwest European	22	15.7
Catholic	8	16.6
Non-Catholic	14	15.2
Eastern European	8	4.0
Overseas European Industrial	5	9.8
Moslem (North Africa, Turkey, Pakistan)	7	2.0
Asian	12	2.2

a/Ireland is excluded throughout. If added, the northwest Europe average rises to 16.1 and the "Catholic" group to 17.5. France is counted as a "non-Catholic" country. Scotland, Northern Ireland, and England and Wales are counted as separate countries.

Source of data: United Nations Demographic Yearbook 1960, Table 10.

A third Irish response—again typically west-European and extreme in character—was the very high and prolonged rate of out-migration. The peak of Irish-born living abroad was reached about 1880, when, in four countries alone, they represented 60 per cent as many as lived in Ireland itself. From 1901 to 1956 the net emigration came to an estimated 1.34 million, an average of 24,300 per year. The loss during 1946 to 1956 amounted to about half the number of births./42

It is commonly claimed that the Irish postponed marriage or migrated as an alternative to practicing birth control within marriage. However, as Glass has noted,/43 data from the 1946 census show class differences in marital fertility. Furthermore, a decline of 25 per cent occurred in overall marital fertility between 1911 and 1946. Couples in Ireland, as elsewhere in Europe, were apparently taking to birth control, though not to the same extent as in neighboring countries. One should note, of course, that a shift to a later age at marriage, other things equal, will independently bring a reduction in marital fertility by pushing a greater part of the marital exposure into the less fecund years of the reproductive span. It will cause an additional loss through the greater proportion of women who die before marrying. For these reasons the influence of a shift in the age at marriage is greater than the simple proportion of the reproductive years added or eliminated.

If, then, Ireland exhibited a multiphasic response similar to that shown by her neighbors, differing from theirs only in the relative emphasis placed on the various means and in its vigor—so drastic that it halved the absolute population within 80 years—the explanation must be in terms applicable to the rest of the region. A significant fact is that Ireland was, and has to a considerable degree remained, a rural part of northwest Europe. It was a rural backland when it belonged to Great Britain, and after its independence in 1922 it was cut off from its most industrial section, the northern six counties—much as if Mississippi, Arkansas, and Louisiana were given their independence but with New Orleans and the rest of the Gulf Coast removed. A late age at marriage, as we have seen, was particularly characteristic of rural northwest Europe; and in Ireland it prevailed more in the rural areas than in the towns. Ireland's continued rurality, together with the circumstance that Catholicism became a symbol and rallying point of Irish Nationalism as against the Protestant British, enabled the Catholic clergy to remain strong. Being in control to an unusual degree, the celibate clergy could implement its ascetic supervision over courtship and instill its negative attitude toward marriage, including state enforcement of the indissolubility of wedlock. It thus gave its blessing to marital postponement and lay celibacy, and at the same time kept down illegitimate fertility. Concomitantly, the exceptional power of the clergy tended, as in other Catholic countries, to discourage economic development and thus to keep the area rural.

As an agrarian region, Ireland partook of the exodus out of agriculture that accompanies modern economic development—except that, without economic development in its own territory, the migration out of agriculture was simultaneously a migration out of Ireland. In other words, international and overseas migration and rural-urban migration were one and the same thing for Ireland. The lack of economic opportunity at home powerfully discouraged marriage, while ecclesiastical determination of family, criminal, customs, and censorship laws made abortions, contraceptive materials, and birth control information and services difficult to obtain. Marriage tended to be postponed not only because the economic requirements for it were hard to secure, not only because it could not be dissolved if it proved personally obnoxious, but also because it was likely to lead to several children. In addition, clerical control, poor economic development, and rural community opinion worked together to discourage married women from entering the labor force, thus reducing still more the economic support for marriage. Recently the proportion of married women aged 15 to 65 in paid employment was less than 3 per cent, as compared to about 23 per cent in England and Wales./44

Ireland thus manifests a combination of the demographic responses of Europe, extreme in its totality and in its result but composed of familiar strands indeed, all understandable under the circumstances. It thus illustrates the principle that the explanation of as fundamental a feature of society as its demographic changes is not to be found in some inflexible biological or economic law or in some particularistic cultural idiosyncrasy, but rather in the main features of the operating social organization on the one hand and, on the other, in the changing conditions which arise from past performance and the altering international politico-economic environment.

Conclusion

My thesis is that, faced with a persistent high rate of natural increase resulting from past success in controlling mortality, families tended to use every demographic means possible to maximize their new opportunities and to avoid relative loss of status. An understanding of this process in population theory has been hindered by a failure to see the multiphasic character of the response and by an interpretation of demographic behavior as a response either to absolute need or to some cultural idiosyncrasy such as a particular "value system" or "custom." When the demographic history of industrialized nations is analyzed comparatively, an amazing similarity of the response syndrome seems to me to emerge. An explanation of a country's demographic behavior by reference to a peculiarity or accident of its culture fails to cope with this basic similarity of response. Curiously, we do not adopt such an easy way out with respect to mortality. We do not "explain" India's high death rate and Sweden's low death rate by saying that the one "values" high mortality and the other low mortality. Yet we sometimes come perilously close to this in regard to other aspects of human demography, especially fertility.

As for the view that the motivational linkage between change and response depends on fear of absolute poverty, we have seen that it fails to account for the fact that the multiphasic effort to reduce population growth occurs simultaneously with a spectacular economic growth. Fear of hunger as a principal motive may fit some groups in an extreme stage of social disorganization or at a particular moment of crisis, but it fits none with which I am familiar and certainly none of the advanced peoples of western Europe and Japan. The fear of invidious deprivation apparently has greater force, and hence the absolute level of living acts more as an environmental condition than as a subjective stimulus. If each family is concerned with its prospective standing in comparison to other families within its reference group, we can understand why the peoples of the industrializing and hence prospering countries altered their demographic behavior in numerous ways that had the effect of reducing the population growth brought about by lowered mortality.

FOOTNOTES

- 1/ Kimura, Masabumi. "A Review of Induced Abortion Surveys in Japan." Paper No. 43 in mimeographed proceedings of the 1961 conference of the International Union for the Scientific Study of Population. P. 1.
- 2/ Muramatsu, Minoru. "Effect of Induced Abortion on the Reduction of Births in Japan." Milbank Memorial Fund Quarterly 38:152-166. April 1960.
- 3/ Glass, D. V. Population Policies and Movements in Europe. Oxford, Clarendon Press, 1940. Pp. 278-280. Other health-fund data showed more abortions than births in the late 1920's.
- 4/ Ibid., pp. 444-445.

- 5/ Bachi, Roberto, and Judah Matras. "Contraception and Induced Abortions among Jewish Maternity Cases in Israel." Milbank Memorial Fund Quarterly 40(2):207-229. April 1962. P. 227.
- 6/ See: Tietze, Christopher. "Legal Abortion in Eastern Europe." Journal of the American Medical Association 175:1149-1154. April 1, 1961; Idem. "The Demographic Significance of Legal Abortion in Eastern Europe." Paper presented at annual meeting, Population Association of America, April 25-27, 1963. Mimeographed.
- 7/ Tietze, op. cit., pp. 1149-1154.
- 8/ In Denmark and Sweden, 1953-57, there were only 6 or 7 deaths per 10,000 legal abortions. See: Tietze, Christopher. "The Current Status of Fertility Control." Law and Contemporary Problems, Vol. 25, Summer 1960. P. 442.
- 9/ The combined rate would doubtless remain even more unchanging if the number of unregistered abortions were known.
- 10/ According to surveys in 1949-50 and 1953-54, the gestation preceding abortions in Japan lasted between 9 and 11 weeks, depending on the order of the abortion. Kimura, op. cit., pp. 3, 9.
- 11/ Taeuber, Irene B. The Population of Japan. Princeton, Princeton University Press, 1958. P. 274.
- 12/ Legitimate births per 1000 married women:
- | Age of married women | 1950 rate as % of 1925 rate |
|----------------------|-----------------------------|
| 15-19 | 92.8 |
| 20-24 | 96.4 |
| 25-29 | 93.3 |
| 30-34 | 75.4 |
| 35-39 | 54.4 |
| 40-44 | 40.5 |
| 45-49 | 13.2 |
- Derived from data in: Taeuber, op. cit., p. 265.
- 13/ Ibid., pp. 278-282.
- 14/ Ibid., p. 203.
- 15/ Annual gross reproduction rates, 1920-55 from: Taeuber, op. cit., p. 232. Annual gross reproduction rates, 1956-59 from: Population Index 28(2):205. April 1962.
- 16/ Taeuber, op. cit., pp. 50-51.
- 17/ Dudley Kirk pointed out in 1944 the similarity between the Japanese birth and death rates of 1921-41 and those of England and Wales in 1880-1900. ("Population Changes in the Postwar World." American Sociological Review, Vol. 9, Feb. 1944. P. 34.)
- 18/ Patel, Surendra J. "Rates of Industrial Growth in the Last Century, 1860-1958." Economic Development and Cultural Change, Vol. 9, April 1961. Pp. 317-318.
- 19/ Based on average rates of growth over various specified periods, with constant prices, as given in: Kuznets, Simon. "Quantitative Aspects of the Economic Growth of Nations: VI. Long-term Trends

- in Capital Formation Proportions." Economic Development and Cultural Change, Vol. 9, July 1961. Pp. 76, 82, 88.
- 20/ Banks, J. A. Prosperity and Parenthood. London, Routledge and Kegan Paul, 1954.
- 21/ Op. cit., p. 145. Dr. Taeuber shows, p. 71, that the communes of less than 10,000 inhabitants—which in 1930 had 68.1 per cent of their occupied population in agriculture—lost 4.6 per cent of their population between 1920 and 1940, while the whole nation gained by 31.0 per cent. Since the farmland of Japan was densely settled already, "absorption of additional population would have jeopardized economic well-being, social organization, and political stability. The preservation of the status quo required the exodus of younger sons and daughters to urban areas and non-agricultural employment." (p. 73.)
- 22/ As a consequence, agricultural density may be highest in those areas where the most farmers are only part-time in that occupation. See: Ishino, Iwao, and John W. Bennett. Types of the Japanese Rural Economy. Columbus, Ohio State University Research Foundation, 1953. Mimeographed. Pp. 24-25.
- 23/ Mitchell, B. R., and Phyllis Dean. Abstract of British Historical Statistics. Cambridge, University Press, 1962. Pp. 60-61. For data on agriculture's diminishing proportion of the labor force in the evolution of industrial countries, see: Kuznets, Simon. "Industrial Distribution of National Product and Labor Force." Economic Development and Cultural Change, Supplement to Vol. 5, No. 4, July 1957. Appendix Table 4.
- 24/ Ishii, Ryoichi. Population Pressure and Economic Life in Japan. Chicago, University of Chicago Press [1937]. P. 78.
- 25/ Japan. Ministry of Foreign Affairs. Statistical Survey of Economy of Japan 1959. P. 9.
- 26/ In one village of 2,752 population in 1948, a total of 58 girls were working in the city. Out of 72 girls marrying in 1948, 16 married men in distant cities and towns. (Ishino and Bennett, op. cit., p. 91.) The exodus of women out of agriculture in Japan is shown by the fact that the younger the age group, the smaller the proportion in farming. (Taeuber, op. cit., p. 94.)
- 27/ Davis, K. "The Role of Class Mobility in Economic Development." Population Review, Vol. 6, July 1962. Pp. 67-73; and Idem. "Internal Migration and Urbanization in Relation to Economic Development." Proceedings of the World Population Conference 1954, Vol. 2. New York, 1955. Pp. 783-801.
- 28/ In 1930 some 435,800 girls, representing 4.2 per cent of the female labor force, lived in factory dormitories. (Taeuber, op. cit., pp. 87, 116.)
- 29/ The proportion of women ever married by age was:

Age Group	Shi (Towns and Cities)			Gun (Small Towns and Villages)		
	1920	1935	Ratio	1920	1935	Ratio
15-19	13.5	5.7	2.4	18.9	8.7	2.2
20-24	60.6	48.4	1.3	70.9	59.8	1.2

Age Group	Shi (Towns and Cities)			Gun (Small Towns and Villages)		
	1920	1935	Ratio	1920	1935	Ratio
25-29	86.3	85.4	1.0	92.0	91.1	1.0

From: Taeuber, *op. cit.*, p. 211.

- 30/ Between 1878-82 and 1913-17, land productivity in Japan rose by 80 per cent and labor productivity in agriculture by 136 per cent. See: Ohkawa, Kazushi, and Henry Rosovsky. "The Role of Agriculture in Modern Japanese Economic Development." Economic Development and Cultural Change 9(1, part 2), Oct. 1960. P. 46.
- 31/ When the urban sector is small and the farm sector large, a rural-urban migratory stream that is big from the standpoint of cities will be insignificant from the standpoint of the countryside. See: Davis, K. "Internal Migration and Urbanization in Relation to Economic Development," *loc. cit.* However, it should be clear that there are other variables. One is the magnitude of the rural natural increase, which is greater today in underdeveloped countries than it was in nineteenth century Europe. This means that, given the same rural-urban distribution of the population, the out-migration from agriculture has a greater burden to carry in currently underdeveloped countries. See: Davis, K. "Urbanization in India: Past and Future." Turner, Roy, Editor. India's Urban Future. Berkeley, University of California Press, 1962.
- 32/ Stys, W. "The Influence of Economic Conditions on the Fertility of Peasant Women." Population Studies 11(2):136-148. Nov. 1957. The change is graphically shown, p. 148, by two maps of the farms in the area at the beginning and at the end of the period.
- 33/ My estimate based on: Connell, K. H. The Population of Ireland, 1750-1845. Oxford, Clarendon Press, 1950. Pp. 163-164.
- 34/ Vance, Rupert B. All These People. Chapel Hill, University of North Carolina Press, 1945. P. 164.
- 35/ Per-acre productivity rose early in the economic transformation because of shifts in land-use, better methods and instruments of tillage, and higher-yielding types of plants and animals. Thus "during the eighteenth century the traditional bias of Irish agriculture towards grazing had shifted to tillage," methods of tillage were steadily improved; and the potato, introduced in the sixteenth century and yielding more calories per acre than any other plant, became the main food crop. See: Green, E. R. R. "Agriculture." P. 90 in: Edwards, R. Dudley, and T. D. Williams, Editors. The Great Famine. New York, New York University Press, 1957. Also, Connell, *op. cit.*, pp. 136, 158-159.
- 36/ E. g., Tsarist Russia, China between the two world wars, Japan in 1940, Bulgaria, and India. See in particular: Skinner, G. Wm. "A Study in Miniature of Chinese Population." Population Studies 5(2): 98-103. Nov. 1951; United Nations. The Mysore Population Study. New York, 1961. P. 86; Okazaki, A. Investigation on Differential Fertility. Japan, Welfare Ministry, Institute of Population Problems, Research Data, B, No. 2. Additional references, with tabular data for Germany and China, are in: Stys, *op. cit.*, pp. 143-144.

- 37/ For evidence, references, and discussion, see: Davis, K., and J. Blake. "Social Structure and Fertility: An Analytic Framework." Economic Development and Cultural Change 4(3):214-218. April 1956.
- 38/ Honohan believes that the famine created "in the minds of the people a hard-headed and somewhat irrational scepticism in regard to the prospects and permanence of material betterment in Ireland," and that "a strong religious faith" led to resistance to trends that developed elsewhere. (W. A. Honohan. "The Population of Ireland." Journal of the Institute of Actuaries 86(1, 372):30-49, 1960. Pp. 48-49.) He does not explain, however, why a famine should have an effect different in Ireland from the effect in India, why this attitude should last for a century, or why the Irish should happen to have such a strong religious faith. If the Irish were hard-headedly sceptical about future prospects in Ireland, why were they not also sceptical about the Roman clergy?
- 39/ Connell, op. cit., p. 39.
- 40/ Honohan, op. cit., p. 37.
- 41/ Austria, Belgium, Italy, Portugal, Spain. France is omitted because of some question about its being a "Catholic country."
- 42/ Honohan, op. cit., p. 42.
- 43/ Glass, David V. "Malthus and the Limitation of Population Growth." In: Glass, D. V., Editor. Introduction to Malthus. New York, Wiley, 1953. Pp. 35-37.
- 44/ Honohan, op. cit., p. 39.

CURRENT FERTILITY EXPECTATIONS OF MARRIED COUPLES IN THE UNITED STATES

Actual fertility and the future fertility expectations of American couples in the childbearing years are being followed by a time-series project conducted at the Population Studies Center of The University of Michigan. In the January 1963 issue of Population Index this project was described in detail./1

This, the second report, consists of results based on interviews taken from a national sample in three separate studies conducted by the Survey Research Center of The University of Michigan in May, August, and November 1962. The cases from all three are combined to get a composite picture for the year 1962. A total of 1402 interviews were taken with couples where the wife was less than 40 years old and living with her husband. Approximately half the respondents were male and half, female. These interviews were merged for this report, as in the first one, as representing in general the couples' fertility expectations./2. The questions asked to obtain information on number of births and expected family size are similar to those in the 1955 and 1960 Growth of American Family (GAF) studies,^{/3} and thus it is possible to compare these series to locate possible trends, and to test past predictions.