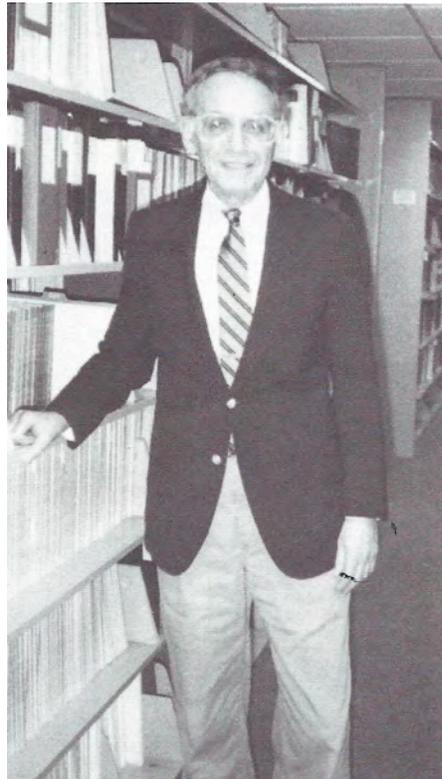


# **DEMOGRAPHIC DESTINIES**

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

### **Interview with George F. Stolnitz PAA President in 1983**



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

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

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

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

## GEORGE F. STOLNITZ

PAA President in 1983 (No. 46). Interview with Jean van der Tak at the Population Reference Bureau, Washington, D.C., January 20, 1988.

**CAREER HIGHLIGHTS:** George Stolnitz was born in 1920 in the Bronx, New York City, where he also grew up. He received his B.A. in economics from the City College of New York in 1939, worked a year with the New York City Department of Health, another year with the Census Bureau, and then went to Princeton as the third Milbank Memorial Fund Fellow (following John Durand and Ansley Coale) in the Office of Population Research, where he received his M.A. in economics and demography in 1942. Returning to Princeton in 1946 following wartime service in the Air Force, he was a research associate at OPR, received his Ph.D. in economics and demography in 1952, and taught economics until 1956. Since 1956, he has been at Indiana University at Bloomington, where he has been Professor of Economics, Director of the International Development Research Institute, and continues as first Director (since 1986) of the Population Institute for Research and Training. As a demographer and development economist, he has been a consultant to many agencies and institutions, including the Agency for International Development, the Departments of Health and Human Services, of Energy, and of Defense, NICHD, and other U.S. government agencies, the Ford and Rockefeller Foundations, the United Nations, and UNFPA. He has served on the Technical Advisory Committee of the Census Bureau and on the committee for the 1960 census monograph series. He has been a senior research associate with and later consultant to the population activities unit of the Economic Commission for Europe in Geneva and a principal officer for population and development at the UN Population Division in New York. He has also been a visiting scholar at Resources for the Future and at the Population Reference Bureau in Washington, D.C. In his research and writing, he is particularly well known for his perceptive work in the area of mortality and he has also worked and published on economic-demographic interrelations, migration, and world population trends. [Dr. Stolnitz died in Bloomington in 2001.]

**VDT:** We are speaking at the Population Reference Bureau in Washington, D.C., where George is the Andrew Mellon Visiting Scholar for 1987-88. George, what led to your interest in demography? You started out in economics. You must have been one of the early breeds of economists who were interested in demography. Traditionally, many of the earliest demographers in PAA came out of sociology.

**STOLNITZ:** An interesting exception to that is Frank Notestein. He came out of economics at Cornell [Ph.D. in social statistics, 1927] and studied under a leading economist of that period, H.J. Davenport.

How did I get into population? It was in a sense by happenstance. I took no population course as an undergraduate at the City College of New York; I don't think any such course was offered, as a matter of fact. The day after I graduated, in the late spring of 1939, a bad time to get a job, I was by chance able to land a job with the New York City Department of Health as a sort of population statistician. Basically, my work was with vital statistics--mortality, morbidity, and so on--by health district: how many births, how many deaths. It wasn't profound demography and the job was for one year until the person I was replacing returned.

Then I got a civil service job with the U.S. Bureau of the Census, working on the 1940 census. This is what really started me in demography. It brought me to Washington, where my first boss, the one who had the biggest influence on me--Abe Jaffe--introduced me to historical and contemporary demographic research analysis on the 1940 census and historical censuses.

After a year, I decided to go back to graduate school. This was in 1941 and I was accepted in the Harvard economics department. On a Thursday when I was all set to go to Harvard the following Monday, Abe Jaffe asked if I'd be interested in going to Princeton; they had an immediate opening for a Milbank Fund fellowship. That very Friday I went to Princeton, Frank Notestein and I met, and by Friday evening I was a Milbank Fellow. Monday I showed up at Princeton rather than Harvard. That's really how I got into population seriously.

**VDT:** You had come to the seat of population training at that time.

**STOLNITZ:** That's right. Those were war years and there weren't many people around in 1941 and 1942. I managed to earn my M.A. in economics in 1942, went away to serve in the Air Force, and came back to the Office of Population Research in 1946. I resumed working on classes, taking more courses at OPR, no longer as a Milbank Fellow but one supported by government grants to the military.

Then I became a research associate at OPR, where I did my thesis on a mortality subject--how to derive a life table from a single census only, namely, assuming no information that you could use on deaths by age. This was then, and still is, the situation in quite a few Third World countries. If you want to get an approximate life table under such circumstances, how would you do it? That was the thesis I did under Frank Notestein and a number of economists in the department, with outside evaluation by Mortimer Spiegelman of the Metropolitan Life Insurance Company.

I received my Ph.D. in 1952, taught at Princeton for three years in the department of economics, and then went to Indiana University, where I've been teaching economics of various kinds, mainly development economics, population courses, and graduate economic statistics. In the late 1960s and early 1970s, I headed up the International Development Research Center at Indiana for about six years. In 1986, I became director of the Population Institute for Research and Training; the acronym is PIRT, which may be the best contribution I've made so far for the Institute. Happily, we received both inside--Indiana University--and outside--Hewlett Foundation--support at an early stage; the future of the Institute appears promising.

Beyond this, I have consulted widely on population with a variety of agencies and institutions. In the case of government, it's been the State Department, AID in particular, for which I did four missions in India.

**VDT:** What kind of missions?

**STOLNITZ:** In India, the mission essentially was to see about the status of family planning programs, the state of population knowledge there, what kind of policy prescriptions seemed to be working and which were not working. I went there in 1967 and 1968 and again in 1969 and 1970--about four years running.

I was also on the Technical Advisory Committee for the Bureau of the Census in the late 1960s, early 1970s, for about five years; served on the 1960 Census Monograph Committee for the Social Science Research Council; have consulted with the Ford Foundation, the Rockefeller Foundation; also for the Department of Health and Human Services; and have been for years on review panels for the National Institute for Child Health and Human Development. In other economic connections, I've long consulted with other U.S. government departments, such as Energy, Defense, and so on.

**VDT:** What did you do for them?

**STOLNITZ:** I've consulted with them on population policy, on economic-demographic interrelations programming, served on review panels for them, consulted with respect to the kinds of steps they

should take in the teaching of economics and population, or, rather, the teaching of population with adequate attention to economics, which is the area I'm most specialized in.

In more recent years, I've done a number of things for the UN, both in New York and Geneva. I was a principal officer for population and development at the Population Division in New York from 1976 to 1978, where I authored its 1977 Concise Report on the World Population Situation: New Beginnings and Uncertain Ends. In 1974 I was a senior research associate with the population activities unit of the Economic Commission for Europe in Geneva, where I wrote the first half of the supplement to its 1974 annual survey, dealing with Europe's population outlook to the year 2000. Since that time, I've consulted with that unit on aging and population change with respect to health care, household approaches to aging analysis, labor market-aging research needs, "echo" effects on current aging tendencies in developed regions, and several other things.

Still other activities? At Resources for the Future where I was a visiting scholar, I developed a system for analyzing migration movements among regions and industries and combinations of the two by using multi-year Social Security source materials to derive longitudinal conclusions, that is, tracing individuals over successive years--data which I believe I pioneered in. This was especially useful and uniquely indicative in that I could trace people longitudinally, with respect to incomes, doing so on a scale never before traceable for such large numbers and categories of population.

**VDT:** You're talking about U.S. data?

**STOLNITZ:** Yes, the data that the U.S. Social Security Administration keeps confidentially for individuals by their true Social Security number. They have the confidentiality problem and I wanted to trace individuals from annual tapes which required identification numbers so I could say, "This is what individual A was doing in 1960 and also what he or she was doing in 1968." Well, the Social Security Administration had to be--and became--persuaded to work out a system whereby they used garbled numbers in place of the social security identification numbers. Each individual A with a true social security number X was given a consistent garbled number Y. This enabled me to trace about a million people in the 1960s by industry, counties, and combinations of the two with respect to economic determinants and migration consequences. So far as I know, no one else has done that on a comparable scale. I still think--I'm stubborn about these things--I would want to do another such study, updated, before I'm through researching.

**VDT:** Could you do this by industry and much else?

**STOLNITZ:** I could trace individuals by industry down to four-digit levels by counties and in ways that couldn't be shown from any other source--by economic earnings, cross-classified by whether they moved or didn't move, stayed or didn't stay in the same industry or region; I could do so by race, age, and sex subgroupings of the population.

**VDT:** All this was attached to the garbled number you had?

**STOLNITZ:** Yes. An individual with a garbled social security number in 1960, say, was of such and such race, sex, age, lived in such and such county, worked in an industry of four-digit level--which is very detailed--earned so much through wages or self-employment, and this information was available at all successive dates under study. Moreover, this could be identified for combinations of counties in ways that no other source could provide--by regional groupings, of course, but also functional groupings like metropolitan and nonmetropolitan areas, urban/rural areas, central city and suburban counties, and so on, and similarly for industries and income levels. This provided insights into migration movements of the labor force--as distinct from population--that you couldn't get in any other

way.

**VDT:** You said in your PAA presidential address of 1983 ["Three to Five Main Challenges to Demographic Research," published in Demography, November 1983] that internal migration was one of the neglected areas of research, in part because one does not have the data. This was obviously an ingenious way of getting at it.

That leads me back to another question: How did you get into your specialties of mortality and migration? You mentioned that your Ph.D. dissertation already was on life expectancy derived from a single census. How did that happen?

**STOLNITZ:** Serendipity. One day at OPR I was thinking about some problem about Brazilian data. I needed national mortality data, but Brazil at that time had no reliable mortality data for the country as a whole. I knew Brazil had censuses going way back and was well acquainted with the work of Giorgio Mortara, the Italian demographer, who had reconstructed demographic times series for Brazil going back to the 19th century, based on censuses.

**VDT:** You knew him already as a graduate student?

**STOLNITZ:** I knew of him. In fact, I may have been the first American demographer, student or otherwise, who read his original articles in Portuguese. It's a semi-obstacle; with demography and just a bit of Portuguese you can do all right. What I did was ask myself: What could I do from a single census--which was basically all I had--to say something about mortality?

**VDT:** What census were you using?

**STOLNITZ:** At that time, I was using only the 1940 census for Brazil, and then the 1950 census, similarly. But I didn't want to use the inter-censal method, because it was too unreliable if you shifted from one to the other, since there was a lot of migration. Therefore, I just used the 1940 census to work with. I asked myself: What can I make out of this in terms of reconstruction for a life table? That turned out to be my thesis, and that's how I got into mortality.

At that point I got into mortality quite a lot with respect to international comparisons, as in one of the early issues of Population Index, in those short articles they have at the beginning. Then I looked at Third World life expectancy and found out that things were happening that had never happened before in history. In doing the Brazilian pieces, I had absorbed enough background in historical life tables that I had a sense of what had happened in terms of quantitative movements. I kept finding trends I'd never found before.

**VDT:** Now you're talking about your Ph.D. thesis, which was in the early 1950s?

**STOLNITZ:** Right. In that period I had had occasion to work on historical populations, because in the late 1940s and early 1950s, all you had was prewar data and going back. I couldn't use anything from Brazil that I knew of and I had the Brazilian census, so what to do? In a Mortara article, I found he did all kinds of wondrous things from single censuses, and I asked myself: How could I reconstruct a life table? I worked out a system and that became the thesis. Since I had to use comparison life tables at my disposal, I looked at data from Sri Lanka (then Ceylon), Jamaica, Trinidad and Tobago--about five to ten Third World countries, which was all that you had where you could believe the data. And I asked myself: How come what's happening there is so different, so remarkably revolutionary, as compared to what the West went through? That's how I got started after the thesis and put out a series of articles on international comparisons of life expectancy.

**VDT:** You apparently became one of the first to point out that the postwar mortality decline, which was happening so fast in developing countries, was probably due to public health technology, which was imported, and not to socioeconomic development, which is what the demographic transition had traditionally said.

**STOLNITZ:** There is another especially interesting aspect there. In doing this, I came up with the notion that demographic transition formulations, including theory, really didn't have much explanatory power with respect to mortality and, moreover, either didn't have any predictive power or very limited predictive power beyond that. Mortality was going to go down; that wasn't any large mystery. But if you asked, when would it start, how fast would it go, where would it go next, transition theory just wasn't getting us very far. If you go back to an article of mine in 1955, I think you'll find one of the early critiques of demographic transition theory.

**VDT:** Is that the one in the Milbank Memorial Fund Quarterly ["Comparisons Between Some Recent Mortality Trends in Underdeveloped Areas and Historical Trends in the West," MMFQ, 1956]?

**STOLNITZ:** Yes, you'll find it there, but one that comes particularly to mind is in the Population Studies of July 1955 and then there's a follow-up in July 1956 [together titled: "A Century of International Mortality Trends"]. I think it's in 1955, about a half page where I really get at it. Frank Notestein, I must say, wasn't too happy.

**VDT:** I would think so. There he was: your boss, your professor, one of the prime originators of the demographic transition theory, and you were criticizing the mortality side. Everyone was a little uncertain about the fertility--but the mortality! That brings me to your impressions of Frank Notestein, one of the greats both in PAA and in the beginnings of demography in the U.S. What was he like to work with and be a student of?

**STOLNITZ:** An excellent teacher. That I was a student was almost not a real situation; it was basically just Frank and myself. It was a war year when I took the courses and a lot of men were off to war--Princeton at that time was male only. So it was one on one, but we went through a rigorous workout, basically on fertility. Frank Notestein--I picture him still--I pictured him then as a research or academic statesman, less concerned really with technical details and not really very much up on it, because in his day one didn't have to have advanced mathematics, calculus, to be a leading demographer. Leading demographers of that time--Irene Taeuber, Frank Lorimer, and so on--didn't work at the calculus level of mathematics or advanced statistics. But he was very keen on sizing up political situations, interrelations, and some of his work on the factors making for demographic transition, I think, still stand up as among the very best in the field.

**VDT:** Some of the factors? You came around to criticizing his demographic transition theory.

**STOLNITZ:** It involved what I regarded and still regard as "theory," something explanatory of quantitatively specifiable relations. One of my criticisms was that transition doctrines didn't have theory in terms of a quantitative or modeling formulation and not much of theory in a predictive sense, except directionally. Presumably fertility would come down rather than go up and stay up, and ditto for mortality. But where it would start, why it would start, how fast it would go, and what the longer-run prospects were--for none of these did you really have a theory or body of theories. What you had was a set of descriptions, which came down to saying that before modernization birth and death rates were high, and that with modernization in adequate amounts, birth and death rates became low--that's

directional--but in terms of when and even why was not deeply clarified. That was my sense of demographic theory at the time and I don't think it's much more advanced now. I wrote an article on this for a book Ron Freedman edited, World Population: The Vital Revolution [1964].

Since that time, the Princeton European Fertility Project, which was directed by another Notestein protege, Ansley Coale, has come out much more than I did in terms of stating that the theory doesn't seem to work well in terms of the socioeconomic variables that transition analysts stressed. If you looked at smaller areas, say provinces, you found areas that moved faster than other areas but you did not find that this correlated well with socioeconomic levels or trends. There's a famous case involving Spain, where a student working on its demographic transition came up with a cross-sectional pattern which didn't have satisfactory ascertainable socioeconomic correlations. He happened to go to a linguist who said, "What you have here is a linguistic separation between those that moved transitionally and those that didn't change or changed less." So all of a sudden cultural aspects became prominent, which were not prominent in the early days of transition descriptions, even in Frank Lorimer's early writings or in Warren Thompson's, the latter being the earliest transition identifier, really before Frank Notestein. After Notestein, it began to come out largely with his proteges, although it's gone far beyond that since. The field has become explosive in terms of numbers.

That's one point I want to stress to you: the difference in research scales between then and now. At one time, almost everybody who was anybody in population came by OPR at Princeton; even despite the war, pretty much they all came by. So you knew them by name and, as a matter of fact, knew them personally. Now it's become enormously larger, so there are lots of people who are very prominent in the field who don't personally know lots of other people who are also very prominent in their own bailiwick. Or they know each other by name only, if that. As a youngster starting out today, you'd know almost nobody in the field personally. That was not the case in those days. I knew Lorimer, Irene Taeuber, Frank Notestein, and soon met very nearly all the then "greats."

**VDT:** May I ask you about some of those "greats" that you knew personally? Frank Lorimer, for instance; he died in New Zealand just a year or so ago at over 90, still writing.

**STOLNITZ:** I knew Frank Lorimer in 1940 when I worked here at the Bureau of the Census for a year, before I went to graduate school. In that year, I took a course at American University given by Frank Lorimer, so that's when I first got to know him. Then, of course, I came to know him much better through OPR, where he was working on his book on the Russian population [The Population of the Soviet Union: History and Prospects, 1946]. Irene Taeuber, of course, worked directly with the OPR.

**VDT:** She lived in Washington; did she travel up regularly?

**STOLNITZ:** Yes. She was involved with [co-editor of] the Index and often wrote introductory articles on timely topics for the Population Index. She was working on Japan at that time. I remember distinctly many talks we had on what was happening in Japan. Was Japan really a breadbasket case which would never develop because of limited land, less than in California, with umpteen tens of millions of population? How could it possibly survive? This was around 1950. Times have changed!

I got to know Henry Pratt Fairchild during my OPR days, and Clyde Kiser, whose wife Louise worked there; Louise was there all the time. We had Milbank contacts; we knew all the Milbank people through the Milbank Memorial Fund annual meetings. There was almost nobody you didn't know. Also, the annual PAA meetings were much smaller.

**VDT:** What kind of person was Frank Lorimer?

**STOLNITZ:** He was a very exciting individual. I saw him as an enormous idealist, a liberal in the best sense, old-fashioned in some ways, yet in the best sense, always. Remarkably young for his age, even though I'm going back now to about 1940, a very long time ago. Frank was then middle-aged [born in 1894], but he was like a youngster in terms of interest in people, in events, in size-up of things. Frank was remarkable in that way and as far as I know remained that way. He was also a very humane person, who understood what made people tick and who was always willing--in my experience with him--to think the best of a person. Not that he was lacking in critical faculties, but if he could give you the benefit of the doubt, he would. That wasn't something Frank and I were involved with personally, but I saw it time and time again. We'd be talking about who's good in an area or who's promising, or about leading figures when they criticized each other. Frank was always tolerant and humane in all these instances.

Irene Taueber was an enormous personality, just tireless in work, full of enthusiasm. If she ever came through a door to where you were working, she was into the latest discovery she'd made almost before the door closed. There was a constant interchange of ideas. Dudley Kirk, younger than Irene but a relatively senior OPR citizen, moved on eventually to the State Department [in 1947].

Also at that time, right after the war, we had a rather remarkable group of young people assembled at the OPR. Harvey Leibenstein was one such case. George Barclay, who left population after about a decade but did some very important work on Taiwan, was another. His book on techniques [Techniques of Population Analysis, 1958] is still in some ways one of two or three of the most widely used books around. Norm Ryder was a third. There were people of that sort around. And you also had visitors coming in who really livened up the place.

**VDT:** Do you remember people coming from developing countries after the war?

**STOLNITZ:** No, I don't remember them. I do remember Hannus Hyrenius from Sweden, a well-known demographer, and John Hajnal from England, who remains very well-known. But if you ask if I remember anyone from India or Latin America, no. There was an Argentinian whose name I don't remember. Argentina at that time was dubious as to where it belonged--LDC or DC? The UN at that time was calling it developed. Since that time it's become developing; it's had severe, self-made economic setbacks. They didn't yet at the time have programs whereby, say, the Ford or Rockefeller Foundation would send students over here to study in some numbers. One exception was Agarwala of India.

**VDT:** After OPR you went to Indiana, where you eventually developed the Population Institute for Research and Training. Does that bring people from developing countries?

**STOLNITZ:** Well, the Institute is relatively new. We just became constituted formally as of 1986. Still, we already have a great deal of representation from Third World areas. Indiana University happens to be very strong with respect to African and Latin American area studies. So we're having a number of their students come by in our courses and/or for our recently approved Ph.D. minor in population studies. In my courses in population techniques, problems, and economic interrelationships, I've had a lot of representation from all over the world--Orientals, Europeans, Asiatics, Africans, you name it. And we'll have more so as PIRT develops its master's and Ph.D. minor programs. [*Note: PIRT closed its doors in 2007.*]

**VDT:** You said you'd been at the UN Population Division in the early days, in the 1940s. Can you tell us something about the early UN involvement in population studies?

**STOLNITZ:** I was at the UN in the summer of 1947 as a graduate student, when Frank Notestein had

taken over as temporary director of the then newly-formed population unit of the UN secretariat. Because the Population Commission served by the unit was in heavy debate--the French in effect against the Russians, with the Americans squeezed in between, about what is meant by an "optimum population"--Frank had me do a summer's worth of work on its possible interpretations. Nowadays, there's a sort of standard critique of optimum population theory, its applicability to practical policy, and reasons to criticize it heavily or even discard it for practical purposes. At that time, it was heavily espoused by classical economists, in a limited way, and by the French. The Russians were against it--part of the old Marxian dogma that population isn't a problem since people are wealth. The more people the more wealth, diminishing returns was a capitalist plot and Malthus was ineffable, preferably kept out of sight from the viewpoint of rational analysis--all obvious grounds for argument between the Russians and the French. The Americans--Frank Notestein and myself, based on training as a graduate student--couldn't buy any of this. But the question was: How do you formulate for the Population Commission an approach which says, "If you mean something about optimum population, what's the most you can say for it?" I went into the various problems about formulation and ambiguities. John Durand, another Princeton product, inherited the problem. John was also running what became the Population Division of the UN during Frank Notestein's tenure. [Durand was assistant director of the population unit/division under the half-time directorship of Notestein until 1948, followed by P.K. Whelpton, and became full director in 1953--until 1965.] At the time, the going was rough. There was a lot of concern as to whether the population unit would remain as a separate branch--a lot of opposition.

**VDT:** This was the late 1940s, at the beginning?

**STOLNITZ:** It extended into the 1950s. The feeling of Irene Tauer was that John was heroically defending the role of population in the United Nations. And I would say from what I know--not long ago I had occasion to review 40 years of research in the Population Division--that John performed an excellent piece of service. It was, and today surely is, largely unknown, unsung, inadequately evaluated, and an excellent, enormously important job of furthering demographic research, not only in the Population Division but in the field at large, through the research and statistical programs he introduced at a very early stage in the 1950s, when there wasn't very much to work with, and, as I say, a good deal of internal opposition about what role concern with population should have in an international body. I think the profession owes John a considerable debt, which some people know about, but not many and not enough.

**VDT:** You also went on to say that a chief source of our demographic knowledge, our data, in a way the fountainhead of research, has been the UN Population Division.

**STOLNITZ:** Right. And Durand built it up at a time when it could easily have been discontinued. Perhaps it didn't seem like much, because there wasn't much by way of data to be had at the start. Still, John pioneered in many ways in getting surveys started. It's not well enough known that the Population Division did pioneering work in getting missions started in Third World countries that were based on surveys. Nowadays, it's commonplace, but if you go back to the late 1950s and early 1960s, it was pioneering.

**VDT:** You mean the KAP [knowledge, attitudes, practice of family planning] surveys or something like that?

**STOLNITZ:** No, before that time. I'm going back to the days of the Mysore study [begun approximately 1950, conducted by C. Chandrasekaran, then head of the UN Delhi office]. That's when

it was parlous to consider whether demography would play a role in UN considerations other than in technical or statistical aspects of the field. John, I think, was a very central figure in making it work and branch out, despite considerable conservative opposition.

**VDT:** That's good to know. Did you ever work with Phil Hauser?

**STOLNITZ:** No, but I have known Phil since the days when he was already one of the great figures and I was a mere fledgling in the field. I knew him first from my 1940 census work when I was at Census; he was assistant director at the time [assistant chief statistician of the Population Division, becoming assistant director of the Census Bureau in 1942]. But I knew him after a while at OPR, when I got to know him well. Phil was another very important figure, but that's another story.

**VDT:** Going back to the UN, was John Durand the first director of the Population Division?

**STOLNITZ:** Technically, Frank Notestein was the first director. Notestein went back to Princeton, still consulted, but John Durand took over. Frank had me in there because he was still consulting or may have been director still as of the summer of 1947 [called "consultant-director," 1946-48].

Another big issue at that time was who was going to put out the Demographic Yearbook. Should it be the Population Division or the Statistical Commission? Eventually, as we all know, the Statistical Commission won out, though the Population Division has provided much informational input to it.

The Division was just getting focused on what was going on in the world of population. As far as the Third World was concerned, it was just beginning to collect data. I think I knew about as much as anybody--I say this quite modestly--about what life tables were around, what they were showing. But, of course, the number of life tables in Third World areas that you could find--let alone the number you could use--was very limited. I don't think you could go through your fingers and toes and find that many with adequate reliability for establishing trends, differentials, or, in many cases, even one-time situations. The very first life table based on a survey that I remember using was for the Belgian Congo, now Zaire, the first such entry, I believe, found in the Demographic Yearbook. And this was based on a 4 percent sample only, clearly not enough for many of the requisite age-specific rates for completing a life table exercise. You also had the Indian life tables, though here again there were major questions of reliability, in this case about time trends, infant mortality, and sex differentials. You did have, fortunately, Ceylon--now Sri Lanka--Trinidad and Tobago, Jamaica, and that about ended the list of life tables you could use, since there was no indirect estimation, practically no surveys, and too soon, often, for series of censuses. It's completely different now. If you look at the 1948 Demographic Yearbook, you can see everything the UN knew of and could publish going back to 1900 for all countries in the world. If you look for anything outside of Europe, North America, Australia and New Zealand, you'll find very little. That's what I and others were working on in those days.

What's happened since as a result of a flood of censuses, new vital registration systems, and innumerable surveys is that we have today, in one book, comprehensive biennial assessments of world, regional, and national population estimates and projections, with uniformly organized information relating to size, age structure, vital rate characteristics, and urban-rural composition. You can get all this on a computer and analyze in one day basic data that I couldn't handle in years of research in population back in the latter 1940s when I started. Certainly not in mortality, not in fertility, and really not in any topic on comparative international population change or characteristics. The UN about 1950 developed a code on the quality of the vital statistics, which has appeared in the Demographic Yearbook ever since. I happened to review the first Yearbook with such information for the Milbank Memorial Fund Quarterly, where I paid tribute to this important innovation.

As to surveys, we now have these in the many hundreds. In the U.S., the Bureau of the Census was just starting its survey series in the 1940s. If you want to see the difference between then and now, you need only go back to the P-25 series, the P-20 series of the 1940s, to realize how primitive we were then compared to what we are now in terms of data availability and access.

Both of these aspects have been critical for research advance. Censuses were available for individuals and households, but you couldn't sit there and transcribe by the many thousands. To illustrate this point, let me cite a counter case. When I worked for the Bureau of the Census in 1940, Abe Jaffe decided to look into fertility among the Mormons in Utah in 1850. At this point, confidentiality was no longer involved, so Abe could assemble a team to do case-by-case transcriptions from the original census forms. I headed up the copying team. Since this was not exactly a priority project, the women used were basically elderly ladies who had been given special access to jobs for the U.S. Census Bureau because they were widows of military service people. I'll never forget a little lady, 60 or so, who introduced herself by announcing that her name was Pope and quickly added, "but I'm no angel." I remember struggling over understanding an item about location until I discovered that the double "s" in Massachusetts was written in a very different way before the Civil War. The point is that we actually did a written equivalent of a fertility survey by meticulously copying data on number of children, associated, as best we could identify, by father or by mother, among Mormon families. And, although a minor affair, this went on for months. Today any schoolchild, undergraduate student, or graduate enrollee taking a first population course would immediately want to get on the phone and ask, "Where's the tape on this?"

**VDT:** You obviously have moved along with the changing times. This talk about the explosion in technology and numbers of people in the field leads into my next question: What do you see as the main issues in demography over the years you've been in the business? How have those issues changed since World War II?

**STOLNITZ:** Well, there've been lots of changes. Let me again emphasize initially the importance of data availability and their creative contributions to what one can deduce. It's not just that you have something for today, but you can evaluate it better because you also have something for yesterday. When I started, there was nothing on Africa. You looked into Kuczynski's book on Africa [Demographic Survey of the British Colonial Empire, volumes 1 and 2 on parts of sub-Saharan Africa, 1948-53], a work which was anecdotal by today's statistical standards, even for Africa, where data conditions are still far from satisfactory. So the availability of data, the ability to use it, has been one major facet that has transformed the profession.

A second major difference, I would say, derives from this. Whereas at one time you had to deal with nations or large units like states or provinces as the only available units of observation, today we can get down to individual and household levels. What used to be a macro-oriented field almost exclusively is now micro more than macro because of this revolution in availability of micro data and computational abilities to analyze them.

**VDT:** Perhaps micro and macro work side by side?

**STOLNITZ:** It's easy to infer that as a possibility. But the case remains that it's only recently, in the fertility area, that researchers have given serious thought to how they can incorporate both micro--household--levels of data and what are called externality or contextual variables. As I discussed in my PAA presidential address, this issue still hasn't been straightened out even conceptually, in my opinion, as to what's wanted and what's the best way to go about it. The World Fertility Survey, almost by happenstance, has some contextual variables along with its household data core, but everyone so far as I know agrees that much remained to be demonstrated and executed to achieve improved analyses.

We're not at the beginning stage of this issue, but we're also not far into an intermediate stage which would identify a best approach. That's another major area of focus compared to the early post-World War II decades.

**VDT:** Micro-level household data have become so available, particularly on fertility?

**STOLNITZ:** Also increasingly on mortality. You now have on tape data that I had to work months on with a cracker-jack clerk on a calculator, who worked endless hours without complaint, accuracy was perfect. That was a person by the name of Erna Härm at the Office of Population Research. It's worth having her name in the annals somewhere; she's done lot of good work for the profession.

**VDT:** I know the name. That's how you pronounce it, with the umlaut over the "a"?

**STOLNITZ:** Yes, umlaut over the "a". Her work involved a minor fraction of what we now have available and it took us months to complete--a factor of ten to a hundredfold in terms of amount of time to see the results. Today, it's overnight, if that.

Another very major change has been the recognition that population policy is something legitimate, much less something analyzable. This is really a product of the 1950s and into the 1960s, largely the result of pioneering UN work, lobbying in fact, by Milos Macura, a former head of the Population Division. At first, the UN could not, without damage to itself, talk about getting involved at policy levels in family planning programs or other policy areas. Before 1965, it was even dangerous for a UN agency or individual to get into policy. Now policy is very much to the fore as a special concern of a great many people in the population field. There are a great many policymakers, leaders but also analysts, what you'd call applied demographers as well as policy analysts proper with a professional specialty in demography. That's a very important postwar aspect.

Family planning programs, of course, basically date from about 1965, the mid-1960s, and in many countries more like since 1970. This is an enormous research field now, as witness entire publication series on family planning.

**VDT:** Is this legitimately part of the field of demography?

**STOLNITZ:** Well, if demography is what demographers do, certainly that's legitimate. I would say it's legitimate methodologically, too. If you ask about theory in demography or, if you like, about abstract, objective, non-pragmatic research--where it's going, what it looks for--we find a great amount of this is generated by looking at the fact of policy needs. Take, for example, evaluation of family planning programs. In my opinion--not every demographer would agree with this by a long shot--if you want to know really what happens in early fertility transition in micro-level detail, not just statistically speaking but in terms of how the process actually works, a large part of what we think we know now and a large part of what's been documented has come out of servicing family planning program needs. What does it mean to say that somebody is exercising family planning decision making or thinking or behavior of any kind? If you look at the early transition theoretical or descriptive formulations, you'll find them miles behind what we've found because of such things as a family planning person asking: What do I call a family planning user or non-user; how do I define this? This opened up the whole area of acceptors and how do you measure them. What do I mean by a family planning program? What do I mean by population policy? You weren't defining the latter as of the mid-1960s and we were still debating this as of the 1970s. And we still argue--much as we should--about the distinction between policy which works in response to population change or as a determinant of population change. Political and management disciplines are other aspects of the family planning area. Psychology as a significant source of explanatory variables has by now a place

in population analysis, though not nearly enough. I think there's a return now to economics in increasing degree.

**VDT:** Return? You said before we started this interview that demography began with economics.

**STOLNITZ:** Well, the first social scientists, so to speak, were economists, since societal analysis talked about population even before Malthus and these were economists, primarily, in addition to social philosophers. Even Plato talked about the optimum size of a city state--about 5,000, if I recall. The first great demographic figure is obviously Malthus. If you look at who was great in the 19th century, you'll find some statisticians like William Farr in England, but the important work that commands most attention is John Stuart Mill, for example, an economist, and there were others, notably Wicksell, the Swedish economist at the turn of the century. Optimum population theory was clearly an economist formulation, miscast, but nevertheless much at the center of population-economic international analysis up to at least World War II. Increasingly since then, sociologists and population statisticians have become paramount numerically.

Today, I'd say, because of central interrelations between population change and economic development, population change in relation to development planning specifically has come to the fore, with enormous programs of research and application under way at this very moment. I was consultant, by the way, for the United Nations Fund for Population Activities, for whom I gave a major paper in its Mexico City forum on population policy in 1987 on the subject of population growth effects on development planning. Today, in order to incorporate population units in development planning units, the ones you need to anticipate dealing with mainly are economists. If you're going to train people to work there, you've got to train them in economics to a considerable extent, in addition to training them in demography proper. Indeed, that kind of retransformation of focus has occurred in the UNFPA's program within the last few years. I happen to know this pretty well, because I've been a consultant with UNFPA and indeed gave a series of lectures on the subject at the UN regional training program in Trivandrum, the capital of Kerala state in India.

**VDT:** Do you think that the growing emphasis on population and development planning may be one of the reasons that demography has become so quantitative in recent years? Do they go hand in hand? Demography always was quantitative, of course, but you mentioned that some of the earlier leading demographers--Notestein, Irene Taeuber, and others--tended to be transition theorists, not statistical modelers.

**STOLNITZ:** Irene was a sociologist. So was Lorimer. Notestein happened to come out of economics but it didn't carry him very far in his demographic research.

But I think you're onto something important when you ask about methodology. It happened that the economists had pioneered since the 1940s in econometrics, the use of formal statistical models for explaining economic behavior over time or cross-sectionally. It's the sociologists who caught up later on, when they began to branch out in terms of path analysis and other regression-type methodologies, in which, in fact, sociologists have led the way.

The original work on basic regression-type statistical models, examining the relationships between causes and effects in the case of fertility, for example, followed along lines laid down by economists. What's happened since is that sociologists and demographers increasingly use modeling procedures for which they still have to go often to econometric sources but also to sociometric sources to formulate demographic models. This now runs across fertility issues, as in use of WFS data, mortality increasingly, and migration where, by the way, economists have perhaps been more prominent than sociologists in formulating theories of why people move, since it's often an economic issue, though one with sociological aspects. Now with recent policy concerns with aging and its very

important economic consequences for the low-mortality, low-fertility world, you'll find still more economists stepping in. Aging ties in with labor markets, with fiscal policy, with rapidly rising costs of meeting health care needs, hence with medical economics.

**VDT:** So, you have sort of been in the vanguard since the beginning?

**STOLNITZ:** Well, around, anyway.

**VDT:** I'd like your reminiscences about PAA and your connections with it. Can you remember when you first became associated with PAA, your first meeting, perhaps?

**STOLNITZ:** I think the first time was when I first came to OPR; this goes way back to 1941.

**VDT:** The 1941 meeting was in Princeton [May 16-17, 1941].

**STOLNITZ:** Right. I was half guessing, but right. Then the big names were Fairchild, Lorimer, Notestein, the two Taeubers, and so on, so I met them. That was the very first one. It was a small meeting; it was wartime also.

**VDT:** The next one, in Atlantic City in 1942, was the last one and then they closed up till 1946. Were you in Atlantic City?

**STOLNITZ:** No. In [May] 1942, I was cramming for my Ph.D. field exams, just before leaving for military service. Then I was at the PAA meetings in 1946 and 1947 [in Princeton]. But much more than those I remember 1941, because I was the awkward student gaping at the big names. It was sort of a roundtable. Of course, it was almost laughable by today's standards of PAA meetings, where you have five to seven concurrent sessions [nine in 1990 and 1991], which range in terms of size, availability of seats, from 50 to 75--which would have encompassed all of the 1941 meeting easily--to several hundred, often only with standing room or sitting on the floors.

And, again, you knew everybody, or Frank Notestein, who was very good at this, would introduce you. He had a great belief that personal contacts would have effects both ways: I to people who were already in the field and the people looking at who the newcomers might be. Another very basic figure then, whom I knew at the time, was Fred Osborn, co-author of a Lorimer-Osborn demographic text which still has useful material [Dynamics of Population, 1934], and who was important in terms of providing support for OPR, both monetary and institutional, as well as being a key player in encouraging foundation support for population generally, for example from the Milbank Fund.

**VDT:** What offices have you held in PAA?

**STOLNITZ:** Well, I was president in 1983, first vice-president in 1979, and then there's a long hiatus back to when I was second vice-president [1961-62]. And there were other offices besides; when you become president, you automatically serve for a while as a PAA Board member. I have also served on various committees: for example, the nominating committee and one which decided who should be funded to go to a quadrennial IUSSP meeting abroad.

**VDT:** Can you remember some of the best meetings you've attended?

**STOLNITZ:** I've always come away fired up: "It's high time to get into this, get into that." Or, why

don't I do this in fertility or in policy, or why don't I rewrite a paper or read one I first heard about at the meeting? It's always been an exciting occasion in that respect.

However, it gets harder and harder to keep up with at a PAA meeting today, since it really has proliferated in terms of people, concurrent sessions, and papers. There's an enormous competition now for getting papers accepted. The year I was running the program, 1983, I found that so many people were trying to get a paper accepted for presentation compared to what could be accepted that many such applicants had to be disappointed. Even so, I'm always refreshed in terms of what I learn is going on actively and would like to get into somewhere. I'm always learning of things I should know. Some people, I'm sure, do far better than I do in terms of keeping up, but the meetings enhance the urge to try.

**VDT:** You are one of the rare demographers who has been sort of working on your own elsewhere for a long time, as a demographer-economist.

**STOLNITZ:** That's right. But I think in general even those who know a lot more about the field than I do, don't know all about it. Today, if you want to know who's doing what that's useful in the field, you've got to rely on at least several phone calls, or maybe more than several, in just one area. The big stuff you can find out easily--a call to Washington or to somebody who's heading a committee, say a National Academy committee--and find out these things very fast. But if you want to know who are the good graduate students coming up, what's a new program that's being launched, who's making a trip for, say, a field study of immediate or potential interest, it's kind of hard to keep up. You're dealing with hundreds of people today, whereas in my early career you were dealing with maybe two or three dozen. It's no longer the case that the U.S. or Western Europe has to educate all Third World demographers. There's still lots of such educating to be done, but there are a great many well-trained, competent people out there already in the field.

One of the things I did, by the way, when I worked for AID on missions to India was to consult with the UN Indian regional population center in the Bombay metropolitan area. If you were talking about who's going to do what, what Third World demographers know, how much data could they give me, how much did they need, what would they do when they went back--in those days it basically came from international foundations and AID and, more recently, UNFPA. For a decade or two after World War II, there was almost no developing area conducting research of world-class stature.

That's now changed. There are now world-class people in all three developing continents and in places like Latin America, there's a lot more than that. The Colegio de Mexico and now CELADE have top-flight demographers, both seasoned and training students competently. And to know all that is now practically impossible unless you make field trips. There are people like Ron Freedman who gets around in Asia, but he won't know Africa or Latin America. So you're getting specializations now.

**VDT:** Do you regret that--not being able to keep up with everything?

**STOLNITZ:** No. Although it was certainly psychologically pleasing, in a way, to feel you knew all of what was going on, it was only at the margins that you were really doing so. Today you are simply excluded even from the margins. You quickly learn that all the behavioral disciplines are this way--sociology, economics, and so on. Population after all is so small compared to these, but in much the same fix by now.

**VDT:** That's right. You must know from being an economist, the American Economic Association is a multiple of PAA.

**STOLNITZ:** That's right. That raises another point. Look at the publications now in the

demographic field. At one time we had Population Studies in English and Population in French; that was all. Now look at the proliferation of journals that come out. In my days at OPR, I remember distinctly that when a book on population came out, we all knew of it, we all could read it within a month; we could stay reasonably well on top of the field. Nowadays, just to read the titles of what comes out, let alone read the books, or just to be able to read a preface and size up summaries and conclusions has become impossible. It's more than a full-time job. What's inevitably happened, of course, has been a very heavy trend toward specialization and sub-specialization.

You ask me if I regret it, and of course I do. I'd like to know an awful lot that I know I'll never have time to really encompass even at a superficial level, much less in depth.

I remember once that Frank Lorimer, in the latter 1950s, acting on the thought that biologists and demographers ought to get together, helped organize a Cold Spring Harbor symposium, from which a book came out, with contributions from world-class representatives from both disciplines. Cold Spring Harbor was a leading center of biological research and Frank Lorimer's feeling was that the two fields didn't meet, despite their enormous overlaps.

**VDT:** What was the general topic?

**STOLNITZ:** Interrelations between demographic behavior and biological and genetic determinants, primarily. I don't think the organizing idea took hold; it didn't evolve into an ongoing thing. Geneticists and biologists went their way, as did the demographers. Only in recent years have they started to come together, as people like John Bongaarts, Jane Menken, Mindel Sheps, and so on have done useful work on biological and biostatistical aspects close to demography concerns. Also, some biologists have gotten involved with demographic areas of behavior of special interest to biologists. Obviously, each has a lot to teach the other.

**VDT:** What are you doing now [1987-88] as visiting scholar at the Population Reference Bureau?

**STOLNITZ:** What I'm doing this year at PRB is two projects. In a sense, I've come back to mortality more than ever. I've always promised myself in the last ten years that although I've continued to write on mortality questions in all these years, really digging in for a full year's uninterrupted work on mortality on two levels would be of primary interest to me.

A first project, which is almost done now, is how mortality affects aging. How do you analyze it in ways which link them up most directly from explanatory, descriptive, and policy viewpoints? We all know there's a lot of aging going on, also much mortality change, but a system for analyzing how specific mortality changes lead to changes either in numbers or in proportions of population by age, that system hasn't been worked out satisfactorily, so far as I know. I've worked out a framework that I think does the job for any population, LDC or DC. But more than that, and what I'm working on particularly for the U.S. and want to do next for other low-mortality nations, is to focus on the fact that there has been what I call a new stage of demographic transition, involving a radical change in the age function of survival rate percentage changes. Whereas the main such declines used to be in the infant and young ages, this can no longer be the case with declines of any magnitude that occur in the future. Why? Because under age 50, there's not much room between actual survival rates by age and their upper bounds of unity, i.e., the rate when nobody dies between successive ages, such as 0 to 1, 1 to 5, 5 to 10, and so on.

In the United States, and it's true of 15 or 20 other countries, if no woman dies under age 50, life expectancy would go up by about only two years. That's a small fraction of what's happened in the past. For example, life expectancy since 1900 in the U.S. has risen from something like 50 years to close to 80. This is a gain of 30 years. But now if anything big is going to happen in U.S. female life expectancy, by which I don't mean a 30-year increase but, say, even three- or four- or five-year

increases, it will have to happen mainly because of survival rate changes at the upper ages of life, that is, over 50 years--unlike what used to be the case until not so long ago, say for a decade or two after World War II, when the main concentration by far of survival rate percentage changes was in the young ages of life.

**VDT:** Which is happening now in developing countries.

**STOLNITZ:** Yes. The developing region patterns today are those of the developed regions historically until about a quarter century ago, but are fast disappearing or already gone in the developed regions. There the age curve of survival chances and their effects on age proportions have changed radically. What happened before World War II in the West was that the long-run mortality decline transitions we all talk about reduced, not raised, the average age of populations, by raising the fractions under 15, raising the fractions over age 60 or 65 by lesser amounts, and maybe depressing the 15-to-65 fraction to some extent--in all three instances by not much. If you had more people surviving under age 15, they eventually moved up into the older ages, so the effects balanced out to an extent over time.

But today if you have most people surviving only after reaching 50, there's practically no compensating effect from added numbers in the younger ages. There are not many more young people than there would be whether older people died at 50, 60, 65, or survive beyond these ages. What's happening, therefore, is no longer just more surviving at all ages in greater numbers but not much in altered age proportions, but a piling-up tendency at the elderly ages only, uncompensated at the lower ages. So what is happening--no surprise to me, though it seems to surprise projection-makers--is that the most rapidly increasing age group in the United States is the 85 and over sector.

**VDT:** In actual numbers?

**STOLNITZ:** No, in percentage rates of change. Not in absolute numbers, because there are so relatively few aged 85 plus that a 1 percent increase under 15 can swamp a 10 percent increase over 85 in terms of absolute numbers. Of course, once the post-World War II baby boom cohorts move into the elderly ages, the absolute number increase is also going to be exceptionally large--I think far beyond what anybody is anticipating, including our social security projections people who are exploring whether social security funds will be enough. Although present plans are to accumulate trillions of dollars for the 2020s, the dollars needed when the baby boom people retire will be greater than that. Much greater, in my opinion, because there are going to be many more people in the retirement ages, and especially the late retirement ages, than we anticipate.

Not only will the number of baby boom survivors to old age be much larger than that of any of their predecessor old age cohorts--this is anticipated, from the relative numbers of births in these cohorts. But further, and not sufficiently anticipated, is what is going to happen to the age function of percentage survival rate changes. Where we used to have reversed-J age functions, they have become J-shaped as the probability of surviving, say from 75 to 85, has suddenly jumped after decades or maybe a half century of stagnation. The reasons involve medical advances now and clearly in prospect. Through medicine, there's in effect conventional technological sources of longevity gains, but there are also dietary changes, less smoking, more exercise, less alcohol consumption, as tomorrow's elderly cohorts implement their own chase after ever greater longevity. Behaviorally, we already can begin to see this to an extent: today's 70-year-old is like a 55-year-old of 40 years ago.

My own guess, and I won't be around to either prove or disprove it, is that the 90-year-old of, say, the year 2025 is going to be like the 70-year-old or 60-year-old of today. Why? Because of an entire range of health-related changes. Now, whether that's good or bad . . .

**VDT:** That was going to be my next question.

**STOLNITZ:** It certainly raises problems.

**VDT:** It does, indeed.

**STOLNITZ:** The problems raised are being taken up by a two-volume set of studies I'm editing and contributing to for the UN Economic Commission for Europe in Geneva. However, to deal with such problems, one has to consider first the demographic magnitudes and causes that may be involved, and this has been the focus of my work at PRB, based on what one sees for the U.S. for 1945-85, also on what is readily foreseeable for the next 40 years--barring major holocausts. I'm prepared now to say with no small confidence that the mortality-induced aging linkages now in prospect would make the 1945-85 linkages piddling. True, that's what I foresee, but is also what makes this work exciting.

**VDT:** Is this one of the important highly probable transitions that demographers can point out policy-wise--warn that it's coming?

**STOLNITZ:** Yes, particularly since the likely dimensions of this transition process are greatly reinforced by what is happening not just here in the U.S. but throughout the lowest-mortality developed regions. Not so incidentally, by the way, analogous such developments may with surprising speed begin to emerge in the Third World, as diffusion of health-related technological advances moves way ahead of the rate of development advances. An excellent example is China. And this is by no means a single case: add, surely, the Pacific Rim "tigers" or "dragons"--South Korea, Taiwan, and others. When I wrote the UN's 1977 Concise Report almost 15 years ago, I was already surprised to see that the most rapidly increasing population age sector in the Third World also occurred well up among the elderly ages. Although the data are not all that reliable, they are probably indicative. Add to the Pacific Rim cases Sri Lanka, where perceptibly rapid aging is going to happen shortly, also Jamaica and Trinidad and Tobago--three standard examples of especially rapid mortality declines right after the war. Mexico also is going to get swamped by elderly population issues, though this isn't well enough realized yet.

I hope with the framework I'm developing, more countries will be motivated to start looking for the facts and possible policy responses. You will get a lot happening at the infant and early childhood ages, but you're also getting a lot happening at the elderly ages, to a degree that didn't happen in the West. Demographic transitionists have not really taken this up yet, since they haven't analyzed mortality-age composition linkages as they should have for the West when considering non-Western possibilities.

Finally, it may be a matter of interest that I have a computer system which basically anybody can use and, within a matter of hours or certainly in a day or two, work out what might happen under alternative scenarios of how age composition is likely to be altered by likely mortality transitions.

The other project has to do with the fact that I still think--I've been heavily criticized about this, which goes as far back as my 1950s articles--that there's enormous room for mortality decline in high-mortality countries faced with low, stagnating, and even retarded development, provided they compensate for this by what I call health-related technological inputs. I'm not talking about hospitals; the Third World can't afford hospitals. I'm talking mainly about public health and sanitation programs, cheap public health mass programs--ORT [oral rehydration therapy] for children, for example. If you exert efforts to teach people how to protect the lives of their young, you can do a lot to knock down infant and child mortality, even beyond where it's gone down, as has often been the case since, say, 1950. What I want to do is work out an analytic system for identifying possible tradeoffs revealed by experience between developmental change patterns and health-related improvements or inputs that can

lead to three, four, or five years of added life expectancy under LDC circumstances. That's the second project I've started at the Population Reference Bureau.

**VDT:** That's sort of going back to what you'd already identified, that after World War II, it was public health inputs that had the largest impact on LDC mortality trends. Even in those countries that aren't developing much, or at all, because of rapid population growth and/or non-demographic reasons, perhaps if you just introduce more ORT, etc., mortality will go down?

**STOLNITZ:** The main issue here is not dollars, though obviously dollars count and money or affluence can help. The issue here is political will and organization, because feasible technologies for major results, if implemented, exist already. Even within severe budgetary confines, they can do a great amount. Needless to say, I'm not talking about civil war, hence I'm not talking about Ethiopia and its periodic, largely war-induced or ethnically-derivative famines. Even here, I'd note, recoveries have been remarkably fast, largely owing to powerful, yet cheap, technological antidotes to lasting injuries. Even in such worst cases--add the Sahel--infant survival and longevity are normally well beyond what they used to be, so that where we talked about life expectancies of 30 to 35, it's more like 40 to 50 in lots of very, very poor countries. China is another outstanding example, very poor if you really average its regional income levels, but it has life expectancies of between 65 and 70. They say more like 70; I'll settle for 65. I remember when 65 was high right here in part of the Western world, as I guess you also remember.

**VDT:** Yes, indeed. So all this means you've found a lot of satisfaction and excitement in your career.

**STOLNITZ:** There's been much of this.

**VDT:** And you're obviously not looking ahead very soon to real retirement.

**STOLNITZ:** I'll continue to direct the Population Institute for Research and Training--PIRT--for a while longer. I have a history it happens, very personal and not demographic, in that the last two sabbaticals I had I did work for others. I directed a center way back in the 1960s and early 1970s in which I edited books of others to get their publications out. My stay at the Population Reference Bureau has been the first sabbatical when I could say I'm on my own, able to answer to my four walls--a good, good feeling. No hostile interruption. There are things you can get in Washington which you don't get in Indiana.

On the other hand, we now have in Indiana for the first time since I've been there some very good demographers, as well as historians who are interested in population. We have, it happens, one of the active groups of historical demographers in the country and are also especially active in mortality-morbidity areas. There's an economic demographer, a historical demographer, one in sociology, and an anthropologist with unparalleled long-term direct observation of mortality and associated social transitions in an African village area.

**VDT:** Who are they?

**STOLNITZ:** Core people with me at PIRT include George Alter out of Michigan and Pennsylvania, who has been publishing in depth on Belgian historical demography and getting into U.S. aging historical questions, for example, with the use of insurance records to improve what we know about upper-age mortality in the U.S. early in this century. True, we have the U.S. life tables, but you can't trust them for the advanced ages, for which the death rates come from extrapolations to an assumed end of life. As to trends rather than levels, you especially don't know what you've got. George Alter

had the idea of going to insurance records, medical and health insurance records, which are reliable because they're administratively run and affect dollar returns; also to go to insurance companies and companies that have health programs. You can learn how many stayed away from work because they were sick, how many died, and so on.

An historian, James Riley, is doing analogous work based on British records. Laurel Cornell works on Japanese historical demographic patterns and family sociology. Jerry McKibben is the state's demographer for providing estimates and projections, plus recently providing attempts to enhance the accuracy of the state's 1990 census. Phil Cutright has turned his long-practiced skills for unearthing previously undetected or inadequately explored relationships to the question of child homicide phenomena. John Kennedy directs a program of ongoing social surveys with the state. Elyce Rotella, an economist, is interested in historical demography, women's roles in the labor force and other women's studies.

**VDT:** And you have your historical interests. You worked with Abe Jaffe on the historical censuses, and the Mormons, who have their magnificent records going back to when the first Mormons came.

**STOLNITZ:** That's right. You're looking at the eyes that have looked at the 1850 census, which later appeared as an article by Abe. But that's nothing like getting today to micro sources, as I stressed earlier with respect to this project and my own work on earlier life tables. Apart from computational and accuracy factors of prime importance, we have much richer--I didn't mention this but I should--theoretical frameworks now to work with. There are many more hypotheses, both tested and untested.

**VDT:** You said the demographic transition theory was little or no theory. What theoretical frameworks are you referring to here?

**STOLNITZ:** There are many middle theories as to what's the role of this and that variable affecting population, let's say at the household level. There is much more sophistication about which variables are involved and how to treat them statistically than we had before.

As to demographic transition theory, I certainly didn't see it as the be-all and end-all in theoretical terms, and I don't now, but it has had a very major role in enhancing empirical descriptions of what actually happened. It also was the first indication of possible main explanatory variables, which have led to theoretical formulations of much more limited compass. But if you mean, do we have a predictive modeling that enables us to put transition statistics to work, my answer is no, not yet.

**VDT:** Well, perhaps that's what will make demography continually interesting.

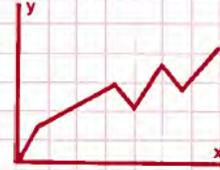
**STOLNITZ:** Yes, since we're discovering things about fertility that we didn't know 40 years ago, such as with the Princeton European Fertility Project. In another connection, mortality, I'd say that I came up with things, at an early stage, with respect to mortality transition possibilities in backward areas, that you simply could not read into what the demographic transition people or other analysts were saying before, when they simply referred to public health plus medical technology and improved levels of living as main causal factors of such transitions. That wasn't good enough either for explanatory analysis or policy guidance purposes.

**VDT:** Well, it's wonderful that you're here in Washington, having such a refreshing year, and we'll expect to hear from you for decades to come.

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Indiana University Bloomington

# THE TREND LINE



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## In memoriam

### George J. Stolnitz

George J. Stolnitz, professor emeritus of economics at Indiana University, died Tuesday, Dec. 18, 2001, at his home in Bloomington at the age of 81. Stolnitz was an international expert on demographic trends and a frequent consultant to the United Nations and U.S. government agencies. He was a past president of the Population Association of America.

Born on April 4, 1920, in New York City, N.Y., he was the son of Isadore and Julia (Jurman) Stolnitz. He received a bachelor of arts degree in economics, at age 19, from the City College of New York, where he was a member of Phi Beta Kappa. After graduation, he worked as a statistical analyst for the U.S. Census Bureau before winning a Millbank Memorial Fund Fellowship for graduate work at Princeton University. Stolnitz earned a master of arts degree in economics from Princeton in 1942 and then served in the U.S. Air Force until 1946. Upon his discharge, he studied mathematics and statistics under the noted Abraham Wald at Columbia University before returning to Princeton in 1948. In 1952, Stolnitz earned a PhD from Princeton, after working at its Office of Population Research and Econometric Research Institute, and was appointed an assistant professor there the following year.

Stolnitz joined the Indiana University Department of Economics faculty as a full professor in 1956 and subsequently became involved in a diverse body of research. Stolnitz gained international recognition for his research on international mortality comparisons, work population trends, population-development interrelations, population policy assessments, world population trends, and population aging. His writing include the books *Demographic Causes and Economic Consequences of Population Aging: Europe and North America*; *Technological Prospects and Population Trends*; *Population and Environment: Patterns, Problems, Some Pathways to Solutions*; and *Life Tables from Limited Data: A Demographic Approach*.

Memorial contributions may be made to Indiana University Jewish Studies Program, Goodbody Hall, 308, 1011 E. Third St., Bloomington, IN 47405.

## THREE TO FIVE MAIN CHALLENGES TO DEMOGRAPHIC RESEARCH\*

George J. Stolnitz

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### ORIENTATIONS

A presidential address is poor occasion for changing one's ways, and inveterate compromisers compromise. So that, unable to choose among single integrated topics, I have decided to consider a topic per component of population change, much in the order in which my own work has evolved. Mortality heads the list, as befits pride of place to one's research first born; fertility will come third. Internal migration will come between, at some underserved risk of being squeezed.

So much for the "Three" of my title. The "Five" reflects my intention to deal with issues involving basic methodology. These could have been as usefully integrated with each substantive topic as considered separately; the numerical distinction is to draw attention to the small importance of taxonomic precision in such matters.

The challenges to be singled out are mainly macrodemographic in content, quantitatively oriented, and of major interest for policy. As one whose student ways of counting degrees of freedom was more by countries than by household, I should perhaps add that my macrodemographic emphasis here is not intended to take sides concerning the relative importance of macro *versus* micro main issues. Indeed, I believe and argue below that one of our discipline's main unmet challenges is to evolve improved means of accommodating macro and micro ap-

proaches to each other. We are far from the stage where adequate bridges exist between microlevel indications and macrolevel uses—a shortcoming by no means limited to demography. This said, I would stress nonetheless that macrodemographic issues and variables must continue to be foremost and uniquely oriented parts of our research endeavor—a point quite obvious in principle but easily obscured by our growing reliance on household-level data sources and models.

Certainly, today's research opportunities and needs for entire-population analysis are far greater than ever before. International differences in expectation of life at birth are at historically high orders of magnitude—35 years or more—with unprecedented convergence among the developed countries (DCs), equally unprecedented disparities in the less developed countries (LDCs), and vast consequent interregional variations in causes of death, quality of life, and socioeconomic arrangements. Fertility ranges from as yet unyieldingly high premodern levels, such as in Black Africa, to the apparently entrenched subreplacement patterns now frequent or even modal in high-income societies. As a result, national age distributions differ enormously, from the traditional young-age patterns of pretransition or early-transition populations to the accelerated aging trends found in large parts of the West. LDC-DC differences in urban growth and global urbanization processes continue at peak orders of magnitude, fueled in large measure by apparently record amounts of rural-to-urban migration movements. Rural population in the

\*Presented as the Presidential Address at the Annual Meeting of the Population Association of America, Pittsburgh, Pennsylvania, April 15, 1983.

DCs has begun to decline steadily in absolute size, the first such apparently irreversible downtrend in history for a substantial number of spatially dispersed regional populations. In contrast, LDC rural populations are still expanding at an average aggregate rate which matches or exceeds the peak growth rates ever recorded for long-settled Western national populations throughout their modern demographic transition—despite amounts of out-migration which may well be reducing current LDC rural-area growth rates to half their rates of natural increase. International migration, long relatively quiescent under peacetime conditions, has become more varied in demographic dimensions, main directions of movement, and legal status than ever before. (See, for example, United Nations 1979a, 1980, 1982a through 1982e.)

Meanwhile, demographic-development interrelations have become more diverse over space and more threatening over time. In the DCs, the new labor-force consequences of long-term subreplacement fertility, the enormous aging impacts likely to emerge for the first time as large future mortality declines become necessarily concentrated beyond the midlife years, and the accumulating negative externalities which have become associated with large-city population growth—all are placing newly increasing demands on productive systems, even as extended economic slowdown appears to have become entrenched and potentially endemic.

To cite U.S. patterns specifically, these may well be the most dramatic years of our history from demographic viewpoints:

The signs are everywhere: the nation's lowest-ever childbearing rates, which are remaining below replacement levels even in prosperous times; vast gyrations in numbers by age; the major new social costs to expect if we learn how to live much longer than we do; more immigration outside the law than within it; more uprooting of family or household lifestyles in the last few de-

acades than in previous centuries; new responses to city and to non-metropolitan living; and much else. Any one of these in isolation would rank as a major national happening. Yet all in combination are already occurring or appear imminent. (Stolnitz, 1982, p. 91)

(See also Population Reference Bureau, 1982. For recent DC economic trends, see International Monetary Fund, 1982, pp. 3–14 and Organisation for Economic Cooperation and Development, 1982.)

In the LDCs, where patent or potential Malthusian threats to development have been seldom far distant and only occasionally safely surmounted, average economic prospects over the past decade have taken a substantial turn for the worse. Despite substantial fertility declines in numerous individual areas and overall (the extent being largely dependent on estimates for China), population growth has changed relatively little and has begun to encroach upon or overtake growth of output and food. From about 1950 through much or most of the 1970s, gross domestic product had risen at remarkably high annual rates when compared to historical and contemporary rates in Western areas. In recent years, however, it has suddenly decelerated to barely 2 percent on average, a level essentially equal to the current average population rate (World Bank, 1983, pp. 1–39). More LDCs have experienced declining outputs of food per capita between the 1960s and 1970s than have had increasing such outputs during this period (FAO, 1982, pp. 48, 50).

For about a quarter of a century after World War II, Third World population pressures on development appeared to affect productive performance much less than rising levels of living, as both output and numbers rose at historically high rates. Since then, however, the production side of this relation can no longer be assumed, at least for Black Africa and other "least developed" regions. Agrarian densities and urban overcrowding have continued to mount up spectacularly and to add to such nondemographic

development burdens as gross economic mismanagement, political instability, rising energy costs, and near calamitous shifts in international trade and capital-flow patterns.

The net result—quite apart from historic LDC-DC contrasts—has been a degree of diversity in regional economic prospects not witnessed since development became a global rallying cry. Among the larger (over one million population) Third World areas, perhaps a half-dozen to a dozen nations appear to be well under way to achieving assured breakthroughs to sustained development and long-run demographic transition. Another 20 to 40 countries, including all of the so-called “least developed” areas, have yet to give any persuasive demonstration that long-run transformations are foreseeable, except possibly in death control and education. Perhaps the most somber current reading of demographic-development interrelations in any less developed region is that, despite projected economic growth at substantial rates by long-run development standards, sub-Saharan Africa might well have lower per capita incomes in the 1990s than it had in the 1960s (World Bank, 1983, pp. 7, 35). The remaining group of 50 or so LDCs, consisting mainly of the World Bank’s euphemistically labeled “middle income” areas, occupy intermediate positions between the previous two groups. All or nearly all of these populations have experienced considerable degrees of development and demographic change, but few as yet are on time paths which would assure modernization in either respect.

Adding no little variety to the macro-researcher’s study perspectives is the fact that development once again is proving to be an erratic predictor of fertility transitions. In Sri Lanka, where per capita income is low even by LDC standards and economic growth—moderate at best—has been punctuated by heavy setbacks, there have been sustained fertility declines for at least three decades. In contrast, with much higher per capita

income and reported accelerating rates of GNP growth, the Ivory Coast has apparently had no change in period fertility. China, with rapid postwar economic growth as a rule but with one of the world’s lowest per capita income levels even now, started on a rapid fertility downtrend at least a quarter of a century ago. Brazil, with similar growth rates but also far more developed in terms of economic structure, per capita income, degree of urbanization and modernizing social patterns, first began a sustained fertility transition after 1965. (See United Nations, 1982d and World Bank, 1980, national tables.)

Additional current examples could be cited easily, while historical examples have proliferated owing to both revisionist reconstructions of nineteenth-century fertility transitions in Europe and the non-traditional patterns characterizing the post-1940 mortality revolutions in all three Third World continents. In brief, we are still far from able to develop persuasive quantitative linkages between national demographic change and its non-demographic determinants. It is no accident that both national and international projections analysts, though continuously exhorted to relate their exercises to widely accepted non-demographic determinants of mortality and fertility change, find it less risky and more prudent to rely on demographic determinants in all essential respects.

In particular, we are still very far from where we have long hoped to be—or where we once thought we were—with respect to demographic transition theories. More than a quarter-century ago, when I first had occasion to test such theories in the light of both international mortality trends and their temporal interactions with fertility trends, I pointed out that

the essential aspects of the [demographic transition] approach are that it is dynamic, it identifies the direction of change in vital movements and, more specifically, it stresses a lead-lag relationship between

mortality and fertility trends. At the same time it will be apparent that nothing is said about magnitudes. The inception, timing, and degree of the anticipated vital transitions are left in question, as is the duration of the lead of mortality declines over those in fertility. As a result, the approach accommodates any of a very large number of alternative paths of population change. (Stolnitz, 1955, p. 51)

It seems to me fair to say that essentially the same critique of transition theories hold true today, despite our enormously richer factual and conceptual bases for applying such theories to current cases.

I believe something more is at issue than our still frustrated attempts to identify robust parameters, statistical relations, or threshold values when dealing with macrolevel temporal patterns. Shortcomings of research arrangements are also involved. A literature review in which a number of colleagues and I have been recently engaged at Indiana University reveals that quantitative estimates of how main development processes may affect macrolevel fertility are quite often astonishingly sparse, even when the determinants in question are among the most widely cited and accepted in the literature. Relatively comparable or updated estimates are much sparser still, as I shall be illustrating below for four main classes of development processes.

I noted earlier my intention to focus on research challenges which have special interest for applications to policy. Not many years ago, such an emphasis would have been widely regarded in our discipline as a threat to its integrity. Today, these fears of yet another victory for Satan have largely faded, I expect because the policy demands placed on our profession, while heavy, have often been scientifically rewarding. We need only ask whether the postwar rise of international technical assistance for mortality control, family planning, data collection, and similar purposes has enriched or dulled our knowledge of early demographic transitions—not only in today's

LDCs, but in those of the West a century or more ago. Or, for that matter, whether our interest as scientists in the later stages of demographic transition has been stimulated or stunted by the spread of governmental socioeconomic interventions in DC areas. Obviously, the threats to ongoing research from possible fiscal shifts, or to scientific integrity from possible censorship, are continual and unwelcome uncertainties. But judging from substantive research viewpoints only, today's and tomorrow's opportunities for combining scientific advance with policy-serving functions in our profession appear to me no less promising than they have been since demography came of age some 30 to 40 years ago.

A final introductory comment is in order on the spirit of the discussion to follow. Obviously, the very notion of "challenges" connotes a state of unfinished business. But it is also true that our main challenges ahead—however these may be selected and however gauged—stand out precisely because of advance achieved. Although the main problem areas to be singled out below are as often long-standing as suddenly emergent, our awareness of what they imply has been made enormously richer by virtue of what we have accomplished. By any criterion—theoretical and data advances, quality and quantity of personnel, scope and reach of research goals, or achievements of such goals—ours is a flourishing activity, surely incomparably beyond where we were as of, say, 1950 and surely far beyond the benchmarks of only a decade ago.

Parkinson's ever malleable law holds for demography as for all disciplines: Awareness of what we need to learn tends to increase at least in proportion to what we have already learned to know. It is within this context that I would hope to be understood as I turn to specifics.

### THREE SUBSTANTIVE CHALLENGES

A ready approach to the substantive challenges I have in mind can be suggested by posing three initial questions:

1. To what extent can causal factors of a health-focused, relatively nondevelopmental nature be regarded as tradeoffs or substitutes for development in effecting next major gains in international mortality levels?

2. What obstacles explain our lack of progress in evaluating internal migration patterns from policy, benefit-cost, or development perspectives?

3. Where does demography stand today in assessing the quantitative influence of main socioeconomic factors believed to affect macrolevel fertility transitions?

The first two questions can be applied in more or less analogous fashion to both LDCs and DCs. The third, for reasons of manageability, will be limited here to LDCs.

### *Mortality*

The first or "tradeoff" question requires a number of clarifying definitions before being addressed directly. By "factors of a health-focused, relatively nondevelopmental nature," I mean those which are neither closely tied to fiscal capacities of governments nor to developmental advances of economies, and hence are largely exogenous to planning-period changes in income per capita, income distribution, educational levels, urbanization, size or structure of labor force and output, capital-labor ratios, and the like. In the case of most LDCs, for example, the nondevelopmental factors in question may be taken to refer to death-control and disease-control program measures which make limited (say 5 percent or not much higher) demands on national budgets and which require very much smaller allocations of national product. Public-health programs, rudimentary sanitation, and the costs of mainly low-order medical personnel would be included among the factors in question. On the other hand, most individual medical care resources, most nonrudimentary hospital facilities, and nearly all state-of-the-art categories of medical equipment would be excluded.

I also subsume among nondevelopmental presuppositions a reasonable (though not outstanding) degree of political stability, credible political commitments to achieve five-year or so mortality gains under existing ecological circumstances, and the existence of technological know-how for achieving such gains in principle.

In effect, the question being posed for LDCs concerns the extent to which political commitments, abetted by a modicum only of added economic and health-related administrative resources, could be expected to substitute for—or significantly complement—limited, erratic, or even near-stagnant development.

Formally, much the same question could be asked for DCs, although the mortality and development indices relevant for applications would obviously differ greatly in the two sets of instances.

Obstacles to establishing viable indices are not likely to be small but are probably surmountable. The analysis I am proposing is formally close to the use of economic and educational production function variables in other disciplines, where analogous concepts have played valuable theoretical and measurement-guiding roles. Economists and many noneconomists will recognize that the tradeoff relations mentioned earlier are like those provided by the isoquants or equal-output schedules of production theory. We may regard "next major gains" of mortality as analogous to outputs of goods or services, for example; similarly, the health and development change indices just cited are analogous on the input side to increments of labor, capital, or other main factors of production.

Given the indicated indices and relevant mortality starting points, alternative combinations of health-focused and development-related inputs could be identified which could be expected to yield a targeted mortality gain as a consequence. One procedure among several for doing so would be through the use of regression models, with the posited indi-

ces appearing as nonlinear determinants (to allow for changing rates of substitution among inputs for a given amount of gain) and with additional or control variables inserted as necessary.

Two major classes of question could then be answered. First, how much added input of health-focused factors might compensate for development-related (and other) impediments to a targeted change, or might adequately complement limited developmental contributions to such change? Second, how should such tradeoff or complementary input relations be expected to vary with starting levels and amounts of targeted changes? For example, what input relations might be expected to hold when attempting to move from a 40-year to a 45-year level of life expectancy rather than from 55 years to 60 years, or in moving from an infant mortality rate of 100 per 1,000 births to one of 75 as compared to a shift from 75 to 50? Or equivalently for many or most applications in developing areas, what powers do governments have to effect technically feasible mortality gains when developmental or related impediments to such gains continue in force?

Answers to these questions, in my opinion, have largely eluded us since the beginnings of demographic study. For over a century at least and up through our latest textbooks, we have been told that the mortality revolutions of the past have been owing to medical advances and rising levels of living. The difficulty with such explanations is not that they are wrong in a qualitative sense. Rather, it is that they provide such thin gruel indeed for nurturing our sense of relative causal contributions to change, of especially probable strategic determinants, or of main policy options to consider for targeting next trends. As a result, I believe, we continue to misinterpret the great mortality transitions we have seen and are likely to see. A century or so ago in many of today's lowest-mortality areas, death rates in over a dozen coun-

tries began—almost simultaneously—to plunge sharply downward following decades of minor change. Why this occurred despite vast differences in levels of development, political systems, ongoing economic trends, or socioeconomic structure remains unexplained to this day in terms of causal factors assessed for relative importance. Similarly, we may well ask why, for at least a generation during the interwar years in this century, we failed to grasp the probable import of trends observed in an array of colonial areas, for all of which reasonably reliable data were already at hand. Or, still again, why we were so slow to recognize the nature and implications of the greatest mortality transformations of all: those recorded since about 1950 in so many dozens of the world's highest-mortality and lowest-income areas despite enormously varied socioeconomic circumstances, diverse initial mortality conditions, dissimilar ecological backgrounds, and heterogenous political systems. Or, finally, why such transformations have remained so notably resistant to reversal in the face of enormously variable development trends and endlessly frequent political shocks. Probably no pre-1950 mortality transition, however well understood, could have prepared us in depth for the Third World's experience that lay ahead. But it is also true that we should have been much better prepared than we were to assess the new possibilities in the light of the evidence at hand even by 1940 and surely by 1950. (See, for example, Davis, 1956; Stolnitz, 1955; United Nations, 1963, Chapters 2 and 4.)

In all of the above instances, I believe, demographers have greatly underestimated the powers of specifically public health, sanitation, and associated technologies to offset low levels of living and underdevelopment more generally.

Conversely, we should have been much less surprised than analysts have generally been by the recent slowdown of LDC mortality declines. There is am-

ple empirical evidence by now to the effect that nondevelopmental influences should tend to become decreasingly potent in relative terms, and that developmental influences should become increasingly important, once longevity has made an appreciable rise from less than or near 40-year levels and reached 55-year or perhaps 60-year levels (Gwatkin, 1980; Stolnitz, 1975). Beyond this approximate range, the disease-control inputs needed to cope further with changing cause-of-death regimes are likely, under current state-of-knowledge conditions, to place increasingly heavy demands on economic, educational, and public administrative resources. A few LDCs, such as Sri Lanka and possibly Cuba and China, appear to contradict this rule, but many more conform to it (World Bank, 1983, Table 23).

Beyond about 75 years, or today's peak longevity level for combined male and female populations, the tradeoff or complementary relations to be expected are, of course, bound to be highly speculative. Conceivably, these could shift back to a health-related emphasis, with health research policies, advances in medical technology, and individually adopted shifts in life styles becoming relatively predominant compared to previous decades and changes in the conventional indicators of development becoming relatively less strategic.

To illustrate, let me compare briefly the cases of a region with approximately a 40-to-45-year level of life expectancy and one with a 70-to-75-year level. In much of Black Africa, where food-supply conditions have been generally deteriorating since about the early 1960s, substantial development would appear to be a nearly nonsubstitutable factor for achieving near-term mortality gains. But even here, an opposite interpretation seems to me more plausibly indicated. No more than average food-supply and average health-related inputs, supplemented at times of agrarian crisis by amounts of outside food and medical

contributions which are trivial by international standards, should prove decisive for several added 5-year or so longevity gains over the next several decades—assuming, that is, that no more than “tolerable” political stability and moderate gains in public-sector efficiency can also be achieved. Approximations of such possibilities have been frequent over the past three decades, both in Black Africa itself and in most of Latin America and Asia.

For populations with peak-level longevity, on the other hand, a possibly unique blend of developmental and nondevelopmental influences may be needed for a next major-scale uptrend. In the United States, for example, developmental tendencies over the last decade or two may well have had only second-order impacts when compared to policy and health-related influences. Indeed, rising affluence, continued urbanization, and the growing predominance of service industries may well have been counterproductive rather than supportive from longevity gain viewpoints—by fostering sedentary life styles, enhancing city-life tensions, and making possible major-scale reductions in the real (labor-time) costs of excessive eating, drinking and smoking. Meanwhile, resources committed to medical and public health research and development, while clearly not small, have been only minor or trivial fractions of GNP and public-sector budgets. And the ever-increasing differences which continue to be registered between female and male life expectancies—a spectacularly uniform directional pattern found repeated in all industrial societies ever since World War I—are not easily correlated with level-of-living indicators as such. If these considerations were all, one could expect the relative weight of developmental factors affecting U.S. mortality to contract, rather than to expand, in the years ahead. On the other hand, total health-related outlays in this country have risen to fully one-tenth of total GNP, as both amounts and relative

costs of medical care have continued to soar—a pattern which has been clearly dependent upon expansive economic trends. Nor are these isolated patterns. Similar tendencies are found in numerous other developed areas.

Which set of influences will predominate over the decade or two ahead, those closely dependent on long-run economic expansion or those more nearly linked to changing medical technology and health-supporting shifts in life styles, has become especially obscured by a further development, unique in demographic history. Starting about now and for the first time ever, even moderate or five-plus year gains in the life expectancy of the world's lowest-mortality nations will have to stem mainly from declining death rates beyond midlife. With mortality below 50 reduced to zero, for example, expectation of life at birth in nearly all DCs would only rise by much fewer than five years for females and by little more for males. Since previous post-midlife mortality gains among DC populations have rarely if ever raised their expectations of life at birth by more than minor amounts, it follows that future five-year or larger advances will necessarily be dependent upon novel configurations of age-specific and causal factors. The arithmetic is such that the requisite upper-age gains would have to swamp by several orders of magnitude the gains actually achieved over the entire past century or even longer.

Assessment of these possibilities is likely to merit priority research attention for reasons going well beyond mortality analysis and policy proper. Involved also are probable unprecedented impacts on aging, since the envisaged later-age mortality downtrends would not only reinforce aging impacts caused by low or declining fertility, but might also begin to rival fertility in degrees of such impact. Either development, when or if it happens, would be the first such occurrence in the history of modern demographic transitions.

Demographers have tended to overlook these prospects, I believe, by focusing excessively on comparative-static analyses of stable-age or quasi-stable models. Possibly for this reason, they have devoted much too little attention to the age-specific percentage changes of survival rates which are at the heart of empirical mortality-trend impacts on age composition (Stolnitz, 1956).

### *Internal Migration*

I need hardly emphasize that internal migration is easily the weakest sibling among the components of population change, analytically speaking, or that the research obstacles in this field are legion even at the simplest levels of data collection and presentation. It is no accident, for example, that the United Nations *Demographic Yearbook* series, now more than 30 years old, has yet to publish its first table on internal migration anywhere, despite repeated reports by governments that their most frequent population policy concern is with spatial distribution patterns (United Nations, 1979c, pp. 94–95). However, the question of main data needs and general research priorities has been comprehensively discussed elsewhere (see especially Goldstein, 1981) and I prefer to focus on the specific theoretical problem cited earlier in this section. More precisely put, how can one formally identify desirable amounts or even directions of internal migration flows among multiple areas of origin and destination?

Remarkably, an extensive literature review has so far been unable to unearth a single demographic source on the subject. (For some main sources of relevant literature reviews, see Findley, 1977; Goldstein, 1981; Myers and Macisco, 1975, pp. 221–231; National Institutes of Health, annual inventories of sponsored research; Rogers, 1968, especially Chapters 6–7; Shaw, 1975; United Nations, 1973, especially Chapter 6.) Indeed, the continued neglect by demographers of the evaluative aspects of migration pro-

cesses is the more striking when we consider the intimate linkages connecting internal migration flows to both short-run economic change and longer-run development. To an extent unmatched by either mortality or fertility, such flows tend to be directly and immediately responsive to shifts in industrial structure, interarea income differentials, sectoral employment opportunities, and regional development tendencies, among others. In sheer demographic terms as well, internal migration stands out for its relatively immediate significance for development trends and prospects. In the less developed regions, for example, where average urban growth has been at a 4 percent order of magnitude for over a quarter-century, or far higher than national rates, internal migration is estimated to account for at least one-third of such growth. In the more developed regions, the contribution of net migration to urban growth has been even higher,—some 50 percent (United Nations, 1979b, Table 32; 1980, Chapters 2–3). For these reasons alone, internal migration would tend to be more closely attuned to the short-run and intermediate-term concerns of governments, development planners, and other development analysts than either mortality or fertility.

I expect that neglect of the migration allocations question being posed is owing to something more than data deficiencies as such, large as these are. In addition, three theoretical difficulties in particular seem to me to stand out as resisting solution. One is that the problem of identifying desirable or ameliorative spatial flows of population calls for satisfying simultaneously multiple optimization criteria. Any migration movement, actual or potential, involves consideration of a pair of areas at least, and the number of possible pairings (of two or more areas) mounts up very rapidly with the number of origin and destination places singled out for study.

A second main problem, one likely to be central for purposes of policy analy-

sis, is that the benefits and costs of migration as viewed by individuals often tend to differ sharply from those relevant to societies or economies. Consequences of migration which are rarely if ever considered by individual migrants include such factors as their impacts on: volume of needed public-sector services; size of tax base in areas of origin and destination; agglomeration economies or diseconomies; costs of congestion and pollution, or of quality of communal living more generally; efficiency of education and health programs; and costs of subsidized housing. Most or all of these are likely to be unknown, unknowable, and ignored by individuals and households when deciding to migrate or to remain where they are.

A third source of complications concerns the variable life-cycle intervals that may be involved in many or most migration-type decisions. These may relate to temporary or permanent moves; one-time, circular, sequential or return moves; experimental or confirmed shifts of location; or to any one of numerous possible combinations of these. And however certain an observed or contemplated migration sequence may appear to be, there is always the difficult question of how to evaluate alternative time streams of benefits and costs (private, social, or both) when these differ markedly as between areas of origin and those of destination.

Our Indiana research group has attempted to develop methods for dealing with each of these problems. With respect to the first or allocations problem, the only one I can report on at this time, we have developed an approach which lends itself to numerical solutions and which can accommodate variable numbers of origin and destination areas, flexibly defined productive sectors in each such area, and a succession of time periods. The approach utilized is that of mathematical programming, which uses regional production functions by industry sector as parts of an overall objective

function and thereby identifies the labor-force distributions needed to maximize national output in each study period. Differencing each previous period's distribution of labor force (adjusted for mortality if important) from the "optimal" distribution obtained for a following period yields the interperiod migration or mobility movements requisite for aggregate output maximization. The indicated labor-force transfers, in turn, can be specified to occur in a way which would minimize the private or social costs of movement, based upon indications deducible from transportation programming procedures.

Labor force in this approach can be disaggregated by skill and other main attributes, and probable population relocations associated with labor-force movements can be estimated on the basis of population-to-labor force transfer ratios.

For the sake of initial illustration, use has been made of separable and piecewise linear concave production functions, but substantially wider classes of input-output relations can be accommodated by the underlying technique. (See Sabbaghi, 1983, for a more detailed description; Schinnar, 1976 and 1977, provides a useful discussion of some underlying general theory.)

With respect to the other two special problem areas cited earlier, work has begun on assembling a comprehensive array of private and social benefits and costs likely to be associated with LDC rural-to-urban migration. This too, so far as we can tell, is being explored in depth for the first time.

I would be reluctant indeed to suggest any more than that these beginnings are not without promise. And I would surely hope that others in our discipline would join the search.

### *Fertility*

Moving to the third question I have posed, I am aware of course that the issue of socioeconomic or development

factors affecting LDC fertility is surely among the most widely and intensively investigated subject areas of our discipline. The forthcoming National Academy of Sciences report on LDC fertility determinants (expected in 1983), with its over 1,000 pages of manuscript and several thousand citations, is evidence enough of demography's enormous research investment in this area from conceptual, modeling, measurement, hypothesis-testing, and case study points of view.

However, another look at the matter suggests a less encouraging and even disconcerting conclusion. The literature search project at Indiana indicates that our knowledge base for assessing the influence of LDC fertility determinants in quantitative terms is often remarkably narrow and shaky. Far from being assured of richly documented sources or possible or probable degrees of influence—or even on how to estimate such influences—one frequently finds only thin or wavering research guidelines to go by, even with respect to presumably major determinants.

Since these comments are selective rather than broad-brush, it is necessary to deal with specific cases. Each of the illustrations to be discussed next is widely regarded in the professional literature as a major actual or potential determinant of fertility transition in high-fertility areas, and each has been singled out by the World Population Plan of Action as having special importance for formulating population-related development policies (United Nations, 1975, pp. 12-16).

Consider, for example, the status of our cumulative research on the probable importance of old-age security motivations or benefit programs as determinants of LDC fertility behavior. So far as we can tell after a considerable search for sources, no study has attempted to deal with the role of such motivations in quantitative terms—meaning by this last regression-type, cross-tabular, or equivalent quantitative estimates rather than

attitudinal-related assertions or assumptions, about which much has been published.

With respect to the possible role of security programs, the studies we could identify which focused on the subject were extremely limited in number, less than a dozen in all; used cross-national data as units of observation, so that their relevance for intertemporal analysis of individual populations is in serious question; employed highly variable measures of old-age or other social-security program magnitudes; differed widely in choice of control variables; and often relied on varied and questionable procedures for imputing missing data values.

As to the results actually encountered, the theoretical and available empirical literatures are in directional agreement, in that national fertility levels do in fact appear to vary inversely with nationally aggregated sizes of benefit programs. Beyond this, however, little of analytic or policy interest can be deduced from the results as presented. To begin with, the quantitative effects suggested by the better or best designed studies we could locate were found to be enormously variable relative to each other, with the larger estimated effects dozens of times higher than the smaller estimates. Moreover, none of the studies examined controlled even approximately well for alternative possibilities for seeking old-age security protection, say by relying on own savings or other assets rather than on government programs or children. Finally, the size of old-age or other social-security programs needed to bring about one-child decreases in lifetime fertility would appear to require program expansions which are clearly far beyond the fiscal capacities of all or very nearly all LDCs—and wholly beyond the pale of any least developed region's capacity.

I conclude that the most positive statement demographers and policymakers can make about institutionalized old-age security programs as fertility-reducing mechanisms is that their potentials for

impact have neither been established nor disproven. The LDC programs now in existence appear to be too recent, as well as much too partial in coverage, to be assessed for mass credibility or efficacy. And more to the point of widely held theoretical anticipations, no study as yet has sought to quantify the role of old-age security motivations at household decision-making levels. (For a fuller discussion of these findings, see Wildasin, 1983.)

Child labor, my second example of a widely recognized significant determinant of LDC fertility propensities, leads me to a less negative assessment, but only relatively so. On the one hand, measures of LDC household fertility are found, with impressive consistency, to vary positively and elastically with indicators of child labor-force activity or earnings, other things equal. Moreover, child labor appears to account for surprisingly large amounts and fractions of household incomes. Presumably, since substantial decreases in nonfamily labor by children should be associated with substantial decreases in childbearing, child-labor laws should encourage fertility downtrends. A further conclusion, again persistently indicated on a *ceteris paribus* basis, is that wages of adult females vary inversely with household fertility, so that expanded labor-force opportunities for women should also have significant fertility-reducing effects.

On the other hand, although these are significant findings if valid, I must caution that they rest on an exercise in maximal interpretation. The literature so far available on LDC child labor-fertility interrelations varies greatly with respect to guiding definitions, types of data sets employed, choice of statistical control variables, and choice of model specifications more generally. More than this, it suffers from severe limitations of omission. Perhaps a dozen studies are at hand for all Third World populations combined; fewer still authors and populations have been involved in the available

research; urban data are very largely lacking; and child labor in the middle or later teenage years, when it becomes especially important, is often not considered. Life-cycle and incremental aspects of fertility decision-making are ignored as a rule, and teenage unemployment risks are ignored uniformly. The possible feedback influences of fertility on child labor, and the possibly significant role of "supply-side" influences (for example infant mortality) on fertility, often do not receive explicit attention when such attention seems called for to avoid estimation biases.

The literature is further unrevealing with respect to its implications for policy. Assuming that LDC fertility levels always moved in parallel with amounts of child labor, it would still be necessary to show that the expected welfare gains from child-labor laws or other disincentives would persuasively outweigh prospective welfare losses. High LDC fertility among impoverished families, especially in rural contexts, is often associated with extreme dependence on all sources of family income. In the absence of effective income maintenance programs—a wholly unlikely prospect—child-labor reforms, however desirable on demographic grounds, could be welfare-damaging rather than welfare-enhancing on balance. (The above discussion is based on Rivera-Batiz, 1983.)

Such *policy-focused* reservations are, of course, not unique to child labor. They surely apply to other major areas of population-responsive policy issues as well, or whenever the demographic and nondemographic consequences of fertility-related development policies are potentially in conflict. Urbanization is another such case in point, as are the last two illustrations which I have time to discuss, involving internal migration and income distribution.

The importance of rural-urban population transfers for LDC urban and rural growth has long been apparent, as noted earlier, while their potential impacts on

fertility levels have been actively debated by proponents of adaptive and selective migration theories for nearly a half-century. (For two relatively recent and extreme versions of these positions, see Lee et al., 1981, and Ribe and Schultz, 1980.) Yet so far as I can tell, no study has yet been published which has used a data base adequate for the question.

Such data bases should, in principle, be sufficient to provide the following information: (a) national origin-destination cross-classifications of rural and urban areas, distinguished by regional, size-of-place, and socioeconomic characteristics; (b) data on birthplace and on previous and current residence for all surveyed individuals, in addition to fertility-relevant characteristics (thereby permitting documentation in equal depth of fertility behavior and propensities among mover groups and closely similar comparison groups in origin areas, a prerequisite for making adequate selectivity-adaptation distinctions); (c) fertility data in sufficient chronological detail to distinguish pre-migration from post-migration amounts of childbearing, current from cumulative fertility, and temporary disruption effects from longer-run consequences associated with migration; (d) durations of current residence among both mover and stayer groups (and, to the extent possible, documentation of lifetime urban and rural or size-of-place residence durations).

This list is less complicated than it may appear. Nationally surveyed joint fertility and residence histories, if added to conventional census-type questionnaires, could in principle satisfy all of the above prerequisites for analyzing migration-fertility interrelations in adequate depth. Moreover, they could do so not only for urban-rural population transfers, which have attracted most attention to now, but also for urban-to-urban and rural-to-rural movements, which are often much more numerous. As matters stand, most studies of urban stayer-urban migrant fertility differentials fail to

distinguish between rural and urban migrant origins, while the relatively few that do so practically never distinguish between pre-migration and post-migration fertility measures. No study I have seen distinguishes adequately among urban size-of-place categories and rural areas distinguished by main socioeconomic characteristics. And with apparently one exception only (Lee et al., 1981), demographers have simply ignored the challenging problem of converting estimated migrant-nonmigrant fertility differentials into national, urban, or rural fertility impact measures (Stolnitz, 1983a).

Finally, the LDC research situation seems to me to be worse off still with respect to estimated fertility impacts attributable to income distribution or redistribution patterns. As I have recently had occasion to conclude, the quantitative literature on this highly charged subject "can be fairly described as extraordinarily limited in scope, schizoid in methodology, largely irrelevant in one of its two main proposed approaches and wholly ambivalent as yet with respect to the other. Studies which even moderately resemble quantitative investigations of the subject barely number a dozen, vary enormously in approach, and can be prudently interpreted to imply contradictory policy conclusions" (Stolnitz, 1983b, p. 1).

Most of the few studies to be found on how LDC fertility varies with income distribution are cross-national and deal with entire-population income share measures or inequality indices. As a result, they can provide limited guidance at best on how income redistribution might affect fertility within nations. While such macrolevel relations are inverse as a general rule, the causal directions actually involved may well be underidentified, since fertility may affect income distribution in significant ways as well as be affected by it. Weak fertility responses to interarea variations in income distribution are indicated in any event, and pre-

dictions in parts of the literature that early fertility transitions would be unlikely in countries with relatively pronounced economic inequality patterns have often been belied by the numerous downtrends registered since about 1970.

Within-country, microlevel indications based on household data have been even less explored or revealing. Only two authors are known to have dealt with the question of how altering income differentials might affect aggregate fertility, and these reach apparently opposite main conclusions. Specifically, available data sets can be interpreted to show that egalitarian income redistributions may either moderate or enhance a population's aggregate fertility, depending on whether one relates household fertility to household total or to per capita incomes. (See Boulier 1982; Repetto, 1979 and 1982; Stolnitz, 1983b.)

In concluding this recital of research shortcomings, I hope it is clear that my list of illustrations, though readily expanded, is intentionally selective. Counter-examples pointing to opposite indications are also relatively easy to find. The World Fertility Survey (1981), for example, has surely added enormously to our quantitative knowledge of how numerous LDC fertility determinants operate; and many individual efforts, such as the pioneering work by Cochrane (1979; 1983) on education-fertility interrelations, clearly merit being further singled out in this connection.

Unfortunately, it is also true that the WFS does not deal with many developmental processes and that education has been much more the exception than the rule with respect to the documentation available on its quantitative significance for LDC fertility behavior. Unfortunately, too, the WFS contributions appear all too likely to become the product of a historically unique exercise, one soon slated for extinction. As an overall statement, therefore, it surely seems fair to say that we have not yet established either a secure or a crecive quantitative

basis for explaining many main aspects of why, how, and when development induces high-to-low fertility transitions.

Most of the difficulty, surely, is intrinsic to the complexity of the subject itself. But some I believe to be "managerial." We seem to lack systematic means for quantitatively verifying widely cited and accepted propositions concerning LDC fertility levels or trends and their main determinants. In some measure, too, the difficulty to which I refer may stem from methodological origins, among which the two cited next seem to me to rank relatively high in importance.

#### TWO AREAS OF METHODOLOGICAL CHALLENGE

##### *Macro-Micro Linkages*

Most of us have lived through several statistical revolutions in our professional lifetimes, but I doubt that any will prove more far-reaching than our new potentials for gaining ready access to very large collections of microlevel data. The advantages of microlevel over macrolevel units of observation in demography, as in all behavioral disciplines, are both numerous and mainly obvious: degrees of freedom advantages; deeply dimensioned cross-classifications in place of necessarily shallow ones; effective avoidance of cluster and aggregation biases; closer adherence to the standard assumptions of regression analysis; and direct access to decision-making units rather than at best inferential access, among others.

To these I would add a further major advantage, frequently overlooked, concerning the reduced risks of simultaneity biases. Regressor variables in demography, when viewed at macrolevel scales of observation, may well be affected by dependent fertility, mortality, and migration variables in addition to affecting them. In contrast, the same regressors may at microlevel scales be safely specified to be exogenous or given from the viewpoints of both statistical theory and

its nonproblematic application. Area wage or unemployment levels, for example, may well be partly determined by area fertility rates, yet are unaffected by the fertility rates of any of the individual households belonging to the same areas.

The advantages of microdata over macrodata sources are not entirely one-sided, however. Special problems may arise with the former sources which, though surmountable in principle, often pose unrecognized or at least unmet research challenges in practice. Moreover such problems may apply whether the variables under study are attitudinal or behavioral. With respect to attitudinal data, individual households when surveyed individually are likely to respond quite differently than they would when in interactive communication with peer groups or opinion leaders. In addition and more important from many policy viewpoints, such responses are likely to be unrevealing of how attitudes might change under policy-induced shifts in social, institutional, or educational environments—a point long driven home by LDC surveys related to family planning. The challenge here, seldom met adequately except in some of our most carefully planned surveys on fertility aspirations, has been how to convert recorded decision-affecting attitudes, individually expressed, into indicators of socially conditioned decisional propensities. Similarly in the case of internal migration surveys, usefulness for policy may well require that we record a good deal more than the benefits and costs of movement as perceived by household decision makers. Also needed may be indications of how such perceptions are likely to respond when benefits and costs become altered or increasingly uncertain, whether because of policy itself or for other macrolevel reasons.

A very different linkage problem arises in the case of household behavioral variables. No matter how well we can explain, predict, or otherwise analyze microlevel behavior by analytically

relevant cross-classifications, it is still necessary for many major research purposes to obtain entire-population behavioral aggregates, say a national fertility rate from expected rates by age-marital status-residence-skill level sub-aggregates. The problem this raises, of determining relevant compositional frequencies, is an ancient and obvious one in a sense, going back to when the first demographer decided to look at sub-population differentials as a means of explaining entire-population outcomes. What is new, however, is that the problem has grown greatly in complexity as we have come to deal more and more with population categories other than sex and age. Consider, for example, how one might deal with the changes in a national fertility rate associated with expected migrant-nonmigrant fertility differentials, with income distribution shifts among married-couple households, or with changing female labor-force participation patterns. Problems of this kind are sure to grow in importance as demographic analysis becomes increasingly integrated with human resource development issues or with development process analysis more generally.

It is easy to indicate briefly why our traditional sex-age standardization-type approaches are likely to be inadequate for problems of the kinds just cited. Standardization procedures intended to hold composition constant, for example, are inherently arbitrary at best, since behavioral rates are combined with frequency weights mechanically rather than with attention to their possible interrelations. Arbitrariness again arises when such procedures are applied to partitioning an aggregate demographic change into behavioral and compositional components—a frequent recent example being the relative contributions of age composition, marital status, and marital fertility rate shifts to a national change in total fertility. The difficulty here is that the contributions as measured depend on the order in which contributing factors

are introduced in succession, and it is plausible to expect such “succession effects” to increase with rising numbers of compositional attributes.

To my knowledge, many of the outstanding problems in this general area have not been addressed in the literature.

#### *An Overdue Statistical Transition*

Some 25 years ago, econometricians began an intensive investigation of the comparative merits and limitations of multi-equation and single-equation statistical models in economics. The venture was not without its combative side and, predictably, the issues were never fully resolved. Nevertheless, considerable clarifications and advances were registered, with the result that economic regression analysis has been greatly transformed in its modeling and inferential procedures.

I mention this bit of history because I think it has much to inform demography even today. At least three main classes of reasons lead me to believe that demography is overdue for a rapid departure from its still standard single-equation modes of approach.

The problem of avoiding simultaneity biases surely ranks high among such reasons, since it arises whenever significant two-way influences connect explanatory and dependent variables. We know from theory that the statistical cost of ignoring such influences can be heavy and from practice that their prevalence is likely to be wide-ranging. Feedbacks between dependent and explanatory variables are likely to be especially prominent in the case of area-level demographic and socioeconomic observations, as noted earlier, and frequent similar relations among microlevel variables are also to be expected. Thus, if high household rates of infant and child mortality are likely to raise household fertility, it is also the case that high fertility acts to raise young-age mortality, again within households. Education and fertility link-

ages may be similarly bidirectional; in both LDCs and DCs rising education tends to reduce fertility, but also high fertility levels often inhibit education. Rural-to-urban migration, long hypothesized to have a downward effect on national fertility, may itself be encouraged by low-fertility propensities among migrants. And in the probably outstanding instance of widely recognized bidirectional linkages in fertility analysis, or those holding between childbearing rates and female labor-force participation rates (whether at microlevel or macro-level scales of observation), it is commonly agreed that we are still far from developing statistical models adequate for representing both sides of the feedback processes involved. Numerous further examples could be cited, but these may suffice.

A second main need for multi-equation modeling is to estimate by statistical means an explanatory factor's total causal influence, as opposed to its direct influence only under *ceteris paribus* assumptions. If, for example, we consider a single-equation model in which fertility is being regressed on education and other explanatory variables, the sign and magnitude of the education coefficient are only designed to convey its influence when the remaining regressors are held constant. Yet education may exert significant additional influences on fertility indirectly, through its effects on income, labor-force participation, age of marriage, urban-rural residence, or other of our main socioeconomic variables typically included when analyzing childbearing behavior statistically. Such indirect effects can only be adequately captured through added equations. A similar conclusion applies when there are indirect influences between one or more included variables and an omitted main variable; here standard single-equation regression methods lead to biased and inconsistent estimators.

The third reason I would stress derives from the second. Indirect influences on

population change are often interactive rather than simply additive, say as exercised on mortality by both education and income or on fertility by both urban-rural residence and occupational status. The findings of the Indiana project on LD fertility determinants suggest to me that synergistic explanations, rather than one-at-a-time causal attributions, may well be central or even crucial for seeing how high-fertility patterns become dislodged from dead center. Such explanations can only be obtained from entire systems of equations, without which—I have become personally convinced—demographers' attempts to endogenize the main variables of demographic transition are bound to remain frustrated.

I also believe that, very probably, the expanded models that will be called for by tomorrow's main macrodemographic issues will come to rely increasingly on the use of time series sources, once these become sufficiently valid and extensive to permit the use of statistical inference. But this would take me into a sixth area of main research challenges, and I have reached my allotted five.

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