

# **DEMOGRAPHIC DESTINIES**

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

### **Interview with Ansley J. Coale PAA President in 1967-68**



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

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

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

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## ANSLEY J. COALE

PAA President in 1967-68 (No. 31). Interview with Jean van der Tak in Dr. Coale's office at the Office of Population Research, Princeton University, May 11, 1988, with excerpts from Dr. Coale's interview with Anders Lunde during the PAA annual meeting in Philadelphia, April 27, 1979.

**CAREER HIGHLIGHTS:** Ansley Coale was born in Baltimore, Maryland in 1917. Except for four years during and after World War II, he has spent all of his career at Princeton University, beginning as an undergraduate. From Princeton he received the B.A. in 1939, the M.A. in 1941, and the Ph.D. in 1947, all in economics. He has also received honorary degrees from the University of Pennsylvania and the Universities of Liege and Louvain in Belgium. From 1947 to 1954, he was Assistant Professor of Economics at Princeton, before returning to demography and the Office of Population Research, where he had been the second Milbank Memorial Fund Fellow and a research assistant before his wartime service. He was the second Director (following Frank Notestein) of the OPR from 1959 to 1975 and Professor of Economics from 1959 until 1986. Since 1986 he has been Professor Emeritus at Princeton, but continues his research and writing at OPR. Among his other posts, he has been U.S. Representative to the United Nations Population Commission (1961-67), President of the International Union for the Scientific Study of Population (1977-81), and Chairman of the Committee on Population and Demography of the National Academy of Sciences (1977-82). He is a member of the National Academy of Sciences and winner of both the PAA Mindel Sheps Award in Mathematical Demography and Demographic Methodology (1974) and the PAA Irene B. Taeuber Award for Excellence in Demographic Research (1989).

Ansley Coale is famous in the field of demography for his innovative work in both the mathematical aspects of population and the social and economic implications of population structure, growth, and change. In addition to many articles, his influential publications include such monographs as Population Growth and Economic Development in Low-Income Countries: A Case Study of India's Prospects (with Edgar M. Hoover, 1958), New Estimates of Population and Births in the United States (with Melvin Zelnik, 1963), Regional Model Life Tables and Stable Populations (with Paul Demeny, 1966), The Growth and Structure of Human Populations (1972), and two monographs in the Princeton European Fertility Project which he initiated in 1963: Human Fertility in Russia Since the Nineteenth Century (with Barbara Anderson and Edith Harm, 1979) and The Decline of Fertility in Europe (edited with Susan Watkins, 1986).

[He died in 2002 at a retirement village in Newtown, Pennsylvania, at the age of 85. The cause was heart failure following several years with Parkinson's disease.]

**VDT** [from biographical introduction]: You retired from the Office of Population Research in 1986, I understand, but here you still are, having just ridden in on your bicycle, which do every day. Do you also play tennis every day? Charles Westoff told me yesterday that he plays tennis every day.

**COALE:** Well, no more than six or seven days a week. Sometimes it rains or my opponent cancels out.

**VDT:** That's at lunch time, between riding in and out on your bicycle. How far is it to ride?

**COALE:** Counting the ride over to the tennis courts and back, I do five miles a day--on an old Sturme-Archer three-speeder.

**VDT:** That means more exercise.

**COALE:** I never got adjusted to the ten-speed.

**VDT:** When and how did you first become involved in the field of demography?

**COALE** [This combines Dr. Coale's answers in 1979 and 1988]: I started in population for reasons of what you might call cupidity. I was an undergraduate at Princeton. I had no firm idea of what I wanted to do. I wasn't even sure that I wanted to go to graduate school. I didn't have a job and it was in the Depression. My father was a Presbyterian minister. As I recall, it cost my parents something like a total of \$200 for me to go to college. Things were different in those days. The tuition and fees for the library and so on totaled \$550 for a year. I had a scholarship that was worth \$625, so that paid my tuition and fees, with \$75 left over. My room, which was part of a two-bedroom unit with a living room--you would call it a suite now--cost \$125. So I needed only \$50 more than my scholarship to pay for tuition and room. I waited on tables in the common; 11 meals a week, it turned out, was enough to pay for my board. I was self-supporting from the end of my freshman year. I was not in a position to ask my parents to support me in graduate school.

I went to see the dean of the graduate school at Princeton, who was also chairman of the math department, and said I was toying with the idea of going on to graduate school. When I entered Princeton as a freshman, I had intended to major in mathematics or physics, which both fascinated me. My switch to economics at the end of my sophomore year probably had something to do with the salience of economic problems during the Depression. I told the dean I'd like to go to a place where I could combine mathematics and economics in my graduate work; mathematical economics didn't really exist as a discipline in 1939. He said, "Well, Harvard and Michigan have good mathematics and good economics. But probably the best place would be to continue right on at Princeton."

Not long after that, he called me in and told me that Frank Notestein had become director of the Office of Population Research two years earlier [in 1936] and the funding of the Office included a graduate fellowship provided by the Milbank Memorial Fund for an economics Ph.D. candidate interested in population. It paid tuition and living expenses. I had no money, so the only way I could go to graduate school was with a full ride. I was willing to have an interest in population in order to have a fellowship that would pay my way. In those days, one had to qualify in five different fields within economics for the general exam to qualify for the Ph.D., and demography could be one of those fields. All I had to do to get this fellowship was to agree to make demography one of the fields.

That's how I got started. I became the second Milbank Fellow, after John Durand. John and I overlapped for a while. I did get very interested in demography. I took my general exams for the Ph.D. in economics, including demography, in the spring of 1941.

[On OPR before the war, from the 1979 interview with Anders Lunde]: The OPR when I joined it consisted of Frank Notestein and his secretary, Henry Shryock as research associate, and John Durand as a graduate student, and that was it. Dudley Kirk joined the Office while I was the Milbank Fellow. I left out a crucial person--Irene Taeuber. I didn't think of her because she always worked in Washington. She was editor of Population Index, raising her two children and writing about 20 articles a year. She used to send up slips with the abstracts of the titles that were to appear in Population Index. In those good old days, the Milbank Fellows had to proofread all the copy of Population Index four times a year. So I would sit with John Durand and Dudley Kirk and we would proofread. It went on to the secretaries who typed it for photo offset reproduction. That was how it came out.

Subsequently, while I was there, the Office started to work on a series of books for the League of Nations, starting with The Future Population of Europe and the Soviet Union. Different people came to work on that, including Wilbert Moore. Frank Notestein was not only director of the Office but he also taught the two graduate and undergraduate courses on population. In fact, we continued to offer only two courses at Princeton up until about 1970.

**LUNDE** [from the 1979 interview]: Population Index, was that supported at the time from outside?

**COALE**: It was supported partly by the Milbank Memorial Fund, which also gave the handsome sum of \$10,000 a year for the Office. From the beginning, it was the essential organ of the Population Association of America. Part of the dues of the members went to Population Index.

**LUNDE**: I'm interested in what you say about Irene. I asked her once, "How in the world did you get all of this done with the kids in the house?" She said, "Well, with card tables in the living room, a card table for each job." And the kids were told, "Stay away from the card tables." She must have been well organized as a person.

**COALE**: She was extremely productive and scholarly. In addition to raising a family, she prepared the entire bibliography of Population Index for many years, wrote hundreds of articles, and several books, including the encyclopedic Population of Japan [1958].

**LUNDE**: What was Dudley Kirk like in those days?

**COALE**: A good friend and productive scholar, who sometimes had trouble meeting deadlines.

**LUNDE**: Maybe you ought to put into the record your discussion with me a minute ago about what the heck is happening to the money for this year's Irene Taeuber Award [for Excellence in Demographic Research].

**COALE**: Well, it's true that Dudley Kirk upbraided me. He said, "Did you bring a check for \$1000?" The Irene Taeuber Award was funded by contributions that went to Princeton, which agreed to hold the money and provide from it \$1000 for the award every two years. Dudley has been chairman of the committee deciding who should receive it [in 1979]. So he asked me, "Do you have the check for \$1000?" I thought, my God! Am I supposed to have it? Dudley then told me who the recipient is.

This morning, after feeling guilty all night, I realized that I couldn't have had a check for \$1000, since Dudley had not told me the name of the winner! So we'll have to give the winner a promise to send him the check later on.

**VDT** [continuing 1988 interview]: When you were starting graduate work, did you get your master's as a Milbank Fellow?

**COALE**: Well, Princeton has a snobbish attitude about advanced degrees. In the liberal arts and sciences, they will not accept a candidate for a master's degree. They don't have master's degree programs, except in the professions like engineering and architecture. What happens is that you complete the course requirements for the Ph.D., passing the examinations in five fields, plus language and mathematics and things of that sort. Then you are qualified to write a dissertation. If the department decides that you've barely passed the general examination and they don't think you're very promising, they will give you--as a booby prize, so to speak--a master's degree. You get it en route to the Ph.D. anyway. I remember that they charged \$35 for the M.A. diploma and I didn't want to take it; I figured I'd get my Ph.D. My mother said, "That's ridiculous." She came up with the \$35--and kept the diploma! There's no thesis for a master's degree. It is just a degree awarded to someone who has fulfilled all the qualifications for a Ph.D. except the dissertation.

**VDT**: And you'd managed to do that within two years?

**COALE:** Yes, 1939 to 1941. That was fairly standard in those days, and I still think it's the right thing. I think that in two years you can do enough work in a field to master the basics of it. I sort of regret the four years of course work and two more years on a thesis common today. I don't think the students are coming out any better qualified after spending five or six years than they used to in two, plus one for the thesis.

**VDT:** You think three years beyond the undergraduate degree is enough for a Ph.D.?

**COALE:** The best students that I've trained, who got Ph.D.s under my supervision, have done it in three years from the B.A. People like Sam Preston.

Alvaro Lopez, from Colombia, had gotten his degree in some kind of engineering from the University of the Andes and then went to the Sorbonne and studied mathematics. While there, he got in some kind of exchange with Alfred Sauvy. When he was in Washington at the Colombian embassy, Frank Lorimer encountered him. They discussed his interest in population and Frank said he ought to apply to Princeton. He came here as a visiting student and decided to become a degree candidate in economics. He had never had a course in economics before in his life. He took his general examinations in mid-year of his second year. He never had a course in which he didn't get the top grade. Between the first of February and the end of May in his second year, he finished his dissertation, so he received his degree in two academic years. The dissertation was published by September, by the second anniversary of his arrival. He was the most brilliant student we've ever had.

**VDT:** His dissertation--remind me--was seminal in your work?

**COALE:** That dissertation [Problems in Stable Population Theory, 1961] was a proof of a conjecture that I'd made to the effect that all populations, not just stable populations, have an age composition that is independent of the remote past. It depends only on the fertility and mortality in the recent past, say, the past century or so. Two centuries are enough to wipe out the effects of the past. In explaining it to students, I say that the age distribution of France is no longer much affected by the Napoleonic wars. Lopez devised an extremely subtle proof and explored other implications in addition to proving the proposition in his dissertation. He did his degree in two years altogether because he was a genius.

Sam Preston spent two years before his exams and one more year for his thesis.

**VDT:** You could tell right away that he was also exceptional?

**COALE:** Exactly. I shouldn't say it, but of the 50 presidents of PAA [through 1987], 16 have been either students or staff at the OPR. Many of these students spent no more than three years on their Ph.D. More of them finished in three years than took more than that.

**VDT:** Of course, your own Ph.D. completion was interrupted by the war. In the two years from 1939 to 1941, you did not go beyond your generals.

**COALE:** The generals were in 1941, so the period between my completing the generals and going into the Navy was just between June of 1941 and the first of February, 1942. I had a low draft number and I knew I was going into the service one way or another. So I thought there was no point in trying to start a dissertation, because it was sure to be interrupted by my going into the service. So, instead of that, I worked as a research assistant at OPR.

**VDT:** You were working on projections of European populations?

**COALE:** Yes; in fact, I designed them. During that time, the Economic, Financial and Transit Section of the League of Nations left Geneva and came to Princeton; they were given headquarters at the Institute of Advanced Study. They negotiated an agreement with Frank Notestein that the Office would undertake studies of the population of Europe, trying to create a picture of what the populations of the different countries were likely to be at the end of the war, so when they had the next postwar treaties, they would at least have a pretty good idea of what populations they were working with.

The Office undertook several books. The first was The Future Population of Europe and the Soviet Union [by Notestein, Irene Taeuber, Dudley Kirk, Coale, and Louise Kiser, 1944]. Another was The Population of Europe in the Interwar Years [by Dudley Kirk, 1945] and the third was The Economic Demography of Eastern and Southern Europe [by Wilbert Moore, 1945]. The fourth was by Frank Lorimer, The Population of the Soviet Union [1946], who wrote it in Washington but for the Office.

The first book was organized around population projections through 1970 for every country of Europe. My contribution was to work out a general method for projecting each one of the European populations and to supervise the actual calculation of the projections. I did that part of the work and drafted part of a technical appendix describing the projections and had nothing to do with the rest of the book.

**VDT:** Those projections turned out to be somewhat wide of the mark, or low, because--as you pointed out in the paper ["A Reassessment of World Population Trends"] that you wrote recently for the United Nations Population Bulletin [No. 14, 1982]--you didn't anticipate the rapid fall in mortality, even in the developed countries, in Europe.

**COALE:** Right, indeed I did not. There was a discontinuity in mortality, and also there was the baby boom in Western Europe and the United States. However, the fertility and mortality that we projected for 1970 is not so far off the mark, because the fertility decline has resumed. So it sort of went back onto what we were projecting.

**VDT:** Then during the war you had a very interesting project. You taught electronics at MIT?

**COALE:** It was radar. What happened was that in 1941 there was a roster of scientific and specialized personnel that had been circulated to universities by the National Research Council. I didn't realize then that this was the operating arm of the National Academy of Sciences. As a graduate student, I'd filled this thing out and listed the substantial amount of statistics I'd taken as my technical background. After Pearl Harbor, I knew I was going to be called into the service right away. I went down to Washington to see the National Research Council, which, as I say, was the NAS. The man whose office I entered turned out to be a former professor of physics at Princeton. I said I thought I would be more useful in the war effort doing something that capitalized on my training rather than serving in the infantry. I explained that I had passed my preliminary exams for the Ph.D. in economics and had a lot of statistics. He said, "We don't know anything about the social sciences here; this is a natural sciences organization. Do you know anything about physics?" I said, "Yes, as a matter of fact I started out to major in physics and I continued to take courses in physics as electives." I told him the courses I'd had; I realized by then he'd been at Princeton so he knew what they meant. He then asked me if I knew anything about radio and I said, "When I was in high school I built several radios as a hobby." He said, "Do you think you could handle a course in radio engineering?" I said, "I'm sure I can." I'd had such courses as the functions of complex variables and other subjects that are directly pertinent. He picked up the phone and called across the street, from the National Academy of Sciences to the Navy Department, and said, "I've got a candidate here who ought to fit into the radar program." He sent me over to the Navy commander who was in charge of that program, who said they would give

me a commission as Navy ensign, as an officer specialized in electronics. I would go to Bowdoin to study radio engineering and then on to MIT to study radar. These were programs that the Navy was supporting.

I was sped up. Instead of going to Bowdoin on April 1st, I went to Harvard on March 1st for a pre-radar radio engineering program for three months. It was very intensive. In three months of six days a week of only one subject with labs and so on, you learn a lot--equal at least to a full academic year. Then on to MIT. MIT was training all of the Army and Navy officers who were going to be in charge of radar all over the world--maintenance, installation, operations. It was a combined Army-Navy school. In addition to the civilian staff, they took qualified people from the services to teach. I went through the three months of training and did well. They kept me on as an instructor and thus I ended up teaching radar.

**VDT:** You took the training course at Harvard and then switched to MIT?

**COALE:** It was a sequence. Harvard gave pre-radar radio engineering, without security clearance. They were just teaching the fundamentals of radio engineering. MIT was teaching the operation and maintenance of radar sets, which were just being designed. As I stayed there for three years, the composition of the course changed altogether, because new sets kept coming in. We had to master the sets from the instruction books and teach it to these officers who were responsible at Navy yards and on big ships and so on for the operation and maintenance of the radar sets for the whole fleet, including airborne radar. I actually wrote part of a book called Principles of Radar. I really did learn a lot about electronics.

In the last year of the war, I went to the Radiation Laboratory, which was also under MIT administration. It was the big civilian laboratory that worked on radar. It was full of people who won the Nobel prize--Rabi and others of that caliber. It was an extraordinary place. I was assigned to the Navy liaison office, providing liaison between the laboratory and the Navy. Thinking back on it, I was really an emissary from the Radiation Lab to the Navy and not the other way around. My sympathies were all with those scientists, not with the somewhat unimaginative people in the Navy Department.

At the end of the war, I had an offer from Bell Labs to become an electronics engineer there. I decided that having once switched from physics into economics, I wasn't going to go back. So I decided to complete my degree and go back into the social sciences.

**VDT:** How was it that you were appointed to this committee that led to your writing of the book, Problems of Reducing Vulnerability to Atomic Bombs, that became your dissertation?

**COALE:** What happened was that, in an extraordinarily generous program, the Social Science Research Council gave a limited number of what were called demobilization awards. I was actually a sort of trial candidate for this. The idea was to prevent the loss from the social sciences of people like myself who'd gone into the service. I had a wife and two children; I was a full lieutenant in the Navy, drawing pay about equal to an assistant professor. It would be very difficult for me to go back to grad school. So they offered me this demobilization award, which gave to a person who needed to write a dissertation one year of full support at the same stipend that he'd been earning in the services.

I said, "Splendid." I was still living in the Cambridge area and started going to the Harvard library to look around for a thesis topic. Then I got a call from the president of the SSRC, saying they were establishing a committee on the social implications of atomic energy and they'd like to have me be the secretary, on salary. It was going to last a year. I said I really wanted to get my thesis done. He said, "Come down and talk about it." I went down to New York and he said they would hold the demobilization award, that I would spend a year as secretary and then I could do the thesis. Professor Rabi was a member of this committee, a very distinguished group of both social and physical

scientists. The bomb had just gone off; I had known some of the people involved at Los Alamos and was very fascinated by the project. I had read the Smyth report with special interest when it came out; Professor Smyth had taught me freshman physics here at Princeton. So I agreed to be the professional secretary on this committee.

The committee commissioned several studies by distinguished people on such topics as the problems of establishing international agreements to control weapons and the economics of the production of electricity from nuclear fission. Then they said, "There's another problem. If these agreements don't work, other countries will develop arsenals of nuclear weapons and all the advanced countries will live in fear of being decimated by these terrible weapons. What's that going to do to society?" People were talking about having linear instead of circular cities--a city strung out from Pittsburgh to Philadelphia so an atomic bomb wouldn't do too much to it. The committee said they didn't know what they ought to do about it, so they said, "Okay, you're the secretary, you decide what kind of action the committee might take in this field." So I sat down to do an outline that kept expanding and it turned out to be a book. It was called The Problem of Reducing Vulnerability to Atomic Bombs. When I finished a draft, the committee liked it and recommended that it be published, which Princeton University Press agreed to do. Then the committee was dissolved.

I got the idea of saying to the economics department at Princeton that one was supposed to write a Ph.D. dissertation on a social science subject and one requirement was that it was supposed to be publishable. I asked, "How about one that's already published?" They generously agreed to accept it if I wrote an additional chapter with some more economics.

**VDT:** Did you then have to have it redone in proper dissertation style, retyped and on the special paper? I have a son who's just finished a dissertation and we talked about this acid-free, archivable paper.

**COALE:** The extra chapter was on regular typescript. I had an idea. I went to the university store and bought two copies of the book. I took a papercutter and cut the spines off. I had to have two copies because the pages were printed on both sides. Then I sat pasting these to the center of pages.

**VDT:** It was literally just one additional chapter!

**COALE:** One of my graduate student colleagues came into the seminar room where I was pasting pages and said, "Writing your thesis, I see."

**VDT:** You were back teaching in the economics department by that time?

**COALE:** No, my appointment to the faculty at Princeton as an assistant professor didn't come until after the thesis was finished and handed in. It started in September 1947.

**VDT:** When I read about this being your thesis topic, in your 1979 interview with Andy Lunde, it seemed far removed from your subsequent work. But you brought it up again in an article you did for Population and Development Review in 1985 ["Nuclear War and Demographers' Projections," PDR, September 1985]. You were reminding demographers that their projections always assume that there will be no nuclear holocaust, but actually the chance of nuclear holocaust is not zero. Why did you write that at that time, in 1985?

**COALE:** The reason I did it was that the address I gave to the IUSSP meeting in Manila in 1981, when I was outgoing president, was on world population trends. I mentioned a lot of ways of trying to decipher what the prospects were in fertility and mortality. In the closing section of that speech, I said



that having written this book, I was aware of the fact that there was something in the human scene that wasn't encompassed by assuming a steady progression in fertility and mortality. The weapons that had been developed and were stored in the arsenals of the world were sufficient to kill a quarter of the world's population if there were unremitting war. More than a billion people might be killed.

Then scientists had come up with the idea of nuclear winter. The climate of the world might be changed and radioactivity of an unknown degree might jeopardize the remaining people. That was a discontinuity that wasn't in our normal thinking. I thought some acknowledgement of this possibility should always be made. The probability of war had, in my mind, been greatly reduced by the existence of these weapons, so that, for example, at an interval very much longer than that between the two world wars we had not had a real confrontation between the two major powers. Many occasions might have provoked it in a different weapon situation, but, given the existence of these terrible weapons, there was a restraint shown that had prevented a confrontation, because both sides realized they might be ruined by it. So, the probability of a major war, I thought, was reduced. Not minor wars; there were plenty of them. The probability of a major war had not been reduced to zero.

One of the things you learn from statistics is that something that has a non-zero probability becomes inevitable if you wait long enough. Even a one percent chance that a war might break out in each year would make it inevitable within a couple of centuries. And I thought there was at least a one percent chance, given the possibility of a Colonel Quadafi, Hitler, or perhaps a Richard Nixon. There could be an irrational person in a responsible position who could trigger the catastrophe. Given that situation, I felt we had to acknowledge it--I didn't know what to do with it beyond that--the possibility that population projections would be totally out of line because of the occurrence of something of that sort. It grew out of the fact that I had continued to think about it, having written that book.

**VDT:** Are you glad that you're not going to live through the next two centuries?

**COALE:** Oh, certainly, I'm glad for that.

**VDT:** You explained in the Lunde interview that from 1947, when you were appointed to the economics department at Princeton, to 1954 when you rejoined OPR, you did "other things." What brought you back to OPR?

**COALE:** It was sort of economics again--cupidity. Not quite, it wasn't so directly financial. The situation was that as a result of having written this book, I was in demand for the kind of thinking that social scientists and economists do about military or quasi-military things. I spent a brief time as a consultant to the Pentagon. Then the Rand Corporation was started. It was originally a project within the Douglas Aircraft Company and was moved out and made independent. It was an organization that then, and still, works on social science questions relevant to the military. It was supported by the Air Force--very imaginatively. At their organizing meeting, they said that part of their concern was how you prevent a war as well as other factors of that sort. I was made a consultant because I'd written this book. I spent the summers of 1951 and 1953 out there.

In the meantime, 1948 to 1950, I spent two years at the Institute of Advanced Studies here at Princeton. The Social Science Research Council, jointly with the National Research Council, had offered a fellowship to allow social scientists to study the natural sciences, or vice versa. I applied, saying that I already had a fairly firm background in both areas and that I would like to study the economic implications of technological change, because I had professional training in both fields. That was a great mistake on my part. I tell my students not to do what I did. I was calculating that I had a comparative advantage, as we say in economics. There weren't many people who knew both technology and social sciences at a professional level. It was an opportunity for me to study the interaction. I was trying to capitalize on the circumstances, rather than having a brilliant idea that I

couldn't resist following up. I wrote many drafts, but it never led to much. Also, I had a diversion in the work for the Rand Corporation.

I taught some courses here in the economics department satisfactorily, but I had published only a very few things when my six years were up. As a rule, at the end of six years, if you're not promoted you're terminated. This is designed to prevent the exploitation of junior faculty, keeping a person on as assistant professor forever, paying a low salary for routine teaching. To prevent that possibility, you are either promoted or terminated. The economics department recommended that I be promoted to associate professor. The all-faculty committee, with the president and deans and so on on it, which makes the final decision, rejected that recommendation and said, "He has one more year"--a so-called terminal year.

That summer was my second at the Rand Corporation. I had a standing offer for a job at Rand at twice what I'd been making at the university. Charlie Hitch, who was then head of the economics division at Rand and subsequently president of the whole University of California system, offered me a job right then at Rand. I told him I was very attracted, but I was determined to go back and use my terminal year to return to the Office of Population Research doing demography, which I was pretty sure I could still do, to establish a firm professional base in academia. I had no intention of making a permanent career at Rand.

That's why I say that my return to OPR was kind of self-serving. I asked Frank Notestein if I could spend my terminal year at the Office. I did several good things during that year. I wrote an article estimating the undercount in the 1950 census by demographic analytical techniques ["The Population of the United States in 1950 Classified by Age, Sex, and Color--A Revision of the Census Figures," Journal of the American Statistical Association, 1955]. [From the 1979 interview]: When I did this work, it had an implication for the publications of the Division of Vital Statistics of the Census Bureau. It was this way. The vital statistics authorities had run a test of completeness of birth registration in 1940 and 1950 and had determined the extent of underregistration as well. By allowing for the degree of underregistration for different categories of births, they were able to estimate the changing deficit and give, for every year, a corrected number of births, from which they calculated the birth rates. What they realized from my work was that by making adjustment for 2 or 3 percent underregistration and then dividing through by a population figure based on the census, they were overstating the birth rate, because they were adjusting only the numerator and not the denominator of the rate. [Continuing with 1988 interview]: I developed an outline for a study of population growth and economic development in low-income countries. I had a couple of other things well on the way. So, Frank went to the other members of the economics department, where he was a professor, and proposed that they reopen the issue. This time my tenure appointment went through.

**VDT:** Thank goodness. You were saved for Princeton and demography.

**COALE:** When I came back to Princeton in 1947, I had said instead of going into demography, I was going to do other things. Notestein said there was a tendency for his students to do that. Demography isn't a department; you're a student in sociology or economics. It's like choosing labor economics instead of international trade or something; it's a field, not a department. What he said was that a lot of his good former students declared independence by entering one of the other social science fields instead of demography. I had done that.

The particular mistake that I warn my students against was trying to capitalize on certain skills I had, rather than being enthralled by an idea that I couldn't resist following up. The latter is the way to do good research. If you find something of that nature, never mind what field it's in, pursue it. Demography has features that suit my temperament and I just failed to understand that. I still regret it. I think those were seven years when I could have done a lot. That's the age when you're normally most productive.

**LUNDE** [from the 1979 interview]: After the war when you wound up again at Princeton, how did you find the Office of Population Research then?

**COALE**: When I first came back I had only a kind of social connection with it, because for the first seven years I turned to other fields to investigate. Of course, Frank Notestein had always been one of my best friends. I visited the Office. It had grown and Kingsley Davis and Wilbert Moore had joined the staff. They became the nucleus of the department of sociology. First there was a program within the Department of Economics and Social Institutions. During the 1950s, this program broke off and formed a department. The Office of Population Research had brought these very distinguished personalities to Princeton and they became the nucleus of the Department of Sociology.

Kingsley came to Princeton in 1946 or 47 [in 1942-44 as a visiting research associate and 1944-48 on the faculty]. It was at Princeton that he wrote his monumental book on India [The Population of India and Pakistan, 1951]. It's been reprinted and it's been a continued seller as a reference work on the population of India. It's quite different from the kind of work one would ordinarily associate with Kingsley. He's an original and insightful social theorist. This book does not lack insights, but it is conspicuously factual and historical--a monument indeed.

The Office of Population Research remained small, moved around into various rooms on the campus, and finally in the late 1950s, it moved into 5 Ivy Lane, where it remained for about 20 years. The building was an eating club that had gone bankrupt and the university had taken it over. It was the first time the Office had ample space under one roof. It just gradually grew, taking on more research projects and staff to carry them out.

[On his work after rejoining OPR, from the 1979 interview]: I think probably the most influential writing with which I was associated was the book that Ed Hoover and I wrote on Population Growth and Economic Development in Low-Income Countries [1958]. That's been nearly a bestseller and I think it had some influence.

That was what I started to work on when I went back to the population field in 1954. I worked on the study of the undercount in the U.S. census. Then the International Bank [World Bank] said they wanted a kind of pamphlet. Eugene Black was president of the Bank at that time and he was concerned about population. He wanted to go to an African country which asked for a loan and say, "If you want a loan, you ought to be concerned about your birth rate." He asked Frank Notestein to write a pamphlet he could hand out. Frank told him there wasn't enough known in this field to make an authoritative statement. So the Bank offered to fund a research project to look into it. Frank hired Ed Hoover to be the Washington economist and I was the Princeton demographer. We went to India and made a study of the Indian situation and then to Mexico. It was a kind of economic-demographic model. My personal regret is that I don't keep up with this area of research. I think we raised a lot of questions, but I haven't seen any new cogent formulations and they are overdue [but see below].

**VDT** [from the 1988 interview]: You certainly made up for lost time once you were back [in population]. You and Edgar Hoover produced the very important book, Population Growth and Economic Development in Low-Income Countries, for the World Bank, pointing out the great advantages there would be for India and also Mexico and similar countries if they reduced their fertility by 50 percent in 30 years. You went back to that in the paper you did for the American Academy session in 1986 ["Population Trends and Economic Development," in Jane Menken, ed., World Population and U.S. Policy: The Choices Ahead, 1986], which looked again at population growth and economic development in light of the revisionist, turnaround, stance of the U.S. delegation to the 1984 Mexico population conference. I'd had a question to ask you before I read that: Were you discouraged about how things turned out, because of course India's population growth was close to your high variant in the 1958 book?

**COALE:** National product under the high variant was also well predicted. We made an estimate using the parameters that Ed Hoover dug out of what was happening to economic growth in all fields--agricultural productivity and so on. He projected the growth of the GNP in India under the assumption of no change in fertility. Using the population projection with a 50 percent reduction in fertility, he made another economic projection. The first one, with no change in fertility, is within 2 or 3 percent of what has actually happened. So it's not true that we foresaw a catastrophe; people imply we're Malthusians or something. We foresaw that India was going to do quite well and just said that they would do still better if they reduced their fertility.

What I'm sorry about is that we used "population growth" in the title. That really isn't the theme of the book at all; growth is peripheral. For example, I realized in going back and looking at it that of the two countries we analyzed, India and Mexico, Mexico had much the more rapid rate of population growth and also a much more rapid rate of economic development. And we didn't see that as contradictory. We were simply saying that in those two countries, under very different conditions, both would profit from a substantial reduction in fertility as compared to not changing.

I went back and looked at Mexico 20 years later in a lecture I gave in Mexico in 1977 and said that Mexico had in fact just about doubled their per capita income during those 20 years. At the same time, their population had doubled, school enrollment had multiplied by three and a half times, life expectancy had risen, and so on. Didn't that contradict what Hoover and I had said? Not at all. What we had contended, and what I still thought was true in 1977, was that Mexico would have been still better off had they reduced the birth rate. In spite of having three-and-a-half times the number of kids in school, there were more kids not in school than there had been 20 years before, because the population grew so rapidly. If they'd brought the birth rate down, they could have had them all in school. So things had gone very well, but they could have gone still better. That was our theme. I still think it's correct--revisionism to the contrary.

**VDT:** I hope you'll keep harping on that theme. I think this is a very uncomfortable period for demography, this revisionist theory of population growth, the Julian Simon theme, the turnaround in the American long-term support of family planning programs.

**COALE:** Well, you have some guy like Ed Meese deciding what's going to happen! In this [Reagan] administration, we can expect to have ignorance and ideology predominate everything. Excuse me if I'm treading on your toes, but I really think that's the case.

Star Wars is an example. There isn't a reputable scientist--except Teller, who's sort of nutty--who thinks it has any possibility of working. And we're committed to it, pulling our best brains into this will-of-the-wisp. So I don't expect to get sensible population policy out of that crowd.

**VDT:** Well, let's hold our breath till [the elections] next November. As you said in your 1979 interview with Andy Lunde, you've always had two streams of interest in your career. One is abstract mathematical demography, stable populations, that began already with your work on the projections of European population in 1941. The other is in social problems, of which you've just given an example--your concern is not just about the recent lack of U.S. support of family planning programs in developing countries, but also backing of Star Wars. The mathematical interest obviously grew out of your first interest--as an undergraduate you started out to major in mathematics and physics--and that stream in demography began with your work on those projections.

**COALE:** That's true. I started out to major in math and physics and switched at the end of my sophomore year, when at Princeton you had to make the choice of what department you were going into. Your major doesn't start in the first two years. I chose economics rather than math and physics, but I did continue to have an interest in math and physics.

It's true--I say this to my students--that what I find appealing about demography is that there is a mathematical side to it that is not imposed on it as to some extent it is in economics. Mathematical economists talk about indifference curves, possibility curves, and tangents to these curves. Those are all constructions in their heads. You don't actually observe them, you construct them and use them to explain the way complicated situations develop. To a certain extent, it's a chess game. But in demography there are complicated and sophisticated and interesting mathematical relationships that arise fundamentally from the fact that we are a collection of objects which all get one year older every year. There are several effects that come out of that. The difference between demography and mathematics in, say, sociology or economics is that people really do get one year older--not approximately. Age is a continuous function. We can record the date of birth and the current date and determine age with precision. A person is either there or not; we count people. The same is true with births and deaths. So we're dealing with observables that can be given numerical reference with exactitude. And the relations among them lead to some complicated and interesting theorems. There's a genuine suitability of mathematics applied to demography, which is less true of other social sciences. At the same time, the implications of, for example, the changing age structure of a population, constitutes a very interesting social problem as well.

I left physics back when I was an undergraduate, because in the Depression I was concerned about economic problems and economics seemed more relevant to issues that I was aware of. In demography I could continue to do the kind of things I might have done in physics and math and still maintain a direct connection with social problems.

[On mathematical work, from the 1979 interview]: When I worked on population growth and economic development in India and Mexico, I became further interested in how the age composition of a population is determined. I used stable population analysis to estimate fertility and mortality and the true age distribution of India. At the same time I was working on the India book, I wrote a paper for the Milbank Memorial Fund Quarterly on stable population analysis. Later, at a conference on biology, I presented a piece called "How the Age Distribution of a Human Population is Determined." In it I had the insight that not only does the population become stable if fertility and mortality rates are fixed--which means the age distribution becomes independent of where it started--and is determined by the fixed vital rates, but by analogy, even if the vital rates are varying, as they always are, it's still true that the age distribution gets determined by the recent past of fertility and mortality. That got to be known as "weak ergodicity" and was proven correct by Alvaro Lopez.

I also like to work on social problems. The attractive feature of demography is that it addresses social problems, but also has a rigorous and subtle mathematical and analytical side, involving real numbers.

[Continuing from 1979 interview]: There was The Demography of Tropical Africa [1968]. When Frank Notestein left the Office to become president of the Population Council and I became director of the Office [1959], Frank wrote to me to say that Frank Lorimer wanted to start a project on Africa and the Carnegie foundation was interested in funding it. So I wrote a proposal and Frank Lorimer came to the Office and we assembled a staff including Bill Brass, Etienne van de Walle, Paul Demeny, Anatole Romaniuk, Karol Krotki, and Don Heisel. We had it going on for several years. That was typical of OPR work. It had international scope, but it also involved the development and application of a lot of analytical techniques for abstracting valid information from bad data. That in turn led to the work that Paul Demeny did for the United Nations on methods for extracting information from faulty data.

Parallel to that, I was working on model life tables. When on leave of absence in Rome, I carried with me a lot of material on model life tables. I had the idea for model life tables when making projections for the population of Europe, back in 1941. And the UN then came out with a full set of model life tables. But I thought they had certain weaknesses and I was trying to find another way to prepare the tables. I brought preliminary results back with me and played around and finally Demeny

and I brought Regional Model Life Tables and Stable Population to a publisher [1966].

The interest in census undercount that started in 1953 led me to suggest a thesis topic to Melvin Zelnik on trying to estimate the completeness of birth registration by projecting censuses backward and then projecting forward again and getting estimates of undercount. He and I produced a book on the history of fertility of the white population in the U.S. [New Estimates of Fertility and Population in the United States, 1963]. I followed that up a few years later with a parallel study of the black population, with William Rives ["A Statistical Reconstruction of the Black Population of the United States, 1880-1970," Population Index, 1973].

The thread of interest in analytical and mathematical demography that was always there culminated in two ways. One was a couple of articles involving a mathematical investigation of an extraordinary regularity in the age patterns of marriage. I wrote a paper for the 20th anniversary of Population Studies on "Age Patterns of Marriage" [1971]. Then, working with Don McNeil, a very able statistician, I found a very pleasing mathematical explanation of why there was a regularity in the age of marriage of Americans. With T. James Trussell, I worked out a combination of age at marriage and marital fertility to formulate a set of model fertility schedules.

During those years, I had different ideas about stable populations. In the paper I wrote in 1957, I had the idea that age distributions are independent of the remote past, whether stable or not. Alvaro Lopez picked that idea up and made it the subject of his Ph.D. thesis and proved the existence of "weak ergodicity." This range of ideas are incorporated in my book on The Growth and Structure of Human Populations [1972]. I have gone back and forth between the mathematical aspects of population and the social and economic implications of population structure, growth, and change.

**VDT** [1988 interview continued]: One of your works that nicely combined your interests in both the mathematical aspects and social problems aspects of population was your part in The Demography of Tropical Africa. There were so few data that you had to apply yours and Brass's methods.

**COALE**: Brass's methods. Brass is the real designer of indirect methods. Can I tell you a story? We had a bright student, an economics major, who wanted to come into demography and he hadn't taken the first semester course. I allowed him to take the second semester course without the first; he had a lot of mathematical background and seemed very bright. At our picnic that year--we always have a picnic about now, in May--as is not unusual, it rained. We had some kind of shelter and we all hastened for it. He said, "We should use the Brass method for keeping off raindrops." There's a Brass method for everything.

**VDT**: But you and Paul Demeny did your Regional Model Life Tables.

**COALE**: Sure, I've done some work in analytical demography.

**VDT**: I've heard a story that William Brass, when he first presented this method at a seminar in Africa, said, "Of course, these things are no substitute for good data," and eventually the aim should be that Africa get some good vital registration. But now, I understand, he rather defends them; obviously they are splendid methods. Have they held back the collection of good data?

**COALE**: I don't think so and I don't think Brass has changed his position on that. It's going to be decades before you can imagine having a full and accurate registration system in many of those African countries. You must deal with the fact that people don't know their ages; it's a very difficult situation. It takes a high degree of effective central control to have a registration system that works. So I don't think Brass would be in the camp of saying, don't bother with registration, you can use these techniques. There are many things identifying the individual circumstances--births, for example--that

don't come out of these retrospective data. I have just heard from a student of Brass--something he said that I'm going to quote very frequently. He said, "All data are guilty until proved innocent."

**VDT:** What do you regard as your leading work--the project that has given you the most satisfaction?

**COALE:** The individual piece of research that has given me more satisfaction than any other was devising an age schedule of entry into first marriage that seems to be almost universal--in developed countries, in developing countries, everywhere. It is universal in the same sense that the so-called normal distribution or the Gaussian distribution, the bell-shaped distribution, can be applied to the weight of newborn chicks or the test scores of army recruits. It's the universal curve that expresses what happens when the factors affecting the variate are random. The marriage curve is similar; it says what proportion of a given collection of women in a population get married at individual ages. Like the normal curve, it has a different mean and a different spread in different populations, and even a different area, because there's a different proportion that doesn't marry at all. But if you specify the mean and the standard deviation, the same curve will fit in all those different populations.

I just found out that it fits in the People's Republic of China, for individual cohorts of woman, even though the mean age at first marriage was changing under the rapidly changing social situation. Each individual cohort has a remarkable degree of fit. There must be some adjustment for a catastrophic year in which people don't get married; that affects them all. But then they go back on the curve.

What was satisfying about the standard curve of marriage was that we stumbled on this uniformity while looking for something simple to make it possible to estimate the distribution of European populations by age and marital status for our European project [Princeton's European Fertility Project]. Some 19th century censuses had a distribution of women by age and a distribution of women by marital status but no cross-tabulations. We needed to know what proportion were married at every age and I thought if I could find some kind of uniform curve of proportion ever-married by age, I could slide it along the age axes. I set out to look for it and it turned out to exist.

It was especially satisfying that when working with Don McNeil, through different forms of mathematical analysis, we stumbled on a behavioral explanation of why the curve was uniform. It's because there are several constituent steps in entering marriage. The first is becoming marriageable according to social norms--reaching the age of menarche, in many populations. Age of becoming marriageable has a normal distribution. Once marriageable, there must be a search for the ultimate spouse. Once the spouse is located, there is a period between locating the spouse and getting engaged, and then between getting engaged and marriage. All of those constituent stages have their own distributions. When you put them together, they form the universal curve.

What happens in Asia or Africa is that the whole process goes very fast, because the marriages are arranged. As soon as a girl reaches menarche, the family searches around to find her husband. After the husband is found, there's a contract written, equivalent to engagement, and then a period before the marriage. All of it happens very fast. In Sweden or Ireland or the United States, the bride and groom have to find each other congenial and suitable and the search takes a long time. Also becoming marriageable occurs later--the age at which steady dating is accepted as okay. The whole process is much slower. But the shape of the curve is just the same; it's just later and more spread out.

**VDT:** Marvelous! And you discovered this in the process of working on the European Fertility Project. For which you also devised your ingenious indexes of overall and marital fertility. I used those for a Georgetown term paper comparing historical trends in Quebec and Ontario fertility, which I'm still excited about. Then you and Trussell went on to devise the little m's and so on.

**COALE** [On the European Fertility Project, from the 1979 interview]: My mentor Frank Notestain

had made a very cogent statement of the theory of the demographic transition. I've always been interested in factors that contributed to major changes in fertility and mortality in the developed countries. There were a couple of students at Princeton who wrote doctoral dissertations synthesizing Notestein's work who found that the decline of fertility in England and Hungary began about the same time and proceeded at about the same pace. England was the leader in industrialization and had an almost fully literate population by 1880. Hungary was mostly agricultural, mostly illiterate, and still had high infant mortality. The two very different countries started their fertility decline at the same time. That had been overlooked because of the higher proportion married in Hungary. The crude birth rate was always higher than in England, so in any cross-sectional study, England and Hungary didn't look alike.

This anomaly led me to think the demographic transition theory had been based on a kind of impressionistic look at the statistics of Sweden. So, back in 1963, I thought that we ought to collect all the data on the fertility of individual provinces that had experienced the decline and try to find what the circumstances were.

I probably wouldn't have started the project had I known how large a job it would prove to be. The statistics for different countries have surprising gaps; the data need a lot of adjustment before you can compare them. Each country has particular peculiarities; some don't have rural-urban classifications, for example. And then the factors that influence fertility are not the same. One needs to look at the culture of each country and become a social historian. So, each person drawn into this project has ended up spending five or six years accumulating enormous files of computer output. But it may produce a better understanding, or at least an appreciation, of the complexity of historical demography.

**VDT** [back to 1988 interview]: That European Fertility Project has been praised to the skies by Richard Easterlin in an article in Sociological Forum ["Toward the Cumulation of Demographic Knowledge," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987]. He called it "the most important and successful demographic research undertaking of recent times." And the great thing about it was it didn't cost too much for the amount of output it had. The World Fertility Survey, he said, was also a great project but it cost a lot. You must be very satisfied with that.

**COALE**: Well, yes. It's different from other kinds of projects I've been involved in. It was an inordinately ambitious scheme. I wouldn't have started it if I'd known it was going to take 20 years. Each of the people who got involved and wrote a book on a European country found that they had to spend four or five years on it, because one is never satisfied; you have to know everything about a country before you can begin to write its fertility history. You start off with an idea and it leads to something else. It has taken an inordinate amount of work; it's been slow at getting to the stage of completion that it's reached.

Early in the project, I said at a conference of our staff that as a minimum we would document the decline of fertility in Europe and the circumstances under which it took place and as a maximum we would find out some kind of generalization about what had caused it. The minimum would probably be achievable. I remember saying it would probably be Kingsley Davis who would come out with the maximum.

**VDT**: He's always the one who wants the overall context.

**COALE**: We did not produce simple new generalizations. In terms of the demographic transition, I think the project has done more to qualify or destroy explanations people had already made than it did to create overall generalizations. Thus, it's not always true that infant mortality goes down before fertility; very often fertility goes down before infant mortality. There were a lot of ideas that had been



in the standard explanations of the demographic transition that didn't hold generally. There have been, I think, some quite good new ideas that came out of it. I think it means that from now on research on the decline in fertility will have to be reconciled with that body of information. Perhaps we will find out that it's different in different times and places. That may be the answer.

Actually, my personal disappointment is that my biggest individual contribution was to be the principal author of a study of the decline of fertility in Russia [Coale, Barbara Anderson, and Edith Harm, Human Fertility in Russia Since the Nineteenth Century, 1979]. Every now and then I look at that and I think that is as good as anything I've ever done. And it attracted very little attention.

**VDT:** I have to admit it must have, because I came across it in Easterlin's article and I did not know it. I knew from working on a Population Bulletin by Murray Feshbach on the Soviet Union that the Central Asians did not reduce their fertility as you would expect under the demographic transition theory. You're right; it should have had more attention.

**COALE:** I just went back to it. It really does illustrate the effect of cultural factors in fertility decline. It's a marvelous laboratory for doing that. The book just got lost; never got reviewed.

**VDT:** That's too bad. It's sort of common knowledge in the demographic world--and I'm sure you're the one that's brought it out--that those Central Asian republics are a peculiar anomaly; they will not bring down their TFR, despite modernization in all other respects.

**COALE:** There are other instances of the same thing, not as extreme as that. That's the kind of cultural context Ron Lesthaeghe found among the Belgians. The French-speaking areas reduced their fertility and Flemish-speaking areas did not. There were two villages ten miles apart with very similar socioeconomic conditions. The French-speaking village reduced its fertility a long time ago and nothing had happened in the Flemish-speaking one. In that particular case, as in the French-speaking and English-speaking populations in Canada, the laggards finally caught up, and then some. Fertility is now lower in the Flemish-speaking areas than in the French-speaking, just as the French-speaking population in Canada now has lower fertility than the English-speaking.

I wouldn't jump to the conclusion from that that they are going to have a big reduction in fertility in the Central Asian republics. I think that the strongly self-identifying Islamic cultural areas have attitudes toward women and children that are very slow to change, education or not. Like Irish Catholics.

**VDT:** That may retard the possibility of bringing down world population growth as rapidly as hoped. Let's talk about your connections with the IUSSP. It was reestablished after World War II with individual members. There were 29 Americans among the 147 demographers on the first list. When did you get elected to IUSSP?

**COALE:** I would guess in the late 1950s [1956].

**VDT:** Have you attended most of their meetings?

**COALE:** I must have been elected by 1957, because I went to a meeting in Stockholm that year; wrote a paper for it. It was every two years in those days and I went to Vienna in 1959, New York in 1961.

I'll tell you a funny story. In the fall of 1972, I had a leave of absence and was spending it in Florence. I've spent three leaves of absence in Italy. I did the work on model life tables in Rome; that was the first one. I went to Italy on leave of absence at the advice of a hyper-rational colleague in the

economics department. At Princeton, you don't get an automatic sabbatical leave every seven years. Instead, each department can have one seventh of its members on leave at any given time. You have to justify a leave; apply for it. If you haven't had one in a long time, the justification doesn't have to be too strong. I didn't realize all of this. I hadn't had a leave for many years and was thing of asking for one and talked to Will Baumol about it. I said I was thinking about going to INED or the LSE, where they had strong demography. He said, "Don't do that; they'll ask you to give lectures, supervise students. Get your research project to the point where all you need to do is to write the text and then go to Majorca." I followed his advice, but for me Majorca was Rome.

On a later leave in 1972, I was in Forence, my third leave in Italy because I love Italy. I was not studying the population of Italy but working on a continuation of my projects at Princeton. Massimo Livi-Bacci, who was then secretary-treasurer of the IUSSP, asked me if I'd be willing to be a candidate for vice-president of the IUSSP; the succession from vice-president to president is automatic. I said yes, under the belief that there was enough anti-Americanism in the membership that I wouldn't get elected. I went to the Liege meeting in 1973. I didn't attend the business meeting because of some sort of conflicting activity. Someone came out of the meeting and said, "Congratulations, you've been elected vice-president of the Union."

**VDT:** Elections were held on the spot? I thought they were by ballot.

**COALE:** They were on the spot then. The nominating committee came up with the nominees at the meeting and then the election was held on the spot. I didn't know that. I was already suffering from being the director of the OPR, with a lot of administrative responsibilities. I knew the vice-president spent four years going to the Council meetings and then four more years being president. I was very depressed. I went back to our hotel room and told my wife I was facing eight years of going constantly to meetings and doing administrative work and she said, "Maybe you won't live that long."

**VDT:** One of the aims of IUSSP has been to increase the proportion of people from less developed countries, but I think we need to have some Americans in there still. And you're right, there's anti-Americanism, as in almost any international organization. So, I'm glad you were there.

**COALE:** Actually, I enjoyed it. It was a lot of work, but I enjoyed it. I like dealing with people, I guess.

**VDT:** The IUSSP is important in providing many ways for demographers from the two parts of the world to get together, although here at OPR, you have so many demographers from less developed countries, many of whom go back and become well known. Perhaps that connection is enough.

**COALE:** Oh, no. I like to see them again at the IUSSP meetings. We have trained many LDC demographers who are good. I kept count of OPR alumni at a couple of IUSSP meetings. They listed the participants by country. The number present ran: U.S. members first, then Indian members and French members, in that order, as I remember. OPR alumni were more numerous than participants from any country except those three.

**VDT:** Whom do you consider your outstanding students, those in whose subsequent careers you've taken particular pleasure?

Ansley is showing me a whole section in his library that has the theses of his students. Here are Pravin Visaria, Barbara Anderson, Jeremiah Sullivan, Linda Martin, Jacki Forrest, Howard Goldberg, Mary Breckenridge, Jane Menken of course, Miroslav Macura, David Bloom, Neil Bennett, who raised such a flap, unwittingly, over the poor marriage market for educated women in the U.S., and so on.

That's a tremendous array. It seems like most of the younger people in U.S. demography and elsewhere have been your students.

**COALE:** There are others. Al Hermalin, Sam Preston and Alvaro Lopez whom I mentioned earlier, Pravin Visaria's wife Leela, and many others. I have a terrible memory for names and I hesitated to try to list them, because I would leave out some favorite who would then be offended. I think that's the most rewarding aspect of being an academic, having such an outstanding and congenial group of students.

**VDT:** Many of those students must of course have been attracted to Princeton, to demography, because of you. Did they usually deliberately ask to work with you or were they those who were more quantitatively oriented?

**COALE:** Actually, during a long period after Frank Notestein left [1959], I supervised almost all the theses. Less than half of the students, I think, come to Princeton to study demography. Sam Preston was an example of someone who did not; David Bloom was a more recent one. The long list of students we've had, especially of economists, have come here because we have an outstanding economics department and they've come to study economics. Those who plan to specialize in labor economics may hear that it is a good idea to study demography, because it's very pertinent to labor economics, and so they turn up to take the two one-semester standard courses in demography that have been given for many years, and some of them love it, decide to write a demographic thesis, and they become demographers.

When I've been boasting about the quality of these students and the fact that 16 of the presidents of PAA have been here one way or another, it's only fair to say that that isn't primarily due to the quality of the demographic training. It is the selection of the students. The mean score on the Graduate Record Exam of students we admit in economics is in the 95th percentile. Sam Preston isn't a great demographer because he studied with me. He's a great demographer because of his ability. And it's very, very nice to be teaching in a place that selects its graduate students so carefully.

I've left out people like Etienne van de Walle and Francine, and Susan Watkins, Doug Massey, Jim McCarthy, John Knodel, and Doug Ewbank. I recently went down and gave a lecture at Penn. A very funny thing happened. When I was introduced, they said I was the mentor of Sam Preston and Susan Watkins and the two van de Walles and others and one of the students came up to one of these people and said, "Does that make him our grand-mentor?"

**VDT:** It's a wonderful line of heredity that you've spawned, now into the second generation.

**COALE:** I left out Paul Demeny.

**VDT:** Who was a student of yours as well as a collaborator. That's a great attribute, that many of your students have gone on to work with you in projects as well. You'll be sorry to lose Jane Menken [leaving Princeton for the University of Pennsylvania at this time]. Tell me about Jane, as a student and colleague.

**COALE:** I did an unintentionally mean thing to Jane. I was asked to introduce three successive presidents of PAA [as they gave their addresses at annual meetings]: Sam [1984], Jane [1985], and Paul [1986]. As I was thinking what I was going to say about Jane, I remembered a funny feature of her career. She had gotten a master's in biostatistics at Harvard and had then been employed at NIH [and Columbia] before we recruited her to be a junior research staff member here at the Office. When she got here, she decided that she needed a Ph.D. We didn't have one in demography so she opted to

get a Ph.D. in sociology. The reason that she hadn't gotten a Ph.D. at Harvard was that she was so pathologically shy that she didn't want to follow a program that required her to take an oral exam. When she was here, she was asked to give an informal Friday afternoon seminar and she couldn't sleep before. So, when I was introducing her as president, I tell this story--and it brought back her nervousness! She could hardly talk when she started.

**VDT:** I don't remember that; I heard that address.

**COALE:** The nervousness was not apparent, but she told me afterward that she really had trouble getting started. Then her confidence reasserted itself as she went on. Jane has been a delight. She would have done well anywhere. She was a brilliant staff member and a first-rate student, of course. She had a readymade thesis as a result of her work with Mindel Sheps. She has been an ornament to the field. While she was still finishing her Ph.D., she spoke from the platform at a PAA meeting. I turned to the person next to me and said, "That's a typical graduate student at Princeton!"

I was asked by the IUSSP to be chairman of a committee on some aspect of fertility. IUSSP committees design a research program and hold seminars and so on. I said I was already swamped with administrative responsibilities. I knew from experience that an IUSSP committee chairman has a lot of work, involving a lot of correspondence. I said I would be willing to do it if they would appoint Jane as a paid secretary. That launched her on her international career. It was during the period when I was vice-president, I think. We had the first committee meeting in Florence. It was Jane's first trip abroad and she just had a ball. She flourished in this position, was really good at it, ran the thing extremely well, and then went on. It was part of her belated maturation, getting over her shyness, and having her administrative talents come to the fore.

**VDT:** And Paul Demeny came here as a student?

**COALE:** Yes. What happened was he left after the 1956 revolution in Hungary. He had been very disaffected by the government before that as well, but the situation was extremely uncomfortable after that uprising. He was a professional employee of the government and he went as a statistician to a meeting in Geneva and asked for asylum when he got there. [Not quite how it happened; see Demeny's interview] He was a stateless person in Geneva, which is very uncomfortable; the Swiss allow people asylum but they don't give refugees any kind of citizenship. Dudley Kirk was in Geneva. He was then head of the Demographic Division of the Population Council. He ran into Paul [was directed to him by Frank Lorimer] and suggested that he apply for a Population Council fellowship. Paul did and came to Princeton. He got his Ph.D. in economics and wrote a demographic dissertation. He worked on The Demography of Tropical Africa, model life tables, and Manual IV of the UN Population Division.

**VDT:** You've had a lot of outstanding students.

Ansley has just marked off on an OPR alumni list ["OPR Alumni List," OPR Newsletter, Spring 1987] all the students who have done their dissertation under him and they range, column after column, from Barbara Anderson, the first one on the list, to Hania Zlotnik--those two in themselves are well known. So indeed you can be called Father, Grandfather, Progenitor of the U.S. demographic scene. That must indeed be one of your great satisfactions.

**COALE:** It is.

**VDT:** I'd like to ask you to repeat a story you told in your 1979 interview about Frank Lorimer [PAA president 1934-39], who wanted you to come back when you were on your honeymoon, so you must

have known Frank quite well. Tell me about him. I regret I didn't get to New Zealand in time to interview him before his death.

**COALE:** Frank was a free spirit. Here is that story. Frank was working on The Population of the Soviet Union [1946], part of the OPR work on Europe for the League of Nations. I had worked out a way of correcting the understated mortality rates at older ages in Eastern Europe. The rates were clearly understated in Russia, as well. He wanted to adapt the system that I had used for the Balkan countries and apply it to Russia. I had corresponded with him about it. He asked me to come down to Washington and speak before his graduate seminar at American University on this subject. I wrote back saying I'd love to come but the time he mentioned was right after our scheduled honeymoon and I wasn't sure I would be ready to come. He sent me this telegram: "Please come and bring the new Coale to our castle."

Later on, Frank and his second wife had a problem about living in a hippie commune. Earlier he was traveling in Africa and, typical of Frank, he was staying in African hotels, deliberately staying away from European hotels. Staying at such a hotel was a nurse who had the same ideology, determined not to be identified as a European and to share the African life. They met and fell in love. This was Petra, many years his junior. Later they were back in the United States and Frank was always a rebel, so with Petra he moved into a commune in Connecticut, with a lot of hippie types. Frank had to leave, because the commune wouldn't tolerate dissent. If you did not follow their precise hippie line, they didn't want you there. Seventy-year-old Frank couldn't stand the hippies' intolerance! Actually, he was in his sixties then. His baby was born when he was 69, I remember that. Surpassed by Kingsley Davis, whose [last] baby was born when he was 79. I think it's lovely that this man in his sixties couldn't tolerate the intolerance of young radicals.

**VDT:** Another person I wanted to ask you about whom I regret we didn't get in this series of interviews is Robert Lapham [PAA secretary-treasurer 1984-87]. You must have known him when you were chairman of the National Academy of Sciences Committee on Population and Demography and he was study director.

**COALE:** I hired him for that committee. On a leave of absence, one of the few that I didn't spend in Italy, I was at the East-West Center in Honolulu. I got a phone call from the NAS, asking me if I'd be willing to be chairman of a committee on population and demography that they were founding. The formation of the committee was requested by AID to make estimates of fertility trends in major LDCs. AID was in trouble with Congress because Ravenholt was making claims about reductions in fertility that were not credible. The AID administration wanted independent estimates. I gave preliminary agreement to serve and went to Washington to plan the membership of the committee and the staff. Lapham was then at the Population Council. I knew him slightly. He was very highly recommended by Parker Mauldin, so I suggested that he was ideally suited to be head of the staff. He had the background and administrative experience. Ken Hill, a Brass product at LSE, was a technical staff member and Hania Zlotnik, a Princeton Ph.D., was another. I worked for five years with Bob Lapham as the senior employee of the committee. He was a remarkable man.

**VDT:** He did so much. He overlapped that job with the Demographic and Health Surveys and with being secretary-treasurer of the PAA. Amazing how much he got done, and he must have been ill too. [Robert Lapham died of cancer on February 20, 1988.]

I'd like to touch on your time with the UN Population Commission, as the third U.S. Representative [1961-67], following Phil Hauser and Kingsley Davis, and before General Draper. Does that time stand out in your memory?

**COALE:** Oh sure. It was a very interesting experience. Let me give you an idea of what was interesting and kind of frustrating about it. The Commission was like a legislative committee. That is, it was an organization that made recommendations about what the actions of the UN should be. Its report went to the Social and Economic Council; the Commission couldn't act itself. When I first went on it, there was a coalition against any kind of action, at least, having to do with human fertility--a coalition consisting of devout Catholic developed countries, the Soviet bloc, and, to a certain extent, other Catholic or Marxist countries from Latin America or wherever. The Russians were the most inflexible. When I was first there, the Russian delegate would object any time there was a mention of the word fertility in the Commission report. We'd have to think of some euphemistic way of saying the same thing. They would make speeches about how concern about lowering fertility in the Third World was capitalistic, imperialistic, and cannibalistic. Marx had denounced Malthus, hence they had to say the problem of poverty came from capitalism or colonialism and not from the high birth rate. It was a little wearing.

Then also there was a very inflexible Stalinist who was their representative, a fellow named Podyatchikh, who'd been the census commissioner. He was absolutely inflexible and also was obviously operating under instructions. I remember vividly that in the late 1960s there was an item on the agenda about the 1970 round of censuses. That was officially the business of the Commission on Statistics, rather than the Population Commission; nevertheless, we were asked for our recommendations. Various recommendations were made and then Podyatchikh got up and talked for half an hour about how the 1959 census in Russia had shown that everything good in Russia had quadrupled, they were the best country in the world, and so on. He went on and on. Members turned off their translators and read their newspapers. It was not very effective to give such a long propagandistic speech. I think he may have felt uncomfortable, but he was under instructions.

On the last day and a half of its meeting, the Commission devoted itself to accepting the report. A rapporteur took notes, drafted the report, and then read it. The members of the Commission reacted to it. They started off objecting to everything. Then on the last day they began to accept it all, because shopping time was coming to an end and they were going to leave. At the end, everyone said what a great job the translators, the rapporteur, and the chairman had done.

After we had adjourned, I met Bourgeois Pichat and I said, "Jean, this is the only report I know that has more authors than readers." And he said, "No, no, the same number." It's true. I never read the report when I wasn't on the Commission. It would be published, but no one read it.

It was an interesting opportunity for interchange with other people and it performed a certain useful function, that I tried my best to foster, of constructing the best agenda for the Population Division between the meetings of the Commission. The Division had to pay attention to it. We tried to include the best technical material and members like the UK representative, Bourgeois Pichat and a few others, who were themselves professional demographers, would be able to select items of the agenda to suggest something sensible. That made the Commission worthwhile. The long political discussions were interesting, but frustrating.

**VDT:** Do you think it regrettable that the U.S. does not now have professional demographers as representatives?

**COALE:** Well, I think it's regrettable, but the U.S. participated in the further politicization--if that's the word--of the Commission. Frank Notestein actually founded it. He was the first director of the Population Division and had a lot to do with forming the nature of the Commission. And the Commission was initially listed as a technical body that would give advice to the population activities in the UN. Very soon--the way things happen--the countries nominated their representatives. In fact, most of the smaller countries on the Commission just sent people from their New York staffs who didn't know anything about population. They spoke up to make sure that their individual Marxist or

Roman Catholic or whatever view their government represented was spoken for. The appointment of General Draper brought a political point of view to the U.S. representative. He was very effective in doing what he thought was right. He succeeded in pushing the Commission to advocate stronger birth control policies. He was a skilled political operator rather than a technical person. And that contributed further to the change in the nature of the Commission.

**VDT:** Do you think the Population Division is still the progenitor of the best international demographic statistics that are available? After all, they all have to accept what the countries report.

**COALE:** Well, sure. Actually, the Demographic Yearbook is produced by the Statistical Office rather than the Population Division, but a lot of the work is done in the Population Division. These periodic assessments of world trends are quite useful documents.

I'll give you an example of what I think is their greatest handicap. At a certain predictable time, they didn't publish any more statistics from Taiwan. When China was admitted to the UN and then to the Security Council, the UN took the official position that Taiwan was a province of China and they no longer published any data--perhaps the most valuable data from East Asia! I think it's too bad that the UN follows such a political line. They have to give priority to the political reality.

I think my relatively warm impression as a member of the Commission was a result of the fact that I wasn't speaking for the U.S.; I was speaking for the field. In fact, when I was asked to be the U.S. representative, I said I would feel very uncomfortable getting up and making speeches about the U.S. position on population. I said I knew Frank Notestein had been instrumental in establishing the Commission and had wanted it to be a technical body. The Under-Secretary of State who was negotiating with me to accept the position said, "I want to make it clear to you that you are appointed by the U.S. but you will speak as an individual professional demographer. Should something come up, such as the acceptance of Mainland China on the Commission, where the U.S. has an official position that comes from the Secretary of State, you will have to speak and you are authorized to say, 'I am instructed to say.' Anytime you feel uncomfortable--and it will almost never happen--but if something that reflects an important U.S. policy position comes up, you are entitled to present it with that introduction--'I am instructed to say'--that is, you will separate yourself from the political aspect of it." I felt very pleased about that. I never had to say it. There was never any pressure put on me.

**VDT:** Now on PAA. You and Andy Lunde talked in your 1979 interview about the flap that occurred when you were president-elect [1966-67] over the recommendations of the Forrest Linder Committee on Organizational Management [including a paid business manager, "positive membership recruitment activities," the establishment of "professional standards for membership qualifications" including two classes of members, and a small-grants research program, from PAA history vignette on this committee by Paul Glick, PAA Affairs, Summer 1982]. You agreed that a business arrangement should be made, but objected to other recommendations.

**COALE** [from the 1979 interview]: Up until that time [1966-67], the secretary-treasurer of the Association maintained all the business and correspondence of the Association, but he did that through his employer. Paul Glick [secretary-treasurer 1962-65] worked at the Census Bureau. A substantial amount of his secretary's time was devoted to PAA affairs. When his term was completed, I think it was he who said no government office or university could be asked to devote that much resources to the PAA. We decided that we'd have to find someone who would do this and get paid for it. We would have to increase the dues to make it possible. That decision was made and I believe you [Anders Lunde, secretary-treasurer 1965-68] were asked--or volunteered--to see if we could find an arrangement. That's how it all came about [PAA business affairs were taken over by Ed Bisgyer and his staff at the American Statistical Association; see Lunde interview above]. I think it's been a

marvelous thing.

[On other recommendations of the Linder committee]: When I was president-elect, there was a proposal circulated that the Association should undertake a number of additional functions and that the budget should be increased by a large amount and a manager be hired. For example, the Association should solicit funds that would be used for grants for population research.

It was my feeling at the time that the function of the Association was to provide a place for interchange of ideas, to make it possible to conduct meetings, and to sponsor publications. These other diverse activities are not effectively carried out by an association such as the PAA. To administer funds would require a big staff. Since the officers of the Association are elected and change all the time, it is not an organization that could allocate research grants as NIH and private foundations can.

When the proposal came out, when I was president-elect, I wrote Paul [then president, 1966-67] stating my reasons for opposing it. At the business meeting that year [during 1967 annual meeting in Cincinnati], there was a discussion of the proposal and it was voted down. I still feel it was the right decision.

**VDT** [continuing 1988 interview]: The Linder committee proposed two classes of membership and broadening the membership greatly and you felt it could get a bit too broad. Do you think that demography and the PAA should really stick with those whose real work and interest is demography? What do you think about the applied demographers, for example?

**COALE**: I said at the time, and stated in my Lunde interview, that I was not in favor of restricting the membership. That wasn't my point. What I was in favor of was restricting the activities of the PAA to the development of the professional field. Applied demography is certainly part of that. What I was opposed to was taking resolutions in favor of zero population growth or pro- or anti-abortion or on any other issue of that sort. I thought the resolutions that the Association should take should be in favor of some aspect of census-taking or having accurate vital statistics or something else that was a professional, technical matter, which the PAA, as a professional society, could take a stand on--not political issues. I still feel that was correct. I think the Association would be weakened by taking professional stands.

I didn't like the idea of two classes of members. I wanted everybody interested in population in the PAA, with no restrictions. But just never let the society get into being an advocate of a particular political point of view.

**VDT**: Can you remember your first PAA meeting? It must have been at Princeton.

**COALE**: Not really; I remember its happening.

**VDT**: Here's a list of all the meetings and where they were held.

**COALE**: The PAA met in Princeton in 1936, 1938 and in 1941. I certainly would have been at the 1941 meeting; that was before I passed my generals. There wasn't any meeting in 1937, so two consecutive meetings were in Princeton, 1936 and 1938, and then another one three years later in 1941. And then again in 1946 and 1947, 1949, 1950, and then in 1952 and 1955. Note the registrations at the early meetings: 20 [1934], 38 [organization meeting, 1931], 17 [1933], 67 [1932].

They used to meet here in Princeton and the members would be put up in the Princeton Inn. It was a small group of people who all knew each other quite well. That was my memory of the early meetings. I'm not sure I was present at the one in 1941. I remember the flavor of it, but not any more than that.



**VDT:** Do you regret the relatively enormous growth of the meetings? At the one we've just been to in New Orleans [April 21-23, 1988], there were over 1,100 people [1,115], 87 sessions, seven and eight overlapping at a time.

**COALE:** I think that's inevitable. The PAA meeting is not as big as the Economic Association meeting. Too many sessions can be frustrating, because you can't be at more than one at a time. In the interview I had with Andy, I explained that I tried to increase the size of the program when I was first vice-president [1964-65; responsible for program of 1965 meeting in Chicago]. The first vice-president in those days had the responsibility for the program. I heard complaints--not said to me directly--about an old boys' network. The program always consisted of well-known people and the youngsters didn't have a chance. I thought to an extent that was true and unfortunate.

So I instituted a reform. I sent out a notice to all members that any member was entitled to give a paper. He or she could send in something like 20 copies of a paper to the program chairman--that was me. I took the responsibility of organizing sessions out of the different papers and finding chairmen for each session. We got a dozen or more sessions for those submitted papers. I felt that the average quality was just as good as for the invited papers.

The system did not last. Within the next year or two as the program chairman tried to follow this scheme, hundreds of papers came in. Members would get a free trip to the meeting from their universities if they were giving a paper. Self-selection of authors did not work. The idea of giving everyone a chance is good, I think, as long as you don't compromise the quality. Part of the function of the meeting is to give everyone a chance to get an audience for whatever idea they're working on. With a large membership that means a lot of sessions.

**VDT:** That's very important indeed, even though there might be just two or three left to hear you on Saturday afternoon at the last session. You can put it on your resume that you gave that paper. Actually, demography has remained quite a small discipline. You mentioned the Economics Association meetings, which I know are huge. The membership of the PAA is just around 2,600 [2,679 at the end of 1989] and has been for years; we seem to lose some and gain some. That's pretty small as professional organizations go.

Your presidential address [in 1968] was on "Should the United States Start a Campaign for Fewer Births?" [Population Index, October/December, 1968]. That sort of ties in with what you say about not wanting PAA to become an advocacy organization. Of course, it was at the height of the Paul Ehrlich "population bomb" scare of the late 1960s. You concluded in that speech that there was no need of a policy to bring down fertility in the U.S. It would come down of its own accord.

**COALE:** It was in the middle of doing so. I said then and feel now that was like the generals who always want to fight the last war. There was alarm about a high birth rate when it was on its way down. It was really almost down to replacement at the time. I'm not one who says that giving people freedom of choice necessarily leads to an optimum outcome. I would not be surprised if in the next generation we will come to feel the need for policies aimed at raising the birth rate.

**VDT:** What would you suggest could be such a policy?

**COALE:** Well, in general the kind of policy that appeals to me, as a staunch libertarian, is having maximum freedom of choice. I would like to have everyone informed about the possibilities of controlling fertility. The means of effective control should be made available to those who might not otherwise have them. Then policies could be aimed at influencing decisions at the margin. Gunnar Myrdal in the 1930s said it was wrong to give people a cash bonus to raise the birth rate. That might induce the wrong people to have more children. It was better to try to offset the disadvantage that

people see in having more children. A policy that I liked was making university education free, with admission by competitive exam. People need not refuse to have children because they feel they won't be able to educate them. The aim is to affect decisions at the margin, to reduce the marginal cost of having an additional child. Then let those who want to have four or five children do so. Fine. With several siblings, children grow up with a minimum of rivalry. The people who love children are the ones that have them. I don't like the idea of "Stop at two" or any other uniform rate. When the birth rate's too high, one should try to raise the marginal cost of a child. When fertility's too low, you try to lower the marginal cost.

**VDT:** Now you're saying something might be done about it, but you didn't . . .

**COALE:** I said so then. At the end of my address, I said I would like to see fertility influenced in the tradition of the Chicago economists--making childbearing a free choice, but trying to influence it at the margin, not trying to propagandize.

**VDT:** Do you think there will have to be a stronger push for reducing rapid population growth in developing countries? The latest Population Reference Bureau World Population Data Sheet and the press release that went out with it show that world population growth seems to be stalled at 1.8 percent and the trend is getting closer to the UN high variant than it is to the medium variant.

**COALE:** I think that it is advantageous for countries that have high fertility to reduce it. I do not concur with the Chinese one-child policy, including aborting women who get pregnant with a second child or sterilizing women after a second delivery without a free choice. A very effective way of controlling fertility, but I don't like it. I don't like interference with individual freedom. The situation in China is not nearly as desperate as it is represented. The total fertility rate is down to less than 2.5. That's splendid. Let's use moderate policies; give people as much choice as you can; try, as I say, to influence at the margin. More work points for people with fewer children is the kind of thing I think is not too bad. I do not want to see coercive measures introduced into Bangladesh or anywhere else. I remember Notestein saying when coercion was being proposed in India, "Coercive measures to force contraceptive practice are more likely to bring down the government than the birth rate." That's what happened in India. I'm not especially optimistic about what is going to happen. I view high birth rates in poor countries as one of the many problems in the world. Agricultural productivity is not going up as fast as it might with better organization and agricultural policies. There are irrational policies of subsidizing low agricultural prices for the benefit of people in the cities who are the ones that have political clout. That ruins the farmers' profit and keeps agricultural output from going up.

Those things need to be changed. How? I don't know. Basic social changes are needed in many countries before the birth rate will come down. Nothing will happen easily. Bangladesh will double its population in a short time. It will be very impoverished. Population growth is not going to lead to a famine or the world coming to an end, but it certainly will make a lot of problems worse.

When I see a situation that's bad, I'm always interested in trying to make it better. There's a side of me that says lots of situations aren't going to get much better. We do our best, but we have to avoid getting into extremist positions, like the book Famine 1975. Most famines since World War II are the result of bad policy, not a growing population. Fewer people die now as the result of malnutrition than used to. In fact, per capita output of food has risen.

**VDT:** Not in Africa.

**COALE:** Not in Africa, but that's not because of population growth primarily. It's because they have the most incompetent agricultural policies in the world. And it's much more important to reform those

policies than it is to work on the population growth. Not that population doesn't need attention. Population growth is not the source of the problem; it is the incredibly inept agricultural policy. And they need to work on it.

I agree with the National Academy report [Population Growth and Economic Development: Policy Questions, 1986] on that part of it. Where I disagree with the report is the view that population isn't important. It is. It's not the primary cause of the things that people like Ehrlich blame it for. I've been advocating this since 1955. But Hoover and I did not say that there was going to be a disaster. In fact, our projections for the economies of Mexico and India with no decline in fertility implied a substantial improvement, as indeed did happen. It just would have been a lot better--and still would be--if they would reduce fertility. And if the United States had not gone to Mexico City [International Population Conference 1984] and said that population doesn't matter.

**VDT:** What are you doing now?

**COALE:** Right now I'm finishing up a paper for a conference on mortality in Asia that's going to be held in Beijing in August. I've written a paper showing that even in China, where ages are reported with extreme precision, at the very oldest ages their mortality rates are not reliable, because of a minority--less than 1 percent of the population--that misstates its ages terribly. Xinjiang province in the west has 50 percent Islamic minorities. It has 1.3 percent of the population of China, but 47 percent of the males aged 95 to 99. Of course, that's not true; the true proportion is no more than about 1 percent. Half of the mortality rate for China at 95-99 is based on the terribly understated mortality rate for these non-existent people. Even in China, the death rate at the very highest ages is contaminated by misstatement of age. If you leave that province out, the death rate at 95-99 is acceptable.

**VDT:** Carrying on from a paper of yours I reread while preparing for this interview about the apparent crossover of mortality rates at the oldest ages [rates for blacks become lower than for whites] having much to do with this misstatement of age.

**COALE:** That's right. Even in China, where for the most part the ages are incredibly precise, there are minorities that don't share that precision in reporting age and it contaminates the rates.

**VDT:** You're doing quite a bit of work on China after your excellent piece on the 1982 retrospective fertility survey.

**COALE:** I've published a thick volume at the East-West Center on fertility rates by age of women and by duration of marriage for all 28 provinces [Coale and Chen Sheng Li, Basic Data on Fertility in the Provinces of China, 1940-82, Papers of the East-West Population Institute, No. 104, East-West Center, 1987]. It's based on the survey. It goes back historically. Also based on the survey, I have analyzed the distribution of intervals between births.

I am scheduled to go to London and give a paper at Brass's retirement in late June, a paper on the use of models in demography. Then what I want to do next year is write a book with Sam Preston on the so-called "variable  $r$ " analysis, an extension of stable population analysis to any population. The rate of increase of the population is a function of age, instead of being constant as it is in the stable population. Using the variable rate of increase, one can generalize the stable population relations between fertility and mortality and the age distribution to any situation. Variable  $r$  extends the understanding of basic relations in demography and their use in estimation. Sam and I had an incomprehensible, too dense, article in Population Index four or five years ago setting this idea forth, with too many illustrations ["Age Structure, Growth Rates, Attrition, and Accretion: A New

Synthesis," Population Index, Summer 1982]. It was just too constipated to read. We've had our minds set on writing a book. We've each drafted some chapters. Both of us have had to set it aside. I've told Sam that next year I'm going to do that.

**VDT:** Well, you continue to be a font of ideas. And now you're going to go play your tennis that gives you extra energy. Are these pictures of your grandchildren?

**COALE:** Yes, those are grandchildren.

**VDT:** How many children do you have?

**COALE:** I have just two boys. The younger has two daughters and those are them. Our older boy doesn't have any children.

[In response to Lunde's question in the 1979 interview, "Where did you first meet your wife?"]: I was a senior at Princeton. When I was an undergraduate, my family was poor; my father was a Presbyterian minister. I took the train to Princeton to enter my freshman year and never took the train again. I hitchhiked about 45,000 miles. I used to hitchhike home; I lived in Annapolis. I went through Baltimore all the time. One of my best friends in college had a first cousin who went to Goucher. He said I should meet her. So when I was hitchhiking through Baltimore, I looked her up. Her name was Campbell. It was the best stopover of my life.

[Back to 1988 interview]: That's a joke [photo of a painting of a sailing ship]. There are terrible jokes in our family about the old song "Red Sails in the Sunset." For example: Why did the newly rich Indian oil millionaire want to join the local yacht club? Because he'd always wanted to see his red sons in the sail set. When we were sitting looking out over the sea in Honolulu, I would say, "Sue, look at what is over there." And she would ask, "What?" And I say, "It's jet trails in the sunset." She bought me that picture as a memento of these terrible jokes.

**VDT:** I heard a bit about that from Charlie Westoff yesterday. He said you're full of bad jokes. But they're cute jokes. Are you ever going to write your own autobiography?

**COALE:** I don't have any plans to do that. It might be fun. Who knows? [He did, in fact, write an autobiography: **Ansley J. Coale: *An Autobiography*** (Philadelphia: American Philosophical Society), 2000.]

**VDT:** You could talk into a tape recorder.

**COALE:** Well, I'll tell you what I'm going to do this summer, in addition to starting on this book with Sam. I'm going to learn word processing.

**VDT:** Oh, you're just now learning it. I'm impressed--Word Perfect [Coale showing book].

**COALE:** I haven't yet done a thing with it. I just got that book out. I've gotten fairly adept at APL and I do my own programming, but I haven't learned any word processing. I realize that although the Office is very nice about it, it really is not fully correct for me to continue to get the same level of secretarial service I got before I retired.

**VDT:** Senior corporate executives are supposed to be able to use their own computers now.

**COALE:** That's what I want to do. I want to learn to type. I can't type at all.

**VDT:** You never have? You've done everything by longhand?

**COALE:** And dictating. And all of my colleagues who've gotten into Word Perfect say that it is really so much more efficient than writing by hand, because you can edit it so easily.

**VDT:** Once you've learned word processing, what are you going to do with it?

**COALE:** Do all my own correspondence and my own manuscripts. I have a terrible story and probably shouldn't put it on tape. It is the Ron and Nancy typewriter, which has no colon, no period, and no memory.

**VDT:** I think we'd better stop at this point!

**Additional note on Ansley's year ahead, in 1988.**

**COALE:** After going to LSE for Bill Brass's retirement in June, my wife has persuaded me to go on a theater trip organized by the local university theater, where an expert in London lines up a bunch of plays for us to go to. I did that once several years ago. Then in August, I go to Beijing for the IUSSP meeting on mortality in Asia. And then I just got a phone call from Massimo Livi-Bacci a couple of days ago, asking me to attend a conference in Firenze in December. I said, "Massimo, I can't write another paper." And he said, "No, no, we just want you to be chairman, get your expenses paid, and you come and chair."

**VDT:** What explains your particular love of Italy?

**COALE:** I stopped there my first trip abroad, on our way to India in 1955. My brother had been in Europe for several years and I was asking him where I should stop. He said, "Don't try to go to London, Paris, Rome, Athens, and so on. If you have only a week, spend it in one place. I would recommend Italy." We did as he suggested. We had to stay overnight in Paris and then went to Rome and spent four days in Rome and four days in Florence and just loved it. So when I got this advice about when you take a leave, get everything ready and go to Majorca, I said Rome was my idea of Majorca. I took my research with me and my wife and I studied Italian the year before we went. So I went back there.

**VDT:** Lovely. Great country.

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*Ansley / Coale*

## ANSLEY J. COALE

November 14, 1917–November 5, 2002

BY THOMAS J. ESPENSHADE, JAMES TRUSSELL, AND CHARLES F. WESTOFF

**ANSLEY JOHNSON COALE**, William Church Osborne Professor of Public Affairs and professor of economics emeritus at Princeton University, died on November 5, 2002, at Pennswood retirement village in Newtown, Pennsylvania, at the age of 85. The cause was heart failure following several years with Parkinson's disease.

Coale was born in Baltimore, Maryland, on November 14, 1917. He attended public high school in Annapolis, graduating in 1934 at the age of 16. Since his College Entrance Board scores in Latin were not acceptable for admission to Princeton University (he scored only 28 percent in Virgil), he spent one year at Mercersburg Academy to correct that deficiency on a scholarship for boys from low-income families, and he matriculated at Princeton in the fall of 1935.

Coale was educated entirely at Princeton University (B.A. in 1939, M.A. in 1941, and Ph.D. in 1947) and spent his whole academic career at its Office of Population Research, serving as assistant director from 1954 to 1959, as director from 1959 to 1975, and as associate director from 1975 to 1986. He was appointed assistant professor of economics in 1947, promoted to associate professor of economics in 1954, promoted to professor of economics in 1959, and named



William Church Osborne Professor of Public Affairs in 1964. He retired from the faculty in 1986 to become senior research demographer at the Office of Population Research, a position he held until 2000. During his many years on the Princeton campus, Ansley was a familiar figure on his bicycle and on the tennis and squash courts. In June 2002 Princeton University honored Coale by naming its demographic research library the Ansley J. Coale Population Research Collection.

He served as president of the Population Association of America in 1967–1968 and as president of the International Union for the Scientific Study of Population from 1977 to 1981. He was a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society, and he was a recipient of honorary degrees from the University of Louvain in 1979, the University of Liège in 1983, the University of Pennsylvania in 1983, and Princeton University in 1994. He was also a corresponding fellow of the British Academy. He received both the Mindel Sheps Prize in Mathematical Demography and the Irene Taueber Prize, the most prestigious prizes awarded by the Population Association of America. He was appointed by President Kennedy as the United States representative to the United Nations Population Commission and served in that post from 1961 to 1967.

He was very prolific, publishing more than 125 books and articles on a wide variety of demographic topics. He also trained and served as mentor to many students who became leaders in the field. Indeed, he was the principal advisor on more than 35 doctoral dissertations and more than 90 research papers by visiting graduate students who earned the certificate in demography offered by the Office of Population Research.

His first major influential work was *Population Growth and Economic Development in Low-Income Countries* (1958),

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coauthored with Edgar Hoover; the results, which showed that slowing population growth could enhance economic development, had a major impact on public policy and set the research agenda in this field. This was followed by *Regional Model Life Tables and Stable Populations* (1966), coauthored with Paul Demeny. These model life tables both established new empirical regularities and proved invaluable in the development of later techniques for estimating mortality and fertility in populations with inaccurate or incomplete data. Along with William Brass, Coale pioneered the development and use of these techniques, first explicated in *Methods of Estimating Basic Demographic Measures from Incomplete Data* (1967) and in *The Demography of Tropical Africa* (1968).

Coale was an able mathematician (he taught radar at the Massachusetts Institute of Technology during World War II as a radar officer in the U.S. Naval Reserve), and his *Growth and Structure of Human Populations* (1972) is an essential textbook for those interested in formal demography. The publication of this book is all the more remarkable since the original source materials (notes, hand-drawn figures, tables), carefully collected over the course of many years, were accidentally discarded by a new custodian who did not recognize their significance; everything had to be reconstructed from scratch. Toward the end of his career Coale became interested in the population changes in China and in understanding the fertility transition there as well as factors affecting the sex ratio at birth.

All three of the writers of this memoir have vivid memories of Ansley as a person. Ansley was the primary mentor and thesis advisor to two of us (T.J.E. and J.T.). We reflect, in turn, on those memories. One of us (T.J.E.), having attended the College of Wooster, a small liberal arts college in Ohio affiliated with the Presbyterian Church, had heard of Ansley



Coale some years before meeting him. Ansley's older son, Pete, was a classmate of mine at Wooster. Wooster College was also the alma mater of Frank Notestein, the first director of the Office of Population Research at Princeton. With these connections, I should have known I was predestined to go into demography.

But as an undergraduate I actually had little idea what demography was. I was interested in mathematics but ended up majoring in economics because several college classmates and I were determined to spend our junior year abroad, and the chair of Wooster's mathematics department convinced me that the European way of sequencing courses in mathematics was so different from the American system that I had to choose between going abroad and staying home to major in mathematics. The London School of Economics won out. Following college I enrolled in a one-year Master of Arts in Teaching program at Yale expecting to become a high school math teacher. But the Vietnam War intervened, and the draft made staying in graduate school an attractive alternative. I singled out Ph.D. programs with a concentration in mathematical economics and arrived at Princeton in the fall of 1966 still not knowing anything about demography. In fact, one of the ironies of my professional career is that I turned down a demography fellowship for graduate study at Michigan for fear that I would be committed to studying a subject that I might actually dislike.

John Isbister introduced me to Ansley during my first semester at Princeton. I wanted to meet him because I had known his son, but I was not in any of Ansley's courses. Two of my graduate school classmates (Kevin Young and Yukon Huang) tried to convince me to study demography, but the whole subject sounded rather uninteresting. During the summer following my first year in graduate school, I was a research assistant in the Department of Economics at

the University of Southampton in England, assigned to work on the dwellings (or household) sector of the U.K. econometric model. I began thinking that there might be a dissertation topic here and decided, therefore, to take the demography sequence in the next academic year.

All it took was a couple of weeks in Ansley's first semester course (Survey of Population Problems) to convince me the subject matter was fascinating. What appealed to me most was Ansley's infectious enthusiasm for the subject, especially his treatment of stable population theory (his disinterest in migration or immigration is another story). The subject involved just the right amount of math, and soon the equations for the stable age distribution, birth rate, and intrinsic growth rate became as familiar as the back of my hand. Ansley also guided me through my dissertation (actually through two false starts and then a dissertation that was completed while I was doing a two-year postdoc at Berkeley under the auspices of Kingsley Davis). I'm still grateful to Ansley not only for shepherding my work long-distance but also for prodding me to consider how estimates of parental expenditures on children might be affected by alternative specifications of the underlying regression models.

Ansley had a strong competitive streak. I experienced this directly during a student-faculty squash tournament in graduate school. I was so nervous playing Ansley in the second round that I lost track of where we were in the game. After one serve Ansley pronounced, "That serve was out." And when I went to serve again, he said, "And that was your second serve!"

Ansley enjoyed a good joke. When he began teaching a radar class at Harvard during World War II, he said, "I'm sure there are many people who know more about radar than I do. But seeing none of them present..." And he was fond of saying that so and so was "sui generis to a fault."



There were things that could make Ansley irritable. Just the mention of Ronald Reagan (our “acting” President) would set him off. So, too, could people who used improper grammar. He would always correct someone who began a sentence, “Hopefully, it will ...” And his frustration boiled over when he once had trouble figuring out the tip at his favorite Italian restaurant and remarked, “I’m usually infallible in such matters.” Ansley kept a weight chart in his office and was proud of the fact that it seldom deviated over many years by more than a pound from a perfectly flat trend line. Being able to wear sport coats that he owned in college was another source of satisfaction. Ansley’s sometimes puritanical streak extended to turning out lights at the office before he went home—a habit that was cut short after someone let out a scream when he turned off the lights in the ladies room.

The things for which I will remember Ansley are his respect for data quality, his attention to detail in his research, and rum and tonic drinks (with Bacardi’s light) and a twist of lime.

The memories of another of us (J.T.) start before coming to Princeton University. After college I went to Oxford University for two years of graduate study in economics. I had no idea what I wanted to do next. In the fall of my second year Professor William Branson visited for a couple of days from Princeton. After I told him what I was interested in, he said that the economics department at Princeton would be the best place for me to finish a Ph.D., because I could study demography with Professor Ansley Coale. So I applied to Princeton and later received a letter from Ansley Coale offering me (he said) a magnificent fellowship with a \$3,000 stipend if I wanted to study demography at the Office of Population Research.

I had never heard of demography, and I dutifully went to the library to look up this Ansley Coale in the card

catalogue. The only book I could find was *Regional Model Life Tables and Stable Populations* by Coale and Paul Demeny, published by the Princeton University Press. This is a substantial book of 900+ pages, weighing in at 4 pounds, 12.5 ounces. It is also the world’s most boring book, with only 4 pages of figures and 25 pages of text, but a staggering 875 pages of tables. Altogether there are only 14,850 words of text but 553,609 numbers. So my heart was filled with dread that I would die of boredom at an early age.

However, my fears were groundless. From my first day at Princeton, Ansley became my mentor and friend. His two-semester demography class was the starting point in my subsequent career. And he and his wife, Sue, introduced me in the first week to the Homestead Inn, their (and now my) favorite restaurant in Trenton. Ansley was simply a terrific mentor. As with more than 35 other Ph.D. students over many years at Princeton, he was my principal thesis advisor. I also had the good fortune to stay on at the Office of Population Research after finishing my dissertation, and Ansley and I continued to work together for many years, eventually publishing nine papers together.

What made Ansley such a great mentor? In part it was his infectious enthusiasm for any demographic problem or issue, with the single conspicuous exception of migration. In part it was his extraordinary brilliance and insight, but most of all, it was his integrity. Jane Menken and I were working on our theses at the same time, and we each had the experience of having to completely redo our empirical analyses when Ansley discovered a small error in our calculations. We knew, and Ansley knew, that redoing the calculations would make absolutely no qualitative difference, and only a miniscule quantitative difference, to our results. But we each knew that we had to redo the calculations, even if that took much time and effort, because Ansley would have



done so. "Because Ansley would have done so" is a phrase that I have silently spoken to myself or said out loud to my students many times. Recently, Allison Hedley, a Ph.D. student at the Office of Population Research, handed in a complete draft of her thesis. A week later, she came to tell me that she had discovered that she had miscoded a handful of cases out of 6,568 in the entire dataset and so she would be rerunning all of her analyses. Imbuing that sense of integrity is Ansley's finest legacy.

Was Ansley without fault? Hardly. He could be incredibly stubborn. And he could also be controlling. In the many times he took me to dinner at the Homestead Inn, he never allowed me to order; instead he always ordered family style for the whole table. He was also extremely competitive. Often, after talking with Ansley about a problem during the day, I would work late into the night trying to solve it, knowing that if I did not, he would arrive the next morning with solution in hand. Ansley could be charmingly naive. The day that Lawrence Altman's first piece on AIDS appeared in the *New York Times*, a group of us was discussing the content. Regarding the description of a man who had had thousands of sex partners, Ansley proclaimed that the *Times* had made a typographical error by inflating the number by a factor of a thousand.

The last of us (C.F.W.) knew Ansley longest. Ansley is someone I knew well since I first arrived in Princeton in 1955 and with whom I interacted virtually every day at the Office of Population Research.

Ansley had a habit of walking around the office looking for open doors and cornering people with some new idea. Several memories stick with me, especially the incredible and ingratiatingly boyish enthusiasm he had for what would sometimes turn out to be the germ of a really important idea. His chief box of tools would be a piece of paper or a black-

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board on which he would depict some relationship with a scribbled graph and some illegible notations. His enthusiasm for ideas was really infectious and made for an exciting intellectual atmosphere at the Office of Population Research.

One of his teaching achievements that he was proud of was to inject an attitude of skepticism into graduate students, many of whom came from developing countries in the 1960s and 1970s. He would rejoice when a student would begin to question the accuracy of a printed number in a census or other publication.

Ansley introduced me to two activities that were to become lifetime habits for me: tennis and squash, and wine. He taught me to play, and it became my lunchtime activity for 45 years. Ansley had a highly competitive streak on the court (as well as at the bridge table) to which his wife, Sue, his two sons, and many others can attest. He also had a low tolerance for any extraneous noise while playing tennis, especially loud music emanating from loudspeakers in nearby dorms, particularly audible after he had made an error.

He also introduced me to wine. I especially remember the 1959 Beaujolais, which at the time sold for \$1.29, later to be regarded as one of the great wines of the century. I could not tell the difference then but I can now; it turned into an expensive habit.

Ansley loved to argue, especially about politics. We certainly agreed on important issues, but if there were any inkling of disagreement, he would interrupt and repeat his position. If you then made the mistake of persisting in your mistaken view, he would interrupt again with an insistent "Excuse me, excuse me" (a polite way of telling you to shut up and listen to him) and with an ever-reddening complexion would begin to question your understanding of the gospel truth. And there was a lot of "gospel" in that Presbyterian conscience he acquired from his father.



He was in love with an Italian restaurant—The Homestead Inn, also known as Chick and Nello's after the two founders—in a Trenton suburb. Ansley loved that place and once confided to me, *sotto voce*, that Chick's was perhaps the best restaurant in the world! (He later denied having said that.) Ansley had learned Italian and used the staff at that restaurant to practice and to impress them (*Il capo di tutti capi!*). He and Sue spent many pleasant summers in Italy at a villa in Florence and frequently as the guest of his close friend Massimo Livi-Bacci.

Ansley was a stickler for grammar and for spelling, an obsession I shared with him that he thought was one of my really good points. I did catch him in a grammatical error once that provoked an argument, but he later sheepishly confessed that he was wrong.

There are so many other memories: the annual office picnic softball games in which we would each pitch for opposing teams; the trips we took together to the Caribbean; his accidents on his bike and especially the accident diving into the shallow pool in a Manila hotel (that resulted in his appearance with a huge Band-Aid on his face on the stage at the opening ceremony of an international population conference with President Ferdinand Marcos); and the many lovely dinners at the Coale's home. Ansley had a very important influence on my life, on the Office of Population Research, and on the field of demography.

Perhaps Coale's major scientific contribution was to our understanding of the demographic transition. He was the intellectual architect of the European Fertility Project, which examined the remarkable decline in marital fertility in Europe. Initiated in 1963, the project eventually resulted in the publication of nine major books (culminating in *The Decline of Fertility in Europe* [1986]) summarizing the changes in childbearing during a century in the 700 provinces in Europe.

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Should the United States Start a Campaign for Fewer Births?

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## CURRENT ITEMS

SHOULD THE UNITED STATES

START A CAMPAIGN

FOR FEWER BIRTHS?

Each generation seems to assume that the demographic circumstances it is experiencing will persist and tries to formulate population policies to insure that they won't. During the 1930's social scientists, noting that fertility had been declining for more than a century in the United States and for fifty or sixty years or more in most of the wealthier European countries, and observing that although natural increase was still positive for most of these populations, current fertility was below replacement and the intrinsic rate of increase was negative, foresaw a declining population in the near future. The economic and social implications of a declining population were expected to be unfavorable. In fact, according to some economists, slow population growth in the 1930's was one of the major reasons for the worldwide depression. These circumstances and trends led some alarmists to speak of incipient race suicide, and others to deplore the supposed dysgenic effects of especially low fertility among the educated and the wealthy. There was much discussion of the need for pro-natalist policies. A number of countries, some with authoritarian governments and others quite democratic, started a pro-natalist program of one kind or another.

Today, after some twenty years of fertility well above the pre-World-War-II levels, it has become fashionable to explain almost every national failure or shortcoming by rapid population growth — the ugliness and hopelessness of slum life, wasteful and irritating traffic jams, unemployment and delinquency among the disturbingly large fraction of adolescents who drop out of school, the pollution of air and water, and the disappearance of the natural beauty of our country behind a curtain of billboards and under a blanket of kleenex and beer cans. The most dramatic example of blaming population for our problems was a full-page ad that appeared recently in New York and Washington papers showing an older man being assaulted; his glasses were falling off, and around his neck was one hand of his assailant; the other held a knife. The caption asked, "When were you last mugged?" and ended with a plea for funds for an anti-natalist organization.

Tonight I am taking the opportunity of expressing my own opinion, in large part an uncertain and tentative one, concerning what policy on population might be appropriate in the United States. My subject is not population policy within the less developed countries, nor the appropriate policy of the United States with regard to population problems in these countries. Within the national context, I shall consider only policies directed towards influencing the growth of the population (not its location or quality). I shall indeed confine myself to policy designed to in-

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Editor's Note.—This is the revised text of the address delivered by Ansley J. Coale, Princeton University, President of the Population Association of America, at the banquet on the evening of April 19, 1968, at Boston, Massachusetts, as part of the annual meeting of the Association.

fluence natality, not touching on migration, and assuming that the general policy with respect to mortality will be a continued effort to reduce it. I shall discuss, in other words, the considerations that might underlie a national policy designed to influence the future of American natality.

One policy towards natality that has very widespread, though of course not universal, support is to seek to make available to every couple, and indeed to every woman, the means of insuring that no pregnancy should occur except as the result of a deliberate decision to become pregnant. Opposition to this policy is on primarily religious grounds, and this does not seem an appropriate time or place to discuss moral or religious issues. Recently, the movement to insure that each pregnancy is a planned one has been criticized as an inadequate population policy, since, if every woman in the United States were to experience the number of births that she seems to want, the result would be an increase in the population of some 40 per cent per generation, or an average annual growth of more than one per cent. In the long run a continued growth at such a rate would be disastrous, leading to such well known absurdities as one person per square foot in six to eight centuries and after a few more centuries a mass of Americans outweighing the earth. The continued achievement of recently expressed family-size targets would yield a population of over 300 million before the end of this century, and over a billion before the end of the twenty-first. Thus, if preferences for the number of children remain fixed, and if the supposed association between traffic congestion, air and water pollution, juvenile delinquency and the like, on the one hand, and population growth, on the other, is accepted, it appears that a policy of extended planned parenthood may be wholly inadequate, even during the next few decades.

One reason the policy of universal voluntary fertility is appealing—whether adequate or not—is that it is a natural extension of traditional democratic values of providing each individual with the information he needs to make wise choices, and of allowing the greatest freedom for each to work out his own destiny. The underlying rationale is that if every individual knowledgeably pursues his self-interest, the social interest will best be served. Indeed, I am confident that in America universal and completely effective birth control would cause population growth to cease before the Malthusian positive checks of famine and disease reassert themselves. If the cost of living space became high enough relative to other costs, people with a free choice would come to prefer a smaller number of children. But it is clearly fallacious to accept as optimal a growth that continues until overcrowding makes additional births intolerably expensive. In fact, this is a classic example of the fallacy of composition: the fallacy of assuming that in social affairs the whole is the sum of the parts. In many instances the individually advantageous choice is not socially desirable. For example, in a period of incipient inflation, the most advantageous action for each individual is to get rid of his money as quickly as he acquires it and to obtain and hoard real assets—an action which, generally followed, naturally makes the inflation worse.

In other words, to fulfill the stated preferences for numbers of children of American couples today would lead to a rate of increase well above zero, a rate that would in the long run be catastrophic. We cannot count on the automatic effect of self interest to reduce the growth rate soon enough, even if contraception were universal and virtually

perfect. In fact, we cannot be sure what the future growth of population will be, with or without explicit measures to influence desired family size. We must recall that fertility was low and rapidly falling in the prosperous 1920's, rose to unexpected heights in the postwar period, and has been falling steadily for the past decade. Not all of the fluctuations are caused by changes in completed size of family. We know that a large part of the "baby boom" was a change in the tempo of childbearing, and so is some of the decline. Nevertheless, no one — not even Professor Ryder — can confidently predict the average number of children that will be born to today's adolescents. The possibility must be taken seriously, however, that fertility in America, even when wholly subject to voluntary control, may result in persistent growth at a rate of perhaps one per cent annually. Since this possibility implies that a policy aimed merely at improving the means, and extending the availability, of contraception may permit a socially undesirable growth in population, ways to affect the number of children people want must be examined.

### Goals of Population Policy

Two questions are raised by the design of policies to influence the number of children people want. First, by what means are couples to be induced to alter the number of children they want, and what are the associated side effects, so to speak, of these specifics on social and political goals other than population growth itself? Second, leaving aside the means of influencing desired family size, what should the target of policy be in terms of the socially desired number of births? I shall discuss the second question first.

Assuming that natality could be influenced with no adverse social or individual costs, what should the target be? A few years ago, the answer to this question would be sought by trying to estimate the optimum population. If the optimum were larger than the current population, the target should be a positive rate of growth until the optimum is reached. The rate of growth should be zero if the optimum were approximately the same as the current population, and so on. More recently, the emphasis has shifted from attempting to choose the best population size to choosing the best rate of increase. Lincoln and Alice Day, in their book, Too Many Americans, proposed the target of a zero rate of increase at an early date. This goal was vigorously endorsed by Kingsley Davis in his provocative article in Science in November, 1967.

In the long run, an average rate of increase of zero is not only desirable but inevitable. The population of the world in 8000 B.C., prior to the appearance of the earliest known farming communities, has been estimated as 5 million. Today — 10,000 years later — the world population at 3.5 billion, is nearly 1,000 times larger, yet the intervening average rate of increase has been less than 1 per 1,000 per year, a very precise balance indeed between births and deaths. If we agree that the earth will have at least 5 million inhabitants or at most 5 trillion 10,000 years hence, the average annual rate of increase, positive or negative, must be within 1 per 1,000 of zero. In other words, a long-range average growth of zero will be the inevitable consequence of inevitable limits — on the one hand of standing room only, and on the other of extinction. The question really is, when is it desirable to attain



zero rate of increase. We can rule out a target of zero growth beginning this year, since to achieve it we would have to reduce the birth rate to about 9 per thousand — that is, to equality with the death rate — and the consequence would be such a precipitous decline in the number of children, and such an irregular subsequent age distribution, that a large number of unnecessary social burdens would be incurred. A more acceptable goal would be the achievement of mere replacement for the cohort of women now entering childbearing age, i.e., women 15-19. This more modest target would bring an increase to over 250 million by the year 2000 and a stabilization a few years later at about 290 million, in the absence of further immigration.

If population growth is accepted as the cause of overcrowding, poverty in the slums, daily traffic jams, and pollution of air and water, then the early achievement of a zero growth rate is clearly desirable, and it might even seem worthwhile to contemplate a shrinking population to diminish these problems. It seems to me, however, that the association between the overall size and growth of the United States population and these problems is really rather indirect. For example, high fertility is one of the factors helping to perpetuate the deprivation suffered by racial minorities in this country. To attribute their deprivation primarily to this cause, however, is a particularly flagrant error and to advocate special measures to reduce their fertility is a very dangerous doctrine with overtones of prejudice. The fact is that Negroes with a northern urban background have no higher fertility than whites with equivalent education and income. Negro women married to men with college education, with professional or related occupations, and with incomes of about the \$7,000 level, have lower fertility than comparable white groups. Nearly half of the Negro urban population 20 to 30 years old in 1960 had been rural residents in 1950. Rural Southern residents did not overcome the effects of meagre education and the other shortcomings of the environment in which they had been raised when they moved to urban areas, but rather took the effects of their upbringing, including high fertility, with them. Fertility in the urban ghettos will fall if discrimination is alleviated, if educational and employment opportunities are equalized.

Pollution is caused by internal combustion engines, as operated at present, and by the unrestricted discharge of noxious fumes from other sources into the atmosphere. Similarly, water pollution is caused by the discharge of noxious effluents into rivers, lakes, and oceans. A population one-half or three-quarters the size of the current one in the United States could ruin the potability of our fresh water supplies and poison our atmosphere by the unrestricted discharge of waste. Australia has a population of less than 12 million in an area more than 80 per cent that of the United States. Yet Sydney has problems of smog, water pollution, and traffic jams. In fact, most of the social and economic problems ascribed to our excessive population in the United States, or to its excessive rate of growth, are affected more by how our population has chosen to distribute itself than by its size. The problems arise from excessive concentration in the metropolitan areas, not from excessive total numbers. More than half the counties in the United States have lost population in each of the last two intercensal decades. The density of population is 4.5 times greater in France, 10 times greater in the United Kingdom, and 30 times greater in the Netherlands than in the United States, yet pollution, traffic jams, and delinquency are no worse in those

countries than here. Even if our population were to rise to a billion, its average density would not be very high by European standards. It seems to me that we must attack the problems of pollution, urban deterioration, juvenile delinquency and the like directly, and if sensible programs are evolved, continued population growth in the order of one per cent annually would not make the programs tangibly less effective.

But what reason, if any, is there to postpone the target of zero growth? One such reason is that a stationary population has unfavorable as well as advantageous effects. The most conspicuous disadvantage is the age composition implied by a stationary population, especially at the low mortality that has been achieved in advanced countries. A stationary population with an expectation of life of 70 years has as many people over 60 as under 15. The distribution is essentially vertical up to age 50 or 55. The median age is about 37 years. A society with such an age structure is not likely to be receptive to change, and indeed would have a strong tendency towards nostalgia and conservatism. A French writer has characterized a stationary population as "a population of old people ruminating over old ideas in old houses." In a stationary population, as Myrdal pointed out years ago, there is no longer the consonance between the pyramid of responsibility and the age pyramid that there is in a growing population. When the population is stationary, there is no longer a reasonable expectation of advancement in authority with age, since the number of 50-year olds is little different from the number of 20-year olds. I repeat that zero growth rate is inevitable in the long run, and desirable before overcrowding becomes painful. It is not clear, however, that this generation would choose wisely to accept during its lifetime the costs of a stationary society rather than to pay during its lifetime the costs of continuing growth. To put the point in different terms: the logical argument that the population must become stationary could have been made in 1920 or in 1900 or in 1850. It might have been wiser (indeed, I think it would have been) to have encouraged slower population growth than that experienced by the United States in the late nineteenth century. It does not seem obvious, however, that it would have been to the advantage of Americans, then or now, had a stationary population been chosen at some earlier time. Nor does it seem certain to me that the earliest possible stationary population is the best choice today.

The question is one of balance between the disadvantages of further growth and greater population size, on the one hand, and the disadvantages of a stationary population, on the other. The problem is complicated by the fact that the choice is inevitably not only for ourselves but also for future generations. Many of the burdens of allowing the population to grow during the next 100 years will be borne by descendants who will live after the middle of the next century. It is difficult to visualize these burdens, however, (and also difficult for many of us to be concerned about them); it is also uncertain how the burdens will be viewed by the future citizens themselves. It is my observation that the disadvantages of a larger population are seen most vividly by those who were born in an earlier era. Often the current inhabitants see nothing wrong with many of the changes that the older citizens decry. I feel deprived by the disappearance of open land around Princeton. My children never miss it. People raised in Manhattan find life there not only tolerable, but preferable. In other words, to pay the price of an early achievement of a stationary population in order to spare our descendants higher

population density may be to do them a favor that they will never appreciate.

### Methods of Influencing Population Growth

Whether or not we now need programs to affect people's preferences for more or fewer babies, we may well need them in a generation or two, and we must face the question of what sorts of programs would be suitable.

One can begin by describing some of the properties a good program should have and some of the pitfalls it must avoid. A major consideration is the effect of the program beyond its effect on the birth rate. Preoccupation with population growth should not serve to justify measures more dangerous or of higher social cost than population growth itself. (At the end of a working day during the construction of the Brooklyn Bridge, an Irishman was returning to the ground from work high on one of the towers. He was sliding down a rope, and was still more than 30 feet above the ground, when he fell. One of his friends came up as Pat was waiting for the ambulance and asked, "What happened, Pat? Did you lose your grip?" "No," he said, "I let go of the rope." "Why, in heaven's name?" "Because I thought it was going to break.")

An ideal policy would permit a maximum of individual freedom and diversity. It would not prescribe a precise number of children for each category of married couple, nor lay down a universal norm to which all couples should conform. Within any overall social context, some people have a stronger preference for children than others, and the ideal policy would permit the expression of this diversity of individual values. Some of the changes that contributed to the postwar baby boom seem to me desirable from the point of view of the individual family. The changes to which I refer include a substantial decline in the proportion remaining single, a very large decrease in childlessness, and a marked diminution in the proportion of couples having only one child. I think we should not view it as individually reprehensible if some couples desire to have three, four, or more children. After all, the psychological burden of being an only child is well known, and in a two-child family sibling rivalry is at a maximum, and when there are three children, two are likely to gang up on one. Thus, when the extra burden of the fourth child does not worsen parental dispositions or health, four does not seem an undesirable number from the point of view of the children in the family. It would be clearly inconsistent when discussing a policy intended to produce a zero rate of increase, to argue that four should be the norm, since to achieve replacement the average number born to married couples must be about 2.3. The ideal policy, however, would permit a minority of families (families in which the parents are prepared to substitute expenditures on more children for expenditures on luxury goods, and in which the housewife is prepared to devote her talents and energy to child-raising rather than to some alternative career) to choose an above-average number of children without being viewed with scorn or disapproval.

An ideal program designed to affect the number of children people want would help promote other goals that are worth supporting on their own merits, or at least would not conflict with such goals. Specifically,

the program should not have the inequitable effect of adding to the burdens of the underprivileged, but rather should reduce inequity by easing their burdens. A program for increasing the number of children people want is more readily reconciled with humane goals than is a program for diminishing the number they want. In the 1930's, when the Swedish birth rate was well below the level required for long-range replacement, the liberal Swedish government proposed a number of measures intended to increase fertility. The Swedes were opposed to the cash benefits then being introduced in totalitarian Germany and Italy. They felt that financial inducements might appeal to the persons least equipped to provide a good home environment for the next generation. The Swedish policies were designed to remove or to reduce the restraints that were then preventing responsible parents from having more children. These policies provided free prenatal care and lying-in hospitals to minimize the danger and cost of childbirth, offered to large families lower rates of rental per room in government housing, so that the cost of extra space would not serve as a deterrent to having more children, and increased the number of state scholarships in Swedish universities, so that parents could hope to educate an extra child. No one would advocate the converse of these proposals as an ideal program for reducing fertility when it is too high. The reason that there is an instinctive negative reaction to deliberately increasing medical charges for prenatal care and delivery, purposely raising rents for large families, and reducing scholarships at the universities, is that such measures, and others that have sometimes been proposed, such as tax scales that increase with size of family, would all have the effect of imposing hardships on those who are already deprived. Measures to discourage fertility by imposing additional burdens on those who bear children will almost inevitably be unfair so long as a substantial proportion of births, especially to those with high fertility, result from accidental pregnancies. If another unwanted child is added to a large family, his lot will scarcely be improved if his parents face some kind of financial or other penalty as a result of his birth.

The planned parenthood program of making people aware of the possibility that the number of children born can be regulated consciously, and of making readily accessible truly effective means of contraception that do not interfere with sexual pleasure, meets these criteria. To eliminate unwanted childbirth clearly increases freedom of choice. Since the burden of unwanted pregnancies is greater for the underprivileged, as is the incidence of such pregnancies, extension of effective fertility control contributes at least slightly to a narrowed difference between the privileged and the deprived. The proportion of most recent births characterized as not wanted is especially high among Negroes at the lower end of the scales of income and education. However, we must repeat, a planned parenthood program is not necessarily adequate to achieve the growth that public interest requires.

Economists at the University of Chicago are famous for judging a public policy as desirable to the extent that it operates through the mechanism of a free market. I have heard it facetiously suggested as the ideal form of fertility regulation to introduce a highly effective anti-fertility agent in the water supply and then to sell the antidote at a controlled price, allowing market forces to do the rest. When the birth rate is too high, the price of the antidote can be raised, and vice versa. People who place an especially high value on children can have as many

as they can afford. The less doctrinaire economists of this school might be willing to complicate such a method of regulation by a scale of prices to allow for the different economic circumstances of different purchasers. The system could be extended to cover fertility below replacement, even when the price of the antidote is zero, by offering a subsidy under these circumstances to persons willing to take the antidote. The feature of an approach like this that I find most appealing is the fact that it does not attempt to enforce a limitation of two births or three births per couple, as has been recommended, for example by Lincoln Day.

Of course, the solution I have attributed to the Chicago school of economics is an exercise in science fiction. It is probably more unrealistic politically than technologically. I describe it only to characterize, even if in an unrealistic fashion, features that one would like to find in a program for regulating fertility. There are more feasible measures that might be implemented, such as reducing the remaining discriminatory rules and attitudes about the employment of women, further equalizing educational opportunities at both the undergraduate and the professional level, and in general opening further career opportunities as an attractive alternative to motherhood. A liberalization of laws on abortion would be a useful supplement to the planned parenthood program outlined earlier. A set of measures of this sort does not add up, of course, to a tidy and logical but unrealistic Chicago economists' program. Perhaps the greatest weakness of such measures is that their effect on fertility is uncertain and occurs after a substantial lag in time. Also, the measures are irreversible — it would hardly be feasible to increase discrimination against women if the birth rate fell too low. Fortunately, as was stated in the first part of these remarks, I do not think that we need to rush into a program of special inducements for lower family-size targets. After all, the gross reproduction rate in 1967 was only about 1.30, some 28 per cent below its peak in 1957. The birth rate of January 1968, moreover, shows an especially sharp additional drop, even after seasonal adjustment. It is possible that by the time a feasible program is ready for formulation in legislative proposals, we shall be seeking ways of increasing fertility in order to avoid a declining population.

DEMOGRAPHY IN THE INTERNATIONAL By the mid-'fifties the  
time seemed to have  
ENCYCLOPEDIA OF THE SOCIAL SCIENCES come for a successor to  
the Encyclopaedia of the  
Social Sciences, the 15-volume work edited by E. R. A. Seligman and  
Alvin Johnson, that had been published between 1930 and 1935. In the  
interval since then, Dr. Johnson remarked at a session of the annual  
meeting of the social science associations in 1955, a few new ideas had  
appeared and many old ones had been rechristened. The Study Group  
whose recommendations in August 1955 led to the undertaking of this  
second generation encyclopedia had several purposes in mind for it. "A  
new cyclopedia would have an important impact," their report states,  
"especially upon the social sciences in small colleges and other places