

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Ronald D. Lee PAA President in 1987



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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RONALD D. LEE

PAA President in 1987 (No. 50). Interview with Jean van der Tak at the Graduate Group in Demography, University of California at Berkeley, April 28, 1989.

CAREER HIGHLIGHTS: Ronald Lee was born in 1941 and grew up in Poughkeepsie, New York. He received the B.A. in philosophy from Reed College in 1963, the M.A. in demography from the University of California at Berkeley in 1967, and the Ph.D. in economics from Harvard in 1971, interspersed by two years (1963-65) with the Peace Corps in Ethiopia. He spent 1971-72 at INED (L'Institut National d'Etudes Demographiques) in Paris and was Professor of Economics with the Population Studies Center at the University of Michigan from 1972 to 1979. Since 1979, he has been Professor of Demography and Economics with the Graduate Group in Demography at Berkeley.

He is well known in the demographic world for his research and writing on mathematical models of fertility and population growth, particularly applied to U.S. trends and historical trends in England and Europe. He won the PAA Mindel Sheps Award for Mathematical Demography in 1984. He has also done much work related to less developed countries. He chaired the working group on fertility determinants of the Committee on Population and Demography of the National Academy of Sciences and was co-editor, with Rudolfo Bulatao, of that group's report, Determinants of Fertility in Developing Countries (1983). He was co-chair of the NAS working group which produced the famous report, Population Growth and Economic Development: Policy Questions (1986), of which he was co-author with Samuel Preston and Geoffrey Greene. He chaired the IUSSP research committee which produced the monographs, of which he is a co-author, Economics of Changing Age Distributions in Developed Countries (1988) and Population, Food, and Rural Development (1988). He has also served on the grants review committee of the National Institutes of Health.

VDT: How and when did you become interested in demography?

LEE: Well, I should go back to my undergraduate career at Reed College. I started out as a major in physics and then switched in my sophomore year to a major in mathematics and then switched in my junior year to a major in philosophy. By my senior year, I was getting interested in social problems and actually had applied to and been accepted in a graduate city planning program. But I decided to go into the Peace Corps instead and spent two years in Ethiopia, teaching school from seventh grade through seniors in high school, in math and physics.

VDT: This was in the mid-1960s?

LEE: This was from 1963 to 1965, so it was in the years of the Kennedy euphoria--what you could do for your country and so on. The Peace Corps had just begun. It was also the time that the Vietnam War was heating up and more and more people were being drafted. The Peace Corps didn't provide any exemption from the draft afterwards. In 1965 it didn't even merit you a deferral, as people were being drafted out of the Peace Corps. I was very concerned about being drafted and didn't like the war or any part of it. That was the background when I was thinking what I was going to do after the Peace Corps.

I thought about continuing to teach school, which was a deferrable occupation at that time. But I really wanted to go to graduate school; however, I didn't know what I wanted to go to graduate school in. What I wanted to do was continue my very diffuse undergraduate school career because it was interesting; it was fun taking courses in a lot of different things. But no graduate school that I knew of had a "graduate-student-at-large" program--you had to be in something. I did know that I wanted to go

to Berkeley; I liked Berkeley.

VDT: Why did you know that?

LEE: Well, at Reed College people thought of Berkeley as a wonderful place to be. And I had been down here. I'd taken summer school courses in economics and I had a couple of sisters living in the Bay area and I'd come down and visited them, so I knew a bit about what it was like here.

In any event, a brother-in-law in Berkeley mailed me a letter to the little town in Ethiopia where I was, mentioning that there was a new program in demography and it was very interdisciplinary; you took courses in many different fields--social science, math, biology, and so on. I had no idea really what demography was, what the word meant; I had to go look it up in the dictionary. Having looked it up I thought, well, all right, why not? It didn't sound fascinating to me, but it sounded as interesting as anything else did, but I wasn't particularly interested in any subject.

So I applied to that program. And eventually I learned in Calcutta, on my way home and my mail caught up with me, that I'd been accepted. So I came to Berkeley. I think it was the first year the Group in Demography here was really under way. So I was in the first cohort.

VDT: In 1965?

LEE: It started in 1965, yes. I studied demography and it was indeed a very interdisciplinary program. I took courses in economics and biology and statistics and math and sociology and public health. It was a very demanding two years of courses for the master's degree. Then having gotten my master's, I decided I was tired of graduate school and left.

VDT: You didn't have a thesis, just coursework?

LEE: Just coursework and then an oral exam. I think you got to choose whether you wanted to write a thesis or do an oral exam. My teachers here were Judith Blake, who was running the program, and Kingsley Davis, who was in the sociology department, and Nathan Keyfitz, who came as a visitor a couple of times and was very influential in the development of my career. Peter Mazur, who is now in a college in the state of Washington, Thomas Burch, Karol Krotki--these people were visitors. Eduardo Arriaga was around in a semi-student, semi-faculty status.

VDT: What a battery of famous names!

LEE: Yes. And William Petersen and Carlo Cipolla, who does Italian demographic history. There were people like this. It was an exciting place to be. I remember the Friday afternoon seminars, extremely lively, and I think often uncomfortable for the speakers. Calvin Goldscheider was another name, also teaching.

VDT: Those seminars were by the professors on anything?

LEE: Sometimes a professor, sometimes a visiting speaker. I remember Ronald Freedman coming through, for example. These were the days when there was a deep split between people with a public health, family planning orientation toward population problems in the Third World--people who believed that making contraceptives available would lead just in itself to fertility decline--very much opposed to people with, say, a more sociological structural view of the problem, and certainly Judith Blake and Kingsley Davis were in this latter group. So when someone like Ronald Freedman came through, I think we students expected someone far less well informed from the sociological point of

view than in fact we encountered. Ronald Freedman clearly understood that fertility was a matter of both institutions and norms and of access to contraceptives. But that was also a lively debate, a controversy, within the Group in Demography and then when Freedman came.

VDT: That was about the time when Kingsley was preparing his famous article, "Population Policy: Will Current Programs Succeed?" published in 1967 [Science, November 10, 1967], when he became skeptical of family planning programs because they were pushing the number of children that people desired and that was too high.

LEE: Yes, so that was all very much in the air. I don't remember hearing Kingsley talking about that article specifically. I do remember a lot of discussion of Kingsley's also very influential article on the demographic transition and multiphasic response.

VDT: That was his 1963 PAA presidential address, "The Theory of Change and Response in Modern Demographic History" [Population Index, October 1963].

LEE: I should mention also my fellow students at that time, many of whom are active now. Ruth Dixon and Harriet Presser, our current [1989] PAA president, were graduate students at the same time.

VDT: Also in that first cohort of the Group in Demography?

LEE: Ruth and Harriet were doing PhDs in the sociology department, but they were taking demography courses so I encountered them in classes. George Simmons was doing an economics Ph.D., but I knew him also through courses we took together, in particular one that Karol Krotki taught in population and economic development. The influential Coale and Hoover book [Population Growth and Economic Development in Low-Income Countries, 1958] had come out fairly recently and I remember that much of the discussion in that class was about that book. Cam Gibson, who for many years constructed projections at the Census Bureau, was in the class, and Roger Avery, very active in research at Cornell and now at Brown, and Peter Uhlenberg, now at North Carolina.

VDT: A tremendous battery of those who went on in the field. Did you realize you were sort of the beginning of a new era?

LEE: Not at all. There was also George Masnick, who was at Harvard Population [Sciences] for quite a number of years. Barbara Heyns, who's now at NYU, was an undergraduate but a research assistant for Judith Blake and also involved in the program. Shirley Hartley, who is now at Cal State, Hayward, Robert Retherford, who's at the East-West Center. There were probably others whom I'm not remembering, but there were a lot of people who are quite well known. And it was a small program, so that's a high proportion who were around in the two years I was there.

VDT: You were there because your brother-in-law sent you something. Do you think these others were drawn by Judith Blake and Kingsley Davis?

LEE: Most likely, yes. And I think most of them knew much better than I what demography was, what it meant, what they were doing. They perhaps had some background in it before. Harriet Presser, in her presidential address [PAA meeting, Baltimore, April 1989], mentioned that she'd been at North Carolina. Maybe she was at Berkeley to do her dissertation, I'm not sure. [Did M.A. at North Carolina under Daniel Price; came to Berkeley specifically to do demography-focused Ph.D. in sociology.] Many of these would have been people who didn't come to Berkeley to do demography, but got

interested in demography through coursework there and then continued it. Many were in economics and sociology, rather than demography per se.

VDT: I wanted to ask you later, when we come to your current time here at Berkeley, whether you think it's important to have a separate department of demography, which the Group in Demography went on to become for five years [1967-72] and then ended.

LEE: It was just becoming a department as I left. It was the Group in Demography for two years [1965-67] and became a department thereafter. It had some sort of existence before I came, but I think it did not actually admit its own students in demography and probably didn't teach courses in its own listing. I think that sort of change happened at the time I came in 1965. But you'll be able to confirm that with Judith Blake, who'll know better than anyone [see Judith Blake interview].

VDT: Why did you leave Berkeley if they were just setting up a new department of demography? Or why did you go to Harvard? Was it the push or the pull, or what?

LEE: It was a difficult time to be trying to do something serious, the mid- or late 1960s, at Berkeley--perhaps all over the country. This was the time when you didn't trust anyone over 30 and the prevailing morality was that responsibility within the system and any sort of routinized activity was equivalent to brain death and a sort of moral decay.

VDT: Already in 1967 that's the way students felt?

LEE: Absolutely, yes. It was very much the cult of young people; aging was the process of encrusting with responsibility.

VDT: You mean that's the way they viewed the professors--or anyone over 30?

LEE: Yeah, all of that. And the process of life itself. That is, the noble fight was to prevent the growth of these barnacles of, you know, professional life or anything of that sort occurring all over your mind and spirit.

VDT: So you should just come get a quick degree and out . . .

LEE: I sat in Golden Gate Park and listened to Timothy Leary say: "Tune in; turn on; drop out." And I dropped out.

VDT: Having first got your degree [M.A.].

LEE: That's true. But it was really in that spirit of sort of striking out into the world and looking for adventure. I thought I wanted to be a writer, perhaps. I wanted to see what else life had to offer besides this narrow routine of student life, academic life. So I didn't leave with anything else set up to do, really. I had no notion at that time of going to Harvard. I just left and traveled a bit. I went to Mexico; I traveled a bit in the States. I tried my hand at writing for a while. I decided that I was not going to be a writer.

Then I had to decide what I was going to do and I still wasn't sure at all, but eventually I ended up in Cambridge, sometime late in the fall of 1967, five or six months after I'd gotten my master's. I thought about different things I might do--law school, medical school. Eventually I decided I'd look into graduate school in economics and I managed to talk Harvard into letting me enroll in mid-year in

the economics program. So I started working on a Ph.D. in economics in January 1968.

VDT: Did you have a fellowship already?

LEE: I didn't have any fellowship; I had some savings. I had had a National Institutes of Health traineeship at the Group in Demography, the same source of funds that many domestic students studying with us get these days. But at Harvard I was living on savings that first semester and then I got fellowships.

VDT: You still weren't gripped by demography?

LEE: No. I felt I'd had enough of demography.

VDT: You went purely to the department of economics at Harvard?

LEE: Yes. I had no intention of ever getting involved in demography again. But as I started taking my economics courses and had term papers to write, I found myself being led back into demographic questions. But now they were economic demographic questions: How could I relate what I was learning in economics to what I had learned in demography? I was interested in that both at the formal level of mathematical modeling and also just trying to understand the substance of what I was studying. So from the very beginning, I started writing term papers on economic demography. Some of my first term papers were on economic history, which was a requirement in the Harvard program; I think it still is. I wrote papers on the role of population in English economic history and that work eventually led into my thesis.

VDT: Your thesis was "Econometric Studies of Topics in Demographic History." It all came together.

LEE: That's right. In other courses, I had econometric papers to write and I was trying to integrate the period and cohort mathematical models in demography or the interactions of age distribution changes in fertility and economic well-being in the United States, which I then discovered had been pretty thoroughly explored by Richard Easterlin. These are topics I began to work on as term papers and then developed into my dissertation and then into a lot of research afterward.

One interesting thing at that point was in working on these historical economic-demographic questions in England, I came across Tony Wrigley's now classic study of one parish there, Colyton. It was a reconstitution study, but he also showed the numbers of births, baptisms, deaths, and marriages over about three centuries in Colyton, from 1538 to the 1840s, something like that. I looked at the reconstitution results on changes in life expectancy and marital fertility and age at marriage and I looked at the numbers of baptisms and burials and I just didn't see how they could be consistent with one another; how the aggregate trends could be consistent with these estimates of vital rates. So I tried to formalize my dissatisfaction with what I saw and did it by working out a method I called inverse projection, which made it possible to take the birth, death, baptism, and burial series as an input and then estimate what total fertility rates and life expectancy and population age distributions must have generated that series of baptisms and births. I wrote a program to do this, having learned programming in a course with Keyfitz at Berkeley that was ostensibly on sociological methods and was actually a course on FORTRAN programming. I ran the baptisms and burials through this and out came exactly the vital rates that Tony Wrigley had found by reconstitution.

So I was disappointed, but it turned out that that was the development of what has subsequently turned out to be a useful method in historical demography. Indeed, that was the method that Wrigley

and Schofield used in their important book on the reconstruction of population history in England [E.A. Wrigley and R.S. Schofield, eds., The Population History of England 1541-1871, 1981].

VDT: How did you convey what you'd done to them? You published?

LEE: Yes, but before I published I did what I now advise all my graduate students to do. As soon as I finished my dissertation, I had ten Xerox copies made and sent them to all the people I thought would be potentially most interested.

VDT: You had the inverse projection method in your dissertation?

LEE: Yes, that was part of the dissertation. So I mailed one of these to Tony Wrigley and another to Ansley Coale, I'm sure. I don't remember where I sent the others, but some of them landed on fertile ground. Chuck [Charles] Tilly organized a three-week working group at the Princeton Institute for Advanced Studies the summer of 1972 and I think because I had sent this dissertation to a number of people, including Wrigley, I was invited to that group of about eight people and I met Wrigley and Schofield. That was the beginning of my subsequent collaboration with them.

VDT: Which ended up in several books, or at least there was Population Patterns in the Past [1977]--that was later.

LEE: That's right.

VDT: So that's how you got into the group of those who have looked at the past. You summed that up in a sense in your PAA presidential address in 1987, "Population Dynamics of Humans and Other Animals" [Demography, November 1987].

LEE: That's right.

VDT: Your interest in demographic history began at the time you were working on your dissertation. You started out with that economic history course, which fed into the demography that you did have.

LEE: Yes, but I was never interested in the history per se. I was interested in trying to find general laws, general principles, governing the movement of population over time, growth and decline and so on--a sort of Malthusian approach. In order to do that you need a long series of data and if you can't get data for the future, you're obliged to go back into the past. And to look at Malthusian kinds of principles, you pretty much have to look before the Industrial Revolution, so that puts it still farther back. But my first interest was in the demography--demography in the ecological sense of why was the population of the world whatever it was; why was it this in England; why was it bigger than it had been before; why did it decline in the 17th century. That sort of question. Then I just had to use what data were available, which turned out to be historical data.

I think that's somewhat the spirit in which demographers have generally tackled historical demography. There are few of us who are real historians, who really know something of the context of history. Most of us are demographers and were fascinated by demographic data and some of those data are historical data.

VDT: That must have led you into the Easterlin hypothesis.

LEE: Yes, indeed.

VDT: You said you found that he was the one working on age structure and the long swings in fertility.

LEE: Yes. It seems to me it's all part of the same idea, some sort of feedback or equilibrating process in that a population gets too large, encounters economic hardships, which lead to reduction in population growth rates, perhaps population decline, until some sort of equilibrium is established. That is the basic Malthusian idea, which did not have age distribution well incorporated, but as soon as you put age distribution into a model of that sort, then the possibility arises that current fertility is responding to current labor market conditions. By the time the current crop of babies comes of age, labor market conditions may have completely changed. So what may have looked like a good idea at the time the kids were born turns out to be a bad idea in the sense that conditions are much worse--or much better--than anticipated when they're grown up than the labor market was when they were kids. So you can have a kind of disequilibrating response, perhaps falling too strongly in the sense that fertility is cut back too much, so that you end up with a cohort that is in some sense too small 20 or 30 years, one generation, later. Then, responding to the fact that it's small and its economic conditions are therefore good, it has fertility that is in some sense too high and this leads to every-other-generation cycling.

VDT: You wrote an article for the symposium on the Easterlin hypothesis at the 1976 PAA meeting which was published later that year in Population and Development Review ["Demographic Forecasting and the Easterlin Hypothesis," PDR, September/December 1976]. You presented a forecasting method based on the Easterlin hypothesis and, according to that, you suggested that the current U.S. fertility--which was down to 1.7 in 1976, I believe, the lowest it ever got--was only temporarily low and there should be an upturn in "five or ten years."

LEE: Did I say five or ten years?

VDT: Yes, you did [p. 467 of that issue of PDR].

LEE: In what year?

VDT: 1976.

LEE: My recollection is mid- to late 1980s.

VDT: No, that's not what you said then. Easterlin, after all, talked about a fertility upturn by 1984 in his PAA presidential address of 1978 ["What Will 1984 Be Like?"]. You did say "five or ten years" in 1976. And that hasn't really happened, although the current [1989] total fertility rate rounds off to 1.9; it's crept up. And U.S. births in 1988 totaled almost 3.9 million, the highest annual number since 1964, largely due to rising births among women in their thirties. It's not the baby busters. It's the baby boomers who are having delayed children and it isn't the baby busters who are finding jobs and able to afford more children.

LEE: I would say there's no evidence now [1989] of the sort of upswing I was looking at, despite these little jiggles. It really isn't too late for something like that; it's the timing. I thought what I said then was this would be in the late 1980s. What is it?--we're almost out of the late 1980s.

VDT: You had the TFR shooting up in the 1990s. But, okay.

LEE: We will see. On the face of it, it just looks like it hasn't happened. At this stage in the 1980s, if the argument is right, the upturn should be really quite mild. But, as you say, in the 1990s, it should be very visible. But I would say that on the whole, it's not looking good for the Easterlin hypothesis in the last few years. On the other hand, almost every other theory I know of would have predicted continued fairly rapid fertility decline through the 1980s.

VDT: You mean as Westoff thinks, after the social changes?

LEE: Yes, the social changes have continued, divorce going up and so on.

VDT: You mean below 1.7?

LEE: Oh, yes. Below 1.7 isn't so extraordinary. We've got quite a number of European countries below 1.7.

VDT: Except that, did you know that Sweden is almost going to round off at 2.0? I brought you the proofs of the Population Reference Bureau 1989 World Population Data Sheet. I have to admit that when I was in there the last day, Carl Haub said: "Scratch that 2.0; I'm going to put it down to 1.9." But nearly 2.0!--which is something for Sweden. Italy is the lowest now, somewhat delayed, at 1.3. And West Germany rounds off to 1.4.

LEE: In any event, it seems to me that most, if not all, of the theories are predicting continued decline until we don't know what, because most of these theories don't really have any natural stopping point until you get to zero. There's nothing particularly natural about 2.1 or 2.5 as the end point of the social and economic changes that make up the driving force of the demographic transition. So the fact that fertility has just stopped declining and pretty much been flat for the last 10 or 15 years is, I think, more consistent with the Easterlin theory than it is with most of the others. That is, Easterlin theory would have predicted this leveling off even back in the early 1960s when Easterlin was writing. I'm not sure if he did predict a leveling-off in the mid-1970s, 1980s.

VDT: There were going to be the Kuznets swings, which are much longer--40 years.

LEE: Oh, yes. I mean he predicted the baby boom, but it's also implicit in that approach and in the diagrams he drew that you would expect a leveling-off in the 1970s and early 1980s, preceding this upturn that's expected in the late 1980s. So from that point of view, I think it hasn't done so badly, although this prediction of an upturn I would say should have happened last year, the year before--there isn't real evidence that that has happened.

VDT: Just delayed [prophetic! U.S. TFR in 1990 was 2.0]. Let's go back and talk about Harvard. Who did you do your dissertation with?

LEE: Formally, Harvey Leibenstein was chair of my dissertation committee, but in practice he had very little influence on it. He was out of the country most of the time I was working. The person who had most influence on it was not a demographer at all but an econometrician, Dale Jordanson. He'd been a professor of economics at Berkeley when I was here and I had taken a course in econometrics from him. Then I went to Harvard as a graduate student and a year later he came as a professor and I asked if he'd be on my dissertation committee and he said he'd be happy to. He had lots of time then, I think, wasn't surrounded with graduate students since he'd just gotten there. He said to me, "The way I

supervise dissertations is we set up a time each week, half hour, an hour, and you come in and present to me in a little seminar what you've accomplished in the past week."

So that really got me hopping, kept me hopping. Fortunately, I'd done a lot of the work already in these terms papers and it was more a matter of putting a polish on that work, deepening it mathematically and working out the econometrics and so on. It was very helpful, this arrangement I had with Jordanson, and I learned a lot from him. So I think of him as my main adviser, main influence, when I was working on demography at Harvard. Basically, it was very isolated. I don't think I knew another person who was interested in population. I can't think of a single graduate student; there really wasn't any community of graduate students.

VDT: Of course, Harvard didn't have a population center. It was once offered the Office of Population Research and turned it down. Fred Osborn went there first when he was setting it up.

VDT: Yes. The Harvard population program was really sort of a mess when I was there. And it's had a lot of problems since then; had a history of problems. A lot of brilliant people, but who aren't really interested in population.

VDT: Nathan Keyfitz came a couple of years later [in 1972].

LEE: Keyfitz is the great exception. He wasn't there when I was there.

VDT: I want to know more about your interest in mathematical demography. You mentioned that at one point in your undergraduate years you were majoring in math. But the math has always just been there, as a sort of native talent or interest?

LEE: Yes, a lot of it is native talent. I come from a large family of people for whom math seems to be a very natural mode of expression.

VDT: Your sisters included?

LEE: My sisters, my uncles, my aunts, parents. I have a sister who majored in math and a number of female cousins who majored in math; uncles who were math teachers. Math and philosophy seem to be a little family trade.

VDT: Isn't there a connection, two parts of the brain that work together or something?

LEE: Yes. Anyway, I always enjoyed math, enjoyed the puzzle-solving aspect of problem-solving. I always tried to be taking a math course of some sort or a statistics course through my college years, and when I was in Ethiopia in the Peace Corps, I took along math books and worked along in math on my own, evenings. When I was at graduate school in demography, I took a graduate math course in topology, of all things. So I sort of kept my hand in. It isn't that I have any deep knowledge of math, but I feel comfortable in it, have a pretty good mathematical intuition. So it's natural for me to formulate problems in mathematical terms.

VDT: Great. Now what about your year at INED? That was right after you got your Ph.D.

LEE: Yes. Well, I got married after I'd been at graduate school for about six or eight months, in 1968.

VDT: Did you meet your wife at Harvard?

LEE: Yes. She was a graduate student in Slavic languages and literatures, doing Serbo-Croatian and so forth, but she also had a strong interest in France; fluent in French, and had always dreamed of spending a year in Paris. And it sounded pretty nice to me, although I didn't know any French at all. So we started planning on spending a year in Paris after I finished my degree and while she was writing her dissertation. I applied for a Social Science Research Council postdoctoral fellowship. Nathan Keyfitz was somehow instrumental. He had a friend who worked for the SSRC and rather than having a sort of organized open system of applications for these, they seemed to work through contacts and one of the contacts was Keyfitz. He suggested that I apply and recommended me to the selection committee. At the same time, he recommended to Robert Retherford that he apply and he also got a postdoctoral fellowship of the SSRC and also applied to go to INED. So we both ended up spending a year at INED.

I busily tried to learn French that year before we went. I audited a class in intensive French for a semester and spent hours in a language lab listening to tapes and responding to questions in French, with the help of my wife. When we got to Paris, I hired four different French tutors. I'd spend an hour with one and then I'd go to the next for an hour. I did that for four hours a day for two weeks, just doing conversation.

VDT: Did you think you'd wear out one person if you went for four hours?

LEE: It seemed too strange to any of them to do more than an hour. After two weeks of that, I felt like I dared show my face at INED, so I did. I was officially under Louis Henry's jurisdiction.

VDT: Who must already have been aware of you. Did you send him a copy of your dissertation?

LEE: I believe I did, but I don't know whether he read it or not. I think he was unhappy to have me there. I had applied to Bourgeois-Pichat, the director, and I think he sort of imposed me on Louis Henry. Not that Henry had anything against me, but I think he didn't like the procedure or felt that it should have been his slot to award. I had most of my contact not with Louis Henry but with people like Georges Tapinos, Herve Le Bras, and Henry Leridon, all of whom now are well-established senior researchers at INED, but who then were--like I was--just sort of starting out and trying to make their way.

VDT: This must explain your interest in and connection with IUSSP, because these are all in IUSSP, and Tapinos, of course, is the current secretary-treasurer.

LEE: That's right. In fact, I've maintained fairly close contact with all three of those since then, particularly with Tapinos. We go back to France every once in a while and we see them at international meetings.

VDT: Did you set up your own research program in that year?

LEE: Yes. I really worked on extensions, polishing up, of material in my dissertation, primarily.

VDT: With more data that you could get at INED?

LEE: Yes, with more data. But mostly just with time to reflect and a very primitive computing system and an obscure new computing language to learn--PAFF, it was called, a paper tape system, not IBM cards. You punched holes in paper tape and that recorded your program and you ran it through.

Eventually, I decided that computer was just too primitive and someone at INED arranged for me to have access to a computer at a large medical research facility outside Paris and in exchange I did a series of lectures on spectral analysis to the research staff there and it worked out well.

One new project I worked on there was the research on forecasting post-transitional population, using statistical times series methods. I didn't submit an article on that until a couple of years later. That turned out to be quite an important article and that whole approach has become quite important. In fact, now the U.S. Census Bureau is using essentially that kind of method to do their fertility projections. That was a 1974 article in the Journal of the American Statistical Association called "Stochastic Renewal of Serially Correlated Fertility: Forecasting Births in Post-transition Populations."

VDT: I know of that one; I can't say I've read it. It was a fruitful year?

LEE: It was a very fruitful year, although much of the fruit didn't ripen for a couple of years. After INED I went to Michigan. I'd been hired there before I left Harvard, but asked for a leave of absence in my first year so I could go to Paris. I got to Michigan because a Michigan professor of economic history, Alex Eckstein, was spending a year at Harvard and he was aware of the fact that the Michigan economics department was looking for an economic demographer, because Paul Demeny, who had been there, had left for Hawaii to develop the East-West Population Institute. I believe Michigan had just gotten a training grant in economic demography from NIH, but they had no economic demographer. Of course, Eva Mueller and Deborah Freedman, who are indeed economic demographers, were there, but I think they didn't think of themselves as primarily economic demographers. Eckstein and I were attending an economic history seminar, quite a famous seminar run by Alexander Gerschenkron. At some point, Eckstein asked me to lunch and asked whether I might be interested in this position at Michigan and it sounded fine to me. That led to a job interview at Michigan and I got the job. So I ended up at Michigan with my wife. She decided she really didn't want to be a Serbo-Croatian folklorist and went to law school instead at the University of Michigan and we spent the next seven years there.

VDT: Is your wife a practicing lawyer?

LEE: No, she teaches law at Hastings College.

VDT: I had a question here: Did Michigan arouse your interest in less developed countries? But that had come some time before.

LEE: Well, yes.

VDT: I assume you weren't involved in the Taiwan project.

LEE: No, but you couldn't be there without having a lot of that rub off on you and the other . . . I guess that was the major international research project. David Goldberg was working on Turkey and Mexico, and I'm sure other people were working on other countries. Not as much as now; there wasn't such a wide volume of international work. I suppose the World Fertility Survey changed that. The work was more narrowly focused on countries that happened to have data.

VDT: Let's skip to your return to Berkeley. Nathan Keyfitz, in the interview he taped for this series in Jakarta, said the Graduate Group in Demography is the spiritual successor to the Department of Demography and that you're doing very good work here in economic demography and anthropology, under Eugene Hammel. Is it really necessary to have a separate department of demography or does the

Graduate Group serve the purpose sufficiently?

LEE: That's not quite the right question. I'd say that makes very little difference, between a department and a graduate group. You probably don't realize that the Graduate Group, which is what we are still, grants degrees. We admit students in demography per se and we award master's degrees and PhDs in demography per se. That's the real question: Should there be degree programs in demography or should it be done the way it is at Michigan and Wisconsin and most places, where you get a Ph.D. in sociology or economics, but you do a field in demography?

VDT: Is this graduate group the only one that gives these degrees in demography? Although my master's at Georgetown [1970] was in demography; the later ones were in sociology.

LEE: I thought Georgetown still gives degrees in demography.

VDT: Well, now they have a department of demography.

LEE: But they don't give PhDs. The Department of Population Sciences at Harvard gives PhDs [in population sciences]. There are very few who get PhDs, but Noreen Goldman came out of that program. It's a much more public health service type of program.

VDT: What question should I have asked?

LEE: What I said: Whether there should be degree-granting programs in demography, or whether this other model is better. I have mixed feelings. When I came here, I felt fairly strongly that it was not such a good idea to have degrees specifically in demography. So I had strong reservations about coming here, when that was the model. But now I'm not so sure. It's really a question of whether there is enough substance and theory to demography for it to be a field of its own, and I'm not sure one way or the other. I must say that teaching it now and seeing the development of our students, I feel more and more that it is sufficiently rich as a discipline to merit being a department or a degree-granting program--a field in its own right. But I'm still not quite sure. The issue is: Are all our theories really borrowed theories, from sociology and economics primarily, in which case I think there's a strong argument for saying you should get your degrees in economics, sociology, and just learn some demography along the way. Or is there enough of a theoretical corpus in demography itself that is home-grown?

If you look at Caldwell's theory, for example, I think it's fair to say that's a demography theory. It's not really sociology or anthropology or economics, it's a demographic theory. And stable population theory is really not much of a theory--it's a fairly straightforward mathematical model--but it has been twisted and mixed with other ingredients in so many complicated ways that I think now you could say there is a body of mathematical theory surrounding stable population theory, or models of demographic renewal in general, blended with theories of economic growth, intergenerational transfers, or that kind of thing, so there really is some demographic theory in that core. Yes, I think that's true in a number of places where the economic demographic models by themselves don't really make that much sense and the sociological models by themselves are sort of wishy-washy and vague, but when it's sort of all put together in the demographic context, it becomes much more viable; it's synthesizing these different approaches. I think there's really something there that merits separate attention. And I think there's enough in demography that it's difficult to cover sort of with your left hand while you're getting a full-time Ph.D. in some completely different subject with all of its own demands.

VDT: How is your Graduate Group in Demography program different from what you got as a student some 20 years ago?

LEE: That's an interesting question.

VDT: Which was a multi-disciplinary program. You mentioned public health, biology, as well as economics and sociology.

LEE: Yes. In terms of the kinds of courses the students take, I wouldn't say they're fundamentally different. What they include is different because the field has changed in 20 years. But the kind of course that Judith Blake taught, for example--she taught fertility--and mortality was another course. Those courses and their general conception could have been taken as models for the sorts of courses that we teach here and I try to teach, in that they were a blending of social theory with demographic data, demographic measurement issues, and what fresh insight she brought. They were very lively and broadly-informed courses. And I think that's the kind of thing we try to do here.

Then there were also methods-oriented courses. We are considerably more model-oriented and mathematically-oriented than the subject was in those days.

VDT: Part of the development and in part because of you?

LEE: Yes. Partly because of the specialization of Kenneth Wachter and of myself and Hammel. So our admissions requirements also are much tougher in the quantitative direction. I imagine the graduate students with interests in social science are probably generally quantitatively stronger these days than they were in those days. But the general spirit of the training, I don't think, is any different now. It was certainly not an insular program in those days; it was interdisciplinary, it was rigorous.

VDT: You have more or less an updated version of the original Group in Demography. Then there was the Department of Demography in between. Is it important to have a department with that actual name?

LEE: It's interesting that you ask, because Gene Hammel and Ken Wachter and I had lunch just four days ago with the dean of Literature and Science and discussed this question, among others. It seemed to be the leading question: Should we be a department?

I think the truth is that that makes very little difference. It may make some difference to the outside world, which is what I suggested to the dean, when it comes to recruiting students or whatever. It's a model that looks more solid if it's called a department. But in terms of the reality of the situation, it would hardly change anything. We have our own full-time tenured appointments in demography; we admit our own graduate students; we give our own degrees. What would be different if we were a department?

VDT: Is there something about the funding that makes a difference if you're a department of the university?

LEE: Well, it can, but in our case it doesn't. We have a quite good budget that is our own budget. Ordinarily, a graduate group would not have its own faculty appointments; we would all be borrowed faculty from other departments. But in our case, we do have our own FTEs. We just got another one.

VDT: What's that?

LEE: Full-Time Equivalent--that's the unit of currency for faculty positions at universities.

VDT: How is it that the graduate demographic group at Berkeley has all these special privileges that maybe some groups at other universities do not have?

LEE: Well, originally it was Judith Blake who persuaded the administration to set it up in that way. Of course, then it [the department of demography] was liquidated, in the 1970s [1972]. I think it was really the administrative genius of Gene Hammel who got the thing set up again and fought for resources.

VDT: When was that?

LEE: It was happening during the second half of the 1970s. I was first offered a job here maybe in 1976 or 1977; there must have been a good deal of work done before that. Hammel chaired a university-wide committee on demography that was in charge of setting it up.

VDT: But you came in as the first director; it really only started again when you came back?

LEE: I was offered the position of chair and declined, because I didn't want to take on the administrative burden. I wasn't sure I wanted to come into a program that was just starting up when I was so comfortably established at Michigan and I was very well funded and so on. So I turned down the first offer. Then a couple of years later, Hammel called me up again and asked whether I would accept an offer that was separated from the administrative work and come as a professor. So after a while, soul-searching, I decided I would. So I came in the fall of 1979, as professor of demography and economics, within the Graduate Group in Demography. Gene Hammel is the director.

VDT: And you still have to decide whether a separate department is necessary?

LEE: Well, it's an important and controversial question. [*Note: the next year, in 1990, the Graduate Group in Demography was once again organized into the Department of Demography.*]

VDT: But obviously this group is flourishing. Who have been some of the leading influences on your career? You've mentioned the people who were here at the time you were studying at Berkeley.

LEE: Yes. I would say Nathan Keyfitz, certainly. It was sort of a spirit of intellectual excitement. That was very important when I was a student and throughout earlier stages of my career. Ansley Coale was also very important. I was never a student of Ansley Coale's; I was never at institutions where he was. I sent him a copy of my dissertation and he became familiar with it early on and liked it. I guess I corresponded with him. I'd see him at meetings and somehow he was a guiding light. Again, I think it was his enthusiasm, encouragement, appreciation, because that work I'd done at Harvard was pretty much done in a vacuum so far as any kind of demographic advice was concerned. So Keyfitz and Coale were very important. Then once I'd gotten to Michigan, Ronald Freedman was very important, just as a source of advice and, again, encouragement.

VDT: Though the two of you don't do the same kind of demography.

LEE: No, that's true. There wasn't anybody there doing--what shall I say?--mathematical demography, the kind of modeling that I was interested in. But I found him an important influence.

VDT: What about Easterlin?

LEE: Yes, Easterlin too, certainly--particularly his writings. When I was writing my dissertation and came across his work, that was very important to me. It was also very disappointing, because I thought I'd invented something new and I realized that not only it was done, it was published, and it had been done 15 years before. But I enjoyed many discussions with Easterlin; found his work very interesting and still find his work very interesting. He was another participant at this seminar that Chuck Tilly organized at the institute at Princeton, so I got to know him that summer [1972] as well. There are four people who were important.

VDT: And who have been some of your leading students?--so far, because you've got a long way to go. Keyfitz cited you as one of his leading students.

LEE: Well, I'm glad to hear that.

VDT: Students you're proud of.

LEE: Recently, Andy Foster, who just went off to the University of Pennsylvania, and David Lam, who's at Michigan and essentially replaced me at Michigan in economic demography.

VDT: He stepped right into your place, though he was just finishing?

LEE: Five years after I left. Also Mark Montgomery; he was at Princeton for five or six years. He's now abroad someplace and is going to Stonybrook. Andy Mason, at the East-West Center. Barbara Devaney--she was at Duke for a time; I'm not sure where she is now [Mathematica Policy Research, Princeton, New Jersey].

VDT: All these people did mathematically-oriented dissertations under your aegis?

LEE: Quite mathematically-oriented, yes.

VDT: They had to. Now let's talk about your work on the two NAS [National Academy of Sciences] panels. First, on fertility determinants. How did you get onto that panel? I wouldn't have thought of you as a fertility determinants person. There were your long projections?

LEE: Yes. I had worked on the problem of predicting fertility in developed countries. I'd worked on the Easterlin hypothesis; I'd written quite a number of papers on that besides the one in PDR. I've worked on fluctuations in marital fertility in relation to fluctuations in nuptiality and other short-run fluctuations generally and their relation to economic and climate changes--those kinds of things. But fertility . . . Well, actually I've done more substantive work on U.S. fertility as well. I did a paper on target fertility.

VDT: I just read that last night ["Aiming at a Moving Target: Period Fertility and Changing Reproductive Goals," Population Studies, 1980]. I couldn't put my latest Population Studies on the shelf and decided to throw out the whole of 1980, but first looked, and there was your paper.

LEE: "Aiming at a moving target"--that's one of my favorite papers. But there was a predecessor, published in Demography in 1977, on "Target Fertility, Contraception and Aggregate Rates" [1977]. So I've done quite a number of papers in the area of fertility, mostly the technical sort and mostly for

developed countries.

How I got on the panel is difficult for me to say. My guess is that Ronald Freedman and Richard Easterlin, who were on the panel, suggested me, not because I had any special knowledge of fertility in developing countries but because they thought I'd just have generally good demographic skills, bringing another dimension--perhaps for economic demography. Well, Bob Willis was on the committee also. Also, I taught economic demography for many years and in teaching you have to teach everything, which means you have to learn everything, so I'd done quite a bit of reading on fertility in developing countries.

In the end, I was made chair of the working group to summarize our state of knowledge of fertility and its changes in developing countries. I think that came about because at one stage we were all asked to write little summaries on some aspect of the problem. I was assigned to write up something on income distribution and fertility change. Many people sort of ignored this assignment, but being more junior than most of them, I took it very seriously. I think the powers in the committee [on Population and Demography], chaired by Parker Mauldin, must have read this little piece and decided it looked good and that that sort of synthetic and critical skill was just what they wanted for this project of summarizing the state of knowledge.

The working group was Randy Bulatao, John Bongaarts, Paula Hollerbach and myself. We met here at Berkeley and drew up an outline of what we thought were important areas of the field. At some early stage, there was a decision to use basically the Easterlin framework. [This is the Easterlin or Easterlin-Crimmins "synthesis framework of fertility determinants," first set out by Easterlin in "The Economics and Sociology of Fertility," in Charles Tilly, ed., Historical Studies of Changing Fertility, 1978, and described by Easterlin in Vol. 2, Chap. 15, of the NAS report, edited by Bulatao and Lee, Determinants of Fertility in Developing Countries, 1983.] I think it was a good decision and we probably would have done it in any event, but in fact, a separate working group of the committee had looked into the question of the theoretical framework and Ronald Freedman had his framework and Richard Easterlin has his. They were both in this group to hammer out what framework we should use. Ronald Freedman started out by saying he didn't think it made much difference what framework we used and he was perfectly willing to have the committee use the Easterlin framework, which he thought was a convenient one. So that framework was adopted early in the work plan of the committee and it was natural for us as a working group to adopt it.

I think there was a lot of misunderstanding--and still is--about that framework, which I view as nothing more than a framework, that is, without theoretical content, without bringing in any assumptions. People continually believe that it's an economic theory of fertility, which it's not. They think the terms "supply and demand" are economic terms, which they're not in this context. In fact, any sociological or psychological or economic theory can drive that framework. It really categorizes influences into things that work through the culture and sociology and biology of reproduction on the supply side or influence how many children people want, on the demand side. But the framework doesn't say in what way the demand for children is determined.

VDT: That framework got a scathing review by Paul Schultz in Population and Development Review [PDR, March 1986, review of Richard Easterlin and Eileen Crimmins, The Fertility Revolution: A Supply-Demand Analysis, 1985, which Schultz describes as seeking "to bring empirical content to the Easterlin synthesis framework"].

LEE: My feeling is that that was a very small-minded review, that, in fact, Schultz and Rosenzweig had pretty much swallowed whole Easterlin's framework and adapted it to their own purposes and used it in a very nice article in American Economic Review, but essentially a derivative article from Easterlin's framework. So I view that review as nitpicking with details of the way it's done, but it's essentially accepting the broad outlines that Easterlin sketched.

VDT: Did your work on the two NAS working groups--fertility determinants and population growth and economic development--overlap?

LEE: No, we'd finished the fertility determinants work. I was off the working group and had nothing more to do with the National Academy of Sciences for a few months.

VDT: Then you were appointed to be co-chair [with Gale Johnson] of the working group for that report. Sam Preston said he felt an economist should head it, so he turned that down. And he said that the work you had been doing meanwhile on the IUSSP committee was very influential background to the report, Population Growth and Economic Development: Policy Questions [by Samuel Preston, Ronald Lee, and Geoffrey Greene, National Research Council, Working Group on Population Growth and Economic Development, Committee on Population of the National Academy of Sciences, 1986]. Almost none of the research, background papers, were ready at the time he--who did the first draft of the report--was writing, so the work of your IUSSP committee was most useful.

LEE: I was deeply involved with the IUSSP. I had been asked, around 1981 or 82, to chair what was essentially the economic demography research committee of the IUSSP, which I was very honored to do. We were supposed to look at macro consequences of population change, and I, together with Georges Tapinos, assembled a strong committee. There were Brian Arthur, Allen Kelley, T.N. Srinivasan, the development economist at Yale, Gerry Rodgers, English development economist working for the ILO, and a Brazilian.

We planned two conferences, which have now resulted in two volumes. One was Economics of Changing Age Distributions in Developed Countries [1988] and that wasn't really relevant to this topic. But the other one was on Population, Food, and Rural Development [1988] just explicitly on the Third World and that was very relevant for the work of the NAS panel. Putting together these conferences and editing the volumes took a great deal of time. They have both now appeared in the IUSSP Oxford University Press series. And with the IUSSP approach, which I think is very nice, these are very international. You look for scholars from all over the world who are doing interesting work in these problems.

The IUSSP conference on population and development was held in December 1984 and it was ahead of the NAS schedule. Although the book actually came out a couple of years later [only in 1988], most of the papers were available at the time of the National Academy committee's work. The ones that were most useful--well, I'm not sure which ones Sam had in mind.

VDT: He said there was one on income distribution by David Lam that was finished.

LEE: That was one of the National Academy background papers.

VDT: Oh, yes--the only one finished in time for Sam's first draft of the report.

LEE: T.N. Srinivasan had a paper on food and population in the IUSSP conference and then we borrowed it for the NAS. Pingali and Biswanger were two interesting people; they also did a paper subsequently for NAS. Their paper was sort of testing and exploring empirically the Ester Boserup ideas about induced technological change, particularly in sub-Saharan African agriculture. Then there was a very interesting paper by Robert Evenson of Yale, again looking at Boserup-type outcomes of population change in India. We were fortunate to be able to sort of pick off research by Indian economists that had been going on for quite a number of years.

VDT: And all of these were available and sort of fed into your NAS report?

LEE: Yes.

VDT: Let's jump into the flap over the report. As I said to Sam, one can read what one wants to into that report. You concluded that: "On balance, we reach the qualitative conclusion that slower population growth would be beneficial to economic development for most developing countries." Julian Simon felt that you were defaulting to the other camp, those who believe that population growth is still important. In the symposium review of the report in Population and Development Review [September 1986], he criticized you in particular, saying you had been skeptical that rapid population growth impedes economic development, or at least per capita income growth, in your review [PDR, March 1985] of the World Bank's 1984 World Development Report and also in your IUSSP newsletter article of 1983. In other words, he felt that you really had great reservations that rapid population growth was all that important in economic development. That's the way I interpret what he was saying there about you and your past writings.

LEE: Yes, well, Julian Simon being upset doesn't make a big flap. The flap was more on the other side.

VDT: Right, okay. Well, what did you feel about your qualified conclusion, for which you have been criticized?

LEE: I think it's a very good report. I certainly stand by it. My impression of it at the time we were putting it together, writing it up, or as I read drafts of the policy conclusions was that it was very bland and guarded, middle-of-the-road views that we were espousing there. And I still think that's the case. There are really dreadful sentences, like: "Investment in family planning is justified provided the benefits are greater than the costs." Sentences like that don't just happen; they're put in there because someone wants them.

VDT: A committee.

LEE: You have to get something through a committee. So I was very surprised and taken aback by the reaction of people like Paul Demeny to the report. I think basically they're wrong and arguing for a very parochial point of view, and perhaps have not read it carefully. Demeny complained about citations, such and such was not cited, but that report is really written to go with the background papers and many of the citations in it are to the background papers [published separately, in 1987]. The background papers are reviews of the literature. So I would say the citations in the report, although they were ample, were far from exhaustive and far from as many as there would have been had there not been this set of background papers. I think demographers are a rather self-selected group of people who believe this is a terribly important problem. I think that is a good part of why some demographers were upset.

VDT: You had upset the orthodoxy. I guess you've read Dennis Hodgson's just-published article on orthodoxy being overthrown by revisionism ["Orthodoxy and Revisionism in American Demography," Population and Development Review, December 1988].

LEE: Yes. Well, it also seems to me that the National Academy report was not very different from the World Bank's World Development Report of 1984, the paper by Timothy King on population and development, the paper by Geoff McNicoll in PDR on population and development, or, most recently,

the review of the issue by Allen Kelley in the Journal of Economic Literature. It seems to me that there is a very broad consensus on these issues among people who are studying population and development.

VDT: It just wasn't as alarmist as some of the old guard felt. And, of course, it came after the turnaround in U.S. policy at the 1984 Mexico City population conference, and a lot of people were alarmed by that.

LEE: When you release this sort of thing, you could write it with an eye to how it will be used. I'm thinking of stories like one that somebody, I think maybe George Zeidenstein [Population Council president], told me of having been in Kenya and having a discussion with the Minister of Planning, who pulled the National Academy report off the shelf and said, "See, we don't need to be worried about family planning here."

But I think when you write a report like that what you're supposed to do is evaluate the scientific evidence and describe what you think is known and what isn't known and not slant things in order to affect policy in one way or another. I think you have to just trust that the knowledge that you summarize as well as you can will be appropriately used. So I don't think it's right to say, "Well, if you write a report like that, then funding will be cut for family planning." That's some other sphere of activity. The point of a scientific report of a group like the National Academy is not advocacy.

VDT: But wasn't AID money behind it?

LEE: It was money from AID, from Hewlett, and from Rockefeller, I believe. But a point of the National Academy structure is to insulate the activities from the interests of the funders.

VDT: And you came out with something so different from the previous report of the National Academy of Sciences on Rapid Population Growth [Consequences and Policy Implications] of the early 1970s [1971]. Well, it was 15 years down the road. Now you're working on externalities to childbearing, which you made a big point of in that report--more should be known about the externalities of childbearing.

LEE: That's right. That's one of the things I'm working on; I'm very interested in it. It seems to me there are probably many consequences of population growth that simply don't get reflected in standard economic measures. Virtually all the environmental effects, for example, don't. They will be eventually, if the environment turns out to have a strong effect on agricultural yields, things of that sort. There may be quite a number of ways in which population change can affect our lives without affecting what is conventionally viewed as economic developmental type variables. So that's part of the interest here, to look beyond the kinds of things that were in the NAS report.

I was very frustrated as we were working on that report because so little seemed to be known about this subject. But at the same time many people thought it was centrally important and we commissioned a paper from Bob Willis on this topic for the volume and I think it was a very good paper, but it was a narrow, sort of mathematical, theoretical paper, and it seemed to me that there was quite a bit that could be said relatively easily, without needing a lot more theory than we already have.

VDT: You mean the social costs of childbearing?

LEE: Yes, something of that sort. I was asked by the United Nations and INED, who were planning a joint conference, to do a paper for them on my work on demographic history and I accepted. But a few months later I began to think, "I'm so bored with that topic and I'd like to try a paper on this

externalities issue; maybe I'll call them up and see if I can change my topic," and they were agreeable. So I started working on this externalities paper. I presented it to the UN last August [1988].

VDT: The conference Paul Demeny was involved in?

LEE: Yes, he was at the conference also. I've presented it maybe six or eight times now, including at the PAA [meeting, spring 1989], and it always seems to generate quite a lot of lively discussion.

VDT: Indeed. You pointed out in your PAA paper ["Externalities in Childbearing in Developing Countries: The Case of India"] that some costs are just not there [thought of]. What was the one that cost the most--something like \$12,000?

LEE: That, in the case of India, was the fact that when you have another child, you're diluting everyone else's birthright, everyone else's per capita share of the value of publicly-owned mineral resources, which are dominated by coal in the case of India, and that is some number of thousands of dollars per head.

VDT: I hope you will go on with that. What are your recollections of Bob Lapham, who was staff director of both NAS projects you worked on? Alas, I didn't get to interview him [died February 20, 1988]; PAA secretary-treasurers are being interviewed for this series, too. I'd like to get his daughter Susan's lovely accolade to her father that she read at the PAA meeting [1989] when they presented the first Robert Lapham award. He must have been a dynamo. He overlapped his work for the NAS Committee on Population and Demography with directorship of the Demographic and Health Surveys. And he was your PAA secretary-treasurer too [held office 1984-87].

LEE: That's right, so I had a lot of interaction with him. I was very saddened by his sickness and his death. He was very serious, very hard-working, and had an enormous amount of integrity, in particular in the National Academy context, which is where I had the most contact with him. He was always particularly careful to insulate us from the views and feelings of our financial sponsors. He was always making sure that we didn't feel undue pressures--do that--taking pains to let the work of the committee move forward according to its own internal logic. Very well organized; things always went very smoothly. A pleasure to work with.

VDT: Good. Now, which of your publications do you consider most important and why? You've had a lot; you've already mentioned several, but mention them again.

LEE: How many can I mention? I think, of all my work, probably the work that's closest to my heart is the historical work on England. And I think the first article I ever published, which was in 1973, is one of the ones I'm most pleased with, if not the most important. It was called "Population in Preindustrial England: An Econometric Investigation" [Quarterly Journal of Economics, 1973].

VDT: The one where you projected backwards?

LEE: No, that was another one. The 1973 paper was about the causes and consequences of population change in preindustrial England: the effects of population growth on wages, the effects of real wages on population growth rates, and the influence which I thought--and I still think it's very important--of long-run changes in mortality that were essentially independent of the economy but caused these population swings, which then had repercussions throughout the society and economy. It was a sort of empirical investigation of Malthusian kinds of ideas, with some additional theoretical

ideas. I've written probably ten papers along that line, but I think that one was probably the most important.

The forecasting paper I mentioned earlier I think is quite important ["Forecasting Births in Post-transition Populations," Journal of the American Statistical Association, 1974]. Also the inverse projection paper, the methodological one; that was the method for inferring vital rates from baptisms and burials ["Estimating Series of Vital Rates and Age Structures from Baptisms and Burials," Population Studies, 1974]. There was a follow-up in 1981, but that's derivative; the more important one is the first one.

Then there was the "Aiming at a Moving Target" paper ["Aiming at a Moving Target: Period Fertility and Changing Reproductive Goals," Population Studies, 1980]. That I think is quite important; I'm about to do another version of that.

I think--of course, I can't be sure now--but I think this paper on externalities is going to be important. I like it a lot and want to do more on that. Then I've done quite a bit of work now on intergenerational transfers. That's sort of an integration of stable population theory with some economic models of growth: how age groups that don't produce are able to consume, and so on. I've done four papers in that area and I think one of those, which is not yet published--it's under review now--is probably the most important of that bunch. It's work generally on demographic change in relation to economic well-being in certain developed countries. The most recent paper in that line of research is on mortality decline and aging--what it costs. I concluded costs as about 1 percent per year of our consumption for each additional year of life expectancy.

VDT: Are you talking about national averages?

LEE: Yes. The life expectancy of 76 rather than 75, or 71 instead of 70, means we'll all live longer, we'll consume more in total over our life cycle, but we'll have to consume about 1 percent less each year in order to fund the increased year.

VDT: That's feeding right into what Kingsley Davis has been saying about the high costs of our non-productive elderlies.

LEE: Oh, absolutely. At this point, for each year we gain in life expectancy--we can think of that as a gain in person years lived--two thirds of that gain is gained after the age of 65 and the remaining one third is divided between our working ages and childhood. But as life expectancy increases, we move closer and closer to having 100 percent of the gain occurring in the retirement years, and that takes an ever-higher toll in terms of what our consumption has to be. We have to provide for those years either through savings or through pay-as-you-go pension systems or through transfers in the family. But it all comes to the same thing in the end. It doesn't make any difference in some sense which of those you do; it still costs what it costs, and what it costs is about 1 percent . . .

VDT: Who are you aiming that at, other than your fellow professional demographers? Do you think it important to speak to policymakers?

LEE: I think the real message--what I learned when I wrote that paper was that we should not complain about rising payroll taxes and such things, that in developed countries in a low-mortality setting essentially, it's just a matter of having to pay for our own retirement. We're really paying for ourselves at a later stage in our life cycle. It isn't that we're paying for a lot of other people's children or because other people aren't having children. It's just that we're living longer and that has to be provided for and that's the way you do it. We should be glad we're called on to do it, because if we weren't, it would be because we were dying younger.

VDT: A great message. Do you think it important to get such messages out to the general public?

LEE: Yes. I would like to do an op-ed piece or something like that at some point.

VDT: Have you done that?

LEE: No, I have never done that. But I think this is a simple idea, which I didn't really appreciate before. And it's in contrast to the situation in developing countries. In a high-mortality setting when mortality declines, the person years you gain are mostly in the productive ages and consumption can go up, from that point of view. But what also happens is that declining mortality means the population starts growing a lot faster, gets younger, and you've got more dependents. Although you've got more productive years in a life-cycle sense, at any instant there are more dependents, more children, in the population and you end up having to consume less. Now in that situation, you can feel aggrieved, you can feel, "I have to pay higher taxes just to take care of this growing number of young dependents." It's a quite different situation. You're not having to put by money to provide for your own retirement, which is completely minimal--with very high mortality there's only one person year of life expectancy after age 65. In that situation you're much more a victim of societal demographic trends. But in the developed low-mortality setting, you're basically just providing for yourself. I think if people realized that it would make the payroll tax a lot less painful, and the increases we're going to have going into the 21st century.

VDT: I hope you will write that op-ed piece, at least. That leads into my next question: What accomplishments in your career so far have given you the most satisfaction? In a sense you've answered that with your publications, your development of your several influential models.

LEE: Well, yes. I think I've talked about what in my research has been most satisfying. In terms of recognition, the single most satisfying thing was the Mindel Sheps Award [for Mathematical Demography, PAA, 1984].

I'll tell you, I would not rank my work for the National Academy of Sciences high in terms of being satisfying. Extremely time-consuming, though it had its interesting points. Of course, I find the reception by some demographers of the work on population growth and economic development discouraging--the NAS report. I put in a lot of work; I think it's a good report. I think these people are largely wrong and have not read it carefully or have not read the background papers. I don't know what its impact will turn out to have been. Of course, I will be unhappy if its impact is that it has led to reduced funding and so on. As I said, I don't think that should have been, and wasn't, our concern as we were writing. We shouldn't be writing with an eye to its uses. So I'm not thrilled with that.

I'm not thrilled with the results of the fertility work [Determinants of Fertility in Developing Countries, 1983]. Not because that was controversial or anything, but because I think it didn't come out with one crystalized, clear insight. It's sort of a review of an enormous amount of research and it doesn't try to tell you, "Here's truth," which means from an intellectual point of view, it's not terribly satisfying.

I must say that in the last few years I've come to appreciate the teaching more and more. I don't think there's any feeling quite like the feeling that can--when things are going well--develop in a classroom where there's sort of a shared understanding, a shared intellectual effort to reach the truth. I've seen students get excited and try to do their own work; seeing graduate students enter and then maybe four, five, or six years later leave as professionals, publishing interesting work. That sort of thing; that's a very good feeling. So, as opposed to my earlier days, I'd rank teaching right up there with everything else, I think, among the most satisfying of the activities. The teaching and research

have been the most gratifying.

And of the--I don't know what to call the NAS work; it's not really research, it's not administration, it's something in between. Well, of course, it had its satisfactions. The IUSSP work had its satisfactions. The other organization I've been quite involved with, aside from NAS and IUSSP and PAA, is the NIH [National Institutes of Health] and their grant review panel. I'm just now finishing four years--I'm now chair of the Social Science and Population Study Section. That's a major piece of work also.

VDT: Reviewing all the proposals?

LEE: Yes. We meet three times a year and each time it ends up taking two or three weeks of work.

VDT: You go to Washington for it?

LEE: Yes, I go to Washington, but the hard work you do before you go. You write up these long critiques, five or ten typewritten pages for each proposal you have to review and go to Washington and read them off.

VDT: And do you find it discouraging when so few of the approved ones get funded?

LEE: Yes and no. Every time I think of how expensive research is and how many households in the United States are paying taxes to support some demographic research project, then I think we shouldn't give nearly as much; we want smaller projects and to fund only really the most crucial. At other times, I read a proposal and think this is marvelous and I'd like to see it funded and then it doesn't get funded and that's very discouraging. But all we can do is rank them and then which get funded and which don't, how far down that list they go, has nothing to do with what we say about them--that's a matter of how much money there is.

VDT: I understand that of those that are approved . . .

LEE: About 20 percent get funded.

VDT: I understand only about 16 percent are getting funded; it's way down.

LEE: Well, about 99 percent get approved.

VDT: Oh, I didn't realize that.

LEE: Almost all get approved. But then we give them a score, between 1 and 5, or 100 and 500--100 being the best. And based on that, a ranking is established, and based on how much money is available, they just go down that list until they run out of money. It's not just our list, but we are in with other lists.

VDT: What about your own center when it submits a proposal?

LEE: We're reviewing what are called ROIs primarily. Those are individual research project proposals, not center grants, not training grants. But when I submit a proposal that means they have to appoint a special study section to evaluate it; it's not evaluated by the same people at all.

VDT: So you don't find that work terribly satisfying?

LEE: It is satisfying, and I think it is very well done; I've been very impressed by those review procedures. But it's extremely time-consuming; it takes a big bite out of the time available for research. Research comes out of whatever is left after you do everything else you have to do, and something like that will make a big difference. And I know that my career, having been simultaneously involved with all four of these organizations at the same time, has been seriously affected.

VDT: NAS, IUSSP, NIH--what's the fourth?

LEE: PAA.

VDT: Good heavens, yes! Well, you have had a very full plate for a while. My last question and then a little on PAA. This we've also touched on: What do you see as leading issues in demography over the years you've been involved? You got into it sort of by the back door--historical demography. You weren't so concerned with the baby boom and bust, perhaps, until you discovered that Richard Easterlin had already stolen your ideas.

LEE: Certainly not stolen my ideas, but working on it very nicely long before it had occurred to me.

VDT: What do you think have been the leading issues? I think of you as somewhat removed from the hot spots, other than that of population growth in less developed countries.

LEE: Yes, I think my work has always been on topics other than those most people were working on.

VDT: It's a nice position to be in?

LEE: Sometimes yes; sometimes no. It means that getting people to read my work is often difficult. Getting people to review my work for journals is sometimes difficult. I had an article that took a year and a half before it got accepted--that is, that long before I got any kind of response from the journal I sent it to.

VDT: Because they try to get a reviewer and they can't think of anyone?

LEE: Yes. They send it to someone who says, "I don't know what this is about; send it to somebody else," and so forth. When you're working in a well-established school of thought you can cite people who've made this and that assumption and who've tackled the problem this way or that. So you don't have to justify every step in the same way, because much is taken for granted. Those first steps, the sort of assumptions that are made, are made by the first few articles in an area of research and then after that research goes on without questioning. So I think that has been a difficult aspect.

But on the whole, yes, I like being in that position of doing sort of off-beat, unconventional work. I think nothing pleases me more than something that feels really different. What was your question again?

VDT: What do you see as leading issues in demography over the years you've been involved; you were not so immersed in the leading issues.

LEE: Leading issues--certainly, the demographic transition and then as it is applied to developing

countries and the fertility decline in developing countries and appropriate policies. That is one set of leading issues and through the National Academy work I was drawn into that. And the consequences of population growth, into which I was drawn both through the IUSSP and the National Academy, is another leading issue, but one in which surprisingly little research is done, compared to fertility, contraceptive behavior, and marriage, where a great deal of research is done.

I think now the issue of intergenerational transfers and tensions and aging population and so on is quite a hot topic. Again, there aren't that many people working on the macro issues, and that's something I've gotten quite interested in.

VDT: Do you mean by intergenerational transfers something on the "sandwich" generation too--the ones caught between their own parents and children?

LEE: Sure, absolutely. In any case, I think much of that problem is overblown, because so much of the greater costs of the aging population is offset by the lower costs of children.

VDT: That's interesting; some people don't remind you of that.

LEE: These days the costs of supporting the elderly population are primarily borne by the public sector and not only by the public sector, by the central government. The costs of children are borne mainly by the private sector; within the public sector, they're borne at the state and local level--different sources of revenue, in any event. But primarily they're borne within the family, the private expenditures. So people are unlikely to think, "My payroll tax has gone up, but then I only have one child to put through college instead of three." But, in fact, there's a considerable offset. The numbers I gave earlier are what I would say are the costs of aging. That is, if we stay at zero population growth but have higher and higher life expectancy, each additional year of life expectancy costs us about 1 percent a year of consumption. What are we consuming now per person per year? Probably \$12,000, something like that. So a percent of that is about \$120 a year; it's not much.

VDT: Let's jump into PAA. Can you remember the first meeting you attended? Here's the wonderful list that Andy Lunde put together, started it. Not that you have to think that far back; some people have to be reminded of meetings 40 or 50 years back. Was it when you were at Berkeley the first time?

LEE: Heavens, no. I didn't go at Berkeley; I didn't go when I was a graduate student at Harvard.

VDT: Good heavens!

LEE: I didn't go when I was at INED. If I had stayed at Berkeley, at the master's level, I would have been drawn into PAA much earlier. As it was, I left Berkeley and went to Harvard where there was really no sort of socialization in population. I didn't even know what the PAA was.

VDT: It must be in part because you were on the West coast and most meetings were . . .

LEE: That's right, and then I went to Harvard where it never occurred to me. I didn't know any faculty who ever went to meetings like that. I remember when I went to Michigan for the job interview, in 1970, Beverly Duncan, who was then the editor of Demography, sort of chastising me for the fact that I didn't subscribe to it and probably didn't read it and I didn't belong to PAA.

Then the spring after I got to Michigan, I wasn't planning to go to the PAA, which was in Toronto [1972], and I noticed that as the week to the PAA was coming up, suddenly graduate students

and faculty started disappearing from the Population Studies Center. I didn't know what was going on, but somehow I got the sense this was something I was supposed to do. So at the last minute, I found out whether my way would be paid and, yes, it would, and I got reservations. I couldn't get reservations at the hotel where the PAA was.

VDT: That's a good thing, because that was an embarrassing hotel; it turned PAA women away from the bar. That was an old Canadian custom; there was a ladies bar which you could go into with male escorts, but unescorted women couldn't go into the main bar. That led to the business of PAA resolving never again to hold a meeting in a hotel that had such discrimination.

LEE: We're about to go back to Toronto [1990 meeting].

VDT: We're going to the Royal York Hotel. The King Edward Hotel where the 1972 meeting was held was shabby during my growing-up days in Toronto and it was still shabby then; I was embarrassed. The Royal York was once touted as the largest in the British Empire and it's an old dowager, but I hear it's been fixed up.

LEE: I liked Toronto very much; it's a lovely place.

VDT: So you don't have memories of the barriers, okay. What do you remember about the meeting?

LEE: I got into this hotel that was about 14 blocks away; it was a long walk. The talks I remember best were Tom Espenshade's on the cost of children . . . In those days, I don't think this happens much anymore, but then the audience would often applaud, occasionally applaud, and sometimes stand up and applaud if they really liked the paper.

VDT: I never saw that!

LEE: Yes. They stood up and applauded Tom Espenshade's talk on the cost of children. The other talk I remember that was very well received was one by Janet Salaff.

VDT: On Singapore.

LEE: I think maybe it was. She was interested in Chinese demography but, of course, Chinese demography couldn't really be on China then, because there wasn't any data. So I was struck by that and also that she gave two papers at the same meeting and both of them were applauded.

And I remember Ansley Coale being awarded--I think it was at that meeting--the first Mindel Sheps Award [in Mathematical Demography], which was presented by Keyfitz, who was chair of the committee. So that established in my mind forever the value of the award, the magnitude of the honor. [The Mindel Sheps Award was first awarded--to Coale--at the 1974 meeting and presented by Jacob Siegel, chair of the first award committee.] The presidential address, it must have been at a banquet.

VDT: In 1972 it was Amos Hawley.

LEE: I was a fan of Amos Hawley--I mean, I'd just read a couple of his articles, but those worked well. The truth is I can't remember a word of his address ["Population Density and the City"].

VDT: Norm Ryder was the next year [1973].

LEE: The Norm Ryder talk I remember.

VDT: He criticized his own National Fertility Study ["A Critique of the National Fertility Study"].

LEE: Yes--so I remember that. That year, I actually gave a talk at PAA. In fact, it was in the session organized by Julian Simon and it was on my historical work. That was in New Orleans, wasn't it?

VDT: Yes, 1973 was New Orleans.

LEE: Then since that, I haven't missed one. But that means I've only been to 18 or something like that.

VDT: How did you have such a meteoric rise from 1972 to become president just 15 years later, in 1987? I bet that's a record, practically.

LEE: You mean in terms of length of time of attending these meetings?

VET: In the sense of being a part of the PAA crowd, a member. You obviously moved up the ranks very rapidly. What do you suppose accounts for that?

LEE: I don't know. Ask somebody else. [Laughter] I don't know how these things work.

VDT: How do you think people become president?

LEE: Let me think about it a moment more seriously. I remember feeling, when I was at Michigan, that no one was paying any attention to me and no one ever read or cited my research, and I felt annoyed--for a number of years. I'd see other people going off and doing things on committees and so on. Then I do think I was beginning to get better known while I was at Michigan. But I had the most minimal involvement or no involvement in national committees and in PAA; I didn't do any of this stuff. Somehow it all happened at once after I had come to Berkeley.

I think the first. . . well, I got asked to be on the NAS fertility committee. Organizing these things you spend an incredible amount of time on the telephone, calling people up and asking them for papers and so on, so people get to know who you are. Then there was the IUSSP meeting in Manila in 1981. I had never had anything to do with the IUSSP. I was interested in the IUSSP. I'd have liked to be going to their meetings and be invited to seminars, but I never heard anything from them. Then suddenly for this meeting in Manila, I was asked to do a plenary address, of which there were only two. So I really worked hard on a paper and did a plenary address.

VDT: What was that on?

LEE: Well, they assigned a topic, which was: "From Rome to Manila: How Demography Has Changed in 30 years." [Rome was the site of the 1953 IUSSP meeting.] So I was cast in this role of an elder statesman or something, although of course I wasn't. As I say, I took this very seriously and did what I think was an interesting paper.

VDT: I must read that. Nobody that I'm close to went to Manila. It was so far away and it was just before Christmas.

LEE: Yes, they often are. After that, for many years my Christmas was messed up by IUSSP

committee meetings; they always seemed to be just before Christmas.

VDT: When it's cool in less developed countries.

LEE: Yes. And that led also to my then being asked to chair this IUSSP committee, which again involved innumerable phone calls, trying to gather information and put together programs for this conference and so on--just an incredible amount of work. The NAS and the IUSSP and all of these things gave me more visibility, I guess.

VDT: It must have, because it was indeed quite a rapid rise--in PAA. I guess you weren't too involved in PAA at the time the Concerned Demographers were active.

LEE: There was a bit of that going on when I first went to meetings. I was asked to chair the Mindel Sheps Award committee pretty early on. I did that for several cycles, so for a while every other year, I was up there presenting a Mindel Sheps Award to somebody.

VDT: You became known as Mr. Mathematical Demographer.

LEE: Those are the only things I can think of. It surprised me, and also, unlike most people, I never served as vice-president. I went directly from being a Board member to being nominated to the presidency.

VDT: Who did you run against?

LEE: Valerie Oppenheimer.

VDT: That's amazing. In recent years, it's usually the woman who wins if a man runs against a woman.

LEE: There's an interesting wrinkle also, because I grew up at Vassar College and Valerie was an undergraduate at Vassar and, in fact, she babysat for me.

VDT: Was your father a professor there?

LEE: Both my parents were professors at Vassar. [His mother was Dorothy Lee, the well-known anthropologist, who was widowed quite early--his father had been a professor of mathematics--and raised a large family on her own, according to Judith Blake in her interview.] So I had known her. I can't say I had a very clear recollection, but I knew that I knew her and I recognized the name, Valerie Kincade, as she then was. That night, I think I would have felt good if she had won and I felt good when I won.

VDT: What do you remember about the year you were president? We've already mentioned your address, homeostasis, which I still find difficult to follow ["Population Dynamics of Humans and Other Animals," published in Demography, November 1987]. I remember you'd been swimming just before, so you were full of beans. Did you make any special efforts in organizing that meeting in Chicago?

LEE: This is by now a very familiar refrain, but it is an incredible amount of work. And at the same time I was doing that, I was chairing the group here, Gene Hammel was on leave, and I was graduate

adviser here because Ken Wachter was on leave, and I was trying to edit these books for the IUSSP and I was reviewing proposals for NIH and I was still involved in the NAS business. Then there was this PAA program to put together, which I took as a very serious obligation, and the presidential address to write. It was a very difficult, just extremely demanding year.

My view was, and is, that you pretty much have to design the program to be a forum for members to present their research. So what you put on the program should be driven by the kinds and the distribution of research that people are carrying out--of course, taking into account how good the research is. And that there should be a minimum of sort of cute, trendy topics, which look very interesting on paper but for which, in fact, there aren't good papers to be presented.

VDT: Give me an example of a trendy topic. Cohabitation?

LEE: Well, okay. If you do these things prematurely--cohabitation, AIDS, homelessness, immigrants, squatters--all of these things. These things can be, and now are, excellent sessions. But there's a time at which you can do them and you will get hardly any submissions and the submissions you do get will not be qualified.

VDT: The Population Today writeup of your year said it was a subdued meeting. "Econometrics" seemed to be the buzz word; maybe that was thanks to you. And you were speaker at the applied demography breakfast. How did that come about?

LEE: I was asked. I in fact have an interest in applied demography. I taught a course on demographic forecasting for social planning, so I had to learn more about some of these topics. In any event, my interest in forecasting, my recognition of this as an important area of demography . . . It's an area in which many of the jobs are.

VDT: I was going to ask: What do you see as the outlook for demography in the U.S.? Does the future lie with applied demography? Is that where the jobs are?

LEE: Yes, there are a lot of jobs there, but it's hard to say now whether there may not be an opening-up of job opportunities in academics as well.

VDT: There could be jobs in academia still?

LEE: Well, still, or again. There is a big change; everyone was talking about it at PAA this year and I think last year as well. That is, the demographics of the situation are that there's going to be a greatly increased demand for college/university faculty in the 1990s.

VDT: Those who will be retiring, is that it?

LEE: Those who will be retiring and an upturn in births--when? I guess in the 1970s. There has been an increase.

VDT: In numbers of births, right: 3.9 million last year [1988].

LEE: How far down were we in the early, mid-1970s? It was 3.1, 3.2 million, maybe. I'm not really sure.

VDT: Never below 3 million, but okay.

LEE: That's a substantial increase and it's working its way through.

VDT: And you feel that there will be jobs in demography, that there's increased interest in demography?

LEE: I have been surprised at how much interest there seems to be in our demography PhDs that we're turning out now--from sociology departments, for example, and I'm hoping that one of our new PhDs will be hired by an anthropology department. That makes me think that well-trained people in demography will be very much in demand. Social science generally is becoming more and more quantitative and demography is at the core of the techniques, the knowledge of data sources, and all these things that are important for quantitative social science. I think in economics it's a much tougher row to hoe, because economics is already so sophisticated quantitatively and statistically, econometrically, and mathematical modeling and so on. But in sociology, and I hope in anthropology, there'll be more demography.

VDT: You feel a quantitative demographer has a break there; they wouldn't just take a mathematician, for example, if they're looking for someone quantitative?

LEE: No, because you need a deep interest in substantive social problems. So just the techniques by themselves, I think, go very little way, either towards producing good research, interesting research, or towards being a good teacher. You have to have both the technique and training in math and statistics and modeling and a really substantial knowledge of theoretical and substantive issues of demography. You can't just have one or the other.

VDT: What do you think of the changes in PAA meetings over the years? You mentioned a lovely thing that nobody had mentioned before--that they applauded papers. Of course, you haven't been going that long and the changes haven't been so radical in your time, but we're now up to 84 sessions with eight overlapping, many side meetings--even on Wednesday afternoon and evening this last time, we had these side workshops.

LEE: That was going on when I was president and I added seven sessions. I added an early Saturday afternoon time slot.

VDT: Oh, were you the first to do that?

LEE: I ended up with a lot of good papers left over after the sessions I had originally decided to have were filled up. I thought that wasn't right and tried to find places for them, so I myself essentially organized another seven sessions and put in that official time slot, and that worked out. I don't recall how many sessions I had [80], but probably fewer than there were at this last meeting [84]. So it's gotten bigger and bigger. Economic demography has just come a million miles.

VDT: Did you have something to do with starting that particular group that now meets on Wednesday?

LEE: Yes, I did. If I remember correctly, Lee Edlefsen and I did the original organizing of that group. We had a breakfast meeting on a Thursday or Friday; I can't remember where we were or how long ago that was. That came after we had a West coast meeting of economic demographers, which Lee Edlefsen organized and I hosted here at Berkeley.

VDT: Was that under the aegis of PAA?

LEE: It was not, no. But it was a great success; we all loved it. And there was no money involved: nobody's transportation was paid; people paid out of their pockets to come from San Diego or Seattle or whatever. It was nicely attended by 20 or 30 people and we had a delightful time.

That was so nice that it was decided to try to have a special meeting before the PAA; at least that's the way I remember it coming about. And the next year, instead of repeating this West coast meeting, there was a special meeting before the PAA on Wednesday afternoon. That has continued ever since. Unfortunately for me, as I got more and more involved in PAA business, I was always at Board meetings on Wednesday and I haven't been able to go for many years now. This last PAA meeting was the first year I was able to go to Wednesday meetings and this year they didn't hold any. I don't know just why. I guess they will be held again next year, because I'll try to see that they are held again next year. I think that's been nice. But at the same time, there's been a strong feeling that it's important that economic demography be in PAA and not off to the side.

VDT: You don't think people will split off; they will still like to stay within the umbrella?

LEE: I think so. Now it's my impression, however, that in the last two years economic demography has not been very well represented on the program. I'm hoping it will be in the future. I hope that maybe Larry Bumpass [1990 president] and other people will. I would like to see more economic demography on the program. But I think economic demographers, for whatever reason, do not do a good job of writing to the incoming president, filling in the suggestions for sessions, and so on. They all sort of wait passively until the call for papers goes out and by that time the sessions are what they are.

But there's been a great change. In the beginning, there was very little economic demography, and the economic demographers you did meet were regarded as very weird people. I remember it was the New Orleans meeting where the "new home economics" presence was first felt. There were papers by Willis and Jim Heckman and Gronau, and this was regarded as very strange. Rockefeller, or whoever had been funding this work at Chicago, was very concerned about whether it made sense or whether it was a waste, and so on.

VDT: You mean the Gary Becker school of thought?

LEE: Yes--Gary Becker and other people. But I would say that now the economic demographers, although they're not so numerous, are really at the core theoretically of what an awful lot of demographers, and sociologists as well, are doing. That the basic ideas have been largely absorbed into sociological theory about fertility, as well as the techniques--econometric modeling and so on. So there's just been an enormous change in that way.

VDT: And you like that, I'm sure.

LEE: Yes, I like that. I don't think it's become dominant in any way; it's just another flavor at the counter now.

VDT: So they will stay within PAA. Yet, do you think that PAA membership is still a bit elitist and clubbish? It's only been around 2,600--it's fluctuated at that number since the mid-1970s [2,655, end 1988; 2,752, end 1990]--compared to, well, I always hear about the American Economic Association, which is multiples of that.

LEE: Yes, but after all, there are a lot more economists than there are demographers. I think PAA is a delightful organization. I guess attendance in Baltimore [1989 meeting] was far bigger than it has been.

VDT: Yes, for the first time over 1,200 [1,193; record till 1991 meeting in Washington: 1,399].

LEE: I think the meetings are delightful; they are a very nice size. Of course, they have gotten bigger, but still I know most of the people there. One thing in my having been elected president relatively rapidly may have been I know so many . . . I mean, I know all the people from Berkeley when I was here and a lot of people at Michigan I know from when I was there. Of course, there are not many at Harvard.

VDT: One thing Jay Siegel pointed out about being elected president was, "Well, all your students vote for you and everybody who's ever read your textbook." There are only two names and if only one is recognizable . . .

LEE: That's right. I saw someone in an elevator in Baltimore with a nametag that said "Shryock" and I thought, "Ah ha!" [Shryock and Siegel are co-authors of The Methods and Materials of Demography, 1971.]

VDT: You've never met Henry?

LEE: No.

VDT: You had never met Henry Shryock! He's at all the meetings. I hope you introduced yourself.

LEE: Well, no, I didn't. Nor did he.

VDT: Well, he's not that outgoing, but oh, what a pity. You should have done that.

LEE: Next year I will.

VDT: Obviously, you've had a good time in your career and in your connections with PAA and the others--not so much NAS. Are you just going to go on this way--your career plans for the future?

LEE: Oh, I am very pleased to be getting out of all the organizational work.

VDT: You've got it all over with early in your career!

LEE: I think I don't have any obligations with PAA or IUSSP or NAS or NIH. So after this June, I think I'm going to have a lot more time for research. I'm really looking forward to it. I have enjoyed the last year a great deal because my administrative stuff was way down and I felt much more productive.

VDT: You don't think you'll ever become the chair of the Group?

LEE: Oh, I probably will sometime, but I haven't, and I think actually I'd rather not. Research and teaching and just sort of international coordinating, liaison work--I think on the whole I'd much rather

be doing those kinds of things than doing local administrative work. I've not been involved at all in university politics or administration here and I just prefer to have my energies elsewhere.

VDT: It sounds to me like you've had a lovely career and you've managed it very well, even though you didn't know what it was going to be when you started out. Thank you very much.

LEE: Well, thank you. I've enjoyed it.

CONTINUED

VDT: Afterthought. I've just asked Ron if he works with his computer or his pencil and he's claimed it's with the pencil.

LEE: Yes, I work mostly with pencil and paper, that is, when I'm actually doing research. And most of my so-called empirical work is on my calculator watch, which is a fairly serious calculator watch. Casio doesn't make these anymore, but this does inverse trigonometric functions and so on. But, of course, I do my writing on the computer and statistical and spreadsheet analysis.

VDT: But when you're really thinking up your new theory and models, it's pencil?

LEE: Yes, pencil and paper, pen and paper.

VDT: That's the classic way--really classic.

LEE: I wish I could tell you I used a quill, but I can't.

VDT: And you say your oldest daughter is traveling around the world, in Israel at the moment and has been in Italy?

LEE: Greece, France, Germany, Switzerland.

LEE: On her own?

LEE: She went with a friend, but at the moment she's on her own on a kibbutz and then she's coming to Paris in June to study French. I'm going to a conference there in the beginning of June, so we're going to hook up early in June.

VDT: That's great. She's what--19 or 20?

LEE: She'll be 19 in a week or two.

VDT: That's amazing. That also goes back to your interest in the Peace Corps, but I think that's quite precocious--the younger generation.

LEE: Not long before that, she bicycled across the United States--part of this fundraising business--so she bicycled 3,800 miles.

VDT: Wow! Of course, everybody knew about those, with the--what's it called?

LEE: I didn't know everyone knew about it; this is Overseas Development Network. There was some group doing the same thing in Europe as part of it. There were 150 kids in the United States. There was a group that started in Texas, one that started in San Francisco, one started in Seattle, and I think one started in San Diego. They met in Washington and bicycled to the United Nations. That was the final destination.

VDT: They were front-page news in our local paper, the Washington Post. Great!

POPULATION DYNAMICS OF HUMANS AND OTHER ANIMALS

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INTRODUCTION

Darwin and Wallace, the originators of evolutionary theory, independently found in Malthus's analysis of checks to population the driving force behind natural selection. It is, then, ironic that the concept of population equilibrium or homeostasis, maintained by density-dependent checks, plays so small a role in human demography and such a dominant one in the study of animal populations. With some exaggeration, we might say that human demography is all about Leslie matrices and the determinants of unconstrained growth in linear models, whereas animal population studies are all about Malthusian equilibrium through density dependence in nonlinear models, with little attention to age distributions and such.

Of course I exaggerate, and not only because animal population models have become more demographically sophisticated. Homeostatic interpretations emerge in many peripheral areas of demography, even if they are accorded no central importance. Examples include the long-run near stationarity of hunter-gatherer populations, rapid growth in frontier areas, recovery from demographic crises, population growth following economic progress, the cross-regional relation of fertility to density, the association of fertility with size of land holding, and long swings in historical European population. Nonetheless, such interpretations play only a small role in our discipline, and rarely in relation to broad patterns of population change, as originally conceived by Malthus. Integrated treatments are missing altogether, and there has been no general quantitative assessment of the strength of these forces in human populations.

And indeed, there may be excellent reasons for not taking homeostasis seriously in human population dynamics. Viewed in close detail, vital rates seem determined by forces having nothing to do with density or prior growth. Human behavior is distanced in many ways from biological constraints and forces by culture, institutions, technology, individualistic rationality, and the ability to accumulate. Although positive (biological) checks must always in principle stand ready to regulate population at sufficiently high densities, these peculiarly human features might well have kept human populations below the range within which density-dependent responses would occur.

Furthermore, the hallmark of density-dependent regulation is relative long-run stability of numbers. Yet over the last 10,000 years, the human population has multiplied by 1,000, growing not merely exponentially but superexponentially—at an exponentially growing rate. The capacity of humans to manipulate and modify their environments to extract or produce increasing quantities of food and other valued goods may subvert the entire theoretical enterprise, and the more so if we accept Boserup's theory that population growth itself engendered the economic progress that raised the ceiling.

Such points are surely relevant, but they are not decisive. If institutional regulation merely replaces biological, it will matter little to many aspects of the

outcome, provided only that the institutions in some way sense the limits and thereby avoid them—that is, provided institutions operate density dependently. If they do not, then only by accident could they prevent a collision of population size with environmental boundaries and the positive check. Even if Boserup is right and greater density encourages economic progress through positive feedback, nonetheless a more quickly acting negative feedback loop could be constantly operating to bring population toward the slowly rising equilibrium, in an interacting process I have described elsewhere (Lee, 1986). Distinctively human behaviors would alter in many respects the mechanisms and appearance of homeostasis but do not rule out its existence.

Later I will review evidence bearing on the existence and strength of negative feedback in human population growth processes. But why should we care? Certainly not only to understand the relation of human to animal population dynamics. If there is no homeostasis, then population history and contemporary population size are solely the cumulation of density-independent accidents, and a 10 percent loss of population 5,000 years ago would entail a 10 percent lower population ever after, up to the present. With homeostasis, the population tends to forget past historical influences, the more rapidly the stronger the homeostasis. When homeostasis is weak or absent, population changes exert an independent force in history; when homeostasis is strong, population size responds passively to changes in carrying capacity, or the demand for labor. If homeostasis is stronger still, it can lead to overcompensation and oscillations of population about its equilibrium size or growth path, which is one interpretation of Easterlin cycles in contemporary populations. Homeostasis opens a whole range of questions about the influence on reproduction of culture and institutions, not just on levels of reproductive activity but on their sensitivity to density or living standards, and about the average levels of density and well-being at which equilibrium occurs. Finally, we may consider the relevance of these concepts for contemporary populations—Should we expect automatic adjustment of population? Do negative feedbacks still occur?

My plan is first to discuss the concept of homeostasis. I will then establish a rough estimate of the strength of homeostasis or negative feedback in human populations—How much difference does density make to vital rates? This strength determines many important features of population dynamics, and these implications will be considered next. Following this, I will discuss the interaction of climatic change with demographic equilibrium, one source of environmental variation of human population trends. And finally, I will consider what all of this might mean for the contemporary world and specifically whether long swings in fertility could be manifestations of contemporary homeostasis. Of course, this rapid overview will be done with a minimum of references and detail.

CONCEPTS

To begin, I use the phrase *density dependent* for humans only as shorthand for a relation mediated by food or economic productivity and having little to do with space *per se*. The same is often true for animal population dynamics, although there the link of food and shelter to space is tighter (Emlen, 1984; Fowler, in press; Richard, 1985). I will use the terms *density dependence*, *homeostasis*, and *negative feedback* interchangeably. The situation is complicated by the fact that food, shelter, and fuel are typically some other animal or plant, so the interactions here are actually among populations. Nonetheless, I will treat humans as if they were an isolated species.

Among human populations, many mechanisms could in principle establish density dependence, including several shared with larger mammals: the biological link of

mortality to density and nutrition, biological dependence of fecundity on body fat levels and stress, dependence of lactation duration on availability of supplementary foods for children, institutional requirements of livelihood before marriage, explicit communal decision making, the psychic value of costly children, an asset value of children enhanced by land, and so on. There are also mechanisms that work in the opposite way, through the costs of children and the value of time, for example, or the desire not to divide bequests of assets.

Next, let us consider *carrying capacity*, a slippery concept in any context. Here I will take it to be an index of the population supportable at some given standard of living, for given technology and resources, and akin to the demand for labor, rather than as any kind of maximum sustainable population.¹ Like the demand for labor, it depends on many aspects of the economy besides natural-resource availability.

These ideas are readily expressed in the simple diagram in figure 1. The upper portion depicts the hypothesized negative relation of material welfare to population size, other things equal. My first empirical task is to estimate this slope. By carrying capacity I mean the position of this curve. The middle portion depicts the hypothesized positive relation of growth rates to some index of material welfare; the position and slope of this curve depend on institutional and biological influences to varying degrees. I will also try to estimate this slope. Both relationships may be quite weak. Combining them, we get the bottom relation, of the population growth rate directly to population size. The sensitivity of this compound relationship determines quantitative and qualitative aspects of population dynamics, so a major goal is to estimate it, even roughly. Equilibrium levels are indicated on the middle and bottom portions. Note that the carrying capacity is not the same as the equilibrium population size, since equilibrium depends on reproductive behavior as well as resources and could occur at a relatively high density, low income level, or the opposite—what Wrigley and Schofield (1981) referred to as high- or low-pressure systems.

This system operates in the presence of environmental variation—here taken to include all sorts unrelated to density. It is convenient to distinguish two general kinds. Variations in reproduction and survival unrelated to resource availability are called *density independent*—and reflect wars, epidemics, cultural and political change, emulation and fashion, and medical advances—to the extent that these are independent of density. The other kind is variation in carrying capacity or variations in incomes not caused by density. I will call these carrying capacity variations.²

Operating in this stochastic context, homeostasis can be viewed as a filter. Ideally, it attenuates the influence on population size and welfare of density-independent variation in vital rates while heightening the response of population size to variations in carrying capacity, particularly over the longer run. The effectiveness of this filter depends on the sensitivity of the system, which I will assess after briefly reviewing nonquantitative evidence.³

EVIDENCE FOR HOMEOSTASIS AND ESTIMATES OF RESPONSE SENSITIVITY

Hunters and Gatherers

There are many kinds of evidence for homeostasis in human populations, including the rapid expansion of some frontier or colonial populations and the close association of levels of fertility and mortality among pretransitional populations. Here, after briefly discussing paleolithic population, I will turn to more quantitative assessments.

The near stationarity of the human population over the million years or more

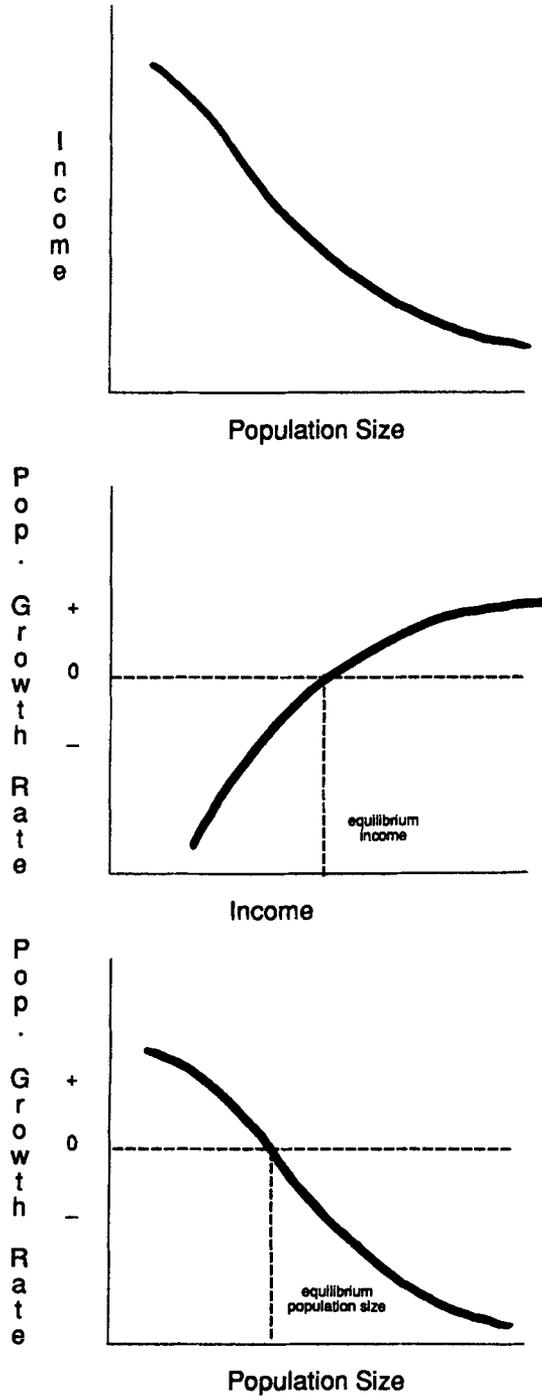


Figure 1.—Population Homeostasis: (Top) Income and population size; (middle) population growth rate and income; (bottom) population growth rate and population size

preceding the neolithic revolution, which doubtless conceals an enormous amount of variation in the local breeding populations (Hammel and Howell, 1987), is often cited as evidence of homeostasis among hunters and gatherers. If homeostasis were absent, surely random variation alone would have brought extinction or, counter to hypothesis, an encounter with environmental limits and the positive check. Wachter (1987b), however, has shown this appealing argument to be false. Given an expected value of the net reproduction rate (NRR) extremely near unity, pure randomness would not necessarily have led to extinction or to an early encounter with environmental limits. Wachter's striking analysis, however, leaves unanswered why the long-run mean NRR was in fact extremely near unity, to within a few ten thousandths. This itself seems to be strong evidence for homeostasis. Although there is much speculation by anthropologists on possible mechanisms, there is no direct quantitative evidence on density dependence among hunters and gatherers, so I now turn to agricultural populations.⁴

European Population and Wages

First, I will consider the relation in the top part of figure 1—the influence of population size on income. For some preindustrial European populations, there are fairly good time series for both population and real wages; despite some weaknesses, these will be used here.⁵ Real wage variations surely reflected systematic changes in income distribution as well as in average incomes.⁶

First consider the effects of population density or growth on real wages, from the middle of the 13th to the middle of the 19th century. I have combined data for England, France, Italy, Germany, Spain, Austria, and Poland into a summary real-wage series for Europe. Figure 2 compares wages with European population size, after removing a time trend to reflect the longer run changes in the demand for labor. The inverse movements are striking. Figure 3 shows a scatter plot of the same data, confirming a close association.

These visual impressions are confirmed by more formal analysis for the individual countries and for Europe as a whole. Table 1 shows the result of a regression of the log of the real wage on a quadratic time trend, to allow for a growing demand for labor, the log of the size of the population, and the decadal rate of inflation.⁷ The coefficient on the log of population gives the elasticity of real wages with respect to population size, which is to say the ratio of the percentage change in real wages to

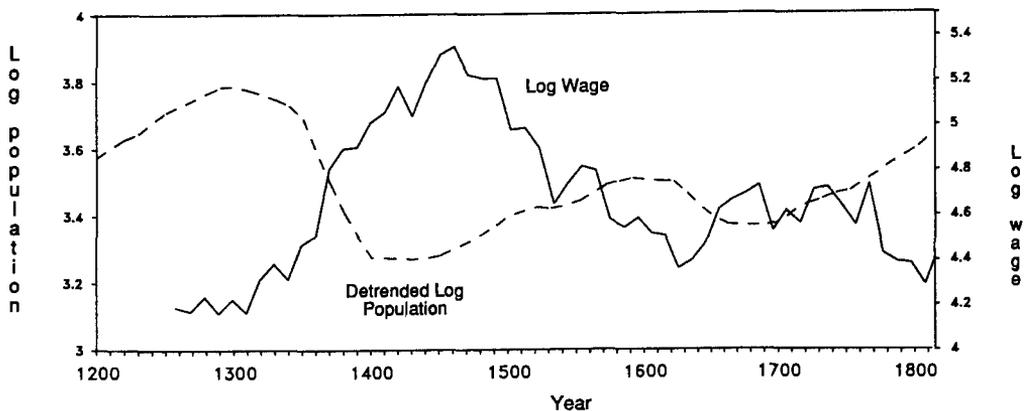


Figure 2.—Real Wages and Detrended Population Size (Europe, logs of decadal data, 1200–1810)

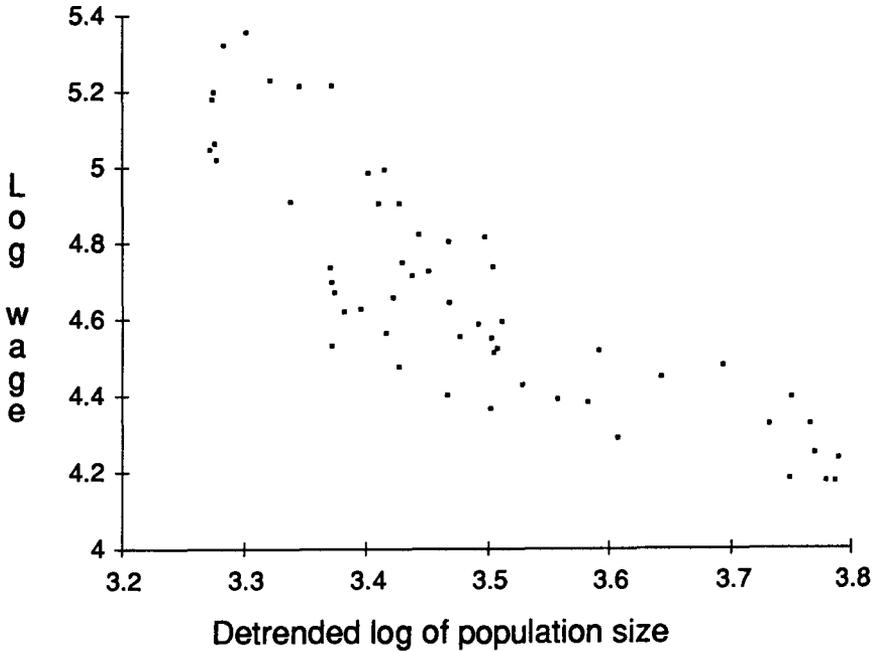


Figure 3.—Scatter Plot of Real Wages and Detrended Population Size
(Europe, logs of decadal data, 1270–1810)

the percentage change in population. For Europe as a whole the elasticity is -1.6 , and a 10 percent increase in population relative to its long-run trend would have depressed wages by about 16 percent.⁸ The estimated coefficients consistently show that the real wage responded negatively to population size, supporting the widely accepted view that population swings were a dominant influence on real-wage

Table 1.—European Wage–Population Elasticities

Area	Date	Elasticity
England	1260–1840	-1.2^{**}
France	1270–1840	-2.2^{**}
Italy	1360–1790	-1.0^*
Germany	1500–1750	-2.4^{**}
Austria	1520–1760	-2.5^{**}
Poland	1530–1800	-0.9
Spain	1410–1610	-2.4^{**}
Europe	1260–1840	-1.6^{**}

Note: These come from regressions of the log of real wages on a constant, time, time-squared, inflation, and the log of population size (for the rationale, see Lee, 1985). The data are 10-year averages of real wages, centered on points of population size. All are adjusted for up to second-degree autoregressive residuals, as appropriate. The Spain regression did not include any time variables, as they were insignificant, or inflation.

* Significant at least at 0.05 level.

** Significant at least at 0.01 level.

movements in preindustrial Europe. These results establish the first of the two relationships necessary for density dependence.

The Response of Fertility to Real Wages or Density in Rural Populations

Turning to the second relationship (see middle panel of fig. 1), I earlier enumerated mechanisms through which density, food availability, or incomes might have affected fertility. However, nothing can replace direct evidence that such mechanisms actually operate as expected in real populations. Here I will concentrate on the outcome, rather than the mechanism, and review many different kinds of empirical study relating fertility (and to some degree mortality) to density, income, or wealth, for predominantly agricultural populations.⁹ I have only used studies for which, for comparison, I was able to reexpress the result as an elasticity with respect to density. The results are shown in figure 4 and the Appendix. Each line in figure 4 shows the distribution of the estimated elasticities for the group of populations indicated by the line heading as well as referring to a different substantive context in which homeostasis is believed to manifest itself.

The first line refers to the long swings of several centuries' duration in the demography of Europe, often interpreted in Malthusian terms (e.g., see Wrigley and Schofield, 1981). Each point represents the estimated responses of the population growth rate with respect to the real wage level for one of the seven European countries whose experience was analyzed above, for various periods from 1270 to 1800, using decadal data. These estimates were then multiplied by the European wage–population elasticity (–1.6) to derive the effect of density. Note that these combine the response to density of both fertility and mortality. These estimates are very fragile.¹⁰

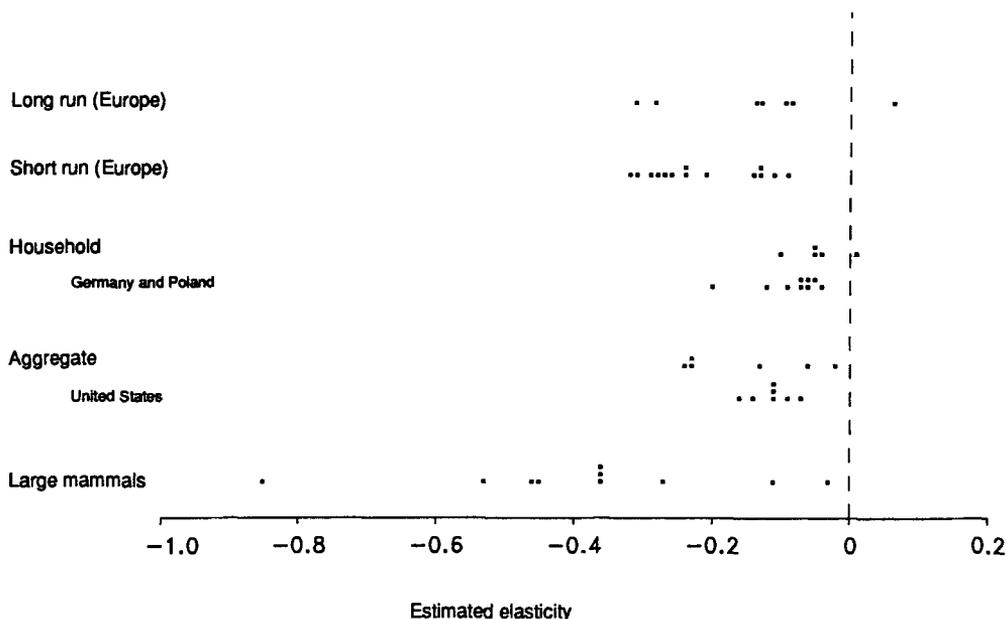


Figure 4.—Estimated Elasticities of Fertility With Respect to Density (various populations, time periods, and methods). Each line shows the distribution of the estimated elasticities for the populations indicated by the line headings.

The responsiveness of vital rates to short-run fluctuations in grain prices or the harvest is often taken to be a sign of homeostasis (e.g., see Habakkuk, 1953). In earlier work, I found a consistent response of fertility to short-run fluctuations in real wages in preindustrial England (Lee, 1981), results confirmed for other countries and periods by Richards (1983), Weir (1984), Schultz (1986), Galloway (1987), and others. I here show Galloway's results for 14 large European populations, from the 16th to the 19th century.¹¹ The effects of changes in timing of births are removed by cumulating the effects over five years, and the results are crudely expressed as elasticities with respect to density, again by multiplying by -1.6 .¹² These results are quite robust, but they are best viewed as merely suggestive, since responses to short-run variations could well be quite different than to long.

The well-established negative association of fertility with size of land holding at the household level can be viewed as an expression of homeostatic tendencies. The next line shows estimated elasticities of this sort for contemporary developing countries in Asia, Africa, and Latin America.¹³ The following line shows similar estimates for various cohorts in Poland and Germany. All of these micro-level studies suffer from possible reciprocal causation, as families with more labor may acquire more land. Also note that here and below, some of the studies use child-woman ratios to measure fertility and so reflect mortality to some degree.

The group of estimates labeled "Aggregate" refers to the outcome of studies of fertility in relation to density, mostly cross-regionally within a country but occasionally internationally (three of the seven are of India); for all of these, reciprocal causation is not an issue.¹⁴ The line labeled "United States" represents the same sort of study, based on cross-state analysis; each point is for a decadal census 1800-1860.¹⁵ This U.S. analysis effectively covers the frontier effect and shows it to be consistent with density-dependent fertility occurring in other contexts.

These studies, viewed together, provide strong support for the widespread existence in advanced agricultural populations of a negative association of fertility—and probably to some degree mortality—with density, with elasticities in the range of 0 to -0.3 . Doubtless these come about in many different ways; a common one is marriage, because when marriage is included as a right-hand variable in these studies, the estimated effect of density is typically much diminished.¹⁶ There is no reason to expect that elasticities are numerically close in different societies; the differences shown in figure 4 arise in unknown proportions from different methods, from biases, from sampling variations, and from true underlying differences.

Let me pause here to pursue the analogy between the population dynamics of humans and other animals, an analogy stressed by Malthus. The central importance of density dependence for animal population dynamics is widely acknowledged (despite a lively controversy in the past), but not for humans. Is this difference in emphasis justified? Comparisons with primates would be interesting, but not possible, so I will here compare human response elasticities with those for other larger mammals. I have calculated elasticities of fertility with respect to density for 10 larger mammals, based on data gleaned by Fowler et al. (1980) from many studies—data that are both sparse and unreliable but perhaps sufficient to convey an order of magnitude (see Large Mammals in the Appendix).¹⁷

There is considerable overlap in the distributions of elasticities for humans and larger mammals, but the mammal distribution is generally higher. Given the uncertainty in all of these estimates, it is best not to make anything of this. To be sure, we would expect distinctively human practices to weaken the biological response of both fertility and mortality to density while strengthening the nonbiological response of fertility through institutional and rational regulation. Thus we would

expect relatively strong elasticities for fertility compared with mortality among humans as opposed to animals. Nonetheless, I am struck by the similarity of these distributions. Unfortunately, I am unable to duplicate them for mortality.

Mortality and Density

I now return to the problem of estimating the response elasticity for humans. Farr's Law, an empirical relation claimed to hold for early 19th-century English counties, asserts that mortality varies as the 12th root of density (Humphreys, 1885). On examination of Farr's original calculation, it is clear that he misinterpreted his result, which actually shows mortality to vary with an elasticity of 0.12, or as the eighth root of density.¹⁸ I am not aware of contemporary studies linking mortality and density in rural populations in the way done in the micro and aggregate studies just reviewed for fertility. There are abundant studies of short-run fluctuations, strengthening what I originally found for preindustrial England: a negative relationship of mortality to real wages, explaining only a small portion of the variance. In Galloway's (1987:305–331) results for 14 European populations, the median cumulated density is -0.16 ; multiplied by -1.6 , this suggests a mortality–density elasticity of 0.26. Preston's (1980) cross-national data provide an interesting comparison, yielding a bivariate elasticity of 0.12 for mortality and per capita income in low-income countries in both 1940 and 1970.¹⁹

Inferred Values of the Control Parameter

Combining typical values from these distributions of estimated parameters suggests an elasticity of around -0.25 for the net reproduction rate with respect to population size or density in advanced agricultural populations.²⁰ That is, a 10 percent increase in population would reduce the NRR by about 2.5 percent, or equivalently, would reduce the population growth rate by about 0.1 percent.²¹ Doubtless this parameter varied across populations, and there is some evidence that it was greater in poorer agricultural areas, where the positive check was stronger (see Galloway, in press). But -0.25 provides a convenient point of departure.

INTERPRETATION OF ESTIMATED PARAMETERS

The next task is to consider some implications of this estimated feedback sensitivity for human population dynamics. Imagine a population reduced 20 percent by war or epidemic, with no change in carrying capacity. The NRR would rise from 1 to 1.06 ($1 \times 0.8^{-0.25}$), and population would initially grow at the slow but positive rate of about 0.2 percent per year, which would decline as the population neared its former size. It would take about 70 years before the population had made up half of the deficit, reaching a point 10 percent below equilibrium. We could say that the half-life of a population displacement is about 70 years. Recovery from density-independent historical influences is evidently very slow. Similarly, if the carrying capacity were suddenly to double, in 70 years the population would grow only halfway to the new equilibrium level.

Now consider the size of the population in any particular year, somewhere in the neighborhood of equilibrium. We could think of its exact location as a kind of weighted average of past density-independent historical accidents. A displacement 70 years ago would get a weight of 0.5, for example. If homeostasis is very strong, the weights drop off rapidly and only recent events matter much. Population stays relatively close to equilibrium. If homeostasis is very weak, then distant events matter almost as much as recent ones, and population will wander farther from equilibrium. The estimated value of -0.25 is relatively weak and a half-life of 70

years is rather long, so for human populations the density-independent forces of history have played a large role relative to the systematic force of homeostasis and changes in the demand for labor.

As it happens, I can be more specific. With a feedback elasticity of -0.25 , only about 0.5 percent of the variance of the annual growth rate would be explainable by density—an R^2 of 0.005—for a population in stochastic equilibrium, what Malthus called an "old state."²² An R^2 of 0.005 is very low indeed. Almost *all* short-run temporal fertility variation is density independent. Thus density dependence is hardly a theory of vital rates; rather, it is a footnote to other theories, a suggestion that in addition to other interesting and important variables, there is a weak but persistent tug from density effects. It is essential to realize, however, that as long as there is any trace at all of density dependence, no matter how weak, this tug, by its systematic persistence, comes to dominate human population dynamics over the long run, if not the short. Its relative unimportance over shorter intervals makes it tempting to dismiss, but doing so would make nonsense of longer run population change. Furthermore, the same mechanism that makes growth responsive to density also makes growth responsive to variations in carrying capacity or the demand for labor, and such induced growth is centrally important historically, in both the long and the short runs.

Homeostasis will be far more important in explaining change in populations that are not in stochastic equilibrium for one reason or another, such as those in frontiers and newly colonized areas, or those recovering from an isolated catastrophic shock. Such situations offer excellent opportunities to study homeostatic response, which will then be strong in relation to the density-independent disturbances while homeostasis steers the population toward stochastic equilibrium.

Sometimes density-independent changes in the disease environment or in institutions may cause long-run change in the vital rates. In this case population size would change, but not without limit; rather, population would move to a new equilibrium, and the extent of change is limited by the strength of homeostasis (here imagine a vertical shift in the curve relating population growth rate to income in fig. 1). With the estimated response elasticity of -0.25 , an extraneous 10 percent increase in the NRR would raise the equilibrium population by 46 percent ($1.1^4 = 1.46$), and per capita incomes would fall accordingly.

So far, these interpretations have depended on only the feedback elasticity. If we also know the variance and structure of the shocks to population growth, then we can calculate the probability distribution of the size of the population. Taking empirical values from preindustrial England, the standard deviation of population size from equilibrium would have been about 7 percent.²³ If homeostasis were stronger, the standard deviation would be smaller, and conversely. If there were no homeostasis, then population would follow a random walk, its variance would become infinite, and we could no longer speak of an unconditional probability distribution for population. If asked why population is such-and-such a size, we could only answer, "Well, it has to be something, and this is what it happens to be."

The standard deviation imparts no graphic sense of population variation, however. A stochastic simulation far better conveys a sense of the paths a population might follow as it wanders within its probability distribution, as shown in figure 5. The simulation shows population size annually for 2,000 years, with fertility and mortality subject to homeostasis and to random perturbation, based on estimated values.²⁴ Also shown is a population with the same random shocks and same initial size, but with no homeostasis, that is, with elasticities set to zero. We can compare the two to assess the influence of density dependence.

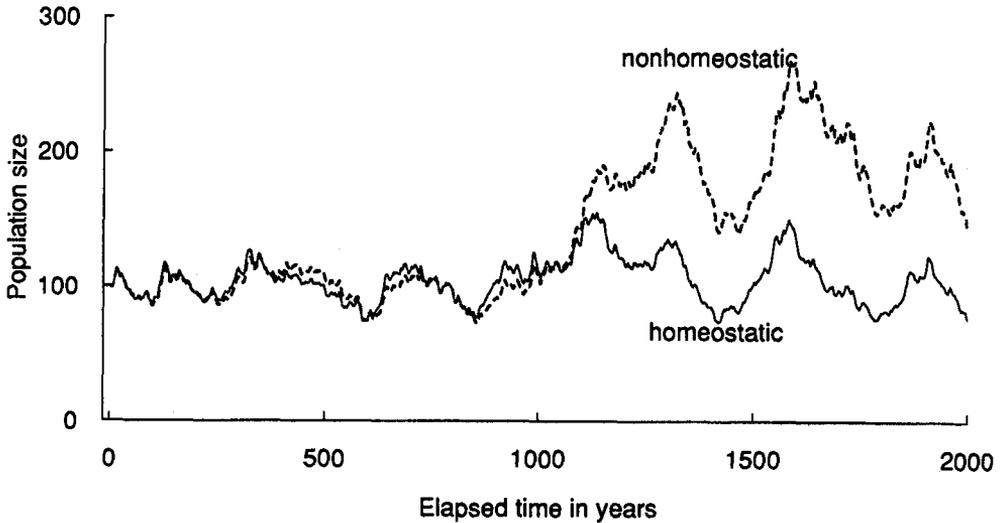


Figure 5.—Stochastic Simulation of Population Size With and Without Homeostasis (2,000 years). For details of the simulation, see text and note 24.

Visually, there is scarcely a discernible difference between the two for the first few centuries; the lines remain very close. Both drift around and appear largely random. These simulations show that homeostasis need not make much difference over the course of a few centuries, making it hard to recognize and limiting its importance as a force within historical periods of moderate length.²⁵ Many historians would disagree, but elasticities would have to be considerably higher for this result to be altered. Yet it would be misleading to leave the matter here, for the gentle nudge of homeostasis becomes a dominant force in the longer run. Eventually the two population trajectories come to differ by a great deal, and the greater stability of the homeostatic population becomes obvious.

CYCLING BEHAVIOR AND STRENGTH OF HOMEOSTASIS

The strength of density dependence also influences the pattern of population fluctuations or cycles. Populations not subject at all to homeostatic forces may nonetheless move cyclically; the generation-long cycle familiar to demographers and thoroughly analyzed in mathematical demography is one example of a nonhomeostatic cycle. But as homeostatic control is made more sensitive, the compensating response becomes more rapid relative to the span from birth to maturity, and the patterns of fluctuation change. The generational cycle may damp more rapidly, as larger cohorts experience lower rates of reproduction. Eventually, it may be replaced or supplemented by some sort of longer cycle as overshooting begins to take place. Easterlin cycles are the most familiar example of this in human populations, but examples are common in other animal populations. As feedback becomes stronger still, bifurcation occurs, and a damping long cycle will be replaced by a locally explosive one, leading to self-perpetuating limit cycles. Finally, if sensitivity of response were to be higher still, chaos, or nonrepeating patterns of change, would occur. Rapid progress has been made in the formal analysis of these various possibilities (see Frauenthal and Swick, 1983; Lee, 1974; Samuelson, 1976; Tuljapurkar, in press; Wachter, 1987a; Wachter and Lee, 1987).

These more exotic dynamic behaviors are not common in animal populations

outside the laboratory. Empirical work finds that even insect populations with nonoverlapping generations are rarely sufficiently sensitive near equilibrium to generate limit cycles, let alone chaos cycles (see May, 1977),²⁶ although *damped* homeostatic oscillations are not uncommon in animal populations (Ito, 1980). But what of humans?

Malthus (1798/1970) himself confidently stated that preindustrial populations do oscillate irregularly about their equilibrium levels "as no reflecting man who considers the subject deeply can well doubt," and historians routinely diagnose long population swings as "Malthusian oscillations," generated by homeostatic response. Returning to the simulations we viewed earlier, we can see that the homeostatic series does indeed typically move in long swings about its equilibrium, in what might be Malthusian oscillations several centuries long, just as in European history. . . . Might be, but are not; for unfortunately the population without homeostasis also shows such oscillations. Rather, such long swings are just what we would expect from processes that are essentially random walks (e.g., see Feller, 1957).

In fact, the range of human control sensitivities surveyed earlier for agricultural populations is so low that it would hardly affect the pattern of fluctuations and would leave the generational cycle undisturbed. Only if feedback elasticities were substantially higher—say with the fertility elasticity over unity—would more interesting dynamics occur. Elasticities as estimated here and in the studies surveyed simply are not high enough to cause overshooting.

On the other hand, despite a lack of evidence, it is at least possible that the response elasticity has increased greatly in modern times, for several reasons. Because fertility and mortality levels have fallen, a marginal birth makes proportionately more difference to the NRR. At the same time, improvements in contraceptive technology have greatly reduced the costs of fertility control. And fertility is increasingly determined by rational calculation.

With the possibility in mind of high contemporary elasticities, suppose we repeat the earlier homeostatic simulation, this time boosting the fertility elasticity from -0.12 to -7.5 . Figure 6 shows the result—a series of long swings about equilibrium, growing in amplitude, with a period of about 60 years.²⁷ Now these are indeed Malthusian oscillations, and they occur with or without random shocks. This is not the place to assess the controversial view that some mechanism of this sort has driven 20th-century U.S. fertility. It is worth noting, however, that the simple association of fertility with the size of the working-age population is consistent with such an interpretation, as seen in figure 7. This shows the total fertility rate and the detrended size of the working-age population from 1900 to the present.²⁸

ENVIRONMENTAL VARIATION AND HUMAN POPULATION DYNAMICS: THE ROLE OF CLIMATE

Now let us turn back from this discussion of Malthusian oscillations to consider random disturbances to the orderly process of population growth and equilibrium. To call these disturbances "random" is only to say that they are unrelated to population density, which would be true for most determinants of vital rates studied by demographers. Recalling figure 1, it is helpful to distinguish between the influence of carrying capacity variations on vital rates (harvest failures, e.g.) and density-independent influences that directly affect the vital rates, as would the introduction of some new disease, a new contraceptive method, or a new institutional arrangement. If the change persists for a long time, we may say that the population equilibrium and/or carrying capacity has changed.

Here I will briefly discuss one particularly interesting source of environmental

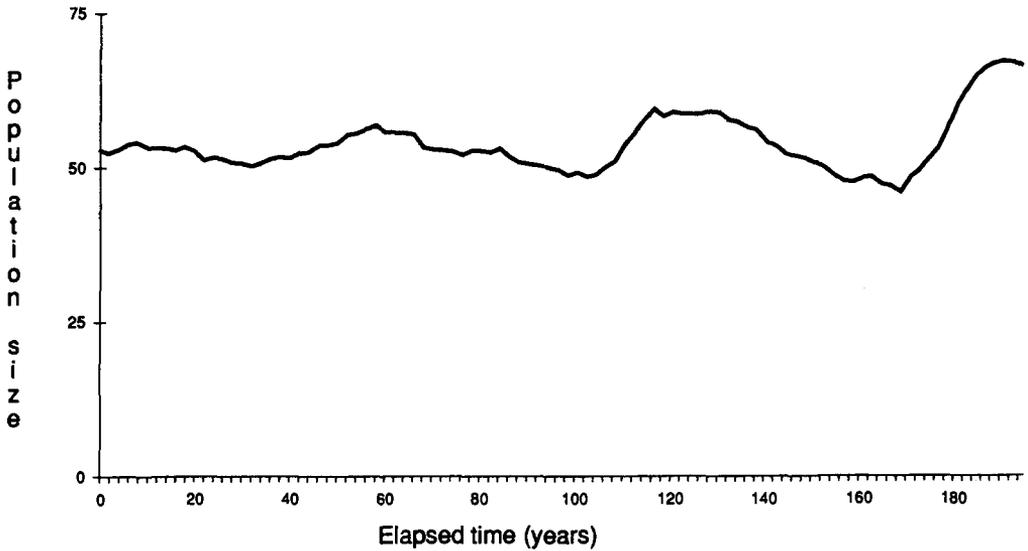


Figure 6.—Stochastic Simulation of Homeostatic Population With Fertility-Labor Supply Elasticity of -7.5 . For details of the simulation, see text and note 24.

change, climate. It is of interest because, unlike many peculiarly human factors, it affects animals as well, and indeed when the prevalence of density dependence among animals was hotly debated some years ago, some pointed to the strong direct impact of weather on vital rates as evidence against the relevance of density dependence. It is also of interest because it might account for the global synchronism of some historical population change (see Galloway, 1986).

The usual theory is that climate affected population growth indirectly, by altering agricultural productivity and carrying capacity, in ways that vary by climatic zone. But it might also affect vital rates directly, as a density-independent factor, and the

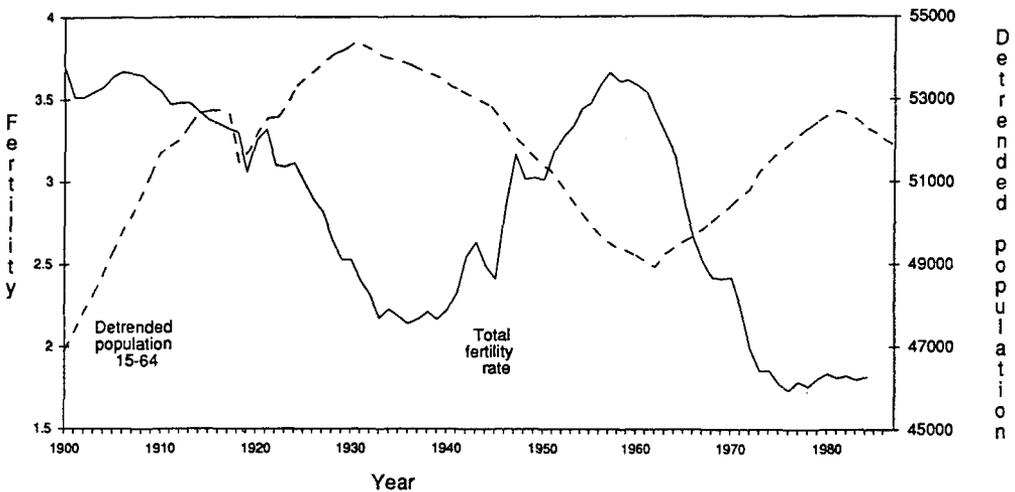


Figure 7.—Total Fertility Rate (—) and Exponentially Detrended Population Aged 15-64 (---) for the United States, 1900-1986

distinction is important. First consider the effects of short-run variations in climate, for which a convincing empirical case can be made. Analyzing English historical data, I have found that both coldness of winter and hotness of summer raised mortality and reduced fertility, net of real wage variations and net of normal seasonality in the rates (Lee, 1981). Other studies (by Galloway, 1987; Richards, 1983; Schultz, 1986) have confirmed these findings.

Figure 8 shows estimated effects of summer and winter temperature on natural increase for 10 historical European populations, based on Galloway. The consistency is striking: in all 10 cases, warmer winters are associated with more rapid population growth; and in 8 of the 10 cases, warmer summers are associated with slower growth. At times in Europe, warmer winters and cooler summers would occur together, known as the maritime pattern, as opposed to the continental (see DeVries, 1981). In this case, the effect would be compounded. Otherwise, 1°C warming would raise the population growth rate by about 0.1 percent per year, or somewhat more in a poorer country (see Galloway, 1987).

Turning to carrying capacity variations, climate also affects agricultural productivity. Although the mechanisms are complex, winter temperature, or annual mean temperature, appears to capture a substantial proportion of climate's influence. Borrowing again from Galloway's analysis of short-run fluctuations in historical Europe, figure 9 shows consistently negative effects of warm weather on prices and thus positive effects on productivity. Typically, a 1°C warming would depress prices by about 10 percent through an unknown increase in output and thereby indirectly raise population growth rates by about 0.1 percent per year. So analysis of short-run variations suggests that climate did indeed affect human populations in the past, in roughly equal measure indirectly through agricultural productivity and directly through vital rates.

But what of the longer run? Did climate drive the centuries-long swings in European demographic history? Eminent historians have cautioned against allocating any simple role to climate in the face of human adaptability to long-run change (e.g., Appleby, 1980; DeVries, 1981; contributors to Kates, Ausubel, and Berberian, 1985). But changes in mean temperatures may well have been associated with

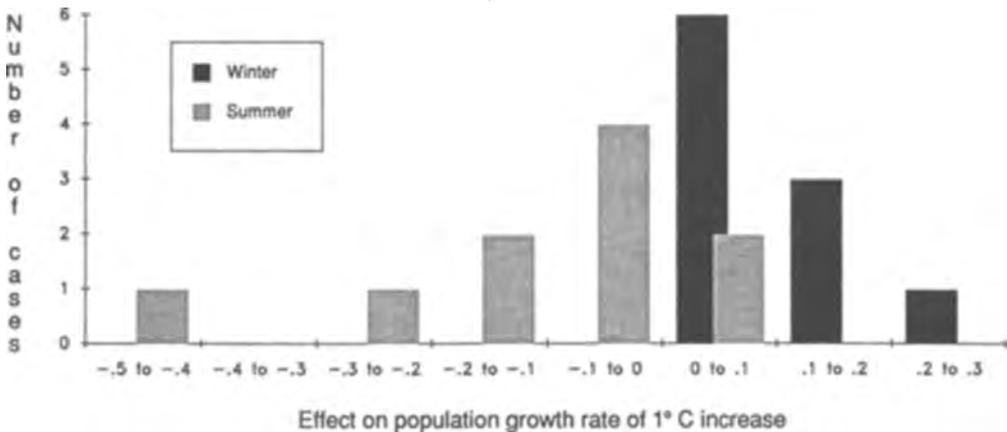


Figure 8.—Seasonal Temperature and Population Growth Rate: Cumulative Effect of 1°C Increase Net of Grain Prices in 10 European Populations (various dates, 1675–1870). Source: Galloway (1987:tables 3.4 and 3.8; numbers in col. 17 multiplied by 1.8).

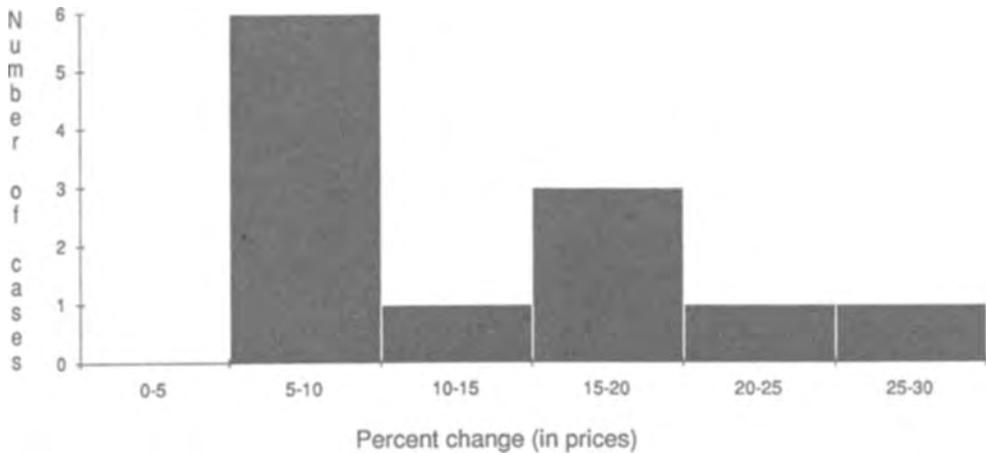


Figure 9.—Annual Temperature and Grain Prices: Cumulative Effect of a 1°C Decrease for 12 European Locations. Source: P. R. Galloway (private communication, 1985).

changes in their variability, in which case an impact of long-run climate change would be more easily understood (DeVries, 1981). Unfortunately, instrumental measures of temperature are not available before the end of the 17th century, and noninstrumental proxies are not completely satisfactory.

Speaking roughly and speculatively, I do find some evidence from England, and for Europe as a whole, that long-run warming affected productivity similarly, but more weakly than in the short run. Direct effects on population growth rates likewise appear similar and undiminished relative to the short run. Taking these results at face value, temperatures that were 1°C warmer, and perhaps less variable, would raise carrying capacity by 5 percent or so in northwest Europe and thereby raise the equilibrium population equally. But the direct effects on vital rates would lead to continued population growth even after the 5 percent increase had been attained; in the final equilibrium, population would be 10 or 15 percent larger than before, and real wages substantially lower. The initial effect of mild weather was beneficial, but its longer run impact may have been impoverishing, by raising population growth rates independently of incomes and therefore requiring a greater degree of economic hardship to reestablish demographic stationarity. Such possibilities make the historical study of the role of climate more difficult, but also more important.

MODERN WORLD

I have tried to show that human population dynamics were homeostatic, at least until the past century. But what about this past century? Few would deny that the phenomenal growth of world population was fundamentally caused by economic progress, without which it could not have occurred or been sustained. But much demographic response to economic change, probably including most during this demographically turbulent century, falls in a gray area; it is related to economic change in ways so devious and indirect that the outcome appears haphazard, not systematic. For example, the neolithic revolution is often said to have fostered population growth because a sedentary life style permitted shorter birth intervals, rather than because agriculture can supply vastly more food than hunting and gathering. To take a contemporary example, economic development may spur

population increase because the growth of markets and geographic mobility undermine traditional constraints to fertility. The outcome is consistent with homeostasis, but the mechanism appears otherwise. It would be easy, and probably correct, to say that if such and such had not happened, then economic growth and rising incomes would in some other way have induced population increase and, therefore, that the appearance of haphazard change is misleading. But today we see continuing economic growth and increasing demand for labor in the industrial nations, combined with fertility well below replacement levels, which appears inconsistent with homeostasis.²⁹

In fact, the relations underlying homeostasis have been altered in important ways since the 19th century. First, the influence of population density on incomes has progressively diminished. The role of land in agriculture has dropped to only about 10 percent,³⁰ and the role of agriculture in industrial economies, to only a few percent. For most economically advanced nations, size and density of population now mean little to economic welfare. Therefore, at least for the present, one of the essential relations underlying homeostasis is greatly weakened. Eventually, if populations of developed countries grow more than now seems likely, densities will be reached at which land and other renewable natural resources will again become important constraints on per capita incomes and human productivity.

Second, the link of vital rates to income or wealth is now very different. Incomes in many countries have entered a range within which, on average, basic nutritional and health needs are satisfied to the point where further income increases are of little relevance to survival or fecundity, and indeed may be harmful, although distributional problems persist. On the fertility side, the dramatic rise in the importance of human resources as a form of wealth not only has raised the cost of children but has raised it disproportionately for those who themselves are more productive. This tends to reverse the positive response of fertility to wealth on which homeostasis depends. Although fertility is now potentially highly responsive to economic conditions, the response is probably not homeostatic.

If classic density dependence appears of little relevance to contemporary population dynamics, there is still scope for nonlinear models in which feedback operates in other ways. The tantalizing covariation of U.S. fertility with other demographic measures, as illustrated by figure 7, is probably best viewed not in terms of density but, rather, in terms of growth rates or relative age group sizes. Mating markets with conventional age gaps between spouses provide feedback from past growth to current fertility. If fertility is influenced by emulation there is positive feedback, as Brian Arthur argues. Positive feedback would likewise result from the historic interaction of Malthusian and Boserupian forces.

SUMMARY AND CONCLUSIONS

Human population dynamics, at least until the past century, have probably been governed by homeostasis and in this resembled those of other animals. Because human population homeostasis was probably substantially weaker than among large mammals, its operation has been less obvious. Nonetheless, the empirical evidence for advanced agriculturalists is compelling.

Unlike animals, the human population has tended toward equilibria that have been tending upward at an accelerating rate. The acceleration might reflect long-run positive feedback between density and technological progress, as Boserup has suggested.

Because homeostasis was weak, its role in shorter run historical explanation is limited; its force was gentle and easily overwhelmed by other particular influences.

Malthusian oscillation, in the sense of distinctive medium-run dynamics arising from homeostasis, probably did not occur. And because homeostasis was weak, density dependence can in principle explain only a minute proportion of the annual variation in population growth rates. Yet homeostasis plays an essential role in demographic theory. Without it, we are incapable of explaining population size and change over time except by recounting a mindless chronology of events back to the beginning of humanity—whenever that was. Without it, we cannot explain the response of population growth to economic growth. Without it, we cannot explain recovery from catastrophe or the rapid natural increase in many frontier regions. Without it, we cannot properly analyze the influence of climatic variation and other partially density-independent factors. Our basic understanding of human history requires a grasp of what homeostasis can explain and what it cannot.

A homeostatic approach to population dynamics also leads to questions about the roles of reproductive norms and institutions, not just whether they encourage high or low fertility, but whether they make natural increase responsive to resource abundance. And if they do, whether they strike the balance of population and the means of subsistence at a relatively prosperous or impoverished level. Such considerations may contribute to an understanding of broad preindustrial differences among the regions of the world in densities, average levels of vital rates, and living standards—which was very much how Malthus viewed the matter.

Ordinary homeostatic tendencies essentially vanish in the course of economic development, and they were probably all but gone from much of Europe by the end of the 19th century. Economic progress lifted populations out of the sensitive range of incomes, despite the relatively rapid population growth engendered by declining mortality. National production came to depend very little on land, mortality became largely independent of income, and fertility came to respond perversely to growing productivity of labor. At the same time, the potential sensitivity of fertility to economic change has doubtless increased greatly, raising at least the *possibility* of overcompensating dynamic behavior, driven not by density but by age distribution fluctuations and appearing as Easterlin cycles. In the long run, if population growth continues, natural resources must eventually reemerge as an unavoidable constraint on human numbers. Current theories of fertility give little insight into how equilibration would then occur.

NOTES

¹ I have never understood its treatment as the maximum sustainable population, since this would surely depend not only on the resources available but also on extraneous conditions directly influencing population reproduction and survival. Such conditions are in turn quite variable—certainly for humans, and I suspect for other animals as well.

² This second kind of variation is referred to in the biological literature as “density dependent.” This somewhat paradoxical nomenclature apparently arises from the mathematical accident that within the standard logistic model used to represent population dynamics, the effect on growth rates of variations in carrying capacity happens to depend on population density; in other models, it need not.

³ The formal analysis behind these comments is given in Lee (1982).

⁴ Howell’s (1979) book on the !Kung is the most detailed study of the demography of a hunter-gatherer group, but it does not contain evidence of the sort needed here. There is an extensive anthropological literature on homeostatic adjustment of human populations, but quantitative evidence is absent.

⁵ The demographic data come from McEvedy and Jones (1978), modified after 1500 based on DeVries (1984). Estimates were interpolated to get decadal points, and within the general trends derived from the above sources, details on turning points were taken from country-specific sources. The sources for wages and grain prices, used to form real wage series, are too diverse to list here. The wage data are typically for unskilled urban labor, and they are deflated by grain prices. For England the standard Phelps-Brown and Hopkins series, as modified by Wrigley and Schofield (1981), was used, and this series served as a template in forming the European average. Series for other countries were scaled to agree on average with

the English series for the first 20 years of overlap. National series were often formed by averaging a number of within-country series.

⁶ If the large wage-population elasticities reported below did not reflect income distribution changes, then they would imply that total output decreased as population grew, which was certainly not the case.

⁷ For a discussion of the rationale underlying this specification, see Lee (1985).

⁸ For contemporary populations, we could infer the wage or per capita income elasticities from estimated production functions; the difficulty with this procedure is that to do so would require very strong *ceteris paribus* assumptions concerning investment in complementary inputs and the development of technology. If we do not make such assumptions, then macro-statistical analyses typically reveal no negative consequence of population growth or density for output per head, even for the agricultural sector, as will be discussed further below.

⁹ Some studies explicitly examine fertility in relation to density, others in relation to farm size, and still others in relation to income or wealth. Since the relation of income to population density has already been established above, at least for preindustrial Europe, this latter kind of study is also relevant. Short-run fluctuations in vital rates provide the most robust and consistent evidence, but unfortunately they are also the least relevant, since they tell us little about long-run relations when many more adjustments to adversity or prosperity can be made.

¹⁰ That is, the elasticity of fertility with respect to wages times the elasticity of wages with respect to population size should equal the elasticity of fertility with respect to population size, unless there are other channels besides wages through which density affects fertility. Several of these estimates are not significantly different from zero, and they are not robust to specification changes. Simultaneity of population and wages further biases these toward zero (see Lee, 1985). These estimates are least solid of all of those in the figure. Note that for purposes of this figure, an elasticity was calculated by assuming that the growth rate responses arose from fertility variation, and a suitable mean fertility level was assumed.

¹¹ Actually, the independent variable is grain prices, lagged from zero to four years, and the number shown is the sum of the lagged effects. Nominal wage measures changed very slowly, so almost all of the short-run variation in real wages is due (with elasticity of -1) to variation in prices. Dependent and independent variables were detrended by division by a centered 11-year moving average. The estimates are from Galloway (1987:304-330, app. tables 2.2-2.15, eq. A). The method used is similar to Lee (1981).

¹² Short-run fluctuations in fertility do not reflect fluctuations in nuptiality to an appreciable degree (see Lee, 1978), so the effects of prices on nuptiality should be added to these. For 14 countries examined, Galloway found 13 cumulative sums for nuptiality negative, with a median of -0.146 (see Galloway, 1987:304-330, app. tables 2.2-2.15).

¹³ These and the following elasticities were converted to population density elasticities on the assumption that average size of land holding is related to population density with an elasticity of -1 . Of course, to the extent that there is unsettled land that can be brought into cultivation, this overstates the strength of the effect.

¹⁴ These studies include cross-regional analyses in Brazil, Mexico, and India and pooled time series and cross-section analyses for villages in India and for international data in Latin America, all for the past few decades.

¹⁵ These estimated elasticities are drawn from a reanalysis of Yasuba's (1961) data. The more recent estimates by Forster and Tucker (1972) and by Schapiro (1982) are not suitable for our purposes, because the measure of land availability they use is itself elastic to density.

¹⁶ I should add that there are many more studies supporting the qualitative conclusion (see Mueller and Short, 1981; Schutjer and Stokes, 1984); I have surveyed only those for which an elasticity can be calculated.

¹⁷ The elasticities were calculated at the sample median population. The slope was estimated for the straight line linking fertility at ± 25 percent of the median population. For deer, the elasticity shown is an average of four computed in this way; for whales, it is an average of three; and for sheep, of two.

¹⁸ He found an elasticity of 0.12, corresponding roughly to the eighth root (see Humphreys, 1885:173).

¹⁹ For present purposes Fuchs's (1980) comment on Preston is more useful because he does not include nutrition, and he estimates a separate regression for less developed countries. I could, of course, look at similar regressions for fertility, which would doubtless reveal a negative association. Once the value of time becomes associated with income and wealth, however, and the possibilities for investing in the quality of children are enlarged, the whole relation of fertility to income becomes greatly complicated, as the search by economic demographers for positive income effects in the analysis of contemporary developed countries attests.

²⁰ This could arise from an elasticity for fertility with respect to density of about -0.1 and for mortality of about -0.24 . The elasticity of the NRR with respect to density equals that for fertility plus the product of that for mortality with the average probability of surviving to the mean age of childbearing.

²¹ The effect on the annual growth rate would be about 0.01 of the percent deviation of population size from equilibrium. Letting p denote the natural log of population size, so that dp/dt is the rate of increase,

and ignoring age distribution, we would have $dp/dt = k - 0.01p + u$, where k is a constant determining population size and u represents all nondensity influences on the growth rate. For many purposes, this representation suffices and has the advantage of tractability.

²² Assuming that the disturbance time is uncorrelated over time. The R^2 depends only on the elasticity and not at all on the variance of the disturbances.

²³ Based on the approximate expression in note 20, in the case of white noise disturbances the standard deviation of the log of the size of the population is the square root of the ratio of the variance of the disturbance to 2×0.01 (see Lee, 1982).

²⁴ Homeostasis is assumed to operate through the size of the population aged 15–65. Each single-year age-specific fertility rate has an elasticity of -0.12 with respect to P , and each mortality rate an elasticity of $+0.2$. The elasticity of the NRR with respect to population size is approximately equal to the proportion surviving to the mean age of fertility times the elasticity of the force of mortality with respect to $P(15-65)$, which is added to the fertility elasticity to get the total elasticity, which is therefore near -0.25 in this case. Schedules are for single years. The mortality schedule is taken from Coale–Demeny Model West Female with $e(0) = 40$. The fertility schedule is Coale–Trussell with a mean age of 29. The disturbances are autoregressive, with $\rho_1 = 0.9$ and $\rho_2 = -0.1$. All fertility rates have the same multiplicative disturbance, as do all mortality rates, with the fertility disturbance independent of the mortality disturbance. The standard deviation of the innovation term for the fertility process is 0.08, and for mortality, 0.2; both are normally distributed. Characteristics of the disturbances are based loosely on historical European experience.

²⁵ In this simulation, the lines remain close for more than a thousand years; in others, they may diverge earlier or later. It is typical, however, for changes in relative size to be minor for centuries on end.

²⁶ They find that laboratory populations tend to be more sensitive, and more prone to cycle, than do natural populations.

²⁷ The figure shows variations in population size; variations in births or age distribution would be far more pronounced. Once these oscillations converge to the limit cycle, the period will be somewhat longer; for an analysis of this case, see Wachter and Lee (1987) and Wachter (1987a).

²⁸ The question of whether the time series of U.S. fertility can be explained in this way is addressed in Lee (1974), Frauenthal and Swick (1983), Wachter and Lee (1987), and Wachter (1987a).

²⁹ Of course, more elaborate arguments are possible, in which the rising investment in human resources is taken into account, and a pure wealth effect on fertility would remain positive.

³⁰ In agricultural production functions fit to international data for developed countries and less developed countries. The coefficient on land in Cobb–Douglas models is typically around 0.1 (see Hayami and Ruttan, 1985).

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APPENDIX: ESTIMATED ELASTICITIES OF FERTILITY WITH RESPECT TO DENSITY
(VARIOUS POPULATIONS, TIME PERIODS, AND METHODS)

Long-Run Europe

The data sources are identical to those for figure 2. The decadal rate of population growth was regressed on a constant and the natural log of real wages, using two-stage least squares with lagged growth rate, log wage, time, and time-squared as instruments, with correction for serially correlated residuals as indicated. The estimated coefficients on the log of wages were multiplied by -1.6 , to reflect the elasticity of real wages with respect to population, and divided by 0.035 , to derive a combined elasticity for the vital rates on the assumption that the crude birth rate and crude death rate were each 0.035 on average. The numbers following are the results, with the asymptotic t statistic in parentheses: England (1260–1800), -0.133 (0.6); France (1270–1800), -0.128 (3.6); Italy (1360–1790), -0.086 (0.9); Austria (1520–1760), -0.091 (0.5); Spain (1410–1610), -0.283 (1.5); Poland (1530–1800), 0.064 (0.7); Germany (1500–1750), -0.311 (0.8); all Europe (1270–1800), -0.128 (1.1).

Short-Run Europe

These elasticities are calculated from Galloway (1987: 304–330, app. tables 2.2–2.15, eq. A), where his estimates are first multiplied by -1.0 to convert to real wage elasticities and then by -1.6 to reflect the real wage–density relationship: England (1756–1870), -0.13 ; Netherlands (1811–1870), -0.26 ; Belgium (1811–1870), -0.21 ; France (1756–1870), -0.13 ; Prussia (1756–1870), -0.11 ; Denmark (1756–1870), -0.09 ; Sweden (1756–1870), -0.14 ; Tuscany (1817–1870), -0.29 ; Austria (1827–1870), -0.24 ; England (1675–1755), -0.24 ; rural France (1677–1734), -0.31 ; Minden-Kleve (1695–1751), -0.28 ; Central Prussia (1696–1755), -0.32 ; England (1546–1674), -0.28 .

Household

In the following table, the date refers to the period studied.

Reference	Country	Date	Estimated elasticity
Anker and Knowles (1983)	Kenya	1969	+0.01
Chalamwong, Nelson, and Schutjer (1979)	Thailand	1979	-0.05
Chernichovsky (1976)	Brazil	1970	-0.05
McInnis (1977)	Canada	1861	-0.04
Schutjer, Stokes, and Poindexter (1983)	Egypt	1978	-0.10
Knodel (1974)	Germany	pre-1905	-0.10
		1905–1909	-0.14
		1910–1914	-0.14
		1915–1919	-0.15
Stys (1957)	Poland	1855–1859	-0.20
		1891–1900	-0.12
		1903–1908	-0.06
		1909–1914	-0.09
		1915–1929	-0.06

Aggregate

Reference	Country	Date	Estimated elasticity
Beaver (1975)	Latin America	1950–1970	-0.06
Firebaugh (1982)	India	1961–1972	-0.24
Hicks (1974)	Mexico	1950–1970	-0.23
Kleinman (1973)	India	1951	-0.23
Merrick (1981)	Brazil	1970	-0.02
Rosenzweig and Evenson (1977)	India	1961	-0.13
Yasuba (1961)	United States	1800	-0.108
		1810	-0.089
		1820	-0.108
		1830	-0.142
		1840	-0.162
		1850	-0.115
		1860	-0.068

Large Mammals

These elasticities are calculated from data provided in an analytic survey by Fowler et al. (1980), and the references listed are those to which data are attributed (see Fowler et al., 1980, for full references). Elasticities were calculated at the sample median, and the slope was estimated for fertility at between ± 25 percent of the median population size. Each species is represented by one elasticity; this value is the average of elasticities calculated from two or more studies in the cases of deer, whales, and sheep. The species, elasticities, and references are as follows: elephant, -0.36 (Fowler and Smith, 1973); elk, -0.27 (Gross, 1969); bison, -0.45 (Gross, 1973); long horn cattle, -0.03 (Gross, 1973); dolphin, -0.85 (Kasuya and Miyazaki, 1975); harp seal, -0.11 (Lett, in press); bear, -0.36 (McCullough, in press); deer, -0.46 (Gross, 1969; McCullough, 1979; O'Roke, 1948; Teer, 1965); whale, -0.53 (Gambell, 1975, blue, fin, and sei whales); sheep, -0.36 (Geist, 1971; Woodgerd, 1964).