

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Richard Easterlin PAA President in 1978



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

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

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

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

RICHARD EASTERLIN

PAA President in 1978 (No. 41). Interview with Jean van der Tak in Dr. Easterlin's office at the Department of Economics, University of Southern California, Los Angeles, May 4, 1989.

CAREER HIGHLIGHTS: Born in New Jersey in 1926, Richard Easterlin received an M.E. (Mechanical Engineer) in 1945 from the Stevens Institute of Technology, and the A.M. in economics in 1949 and Ph.D. in economics in 1953 from the University of Pennsylvania. He was on the faculty at the University of Pennsylvania from 1953 to 1982 as Professor of Economics and, at various times, Chairman of the Department of Economics. In 1982 he became Professor of Economics at the University of Southern California in Los Angeles. [Since 1999 he has been University Professor of Economics Emeritus at USC.] From 1955 to 1962, he was also on the research staff of the National Bureau of Economic Research. He has been a visiting professor at Stanford and at the University of Washington and a fellow at the Center for Advanced Study in the Behavioral Sciences. He was president of the Economic History Association in 1979-80. Among his many publications, he is author of The American Baby Boom in Historical Perspective (1961), Population, Labor Force and Long Swings in Economic Growth: The American Experience (1968), Birth and Fortune: The Impact of Numbers on Personal Welfare (1980 and 1987), and coauthor of Population Redistribution and Economic Growth, United States, 1870-1950, Volumes 1 (1957) and 2 (1960), American Economic Growth: An Economist's History of the United States (1972), and The Fertility Revolution: A Supply-Demand Analysis (1985).

VDT [continuing "Career Highlights"]: Richard Easterlin's name has become a household word in U.S. demography, thanks to two models or frameworks he devised to explain trends in fertility and population growth. He is the progenitor of the "Easterlin hypothesis," the idea that the relative size of a cohort determines that cohort's life chances and, in turn, its fertility, which sets off self-generating waves of high and low fertility, which he feels have characterized the U.S. since at least the mid-19th century, through the post-World War II baby boom and bust, and the rising fertility he expects about now [late 1980s]. He has honed that hypothesis in many influential publications, beginning with his famous 1961 article, "The American Baby Boom in Historical Perspective" [American Economic Review, December 1961], through his provocative 1978 PAA presidential address, "What Will 1984 be Like? Socioeconomic Implications of Recent Twists in Age Structure" [published in Demography, November 1978], and on to the popular bestseller, Birth and Fortune, of 1980, with a second edition in 1987.

In addition, he and his wife and research partner, Eileen Crimmins [PAA President in 2020], are now also well known for the "Easterlin synthesis framework," or Easterlin-Crimmins model, which combines economic and sociological approaches to study the shift from high to low fertility as a country modernizes. The Fertility Revolution: A Supply-Demand Analysis, published in 1985, is one of their publications along this avenue of research.

Now, to begin at the beginning, how did you become interested in demography? You were a forerunner as a population economist, sort of in the tradition of Malthus and Spengler. They are still fairly rare in the field of demography.

EASTERLIN: I was recruited really by Simon Kuznets. He and Dorothy Thomas at the University of Pennsylvania had gotten funds from the Rockefeller Foundation for a project on population redistribution and economic growth in the United States, which aimed to mobilize economic and demographic data from census volumes, by state, since 1870. For workers on that project, they enlisted myself, Everett Lee, Ann Miller, and Carol Brainerd. That was 1953; right at the time I got

my Ph.D.

VDT: Your Ph.D. was not yet in the direction of demography?

EASTERLIN: No, it was not. And actually, it was only in Kuznets's course that I got any exposure to population as a graduate student, and even then it was not very formal demography. So my real training in demography, such as it is, was as a worker on that project, and very largely the result of Dorothy Thomas's tutelage.

VDT: Had you had courses from her as a student?

EASTERLIN: No. She really didn't teach courses particularly in demography; she mainly ran a research seminar. Dorothy's forte was always the sort of one-on-one situation. She had a regular social setup, so a lot of what I learned about the field of demography came out of the course of conversations around coffee tables.

VDT: I've always heard that Dorothy Thomas mothered her students, the graduate students that were around. So, in the course of conversation at these coffee klatches, she got you interested in demography?

EASTERLIN: Right. She guided me to the classics in the field and the great names. She got me to go to the meetings of the Population Association and attend the sessions and get acquainted with people, which I probably would not have done had it not been for her support and encouragement, because at the time there were hardly any economists at all in the Population Association. So, aside from the problem of my being a very junior person in the field, there was this additional problem of being in a different discipline and talking almost a different language. But as a result of her encouragement, I got to know people. I do the same now with my students. I make sure they go to the PAA meetings, try to give papers, and so on.

VDT: So you and Everett Lee and Ann Miller and others were taken on for this project with Simon Kuznets and Dorothy Thomas, funded by the Rockefeller Foundation, which came out eventually in two volumes, 1957 and 1960, on Population Redistribution and Economic Growth in the United States: 1870 to 1950. You wrote the chapters on manufacturing?

EASTERLIN: And income; I did the income estimates for the states of the United States.

VDT: You began working on that from 1953?

EASTERLIN: That's right. Ultimately, there were three volumes. The final volume [1964] was by Dorothy and Hope Eldridge, on migration in the United States.

VDT: Let's talk about some of the people who were at Penn at that time. You've mentioned Dorothy Thomas, who obviously was a great influence on you. You dedicated your 1978 PAA presidential address to her; she had died not long before then. And you've mentioned Simon Kuznets. What about John Durand; where does he come in?

EASTERLIN: I sort of recruited John to come to Penn. I'd known his work, of course, and had met him, but really I had not had much contact with John before the time that I recruited him, which I think was around 1960 or 61, when I was department chairman; the dates are a little hazy now. [Easterlin

was chair of Penn's Department of Economics in 1958-60, 1961-62, and 1968. John Durand was Professor of Economics and Sociology and director of Penn's Population Studies Center from 1965 to 1979.]

John, I think, felt that the United Nations was going in a different direction, and Dorothy and he had always been good friends. So I went to New York and talked to him about coming to Penn. He was head of the United Nations Population Division there and I asked him about giving that up and coming to Penn as professor of economics. Ultimately we worked it out and he came and then took over the directorship of the Population Studies Center at Penn.

It was after he came that I really had a lot of contact with him personally, in seminars and in supervising students. I learned a lot from John, in much the same way as I did from Dorothy, just sort of working firsthand with him. Those two people were the ones that for me were the role models of what great demographers should be. It involved a combination of both careful attention to data but also a concern about interpretation of the data--going beyond description and trying to get at explanation.

VDT: That's what the greats in the field seem to have been so good at. Kingsley Davis, among others, who always stresses that you have to talk about the causes, the background, to changes in fertility or whatever.

EASTERLIN: Yes.

VDT: Among others there at the time, what about Hope Eldridge? There's the sad story about Hope, who was hounded out of the United Nations during the McCarthy era [early 1950s]. I hadn't realized she'd come to Penn after that.

EASTERLIN: Yes. Dorothy was always a very loyal friend, and it was because of Dorothy that she came to Penn, first as a coworker on the project and eventually--I'm not sure what type of appointment she held, but she was teaching in sociology. I think she was quite happy. The situation that was worked out was great for Hope, because she loved to do research. I think her husband was on the faculty at Temple, so they were happy in Philadelphia; they had a pleasant setup. I think it was Dorothy's loyalty that really was responsible for sort of salvaging what could have been a very distressing ending to Hope's career.

VDT: Did she leave Penn before she had to retire? I had the impression they went south.

EASTERLIN: Yes, they did. I'm a little hazy now, but they did retire. I think Hope died shortly thereafter--I can't remember whether it was Hope or her husband who died shortly thereafter. It was a bit the same as in John's situation, because he retired and died within about a year or two. [Durand retired from Penn in 1979; died in 1981.]

VDT: Yes. Fortunately he was interviewed in this series for PAA, by Abbott Ferriss, who went to his retirement home [Spruce Pine, North Carolina] in 1979. It's a short interview, but we're glad to have it. Everett Lee was a fellow student, or contemporary, of yours at that time? He's a bit older than you.

EASTERLIN: Yes. I think at the 1953 graduation ceremonies at Penn, Ph.D. degrees were awarded to Everett, Charlie Westoff, and myself. [Everett Lee received his Ph.D. from Penn in 1952. Sidney Goldstein's was the third demography-relevant Ph.D. awarded at Penn in 1953.] Charlie and I stood next to each other in line. I had very little contact with Charlie, but I'd known him as somebody else in the area of demography at the time. It's interesting that all three of us [thinking of himself, Westoff,

and Everett Lee] graduated at exactly the same time. My degree was in economics and theirs were in sociology [as was Sidney Goldstein's]. There was no demography degree at the time. That program was really set up when John Durand came to Penn, because then we had at least two senior people in economics [Easterlin and Durand] who were interested in demography, along with a good group in sociology. Then we set up the Ph.D. program in demography.

VDT: You had sort of two divisions: the Population Studies Center and then, later, the Graduate Group in Demography. It was Vincent Whitney who applied to the Ford Foundation for funding for the Population Studies Center, wasn't it?

EASTERLIN: Right, he was director of it.

VDT: John and Pat Caldwell in their book on the Ford Foundation and population [Limiting Population Growth and the Ford Foundation Contribution, 1986] point out that Penn had some problems in getting their money because they thought you were U.S.-oriented and had been focused on migration, urbanization, and labor force.

EASTERLIN: Right, that was the Dorothy Thomas heritage.

VDT: Somehow fertility wasn't in there, which, of course, was the goal of the Ford Foundation--to bring that down in the Third World. But the monies eventually did come [see Caldwell book, pages 54-55, 65-77]. What were your connections with the Population Studies Center and the Graduate Group in Demography?

EASTERLIN: I was a co-member of the Graduate Group in Demography. I gave a course in the economics of population that was taken mostly by the demography students; there were a few economics students, but most were demography students. There were the basic methods courses that were taught by the real demographers and then my course was sort of an interpretative course, that had to do with the role of economic and other factors in affecting fertility, mortality, and migration and the effects of population change on the economy and other circumstances. And a number of my Ph.D. students were in the Ph.D. program in demography. There were some in economics and there were also some in a third Ph.D. program that I was involved in, in economic history.

VDT: Who were some of your leading students?

EASTERLIN: Oh, I don't know; I haven't tabulated that.

VDT: Some who come to the top of your head.

EASTERLIN: Well, I guess in demography, Gretchen Condran, who's now at Temple, was one. And Michael Haines, who started in economic history but ended up in demography, was one. He's just written a book with Sam Preston, based on the 1900 census. The title is something like "The Fatal Years" [Fatal Years: Child Mortality in Late Nineteenth Century America, to have been published in 1990].

A number of my students have been international. Shireen Jejeebhoy went through the demography program. She's now director of research in the family planning program in India and very active in Indian demography.

VDT: I think she's a candidate for the Council of IUSSP.

EASTERLIN: That's very likely, because she's sort of at that stage in her career now. And Wongboonsin, in Thailand; I guess he's now the associate director of the population program at Chulalongkorn University.

VDT: That name sounds familiar; haven't you coauthored articles with him?

EASTERLIN: Yes. Jack Chang, who's the director of the family planning institute in Taiwan, was a Ph.D. student of mine at Penn; Lin Chen Chang is his real name. Those are some of the more prominent people, I guess, in demography.

Then in more recent years, two students in economics who have gone into demography are Morton Shapiro, who's at Williams College, and Dennis Ahlburg, who's at the University of Minnesota. They were both Penn people in economics. One of my students here at the University of Southern California, who is just due to get her degree, is Diane Macunovich. She is going to Williams College.

VDT: She's the one you coauthored your recent PAA paper with [Easterlin and Macunovich, "Economic Status and Household Living Arrangements of Children," presented at the 1989 PAA meeting].

Now I'd like to talk about your two streams of research: the Easterlin hypothesis, which began with the U.S. experience, and your research on less developed countries. How did you come to devise the Easterlin hypothesis? And perhaps you'd give your standard, nutshell, description of it.

EASTERLIN: I think the Easterlin hypothesis is, in a nutshell, the conception that an individual's fortunes are affected by the size of the birth rate at the time they're born. Let me add that an individual's fortunes are affected by a lot of things. My argument is that in the post-World War II period in the United States, where the birth rate varied a great deal, this phenomenon has come to be especially important in affecting a lot of outcomes of people as they've grown up, and that includes social, psychological, economic, and even political aspects of their behavior.

Let's take somebody that comes from a small-birth-rate period. This tends to mean they typically are raised in small families, so there are fewer competitors for the resources of the family, including attention and love as well as economic resources, and that tends to result in the development of more positive attitudes and greater self-confidence as they grow up. In schools, in smaller cohorts the prospects of your being in smaller classes are greater and therefore having the benefit of a greater teacher-to-student ratio. The prospects of your succeeding in extracurricular activities--making the baseball team, being a cheerleader, being the editor of a yearbook, having a lead role in a play--things like that are all greater.

VDT: Did you have all those things, coming from the "Good Times" cohort [late 1920s, 1930s]?

EASTERLIN: More or less, yes. And when you get to the labor market, again you benefit from the fact that there are fewer competitors, so that it helps your wages and employment and you move up the occupational ladder through promotion more rapidly.

So, I think the Easterlin hypothesis is that there's this whole succession of effects, which go right from almost birth, in the family, educational institutions, labor market institutions, that tends to be beneficial to members of a small cohort and less beneficial to members of a large cohort.

VDT: How did you come to devise it? Your first article, "The American Baby Boom in Historical Perspective"--obviously you'd been thinking about it before--came out in 1961 [American Economic Review, December 1961]. Could you see this happening already in the mid-1950s?

EASTERLIN: I think one of the advantages one has in working in social science is that you do have the benefit of personal experience. It seemed to me that the kind of argument that I was led to through the data, having to do with the scarcity of young people, was confirmed by my own experience and the experience I observed among my contemporaries. So, although the idea didn't come directly from personal experience, it was confirmed more or less by personal experience.

VDT: By the mid-1950s you already had four children; you were adding to the baby boom?

EASTERLIN: I was working at it, but I hadn't had four by then--just two.

VDT: You're thinking more of your own social experience as . . .

EASTERLIN: Yes--or an economic sense, basically. But I never had a nicely worked out model in advance, which I then tested against the data. I had inherited from Kuznets and Dorothy Thomas this approach where one has certain theoretical ideas and one looks at the data, learns from the data, and they reshape your theoretical notions. You work back and forth between the theory and the data, using the data to guide the development of your theoretical notions.

And it was really the data that eventually led me to the relative income view, because I became increasingly disillusioned with the usual hypothesis generated by economic theory, which was that as incomes go up people want more children. I didn't disagree with that, but the argument was always based on the idea that tastes are given. And the more I looked at the data and the more I learned about the nature of the world, the more I became convinced that people's tastes themselves are shaped by social processes and that you had to build a model that took account of that process of taste formation--along with what the economists were interested in, which was what they call technically the "budget restraint."

So it was in an effort to try and reconcile the data with economic theory that I was forced eventually--and, at first, quite a bit against my will--to develop a model that built taste formation in. Even today, many economists absolutely reject the idea of trying to look at taste formation, although there is now more marginal acceptance in the field of that type of work. But back in the 1960s, it was just about verboten. And I resisted it myself for a long time.

VDT: Was that about the time that the Chicago school of household economics . . . Gary Becker . . .

EASTERLIN: Oh, yes. Gary and I were friends, quite good friends actually. We were together for a year at the National Bureau of Economic Research right around the time that I was working on the baby boom paper and he was working on the paper that he gave that was one of the first statements of the economic theory of fertility, in, I guess it was the 1960 conference of the Universities-National Bureau of Economic Research. The volume was called Demographic and Economic Change in Developed Countries.

So Gary and I used to talk a lot, and we agreed a lot. I think that we still agree a lot, because he really has now incorporated in his own work a view that, although he doesn't call it taste formation, it's endowments, and it comes often to much the same type of conceptual view: that people in their upbringing and in their origins are shaped or molded in various ways and their ultimate behavior is an outcome of that socialization experience and the environment that they're in at a given time. But a lot of Gary's students take a much more mechanistic view, in my opinion.

VDT: You mean the mechanistic, economic view that only behavior counts?

EASTERLIN: Well, it's that tastes don't change, and that seems to me to be questionable.

VDT: You're one of the economists in the field who certainly brought together the sociologists and the economists, which you and Eileen Crimmins have stressed in your Easterlin-Crimmins model. I want to ask about that in a bit, and the criticism of economists like Paul Schultz of that particular model.

It was interesting that you brought that into the Easterlin hypothesis, that your tastes are shaped by the climate, the environment, in which you grew up. I presume that you realized that you were leading the way in one of the most popular demographic research conundrums of the postwar era--explaining the baby boom, which the demographers were rather late in admitting was taking place. Even in the late 1940s--you have a fine quote in Birth and Fortune--they were apparently saying, fertility will never rise above replacement level. And, of course, in the 1930s they hadn't foreseen that at all.

EASTERLIN: They're still saying that.

VDT: Right. What do you think about what's happening now? The U.S. total fertility rate had been stuck at 1.8, but now it's crept up to 1.9. The Population Reference Bureau in their 1989 World Population Data Sheet, which is coming out in a couple of weeks, have got the U.S. TFR at 1.9; it rounded off at 1.9.

EASTERLIN: Right, it's above 1,900 [per 1,000 women].

VDT: It's above! I thought it just rounded off to 1,900.

EASTERLIN: It's about 1,920, as I recall; they rounded it down. [2.0 in PRB 1990 Data Sheet.]

VDT: So you're being justified [in his "suggestion" in his 1978 PAA presidential address, "What Will 1984 Be Like?"], that because of "amelioration in a wide variety of social, political, and economic conditions" for the smaller cohort born after 1960, compared to the preceding baby boom cohort, "there may be a substantial rise in fertility over the next decade". It [the fertility rate] is getting up there?

EASTERLIN: Not wholly. It's still confined to people who are 30 and over.

VDT: That's right--delayed childbirth. And those are the baby boomers, among whom, as you pointed out in the 1987 second edition of Birth and Fortune, in the extra chapter you added, all these adjustments have taken place--which you had projected already--deferred marriage, wives' increasing labor force participation, and reduced childbearing. It's all coming to pass. What do you feel accounts for, well, deferred childbearing among women over 30, who will probably not have more than two children, certainly?

EASTERLIN: Well, they may have more than two. The old business about two girls producing a third child will, I think, still operate to some extent. But I'm still expecting fertility to turn up among people in their twenties. It's still a little early . . .

VDT: The age-specific rates? You're expecting that to happen among the baby busters, those born 1970 and later?

EASTERLIN: Right.

VDT: Well, you're standing by your hypothesis.

EASTERLIN: My expectation was that it would have started already [before 1989]. But there are two circumstances that I think have forestalled that. One is that we experienced in the early 1980s the worst postwar depression that we had.

VDT: Recession, it's always been called.

EASTERLIN: Right.

VDT: Here's an economist calling it a "depression."

EASTERLIN: Well, Frank Levy, who wrote the 1980 census monograph on income distribution [Dollars and Dreams, 1987], refers to the period since 1973 as the "quiet depression." The recovery from that depression [of the early 1980s] was really fairly slow. Even by 1985 and 86, we still had relatively high unemployment rates, and it's only been in 1987, 88, and 89 that we started to get down to unemployment rates in the 6 percent and less area. So part of it has been that aggregate demand has been slacker than I was anticipating.

The other is a point that my coworker, Diane Macunovich, has developed in a paper with Lee Lillard at Rand. The cohort size effect in the labor markets involves not just the absolute size of the cohort, but whether you're at the leading or the lagging edge of the cohort. The ones that come later in the baby boom, the ones that are on the lagging edge, suffer not only by virtue of the size of the cohort, but also from the fact that because so many have gone before the market has become saturated.

VDT: That's interesting. I thought that was part of your thesis, but in my re-readings of your publications lately, I didn't see that. Yes, that seems very obvious.

EASTERLIN: It does, I know.

VDT: I know, because I have them. I have one born in 1956, the year before the baby boom peak, and one in 1960 and one in 1962. But to tell you the truth, the one born in 1962 is doing best of all. But then it took a lot of education and drive to get him there.

EASTERLIN: That saturation effect, you see, changes the timing of the [fertility] upturn by about four to five years. On the size of cohorts coming into the labor market, you would have predicted the start of an upturn in fertility in the early 1980s. Now, that's mitigated in part by the aggregate demand condition I talked about, and also by this saturation effect, that it takes a while for the labor market to get cleaned out from the surplus cohorts.

VDT: So her [Diane Macunovich's] projection is that it will turn up when?

EASTERLIN: Right around now [1989].

VDT: Well, the one born in 1962 was married last June [1988]. I'm giving him about another year [first child born April 1991]. When were your children born? Do you have one at the peak?

EASTERLIN: Yes. My first was born in 1955; then 1957, 1960, and 1964.

VDT: You have them spaced out to touch all the possibilities. How are they doing?

EASTERLIN: They're all doing reasonably well. The one born in 1964 is coming back in the fall to finish his degree here at USC; he's been in England for several years. He's the only one that's still at the college level.

EASTERLIN: He's bided his time while the pipeline cleared out.

EASTERLIN: Yes. Then my second family [with Eileen Crimmins] is 1981 and 1985.

VDT: Well, they were supposed to be the start of this next baby boom, upturn, but it turns out that they're the tail end of the baby busters, I mean the trough in fertility.

EASTERLIN: That's right.

VDT: I hope we all can see what happens to them.

EASTERLIN: Me too.

VDT: Talking about these qualifications as to when the upturn in fertility would happen, I suppose that is one reason you did a second edition of Birth and Fortune in 1987, only seven years after the first. You felt that epilogue was needed? Or was it selling so well that the publishers said you needed a reprint and update?

EASTERLIN: I really hadn't planned on a second edition. What happened was that I had been asked to do a paper for a Hoover Institution conference that Kingsley Davis organized [on nonreplacement fertility in industrial societies, November 7-9, 1985, leading to the 1986 Population Council publication, Below-Replacement Fertility in Industrial Societies]. That was how I sort of got back to the subject. Then I had certain disagreements with Rita Ricardo-Campbell, who was the editor at the Hoover Institution and had rather strong but not, in my judgment, well-supported views that she wanted to see expressed in my paper. So I withdrew it from that conference. At that time I had been talking with the Chicago Press and we worked out a deal, because I was very unhappy with Basic Books [publisher of 1980 edition], for Chicago Press to buy the rights to Birth and Fortune and put out a second edition, that would then include the new material that I'd developed for the Hoover conference. So it was just a fortuitous combination of circumstances.

VDT: So it wasn't that a reader coming to it cold would say, "Ah, but things didn't work out in the early 1980s the way he had projected. Therefore, he had to write this additional chapter on the ex post, relative, conditions, and how the baby boomers have made adjustments to overcome their relative income loss of status, and have ended up being slightly better off, actually, than the parent generation."

EASTERLIN: Right.

VDT: Because they deferred marriage, increased wives' labor force participation, and reduced childbearing.

EASTERLIN: Right.

VDT: How did you come to write Birth and Fortune in the first place? Did you feel that was a message the public was dying to get at and you wanted to put it out in a popular form?

EASTERLIN: Well, of course, it grew out of my PAA presidential address.

VDT: I thought it must have. There was a tremendous reaction to that address, "What Will 1984 Be Like?"

EASTERLIN: Yes. And, mainly I'd been working on fertility for most of my career and I came to realize that there were these ramifications that have affected behavior more generally than just fertility. It seemed like there was this whole syndrome of effects that stemmed from the same sort of circumstances. The basic idea was in my presidential address, but the reason for writing the book was to have the opportunity to develop it more fully--to look at some of the data more fully and some of the related literature on things like suicide that I hadn't gone into very much and try and put it together.

VDT: The first time you looked at these other things--political alienation, suicide, homicide, divorce--was in the PAA presidential address?

EASTERLIN: Right, it really was. Some things I'd done, had a bit of knowledge about. For example, Martin O'Connell, who's at the Census Bureau, had done a thesis on suicide; that may have been in the early 1970s. He'd been one of the ones who had attracted my attention to the idea that these ramifications were really broader. His thesis was really quite a good work. It included not just suicide but talked about how attitudes were affected and so on. So there were progenitors of whom I acquired some knowledge. But other things like divorce I'd really not looked up particularly.

VDT: They had not been looked at in terms of the age structure effects, no. Talk about divorce, you may be right [in his 1978 prediction of "an eventual drop below trend levels in cohort divorce rates," with the "growing relative scarcity of young adults" after 1980]. It's leveled off.

EASTERLIN: So has suicide; so has crime; so has drug use.

VDT: Not where I come from--Washington, D.C.

EASTERLIN: I can't generalize that way, from personal experience. But homicide rates have really fallen, and crime rates generally have leveled off. I think one of the complicating circumstances is the appearance of crack, which seems to be--unlike the other drugs--much more tied up with violence. So even though drug use has leveled off and declined, the incursion of crack has produced a sort of mini-wave of violence and drug use that doesn't alter the overall trend but has produced a very noticeable public effect.

VDT: These are complicating external factors which, like a new recession when it comes--the economists are beginning to say when it comes, not if it comes--would affect the chances of the baby busters in the labor market?

EASTERLIN: I think a recession is not likely to be a major one. You always have to hesitate on these projections. The last recession [of the early 1980s] was so severe because . . . It was really instituted by the Federal Reserve as a way of stopping the very high rates of inflation, upwards of 13 percent a year, that we were experiencing and with an extremely conservative and ideologically-oriented monetary and fiscal policy. The whole setup of monetary versus fiscal policy is still crazy. Anyway, we paid a very heavy price to stop inflation, but they succeeded in stopping inflation through a depression, and that's essentially what produced the depth of that depression. We're not experiencing

inflation rates of that magnitude now.

VDT: But we're still having to worry from week to week, day to day, whether Alan Greenspan is going to try to adjust things the way Paul Volcker did.

EASTERLIN: Yes. The Federal Reserve really had altered its monetary policy in a crazy way and adopted monetarists' goals. Interest rates went way up; the whole structure of monetary policy was quite foreign to what our historical experience had been. They abandoned that in the early 1980s, but they also produced a very severe recession.

We're now back to a more normal type of situation. It's still abnormal in the sense that we're still running a very large federal deficit. But I don't see a prospect of the kind of depression or recession that we produced back in 1981, 82, 83. So I would view that as a pretty temporary interruption.

The qualification to all this is what's going on internationally--the trade deficit, and the extent to which we really are able to pursue policies independently of our developed industrial partners, particularly Western Germany and Japan. If those countries continue to run extremely conservative expansion policies--and Germany is; Japan has become more moderate--if they continue to do that, it makes it harder for us to expand. So that's the qualification there; the extent to which you really get international cooperation.

VDT: So that will have to be watched, in your anticipation of increasing numbers of births?

EASTERLIN: Yes--the possibility of another major depression interrupting my baby bust fertility.

VDT: Do you feel it's important for demographers, social scientists, economists--academic researchers--to reach out to the public, as you did with Birth and Fortune and have done with a number of papers?

EASTERLIN: Well, I've always tried to write in a way that was not so technical that it was inaccessible to the public--or to scholars in other disciplines, because I feel, based on my own experience, that you want to be able to communicate to people in other disciplines, because I've benefited from their work. So I feel writing only for your own profession is not a highly desirable goal. But, on the other hand, the way economics is these days, if you don't write essentially in the highly technical jargon of economics, you're not likely to get published in economic journals.

VDT: There's nothing in the economics field equivalent to, say, Population and Development Review?

EASTERLIN: It's not even clear to me these days that there's anything in the demography field equivalent to Population and Development Review.

VDT: Well, we consider that Population and Development Review is for demographers and economists who talk about population.

EASTERLIN: It used to be that Population Studies was a journal that had articles that were written intelligibly and were of interest comparable to what PDR does, but Population Studies has become a highly technical journal. Demography is a highly technical journal.

VDT: Demography has been ever since Donald Bogue finished as editor [in 1968].

EASTERLIN: Right. They're very comparable in the field of demography to most of the journals in economics, that is, they put primary emphasis on technique rather than on substance, in my judgment. And ideas are a third order of criterion for an article in those journals. So PDR, I think, is an exceptional journal, for demography as well as economics.

VDT: Yes. I was going to mention that when I come to Paul Schultz's criticism [in Population and Development Review] of your Fertility Revolution. How did your interest in less developed countries evolve? You made some trips to India around 1969-70.

EASTERLIN: I always had dual interests in demography and economic history. When Simon Kuznets left the University of Pennsylvania, which was around the time I got my degree, I started giving the graduate course in economic development that he gave there. That is a course not in traditional economic history, but in the historical process of development of the now developed countries and including also the fortunes of less developed areas of the world. I'd always had that perspective of looking at the long-term experience both of developed and developing countries as part of this heritage of Kuznets's training, and from giving this course.

In my research, initially, I focused on the U.S. But in a course that I gave in economics of population I encompassed the literature on developing countries as well as the literature on the developed countries. The India trip was an opportunity to get some firsthand experience in a developing country. Actually, I'd been to India earlier at a conference. Again, Dorothy Thomas was my mentor. She was there at the UN population training institute in Bombay.

VDT: You went while she was there?

EASTERLIN: Right. I was only there about a week or so, but she acted as a tour guide, and introduced me to people. This was about 1965. I think there was a population conference in Delhi in 1965, and I'd gone there for that purpose.

Dorothy was a great person. We were in Bombay, the usual congested situation. I had the usual Westerner's reaction to that: So many people, how did they manage? And she said, "Well, you've got to talk to the people and understand how they feel. You'll find out that Indians if they're off by themselves often feel very lonely. They're so accustomed to having large numbers about that, contrary to the way we're brought up, they feel quite normal and natural in a congested situation."

So Dorothy did a lot to open my eyes, I think, to a less Western-biased perception of people in developing countries, and really to an appreciation of India and Indian culture. Even though I can't say that I'm anywhere like an expert or authority on it, I have a very great admiration for India, and I think it traces back to her tutelage. In 1969 this opportunity came along to go to India as a member of a family planning team to look at the problems of spreading fertility control techniques in India. It was put together by the United Nations and was a team of international experts. There were a couple of Americans, a couple of Swedes and others. The team was headed by a Yugoslav diplomat.

VDT: You say in The Fertility Revolution and in an article before then that your research was based in part on analysis of data from an Indian survey taken about 1970. Did you have something to do with setting up that survey at that time?

EASTERLIN: No, those data just happened along fortuitously. I've never really been involved in generating any primary survey data. In my course and in my theorizing, I had developed some of the ideas that appeared ultimately in the synthesis framework, and it was at about the time I went to India. The benefit that came from that trip was that I became more acquainted with what the issues of family planning policy were and was able to look more at the relevance of fertility theory to family planning

policy. One of the most recent articles I did, with Kua Wongboonsin of Thailand and another good student of mine, from USC, Mohammed Ahmed, who's gone back to Alexandria now, had to do with applying the synthesis framework to family planning policy. That appeared in Studies in Family Planning. We used data for both Egypt and Thailand, the idea being to develop a measure of the demand for family planning in Egypt and Thailand, along with estimates of service supply.

So the India trip was of value in expanding empirically my understanding of issues of family planning and helping to stimulate more theoretical analysis of fertility behavior in developing countries. This involved in part trying to reconcile the concept of natural fertility with the economist's assumption that all fertility behavior is determined by deliberate choice.

VDT: Certainly in your Easterlin-Crimmins synthesis framework you did that, trying to reconcile the sociological and the economic points of view in researching motivations for fertility decline. That approach was heavily criticized by Paul Schultz as it came out in your book, The Fertility Revolution, in his review of the book in Population and Development Review [March 1986].

On the side, I should say that Paul Demeny said [in his interview for this series] that one of his aims for Population and Development Review was that such criticisms could take place there. He felt that demographers have always been too polite about criticizing each other's research and that they take it personally, perhaps because you know each other too well. He said that never happens in the economics field--too big perhaps.

EASTERLIN: Schultz and I are both economists. [Laughter]

VDT: Okay! Perhaps you had already been brought up in that way, and had a forum where you could do it. I'm not quite clear on Schultz's criticism; it was very technical. You said in your reply ["Economic Preconceptions and Demographic Research: A Comment," PDR, September 1986], logically, that demography needs sociologists and anthropologists and the like as well as economists. And you've been in the forefront in trying to bring together these two schools. I suppose only an economist could have done it. The sociologists haven't always been taken seriously by the economists.

EASTERLIN: And the anthropologists don't take seriously the sociologists or the economists. Well, let me state very briefly what I see as a central issue, which really is an issue in economics and what is admissible evidence. Economics in the past 30, 35 years has become highly quantified, with the incursion of quantitative methods, formalized in econometric study. And this has been intimately tied up with a behaviorist conception of the study of human behavior.

The idea is that you observe what people do and draw inferences from that, and that's as far as you go with your evidence. You never pay any attention to what they have to say about why they do something, i.e., to subjective attitudes. It ties in partly with the notion of how you treat tastes. So economists had abjured evidence that has to do with statements about desired family size. They just say, "Well, you can't put any credence in that." Whereas what I tried to do in the synthesis framework was build a framework that made it possible to link such statements to economic theory.

Similarly, economists have been highly resistant to the concept of natural fertility. Again, in the synthesis framework, I tried to build a framework that made natural fertility behavior a rational outcome of certain circumstances that people would be in. Now, my reason for crediting the conception of natural fertility behavior was, again, what I referred to earlier, that I learned from looking at the evidence that demographers have produced justifying the view that people practiced natural fertility behavior in developing countries and in pre-modern times. Similarly, I've felt that if you looked at the data, the kinds of results that you got in statements about desired family size seemed to make sense.

So I felt that you had to have a theory that took account of these phenomena. But the

mainstream of economic thinking in the area of population, which was dominated by the Chicago school, rejected this type of evidence. And that view was not confined simply to the Chicago people. It's much more common in economics because of this behaviorist idea. You look at people's revealed preferences as revealed in their behavior; you don't ask them about their preferences. The result is that an enormous amount of fascinating work, some of which I'm now involved in with Eileen, that is produced by the Survey Research Center of Michigan about people's attitudes is just out of hand omitted from any sort of economic inquiry.

That's the crux of the issue that lies in Schultz's criticism of The Fertility Revolution and my response to it. The Schultz criticism essentially says the kind of evidence that I'm using is not admissible. This evidence says people make choices about marriage and breastfeeding behavior that are not geared to what their fertility consequences are going to be; rather, they're geared to other concerns. This evidence comprises the kinds of things that people themselves have to say when they're asked about why they got married at such an age, why they breastfed their kid for 18 months, and so on. Since his theoretical conception says you can, in principle, control fertility deliberately through breastfeeding or marriage behavior, therefore, people do that.

So that, to my mind, is the crux of what's going on. He's defending an attitude that I think will eventually be rejected--this behaviorist view. It's already starting to crack in economics. But it's a very strong heritage. Its origins go back to an attempt to purge economics of Marxism and social reform and preconceptions of an ideological nature. The idea was that econometrics could make economics into a pure science, devoid of any preconceptions, and, just by studying the data, you could somehow construct a theory about what's going on in the world that explains how people behave.

The trouble with that point of view is that there's no limit to what is endogenous. At the end of my response to Schultz's critique, I asked at what point is he going to start saying that something is not endogenous to the model and is, in fact, exogenous to the model. What I say is that you draw the line between what variables are endogenously determined and what are exogenously determined by what we learn from the people themselves. His view is that you can somehow tell that from the data. And there's no way that that can really be done. The econometricians--not all of whom are of that persuasion, but many of them are of this behaviorist persuasion--will never be able to construct a valid model as long as they insist on a purely behaviorist approach. So that's the issue.

VDT: Thank you for putting it that way. I think you have a lot to do with perhaps changing the views of the holdouts. Sam Preston, who happened to come out of economics but says he doesn't think of himself as an economic demographer, wrote in an article in Sociological Forum a couple of years ago ["The Social Sciences and the Population Forum," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987] that he always himself felt that demographers had to pay more attention to the softer sciences, in which he was including the sociologists and the anthropologists, and perhaps the Jack Caldwell approach, going out to villagers and living with them, supposedly, for a year and really observing what they think and do.

EASTERLIN: Indeed. I used some of Jack Caldwell's findings about the reasons why people are breastfeeding and the reasons why they get married in support of my argument that we're really talking about natural fertility behavior and not endogenous control.

VDT: It goes way back, in a sense, to Kingsley Davis's and Judith Blake's framework of intermediate variables. They have felt that in recent years their framework has stimulated the collection of data just on the proximate determinants, intermediate variables, of fertility.

EASTERLIN: Yes.

VDT: In the World Fertility Survey and also in the National Survey of Family Growth--perhaps it hasn't in the latest one, but it certainly did up through Cycle III. And not enough on collecting the background data, which works through the intermediate variables, breastfeeding and marriage.

Do you think your research could lead to policy to influence the threshold of fertility regulation--in other words, to lower family size desires before full modernization? Now, India has had, at least on the books, laws to delay marriage. With breastfeeding, of course, it's not very likely you can do anything through policy.

EASTERLIN: Well, my view is that the essential pre-conditions for a successful family planning policy are to establish a real demand for family planning, and that the measures most conducive to that are social measures not economic measures: public health measures that lower infant and child mortality, and universal education. Most of the evidence suggests that these social dimensions of modernization are the ones that push families most rapidly into the situation where they're likely to produce more surviving children than they want. And once they're in that situation, then family planning measures can work.

VDT: Something like the state of Kerala in India?

EASTERLIN: Right, that's a good example of a place where social programs have affected fertility behavior.

VDT: It has stressed public health and education, and it still has a very low GNP per capita, but it has lower fertility than other states of India.

EASTERLIN: Right.

VDT: Talking about family planning success or lack of success, what do you think of the current world population growth trend? The growth rate seems to be stuck. Quoting again the Population Reference Bureau World Population Data Sheet--though you're not supposed to use them as a time trends series--they have the world growth rate inched up to 1.8 percent this year; it was 1.7 percent in their 1988 Data Sheet. And, of course, you know that the United Nations in their 1988 assessments jacked up their medium variant and all their projections for the year 2000 and 2020. In other words, we're not doing as well as we had hoped.

EASTERLIN: Well, I have just recently been involved in a conference on European population, sponsored by the Council on Europe and the European University Institute. Live-Bacci was one of the organizers.

VDT: Toward the end of last year; Ansley Coale was to go.

EASTERLIN: Ansley Coale and Paul Demeny were there, although there were probably only four or five Americans; mainly Europeans. The Europeans, of course, are deadly afraid of lack of population growth. My general attitude is that the concerns at both ends of the spectrum are largely illusory, and that the problem in today's developing countries is they're the victims of worldwide economic breakdown that has partly been sown by the monetary and fiscal policies pursued in the developed countries. So, as their trade circumstances have been severely adversely affected, that's forced them to indulge in budgetary cutbacks and so on. They're less able to pursue their social programs as well as their development programs. And one of the consequences is that the fertility transition is correspondingly slowed. But I see this as not a persistent phenomenon. I don't see the conservative

reaction that's dominated the world scene over the past ten years as more than transient.

VDT: You think there's going to be a turnaround in this conservative view and we'll get back to what all our Californians are worried about?

EASTERLIN: Clearly Bush is not the ideologue that Reagan was, and Thatcher's days are numbered. [Prescient!] It depends what your temporal horizons are. I think in terms of periods of five and ten years.

VDT: Five and ten years; you consider that long?

EASTERLIN: Yes, when I talk about change. I don't anticipate that the conservative ideology that's become popular is likely to persist with the strength it has through the 1990s.

VDT: Will that also turn around the low fertility of developed countries?

EASTERLIN: Well, I'm not concerned about it. But, of course, I don't project the low fertility that people do for the developed countries, so I don't have to worry about it. In the paper I did for this Florence conference, I looked at the historical experience of ten European countries that are in the forefront of modern economic growth. When you look at long-term economic growth, per capita income growth, and population growth; when you look at future prospects of dependency, and so on--it's hard to see where such unusual circumstances are in prospect. That the older population is going to be such a pressing burden on the economy.

VDT: What about fertility, which has got to go up if that is to come about?

EASTERLIN: Well, the last time we had a fertility boom we were experiencing high economic growth. My expectation is that productivity growth is going to go up; that economic growth rates will go up; that aggregate demand will be more fully implemented through monetary and fiscal policy; that baby bust cohorts will be coming on the labor markets; and that we will be able to enjoy the benefits of higher fertility--of a transient nature--and of a larger old-age population, which will have a larger number of workers to support it, by virtue of the baby boom in prospect.

VDT: The fertility will have gone up previously?

EASTERLIN: Right.

VDT: Well, you may be right. Again, in the latest PRB Data Sheet, the latest total fertility rate of Sweden--well, in the proofs it was rounded up to 2.0, but at the last moment, Carl Haub decided--he said, "Don't tell Easterlin that"--it was going down to 1.9, but still higher than it has been. Now the low one is Italy, 1.3; it's lagged behind northern Europe.

EASTERLIN: If you look at completed fertility rates in these countries, and people's expectations about completed fertility rates, it's clear that the very low TFRs, like Italy's, tend to be an exaggeration of what their completed fertility is likely to be. Calot, I think it is, had a paper in Population Studies that showed that the completed fertility rates would be around two; I think this was just for France. He also had a little piece in PDR. As I judge it, among most of the European experts, the tendency is not to assume that completed fertility is really going down anywhere as low--even for these low-fertility age groups that we're now observing--as the TFR implies. It's part of this problem with the period rate

giving a distorted impression; completed rates tend to swing to a much more moderate degree than the period rates.

VDT: What are you doing now? You had your interesting paper at the PAA meeting this year on children in poverty ["Economic Status and Household Living Arrangements of Children"], which made the front page of the second section of the Wall Street Journal, in Alan Otten's column. He was the one journalist that PAA brought in officially this year. I've been sort of involved through the years with the effort to get more PAA papers into the press, but they're usually not official. Reporters come on their own; Randy Schmid of Associated Press was there this year, on his own, though I didn't see any results from that. But you made it, and you were saying that child poverty isn't as bad as Don Hernandez, for instance, who followed you in that session, makes out--in part because families have adjusted, even single-parent families have adjusted, by moving in with other adults and so on. Is this some of the research you're doing now?

EASTERLIN: Yes. This argument about children and another article that I published in PDR on age structure and poverty ["The New Age Structure of Poverty in America," Population and Development Review, June 1987] are really connected to that last chapter in Birth and Fortune. They all form the genesis of a more general inquiry into the relationship between demographic behavior and income distribution in the United States. The question basically is one of how people's life-cycle demographic decisions alter their economic status--decisions about forming or breaking up unions, having children, about spouses working, about establishing independent households, retirement--this succession of decisions affects the economic status of people.

What this paper shows, like the chapter in Birth and Fortune, is that people have made these decisions often in a way that helps to maintain and preserve their economic status. If you look, for example, at how well off the baby boomers now are, they really have improved their circumstances compared to their parents at the same age. And they've done it very largely through these demographic devices, because the labor market alone would have put them in a much worse situation. But they've compensated, because they have very strong aspirations for goods. They've foresworn a lot of much more traditional family types of circumstances in order to achieve more goods for themselves.

VDT: And you think that the results are going to be psychological stress, because they've given up . . .

EASTERLIN: They do have psychological stress, yes. Most of the baby boomers you talk to--at least the ones I talk to, but maybe I get a selected sample--all subscribe subjectively to my argument.

VDT: That things are tougher for them?

EASTERLIN: Yes.

VDT: They've had to put off marriage and having children, or as many children as they wanted. Well, perhaps you're right.

EASTERLIN: Anyway, the idea of that project is to try and look at the distribution of income in the United States over about a 25-year period in a way that looks at the different age groups and looks at the way their demographic decisions have affected their situation and the distribution of income.

VDT: You're doing this in this year that you're having a sabbatical? You did come to the University of Southern California in 1982; seven years later you get a sabbatical?

EASTERLIN: Yes.

VDT: There's just this little core group of you at the University of Southern California--David Heer, yourself . . .

EASTERLIN: Van Arsdol, Judy Treas has been here; she's leaving now [to UC, Irvine], and Eileen Marcus Felson--he's in criminology, but he has a demographic interest.

VDT: Is it just because you're here that the University of Southern California has a demographic kernel?

EASTERLIN: Don Van Arsdol established the Population Research Lab here and has always been instrumental in trying to engender a program of some magnitude.

VDT: What is your connection to the Research Lab?

EASTERLIN: I'll participate in their seminar intermittently and then usually I end up with a student or two whose thesis or research work I set up.

VDT: But your primary appointment is with the Department of Economics?

EASTERLIN: Right. But that was true at Penn. It's just that they had a separate graduate program in demography there and they don't have a separate one here; they have a program in sociology. So the students here who are interested in demography end up doing a Ph.D. in economics or in sociology, but they tend to have similar types of training in demography. So my Ph.D. students in economics, like Diane Macunovich or Mohammed Ahmed, the Egyptian I mentioned, have taken courses with David Heer and others in population as part of their economics training.

VDT: What accomplishments in your career have given you the most satisfaction? I think the answer is pretty obvious. And that ties up with, which of your publications do you consider most important and why? Now, of course, you've had your two streams of research and publishing.

EASTERLIN: I guess the principal accomplishments are represented by Birth and Fortune and The Fertility Revolution. Each of them represents what I consider to be a step forward in theory, in the interpretation of demographic behavior, that is tested in some degree against the empirical evidence--that it grows out of the empirical evidence and is consistent with the empirical evidence. I think I would consider those to be the principal ideas . . .

VDT: The principal ideas, and accomplishments, of your career? That seems very logical.

INTERRUPTION

VDT: You say you have an allergy. Is that from the smog?

EASTERLIN: No. I guess it's somewhat better out here. In the East it was ragweed and dust and all those things. Out here it's because they irrigate so much. There's so much pollen in the air and things are pollinating so frequently that the allergy situation is not much different from the East. In the East when the ragweed season came, I was always in a terrible state, and when the heat came on in the winter, that was terrible. But out here, I'm sort of at a level plateau, more or less.

VDT: Did you come West in part because of change of climate?

EASTERLIN: Well, yes, because of the change of climate, not because of the allergy dimensions. Yes, it's just much easier to live out here, and especially to raise young children--a lot easier.

VDT: Who approached whom?

EASTERLIN: USC.

VDT: They came after you? You decided it was time for a change after a lifetime in the East?

EASTERLIN: Right.

VDT: Now I'd like to get onto your recollections of PAA. You've already said that you went to your first meeting because of Dorothy Thomas's inspiration. Do you remember when that was?

EASTERLIN: It was in Providence; I don't remember what year it was.

VDT: Providence wasn't until 1959. You gave me the impression she got at you sooner than that.

EASTERLIN: She probably did, but that's the one I remember most.

VDT: What do you remember about it?

EASTERLIN: I just remember being there, that's all.

VDT: Can you remember the luminaries at the early meetings you attended?

EASTERLIN: The people I remember that were the leading figures were people like Irene Taeuber, Frank Notestein, P.K. Whelpton, whom I remember seeing at an early meeting of the American Economic Association, John Durand, Kingsley Davis, Ronald Freedman, Ansley Coale. I guess those would be the people that I remember most, and whose work I knew most.

VDT: Can you remember when you gave your first paper at PAA?

EASTERLIN: No, not really. It might have been at Providence. [The first Easterlin paper listed in meeting programs in the PAA archives, which lack the program for 1961, was the 1964 paper noted below.]

VDT: Already that first year?

EASTERLIN: Well, we started the population redistribution project at Penn in 1953, 1954, and we gave papers, I think, at various meetings thereafter.

VDT: We, meaning Dorothy Thomas . . .

EASTERLIN: It was usually Everett, Ann Miller, Carol Brainerd, and myself; we would be doing papers of some sort. That was my early involvement in professional meetings and in Population

Association meetings.

The early PAA meeting that stands out most in my mind, where I can remember giving a paper, was in San Francisco, when I gave an early version of "The American Baby Boom in Historical Perspective" ["Interrelations Between Swings in Demographic and Economic Growth"].

VDT: That was in 1964. One question I forgot to ask was about your experience at the National Bureau of Economic Research. Where is that?

EASTERLIN: At the time it was in New York. I was there for a year, but most of the time I was connected with the National Bureau [1955-66], I was in Philadelphia. But I had an assistant working in New York, and I would go in about once a week to meet with her.

VDT: Tell me about that. Is that a think tank for economists, or is it a policy group?

EASTERLIN: The National Bureau of Economic Research was the first major non-profit organization in economic research. It was founded back in the early 1920s right after World War I by several of the great scholars in the field who believed in the importance of empirical inquiry as opposed to the primarily theoretical emphasis in economics. So it established a tradition of its own research program in empirical work. The business-cycle program was the best known, and is embodied today in the government's economic indicators.

When I went there, although initially I was involved in sort of the aftermath of the Kuznets-Thomas population redistribution project, it was to follow up more the question of long-term fluctuations in economic growth and its relation to population. It was while I was connected with the Bureau that I really did the work that came out in "The American Baby Boom in Historical Perspective."

VDT: Here's my copy from my student days at Georgetown in 1968-70; it's written all over the place. [Laughter] It was certainly a dynamic article. So you went one day a week to New York. You were teaching at Penn and commuting back and forth?

EASTERLIN: Right, but one of the advantages of the National Bureau of Economic Research affiliation was that they paid half my salary, so it cut my teaching load in half. It helped me pursue a research career much more fully than if I'd been on a full-time teaching appointment at Penn. So that was a strong benefit, plus the people that were there. Now the National Bureau of Economic Research is under the directorship of Martin Feldstein, former adviser to President Reagan. It's become more mainstream, and it tends to work now more on policy issues. It's changed its character somewhat, but it's still probably the premier research organization in economics, outside of universities.

VDT: Let's go back to PAA. Your first meeting, probably, was at Providence in 1959, and just 19 years later, you had risen up the ranks to become president. Were there any issues along the way to 1978 that you were particularly struck by? For instance, were you caught up in the Concerned Demographers issues of the late 1960s and early 1970s--what some people felt were efforts to politicize the Association, to get it to speak out on policy issues--and the Women's Caucus?

EASTERLIN: I wasn't particularly caught up in them. I guess I didn't hold views as strongly as Otis Dudley Duncan. And it had been Dorothy Thomas's view that the organization was primarily an organization of scholars, whose concern was research, and should not be involved in the promotion of any particular policy view. But Dorothy's attitude was a product very largely of the early days of the PAA, when there was a struggle about whether it was going to be an organization to promote family

planning or an organization devoted to scholarly research. She'd always been dedicated to fundamental research, and that's why she and Kuznets were connected, also. They were both students of Wesley Mitchell, who was the one that started the National Bureau of Economic Research, so it went back to Columbia University, naturally. I was part of that heritage and although I didn't become very active in the debates in the Population Association, my sympathies continued--continued then and continue now--to lie in the view that this should be an organization devoted to scholarship and it's not primarily promoting any particular policy position.

VDT: Well, those debates seem to have abated. Now 1978. You've pointed out that your address brought together many of your ideas on the Easterlin hypothesis, and it led to Birth and Fortune. I've got down here that you stuck your neck out with your predictions and, of course, everyone has been waiting to catch you out on that, but we've discussed that. It was a very provocative speech. You realized that it probably would be?

EASTERLIN: Yes. Well, I thought it was a breakthrough of some sort for me, in bringing together a lot of things that had been lying around loosely and not put together coherently in my own mind. So I felt real good about that one.

VDT: And you felt that was a forum for such a speech? People seem to have differing views on what PAA presidential addresses should be. In many cases, it is indeed sort of a culmination of a lifetime of research, bringing it all together--at least, lifetime up to that point. In other cases, some of my interviewees have felt that they should speak out on policy issues. For instance, Ansley Coale's address, ten years earlier, was on "Should the United States Start a Campaign for Fewer Births?" That was just at the height of the Paul Ehrlich population-explosion furor. The gist of his speech was that, no, because the birth rate had already started down without any policy. That talk was quite aside from his regular research. What was your view of that? Obviously, you opted to give a research-culmination kind of address.

EASTERLIN: In my view, it's sort of laissez faire--people should do what they feel moved to do.

VDT: In your case, of course, it struck policy too--an issue that Americans are very aware of. American Demographics, for instance, that's sort of based its whole existence on the existence of the baby boom, was taking off about then.

EASTERLIN: Right. But my own interests have never been that much in policy applications. Again, it's this heritage of Kuznets and Thomas. I believe before you can design appropriate policy, you've got to answer a lot more fundamental questions about what determines behavior, and I prefer to work on those. But that's not to preclude others from working on policy.

VDT: Do you regret the changes in PAA meetings, which have now exploded to 84 sessions, in the last New Orleans [1988] and Baltimore [1989] meetings, with eight overlapping, and going on through Saturday afternoon? The attendance was close to 1,200 [1,193] in Baltimore this year. And the split offs--there were the economic demographers who split off, well, you can't say that--accompanying workshops.

EASTERLIN: The economic demographers part I can quickly respond to. I had been an opponent of separate meetings, because I felt--for the reasons I expressed early on--that economists ought to be intimately involved in the work of the PAA. They didn't have a separate workshop this year.

VDT: Yes, why was that?

EASTERLIN: I don't know, because I don't really concur with the conception of a separate workshop. I've never attended those sessions, because I think that economists ought to work through the regular PAA sessions, and that they ought to be in direct contact with real demographers, rather than maintaining what to some must appear to be an elitist club, talking among themselves. So I'm in favor of PAA trying to assimilate different disciplines within its framework, as I think it has to a remarkable degree.

And I'm in favor of maximizing participation of the membership, as I think PAA has done much better than the other associations that I'm familiar with--the Economic History Association and the American Economic Association. When I was president of the Economic History Association [1979-80], I tried to use some of the techniques that are employed by the Population Association to involve members more fully in the determination of the program and in participation in the sessions.

VDT: What techniques, for instance?

EASTERLIN: Well, the PAA runs this solicitation of what session topics there should be and then divides it up, announces it, and people then submit abstracts or papers for consideration and the chairman of the session typically tries to make a serious judgment on which papers should be included.

VDT: That's not done by the Economic Association, for instance?

EASTERLIN: It may seem obvious to you that this is the way to run it, but it wasn't. The way these organizations typically have been run is that the president-elect or somebody would say, "These are the topics I'd like to see talked about, and let's have so-and-so be the chairman," and the chairman in turn calls up two or three friends and says, "How about writing papers on this topic." That's putting it a bit in the extreme, but it's not much of a difference. Whereas, the PAA program is more or less a reasonable representation of what the membership is working on. That's not at all true of the way the Economic History Association does programs. Actually, after I was president of the Economic History Association, they continued some of the devices I brought in from the PAA, so I'm pleased about that. Another one is that the PAA has limited the frequency that a person can be on the program, as a way of making it possible for more people to be on the program.

VDT: Other organizations don't do that?

EASTERLIN: No. There are all these techniques that the PAA has worked out that make it much more responsive to the membership, I think.

VDT: Why is that? I hadn't realized how unique the PAA is. Well, we all think of it as a special organization, but I happen not to belong to other big ones.

EASTERLIN: I'm not that closely or intimately aware of what the sociologists do, and it may be that the PAA's format is more like the American Sociological Association. I think the sociologists tend to be somewhat more democratic, less elitist, because they're much more aware professionally of such issues. Economists don't have any theoretical conception of elite, although obviously they have a practical conception; there's a whole status hierarchy in economics, but there's no theoretical notion. Whereas the sociologists are driven more to purge themselves of elitist tendencies, I think. It's possible that the PAA, because of the sociological origins of most of its membership, has been more democratic in the way it's gone about its business.

VDT: But you just suggested that they also would be conscious of an elite. Which you could perhaps say is still true of PAA, which after all only has about 2,600 members [2,679 at end 1989; 2,752, end 1990]. It's fluctuated about that number since the mid-1970s, which is very small compared to other professional organizations.

EASTERLIN: Not compared with the Economic History Association, which is maybe 1,200-1,500.

VDT: Okay. You feel, however, PAA has been quite democratic; it's encouraged participation by anyone?

EASTERLIN: Yes. And democratic in its interdisciplinary attitude. It's been good in this respect. I have some reservations about its greater involvement in the Washington scene.

VDT: You mean the Population Affairs Committee?

EASTERLIN: Right. I have doubts about how much it should move in that way. I think working in concert with the American Economic Association and the Sociological Association for the promotion of scholarly research is appropriate. But . . . I really don't follow much what goes on in Washington. Maybe it's one of the benefits--or costs--of being in Los Angeles. By and large, I consider the PAA to be much more a role model of the way a professional organization should operate than has been true of the ones I'm familiar with.

VDT: That's a great observation! Even with the 84 sessions at meetings, eight overlapping, you still don't . . .

EASTERLIN: That doesn't bother me. The Association is there for the dissemination of scholarly knowledge and if people are willing to go and attend sessions, that's fine. Indeed, in my view, given the rate structure of the airlines now, since Saturday night is a good night to stay over, there's no reason they shouldn't run sessions that take advantage of that.

VDT: Through Saturday afternoon. Although I don't think they're too well attended; people have gone out on their sightseeing.

EASTERLIN: Yes, but that's because they cut them off so early. I think it's conceivable, if anything, they might start later and continue through the weekend. Start on Thursday afternoon, or something like that. But I don't have strong views.

VDT: A couple of final questions. What do you see as the outlook for demography in this country? Is there still room for basic research demographers, as you have been, or are the jobs now all for applied demographers?

EASTERLIN: I think the outlook is good. I think most of the appointments will continue to be of people who are in sociology and, to some extent now, in economics. I mean the people who conduct research.

VDT: Will have to have degrees in one or the other of those?

EASTERLIN: Right. And I think that's appropriate. I think they ought to have training in the parent

disciplines, so to speak. I think the advances in the field, the development of the computer and of microdata sets, set in motion a whole new wave of opportunity to be mined. Even with the waning of the population problem worldwide, I don't anticipate that this is going to mean any serious lack of funds for basic research in demography.

VDT: And, of course, it takes funds to make research happen.

EASTERLIN: Right. But I think demographers won't be competing at NSF [National Science Foundation] and at NIH [National Institutes of Health]. NIH [funding] has been a windfall, to some extent, for demographers.

VDT: NICHD [National Institute of Child Health and Human Development] you mean?

EASTERLIN: Right. And to some extent NIA [National Institute on Aging] now. For a long time, it was Jerry Combs [former Chief, Behavioral Sciences Branch, Center for Population Research, NICHD]. I think demographers have done well.

VDT: Although the percentage of NICHD's approved proposals that they can fund is now down to about 16 percent.

EASTERLIN: But it's still a source of funding that is not available to non-demographers.

VDT: It's much better for demographers than it is for others?

EASTERLIN: Economists have only gotten belatedly into the act. But demographers had this source of funding at the National Institutes, along with the opportunity for going in under the sociology program in the National Science Foundation. So, although the funds may not be as great over the longer term--who's to say? They may be better.

VDT: I think that's a good place to stop. Your plans for the future? Take advantage of some of these research funds? You've already said you're into a lot of research still; you're still working flat out. And there's still your new family of latent baby-busters, two children, to raise.

EASTERLIN: Right.

WHAT WILL 1984 BE LIKE? SOCIOECONOMIC IMPLICATIONS OF RECENT TWISTS IN AGE STRUCTURE*

Richard A. Easterlin

Department of Economics and Population Studies Center, University of Pennsylvania, Philadelphia, Pennsylvania 19104

Abstract—Since 1940, under conditions of restricted immigration and high and sustained growth in aggregate demand, shifts in the relative number of younger versus older adults have had a pervasive impact on American life. Before 1960, younger males were in increasingly short supply and their relative economic position substantially improved; after 1960, the opposite was true. Since the early sixties, as the relative condition of young adults has deteriorated, marriage has been increasingly deferred and fertility reduced. The labor force participation of young women has risen at above average rates, and that of older women has risen at below average rates. Changes in the age structure of the working age population have also contributed to a combination of rising unemployment and accelerating inflation. Cohort divorce rates, suicide among young males, crime rates, and political alienation have worsened. The rise in college enrollment rates has been interrupted, and SAT scores have declined. In contrast, in the period 1940–1960, changes in these various magnitudes were typically of a more favorable sort. The United States is now at the start of a new period of growing scarcity of young adults as a result of the birth rate decline that set in after 1960. This implies that the 1980s will see a turnaround or amelioration in a wide variety of these social, political, and economic conditions, some of which have been taken as symptomatic of a hardening social malaise.

INTRODUCTION

The period around 1960 marked a turning point in many aspects of American experience. I need not rehearse for this audience the precipitous decline in fertility rates that has since occurred. Also well-known is the dramatic shift in the age pattern of increases in female labor force participation rates—the acceleration for younger women and slowdown for older women. But beyond these developments, there have been other major economic, social, and political changes. On the economic side, the unexpected combination of rising unemployment and accelerating

inflation has proved a source of embarrassment to economists (and, thereby, a delight to many noneconomist colleagues). On the social scene, there has been an acceleration in divorce, a rise in suicide rates among the young, and an upturn in crime rates. In the political arena, there has been a growth in alienation from the established system. Some of these developments have been taken as indicating a growing social malaise. Long-established cultural attitudes, too, appear to have changed. There has been growing antipathy toward childbearing and, more generally, toward population growth, and a questioning, as never before, of traditional women's roles. Academic life, a central concern to many here, has been shaken by many of these developments

* Presented as the Presidential Address to the Population Association of America at its annual meeting in Atlanta, Georgia, April 1978.

and others closer to home, such as the unexpected break in the rise of college enrollment rates and the steady decline in the SAT scores of students entering college.

I think it is possible that these seemingly disparate developments—and, in fact, many others—are, in part (and I stress, in part), due to a common cause, to a new type of relationship between population and the economy centering on changes in the age structure of the working age population. These age structure effects operate largely via age-specific rates. Because of this, they have been overlooked in demographic research that focuses almost exclusively on compositional effects of age structure changes. And, if I am correct about the importance of shifts in age structure, then the near-term future will be different, perhaps strikingly so, from that foreseen by those who are extrapolating recent changes into the future.

My interest is not in year-to-year fluctuations or in very long-term trends, but in so-called long swings or Kuznets cycles, heretofore of 15 to 25 years' duration, that have marked the historical record of U.S. economic and population change since at least early in the nineteenth century (Kuznets, 1958, 1961; Abramovitz, 1961, 1968; Easterlin, 1968). Hence, the "1984" in my title—I am interested not in where we will be next year or in the year 2000, but a decade or so hence. Thus, 1984 stands as a convenient symbol for the 1980s.

The analysis rests on the view that any projection of the future must be based on a theory consistent with the past. In consequence, I shall spend the bulk of my time on the nature and causes of past experience.

I shall range over several disciplines. In so doing, I follow in the tradition of a great social scientist who introduced me to demography, and whom I miss very much, Dorothy Swaine Thomas. Throughout her career—in her work on social aspects of the business cycle, on Swedish population and industrial-

ization, and on U.S. population redistribution and economic growth—Dorothy Thomas went beyond the usual confines of demography. It seems most appropriate, therefore, that this talk be dedicated to her. I should also like to acknowledge two other mentors who have perhaps unknowingly left their mark on this talk, Simon Kuznets and the chairman of this session, John Durand. Finally, I must admit what you will see shortly when we come to the heart of the analysis, that I have drawn freely on recent work of other scholars.

THE BREAK WITH PAST EXPERIENCE

Let me start with the assertion that the 1940s marked the dawn of a new age in the relation between population and the economy. Chart 1 summarizes the essential features of the pre- and post-World War II contrast. The main point of the chart is that a reversal has occurred in the long swing roles of aggregate demand and labor supply. Before World War II, swings in labor supply and, thereby, population arose chiefly from immigration and occurred usually only in response to corresponding major swings in aggregate demand. Now, sizable swings in labor supply, involving concurrent changes in the proportion of young to old in the working age population, occur as an echo of prior movements in the birth rate. As a result of government management, aggregate demand fluctuates only mildly compared with its fluctuations in the past. Thus, changes in the supply of labor now occur largely independently of aggregate demand. Before World War II, aggregate demand was the active factor in long swings, and labor supply was a passive factor; at present, the opposite is largely the case. Associated with this change in demand/supply relationships has been an approximate doubling of the duration of long swings.

What is the cause of this new era in the relation between longer term economic and demographic swings? Three causes can be given.

Chart 1.—Contrasting Patterns of Economic and Demographic Long Swings Before and After World War II

	Before World War II	Since World War II
Aggregate demand	Active role: Private investment booms initiate a major swing in aggregate demand independently of labor supply conditions.	Passive role: Relatively high and sustained growth in aggregate demand maintained by monetary-fiscal policy.
Labor supply	Passive role: Swings in labor supply occur because of immigration movements induced by aggregate demand swing.	Active role: Swings in labor supply and in proportion of young to old in the working age population occur independently of aggregate demand as a lagged effect of birth rate.
Duration of swings	15–25 years	35–40 years?

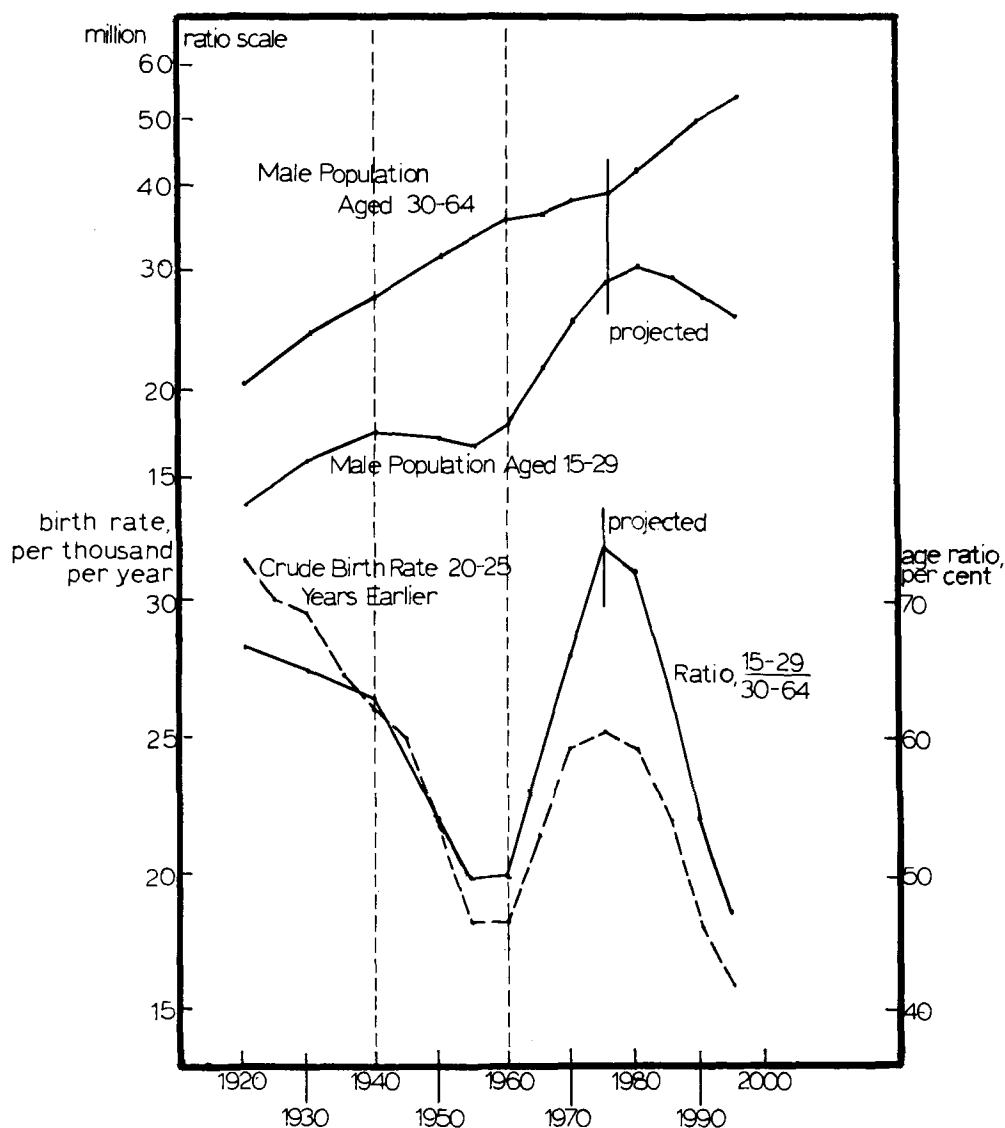
First, severely restrictive immigration legislation of the 1920s meant a sharp curtailment in the labor reserve that had traditionally supplied the demands of major economic booms. Thus, in the 1940s and 1950s when labor force growth from native sources was at an all-time low, a massive influx of immigrants to satisfy the labor demand of a major economic boom did not occur as in the past. In effect, the role that immigration had served to buffer the impact of economic booms on the native population was eliminated by the restrictive legislation.

Second, the Employment Act of 1946 committed the federal government to maintaining a high and growing level of aggregate demand through monetary and fiscal policies. Also, the substantial rise in the government's share of the GNP after World War II compared to its share before that time helped to stabilize aggregate demand. Between 1942 and 1974, the annual unemployment rate rose above 6 percent in only two different years, and even then by less than one percentage point. Compared with any prior 30-year period, this is an unprecedented stretch of relatively uninterrupted growth in labor demand.

Finally, and most important for the present purpose, the declining birth rate of the 1920s and 1930s and the rising birth

rate of the 1940s and 1950s caused, with a 20- to 25-year lag, first a growing scarcity of younger workers and then a growing abundance. This is shown in Figure 1. (The underlying data for Figure 1 and all subsequent figures appear in Appendix A.) Note that between 1940 and 1960 there is a noticeable interruption in the pre-1940 growth of the population aged 15 to 29; after 1960, growth is resumed at a more rapid rate than in the pre-1940 period. This pronounced fluctuation in the growth of the younger age group is not matched in the curve for the older group. As a result, the proportion of younger to older working age population, the solid line at the bottom of the figure, shows a sharp fluctuation in the period after 1940. Through the late 1950s, younger persons are growing in relative scarcity; subsequently, there is a growing abundance of younger persons. The broken line curve in the bottom half of Figure 1 shows that this swing in the proportion of young to old is largely due to a corresponding movement in the birth rate 20 to 25 years earlier.

These shifts in age structure—in the proportion of young to old in the working age population—under conditions of high and sustained growth in aggregate demand and restricted immigration have had ramifications that we are only now



Source: Appendix Table 1.

Figure 1.—Male Population Aged 15 to 29 and 30 to 64, Actual and Projected, 1920–1995, and Crude Birth Rate 20 to 25 Years Earlier

starting to appreciate. Let me turn to the nature of these effects and the causal mechanisms at work.

EFFECT OF AGE STRUCTURE ON THE RELATIVE ECONOMIC POSITION OF YOUNG MALES

Consider first the effect of these age structure shifts on the relative economic

position of young males. Not all age structure effects work through relative income, but many of them do. To make the argument as clear as possible, I shall make some very simple assumptions.

The essence of the reasoning is outlined in Chart 2. To start with, disregard females and suppose that the labor supply consists of only two types of labor—

younger and older males—as in the upper panel. Younger males are a relatively inexperienced and low-skilled group that is fairly new in the labor market, with rather tentative job commitments. This group, a “career-entry” group, is engaged in a considerable amount of job search with consequent high job turnover. Older males are an experienced, skilled group that occupies higher level career jobs and has relatively low job turnover. The degree of substitutability between these two groups is low, but not zero. Both groups are fully in the labor force; that is, their labor force participation rates are close to 100 percent.

Now, assume that the growth in the economy’s aggregate demand for labor comprises some normal division between

younger and older workers. (One need not assume that the demand for each group grows at equal rates, only that for each group the rate of growth in demand remains constant over time.) Suppose, now, that every two decades or so a substantial shift occurs in the relative supply of younger versus older workers, reflecting corresponding changes in the working age population. In one period, younger workers are relatively scarce, in the next they are relatively abundant.

Given the steady growth in demand for both groups, these changes in the supply of labor will create imbalances in the labor market for younger and older workers. What will be the consequences of these imbalances? As shown in the upper panel of Chart 2, a scarcity of younger

Chart 2.—The Easterlin-Wachter Model: Ceteris Paribus Effects of Shifts in Relative Scarcity of Young Adults

Assumption A: Working Age Population Comprises Only Younger and Older Males

Independent variables

Labor demand: growing at trend rates for both groups

Labor supply: large shifts in younger versus older males due to corresponding shifts in the working age population

Adjustments in

dependent variables

	<i>If Scarcity of Young</i>		<i>If Abundance of Young</i>	
	younger males	older males	younger males	older males
Wages	+	—	—	+
Unemployment rates	—	+	+	—
Occupational mobility	+	—	—	+

Assumption B: Working Age Population Comprises Younger and Older Males and Females

Independent variables

Labor demand: Growing at trend rates for all four groups

Labor supply: large shifts in younger versus older persons due to corresponding shifts in the working age population

Adjustments in

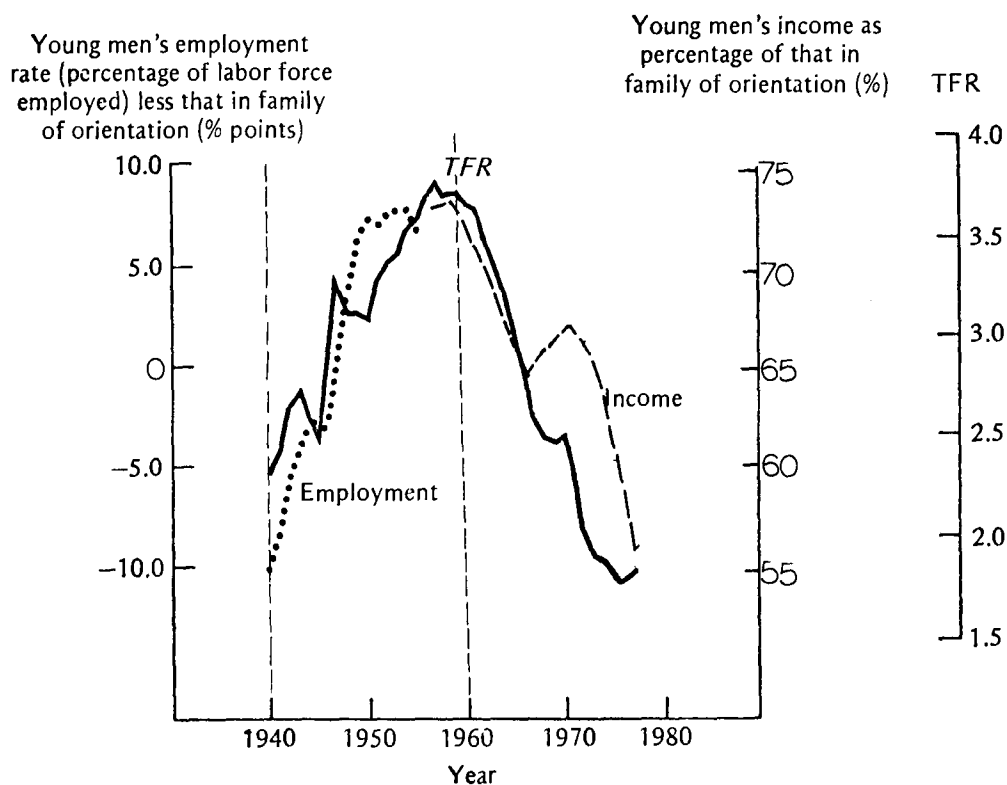
dependent variables

	<i>If Scarcity of Young</i>		<i>If Abundance of Young</i>	
	younger males	older males	younger males	older males
	(same as for Assumption A)			
	younger females	older females	younger females	older females
Wages	(+)	(—)	(—)	(+)
Unemployment rates	(—)	(+)	(+)	(—)
Labor force participation rates	—	+	+	—
	younger males and females		younger males and females	
Marriage	+		—	
Fertility	+		—	

workers will affect favorably their relative wages, unemployment rates, and upward mobility. The opposite would be true when there is an abundance of younger workers. There would also tend to be some substitution between older and younger workers, but by assumption such possibilities are limited. There might also be feedback effects from the labor supply side to aggregate demand—affecting both the size and composition of aggregate demand. (For some discussion of this, see Wachter and Wachter, forthcoming.) Thus, in periods when young males are scarce, their relative economic position would improve; when young males are abundant, their relative economic position would deteriorate. (Appendix B eval-

uates a possible qualification to this argument.)

A variety of evidence indicates that this has, in fact, been the case. The dotted and broken lines of Figure 2 (updated from an earlier paper of mine) give some illustrative measures—for relative employment through the mid-fifties, and relative income thereafter. These show that the relative economic position of young males since 1940 has, by and large, varied directly with their relative scarcity as indicated in Figure 1. [For additional evidence relating to the period of improvement, see Easterlin (1968), pp. 114–118, and Easterlin (1973). For more evidence relating to the period of deterioration, see Wachter (1976a, 1977).]



Source: Appendix Table 2.

Figure 2.—Total Fertility Rate, 1940–1977, Relative Employment Experience of Young Adult Males, 1940–1955, and Relative Income Experience of Young Adult Males, 1957 On

EFFECT OF AGE STRUCTURE ON FERTILITY AND FEMALE LABOR FORCE PARTICIPATION

Next, let us add to the analysis the effects of age structure on fertility and female labor force participation. Here, I shall use what I have elsewhere called the Easterlin-Wachter model (Easterlin, 1978). The working age population is now taken to consist of four groups—younger and older males and younger and older females. Three additional assumptions are made.

First, there is a traditional division between male and female roles—that is, males are the primary “breadwinners” and have labor force participation rates close to 100 percent, whereas females’ attachment to the labor market is less permanent (participation rates are substantially below 100) and their primary responsibility is childbearing, child rearing, and care of the home. This assumption implies no judgment about the desirability of these sex roles; I view it simply as a reasonable assumption about reality.

Second, there are three classes of jobs: (a) “career jobs,” involving considerable experience and skill, that are typically filled by older males because of their continuing labor force attachment and accumulated experience; (b) “career-entry jobs,” that are typically held by younger males; and (c) “noncareer jobs,” typically held by women. As a result, there is only limited substitution possible between younger and older males and between women and men, but a high degree of substitution possible between older and younger women.

Third, marriage and childbearing vary directly with the income of younger relative to older men. [In economics, the relative income hypothesis was first applied to savings behavior in Duesenberry (1949).] The reasoning is that the relative income of younger men may be taken as a rough index of the primary breadwinner’s ability to support a young household’s material aspirations. These aspirations are formed by the material environment that the

spouses experienced as they grew up, which depends, in turn, largely on their parents’ incomes. Hence, when young males’ income is high relative to older males’, it means that they may more easily support the aspirations that they and their potential spouses formed in their families of origin. Young people will then feel freer to marry and have children.

Now, let us consider the effect of imbalances in the labor market arising from sizable shifts in the age distribution of the working age population (Chart 2, lower panel). As before, it is assumed that the growth of labor demand for all labor force groups (this time four in number) is at constant, though not identical, rates. One might also suppose there was a normal trend increment in labor force participation rates of younger and older females. Suppose, now, that there is a scarcity of younger workers as a result of a corresponding change in the working age population. With regard to the labor market for younger and older males, the adjustments would be the same as those in the upper panel of Chart 2; that is, younger males would experience relatively favorable changes in wages, unemployment rates, and upward mobility. Now, however, one must consider additionally the implications of the improved relative income of young males for the situation of females. First, it would be easier for young men and women to marry and start families. This would, in turn, dampen and perhaps eliminate the normal increment in labor force participation of young females as the marital status and child dependency distribution of this group shifted toward categories with below average participation rates. The scarcity of young women would tend to have favorable effects on their relative wages and unemployment rates, but, because there is possible a high degree of substitution of older for younger women, these effects would be relatively moderate—this is the reason for the parentheses in the lines for these variables in the bottom panel of Chart 2. The more important effect would be the replacement

of younger by older females. This means that the less than normal increment in participation rates for younger females would be compensated by a more than normal increase in the rates for older females. [Wachter's (1972) empirical analysis of the effect of relative versus permanent income on labor force participation rates of younger and older females supports this inference. Other studies noting a possible link between the differential changes in participation rates of younger and older women and the relative scarcity of young persons in the post-World War II period are Bancroft (1958), Oppenheimer (1970), and United Nations (1962).] The entry of older women into the labor market might be further stimulated by the situation of older males, whose relative income situation, as shown in Chart 2, is unfavorable. If we compare in simple price-quantity terms the labor market adjustments of males and females, for males the scarcity of young persons induces an adjustment primarily in terms of relative income, for females, primarily in terms of relative quantities, that is, participation rates.

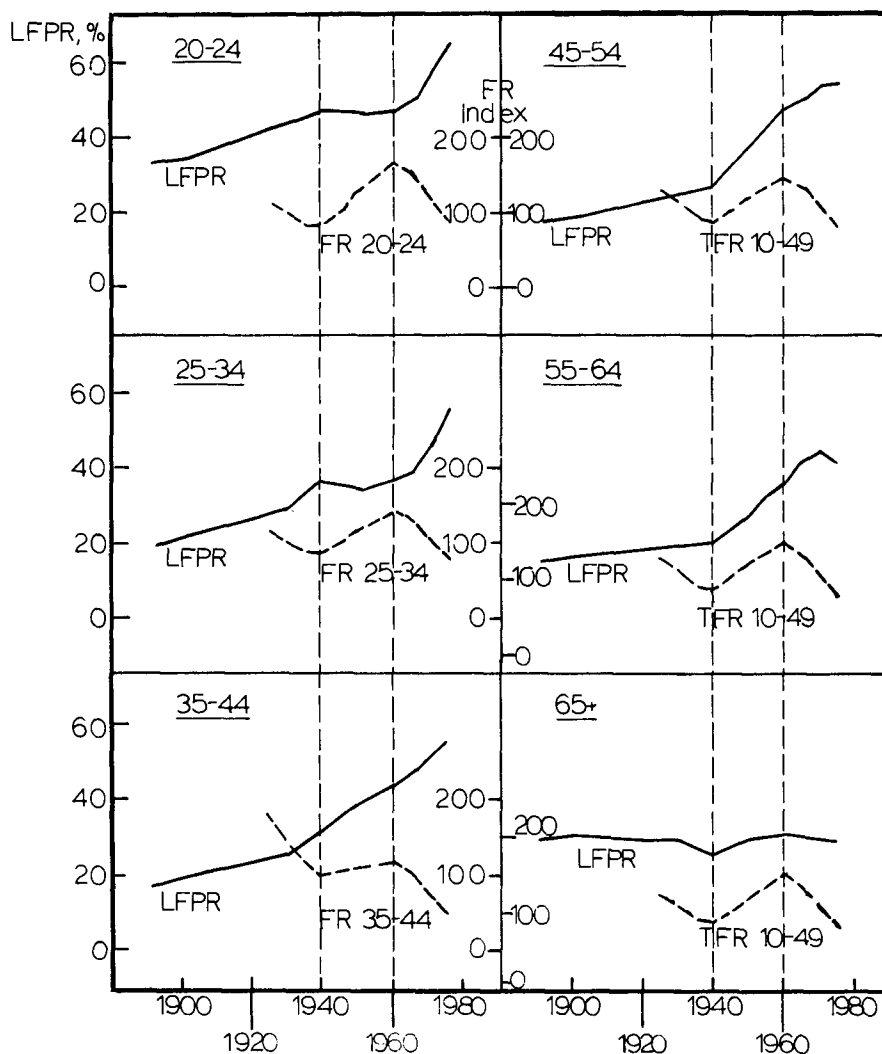
This analysis also shows that a cohort carries its fortunes, good or bad, depending on its size, throughout its life cycle. The cohort implications of Chart 2 are seen by a comparison of the first and last columns. Note how the signs remain the same—as a scarce cohort ages, it carries with it relatively favorable wage and employment conditions. (The argument is most clear-cut if the younger are taken to be those aged 20 to 39, the older those aged 40 to 59, and a turnaround in age structure occurs every 20 years.)

Admittedly, this is a highly simplified view of the makeup of the labor supply and the causal factors at work. One might, for example, distinguish within the group of younger males a "noncareer" group of disadvantaged workers, and within females a "career" group of college graduates. I believe, however, that this view, simple as it is, captures an important part of the forces shaping post-World War II

experience. Consider the patterns of fertility and female labor force participation shown in Figure 3. (In the figure, the fertility rate in a given five-year period is plotted against the labor force participation rate at the end of the period; for example, the fertility rate for 1971–1975 is plotted against the participation rate for 1975.) Starting around 1940, there was a marked break with previous experience. (The fertility trends before the 1920s, for which data are not available, would, of course, be downward.) Between 1940 and 1960, the upward trend in participation rates of young women was interrupted, and their fertility rose markedly. Participation rates of older women, which had previously shown only mild increases, shot up dramatically. Since 1960, there has been a reversal in this age pattern of participation rates—rates for younger women have risen sharply, while those for older women have risen only slowly or leveled off. At the same time, a sharp downturn in fertility has occurred.

Referring back to the lower panel of Chart 2, one finds that the analytical model just sketched generates behavior of just this type—an inverse association between the growth in labor force participation rates of younger versus older women, an inverse association between the growth in labor force participation of younger women and their fertility, and a positive association between the growth in labor force participation of older women and the fertility of younger women. But fertility is not causing participation rate change or vice versa. Rather, all of these developments flow from the imbalance in the labor markets of younger versus older workers due to shifts in the relative supplies of the two groups.

I am not claiming that the model explains completely the magnitudes of change observed; it clearly neglects, for example, the determinants of the general upward trend in labor force participation rates of females as a whole. It does capture, however, the essential outlines of the long-term swings since 1940, and, for



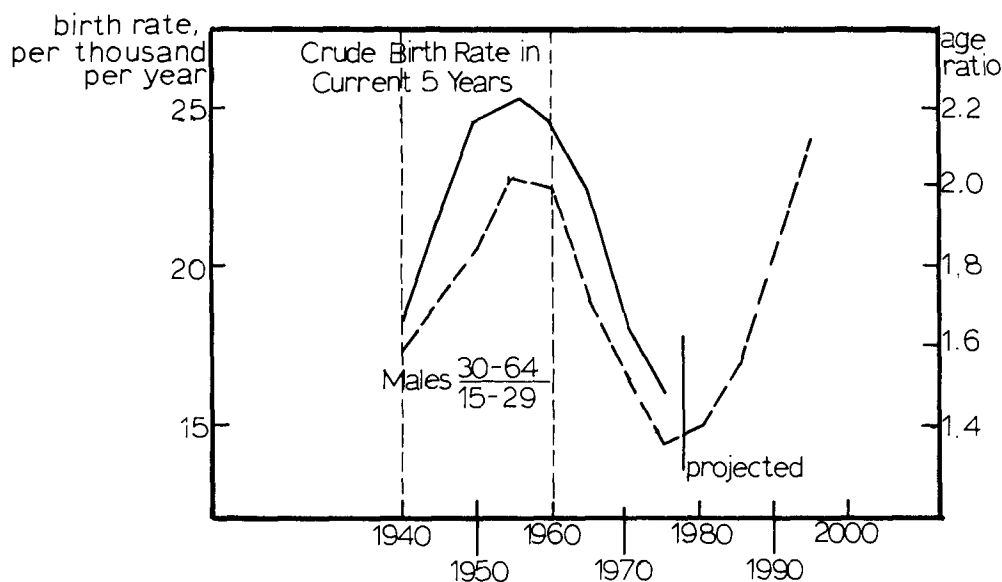
a

Comparable data for 1910 not available.
Source: Appendix Tables 3 and 4.

Figure 3.—Labor Force Participation Rates of Females Aged 20+, by Age Group, Decennially, 1890-1950*, and Quinquennially, 1950-1975, and Index (1940-1945 = 100) of Fertility Rates for Specified Age Groups, Quinquennial Averages, 1921-1925 to 1971-1975

some variables, it may even do well on magnitudes. This is suggested by Figure 2, in which the total fertility rate shows a clear parallel with relative income and its proxy based on the unemployment rate. It is further illustrated in Figure 4, in which the birth rate since the late 1930s is plotted against age structure—in this case, the ra-

tio of older to younger males, the inverse of the solid curve at the bottom of Figure 1. The two curves are not identical, but the similarity is striking—the swing in the birth rate looks very much like that in the relative number of older to younger males. (Figure 4 looks much the same if the total fertility rate is used instead of the



Source: Appendix Table 5.

Figure 4.—Ratio of Males Aged 30 to 64 to Males Aged 15 to 29, Actual and Projected, 1940 on, and Crude Birth Rate, 1935–1940 to 1970–1975

crude birth rate, and Figure 2 looks much the same if the crude birth rate is used instead of the total fertility rate.)

EFFECT OF AGE STRUCTURE ON ECONOMIC STABILITY

We can build on the foregoing analysis to show how shifts in age structure since 1940 have contributed to the macroeconomic problem of rising unemployment combined with accelerating inflation. Here, I shall again borrow in part from research done by my coworker, Michael Wachter (Wachter, 1976a, 1976b; Wachter and Wachter, forthcoming; Easterlin et al., 1978).

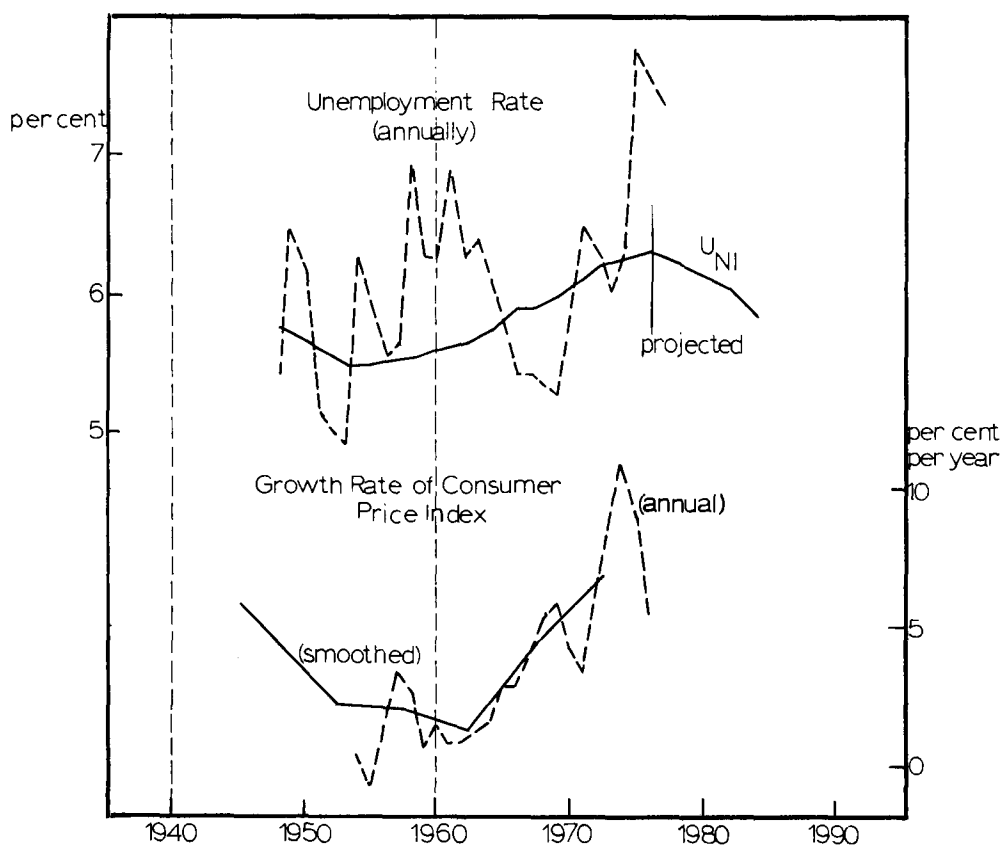
First, note that the developments sketched in Chart 2 would tend, in a period of growing abundance of younger workers, to raise the economy-wide average unemployment rate. The economy-wide rate can be viewed as an average of two component rates—a low rate for older workers and a high rate for younger workers, the latter reflecting their newness in the labor market, their job-seeking ac-

tivity, the tentativeness of their job commitments, and so on. Without any change in the unemployment rate for each older or younger group, a rise in the proportion of younger workers in the labor force (that is, in the proportion of those subject to high unemployment rates) would, other things being constant, raise the economy-wide unemployment rate. This is the standard age composition effect of demographic analysis. But, as shown in the right side of Chart 2, an abundance of younger workers would also affect age-specific unemployment rates, raising those of younger workers relative to those of older workers, and this would further increase the economy-wide unemployment rate. The rates for older workers are typically quite low, and there is little room for a change of much magnitude in the rates for these groups. But there could be a substantial change in the rates for younger persons, males and females. In fact, since the 1950s there has been a marked deterioration in the relative unemployment rates of younger men and women.

Consider, now, the implications of this for economic stabilization policy. Monetary and fiscal policies to correct unemployment conditions are geared to affecting the overall level of aggregate demand, but not the structure of that demand. Specifically, monetary and fiscal policies are *not* shaped to influence differentially the relative demands for two groups, such as younger and older workers, so as to compensate for changes in their relative supply. But, as we have just seen, under given conditions of aggregate demand, an increase in the relative supply of younger workers would tend to raise the economy-wide unemployment rate. If monetary-fiscal policy is initiated to correct a rising unemployment rate caused by an altered

mix of the labor supply, the extra demand does not correspond to this altered mix. Given imperfect substitution between older and younger workers, the extra demand generates relatively little improvement in employment conditions and spills over into price increases—hence, the phenomenon of a rising unemployment rate coupled with accelerating inflation.

The relevance of this argument to recent American experience is illustrated by the series plotted in Figure 5. The solid line curve labeled U_{NI} charts the course of the “full employment” rate that would be consistent with the changing effect of the population’s age mix on labor supply conditions in our economy, as estimated by Wachter. Note how U_{NI} swings upward,



Source: Appendix Table 6.

Figure 5.—Unemployment Rate U_{NI} and Growth Rate of Consumer Price Index Since World War II

after the late 1950s, due to the rise in the proportion of young adults in the working age population shown in Figure 1. As it swings upward, however, the actual unemployment rate in the 1960s, shown by the broken line, which reflects the impact of aggregate demand and, thus, government monetary-fiscal policy as well as supply, moves below U_{NI} . Reference to the two lower curves shows that it is just about at this time that the average annual rate of price increase rises from a level not much above zero to the increasingly high levels of recent years. This rise in the inflation rate is, in part, the consequence of attempting to correct an imbalance on the labor supply side with inappropriate aggregate demand policies.

Accelerating inflation as U_{NI} rises is not an inevitable result of aggregate demand management. Over the past decade, the monetary and fiscal authorities seem to have underestimated the effect of the demographic swing on the sustainable, non-inflationary unemployment rate. Maintaining old unemployment targets for too long resulted in overexpansive demand policies and an increase in the inflation rate. Moreover, a higher inflation rate persisted for some time after aggregate demand policy had been readjusted downward, because of the lagged response of expectations, and the inertia built into the economy due to the presence of fixed price and wage contracts. If the sustainable unemployment rate through aggregate demand management could be estimated with accuracy, and if the government adopted that rate as the target rate for demand management, then the correlation between rising unemployment and accelerating inflation would be broken, insofar as that correlation is due to the altered labor supply mix.

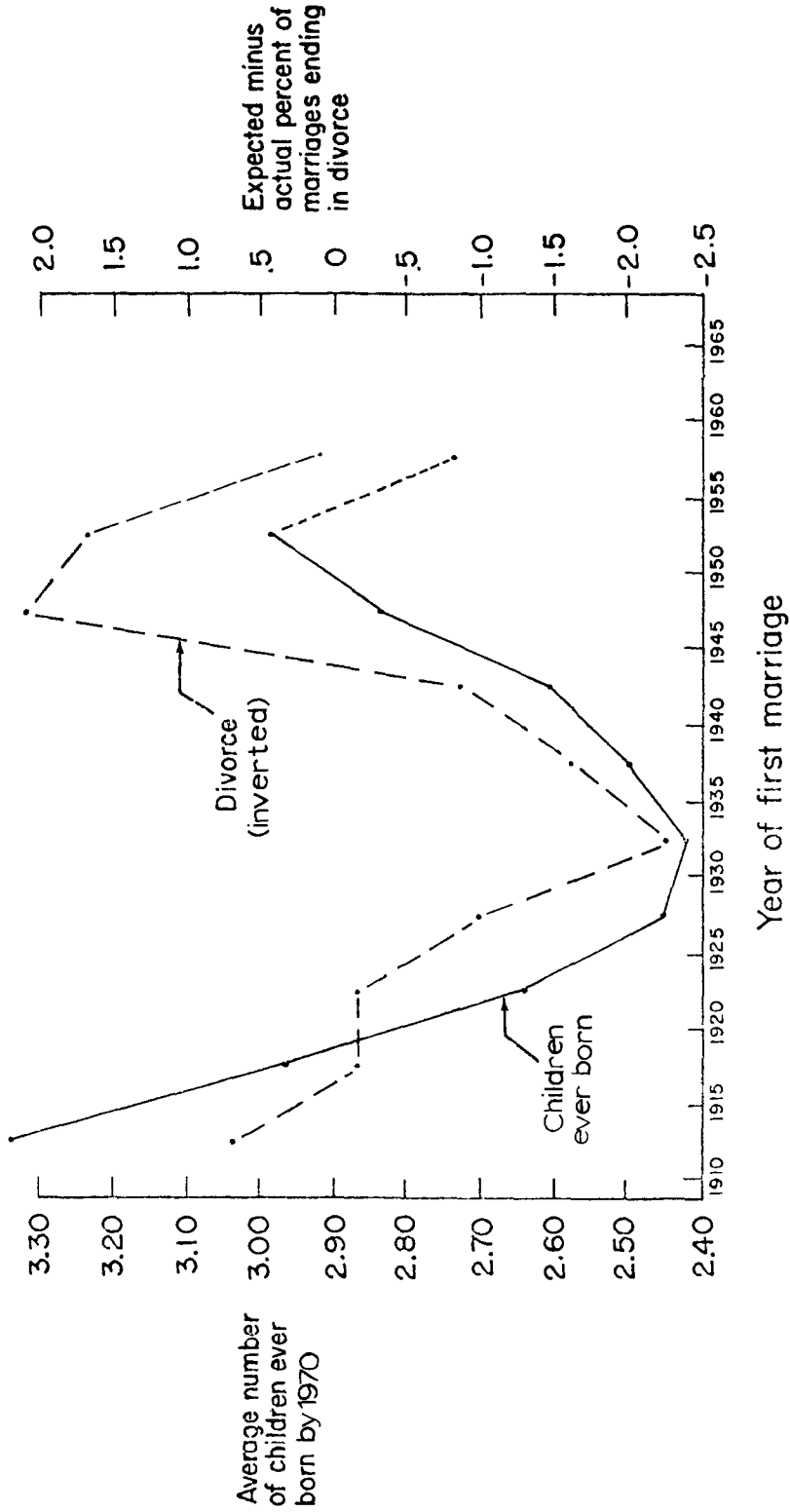
EFFECT OF AGE STRUCTURE ON SOCIAL VARIABLES

Let me turn briefly to some social and political effects of the recent shifts in age structure. The theoretical underpinning for such effects is straightforward. Rela-

tive income or relative status notions have never been the private preserve of economists. Quite the contrary—economists came to the scene (relatively) late and even today are, for the most part, strongly resistant to such notions. In contrast, there is a strong tradition in sociology running from Durkheim's theory of *anomie* through Stouffer's work on relative deprivation down to the present, which sees relative status considerations as pervasive in social phenomena (Durkheim, 1951; Stouffer et al., 1949). Let me cite two examples from recent work by demographers who have strayed somewhat beyond the usual confines in their field.

Figure 6 is taken from a paper by Samuel Preston and John McDonald (1977) that presents historical estimates of divorce by marriage cohort and then—our interest here—speculates on the causal factors at work. The point of Figure 6 is that a trend-adjusted series for cohort divorce rates shows a long swing much like that in fertility, although the relationship is inverse. Thus, the trough in children ever born for the cohort first married in 1930–1934 is accompanied by a peak in the trend-deviation for divorce. The next turning points in the two series are close, but not identical: corresponding to the fertility peak for the marriage cohort of 1950–1954, there is a trough in the trend deviation in divorce for the cohort of 1945–1949. In general, for the marriage cohorts that produced the baby boom, divorce was considerably below the rate expected on the basis of past trends. For the most recent marriage cohorts, as their fertility has turned downward, divorce has risen toward trend rates.

A close association between fertility and another social magnitude, suicide among young males, has been pointed out and studied by Martin O'Connell (1975). For the period 1940–1976 (excluding the war years 1941–1945), the correlation between the total fertility rate of females aged 15 to 49 and the suicide rate of males aged 15 to 24 is $-.88$, between the fertility rate and the suicide rate of males aged 25



Source: Appendix Table 7.

Figure 6.—Children Ever Born and Deviations From Trend in Divorce for White Marriage Cohorts of 1910–1914 Through 1955–1959

to 34, also $-.88$. Note that again the relationship is inverse—higher fertility goes with lower suicide.

Such correlations of divorce and suicide with fertility do not necessarily imply causality. I have already argued that the inverse correlation between the growth in labor force participation of younger females and their fertility is due, not to a cause-effect relation between the two, but to a common response to the same cause, relative income. Both the Preston-McDonald and O'Connell studies suggest that the same is true for the correlations they observe. [Simon (1968, 1969, 1975) developed analogous relative-income-type models to explain fertility and suicide.] Both the Preston-McDonald and O'Connell studies, drawing on the literature, advance a relative income hypothesis (though in somewhat different form) and test for its explanatory power. The results are positive—a deterioration in relative income of the young makes for more frequent divorce and a rise in suicide among young males; and improvement has the opposite effect.

If a relative income mechanism is influencing divorce and suicide, should not its effects be apparent in other social phenomena? The answer, of course, is yes; and there are some hints in the record that they are. Figure 7 presents homicide rates, which may perhaps be taken as representative more generally of crime rates, and two measures of political alienation among the young. Although the series are short and not adjusted for trends, there is a common pattern—a suggestion of slight improvement to around 1960 (a downward movement of the curves) and a noticeable deterioration thereafter (an upward movement). (Evidence that the recent rise in crime rates reflects changes in age-specific rates as well as in the age composition of the population is given in Wellford, 1973.) I am not claiming that age structure and "relative income" are the whole story of the post-1940 change in crime rates and political alienation. In fact, House and Mason (1975) fail to find

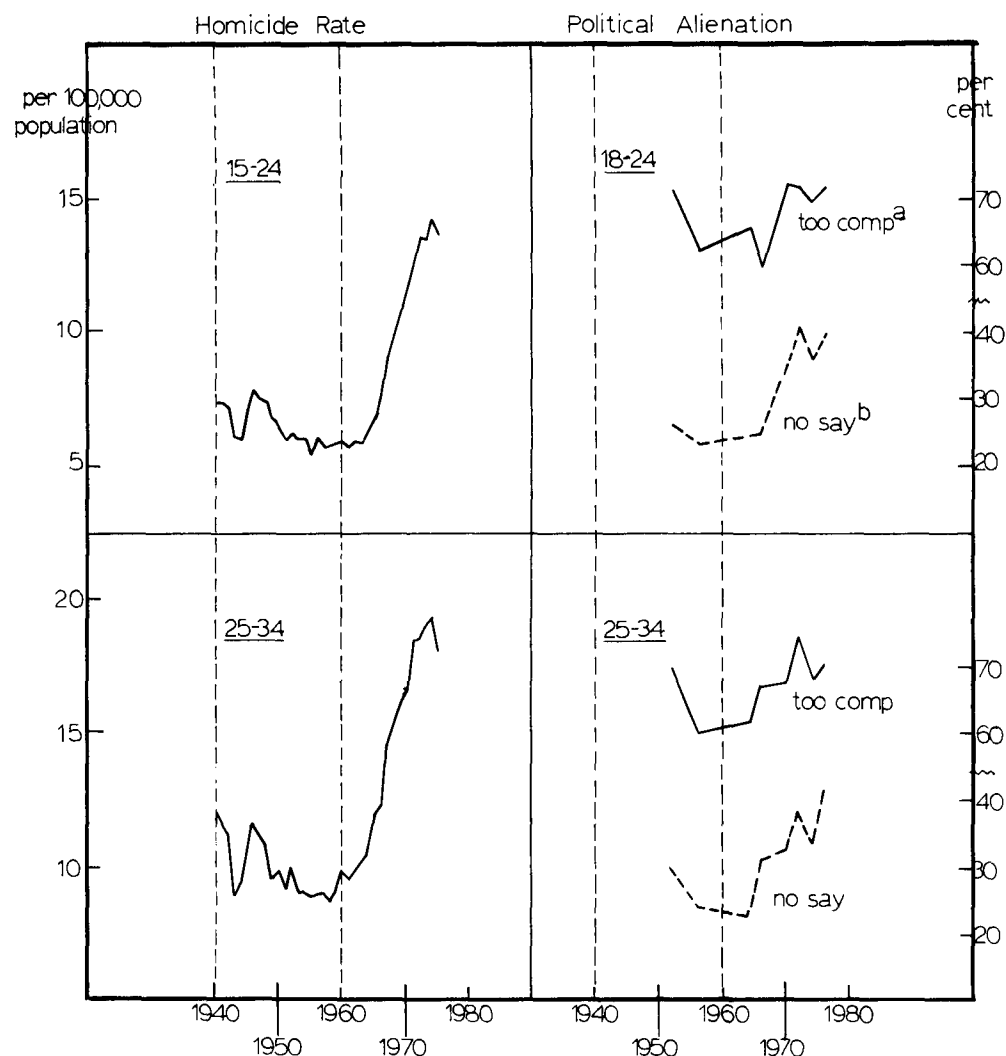
evidence of significant differences among demographic age groups in the data for political alienation. But these are examples of variables that in sociological theory should reflect relative status influences, and there is a hint in the evidence that this is the case.

EFFECT OF AGE STRUCTURE ON HIGHER EDUCATION

I want to conclude this discussion of the effects of age structure by briefly noting two studies that identify important impacts on the college environment—one on college enrollment rates and the other on SAT scores. In each case, in contrast to the foregoing, age structure has its effect with a substantial lag. As before, my interest is not in effects working through age composition as such (although for college enrollments this effect is an important one), but in influences working on age-specific rates.

Let me start with SAT scores, which, as shown in Figure 8, have been declining since the early 1960s. Robert Zajonc, a social psychologist at the University of Michigan, from whose work this chart is taken, has offered an explanation of this decline based on average birth order (1976). Various studies have found a low but significant association between birth order and intelligence—higher birth order being correlated with slightly lower intelligence. The post-World War II baby boom resulted in an increase in average birth order. This is shown by the lower curve of Figure 8, which is plotted in inverted form. Those born in a given year take their SATs about 17 years later. Hence, according to Zajonc, the decline in SATs that started in the 1960s can be traced back to baby boom cohorts with progressively rising average birth order. As Figure 8 shows, when the curves for SATs and average birth order are plotted with appropriate allowance for the lead-lag relationship, they do exhibit a substantial similarity.

Since about 1970, there has been a puzzling interruption in the uptrend in enroll-



^atoo comp=% agreeing with statement "Sometimes politics and government seem so complicated that a person like me can't really understand what's going on."

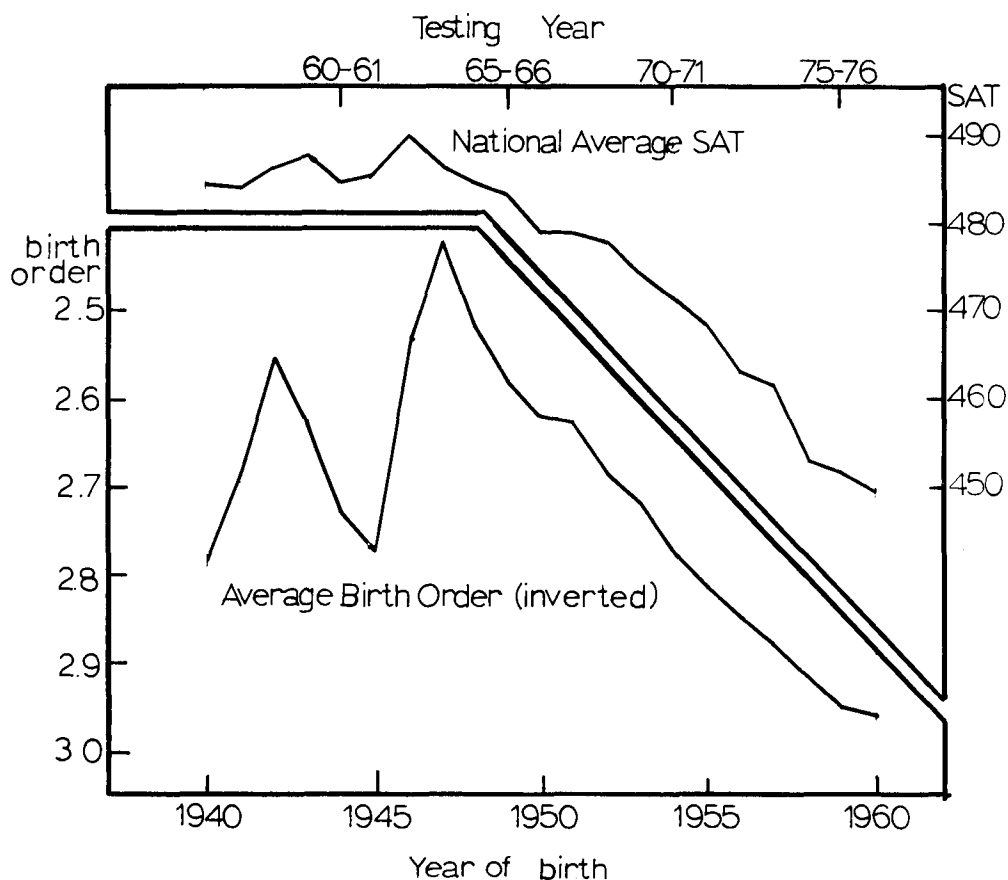
^bno say=% agreeing with statement "People like me don't have any say about what the government does."

Source: Appendix Tables 8 and 9.

Figure 7.—Homicide and Political Alienation Among the Young

ment rates for those of college age—a leveling off among females and a decline among males. Goldberg and Anderson (1974) have advanced an explanation for this based on changes in the proportion of households that have at the same time two or more children of college age. The financial burden of putting a child through col-

lege is obvious. But, Goldberg says, consider the financial predicament of those with two children of college age at the same time—not to mention three! Through ingenious work with the Public Use Tapes, Goldberg has constructed several measures of the changing magnitude of this predicament in the post-World



Source: Appendix Table 10.

Figure 8.—Average SAT Score and Average Birth Order for Annual Birth Cohorts of 1940 Through 1960

War II period. The measures show an upsurge in this problem about the time of the interruption of the rise in college enrollment rates. The movement in the proportion of households with two or more children of college age is itself, of course, an echo of prior fertility movements. Thus, the implication is that the postwar baby boom is currently taking its toll on college enrollment rates because of the rise in the proportion of households experiencing the financial strains of having two or more children of college age at the same time.

Another interpretation of the recent decline in enrollment rates is suggested by Richard Freeman's work (1976, especially pp. 64 ff.). The decline may be due to a drop in the returns from higher education,

caused in part by the recent increase in the supply of those of college age, an effect of the post-World War II baby boom. The Freeman and Goldberg interpretations may be viewed as complementary, because both involve in different ways lagged effects of the baby boom. The Goldberg argument differs from those previously presented in this paper in that the theoretical argument implies errors in judgment.

I have spoken of the movements in enrollment rates and SATs as reflecting age structure changes, but for each of these variables the causal chain involves several links. In both cases, the immediate determining factor is compositional—the population mix with regard to birth order or

proportion with two or more college age children. As has been noted, however, each of the latter depends, in turn, on prior movements in fertility. And, by the reasoning previously stated, the post-World War II swing in fertility has been largely governed by age structure and its relative income effects. By this set of connections, therefore, age structure shifts have, with a lag, affected college enrollment rates and SAT scores.

RESEARCH IMPLICATIONS

Space is running short, but I would like at least to touch in outline form on some of the research possibilities suggested by the present analysis.

1. First, much more work is needed on age structure changes themselves and the underlying mechanisms through which they work.

- a. I have focused here on the proportion of younger to older working age population, with the age of 30 as the dividing line. Experimentation is needed with alternative age ratios. For example, the ratio of those aged 20-29 to 30-39 shows a time series movement since 1940 much like that for the ratio shown in Figure 1. The appropriate age ratio might well differ, of course, from one subject to another, and from one society to another.

- b. Perhaps too much stress is laid here on the importance of relative numbers. There is need to look into the relative "quality" of different age groups, as reflected, say, in measures of educational attainment and work experience (although these may, in part, be dependent on relative numbers).

- c. The appropriate measure of relative income is itself a subject for research. One would not expect the same measure to be applicable to different age groups and all subjects of study.

- d. I have perhaps focused too much on economic indicators of

relative deprivation, to the exclusion of psychological ones. As Keyfitz (1972) and others have suggested, the experience of relative deprivation may have its origins earlier in life, for example, in large families and crowded classrooms.

2. One would expect that the mechanisms described here, whatever their specific nature, would have left their imprint on a variety of other phenomena. One clue, suggested by the Preston-McDonald and O'Connell studies, might be the degree of correlation of other economic, social, and political indicators with fertility. In some cases, the effect might appear in the level of a series, as for fertility itself; in others, it might be in movements around the trend, as for female labor force participation and divorce. The possibilities range over the various disciplines.

- a. In the area of demography proper, one would expect that pressures on the young of the sort I have been discussing would leave their mark on illegitimacy measures, and I believe there is some evidence to this effect. Thus, O'Connell (1978) finds that in recent decades the proportion of premarital conceptions legitimated by marriages shows a positive swing conforming to the relative economic position of young males. Estimates of the first-birth order illegitimacy ratio (the number of first-birth order illegitimate births per 1,000 total first-order births) also show a sharp contrast before and after 1960 consistent with this relationship. Beyond the area of fertility, mortality and migration might be affected, too. The results above for suicide and homicide may be symptomatic of a more general pattern in stress-related morbidity and mortality, as Ingrid Waldron and Joseph Eyer (1975) have suggested. In

the area of migration, Hope El-drige's (1964) work has demonstrated how cohort migration was affected by boom and bust Kuznets cycles of the past. It seems reasonable to suppose that, with the reversal of demand and supply roles noted in Chart 1, recent and current migration patterns might show an effect on cohort size.

b. Turning to economics, the analysis has already suggested effects on a number of other labor market variables, such as job turnover and occupational mobility. One might well expect an impact, too, on the size distribution of income through various wage and work channels. On the demand side, effects might be expected on demands for many specific goods, such as schooling, housing, and other child-related items.

c. In sociology, an impact on family formation, dissolution, and structure is suggested by the previous discussion of marriage and divorce. Also, variations in the economic gap between young and old carry obvious implications for the degree of inter-generational conflict.

3. There is need, too, to extend this line of inquiry to other bodies of data.

a. The possibility of investigating these relationships in evidence for other times and places is obvious, although it must be done with care. It was only under the special conditions that emerged after 1940 in the United States that age structure variations came to play such a pervasive role. One would not expect to find evidence of the same mechanism in earlier periods or in markedly different conditions. The most obvious candidates are other developed countries which have experienced recent major shifts in age struc-

ture similar to the United States. Several exploratory studies suggest that parallel movements in fertility and relative income or age structure have occurred in several other countries. Reversals in the age pattern of growth in female labor force participation rates also seem to have occurred elsewhere (see O'Connell, 1975, forthcoming; Easterlin and Condran, 1976; Brunborg and Lettenstrøm, 1976; Sweetser and Peipponen, 1967; Leridon, 1978; and United Nations, 1962). But studies of other times and places must be conducted in a context sensitive to the special historical and institutional circumstances of each country.

b. My attention has focused on time-series data, but clearly the mechanisms described here should be evident in cross-section data as well. The results so far of cross-section studies have been mixed, probably in part because relative income mechanisms are obscured in cross-section data by factors that change little or not at all over time. But cross-section studies may ultimately clarify the specific nature of the mechanisms at work.

4. In concluding this section, I'd like to add a note on implications of the present analysis for graduate training in demography. Whereas demographers typically emphasize the compositional effects of age structure shifts, the present analysis focuses on effects working chiefly through age-specific rates. These effects stem from mechanisms identified in economic and sociological theory, usually in connection with such notions as relative income or relative deprivation. To the extent that demographic analysis moves more in the present direction, greater training in these underlying disciplines will be necessary.

ALTERNATIVE VIEWS

What of competing interpretations of recent experience? The dramatic decline since 1960 in the fertility of young women and the rapid rise in their labor force participation has excited much scholarly discussion. A variety of reasons have been given for these developments, such as exceptional employment opportunities for young women, new possibilities for regulating fertility, and new conceptions of women's roles due to the women's movement.

With regard to employment opportunities for young women, the argument is that these have expanded rapidly since 1960, thereby encouraging substitution of work in the marketplace for childbearing and work at home. The basic question, however, is not whether employment opportunities of young women have been expanding since 1960, but whether the labor market for young women was better in, say, the 15 years after 1960 than in the 15 years before, that is, whether this market was better when young women were increasingly plentiful in the population, as after 1960, than before, when they were increasingly scarce. I believe most labor market students would argue, as I would, that the post-World War II labor market for young women was, like that for young men, exceptionally good, and that young women opted for marriage and childbearing, not because of inadequate employment opportunities, but in the face of very good opportunities. [The recent Butz-Ward analysis (1977) adopts the opposite view.] Developments since 1960 can hardly be attributed to an improvement in labor market conditions for young women. The economy's occupational structure did not change in a way especially favoring women after 1960, as compared with before 1960 (see Appendix C). Changes since the late 1940s in the unemployment rates of females aged 20 to 24 imply a stronger labor market through 1960 than thereafter.

Interpretations of the last decade's shift of young females from home to workplace

that stress the women's movement or the development and adoption of new fertility control measures are, at least, more consistent with the facts. However, these developments may themselves be attributable partly to age structure effects operating via a relative income mechanism. With regard to fertility control, after 1960 with the growing abundance of young men and the relative deterioration in their labor market position, families felt greater pressure to adopt available fertility control methods—old or new—to restrict births. The rapid spread of new measures of fertility control was, in part, induced by the pressures on young adults described here.

To turn to the women's movement, the recent acceleration in the growth of young women's labor force participation rates is sometimes cited as indicative of the impact of the movement (although a look at the occupational mix of the growth in female employment might give one pause—see Appendix D). It should be noted, however, that the present model, which adopts a traditional view of male and female roles, predicts an upturn in labor force participation among younger women in the period after 1960. Thus, this upturn is not presumptive evidence of the force of the women's movement.

Rather than the women's movement being the cause of the recent accelerated rise in younger females' participation rates, the research findings of Karen Mason and others suggest that the reverse appears more likely to be the case—namely, that the rise in labor force participation led to attitudinal change favorable to the women's movement (Mason et al., 1976). And, since the accelerated rise in labor force participation is, in turn, ascribable to the mechanisms described here, changing age structure was thus one of the forces recently furthering the women's movement. Elsewhere, in regard to the antinatalist shift in population attitudes, I have similarly argued that attitudinal change is primarily an effect, rather than a cause, of changed behavior (Easterlin, 1973, pp. 206–212).

A more general objection to explanations centering on fertility control or the women's movement is that they are confined to the period of the 1960s onward. A basic tenet of the approach adopted here is that a plausible interpretation of experience since 1960 should be consistent with the contrasting experience of the baby boom period. Neither of these views can offer any explanation of the baby boom. Indeed, so far as fertility control possibilities are concerned, the baby boom occurred despite what must have been a major advance in contraceptive knowledge among young persons as a result of information disseminated by the armed forces in World War II and the Korean War.

Moreover, the behavior that those explanations seek to interpret is much narrower than is true here. They apply primarily to fertility and young women's labor force participation. The relative income interpretation I am suggesting seems to fit not only these developments but, as we have seen, a much wider range of evidence, including such magnitudes as divorce, suicide, homicide, and alienation. These other variables are ones that theory would lead one to expect would fit a relative deprivation-type model, and they do. The fact that the present interpretation is consistent with a wider range of socioeconomic data, and that it fits pre-1960 as well as post-1960 experience, tends to strengthen one's confidence in it.

I am not claiming that age structure and relative income are the be-all and end-all in the explanation of these and other developments of the last few decades. The women's movement may have a lasting effect on females' roles through institutionalizing the attitude changes that have occurred, and fertility control developments have not necessarily been wholly induced. It is quite possible that these developments have exerted an independent effect on behavior. Many additional factors have doubtlessly influenced the movements of the variables surveyed here, some specific to an individual variable and

others, such as the Vietnam War, more generally. I am arguing only that the evidence is consistent with the view that age structure and relative income have been much more important and pervasive influences in experience since 1940 than has heretofore been recognized.

THE FUTURE

We come finally to the question posed in the title: what will 1984 be like? I have argued that shifts in the relative number of younger versus older adults under conditions of restricted immigration and high and sustained growth in aggregate demand have had a pervasive impact on American life since 1940. If I am correct, then what happens in the near future depends, in important part, on the age structure outlook.

The age structure outlook is shown in Figure 1, which presents the projection to 1995 of the younger and older working age population. A high degree of confidence can be placed in this projection since it is based almost wholly on persons already born. As one can see, growth in the group aged 15 to 29 is currently slowing, and, after 1980, the absolute number will actually begin a period of decline. The *proportion* of those aged 15 to 29 to those aged 30 to 64 is slated to turn down even earlier, showing a decline from 1975 to 1980. This decline becomes more precipitous in the decade of the 1980s. Thus, the outlook is for a growing relative scarcity of younger adults, even more pronounced than in the 1940s and 1950s.

The implication of this new twist in age structure is that there will be a return to the patterns of the 1940s and 1950s. Specifically, with regard to the developments discussed here, there would be:

1. an improvement in the relative income of young males;
2. an upturn in the crude birth rate;
3. a return to the pattern of larger increases in participation rates for older than for younger women;
4. an abatement of the problem of ris-

- ing unemployment and accelerating inflation;
5. an eventual drop below trend levels in cohort divorce rates;
 6. a decline in the suicide rates of young males;
 7. an improvement in crime rates and political alienation;
 8. a resumption of the rise in college enrollment rates; and
 9. an upturn in SATs.

More generally, I am suggesting that the 1980s will see a turnaround or amelioration in a wide variety of social, political, and economic developments of the last decade or so, some of which have been taken as symptomatic of a hardening social malaise. And, although I am probably alone in predicting such a sweeping reversal, other scholars, reasoning along lines like those presented above, have arrived at similar conclusions for a number of specific magnitudes. Ronald Lee (1976, 1977) and David Goldberg (1978) have each developed fertility projections that foresee a sizable upturn. (See also Goldberg et al., 1977; Wachter, 1975.) Michael Wachter's (1977) projections of labor force participation rates show a return to the pattern of higher increases for older than for younger women in the 1980s. Goldberg and Anderson (1974) anticipate a resumption of the rise in college enrollment rates as the financial squeeze on parents of college-age students moderates in the early 1980s. Robert Zajonc (1976) predicts an upturn in SATs starting around 1980.

A few straws in the wind are provided by the experience of the last couple of years, which suggests that some changes of the last decade may have been arrested. Fertility rates are no longer falling, and the rise in homicide and suicide rates has ceased, at least temporarily. It is too early to tell whether such movements are indicative of a turnaround, but the timing of such developments is consistent with the present analysis.

I want to emphasize that I am talking about the effect of changing age structure

via "age-specific" rates, not via "age composition." Both effects should, of course, be taken into account. Heretofore, demographic analysis has been largely or wholly confined to age composition effects. In the current economy, however, age structure effects via age-specific rates are often much more important than the traditional age composition effects stressed in demography. Experience with regard to the crude birth rate provides a good example. In the post-World War II period, the effect on the crude birth rate of age composition changes has been much less than that due to changes in age-specific rates. Moreover, the age composition effect has been in the opposite direction from the effect of age-specific rate changes, and has dampened only mildly the effect on the crude birth rate of the latter. For example, the negative impact on the crude birth rate of the decline since 1960 in age-specific rates has been offset only slightly by an age composition shift favorable to a higher birth rate.

I realize that some of you think that my suggestion that there may be a substantial rise in fertility over the next decade or so is just plain foolish, if not irresponsible. Although fertility is admittedly a difficult magnitude to predict, how can I ignore or discount so heavily the weight of many factors tending to depress fertility? In much of the demographic community, present thinking about American fertility is, I believe, reasonably well captured in the following quotation (Population Index, 1948):

Short-run birth rates . . . are unpredictable unless based on a combination of demographic, attitudinal, and business cycle research not yet achieved. . . . Long-run birth rates involve even more difficult questions. . . . In regard to these uncertain factors there is at least a minimum of agreement among demographers. No one anticipates the restoration of levels of fertility that could be regarded as high in a world setting. The range of uncertainty is between rates somewhat below permanent replacement and rates slightly above such replacement.

That statement, for which numerous parallels expressed with equal assurance could be found in current writing (see, for example, Westoff, 1978), appeared in *Population Index* exactly 30 years ago, when the post-World War II baby boom was taking off. The fertility prediction was wrong then because, in my view, it ignored a major demographic influence tending to turn fertility behavior around—the growing scarcity of young adults and the resulting rise in their relative affluence. But, on behalf of the writer, a plea of ignorance could justifiably be entered at that time. I think such a plea cannot now be made and that demographers, after enumerating all of the factors making for continued low fertility, should acknowledge that there is at least one important factor working in the opposite direction, a factor that, to judge from the record, has dominated all others over the past 40 years.

Let me turn from the 1980s to the question of the longer-term outlook. Will the changes in age structure and the attendant effects that we are witnessing continue into the more distant future? Clearly, this hinges on the question of the longer-term outlook for the age structure itself.

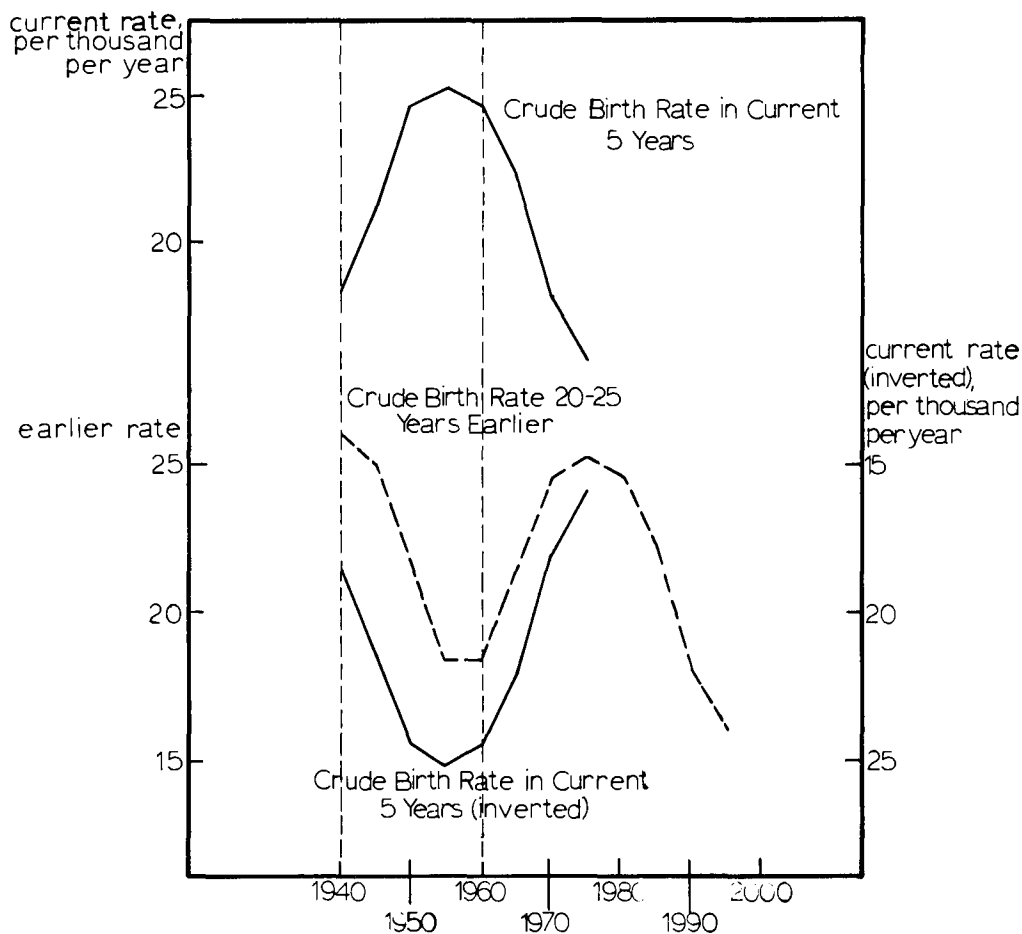
The age structure, at any given time, echoes with a lag chiefly the birth rate. This is shown in Figure 1, where movements in the age ratio are seen to follow movements in the birth rate some 20 to 25 years earlier. Thus, the longer-term future of age structure depends on the outlook for the birth rate.

According to the analysis I have presented, however, under current conditions the birth rate is largely being shaped by ongoing developments in age structure. This was the point of Figure 4. We have, then, the possibility of a self-generating mechanism that, in simplest terms, is of the following sort. Current fertility depends on current age structure. Current age structure depends on past fertility. Hence, deleting the mediating role of age structure, current fertility depends on past fertility. (An equivalent formulation,

dropping out the mediating role of the birth rate, yields, alternatively, current age structure depending on past age structure.)

The empirical plausibility of this reasoning is illustrated in Figure 9. The current birth rate is the solid line at the top of the figure; the past birth rate, that shaping the current birth rate via its impact on current age structure, is the broken line curve in the middle. Inverting the current birth rate curve, one obtains the solid line curve at the bottom. Note how closely the current swing in the birth rate echoes in inverted form that in the birth rate 20 to 25 years earlier. It seems safe to say that, without the theory just presented, no one would have anticipated this dramatic similarity or even thought of juxtaposing the two series this way. And, if the current birth rate fluctuation so closely mirrors that of the past, may not the future birth rate show a swing mirroring that of the present? And so on into the future? I do not know the answer, and, even if the analysis were correct up to this point, unforeseen factors may subsequently intervene. But the record so far seems consistent with the possibility of a self-generating mechanism that produces repeated swings in the birth rate and age structure and, thus, in socioeconomic conditions of a wide variety. A new Kuznets cycle may have replaced the old—one with a duration of around 40 years rather than 20.

Today, as in the past, the economic fortunes of different generations are shaped in important part by circumstances beyond their control. Before World War II, it was the luck of the draw as to the state of aggregate demand—whether one came of age in a boom or bust period; since World War II, it has largely depended on whether one comes from a large or a small cohort—from a “baby boom” or a “baby bust.” But such conditions are not immutable. We have learned from the study of past experience to moderate substantially the variations due to aggregate demand fluctuations—to tame the old Kuznets



Source: Appendix Table 1.

Figure 9.—Crude Birth Rate in Current Five Years Compared With Crude Birth Rate 20 to 25 Years Earlier

cycle. It is reasonable to suppose that, because of recent experience, public policy may eventually be turned, as well, to modifying variations in the fortunes of generations due to their relative numbers, to tempering the new Kuznets cycle.

ACKNOWLEDGMENTS

The research on which this paper reports was funded by NICHD grant HD-05427, held jointly with Robert A. Pollak and Michael L. Wachter. For comments and assistance I am grateful to Robert Cohen, Frank R. Lichtenberg, Morton Owen Schapiro, and Deborah C. K. Wenger. I have also benefited from the

comments of Eileen Crimmins, Robert Higgs, and Michael L. Wachter.

REFERENCES

- Abramovitz, M. 1961. The Nature and Significance of Kuznets Cycles. *Economic Development and Cultural Change* :225-248.
- . 1968. The Passing of the Kuznets Cycle. *Economica* 35:349-367.
- Bancroft, Gertrude. 1958. *The American Labor Force*. New York: John Wiley and Sons.
- Brunborg, Helge, and G. S. Lettenstrøm. 1976. Fertility Trends in Norway since 1965. Paper presented to the Nordic Demographic Symposium, Rungsted, Denmark, June 10-12, 1976.
- Butz, William P., and M. P. Ward. 1977. *The Emergence of Countercyclical U.S. Fertility*. Santa Monica, Calif.: The Rand Corporation.

- Duesenberry, James S. 1949. *Income, Saving, and the Theory of Consumer Behavior*. Cambridge, Mass.: Harvard University Press.
- Durkheim, Emile. 1951. *Suicide, A Study in Sociology*. New York: Free Press.
- Easterlin, Richard A. 1968. *Population, Labor Force, and Long Swings in Economic Growth: The American Experience*. New York: National Bureau of Economic Research.
- . 1973. Relative Economic Status and the American Fertility Swing. In Eleanor Sheldon (ed.), *Family Economic Behavior: Problems and Prospects*. Philadelphia: J. B. Lippincott Company.
- . 1978. Fertility and Female Labor Force Participation in the United States: Recent Changes and Future Prospects. Paper prepared for the International Union for the Scientific Study of Population Conference on Economic and Demographic Change: Issues for the 1980s, Helsinki, August 28, 1978.
- , and G. A. Condran. 1976. A Note on the Recent Fertility Swing in Australia, Canada, England and Wales, and the United States. Pp. 139-151 in Hamish Richards (ed.), *Population, Factor Movements, and Economic Development: Studies Presented to Brinley Thomas*. Cardiff, Great Britain: University of Wales Press.
- , M. L. Wachter, and S. M. Wachter. 1978. Demographic Influences on Economic Stability: The United States Experience. *Population and Development Review* 4:1-23.
- Eldridge, H. T. 1964. A Cohort Approach to the Analysis of Migration Differentials. *Demography* 1:212-219.
- Employment and Training Report of the President. 1977. Washington, D.C.: U.S. Government Printing Office.
- Freeman, Richard B. 1976. *The Over-Educated American*. New York: Academic Press.
- Goldberg, David. 1978. Future of American Fertility: Some Speculations. Paper presented to the annual meeting of the Population Association of America, Atlanta, April 13-15, 1978.
- , and A. Anderson. 1974. Projections of Population and College Enrollment in Michigan, 1970-2000. Paper presented to the Governor's Commission on Higher Education, Lansing, Michigan.
- , J. M. Coble, A. Anderson, and F. Cartford. 1977. Projections of Population and Employment for the Great Lakes States: 1970-2000. Paper prepared for the Upper Great Lakes Regional Commission. Lansing, Mich.: Upper Great Lakes Regional Commission.
- Grabill, Wilson H., C. V. Kiser, and P. K. Whelpton. 1958. *The Fertility of American Women*. New York: John Wiley and Sons.
- House, J. S., and W. M. Mason. 1975. Political Alienation in America, 1952-1968. *American Sociological Review* 40:123-147.
- Keyfitz, N. 1972. Population Waves. Pp. 1-37 in Thomas N. E. Greville (ed.), *Population Dynamics*. New York: Academic Press.
- Kuznets, Simon. 1958. Long Swings in the Growth of Population and in Related Economic Variables. *Proceedings of the American Philosophical Society* 102:25-52.
- . 1961. *Capital in the American Economy: Its Formation and Financing*. Princeton: Princeton University Press.
- Lee, Ronald D. 1976. Demographic Forecasting and the Easterlin Hypothesis. *Population and Development Review* 2:459-468.
- . 1977. Fluctuations in U.S. Fertility, Age Structure, and Income. Report to the National Institute of Child Health and Human Development. Bethesda, Md.: National Institute of Child Health and Human Development.
- Leridon, H. 1978. *Fecondité et Structures Démographiques: Une Hypothèse sur l'Évolution de la Fécondité depuis 1940*. *Population* 33:441-447.
- Mason, K. O., J. L. Czajka, and S. Arber. 1976. Change in Women's Sex-Role Attitudes, 1964-1974. *American Sociological Review* 41:573-596.
- O'Connell, Martin. 1975. The Effect of Changing Age Distributions on Fertility and Suicide in Developed Countries. Unpublished Ph.D. dissertation. Philadelphia: Department of Sociology, University of Pennsylvania.
- . 1978. A Cohort Analysis of Teenage Fertility in the U.S. Since the Depression. Paper presented to the annual meeting of the Population Association of America, held in Atlanta, April 13-15, 1978.
- . Forthcoming. The Effect of Changing Age Distribution on Fertility: An International Comparison. In Julian Simon (ed.), *Research in Population Economics*, vol. 1. Urbana: University of Illinois Press.
- Oppenheimer, Valerie K. 1970. The Female Labor Force in the United States: Demographic and Economic Factors Governing its Growth and Changing Composition. *Population Monograph Series No. 5*. Berkeley: University of California.
- Population Index. 1948. The Population Forecasts of the Scripps Foundation. *Population Index* 14 (3):188-195.
- Preston, Samuel H., and J. McDonald. 1977. The Incidence of Divorce Within Cohorts of American Marriages Contracted Since the Civil War. Paper presented at the annual meeting of the Population Association of America, April 1977, St. Louis, Missouri.
- Simon, J. L. 1968. The Effect of Income on the Suicide Rate: A Paradox Resolved. *American Journal of Sociology* 74:302-303.
- . 1969. The Effect of Income on Fertility. *Population Studies* 23:327-341.
- . 1975. Response to Barnes's Comment. *American Journal of Sociology* 80:1460-1462.
- Stouffer, Samuel A., et al. 1949. *The American Soldier: Adjustment During Wartime Life*, vol. I. Princeton: Princeton University Press.

- Sweetser, F. L., and P. Peipponen. 1967. Postwar Fertility Trends and Their Consequences in Finland and the U.S. *Journal of Social History* 1:101-118.
- United Nations. 1962. *Demographic Aspects of Manpower*. New York: United Nations.
- U.S. Bureau of the Census. 1973. *Census of Population, 1970, Detailed Characteristics, Final Report, United States Summary*. Washington, D.C.: U.S. Government Printing Office, Table 226, pp. 761-763.
- Wachter, Michael L. 1972. A Labor Supply Model for Secondary Workers. *Review of Economics and Statistics* 54:141-151.
- . 1975. A Time-Series Fertility Equation: The Potential for a Baby Boom in the 1980s. *International Economic Review* 16:609-624.
- . 1976a. The Changing Cyclical Responsiveness of Wage Inflation. *Brookings Papers on Economic Activity* 1:115-167.
- . 1976b. The Demographic Impact on Unemployment: Past Experience and Outlook for the Future. Pp. 27-100 in *National Commission for Manpower Policy, Demographic Trends and Full Employment*. Special Report No. 12. Washington, D.C.: U. S. Government Printing Office.
- . 1977. Intermediate Swings in Labor Force Participation. *Brookings Papers on Economic Activity* 2:545-576.
- , and S. M. Wachter. Forthcoming. The Fiscal Policy Dilemma: Cyclical Swings Dominated by Supply Side Constraints. Pp. 71-99 in Thomas J. Espenshade and W. J. Serow (eds.), *The Economic Consequences of Slowing Population Growth*. New York: Academic Press.
- Waldron, I., and J. Eyer. 1975. Socioeconomic Causes for the Recent Rise in Death Rates for 15-24-Year-Olds. *Social Science and Medicine* 9:383-396.
- Wellford, C. F. 1973. Age Composition and the Increase in Recorded Crime. *Criminology* (May):61-71.
- Westoff, C. F. 1978. Some Speculations on the Future of Marriage and Fertility. *Family Planning Perspectives* 10:79-83.
- Zajonc, R. B. 1976. Family Configuration and Intelligence. *Science* 192:227-236.

APPENDIX A

Appendix Table 1.—Actual and Projected U.S. Male Population (in Thousands) Aged 15 to 29 and 30 to 64 and the Ratio of Young to Old, 1920–1995, and Crude Birth Rate (per Thousand per Year) 20 to 25 Years Earlier

Date	Male Population Aged		Ratio of Groups Aged 15-29 30-64	Crude Birth Rate	
	15-29	30-64		Period	Rate
Actual					
1920	13,739	20,607	.667	1896-1900	31.6
				1900-1905	30.0
1930	15,955	24,550	.650	1905-1910	29.6
				1910-1915	27.5
1940	17,442	27,664	.630	1915-1920	26.1
				1920-1925	25.0
1950	17,216	31,671	.544	1925-1930	21.5
1955	16,772	33,781	.496	1930-1935	18.3
1960	17,794	35,478	.502	1935-1940	18.3
1965	21,151	36,295	.583	1940-1945	21.2
1970	25,262	38,115	.663	1945-1950	24.5
1975	28,793	38,908	.740	1950-1955	25.2/24.8
Projected					
1980	30,426	42,184	.721	1955-1960	24.6
1985	29,717	46,210	.643	1960-1965	22.2
1990	27,626	50,585	.546	1965-1970	18.1
1995	25,864	54,759	.472	1970-1975	15.9

Sources: Male population by age. 1920–1930: U.S. Bureau of the Census, *Census of Population, 1950, United States Summary* (Washington, D.C.: U.S. Government Printing Office, 1953), pp. 1–93. 1940–1995: U.S. Bureau of the Census, *Current Population Reports*, Series P–25 (Washington, D.C.: U.S. Government Printing Office), as follows for 1940–1950 — No. 98, p. 115; for 1955 — No. 265, p. 25; for 1960 — No. 286, p. 42, Series C; for 1965 — No. 519, p. 20; for 1970–1975 — No. 614, pp. 11–16; for 1980–1995 — No. 704, pp. 40–60, Series II.

Crude birth rate. 1870–1955: Except as noted subsequently, the basic source was Kuznets (1958), p. 37, Table 1, column 4; Table 3, column 5; p. 41, Table 5, column 7; and p. 43, Table 6, column 5 (underlying unrounded quinquennial estimates were used). 1950–1976: U.S. Bureau of the Census, *Current Population Reports*, Series P–25 (Washington, D.C.: U.S. Government Printing Office, September 1977), No. 706, p. 7. The break in the series in 1950–1955 is due to the shift in sources described above.

Appendix Table 2.—Total Fertility Rate, 1940–1977, Relative Employment Experience of Young Adult Males, 1940–1955, and Relative Income Experience of Young Adult Males, 1957–1977

Year	Total Fertility Rate	Relative Employment Experience	Relative Income Experience
1940	2.30	-10.2	
1941	2.40	- 8.6	
1942	2.63	- 5.8	
1943	2.72	- 4.4	
1944	2.57	- 3.2	
1945	2.49	- 2.9	
1946	2.94	- 3.3	
1947	3.27	0.4	
1948	3.11	3.5	
1949	3.11	6.1	
1950	3.09	7.2	
1951	3.27	7.0	
1952	3.36	7.2	
1953	3.42	7.5	
1954	3.54	7.6	
1955	3.58	6.7	
1956	3.69		
1957	3.77		73.1
1958	3.70		73.3
1959	3.71		73.3
1960	3.65		72.7
1961	3.63		71.5
1962	3.47		70.9
1963	3.33		69.7
1964	3.21		68.0
1965	2.93		66.2
1966	2.74		64.8
1967	2.57		65.0
1968	2.48		65.9
1969	2.45		66.2
1970	2.47		66.5
1971	2.28		64.4
1972	2.03		66.5
1973	1.90		64.4
1974	1.86		62.1
1975	1.80		60.2
1976	1.76		58.2
1977	1.82		55.9

Sources: Total fertility rate. Easterlin (1968), p. 247, column 4; unpublished data provided by Campbell Gibson and Martin O'Connell, U.S. Bureau of the Census, Washington, D.C.

Relative employment experience. Easterlin (1973), p. 195, Table 6, columns 5-6.

Relative income experience. Easterlin (1973), p. 185, updated with unpublished data provided by Campbell Gibson, U.S. Bureau of the Census, Washington, D.C.

Appendix Table 3.—Labor Force Participation Rate of U.S. Females Aged 20+ by Age Group, Specified Dates, 1890-1975

Year	Labor Force Participation Rate					
	20-24	25-34	35-44	45-54	55-64	65+
1890	32.7	18.8	16.6	17.4	14.8	9.4
1900	34.2	21.4	18.9	19.1	16.0	10.1
1920	40.0	25.7	23.1	22.8	17.7	9.1
1930	44.3	29.1	25.6	24.6	18.7	9.1
1940	48.1	35.3	31.2	27.3	20.0	7.8
1950	46.1	34.0	39.1	38.0	27.0	9.7
1955	46.0	34.9	41.6	43.8	32.5	10.6
1960	46.2	36.0	43.5	49.8	37.2	10.8
1965	50.0	38.6	46.1	50.9	41.1	10.0
1970	57.8	45.0	51.1	54.4	43.0	9.7
1975	64.3	54.6	55.8	54.6	41.0	8.3

Sources: Labor force participation rates: 1950-1975, *Employment and Training Report of the President* (1977); 1890-1940, extrapolation of 1950 value in *Employment and Training Report* by percentage point change shown in Bancroft (1958), p. 207.

Appendix Table 4.—Index (1940-1945 = 100) of Average Age-Specific Fertility Rate for Specified Age Groups in Each Five-Year Period, 1921-1925 to 1971-1975

Year	Index of Age-Specific Fertility Rate			
	10-49 ^a	20-24	25-34	35-44
1921-1925	1.23	1.08	1.18	1.84
1926-1930	1.07	.97	1.01	1.51
1931-1935	.91	.83	.88	1.20
1936-1940	.88	.84	.87	1.00
1941-1945	1.02	1.00	1.02	1.04
1946-1950	1.23	1.30	1.20	1.10
1951-1955	1.36	1.49	1.31	1.11
1956-1960	1.47	1.69	1.38	1.14
1961-1965	1.31	1.50	1.27	1.01
1966-1970	1.01	1.13	.98	.71
1971-1975	.78	.84	.78	.44

a - Total fertility rate.

Sources: Fertility rates. 1940-1975: U.S. Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970* (Washington, D.C.: U.S. Government Printing Office, 1975), Series B 11-19, and U.S. Public Health Service, *Monthly Vital Statistics Report* (Washington, D.C.: U.S. Government Printing Office). 1920-1939: Extrapolation of 1940 value in *Historical Statistics*, based on series in Wilson H. Grabill, C. V. Kiser, and P.K. Whelpton, *The Fertility of American Women* (New York: John Wiley and Sons, 1958), p. 31.

Appendix Table 5.—Actual and Projected Ratio of U.S. Males Aged 30 to 64 to Males Aged 15 to 29, 1940–1995, and Crude Birth Rate (per Thousand per Year), 1935–1940 to 1970–1975

Year	Males Aged 30–64 15–29	Period	Crude Birth Rate
<i>Actual</i>			
1940	1.586	1935–1940	18.3
		1940–1945	21.2
1950	1.840	1945–1950	24.5
1955	2.014	1950–1955	25.2/24.8
1960	1.994	1955–1960	24.6
1965	1.716	1960–1965	22.2
1970	1.509	1965–1970	18.1
1975	1.351	1970–1975	15.9
<i>Projected</i>			
1980	1.386		
1985	1.555		
1990	1.831		
1995	2.117		

Sources: Same as for Appendix Table 1.

Appendix Table 6.—Unemployment Rate, U_{NI} , and Growth Rate of Consumer Price Index, 1948–1985

Year	Unemployment Rate	U_{NI}	Growth Rate of Consumer Price Index
1948	3.8	4.52	7.77
1949	5.9	4.42	-.97
1950	5.3	4.34	.98
1951	3.3	4.18	7.91
1952	3.0	4.06	2.18
1953	2.9	3.97	.75
1954	5.5	3.94	.50
1955	4.4	3.96	-.37
1956	4.1	3.99	1.50
1957	4.3	4.00	3.56
1958	6.8	4.03	2.73
1959	5.5	4.10	.81
1960	5.5	4.19	1.60
1961	6.7	4.23	1.01
1962	5.5	4.24	1.12
1963	5.7	4.35	1.21
1964	5.2	4.47	1.31
1965	4.5	4.61	1.72
1966	3.8	4.74	2.86
1967	3.8	4.79	2.88
1968	3.6	4.82	4.20
1969	3.5	4.93	5.37
1970	4.9	5.05	5.92
1971	5.9	5.17	4.30
1972	5.6	5.31	4.12
1973	4.9	5.40	5.38
1974	5.6	5.44	10.97
1975	8.5	5.46	9.14
1976	7.7	5.49	5.77
1977	7.0	5.52	5.98
1978		5.49	
1979		5.44	
1980		5.35	
1981		5.23	
1982		5.13	
1983		5.00	
1984		4.84	
1985		4.71	

Sources: Unemployment rate: *Economic Report of the President* (Washington, D.C.: U.S. Government Printing Office, 1976), p. 199, Table B-24.

U_{NI} : Easterlin et al. (1978), Table 2. See text for explanation.

Consumer price index: *Economic Report of the President* (Washington, D.C.: U.S. Government Printing Office, 1976), p. 221, Table B-43. Smoothed values plotted in Figure 4 are averages for 1940–1950 and five-year periods 1950–1955 through 1970–1975.

Appendix Table 7.—Children Ever Born and Deviations From Trend in Divorce Among Marriage Cohorts of 1910-1914 Through 1955-1959

Year of First Marriage	Children Ever Born	Expected Percentage of Marriages Ending in Divorce Less Actual Percentage Ending in Divorce: Average of 5-year Deviations From Trend
1910-1914	3.337	.67
1915-1919	2.968	-.19
1920-1924	2.637	-.16
1925-1929	2.449	-1.00
1930-1934	2.417	-2.29
1935-1939	2.497	-1.63
1940-1944	2.605	-.88
1945-1949	2.838	2.10
1950-1954	2.984	1.67
1955-1959	2.734	.09

Source: Preston and McDonald (1977).

Appendix Table 8.—Homicide Rate Among Males
Aged 15 to 24 and 25 to 34 (per 100,000 Population),
1940-1975

Year	Homicide Rate	
	Age Group	
	15-24	25-34
1940	7.3	12.2
1941	7.3	11.5
1942	7.1	11.2
1943	6.1	8.9
1944	5.9	9.4
1945	7.1	10.5
1946	7.7	11.6
1947	7.4	11.2
1948	7.3	10.8
1949	6.7	9.6
1950	6.3	9.9
1951	5.9	9.1
1952	6.2	10.0
1953	5.9	9.0
1954	5.9	9.1
1955	5.4	8.8
1956	6.0	9.0
1957	5.7	9.0
1958	5.8	8.7
1959	5.8	9.3
1960	5.7	9.6
1961	5.7	9.6
1962	5.9	9.9
1963	5.8	10.2
1964	6.3	10.5
1965	6.8	11.9
1966	7.6	12.3
1967	9.1	14.6
1968	10.1	15.3
1969	11.0	15.9
1970	11.7	16.6
1971	12.7	18.5
1972	13.5	18.5
1973	13.4	19.0
1974	14.2	19.3
1975	13.7	18.4

Appendix Table 9.—Percent of Males Aged 18 to 24 and 25 to 34 Answering Affirmatively on Two Measures of Political Alienation, 1952–1976

Year	18–24		25–34	
	Agrees that Government is Too Complicated	Agrees that He Has No Say In Government	Agrees that Government is Too Complicated	Agrees that He Has No Say In Government
1952	71.4	26.1	69.9	29.9
1956	62.2	23.4	60.1	24.5
1964	65.7	24.5	61.8	22.9
1966	59.5	25.0	67.0	31.2
1970	72.3	34.9	67.7	32.9
1972	71.9	41.0	74.4	38.1
1974	69.9	35.9	67.9	33.3
1976	72.0	39.9	70.4	41.3

Appendix Table 10.—Average SAT Score and Average Birth Order for Annual Birth Cohorts, 1940–1960

Testing Year	Average SAT	Year of Birth	Average Birth Order
1956–1957	484.5	1940	2.78
1957–1958	484.0	1941	2.69
1958–1959	486.5	1942	2.57
1959–1960	487.5	1943	2.63
1960–1961	484.5	1944	2.75
1961–1962	485.5	1945	2.78
1962–1963	490.0	1946	2.55
1963–1964	486.5	1947	2.44
1964–1965	484.5	1948	2.52
1965–1966	483.5	1949	2.59
1966–1967	479.0	1950	2.63
1967–1968	479.0	1951	2.64
1968–1969	478.0	1952	2.70
1969–1970	474.0	1953	2.74
1970–1971	471.5	1954	2.78
1971–1972	468.5	1955	2.82
1972–1973	463.0	1956	2.86
1973–1974	462.0	1957	2.88
1974–1975	453.0	1958	2.92
1975–1976	451.5	1959	2.94
1976–1977	449.5		

Sources: SAT scores: Educational Testing Service, Princeton, New Jersey.

Birth order: Zajonc (1976), p. 233.

APPENDIX B

It is theoretically possible that labor demands for younger persons might shift in a way compensating for differential changes in labor supplies. For example, the occupational structure of the economy might have changed in a way more favorable to the demand for younger workers in the period of growing abundance of the young compared with that of growing scarcity, that is, in the post-1960 period compared with 1940-1960. If occupations in which young persons account for a disproportionately large share of employment, such as operatives, laborers, and clerical workers, grew at much higher rates relative to the average after 1960 than before, then this would generate a favorable change in the demand for younger workers that would tend to compensate for the adverse change in their supply. To test for this, the following calculation was made using a procedure similar to that of Oppenheimer (1970). At each decennial date from 1940 through 1970, and 1976, the actual total employment (for both sexes, all age groups) in each major occupational class was multiplied by an estimated "standard" share of persons aged 14 to 34 in that occupation. (For each occupation, the "standard" share was the simple average of the actual shares of young persons in that occupation at the decennial dates 1940-1970.) This yielded for each date the hypothetical number of young persons that would have been employed in that occupation if young persons had had their standard or usual share in the occupation. For example, for 1940, the actual total number employed in professional and technical occupations at that date, about 3.5 million, was multiplied by the standard share of persons aged 14 to 34 in that occupation, 42.9 percent, to obtain a hypothetical employment of 1.5 million persons aged 14 to 34 in the occupation at that date. The resulting products for each occupation class were then summed to obtain the hypothetical total employment of

young persons at each date. Comparison of the average rate of change in this total during 1940-1960 with that for 1960-1976 shows how the actual change in the economy's occupational structure would have affected employment of the young if they had always been employed at standard rates for each occupational class. The results are, for 1940-1960, 2.12 percent per year and, for 1960-1976, 2.13 percent. One may have reservations about the technique used here, but, so far as it goes, it implies that the occupational structure did not change in the post-1960 versus pre-1960 period in a way that generated labor demands that would have compensated for the adverse change in labor supply of the young.

The occupation distribution of total employment was from the following sources. 1940-1960: unpublished BLS CPS data, excluding Alaska and Hawaii; 1960, 1970, 1976: *Employment and Training Report of the President* (1977), Table A-115, pp. 161-167. The matrices of occupation by age and sex used in computing the standard shares were, for 1940-1960, from unpublished BLS CPS data; for 1970, from U.S. Bureau of the Census (1973), Table 226, pp. 761-763.

APPENDIX C

To test whether the demand for younger women was especially favored by changes in the economy's occupational structure in the period 1960-1976, as compared with its structure in 1940-1960, the same procedure was used as that described in Appendix B, except that the standard shares refer to females aged 14 to 34, rather than all persons aged 14 to 34. The results show a hypothetical growth in the employment of younger females during 1940-1960 of 2.89 percent per year; during 1960-1976, of 2.74 percent. It does not appear, therefore, that after 1960 the occupational structure of the economy changed in a way that especially favored the employment of young women, as compared to before 1960.

APPENDIX D

Given below is the percentage distribution of the incremental change in female employment before and after 1960:

	<u>1940- 1960</u>	<u>1960- 1970</u>	<u>1970- 1976</u>
All females	100.0	100.0	100.0
Professional/ technical	11.0	20.5	24.0
Managerial	6.2	2.8	11.4
Clerical	40.0	46.4	37.1
Sales	8.3	5.3	4.9
Craftsmen	1.0	1.4	3.9
Operatives	11.0	12.4	-2.7
Laborers	-0.2	0.7	4.9
Private household service	-0.2	-5.5	-7.8
Other service	18.4	21.5	25.4
Farmers	-0.6	-0.4	0.2
Farm laborers	5.2	-5.2	-1.4

Although the recent changes show some

departure from the 1940-1960 pattern, the similarities with the past are more impressive than the differences. Moreover, in those occupations in which women have made a significant breakthrough—in the sense of a substantial increase in their share of employment in the occupation—it might be argued that often the occupation has been downgraded in socioeconomic status and potential for further advancement. For example, the position of bank teller, which today is almost entirely female but four decades ago was predominantly male, appears to have been largely demoted to a clerical function. Sources for the data above are: for 1940-1960, unpublished BLS CPS data, excluding Alaska and Hawaii; for 1960, 1970, 1976, *Employment and Training Report of the President* (1977), Table A-115, pp. 161-162.

News and Events

MENU



Home

News

Events

Media Mentions

Contact Us



Richard Easterlin made significant and lasting contributions to USC's reputation in the field of economics.

NEWS > FACULTY



demographics

USC Dornsife Professor Emeritus challenged conventional wisdom on economic growth and well-being, leaving a lasting impact on the field of happiness economics.

By **USC Dornsife News Staff** December 20, 2024

Richard A. Easterlin, Professor Emeritus of Economics at the USC Dornsife College of Letters, Arts and Sciences and renowned “father of happiness economics,” has died. He was 98.

Easterlin was a trailblazer who connected economics with human well-being and demographic trends, producing influential works that continue to shape economic thought and policy discussions.

“He contributed substantially to the measurement of happiness and explanations for changes in it over time,” said Professor Emeritus of Economics Jeffrey Nugent. “Easterlin himself always seemed happy, with a bright smile and sense of humor, making him a delightful colleague and mentor to his students, but always pointing to the importance of the economics of happiness.”

A journey through economics

Easterlin was born Jan. 12, 1926, in Ridgefield Park, N.J. After earning a master’s degree in engineering at Stevens Institute of Technology, he switched fields to economics, earning a master’s degree and then a PhD at the University of Pennsylvania in 1953, where he remained as a faculty member for more than three decades. In 1978, he was appointed to the William R. Kenan Jr. Professorship in Economics at Penn.

In 1982, Easterlin joined USC, where he spent the remainder of his distinguished career. He served as a professor of economics and later as a University Professor at



EMERITUS IN 2018.

Groundbreaking contributions in economics

Easterlin's work revolutionized the understanding of the relationship between economic growth and human happiness. His 1974 paper "Does Economic Growth Improve the Human Lot?" introduced what became known as the Easterlin Paradox. This influential theory proposed that at a national level, happiness does not necessarily increase with income over time, challenging conventional economic wisdom.

Through another major contribution, the Easterlin Hypothesis, he explained long-term demographic trends such as baby booms and busts. This theory suggests that fertility rates are influenced by relative income rather than absolute income levels.

Throughout his career, Easterlin focused on various aspects of economic well-being and demographics, including:

- Changes in subjective well-being throughout life.
- The complex relationship between economic growth and happiness.
- Long-term demographic trends and their economic implications.
- The American baby boom and subsequent baby bust.

Easterlin's innovative work earned him numerous accolades and honors. He was elected to the National Academy of Sciences and the American Academy of Arts and Sciences, and he was awarded the IZA Prize in Labor Economics, among the most prestigious in the field of labor economics. The American Economic Association recognized him as a Distinguished Fellow, and he served as president of both the Population Association of America and the Economic History Association.

In 1988, Easterlin was awarded a Guggenheim Fellowship, cementing his status as a leading figure in his field.



highlighting the intricate relationships between economic growth, demographics and life satisfaction. His 2021 book, *An Economist's Lessons on Happiness: Farewell, Dismal Science!*, distilled more than five decades of research into the nature of human happiness. His seminal work offers surprising insights into the factors that contribute to well-being, illuminating a more nuanced understanding of happiness that emphasizes the importance of factors beyond economic prosperity.

Reflecting on his life's work, Easterlin once said, "My hope is the same as it was for my undergraduates — that it may help to make people's lives better." It's a sentiment that encapsulates his desire to improve human welfare through economic research and education.

Easterlin's commitment to sharing his knowledge extended beyond his writings. In 2021, he participated in a Dornsife Dialogue, during which he discussed his research and insights on happiness economics. His advice, distilled from decades of research, was characteristically straightforward yet profound: "Focus on your family; stop comparing your status to others." This simple statement reflects the core of Easterlin's findings on what truly contributes to happiness, emphasizing the importance of relationships over material wealth.

His innovative approach to economics, combining rigorous analysis with a deep concern for human welfare, has inspired generations of economists and policymakers to consider a broader range of factors when assessing societal progress and well-being.

Easterlin is survived by his wife, USC University Professor Eileen Crimmins; children Dan, Nancy, Sue, Andy, Matt and Molly; and grandchildren Zack, Emma, Keaton, Tyler, Ryder, Owen, Ada and Enzo.

A celebration of life will be held Jan. 12 at 2 p.m. at Brookside Golf Club in Pasadena, Calif.

The family requests donations be made to Young & Healthy in lieu of flowers.