

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Charles F. Westoff PAA President in 1974-75



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

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

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CHARLES F. WESTOFF

PAA President in 1974-75 (No. 38). Interview with Jean van der Tak at the Office of Population Research, Princeton University, May 10, 1988.

CAREER HIGHLIGHTS: Charles Westoff was born in New York City in 1927. He obtained a B.A. in international relations in 1949 and an M.A. in sociology in 1950 from Syracuse University and the Ph.D. in sociology in 1953 from the University of Pennsylvania. He was a research associate with the Milbank Memorial Fund in New York for two years before going in 1955 to Princeton, where he has been Professor of Sociology (since 1962) and of Demographic Studies (since 1972), Chairman of the Department of Sociology (1965-70), and Associate Director (from 1962) and then Director (since 1974) of the Office of Population Research. In 1958-62 he was also Associate Professor of Sociology at New York University. He was Executive Director (with Robert Parke, Jr.) of the Commission on Population Growth and the American Future (1970-72) and has been an adviser to the Census Bureau and the Demographic and Health Surveys, a director of the Alan Guttmacher Institute and the Population Resource Center, and a member of the Committee on Population of the National Academy of Sciences.

He is famous in the field of demography for his influential work and publications on fertility in the U.S. and developed and developing countries. His Ph.D. dissertation analyzed data from the 1941 Indianapolis Fertility Study. He was director (1955-70) of the Princeton Fertility Study and codirector, with Norman Ryder, of the National Fertility Studies of 1965, 1970, and 1975. He designed the core questionnaire for the World Fertility Survey, with Norman Ryder, and designed the basic questionnaire for the Demographic and Health Surveys. He is author or coauthor of more than 100 journal articles and book chapters and at least dozen books. [He has been Professor Emeritus since 1999.]

VDT: What led to your interest in demography and particularly in fertility?

WESTOFF: When I finished graduate school [at Pennsylvania], I worked for two years at the Milbank Memorial Fund in New York, before I came to Princeton. It was Clyde Kiser who was responsible for getting me interested in fertility research.

VDT: How did Clyde Kiser know about and find you?

WESTOFF: It was through Dorothy Thomas. Dorothy Thomas had been my major professor at Penn and I guess Clyde had been in contact with Dorothy, looking for some graduate students who would be interested in developing a thesis around the as-yet-not-completed Indianapolis Study. That was the first large-scale survey of fertility ever done.

VDT: And the data sat there unanalyzed.

WESTOFF: The war interrupted it. There were lots of loose ends to that project. He was looking for graduate students, and graduate students are always looking for a thesis topic. So it was a nice marriage of common interests.

[Adding to the interviewer's sketchy biographical introduction]: I was an associate professor of sociology at New York University for four years, 1958-62. I was at the Milbank Memorial Fund from 1952 or 53 to 1955. I came to Princeton in 1955. Then, while still on the research staff here, I went on a part-time basis here and took the faculty position at NYU in Greenwich Village. I taught there for four years and came back to Princeton full-time in 1962. With the exception of that stint in

Washington with the Commission on Population Growth and the American Future, I have been here uninterruptedly.

VDT: How did you get into demography?

WESTOFF: I guess it was when I was an undergraduate at Syracuse. I read some of these scare books about how population was beginning to explode. There was a book by Guy Irving Burch and Elmer Pendell [Population Roads to Peace or War, 1945]. I became fascinated with the power of exponential growth. I think that's what first got me started, first turned me on intellectually to the issue of population.

Then I took a couple of undergraduate courses and then did a master's thesis on social mobility and fertility. Then I got an offer of a teaching assistantship at the University of Pennsylvania, in which I could earn some money and have free tuition and go to graduate school at the same time. It was a department of sociology that had a strong population contingent.

VDT: Dorothy Thomas was your professor there. Were you doing migration?

WESTOFF: I was sort of the maverick of the crowd. Everybody else was. Not everybody, but I don't remember anyone else being interested in fertility. Two of my colleagues at the time were Sid Goldstein and Dick Easterlin, though I didn't see much of Dick; he was in the economics department. I think that was before he was interested in fertility. It was a department that concentrated a lot on migration. And it was only, I think, because of the chance contact with Clyde Kiser that I was sort of rescued from the department specialty. I think fertility is a lot more interesting than migration.

VDT: What was Dorothy Thomas like?

WESTOFF: She was--one of the adjectives that quickly comes to mind--a tremendous bundle of enthusiastic energy. Enthusiasm is a big key to her personality. And she would kind of adopt graduate students and push them hard. I was one of her favorites at the time, as were Everett Lee and Sid and others who were closer to her own interests. She was an extremely supportive person, who was always motivated to get the students finished with all their requirements and get down to the serious business of doing their research.

VDT: She must have felt you deserted her if you went off to the Milbank Memorial Fund.

WESTOFF: She may have.

VDT: You did the requirements for the Ph.D. with her?

WESTOFF: Not only with her; she was the main person. Ed Hutchinson was also on the faculty and I studied with him. His interests were quite different too; he was interested in immigration. He had a very keen mind and I learnt a good deal of what might be called--or I got a good taste of the logic of scientific inquiry, methodology in its logical sense, working with Ed.

VDT: What was Richard Easterlin like?

WESTOFF: I don't remember. Actually, my close friends there were not people in population. Marvin Bressler, for example, who was a student there and has been for the past 15 years chairman of the sociology department at Princeton. He and I go back a long way together. And several other

people who are not demographers.

I think all careers have this kind of curious history of part accident--of what determines your interest in a career. You bump into it at a particular time--serendipity. If you look for any great plan for my life, I think you'll be disappointed.

VDT: Has there ever been anyone who started off saying, "I'm going to be a demographer"?

WESTOFF: I don't think so.

VDT: What was the topic of your dissertation? What data did you use from the Indianapolis Survey?

WESTOFF: I first got interested in the social psychology of fertility and did some work in that area. They had asked a lot of questions designed to measure women's self-confidence and personal adequacy or inadequacy and how that related to fertility. Which it did not.

Subsequently, when I came to Princeton after Milbank, we put a heavy emphasis in that so-called Princeton Fertility Study on trying to tap various personality dimensions to hook them into planning--the propensity to plan effectively--how that translates into contraceptive practice; how it might be responsible for contraceptive failure and so forth. But none of that ever turned out. I sort of gave that up many years ago.

VDT: Why did you go on with that in the Princeton study when it really hadn't yielded much in the Indianapolis study? You felt you had to find something there?

WESTOFF: Yes--despite all the evidence. Well, there's always the possibility that what you are really after hasn't been conceptualized clearly, hasn't been measured accurately. The last thing you do is give up the theory. You always think you can improve the measurement first. Well, I had enough of that and gave up after the first wave of the Princeton Fertility Study was over. Haven't gone back to it since.

VDT: The psychologists got into it [fertility research] in the 1970s. I worked with Henry David and have been in touch with the social-psychological angle. They're still trying.

WESTOFF: I think it's a waste of time. A lot of my thinking, I think, about the nature of fertility has been shaped by my close work, contacts, with Norman Ryder. We collaborated on several books in subsequent years. I think I have moved much more in the direction of thinking of fertility as the property of an aggregate, as a population aggregate. I don't think it's useful, for demographic purposes, to think of it in individual psychological terms.

VDT: That sounds plausible. You were director of the Princeton Fertility Study during all its 16 years. You started in 1955, when you came from the Milbank Memorial Fund?

WESTOFF: Actually, that research was begun [in 1954] at the Milbank Fund by a large steering committee that was set up, where Frank Notestein played a major role--and Clyde Kiser. I got moved down to Princeton on July 1st, 1955.

VDT: That was an interesting survey; one of the first in this country, with that select sample of women who'd had a second birth in September 1956 in metropolitan areas. I had here that it, like Indianapolis, found little in the way of influential social-psychological variables. You've just confirmed that. You said then--I glanced through The Later Years of Childbearing [1970]--that a

longitudinal survey was the best way to follow U.S. fertility. Do you still believe that?

WESTOFF: It's only through a longitudinal survey that you can answer certain kinds of questions. I don't want to open this whole line of discussion, because it's endless, but I think they're greatly overrated. The longitudinal design is greatly overrated for purposes of studying fertility. And it's an extraordinarily expensive and time-consuming way of doing research.

Now, it is one way of evaluating--and we mined the data quite extensively for this purpose--one way of assessing the reliability of such information that you collect. And we learned a lot in that effort by reinterviewing the same women about the same events at two different points in time and measuring the consistency of response. You can learn a great deal about what sort of biases occur--recall of critical pieces of information in the study of contraceptive failure rates, for example. We learned that there was some tendency for women who initially reported a pregnancy to be the result of a failure with the rhythm method to subsequently report about the same event that they had not used contraception. That's a plausible kind of thing, but it does have an effect on calculating failure rates that's not trivial. We learned, I think, a lot of things of that kind, but those are methodological results.

We did a longitudinal study also later on in the National Fertility Study [reinterviewing in 1975 respondents from the 1970 survey] and repeated a lot of these same kinds of analysis. There were a lot of substantive things that came out of these, but the major finding was that women are not very successful in forecasting their own fertility--individually. In the aggregate, it's not bad.

Once again, I think there's another lesson that the appropriate level of demographic analysis is in the aggregate. Let the compensating error work in the aggregate. Some women say they're going to have a child and don't, and others say they're not going to and do. A lot of this tends to balance out, so you can get fairly accurate predictions from the percent of women who say they're going to do something, rather than relying on the stability of individual behavior.

VDT: That was one of the great findings between the 1965 and 1970 National Fertility Studies, that they did balance out in the aggregate. Then you had the longitudinal study following the actual individuals [1970 to 1975] and that was pretty much what you also found. What led you to suggest the National Fertility Study of 1965 and then 1970 and 1975, following the Growth of American Families studies of 1955 and 1960?

WESTOFF: I can remember that quite vividly. Norm Ryder and I were attending a conference on population genetics over here in the Princeton Inn, before it became a coeducational dormitory. One of our great losses, the loss of that facility. It was a hotel and a lovely restaurant.

VDT: I know. My husband and I came here on our honeymoon. I had a brother at Princeton and his wedding gift to us was a night at the Inn. Cost \$12 then [1952] and it was a big gift.

WESTOFF: We learned at the meeting that the Growth of American Families study, which was at Michigan--the Scripps Foundation [at Miami University, Oxford, Ohio] developed it and Freedman and Whelpton and later Arthur Campbell collaborated, and Norman Ryder was part of it in the early days; he was out at Miami University. We learned at the meeting--this would have been about 1964 [Christmastime 1964, according to Ryder]--that this was not going to be repeated; people were doing other things. Ryder and I, with the arrogance of youth, said, "What the hell, we can do this out of our hip pocket with all of our survey experience"--him with the original [1955] GAF study and me with the Princeton Fertility Study.

I've never learned that lesson yet in my life, that you can't just take a questionnaire and cross out a few things, add a few things, and go with it. A year later you're still fooling around with that questionnaire. I've had that experience over and over again, with the World Fertility Survey and more

recently with the Demographic and Health Surveys. We spent a year devising a [DHS] questionnaire I thought we could knock off in a couple of weeks.

So we said, "Let's do it." And there was some funding available. I don't remember the details of this too well, but I think originally it was the National Cancer Institute. This was the time the pill was getting some use. I think it had been licensed in 1963 [1960] and was beginning to get used by increasing fractions of American women. And there was some concern about getting some baseline data, demographic characteristics, on who was using it. Although the Cancer Institute was not interested in fertility studies, they were interested in the data on contraception, because it is a national sample. So that gave us an additional impetus. It also coincided with the creation of the National Institute of Child Health and Human Development [which actually funded it. See Ryder interview.]. I remember that Eunice Kennedy Shriver was involved in that. So that's how it happened. A chance discussion at the Princeton Inn started the whole thing.

VDT: It certainly led to a magnificent series. It wound its way through the World Fertility Survey, the Contraceptive Prevalence Surveys. Were you involved in that too?

WESTOFF: No, the Contraceptive Prevalence Survey I was not involved in.

VDT: But you say you've been involved in devising the questionnaire for the Demographic and Health Surveys?

WESTOFF: Yes. And Norman Ryder and I designed the World Fertility Survey questionnaire. Then I took responsibility for this DHS questionnaire design. As I said, it was a year's work. These things are not done quickly. And now, working with the results of it, I'm already making a mental checklist of things that have to be changed. But it's the nature of the whole enterprise--continuous improvement in measurements.

VDT: You have a big niche in the history of these surveys. It sounds like you and Norman Ryder almost singlehandedly carried them through. Well, you had a few predecessors, like Clyde Kiser.

WESTOFF: Ron Freedman.

VDT: Of course, and Whelpton. You made a tremendous contribution to a tremendous achievement, despite Kingsley Davis's very critical review of the World Fertility Survey. Have you seen that, in the Sociological Forum ["The World's Most Expensive Survey," Special Issue: Demography as an Interdiscipline, Fall 1987]?

WESTOFF: No, I saw a reference to it.

VDT: In his usual way, he says that it lacks socioeconomic background, the social and cultural setting in which these parameters unfold.

WESTOFF: Well, that's true. I think the criticism can be made, which I make quite frequently, that they have not made any great contributions to understanding of the social and economic determinants of fertility. Maybe surveys are not appropriate instruments for that kind of research. But they have led to an enormous expansion of our knowledge of the proximate determinants of fertility. I don't know whether Kingsley said that or not, but he should if he didn't.

VDT: Well, he does say they led to large masses of data.

WESTOFF: I saw the title of it, "The World's Most Expensive Survey."

VDT: That's right. He wasn't sure it was worth the \$40 million spent on it. That's peanuts compared to many other things. What do you consider the most important findings to come out of the National Fertility Study? Of course, much of this was covered in that series, initiated by Barbara Wilson, of videotaped interviews with directors of fertility surveys. [Six interviews conducted in summer 1985, shown at the 1986 PAA meeting. See PAA history "vignette" by Barbara Wilson, "Videotaped Interviews About American Fertility Surveys," PAA Affairs, Winter 1985.] But off the top of your head, what do you think were the most important findings to come out of all three?

WESTOFF: I think the single most important finding of that research, although that represents my own kind of pet interest, was the demonstration of the demographic significance of the level of unwanted fertility in this country.

VDT: That was the big finding of the 1965 survey already and everybody was waiting to see what happened in 1970.

WESTOFF: It came out in the 1970 survey as well. It also explained the differences between black and white fertility. I remember getting into a television debate with Jesse Jackson, in fact, in 1973, I think it was, following the Commission report [Population and the American Future, report of the Commission on Population Growth and the American Future, 1972]. He was one of the critics on a WGBH national television program, which also included Ben Wattenburg and Marjorie Mecklenburg. Jackson has changed his tune completely on this subject now, but at that time he was arguing the genocide line, that we were pushing or interested in birth control because it would reduce the political power of blacks. He said that if you looked at the black mayors in a lot of Midwestern cities, this was what we were afraid of. It occurred to me, fresh from these fertility surveys, and I then said that I'd done a great deal of research on black fertility in this country and it was quite clear that a lot of black women were having babies that they didn't want to have. It was not exactly in their best interests, especially teenagers. And I said, "Are you arguing that it's in your political interest that they should continue to do this?" That sort of stopped the argument; it was an effective response. Now, I should say to his credit, one of his most effective speeches is on teenage fertility in the black community and all the liabilities that go with it.

That information--provoked as a matter of fact by Fred Jaffe [of the Alan Guttmacher Institute], who pushed me into writing this paper on unwanted fertility which we did with Larry Bumpass. It was published in Science magazine many years ago ["The Perfect Contraceptive Population: The Extent and Implications of Unwanted Fertility in the U.S., 1960-65," Science, Vol. 169, pp. 1172-1182, 1970]. Had a lot of impact and it continues to have impact now in developing countries.

VDT: That's a very famous paper, indeed. Let's talk about the Commission on Population Growth and the American Future. That was a tremendous task. The implementing legislation for the Commission was passed in March of 1970 and in March 1972 you produced a report. As staff director, you had to collect over a hundred research reports; there were all those meetings. How did you manage all that in that short time?

WESTOFF: And I wasn't even on leave.

VDT: You were on two-thirds time.

WESTOFF: That's right.

VDT: You were still lecturing at Princeton; you went back and forth?

WESTOFF: Yes. That was a fascinating experience, in retrospect. I'm sure I must have complained continuously during it. It was an awful challenge.

VDT: How come you were chosen to be head of that staff? The leading demographer, known to be a good administrator?

WESTOFF: I don't know why I was chosen. You've got to be lucky, I guess. I guess it was at the suggestion made by Barney [Bernard] Berelson to John Rockefeller [3rd] and many very interesting and sort of humorous conversations I had with Rockefeller when he was trying to recruit me for this task. I would relay these conversations to Berelson. One of the central themes of it was that I was going to have to commute back and forth because I wasn't going to leave Princeton and that was going to be pretty damn expensive--maintaining two households, an apartment in Washington, and transportation back and forth. I kept having these conversations with Mr. Rockefeller on how are we going to do this? Barney finally got an idea. "Next time he asks you that question," he said, "say, 'Mr. Rockefeller, if you know a rich man by any chance' . . .". And, indeed, that turned out to be the solution. Because I ended up working for Mr. Rockefeller rather than for the federal government. That's a lot nicer.

VDT: Why did you do that? More money?

WESTOFF: Yes. He was paying all these additional expenses, whereas if I was working for the government, he could not have supplemented my income, because it would have been contrary to government regulations.

VDT: Why did you commute? I phoned up to ask about trains the other day and there's only one a day to Princeton Junction.

WESTOFF: There used to be a lot more. They didn't go to Princeton, they went to Trenton, and the Metroliner used to stop at Trenton all the time.

VDT: It still does.

WESTOFF: Only once or twice a day, but it was regular then, so I took the Metroliner.

VDT: And you really kept up classes, students?

WESTOFF: I don't remember what I did, but I did it. It was pretty tiring. But it was a stimulating time of my life, which I remember fondly. Beginning with Rockefeller, it was a great experience. Indeed, some of the people who were on the Commission became close friends. George Woods became a very close friend of mine, Missy Chandler of Los Angeles. And I think the report had some impact. It fell, of course, on the somewhat unresponsive ears of Mr. Nixon.

VDT: Maybe not unresponding, but responding in a way that one didn't want at that time--the reactions to the proposals for teenage contraceptive services and legalized abortion.

WESTOFF: Yes, that's right. The report happened to coincide with the beginning of the 1972 presidential campaign and Nixon just couldn't resist using it to pick up points with the Right-to-Life people and the right-wing political groups.

VDT: Already then. But other than that, do you think it has had some long-lasting effect? I picked it up the other day; there was an incredible range of people who contributed to the research reports and on the Commission itself.

WESTOFF: Oh, I think a lot of those papers continue to be referred to. The work that Ansley Coale did, for example, which was a little technical appendix and which turned out to be quite important, on the implication of immigration for achieving zero population growth in this country. He worked on the formal demography of that, a statistical appendix paper. The whole volume on the economic and environmental consequences of population growth. That was the first time that that whole subject had ever really been frontally approached. A lot of research has been done on the Third World--what are the economic implications of population growth--but not for developed countries. So that, I think, was breaking new ground.

VDT: You showed that through the various impacts of the two-child or the three-child family?

WESTOFF: That's right--the environmental and economic consequences. Then I think the report itself, which went to the White House and Congress, was--if I may be immodest--very well done. I think a lot of the intellectual quality of that has to be credited to Berelson, because he and I worked very closely.

VDT: He was sort of an adviser to you?

WESTOFF: No, he was on the Commission, but he played a very active role. And as anybody who worked with him closely, or knew him well, would quickly recognize, in his ability to hammer away at certain points, making sure the argument held water, trying to politically bring in some of the minority opinion on the Commission, trying to get a consensus, there was something of a good politician as well as an intellect. Dudley Duncan was on that Commission too and he was one of those who had problems with that.

VDT: In what way?

WESTOFF: He wanted to sort of hit everybody on the head with things, like the birth rate in this country is too damn high, ruining the country, and abortion is a good thing because it will bring down the birth rate. Stuff that would get you politically cut off at the knees. So we had a lot of fun, and a lot of frustration working with people who were politically oriented. But that's natural, I suppose.

VDT: You mean those who were on the Commission?

WESTOFF: Some outside people, outside the Commission.

VDT: There were four Congressmen on it.

WESTOFF: Senator Packwood was very, very interested and went to the meetings and participated in it. There was a congressman.

VDT: Scheuer?

WESTOFF: He came into it at the end, made a lot of noise. He's been very supportive of population activities, but Packwood made more contribution. There was a congressman from Illinois, John Erlenborn, who was really superb. He was very faithful in coming to meetings and had to be convinced, was quite concerned about things, but ended up developing a real interest. I later encountered him at an international conference on population. Good guy.

VDT: Of course, the Commission report came out just the year U.S. fertility dropped to replacement level. Like the committee that we had in the 1930s [Committee on Population Problems of the National Resources Committee], set up because of concern about population decline, that came out with its report [1938] just as the baby boom was about to kick off.

WESTOFF: Yes, that's right. And abortion was legalized at the same time [Roe v. Wade, January 1973]. That cut off a lot of things.

VDT: Both reports [1938 and 1972] have been highly praised, that you were balanced and you came out in favor of the welfare of youth and you didn't use scare tactics.

WESTOFF: I think the essential theme of the recommendations of the report was--there was some sort of fancy political footwork on this--but what we did was that we did not argue for population stabilization in a direct sense, which Dudley Duncan, for example, would have preferred--that zero population growth would be good for the nation and therefore we ought to try for it. But rather that the control of fertility, the avoidance of unwanted pregnancies, was good for individuals and individual families and for the wanted children. And this is sort of equivalent to, as it turned out, ultimate replacement fertility and zero population growth. That would be a consequence of doing what's good for the individual and for the family, which has this additional salutary consequence of leading to zero population growth, but emphasizes the immediate micro objective, rather than the long-range macro objective. The long-range objective is a consequence of this micro-level consideration or concerns. I think that was a nice bit of footwork that we did in there that made it much more politically palatable. So what you do is that you arrange the environment in such a way that people can maximize their self-interest. The consequence of this self-interest is a macro result, ZPG, which for other reasons we think is also advantageous.

VDT: That's a marvelously psychological way to handle people.

WESTOFF: It dissolved a lot of opposition, I think. Disarmed a lot of opposition to what otherwise might be seen as a sort of Machiavellian policy.

VDT: Why don't they emphasize more reducing unwanted fertility in developing countries? Of course they do, but then there are also those who say you've got to reduce wanted fertility too.

WESTOFF: I'm working on that, as a matter of fact. I'm writing a paper for a National Academy of Sciences meeting in October and I was working this morning on the subject of unwanted fertility, using some data from Peru.

VDT: From their Demographic and Health Survey?

WESTOFF: Yes--from Peru, Colombia, Dominican Republic, and Sri Lanka. All DHS surveys.

VDT: Isn't it marvelous how fast their data are available?

WESTOFF: That's the beauty of the use of micro-computers in the input of the data in the countries. The interviewing stops on Tuesday and by Friday you've got a data bank. It used to take a year.

What I was writing about this morning and you can see very clearly from the data is that it's true that even if you eliminated all unwanted fertility in several of these countries, you would still be a distance from replacement fertility. However, if you look at the wanted fertility of the urban populations and of the more educated, secondary-school educated, the wanted fertility is down at replacement. So you can see a future coming with more education and improvements in contraception, which will affect the unwanted fertility of rural populations and the less educated, and also the process of modernization and urbanization will also reduce the levels of wanted fertility.

VDT: As demographic transition theory has already said.

WESTOFF: Yes.

VDT: Despite all the exceptions to that, as found in the European Fertility Study. That's what Kingsley Davis has always hammered at, you have to bring down the wanted fertility. But it's nice to know that you've cleverly found that low wanted fertility exists in one segment of society.

WESTOFF: Yes, but in the segment that the rest of the population is moving toward. It's like looking at Sweden if you want to find out what's going to happen in the United States.

VDT: Yes--living together. Although sometimes you look at Sweden two centuries ago and say it's going to happen. What people are concerned about is whether it's going to happen fast enough, bring fertility down soon enough.

WESTOFF: Well, it is. Look at Brazil; the birth rate has just collapsed there. In their DHS, the fertility rate is down to 2.8.

VDT: National--2.8, incredible! Even including the Northeast?

WESTOFF: Yes, including the Northeast.

VDT: That's what makes this whole series of fertility surveys that you've been involved with so exciting, that you can find these things, and reliably. Of course, they can be found retrospectively in censuses--whenever they're taken.

WESTOFF: All I can think of is what's going to happen in Africa. Because looking at Africa now is like looking at Latin America 30 years ago. We would be very pessimistic 30 years ago, take the line that you're saying Kingsley Davis did, that you'd need all kinds of expensive modernization to increase the per capita income in order to reduce fertility. Thailand is another example of this [fertility decline before extensive modernization].

VDT: Were people so pessimistic as they are now about Africa? Their fertility seems to be so stubborn, with 8.5 in Yemen and Sudan.

WESTOFF: Well, the Muslim countries are another issue.

VDT: Ansley Coale has said if you take the marriage patterns of Africa and you impose on them the Hutterite pattern of childbearing, you get a lifetime fertility rate of 10 and no human population has ever approached that, but they're approaching it in Africa now. It seems very discouraging.

INTERRUPTION

VDT: We're talking about the Office of Population Research. The university spent a million dollars rebuilding the current building after the eating club left?

WESTOFF: Yes. We came here in 1974.

VDT: It's a beautiful place, a lovely part of the world. Princeton doesn't seem to have changed much; it must have been great to live here all these years.

WESTOFF: Yes, it has been, but it's changed enormously. The traffic is terrible; population has just zoomed.

VDT: From 12,000 to 13,000?

WESTOFF: Oh no, you're talking about the metropolitan area of the country here. You're talking about probably a million people within five miles of here. Housing has gone out of sight. You have houses in this town of \$330,000 average.

VDT: What does a young faculty person do?

WESTOFF: Fortunately, the university has a very extensive setup of houses that they rent at subsidized prices to young faculty, and for permanent faculty they have a mortgage system which is heavily subsidized as well.

VDT: You've chosen to live your life in academia. It sounds like an ideal situation. Every once in a while you would migrate to the outer world and do exciting projects like the Commission and the surveys.

WESTOFF: Well, it is. I think that to do nothing but the university would be pretty dull. You've got to mix it up. I could probably use a bit more right now. I have a leave of absence coming now in which I'd like to do something new. This is now the third year it's been postponed because of other demands on running the office here and recruiting and miscellaneous fundraising and so on. But I think I'm beginning to see a proverbial light at the next spring semester.

VDT: Where are you going? Do you have plans for that?

WESTOFF: No plans at all, but I'm going to go somewhere. I've got an invitation to go to the Center for Advanced Study in the Behavioral Sciences out in Palo Alto, so I could spend a semester there, but I don't know. I want to get some rest, a change, so I may not do that. I may go to Paris.

VDT: Did you ever live in Paris?

WESTOFF: No. I spent one leave in Mexico City, and I've spent a lot of time in Honolulu at the East-West Center [visiting senior fellow, East-West Population Institute, 1979 and 1981], although not

in recent years. And time in Sweden.

VDT: Usually attached to some institution, doing some work?

WESTOFF: Doing some work. I've done a lot of work with the Alan Guttmacher Institute. I got started a study in Europe . . .

VDT: The Alan Guttmacher teenage study.

WESTOFF: That came out of some research that I'd done with Gerard Calot, the director of INED [Institut National d'Etudes Demographiques] in Paris. He and I published a paper that showed that the United States was in a class by itself with respect to the teenage birth rate [Westoff, Calot, and Andrew Foster, "Teenage Pregnancy in Developed Nations: 1971-80," International Family Planning Perspectives, June 1983]. That raised the obvious question of why is it so high in this country and so low in European countries. And that in turn gave rise to the research that we did under the auspices of the Alan Guttmacher Institute, which resulted in publication of that book, Teenage Pregnancy in Industrialized Countries [by Elise F. Jones et al., 1986]. We took a hard look at some countries that we took as case studies, such as Sweden, Netherlands, Britain, France, and Canada. We came up with some, I think, reasonable speculations.

I'm also now returning to that subject. Dick Lincoln has persuaded me to write a paper for the 20th anniversary issue of Family Planning Perspectives. He wants me to write a sort of think piece on why the rate of unintentional pregnancy is so high among young people in America compared to Europe--not just teenagers but up to age 25; we've demonstrated that's the case. He wants me to become a sort of armchair sociologist. It's not a research paper, but just some speculations about what I can give the explanations for. [Published as "Unintended Pregnancy in America and Abroad," Family Planning Perspectives, 20th anniversary issue, November/December 1988.] I don't have a hell of a lot of ideas.

VDT: Well, it's obvious. In Holland teenagers can get contraceptives easily; nobody makes such a fuss.

WESTOFF: It's not making a fuss; that's what makes availability possible to begin with. I think it's a more basic explanation than that.

VDT: The Virginia state legislature has now legislated that sex education must be available in schools and there's tremendous grassroots opposition to that. I had that on my list: Is U.S. teenage fertility and contraceptive practice still a problem? You've more or less answered that, yes--and on to age 25.

Could you say a bit about the influence of the Office of Population Research, the first and still the leading academic center of demography in the U.S.? It's been a great place to work, I'm sure.

WESTOFF: It's been a fascinating place to work. It's in a great university and the Office has enjoyed the full and enthusiastic support of the university, which has been translated into this lovely building that they give to us at no direct charge and a great deal of financial support. And they have recently, this past year, enabled us in effect to have a number of "treaties" to connect with the associated departments, like economics and sociology and the Woodrow Wilson School, which guarantee that we can have a certain number of demographers on the faculty.

VDT: You mean you always had to negotiate with them?

WESTOFF: A lot of it happened accidentally and was through research money. Like, for example, Jane Menken is leaving to go to Penn. Nobody says that that slot has to be occupied by a demographer; there are lots of other constituencies that would like to have that position.

VDT: Wasn't she on the faculty?

WESTOFF: She was on the faculty of the Woodrow Wilson School. We are not a department. We do not make faculty appointments and we have to depend on the interests and resources of the regular departments.

VDT: Is it still true at Princeton that you never get a Ph.D. in demography--you get it in sociology or economics or whatever?

WESTOFF: Until this year, yes. Not in demography; it's called population studies. There's a sort of umbrella now, that there are various routes to a Ph.D. You still have all the traditional routes through the various departments. Now we have a new one, which is straight through a program that we designed which comes close to being a Ph.D. in demography, although we don't call it that.

VDT: Still not. It will be a Ph.D. in population studies?

WESTOFF: That's right.

VDT: Princeton degrees, of course, have been very outstanding in the field--economics with specialty in population, or sociology with population specialty. Will this be equally prestigious? Anything from Princeton is prestigious.

WESTOFF: Well, it's not just that. It's an attempt to satisfy what we think is a demand, particularly from the Third World applicants who are not interested mainly in economic or sociological theory, or somebody like Jane Menken who came out of a biostatistics background, or like German Rodriguez, the statistician. These people were not interested in taking lots of courses in these other fields; they were more interested in the internal discipline of demography. So it gives them an opportunity.

VDT: You said in an interview you had in 1977 that appeared in the Princeton Alumni Weekly ["Can Population Research Change History? Charles Westoff explains how Princeton demographers have influenced population policy," Princeton Alumni Weekly, March 7, 1977] that the Office of Population Research, having been so long in the field, was perhaps having a problem in funding because much of the funding in population is now coming from government and it goes to projects or someone who's working on family planning programs in developing countries. Is that still the case?

WESTOFF: We have been successful in lots of ways and not in one other way. Princeton as a university depends a great deal on the contributions of alumni, annual giving and special campaigns. They raised \$35 million in a special campaign five years ago. We have never, with one recent exception, been able to get the interest of individual alumni in making contributions to the Office.

There are two reasons for this, I think. One is that what we do here is pretty academic and esoteric and theoretical as far as the average person who is interested in supporting his local planned parenthood can see. The second reason, more importantly, is that this is not an undergraduate program. The average person can come to Princeton for four years and hardly be aware of the existence of this place. It's a graduate program, so if they don't make any connection to the Office while they're here as an undergraduate, they don't think of us.

Now, to answer your larger question and to respond to my own statement of 1977, we rely extensively on grants, as do other university population centers, on NIH center grants or individual research grants. I think more than other centers, we have also been successful in the fact that the university has given us quite a bit of money. They allocated one and a quarter million dollars just last year from that campaign.

VDT: For just general support?

WESTOFF: It goes into our endowment so interest accrues, but it's ours. And then we have gotten, as have some other places, foundation grants from Hewlett, Mellon, Rockefeller Foundation supported us for 30 years, Ford Foundation.

VDT: The Ford money has dried up now.

WESTOFF: Yes, but they gave us a big terminal ten-year grant. So we have not been impoverished. As somebody said to me recently, any organization that's doing its job right always needs more money. So we always need more money, but . . .

VDT: You also have several graduate students who are supported by fellowships. Any from the Population Council now? The Frank Notestein fellowship, for instance, where does that come from?

WESTOFF: That comes from the Population Council and that's for a person to come here with that title for one year.

VDT: One of your graduates won the Dorothy Thomas Award at PAA last year, wasn't it?

WESTOFF: We've had somebody winning one of those every year; that or the other one [Mindel Sheps or Irene B. Taeuber award?]. We had one--I can't remember her name, but she had been an undergraduate here and recently got her Ph.D. at Harvard and won that award. The Office has turned out a lot of very successful people in this field. Of course, it has the cumulative product by virtue of having been the first one to get going in this business and just having been around for now 52 years.

VDT: And a continually distinguished reputation.

WESTOFF: Well, it sort of always depends on who you get here. And there's Ansley Coale.

VDT: That leads into my next question. I'm going to be interviewing him tomorrow, but I wanted to ask you who have been some of the leading influences in your career. I presume Clyde Kiser; you mentioned Dorothy Thomas.

WESTOFF: Norman Ryder has had an impact, and Ansley Coale.

VDT: The three of you are a triumvirate. You and Norman Ryder and Ansley Coale have been here longest of those who are here now?

WESTOFF: Yes, I guess. A lot of these younger people have been here for a long time, like Jim Trussell has been here for over a decade, Noreen Goldman eight years probably, Anne Pebley has been here ten years.

VDT: How do you and Norman Ryder work? Does one write something out, then the other?

WESTOFF: We haven't done very much together in the past five years. During the National Fertility Study days, I think that our collaboration was quite successful because we wrote on separate topics. We'd just carve up the subject matter; you do this and I'll do that; you handle fecundability and I'll take contraceptive failure rates. So that rather than trying to work on exactly the same topic and having to argue about emphasis or interpretation, we divided the chapters up. And that worked very well.

VDT: I think you bat things off each other, because your PAA presidential address ["The Yield of the Imperfect: The 1970 National Fertility Study," 1975] was a defense of the National Fertility Study because two years earlier, in 1973, Norman Ryder had criticized the study in his address ["A Critique of the National Fertility Study"].

WESTOFF: Exactly, yes.

VDT: What's it like to work with Ansley Coale? He has had almost his entire career here too--even more so than you, I believe.

WESTOFF: Yes, Ansley's been at Princeton since he started as a freshman. Well, Ansley is a--I described Dorothy Thomas this way; there's a great similarity--is a man who exudes an enormous, almost infectious, enthusiasm for ideas and generates quantities of creative ideas each year. He's really devoted to his work and he gets an enormous intellectual pleasure, kick, out of working on various demographic models. He's in the office all day Saturday; something of a workaholic. He maintains this kind of youthful exuberance for ideas which is, as I say, very contagious. The only problem is we have to listen to his bad jokes.

VDT: What's Jane Menken been like to have around, to work with, although you haven't worked on specific projects together?

WESTOFF: I don't think she and I ever coauthored anything.

VDT: She's more quantitative.

WESTOFF: She's done a lot more work on biostatistical subjects, mathematical models of human reproduction. Although both she and Trussell, who's done similar kinds of things, also have a great receptivity to policy-relevant subjects.

VDT: Could some of that have rubbed off from you? After all, most of your topics have been policy-relevant too.

WESTOFF: No, I think they came to it through their own devices. I think with Trussell it came through--he had done a lot of work at CDC [Centers for Disease Control] as a graduate student. He continues with Bob Hatcher down there to revise that book on contraceptive technology. His academic pedigree is in economics. He's moved out of economics the same way that Ansley has moved out of it, and what remains is the set of quantitative skills that is common to those disciplines. Jane is the same way. One of the great things about working here is that the place, aside from its physical attractiveness, has always had a very high morale.

VDT: It is a beautiful place. You don't feel yourself too removed from the world's problems? Coming up here I passed through the grimness of Philadelphia, a huge metropolitan area that's trying to rehabilitate and has not yet succeeded. How can you be concerned about the world's problems in this beautiful Princeton?

WESTOFF: Yes, I know. It seems almost like the stereotype of the ivory tower.

VDT: What accomplishments in your career have given you the most satisfaction? You have many; can you choose from them?

WESTOFF: Not really. I guess the Commission was a very satisfying experience because, as I say, we turned out a report that was both well done and I think a useful--not necessarily politically useful--but a useful intellectual contribution to population policymaking. I think that was certainly one of the highlights; I don't know whether the highlight or not. And I got a great deal of pleasure out of working on the World Fertility Survey, especially in the days when we used to commute to London about every eight weeks, working on that questionnaire.

VDT: Did you go to some of the countries, like Fiji, for instance?

WESTOFF: I guess Panama was the only country I went to in connection with WFS, although we went to meetings in different places. I don't remember now exactly where, nothing exotic--Trinidad a couple of times. Recently with the Demographic and Health Surveys, I've been to both Peru and the Dominican Republic in connection with some experimental work that we were doing.

VDT: The Demographic and Health Surveys seem to be doing extremely well, although there's the tragedy of the death of Bob Lapham [DHS director] halfway through. Did you work with him quite a bit?

WESTOFF: In the beginning of this project I did. I didn't know Bob other than to say hello before we got hooked together in this project and I played a very similar role to that which I had done with Maurice Kendall and WFS, in helping to formulate the thing and get it off the ground, pointing it in directions that I think have paid off, working intensively and in great detail on that questionnaire. Now I continue as--what's my title?--senior technical adviser to DHS and go down there about once every two to three weeks, spend a couple of days with people, sort of trouble-shooting. Then I've done some writing of papers, helping them pull together a proposal for analysis of a lot of these data that goes into the proposal for the next five years of this project, which goes out next month. That's another thing I'm doing now.

VDT: So the data will continue to be exploited, like the WFS data have been, of which you wrote so much. And you also write some for the National Survey of Family Growth. Do you still do that?

WESTOFF: That's not so organized; that's just using the data set. They put that in the public domain and you get ideas.

VDT: Now let's leap into the PAA.

WESTOFF: Was all this by way of preface?

VDT: No. Besides PAA itself, the purpose of this series of interviews is to learn something of the

careers of this very select, non-random sample of people who have floated to the top of PAA. You have to be very special. How does all that come about? Is there some networking that goes on? Those of you who first knew each other at the Princeton Inn, and was that where your first meeting was? Can you remember where your first one was?

WESTOFF: Yes, it wasn't at Princeton. I think I probably hold the world's modern record of attendance at these things, because I think--I'm not quite sure--I've not missed a single PAA meeting since 1949.

VDT: Wow! And the 1949 meeting was indeed at Princeton. That was one of the last ones at Princeton. By then they were beginning to meet elsewhere--Washington . . .

WESTOFF: Chapel Hill; they met there in the early 1950s.

VDT: Chapel Hill was 1951. Of course, you can tell me, like everybody else, that the meetings then were small and intimate; everybody knew everybody else. Who stands out in your recollections of your first meetings?

WESTOFF: Frank Lorimer, Frank Notestein [pointing to old photos on the wall].

VDT: These are wonderful photos.

WESTOFF: Kingsley Davis up there; he was at Princeton too. This one--I think the PAA used to be small enough that the Notesteins had them out to a party in their backyard.

VDT: I hadn't heard about that. You all met and stayed at the Princeton Inn. Of course, you lived here.

WESTOFF: I didn't live here then [early 1950s]. I came from Philadelphia where I was a graduate student.

VDT: And you have not missed one since 1949? Fantastic! You must hold the record. You should get a prize.

WESTOFF: Maybe for lack of imagination.

VDT: How have the meetings changed? Do you regret the growth?

WESTOFF: No, no, not at all. Of course, you reach a certain age. There was a point--I suppose the optimal point would have been 20 years ago or so, where you knew nearly everybody. Now you get to the point where they keep bringing up reserves, these young people and a lot of students, and you couldn't possibly know them. You're looking at them, trying to distinguish . . .

VDT: In 1969--that's nearly 20 years ago--in Atlantic City, there were 486 people registered at the meeting. You probably knew them all.

WESTOFF: As a matter of fact, I was chairman of the local arrangements committee that year. I arranged for the hotel in Atlantic City. I remember going down there in advance to get that hotel.

VDT: By then it was a three-day meeting. Can you remember that particular one? Why were you chairman of the local arrangements? Well, it was because you were relatively nearby, in Princeton.

WESTOFF: They couldn't do it in Princeton anymore because it was too small and Atlantic City was the--Princeton was still the closest to Atlantic City.

VDT: The 1969 meeting must have been the last one in Atlantic City [second and last]. There was a famous one there in 1942, the last one till 1946, because of the war. What did you do for entertainment in Atlantic City?

WESTOFF: There were no casinos then.

VDT: So it was quite pure; you could walk intellectually on the beach.

WESTOFF: On the boardwalk. I don't remember.

VDT: At the 1975 meeting in Seattle when you were president, I think that was one of the last times we combined the presidential speech with the banquet. That year we went out to that Indian island.

WESTOFF: That was my great contribution to the Population Association; I was responsible for that. I made two changes. The first was to get rid of the banquet.

VDT: We went on a boat trip. You didn't consider that a banquet? When we got there we ate well, as I recall [salmon bake]. Did you give your address at that time? No.

WESTOFF: That was the first time the presidential address was given at 5 o'clock, followed by a cocktail party, and not at a banquet where only half the people show up because the price is too high or the food too awful and waiters are running around tuning glasses, making noise, everybody is sleepy, a lot of alcohol. That was a bad idea. So that, I think, was a major innovation.

The other change I introduced was a rule that a person could appear only once on the program. Damn good idea. But that has been so compromised now that it's no longer recognized. There are coauthorships and you can only be in one role [present one paper?], but you can be a discussant, you can be a chairman, you can be six other things, and I think it's a mistake. [1991 program also allows at least one person to present two papers, as senior author of one, sole author of the other.]

VDT: Why a mistake?

WESTOFF: Because it blocks opportunity for other people to be on the program. The counter-argument is that why should you penalize somebody who's just a junior coauthor? What difference does it make that their name is on that program or not? It's only for publication that it counts for their career. Anyway, those are the two things I did.

We also had some issues which are not worth trying to reconstruct or recollect now, having to do with the business office and where it was going to be located, whether we would have our own or continue with the American Statistical Association.

VDT: Right. The American Statistical Association took over PAA's business affairs about 1967, when they were getting too much for the secretary-treasurer. There was some talk about your time about reconsidering whether they should continue. Their price went up about that time, was that it?

WESTOFF: I don't remember.

VDT: So you don't regret that the meeting attendance has exploded to about 1,100 and you can't possibly know everybody and we have seven or eight overlapping sessions?

WESTOFF: Pretty soon we'll start splintering up into sub-sections and they'll begin to have their own national meetings. On the other hand, I don't think the Association is growing very rapidly now, is it?

VDT: No, it seems to be steady at about 2,600; we lose some, we gain some [2,679 at the end of 1989; 2,752 at the end of 1990]. That's small compared to other professional organizations. It's still a small discipline.

WESTOFF: It's always demography, I think. I hope it does stay sort of small. I suppose growth is a sign of health. It is still expanding in all kinds of directions; now we have a market version they call "demographics." That shows some signs of taking off.

VDT: Indeed, it does. But you think there's still room for the academics?

WESTOFF: I don't like to see any major changes in that respect, no. The population centers are by and large pretty healthy places, intellectually, a lot of good people, a lot of good young people coming up. I think the academic group is in a very healthy state.

VDT: What are you doing now? You mentioned that you're writing a paper on unwanted fertility.

WESTOFF: You don't have enough tape for me to tell you what I'm doing. I've got this National Academy paper I'm doing on the demographic potential of improvements in contraceptive technology in the Third World. I've got the paper I'm doing for Dick Lincoln [Family Planning Perspectives] on high levels of unintentional pregnancy among American youth and why they're different from European. I'm writing a monograph on the experimental work that Noreen Goldman and a colleague at Westinghouse--a fellow that was here today, Moreno [Lorenzo Moreno-Navarro], a Mexican who works at DHS--we have been collaborating on an experimental study of fertility in Peru and the Dominican Republic and I have to write up a report for that.

I've got an NIH center grant proposal for renewal for the next five years. That's due on October 1 and that takes several months' preparation and I dread the prospect of having to do that this summer. Then we have another NIH proposal that we had to put in for a continuation grant on the Dominican Republic part of this study. Then I'm giving a paper at an IIASA [International Institute for Applied Systems Analysis] conference in Hungary this October on the future of reproduction in Europe. I've started drafting that. And I'm just finishing a paper on "Is the KAP-gap real?", based on DHS data. And I just finished another paper, which has to be edited yet, on unmet needs in contraception in Latin America. So that's it.

VDT: And you supervise students too?

WESTOFF: Students and all the rest of the staff. Spend much of my time recruiting faculty.

VDT: How do you get it all done?

WESTOFF: You don't sleep much. You learn to do things efficiently and you learn to get help from other people. If you can't delegate some of this stuff, you're dead.

VDT: And you manage to generate all these ideas decade after decade. What about the future, are you going to keep on at this pace? You mentioned taking a semester off.

WESTOFF: Well, that's one of the things that keeps you going. Ansley Coale would certainly subscribe to this tennis; that is sort of sacred time. You don't have to lunch with people; you go out and play tennis. I played today. Sometimes it means a long lunch hour. I got in at 7:30 this morning. And you work late.

VDT: You got in at 7:30; is that normal?

WESTOFF: No, it's not normal, but because of all these things I've got to do the anxiety has apparently picked up, so I wake up early and can't get back to sleep. So I get up and try to use the time productively.

VDT: But you're not riding a bicycle to work like Ansley Coale. Do you walk?

WESTOFF: No, I drive.

VDT: Well, my last question--I promise. Have you thought of writing your autobiography someday?

WESTOFF: No.

VDT: Can you at least write a long paper? Like Frank Notestein's last paper ["Demography in the United States: A Partial Account of the Development of the Field"], which appeared in Population and Development Review in the December 1982 issue, and he died the following February. It covered the field and is a wonderful history.

WESTOFF: He had written that for the Woodrow Wilson School, actually, at the dean's request, as a sort of life history of the Office and its contributions.

VDT: It covered more than that. It pretty well covered the history of demography in the U.S. too. But there are other stories to be told, and I think you have been sitting in the middle of them and can tell them too--about some of the people.

WESTOFF: Ansley's closer to that point than I am, I think. You can get him to do that.

VDT: One last thing. Are you depressed by the fact that growth rates are not coming down faster in the developing world? The news release with the latest [1988] Population Reference Bureau World Population Data Sheet featured the stall in the world's population growth rate decline. That was picked up by the media. World population growth is trending closer to the UN high variant than the medium variant at the moment.

WESTOFF: Well, you can look at it two ways. I look at what has been an incredibly rapid decline of fertility in Latin America, although it has stalled in a few places--Costa Rica is one of the most widely cited examples of the stall. And you look at some of the Asian countries with rapid declines even before then; you're now even beginning to get some concerns about too low fertility, in places like Singapore. And then, of course, there's the China story. So where is one depressed?

One can be depressed about some of the classic cases, India, Pakistan, Bangladesh--although

things are happening in Bangladesh. And then mainly the Arab countries and sub-Saharan Africa--the Middle East and Africa. There the problems may be more intractable.

But I'm optimistic about it, just on the general principle--which, I suppose, is not a particularly sensible basis--which is that we were all pretty pessimistic 20 years ago, 30 years ago, about this stuff, and look what's happened. Africa is going to be tougher, I think. But once it gets going, I think it's going to go like wildfire.

VDT: That's a good note to end on. Thank you.

THE YIELD OF THE IMPERFECT: THE 1970 NATIONAL FERTILITY STUDY*

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Two years ago, on this same occasion, my colleague Norman Ryder delivered an address entitled "A Critique of the National Fertility Study," in which he enumerated a lengthy array of shortcomings of the fertility surveys that have occupied such an increasingly important role in demographic research (Ryder, 1973a). Ryder elaborated many of the major compromises with methodological ideals and some of the analytical mistakes that have characterized these studies, all of which add up to an impressive list of problems if not a major indictment of the species itself. Today I would like to take a different tack and try to summarize the major findings that have emerged from this research—in particular the 1970 National Fertility Study—despite all the methodological inadequacies. To exaggerate the point: What have we learned in spite of the fact that our tools and procedures seem to preclude learning very much of anything? And by "learn" I mean to include findings of both a theoretical and policy significance; research findings should not be denigrated just because they happen to be useful.

THE CONTROL OF FERTILITY

The major substantive contribution of the 1970 NFS, and to some extent that of the 1965 NFS as well (Ryder and Westoff, 1971), has been to document the extraordinary changes that have occurred in the control of fertility in this country

—changes so dramatic that we have entitled our forthcoming monograph: *The Contraceptive Revolution*. As this implies, the 1970 NFS sooner or later will be published as a book; at the rate it is being completed the book will make a contribution to historical demography. We measured the enormous spread of the oral contraceptive to where it had become the single most popular method used by American women (Westoff, 1972; Ryder, 1972). Perhaps even more dramatic is the observation that, by 1970, sterilization for contraceptive reasons was the most commonly used method among older couples (wife 30–44), surpassing even the pill (Westoff, 1972). This trend has continued through the first half of this decade as indicated by the first report of the 1973 National Survey of Family Growth. Our data revealed that ignorance about the nature of vasectomy (which is closely associated with amount of education) had declined between 1965 and 1970 and that the acceptability of sterilization had increased (Presser and Bumpass, 1972). There appeared to be a considerable potential demand for sterilization in the American population.

If we add up the increase in the use of the pill, surgical sterilization, and the IUD, we observe that, by 1970, one-half (52 percent) of American couples at risk of unintended conception (defined as exposure of couples using contraception or those not using for any reason other than subfecundity, pregnancy, or trying to become pregnant) were protected by highly effective modern contraception that is coitus-independent—an increase

* Presented as a Presidential address to the Population Association of America at its annual meeting in Seattle, Washington, April 1975.

from a third in 1965 and from less than 10 percent in 1960.

One feature of the 1970 NFS was the opportunity we had to continue our time series of use of the pill and the IUD. We have now collected information on the use of these methods on a monthly basis for the entire decade and have been able to document the rapid rate of adoption in the first part of the decade, followed by a diminishing rate of increase, then a dip in use occasioned by all of the adverse publicity, and most recently, a resumption of increase (Ryder, 1972). The detailed calendar information we collected also enabled us to estimate rates of discontinuation of these methods and to describe the trend in these rates (Westoff and Jones, 1975). The monthly drop-out rate for women on the pill declined from a high of nearly three percent early in the decade to a low of nearly one percent in 1966; thereafter the rate climbed steadily back to its initial high, reflecting all of the adverse publicity accorded the pill by medical studies. The IUD, on the other hand, has shown a steady decline in discontinuation rates.

Although virtually all American couples at one time or another now use some method of contraception, they still vary considerably in when they begin use. Our 1970 data suggest, however, that during the mid-1960's there was a significant increase in the proportion of couples using contraception before the first pregnancy (Rindfuss and Westoff, 1974). For women married between ages 20-24, the proportion adopting contraception before the first pregnancy has risen roughly from two-fifths to three-fifths between the marriage cohorts of the 1930's and the 1940's. There has also been a smaller and more recent increase in early use of contraception by women married before age 20.

The increase in early adoption of contraception is reflected in other measurements; for example, an inter-marriage

cohort (1956-1960 to 1961-1965) increase to 5-9 years marriage duration in the proportion of pregnancy intervals in which contraception was used both among women intending to terminate and women intending to delay child-bearing.

How effective are these methods? Quite frankly, the kinds of data that we collect permit only the crudest assessment of the efficacy of contraceptive methods. We have calculated a measure labelled "extended use-effectiveness" which defines as a failure for a particular contraceptive any unintended pregnancy terminating an interval during which that method was used (regardless of whether it was used when the pregnancy occurred). Thus, an unintentional pregnancy occurring to women, for example, who had been using the pill, discontinued it because of side effects, and who did not use any method for several months while deciding which method to use, would be counted as a pill failure. Although one can argue, as we have, that there is some logic in associating such a pregnancy with the method or in attributing such a wide responsibility to characteristics of the user as well as to those of the method, the measure can exaggerate any notion of the operational, let alone the intrinsic failure rate of a method, far beyond the concept of simple use-effectiveness, which theoretically excludes months of nonuse. Even aside from this crudity of measurement, we have a great deal to learn about the irregularity of contraceptive practice.

Our measure of extended use effectiveness, which was invented because of our lack of confidence in the reliability of reports on periods of nonuse in the past, shows for all contraception a one-third probability of conceiving an unwanted pregnancy in five years (Ryder, 1973b). In a year's time, the probability of conceiving an unwanted pregnancy was 14 percent; the chance of failing to prevent a birth that was wanted but wanted later than it actually occurred was 26

percent. This finding confirms the generalization advanced in the older Princeton Fertility Study (Westoff, Potter, and Sagi, 1963) that contraception is more effective when its practice is motivated by a desire to avoid additional births than when it is employed for spacing purposes. This generalization appears valid regardless of contraceptive method: the less effective methods when used by couples who have had all the children they want are used with much the same effectiveness as are the more effective methods by couples who intend to have more births but who are using contraception for spacing purposes. This relationship is enhanced by differences in coital frequency as well. Within the same age groups, women practicing contraception to postpone childbearing report a lower coital frequency than women trying to prevent further fertility (Westoff, 1974).

Our measure of contraceptive failure differs widely for different methods of contraception even with age and fertility intention controlled. The overall average failure rate in the first year of exposure varies from six percent for the pill to 39 percent for the douche (Ryder, 1973b). The finding that there is a pronounced direct association between the effectiveness of the method and coital frequency suggests that a theoretically purer but obviously infeasible measure of protection from actual risk—a measure based on coital frequency—would show even greater variation by method.

These failure rates reflect experience during the 1950's and 1960's; with the large-scale increase in the use of the more effective methods, a current picture would undoubtedly show much lower failure rates. A marriage cohort analysis of the 1970 data comparing women married in the 1950's with those married in the 1960's reveals a very definite increase in contraceptive effectiveness, and there is every reason to assume that this trend has continued. More than half of

the observed improvement was due to the shift to the pill.

Group differences in the control of fertility have shown a distinct tendency toward convergence. The most effective methods are being used in about the same proportion by women of different educational achievement, by Catholics and non-Catholics, and by whites and blacks (Westoff, 1972). There is, however, a pronounced racial difference in the incidence of male and female sterilization procedures; vasectomies were virtually nonexistent among black males as of 1970.

We undertook a special analysis of the use of the rhythm method among Catholics (Westoff and Bumpass, 1973) and concluded from a comparison of the two GAF Studies in 1955 and 1960 and the 1965 and 1970 NFS that there has been a wide and increasing defection of Roman Catholic women from the traditional teaching of their Church on the subject of birth control. On the basis of this trend, we predicted that by the end of this decade, Catholics and non-Catholics will be virtually indistinguishable in their contraceptive practices.

One highly effective method of birth control about which we still know very little is induced abortion. Most of our knowledge about the incidence of abortion derives from studies of the New York experience since 1970. Fertility surveys have been notoriously incapable of eliciting candid reports of abortion by married women. Perhaps the climate has changed since the Supreme Court decision, but some European experience suggests that, even where abortion has been legal for many years, women still grossly underreport abortions. The 1973 U. S. survey undertaken by the National Center for Health Statistics employed the random choice technique which, if successful, will at least provide some estimate of the national rate.

Our attempt in 1970 to elicit such information on a direct questioning basis

proved no exception to the generalization that it simply doesn't work. Only about 100 of the nearly 7,000 women interviewed admitted *ever* having had an abortion. Such underreporting of abortion is possibly the main reason why our estimate of fecundability calculated from non-contraceptive intervals seems much too low in comparison with values reported elsewhere in the literature.

We repeated, in 1970, the same questions on attitudes toward abortion that had been included in the 1965 NFS. An analysis of the trend in attitude over the five-year period disclosed a substantial shift toward a more permissive position (Jones and Westoff, 1972).

UNWANTED FERTILITY

Probably the most widely known part of the NFS has been the estimates of the incidence of unwanted fertility in the U. S. This subject has been of particular importance in population policy debates and thus has been subject to more criticism and methodological discussion than less controversial topics. The first estimates from the 1965 NFS were that, if American women had avoided all unwanted births in the 1961-1965 period, well over four million births would have been averted; moreover, the cohort of 1921-1930 would have experienced a total of 2.5 births rather than 3.0 births (Bumpass and Westoff, 1970). The 1970 NFS produced estimates of a potential 2.65 million births averted in the 1966-1970 period (Commission on Population Growth and the American Future, 1972) and a calculation that the elimination of unwanted fertility would have taken the cohort of 1926-1935 almost two-thirds of the way to replacement fertility.

The population policy implications of this research can hardly be exaggerated. Only a few years ago there were voices calling for various radical measures to reduce U. S. fertility. The evidence that important and perhaps sufficient demo-

graphic change could be effected by facilitating the prevention of unwanted births—a nonradical, comparatively inexpensive and, for the most part, a politically palatable “solution”—played a genuinely important role in the deliberations and ideological tone of the final report of the Commission on Population Growth and the American Future in 1972. It seems curious if not ironic that, if unwanted fertility were nonexistent today, a scant three years later, U. S. period fertility would be considerably below replacement.

The measurement of unwanted fertility is a complex business at both the interview and the analytical stages, and I doubt that we have yet said the final word on the subject. The meaning of the concept as communicated to the respondent, the problems of recall and *post factum* rationalization, the reliance on the wife's report only, the simplistic notion that a baby was either wanted or unwanted when our everyday impressions tell us that there is a large gray area of indifference or nonrational behavior—all of these and more remain unresolved difficulties.

The analytical difficulties were encountered in trying to determine the trend in unwanted fertility during the decade of the 1960's (Ryder and Westoff, 1972). The average number of unwanted births per woman showed less decline across the cohorts than we expected. The decline in the number wanted, however, implied an increase in the amount of time women were exposed to the risk of having an unwanted birth. Thus, when unwanted fertility was measured by relating the number of unwanted births to woman-years of exposure to risk, a rather significant decline (more than a third) emerged.

The greatest declines in unwanted fertility rates were observed among blacks and among white Catholics. The race differential still existed as of 1966-1970 with a rate twice as high among blacks,

but signs of rapid convergence were evident. A similar and related convergence among educational categories is in process. The Catholic-non-Catholic difference had virtually disappeared.

FERTILITY

The ultimate demographic objective of our interest in the subject of fertility control, on which we have concentrated so heavily in the 1970 NFS, is the better understanding of differences and trends in fertility itself. Our analysis of group differences in fertility has been organized principally around white-black and Catholic-non-Catholic differences.

Two socioeconomic characteristics have been singled out for special attention: education and income. Education has been shown consistently to affect fertility control and fertility presumably through its exposure of individuals to different values, interests, and skills which play an important role in determining their routes through life. Over and above its connections with education, income has received special attention because of the provocative theoretical work produced by the "new home economics" school in which income is viewed as constraining choice on the exercise of preference for children in competition with other consumer preferences.

The trend in fertility has been measured by two comparisons: the more or less completed fertility of women 35-44 in 1970 (the birth cohort of 1926-1935, whose prime reproductive years spanned the baby boom and who produced the highest fertility in recent U. S. history) compared with an index of marital fertility analogous to the period total fertility rate for the years 1966-1970, and then a comparison of this period rate with the same kind of rate calculated for the years 1961-1965, derived from the 1965 NFS. The cohort fertility and the more recent period fertility have been partitioned into "wanted" and "unwanted" components (the 1961-1965 rate

was not so partitioned because of the problems of comparability between the 1965 NFS and the 1970 NFS in the measurement of unwanted fertility).

The following generalizations have emerged from our analysis of the race and religious differentials:

1. Nearly all of the excess of black over white fertility is due to the considerably higher unwanted fertility among blacks. Given the substantial reduction in the unwanted fertility rate among blacks across the generation, a rapidly increasing convergence of black and white marital fertility can be expected during the current decade.

2. Such a convergence was clearly evident during the 1960's in the trend of Catholic and non-Catholic fertility because of the greater decline among Catholic women. The differential remaining in the period fertility of 1966-1970 (15 percent) is, unlike the white-black difference, due to differences in the number of children *wanted*. The source of this difference between Catholics and non-Catholics is suggested by the further finding that the number wanted among Catholics, but not the number of unwanted births, varies directly and strongly with simple measures of religious commitment.

3. Probably the best single predictor of fertility uncovered in our study is age at marriage. The demographic importance of age at marriage for fertility has been widely advertised for developing societies in which little contraception is practiced, but its force has not been sufficiently appreciated in contracepting populations. The 1970 NFS data show a strong negative association of both wanted and unwanted fertility with age at marriage, holding constant the number of years married (thus eliminating the effects of the simple duration of exposure to the risk of childbearing). The reason that age at marriage exerts such an influence on fertility is that it combines mutually reinforcing biological

and sociological selective factors such as fecundability and education. Age at marriage is a variable that has not received adequate attention in fertility analysis, especially considering how infrequently one uncovers variables with such predictive power.

4. A strong negative association exists between education and both wanted and unwanted fertility. For both cohort and period rates, the two components make roughly equal contributions to the overall variance of fertility across educational categories. Among blacks, however, the association between unwanted fertility and education is the main determinant of the overall fertility-education relationship. The cohort and period rate comparison among Catholics reveals the transition from an older pattern of a non-linear association of education and fertility to the emergence of a sharp linear negative association due almost entirely to the variation by education in the number of children wanted. This transition is due to the sharp declines in fertility of the more educated Catholic women whom our earlier research (Ryder and Westoff, 1971) had described as highly committed to Catholic doctrine; it was the young, college-educated Catholic women who were especially attracted to the pill.

Across the decade of the 1960's, there was some narrowing of the educational differential in fertility. With the greater reduction of unwanted fertility among the less educated and the increasing educational achievement of each new cohort, there is every reason to expect that educational differences will continue to diminish and probably disappear in a decade or two.

5. The 1970 NFS provides no support at all for the hypothesis that income bears a positive relation with fertility. One can object to the use of income as a measure incorporating all of the subtleties of level of aspirations and style of life, and one can raise very legitimate

questions about our measure of current income in terms of its significance for fertility decisions long past. Nevertheless, we have isolated wanted fertility as the relevant component of the consumer choice model; we have examined the association within homogeneous educational categories, which in theory eliminates confounding rising "tastes" with rising income; we have also controlled for whether the woman is in the labor force; and finally, in a separate analysis, we have defined income in terms relative to the income of people of the same social stratum. None of these refinements succeeds in reversing the slight negative correlation that income has with the completed fertility of the 1926-1935 cohort.

6. The analysis of the effects of women's employment on wanted fertility and on the timing of the first birth suffers from all of the typical complications of unravelling cause and effect. The findings are usually consistent with expectation but vary considerably across racial and religious subgroups. The hypothesis of competition between child-bearing and non-familial roles is supported by the finding that sex-role traditionalism is positively correlated with wanted fertility (among whites only, however).

7. The perception of the importance of population growth as a problem may play a substantial role in the number of children desired (Rindfuss, 1972). Among young women (under 30) who already have one or two children, the average number of additional births intended is half as great among those concerned about population growth as it is among the unconcerned. This is at least an indication that the mass media have some effect, either in actually influencing attitudes toward fertility or, perhaps more likely, in providing a highly respectable rationale for a smaller family size preferred for other reasons.

8. An inquiry into preferences for male or female offspring (Westoff and Rind-

fuss, 1974) concluded that, except for an initial temporary excess of male births because of a strong preference for a boy as a first child, the development and use of sex preselection technology would have no effect on the sex ratio at birth in the U. S. at current levels of fertility. A more permanent effect would appear to be a large increase in the first born being a male and the second born being a female—a combination that would be twice its natural incidence (the two-boy and especially the two-girl sequence would be reduced, respectively, to half and one-third their expected proportion while the girl-boy sequence would increase just slightly). If this occurred, it would increase the prevalence in the male population of characteristics concentrated among the firstborn. This is all pretty speculative, of course; even if an acceptable technology were developed, it would have to overcome an initial resistance in the population to use it, since in 1970, at least, more women were opposed to than in favor of using such technology.

The effects of sex composition and preference on fertility appear to be slight. For example, about 25 percent of women with two children of the same sex intend to have additional children compared with 19 percent of women with a son and a daughter.

9. Another analysis (Bumpass and Sweet, 1972), taking advantage of the *ever-married* definition of our sample, examined factors affecting marital instability (prevalence of separation and divorce). The main conclusion was that age at marriage is the best predictor of marital instability. The strong negative association between education and marital instability is due primarily to the joint association with education and age at marriage.

10. A recent examination of the trend in unplanned fertility, that is, all pregnancies that occurred despite the couple's intentions to postpone or to termi-

nate, yields a rather striking picture: *all* of the decline in marital fertility evidently resulted from the reduction in unplanned fertility; in other words, the entire decline in births within marriage across the decade of the "sixties" can be attributed to the improvement in the control of fertility. Part of this improvement is due to the increased proportion of exposure time in which contraception has been used and part to the increased efficacy of contraception, reflecting both more successful (probably more regular) use and the use of more effective methods. Research is currently underway to reconstruct from the two surveys an annual series for the decade of rates of planned and unplanned births for various subgroups of the population.

CONCLUSIONS

These, then, are the principal substantive findings of the 1970 NFS to date. Other investigators as well as ourselves will continue to probe these data with additional questions. One of the major contributions of these fertility surveys is that they provide us with a wealth of reasonably comparable data for the measurement of trends in variables underlying national fertility trends such as contraceptive and fertility planning behavior. For example, I have initiated an examination of cohort data on fertility-planning success from the 1955 and 1960 studies which, when combined with 1965 and 1970 data and new data to be collected in 1975, will permit a systematic reconstruction of the role played by unplanned fertility for a quarter of a century, from the baby boom through the sharp decline of recent years.

One indication of the usefulness of this research is the fact that the government has now institutionalized this work in a biennial series of fertility surveys which began in 1973. One consequence of this is that university-based research can now focus more sharply on causal analysis and leave the parameter estimation busi-

ness to the government. In line with this thinking, we are planning the first longitudinal phase: a follow-up of a large subsample of the women interviewed in 1970. Supplementing the 3,000 women who will be re-interviewed will be a sample of 1,000 women married after 1970. It is our hope that this will be just the first in a new time series of longitudinal studies that will permit answering many of the questions not answerable with successive cross-sectional surveys.

In the meantime, however, I think it is fair to conclude that we have learned a great deal in spite of the various inadequacies of these cross-sectional studies of fertility. Or to paraphrase another colleague of mine, if the subject of fertility is worth studying, it's worth studying imperfectly.

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