# DEMOGRAPHIC DESTINIES

## Interviews with Presidents of the Population Association of America

## Interview with Judith Blake
## PAA President in 1981

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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# JUDITH BLAKE

PAA President in 1981 (No. 44). Interview with Jean van der Tak at the School of Public Health, University of California at Los Angeles, May 4, 1989.

**CAREER HIGHLIGHTS**: Judith Blake was born IN 1926 in New York City, where she (mostly) grew up. She attended Columbia University, where she obtained the B.A. in 1950 and the Ph.D. in 1961, both in sociology. From 1953 to 1957, she was associated with the Conservation Foundation of New York City while conducting a study of family structure and fertility in Jamaica. She and Kingsley Davis were married in 1954 and moved to Berkeley in 1955. (They were divorced in 1977.) From 1957 to 1959, Judith Blake lectured in the School of Nursing of the University of California at San Francisco. She was then at Berkeley from 1962 to 1976. At Berkeley, she was, variously, Professor of Demography, founder and Chair of the Group in Demography (1965-67) and of the succeeding Department of Demography (1967-72), the first department of demography in the U.S., and Professor in the Graduate School of Public Policy. Since 1976, she has been at the University of California at Los Angeles, where she is the Fred H. Bixby Professor of Population Policy in the School of Public Health and the Department of Sociology.

Judith Blake's many publications include the monographs, Western European Censuses (1960), Family Structure in Jamaica (1961), and Family Size and Achievement (1989), and a long list of provocative articles and book chapters on such topics as American attitudes toward abortion, childlessness, the status of women, and federal family planning programs targeted to poor women and teenagers, and on the only child and the impact of family size on the quality of children. [Dr. Blake died in Los Angeles in 1993.]

**VDT**: What led to your interest in demography?

**BLAKE**: I was thinking about that when I saw your letter [with questions to be asked in the interview]. I guess I was really looking for a sub-field of sociology that was empirical and very broad in its disciplinary scope, international, and that would have an influence on world affairs. I wasn't interested in voting and political participation, for example, which was the focus of most survey research at Columbia. This was Paul Lazarsfeld's focus, really his main interest. Although I was interested in the survey side, I wasn't interested in the voting side. And I wasn't a Marxist. There was a very strong, very distinguished, Marxist tradition at Columbia, with Robert Lynd and Herbert Marcuse and all of that. So that was a whole contingent that was kind of ruled out for me.

So, for a while, I was really at sea. Then, strangely enough, I was walking along Amersterdam Avenue one day and met Alice Taylor, who became Alice Taylor Day, ultimately, but was then just plain Alice Day, and told her, "Alice, I'm just not quite focusing in this place and I don't know what to do." Alice was always a dear, sweet person and she said, "Oh, that's no problem; you ought to take a course in demography. I know you're going to be crazy about it; it's just for you."

**VDT**: You were in graduate school at Columbia?

**BLAKE**: Yes, I was.

**VDT**: You'd gone straight on to graduate school after finishing your undergraduate degree there [in 1950]?

**BLAKE**: Right. I was a New Yorker and I really didn't know there was anyplace else; it never

occurred to me to go elsewhere. Also, I didn't have much money. And it wasn't so dumb, because I knew the territory there, and for me to have uprooted myself to go someplace else without any financial backing or anything would have been a little foolhardy. So I stayed.

One of the things that had happened was I was sort of burned out, because I had taken all these graduate courses for undergraduate credit and by the time I was a graduate student, there really wasn't much else to take. And I had kind of worked my way through a series of enthusiasms, which I then worked my way out of, and I was just beginning to feel, "What am I doing here?"

Alice said, "The course is just starting up." So I registered. The course was being taught by Hope Eldridge, who was a wonderful person, but I wouldn't say she was a drop-dead lecturer. In spite of that, I just had a feeling that I'd found it, that this was a very exciting field.

**VDT**: Was this the time that Eldridge was at the United Nations, before she was hounded out of the UN during the McCarthy era?

**BLAKE**: Yes.

**VDT**: I didn't know she also taught at Columbia.

**BLAKE**: It was Kingsley Davis's course and he went to Africa on very short notice, for Carnegie I guess, so she agreed to teach the course. She did a very good job, but, as I say, you would, I guess, have had to have a predisposition to go into demography to have been so utterly overwhelmed by this as a field.

I've asked myself this over time: What was it that just knocked me for a loop? I think partly it was there were so many data. Now this may not sound important to you, but at the time, generating information was not easy. Sociology had just survived the throes--and was really <u>in</u> the throes--of community studies, studies that were mainly asking how many social classes could you find in a community, and some people found six and some found eight and some found twelve. But fundamentally, generating data, information about whether things were true or not true and whatever, wasn't easy. It was really a tough proposition to process those data as well.

So here was a field where you were awash in data; the stuff was just pouring out. That was wonderful. In the course, you were set to work on lab problems where you were actually looking at census data and manipulating them and there they were for any country you could think of. It was just terrific! Also, you could do comparative analyses and I liked that. I liked the idea of looking at a lot of different countries and comparing.

And surveys also seemed to me to be very applicable. As I started thinking about demographic issues, I got beyond the data side very rapidly and started thinking about why things were happening. I started to realize that the training I'd had in surveys should be very applicable, because what people were asking were analytical questions and with censuses it was hard to zero in on those. By and large, censuses were legal documents and you were very limited as to what you could ask, and vital registration data were the same. So I moved rapidly in my own mind from thinking, "This is a field where you're awash in data," to thinking, "This is a field where you're awash in data, but none of them are quite right for what you're asking, what you're interested in." They were suggestive and you would do these analyses, and then you would be left with kind of an empty pot, because the real questions you wanted to ask you didn't have any control over. So at the time, I thought that doing surveys was a way of amalgamating training I had had with what I conceived to be the new demography. And I got really enthusiastic about that.

So I stayed in the course for the semester and was making very rapid strides and thinking about what I would be interested in doing. Then Kingsley came back from Africa and gave the second half of the course. He was full of all of the international comparative stuff and what had been going on in

Africa. That just blew me away, I mean the fact that you would be going to the Dark Continent and seeing all these things; it was a period of enormous change and colonial problems and so forth.

I got a job at the Bureau for Applied Social Research, which was run by Lazarsfeld and Kingsley and Robert Merton and that crowd. I got a job on the cities' project, which was being done for the Air Force. This project was looking at census data on cities, aggregate data. It didn't take me long to realize that I didn't like dealing solely with very aggregated data. Then I thought, "Oh dear, what am I doing in demography, because demography is mainly aggregated data?" And I was beginning to worry again that perhaps I was in the wrong spot.

Then this Jamaica project came along. The Conservation Foundation funded it and they were interested in the birth rate and family structure and so forth in Jamaica. I just flipped out; I thought this was a terrific idea. Actually, it ended up being the third survey of the topic in a developing country. The first had been Paul Hatt's in Puerto Rico and the second was the one Joe Stycos was doing in Puerto Rico; Joe Stycos had worked with Hatt. Joe was going to be the project leader and I was going to be his sidekick and we were going to go down and do this project in Jamaica. You have to realize we weren't very old; maybe Joe was 27 and I was 26, something like that.

**VDT**: And you were going to run the whole thing?

**BLAKE**: We were going to run the whole thing. After some negotiations with the Conservation Foundation and being met by all the people who wanted to know who this "Miss Doolittle" was, I got approval. Joe was still finishing up the second Puerto Rican study, the one he did with Kurt Back and Reuben Hill. This was after Paul Hatt's study; Paul had died by that time. I started working that summer on all the problems of how we were going to work on the multiple unions in Jamaica, and we all met in Puerto Rico in early fall, 1953, and started working on the interview schedule--Joe Stycos and Mary, who then was his wife, and I.

We went in a few weeks to Jamaica and found, I would say, a not overly hospitable environment, which was mainly dominated by anthropologists, who felt that doing a survey in Jamaica was about as loony as you could get. They thought this had to be unreal. We didn't pay too much attention to this. Joe is an incredible field person--just an inborn talent for this. And we had the 1953 census we could use as a sampling frame. It had been done a few months before and we could use that. So we went out and got ourselves a sample and did all the right things. We had our schedule ready and we went to the Census Bureau and other agencies to get trained interviewers, whom Joe trained some more, with great skill, and went into the field and did the survey.

At that point, Joe decamped to write up the Puerto Rican results and I decamped to write up the Jamaican results and they became my dissertation and, ultimately, the book [Family Structure in Jamaica: The Social Context of Reproduction, 1961].

Those surveys--the Hatt, the Stycos and Hill and Back, and the Jamaican studies--I think really laid the groundwork for people no longer thinking it was loony to do surveys in these countries. And I think this work made people recognize that you could do what later became KAP surveys, which were much less interested in the causes of fertility behavior, which was our focus, and much more sort of descriptive knowledge-attitudes-and practice (of birth control) types of instruments.

**VDT**: Your study in Jamaica and Joe Stycos's before in Puerto Rico were looking more at the socioeconomic background?

**BLAKE**: Yes, and the family structure background and incentives and disincentives--what we considered the big questions. The surveys were much less descriptive than what later became KAP surveys. Also I think people came to realize that you could do surveys in developing countries. You could train interviewers; you could get results; and this was a viable form of research.

So I enjoyed that an enormous amount; I enjoyed the whole experience. But my life had changed by this time quite a bit. Kingsley and I had gotten married [1954] and we moved to California [1955] and I was in the process of having a baby and finishing the dissertation and one thing and another. So I was a little less free to be running off and doing surveys.

It changed my life a lot to move to Berkeley, because I got pulled out of my own environment, which had been a very urban one. I'd grown up in Manhattan and been at Columbia and went into a situation at Berkeley that was very alien for me. It was alien not only because it was sort of a suburb [of San Francisco], but the university itself was . . . Well, it was very chauvinist; there were practically no women on the faculty and one had the feeling that they didn't want any either.

**VDT**: That was in the mid-1950s, when Kingsley was Professor of Sociology at Berkeley and setting up International Population and Urban Research?

**BLAKE**: Yes.

**VDT**: You lived at Berkeley, but you for a while were teaching at the university in San Francisco?

**BLAKE**: Yes, they [University of California] had a campus in the city, and I felt it was to my advantage to take lectureships wherever I could so I could get teaching experience and say I had some teaching. I didn't want this long hiatus to develop in my career while I was doing nothing but writing my dissertation. I was very worried about that. I had to have faith that there was going to be a future, which took a lot of faith in the 1950s, because it was not the time when women were doing this sort of thing. I kept saying to myself, "Just take it a step at a time and if somebody offers you a lectureship or this or that, take it. Then take the next step." It wasn't one of these things where you felt, "There's a career out here for me somewhere." You just felt you were going to have to see how it worked out and hope that things would improve.

That's not such a bad philosophy, actually, because life is incremental and I took those jobs and got some experience. I'm sure I was dreadful to begin with and those poor nurses [at the School of Nursing, University of California at San Francisco] probably suffered through quite a few years of boring teaching. But I would have had to do that somewhere. They seemed very appreciative, though. So anytime I got a chance to do something like this I took it, and I gradually got to be, I think, probably a much better lecturer.

**VDT**: You said Berkeley was very chauvinistic; they were not ready to welcome you aboard the faculty there?

**BLAKE**: They were not ready to welcome <u>any</u> women much. The chairman of the political science department said that a woman would never get a job on his faculty as long as he was chair. People didn't mind saying things like that. And you felt that you were totally out of the scholarly community, because I was no longer a student and I certainly wasn't a student there, and I wasn't anything else; I was just sort of a wife. So I really never felt that I had any collegial relationships with people much at all.

Well, Jacob Yerushalmy in the School of Public Health was very interested in demography. He felt that something should be done at Berkeley in demography. So he talked to Kingsley about it and then they talked to me about it. I thought that was fantastic. Again, I figured you take it one step at a time. Yerushalmy gave me a job and it was to try to start a group, which was the vehicle that Berkeley had at the time for establishing academic programs. The idea was that you would bring people together from all kinds of departments who had genuine jobs in those departments but who were interested in a sub-field like this one and they would form a group and put a curriculum together and

get a degree program started. So that eventually was what I started working on.

**VDT**: Which was to become the Group in Demography?

**BLAKE**: Yes. We got that established [in 1965] and then it was clear that we didn't have any say over people's time or anything. I mean, we were accepting [graduate] students, but you get students and then all of a sudden you realize that when people want to go on sabbatical or something like that, they just go and you're not able to say, "Well, this is not a convenient time for us this year. We're accepting x number of students, something has to be done with them, there have to be courses." Nobody's asking.

**VDT**: Their primary loyalty is to their departments?

**BLAKE**: Yes, they're not going to put themselves out of joint necessarily for a group. So it wasn't long before it was clear that this was going to be an economically non-viable situation. And it was pretty scary, because we had students on deck but you couldn't see faculty on deck, at least on a continuous basis. So we went to Roger Heyns and he said, "Well, there's really nothing else you can do but form a department." He was the chancellor of the University of California; he's now head of the Hewlett Foundation.

So the violence escalated, you might say, simply as a consequence of administrative rigidity. The real thought was to get some funding into the Group, but that wasn't being done at that time. So we asked how you could get the funding to get some bodies here that you could rely on and you could have the courses taught and be sure you had somebody there besides me. So we started working on the department [of demography]. That took a couple of years to get that through.

**VDT**: The Group in Demography was 1965 to 1967?

**BLAKE**: Yes, approximately, and there were a lot of people interested in it.

**VDT**: I interviewed Ron Lee at Berkeley last Friday. He, of course, was one of your first graduates--M.A. [1967].

**BLAKE**: Yes. A lot of people were <u>very</u> interested in this idea of a program that was interdisciplinary but that you could do demography. So the department [of demography] got established [1967]. It was established with three faculty positions and never had more. I was sort of beating the bushes, raising money, all the time. Well, money wasn't so hard to get in those days and we could get money to get visitors and get all kinds of people; we could patch this thing together for some years. But it became increasingly hairy. I could keep it patched together on the windfall money, but it was wild, because we kept getting all these students, large numbers of students, and with the doctoral program we had to keep trying to gin up these courses for them to take, people to supervise their dissertations, and one thing and another.

Well, it was going pretty well and we started an undergraduate set of courses.

**VDT**: Oh, you did actually get them started? I knew they were on the schedule.

**BLAKE**: Oh, yes, they started. They were <u>very</u> popular. This was the time, 1965 to 70, of enormous concern for population and younger kids were all excited about this, so you'd have as many as 200 students in an undergraduate course. It was incredible. We even had an undergraduate major that we graduated <u>some</u> people in. So it looked as if it was going to be a success--in part, I think, because of

the great concern for population in the United States.

But, in time, there was an awful lot of political activity on the Berkeley campus, and by the 1970 cohort, I guess, we had many students who were not only interested in population but were very political. They were very concerned about Vietnam--rightly so, of course. We became--it was a very little, obviously not very strong department--we became a big focal point for this activity.

**VDT**: You think more so than other departments?

**BLAKE**: Well, I think that other departments were too, some of them, but they could sustain themselves a lot better than we could, because we were pretty small. And the students felt that we were very vulnerable. A lot of student activity was sort of an effort to take over small departments as communication hubs. They wanted to take over the xerox machines, the telephones, a lot of the typewriters, everything. There was an effort to do this sort of thing.

And part of my problem was that I really felt that all one should be concerned about was demography in the university--I mean, that we shouldn't be political. This itself, I think, probably was a very political thing, in the sense of saying, "Well, I don't think a department should go political." But people who thought it should be political felt that I was being very difficult about this.

**VDT**: I've heard that side of the story a bit before, from Sam Preston, for instance, who was there [on the faculty] then.

**BLAKE**: Right.

**VDT**: I think he felt he was torn, that students were approaching faculty members, asking, "Are you on our side or not.?"

**BLAKE**: Right.

**VDT**: He gave the impression that it was a difficult time, indeed.

**BLAKE**: It was a very difficult time. And I think especially, probably, for a younger person like that, because he was younger than many of them. So much of this was generational; I mean, if you were a younger person you had to be on their side, in a sense. Some people were really very rabid about this.

It sort of erupted and, I think, really was the thing that created the biggest problems for the department. Roger Heyns, who had been very supportive of us, left and Bowker came in as the new chancellor and he was importuned very strongly by the students. A whole coterie of students was not this way, but that didn't count so much. So Bowker's feeling was, "Well, here's a set of faculty positions that I can get hold of and I have no commitment to demography; this wasn't my idea to create a department of demography. This was an achievement that Roger Heyns saw as his, but it's nothing in my life." He just felt that there were all these problems and why continue a department where there were all those problems.

Meanwhile, Nathan Keyfitz and Sam and Etienne van de Walle, who was a visiting professor, really were not very supportive, I have to say. So Keyfitz went to Harvard and Sam left and went to the University of Washington and, of course, Etienne didn't have a job there; he just went back to his Princeton job, I guess. But the fact that people left, left things up to me, and really made it possible . . .

**VDT**: Left?

**BLAKE**: Left the faculty positions open and really made it possible to say, "Well, there's nobody

here." So Bowker closed up the department. And Kingsley and I were left with a whole big bunch of students to see through dissertations and one thing and another through this sort of a nightmare of having all these people and nobody there to do the work. So I went through a period--and Kingsley went through a period--of getting these people through their dissertations and out into the world. Which was a very strenuous period, and was, I felt, a very difficult way of going about things--let's put it that way.

Well, in any event, I was bid for by the Graduate School of Public Policy and went over there, which turned into sort of a five-year period which was a very happy one. There were lots of people I had known before and it was a very friendly environment and it was my first really friendly, supportive environment in academic life. I'd been going from one set of worries and crises to another. It really was a nice situation. I taught courses in population over there and helped them with their program in policy analysis and just had a period that was productive--a very pleasant period in my life.

Then around 1976, the School of Public Health here at UCLA got an endowment fund in population which was for a chair and they approached me. And it really didn't occur to me--I think I gave them six people's names, or something, and said these people would be good and that was it. I never thought about it.

**VDT**: It was a chair for population studies?

**BLAKE**: Yes, and I didn't even ask what that meant; it just never occurred to me to come down here. Sometime in that period, Leo Reeder, who was a professor in the School of Public Health, called me. He subsequently was killed in that plane accident between San Diego and LA some years ago that killed everybody on board. He called me and said, "Why are you not interested in this job." And I said, "Gee, I don't know." I never felt I had to account for why I wasn't interested in the job; it never occurred to me to leave Berkeley. I said, "I'm very happy where I am now." I just hadn't thought of moving. So he said, "It's a very advantaged situation. The faculty position is paid for out of state funds and the chair is just available to you as a yearly income to do research if you wish. This is a nice situation here. You should at least look into it. Why are you so rigid about this?"

So I began to say to myself, "Why am I so rigid about this? It has a lot of advantages, being in the same system, because you're in the same retirement system, and it isn't that big a move." So I came down here and a great many of my northern California prejudices were dissipated quite rapidly. I liked it down here.

**VDT**: You mean the way of life or the university itself?

**BLAKE**: Well, the university I was very impressed by as being in some ways in social and biological sciences a much more dynamic kind of place, a more interesting place to me, than Berkeley. And I felt less political, which was very appealing to me.

And I found the Los Angeles area very attractive. Partly, I was a New Yorker and the proximity to this very interesting city, which was culturally sort of just bursting out all over, and the proximity to the open spaces, like the beach--40 miles of beach. I had had this mental image of its being wall-to-wall people and that's never true; it can be 100 degrees here and the beaches are never wall-to-wall people. I just had a sense of its being an interesting place, being both urban and very cosmopolitan and culturally very interesting and a place where you could get to wide open spaces very rapidly, which is, strangely enough, true.

So I began to see it in a different light and we began negotiations. Part of what I think appealed to me is that they <u>were</u> interested in population, whereas where I was, although they were interested in me and were interested a <u>little</u> bit in population, it was really sort of a concession to having me in the School of Public Policy; population had never been one of their big things that they were interested in.

And I felt here that they were.

So one thing led to another and, by that time, Kingsley and I were getting a divorce and it just seemed like a good idea to start a new existence someplace where you hadn't been all those years. So we negotiated a job here and I got a very substantial promotion and the chair and one thing and another.

I guess I realized that Berkeley since the closing of the department, however wonderful it was to be in Public Policy, was still a place that had had a lot of pain for me and that it was kind of nice to get away from that business. I'd been through an awful lot there, and some of it I had felt was very unfair and that I hadn't been treated very well. So I really wasn't sorry to get away from there. I realized after I left that it was a place that held painful memories for me and that when I got away I was feeling a lot better. [Laughter] I guess I hadn't realized that until I left, but it was almost as if something had stopped drilling and I suddenly realized, "There are other things and other places."

So it has been a very happy situation for me down here. We had our housing problems to begin with and, of course, I remarried.

**VDT**: Really! I didn't know that.

**BLAKE**: You didn't know that? Ages ago--yes.

**VDT**: To whom?

**BLAKE**: I'm married to a man called Leroy Gramer, who is the Director of Humanities, Sciences, and Social Sciences over in University Extension. We've always been on the same campus, fortunately.

**VDT**: You met here?

**BLAKE**: No, we had met at Berkeley. Then as I was negotiating this job and peregrinating up and down on Pacific Southwest Airlines, we remet, and by this time, Leroy was thinking of a new job. The man with whom Leroy had worked at Berkeley had left and gone to the Russell Sage Foundation and Leroy felt it was time for a change for him, so he was looking for jobs up and down the coast. We remet in that situation. That influenced the job he eventually took, of course, but that's another story.

We hit a pretty hairy housing situation, but we ended up doing very well in this, so that wasn't a catastrophic thing. People had said you'll never manage here and we had felt that to begin with, but then things worked out very well. So I've always been happy here. I've always enjoyed being here and it's been a happy situation for me. I certainly never regretted leaving. This environment has been a congenial one. Again, I have colleagues with whom I've very close and happy.

**VDT**: There is a demography kernel here or are you it?

**BLAKE**: Oh, heavens no. There's a demography kernel in sociology and I have a joint appointment with the department of sociology. There is one person in geography, the chair of the department, who is very interested in demography and does a lot of work. There are people in economics who have an interest.

**VDT**: Who are some of your colleagues here?

**BLAKE**: Well, Valerie Oppenheimer . . .

**VDT**: Of course! [Judith Blake's sister.]

**BLAKE**: And Ken Bailey and Georges Sabagh and David McFarland, so there was quite a nucleus over there, in the department of sociology. And there has been increasingly a methodological nucleus, which spills over; we attracted Dick Berk who had worked with Dudley Duncan at Santa Barbara and who came here a couple of years ago. And people at Rand [Corporation], a very large component at Rand [in Santa Monica]--Linda Waite, Peter Morrison, Kevin McCarthy, and Julie DaVanzo.

**VDT**: Do any of them ever teach here?

**BLAKE**: From time to time, yes. And then there's the University of Southern California group. We have a group that meets for dinner once a month and there's a lot of interaction. So there hasn't been a feeling of isolation at all about this area. I'm chairman of the academic advisory board of the Rand Graduate School, so I have Rand connections. When you take it all together, there are really a large number of demographers down here, with fairly different interests, because the Rand group has been, with Peter, heavily into migration, and Kevin McCarthy has been involved in international migration, and Linda's been involved in fertility and female labor force participation; Julie DaVanzo has been involved in the economic analysis of fertility.

So there are just a lot of interesting people here. There is not a sense of, "There's nobody here to talk to." If we had the time to get together more, we'd have plenty to discuss. Then Dick Easterlin and Eileen Crimmins came to USC. The group at USC had quite a few people in it already--David Heer and Van Arsdol and Judy Treas. I've never felt I was out somewhere where nobody had even heard the word demography; it's been a very pleasant situation.

So that takes care of that saga. Let's see . . .

**VDT**: Could we go back a bit? You're marvelous; I just push a button and off you went--wonderful! You always could speak so well on your feet and you've got all your points there.

I'd just like to ask a bit about the department of demography--I know it was a painful time. You wrote an excellent overview of what such a department could be in the first issue of Demography, in 1964. I gather that was the protocol that you'd hoped for, the ideal program in demography: It doesn't need to be in the department of sociology, but it also needs biology, economics, geography, history, mathematics, and statistics. As Ron Lee said, having gotten his M.A. in it, it was a wonderful smorgasbord of things focused on demography. Do you think that it's really necessary to train demographers that way? Obviously, some others don't, because it hasn't happened.

**BLAKE**: It's happened more than you might think, because over the years, there have been these groups and they have incorporated maybe not that many different disciplines but usually about three. There's a group degree at Penn. And there has been an awful lot more interest in demography and really vital participation on the part of people in economics. So over the years, it has gotten much more taken for granted that that kind of training would be useful.

I think it would be a good idea to have that kind of fermentation, because the field I don't think has ever really seen its whole development, especially along biological lines and lines of that sort. It's been very heavily a social science field. I think it would be a very good thing for the field to have more input from, for example, people who I feel are very wrong in the way they think about things but nonetheless of great interest--the sociobiologists, who are out there doing their thing, which in some ways is very demographic. I went to a conference once that Alice Rossi had organized that had a heavy component of sociobiologists. I did a lot of reading in preparation for this and was fascinated at the overlap and realized that here was a whole group of people who really would benefit--and we would benefit--from interaction. Now, I don't agree with them, but that's irrelevant. What's relevant is that they're really thinking very demographically, and it's almost as if they don't know it.

I think the field of demography is very broad. I never agreed at all with Keyfitz's notion that it

was just a kind of simple-minded mathematics or something--something that was pretty low-level. I have always felt that it was a field that impinged upon so many areas and that one could, if one could ever realize this in training, have people who brought a demographic perspective to a lot of other fields, which would make people realize what a vital and fascinating perspective this is. I think this may yet happen as time goes by, because we're getting a lot more demographers--a lot of them trained by Berkeley, I'll have to say. We trained an incredible number of people in the amount of time we were there.

**VDT**: Could you name some of them?

**BLAKE**: Well, there was Ron Lee and Harriet Presser, who got her actual degree in sociology, and Peter Uhlenberg and Bob Schoen and Don Hernandez, who's now at Census [Bureau], and Das Gupta, who's now at Census, and Jorge del Pinal, who's now at Census--who heads the ethnic division--and June Sklar, who died, and Geoff McNicoll, who's in Australia at Canberra, and Greg Spencer, who's now at Census. I don't know how many we have at Census; we must have about six or eight just there alone.

**VDT**: All of those were registered in the department of demography?

**BLAKE**: Yes--and a lot more. We had quite a few foreign students who were trained there. I never really counted, but we must have trained about 25 doctoral students there in that very brief period of time.

**VDT**: And these were demographers; the degree was in demography?

**BLAKE**: Yes. Now June, I think, and Harriet Presser got theirs in sociology, but the others were all demographers.

**VDT**: Richard Easterlin in our interview this morning said he felt that the primary degree of demographers should be in economics or sociology or one of these traditional things, because he felt you weren't taken seriously enough by--looking for an academic post, you had to be a sociologist or an economist or whatever and you would be able to teach population. What do you think of that?

**BLAKE**: I think as things stand right now that's true. How you would change that is the question I guess I would raise. I think one way is that you would start getting people who were from other disciplines taking these courses and training themselves, and then instituting courses in other departments, like psychology, biology, and so forth. This would really start a legitimation of thinking of population as a field that belonged also in some of these other areas.

I think it's true right now that some of these social science departments have a lock on the field, but I think you could broaden the constituency very much and have people come at demography from these other vantage points. I just gave the example of sociobiology. That does mesh social science and biology. But unfortunately, in my opinion, the social science component of it is incredibly naive, so that they never really have gotten what they should in the social science and demographic training--they sort of glom onto ideas which are a little half-baked in these areas--and they've never had the basic training, thinking through some of the things that they should.

So what I would be interested in is trying to think of how one could bring to a lot of other perspectives what I think population training can do for them, and having this broaden out a great deal. That was always my interest. I never thought the status quo was any different from what they were saying; I was interested in how you changed that.

**VDT**: I think for applied demography this broad, across-the-fields training that you had in the department of demography would be the best--don't you agree?--applied demography such as in state and local government and in business, because that's perhaps providing a skill, not leading into research.

Even though you want demography to spread out, would you say that sociology is a good, if not the best, training for demographers? Of course, traditionally, American demographers have come out of sociology, although there are more and more economists. You wrote in your chapter called "Sociological Perspectives on Population Studies" in Schoen and Landman's 1982 book, Population: Theory and Policy, that training in sociology enables a population analyst "to make guesses or predictions that are more valid than might be the case in the absence of a sociological point of view."

**BLAKE**: I agree with this; my bias is very sociological. The only thing that I think you have to recognize to be realistic is that sociology as a field has so many components and the trend in sociology over the last few decades has been for it to fractionate more and more into many different things. It's very difficult from the institutional point of view to get any given sociology department to put a lot behind demography. What we're really talking about when we talk about whether you should train in a department of demography or in sociology or economics is not whether either of those disciplines is a good training as a basic training for demography. The fact of the matter is you're looking at departments that have so many claims on their faculty positions that it's very hard to get them to stabilize a component in this field. You can have periods when things are going very well and then, if you lose a few people, if someone isn't there, it's just very difficult for the younger people to keep that momentum going.

So I see whether a department is wanted or not wanted much more as almost a budgetary thing. It is much more saying, "Are you prepared; are you going to be able to keep this field going within this university, wherever it happens to be?" And if you don't have your own little bailiwick, you're competing for resources with all the new fads, whatever the latest fad is in those fields.

I see that as a major problem. It's not that I would ever argue that I don't think a social science is the best basic training; a social science and statistics and math training is, in my estimation, the best basic training. It's that it's very difficult to get sociology departments today to put a lot of importance on population. That's true here as well as a lot of other places. People just have so many different things that they're interested in in sociology--much more than there used to be. It's an incredible number of things that you probably haven't even thought of. Take something like ethno-methodology, which is a big thing here [at UCLA]. People want to keep a big component in ethno-methodology, so those faculty positions, if they open, you're competing with that. It's a very esoteric field, but it's got a foothold here. I'm not knocking it; I'm just saying that you've got all these people that say, "No, we want that FTE for this."

**VDT**: FTE?

**BLAKE**: FTE is a faculty full-time position. So I think this is the main problem, that you'll get one or two people, somebody like Dick Easterlin, who's in economics, and one other person here, somebody like me and a few people else, and that's going to be it. And if you want to talk of demography as a field, you have half of their time, because everybody's teaching something else as well. They're doubling up; they're teaching courses that aren't in their field in order to have coverage; doing undergraduate courses and graduate courses; they're really running. Because you can't get enough people in the field.

I never would argue with any of the academic reasons for putting a heavy component of social science training--that would always be my bias. The real problem that I see is that we're just not getting resources for population in universities. And if it isn't perceived as something separate, with

people ready to fight for it, you've never going to get those resources. You're always going to be just about where we are now.

**VDT**: Do you feel that some of the training in demography, and the research that is going on, is getting a bit too mathematical--in economic approaches, very quantitative-oriented, and computer modeling and so on?

**BLAKE**: I don't know that that's the problem as much as that a lot of the people who have gone into it have come from a very quantitative background and are really not that much interested in the substantive problems of demography. Now, I think the substantive problems of demography are amenable to as quantitative an approach as one can make work in the field--as one can deal with in terms of measurement levels that we have and this sort of thing. It's not that I see the quantification as the problem; I see sort of playing games with the field.

**VDT**: Lacking the overall framework? You and Kingsley in your all-important 1956 article, "Social Structure and Fertility: An Analytic Framework" [Economic Development and Cultural Change, April 1956], which has influenced fertility research forever after, set out that framework, with intermediate variables. But you have criticized the World Fertility Survey as emphasizing too much the collection of data on the intermediate variables that were in that framework--marriage, breastfeeding, contraceptive use, and abortion [in review of World Fertility Survey Conference 1980: Record of Proceedings, Population and Development Review, March 1983). The same thing has happened somewhat with the National Survey of Family Growth. Of course, there was quite a bit in the National Survey of Family Growth on labor force participation, but not enough was being looked at as the social and cultural background, which determines the motivation for fertility and works through the intermediate variables to influence fertility. You said that your 1956 article had meant to stress that--getting the broad framework, the explanation.

**BLAKE**: Right.

**VDT**: And now you feel that in some cases demography is moving away from looking at the broad picture?

**BLAKE**: I think what's happened is that whenever you give people a framework and you're going to mount something like the WFS effort in which they're trying to standardize the data gathered among huge numbers of countries and you're having to deal across a very large number of people, many of whom are fairly naive from a social science point of view, you are inevitably going to find that you get down to the least common denominator of gathering data on variables that may not be important. We said, "Here they are." They said, "That's fantastic; here are these variables, so you can just go out and crank this out this way with this template."

I think it's been fine that there have been data on those variables. But fundamentally what we ended up with in the WFS was not really attention to--and it would have been impossible; I'm not saying that they could have, given the format--attention to what the social and cultural influences on those variables are. And that was the whole point. The whole point of the [1956] article was to say, "If you want to look at sociocultural influences on fertility, then you have to be aware of what they operate through and that these are the variables they operate through." I think nobody would ever have accused us of thinking that what we wanted to do was to just look at those variables.

So it ends up that something like that provides people with a very mechanical way of looking at things. It's unfortunate that that's happened. And yet a non-mechanical way, for all those countries with all that's going on, would not have been possible. So it's a valid criticism, but I'm not saying it's

anything they could have done something about. I don't think they could have done it differently if they wanted to do something at that level. But it's a problem, because I don't think that survey has led to eye-popping theoretical results; we just have an enormous amount of data.

**VDT**: It generated the Contraceptive Prevalence Surveys and now the Demographic and Health Surveys, and that's a quick and dirty way to get data on fertility and contraceptive use.

**BLAKE**: Right.

**VDT**: How did you get the idea of using the Gallup Poll for collecting data on American attitudes relevant to fertility--the questions you added to those polls, beginning--when did you begin adding questions?

**BLAKE**: I think mid- to late 1960s.

**VDT**: Do you still use it?

**BLAKE**: No.

**VDT**: It went on for about 20 years, didn't it?

**BLAKE**: Not that long. It went on until I left Berkeley [1976]; I did some of it down here for a couple of years. It became incredibly expensive to do it. In the beginning, it had been a very economical way of gathering data and keeping a trend going on a lot of different variables, so it was really terrific for a while. They got all the background data and it was possible to ask these questions very inexpensively and get the advantage of all those background data. But as time went on, the questions became expensive to insert.

**VDT**: That was in a sense going back to your early work in Jamaica, asking attitude questions that you can't ask in a census or a Census Bureau Current Population Survey or whatever.

**BLAKE**: Yes, and it used a very big data set that was available and available for a long time-span, and that's what I thought was so great about it.

**VDT**: Fascinating; every year on the edge of our seats, what would you come up with next?

**BLAKE**: Yes.

**VDT**: In your 1983 review of the World Fertility Survey in Population and Development Review, you laid out three things which I think must be your philosophy for research and writing. You criticized the WFS work and publications to that time as lacking what an "elitist voice" craves: a theoretical relevance and simplicity of conceptual framework, and that an elitist voice responds to lucid presentation of investigative problems, and admires papers honed to a state of deceptive artlessness. I presume that's been your philosophy?

**BLAKE**: Not a philosophy; it's been a goal--not always realized, by a long shot. I've always been interested in what I felt to be major problems and not really that interested in just descriptive stuff. So it didn't thrill me with WFS that people were taking data for 20 countries on age at marriage and cranking them out and on this and on that; I don't find that a very interesting way to live. I guess it's a

fairly elitist thing to say, but looking at some of those papers, I felt that a lot of young people's talents were not being used to the maximum in having to do that. And I wasn't even sure, although I don't know those data, that they were being used to their full advantage.

I felt always, in my own work, that I really wanted to deal with what I felt were some critical problems in the field. I don't know if I did, but I was interested in doing that. And that I wanted to present them clearly if I could, and in an interesting and very lively way, so that people would have access to them.

Also I always had as a goal that people should, I guess, have as much enthusiasm about population as a field as I did. So I wanted to almost shake them and say, "You've got to realize that this can be very interesting and exciting and you should realize this deals with things that are big things and big problems, not just little mathematical sums or something. I want you to see that this is something that can be interesting and that you can get steamed up about." So part of what I had as a goal myself always was that I would sort of try to reach out and grab people and have them suddenly realize, "This is a very interesting thing." And when you read it, you say, "Gee, I didn't realize this, that it related to this or that. I suddenly see this a little differently from the way I did, and it's not that dull after all." I felt so stunned by the notion that this was a very dull field, that people would keep saying, "Oh my Gawd, Dullsville--this is it!" So I always had this sense that what you did should reach out and try to make them realize that this isn't true and that they should get stimulated to be interested in it. This was always a sort of crusade, that I felt they should understand that this was just fascinating.

**VDT**: I think you have achieved that, because your writing is stimulating. And you have worked on your own. You also said in your article on the WFS that "like so much demographic investigation of the past decade, it has been research by committee." And you, I have the impression, have avoided research by committee.

**BLAKE**: Yes.

**VDT**: In other words, you like to be a lone operator?

**BLAKE**: Well, I've worked with other people. I worked with Das Gupta for a while. It was a very good collaboration, because he was so mathematical.

**VDT**: I mean not at least one or two, but not any group project?

**BLAKE**: I've always found that maybe a little tiring. I've worked with doctoral students. But to work on collaborative projects, I don't know why I've felt that that way one got involved with people who might have standard definitions of situations that I wanted to break out of, and I would have found it very difficult to do that if I had hitched on to other people that much.

Part of what I found interesting about what I was doing was always to try to see things somewhat differently, and it's not necessarily so easy to drag other people along with that type of thing; you can spend a lot of energy on that. So I think that's been part of it, that I felt you should see things in a different cast if you're working on them.

**VDT**: Well, you certainly have.

**BLAKE**: Well, I don't know.

**VDT**: And you've written mainly articles. Was that deliberate--because they can get out faster and

address current salient issues?

**BLAKE**: I have a book that just came out.

**VDT**: Oh, I didn't know that! It's Family Size and Achievement [University of California Press, 1989]. It's right on the topic of your PAA presidential address ["Family Size and the Quality of Children," published in Demography, November 1981]. Summing it all up. Good. So that's an important book. That leads to my next question: Which of your publications do you consider most important and why?

**BLAKE**: I don't know. I think that's for other people to say.

**VDT**: Well, of course, "Social Structure and Fertility"--the framework. But what would you like to feel?

**BLAKE**: I guess some of the things that I enjoyed the most doing. I certainly enjoyed the most recent book a lot. And I guess I enjoyed the article I did for the Population Commission, which was "Coercive Pronatalism and American Population Policy" [in Charles F. Westoff and Robert Parke, Jr., eds., Commission on Population Growth and the American Future, Research Reports, Vol. 1, 1972]. And I enjoyed the consumer durables article ["Are Babies Consumer Durables?", Population Studies, 1968], which was the critique of Gary Becker.

Let's put it this way. When I'm working on them I'm always right in the thick of it and I think that there's nothing more important in life. And once they're finished, the fact of the matter is I rarely read them over again; it's over and done. So it's kind of hard for me to ever even think of them anymore as mine--I'm into what I'm doing right at the moment. It comes as a surprise to me sometimes--if I assign them to a class, I have to read them over--I say, "Did I say that?" So I don't dwell on them very much.

**VDT**: Even when . . . For instance, in your article on "The Supreme Court's Abortion Decisions and Public Opinion in the U.S.," that appeared in a 1977 Population and Development Review, you pointed out that in response to a question you'd put on the Gallup Poll in 1976, 46 percent of respondents then favored a constitutional amendment to outlaw abortion, to revoke Roe v. Wade, which had been passed three years previously, and you foresaw a battle to implement Roe v. Wade. Well, that certainly happened. You must think back on that--though 12 years ago.

**BLAKE**: I have to admit I think back on that. And I more than anything think back on my rationale for that. I really had big fights with the Planned Parenthood people about that. My feeling was that they should quit doing things that made it worthwhile for the pro-life movement to really get under way. It's a little bit like, you know, OPEC has always tried to gear the price of oil to a level that would make it uneconomical for us to develop other fuels. They figured this out very carefully; they have never wanted us to feel that that was worth our while, so they always come in right under that figure, whatever that figure is.

I was really trying to say, "Look, you guys, you've won, and the basic thing is in place. Now don't do things that make it worthwhile to have a pro-life movement. Don't be so inflammatory. Don't insist that government money be used for abortion. Don't do the things that are going to create a really hard-core pro-life movement, because you're going to lose the ball game. Don't think that just because you won now, you're going to stay this way." And I would argue with Fred Jaffe [of the Alan Guttmacher Institute] about this, Jeannie Rosoff, with everybody, and try to say to them, "This is going to be a disaster, because you are giving these people a reason to have a strong movement. What you

want to do is come in short of a reason, so that it's hardly worth anybody's while to make this big a fuss. If all you're saying is people should have access to abortion, and you're not using taxpayers' money to have abortions, you're not being basically offensive to anybody in the population, because you're not taking money from Catholics or fundamentalist Protestants for abortions; you're just saying people should have access to it. And you solve the money problem another way, somehow or other, but don't push it so hard." And they just couldn't see it that way.

**VDT**: Well, they should have listened to you! [Laughter]

**BLAKE**: I suppose so. I quit working on it, because I saw such disaster ahead and I didn't want to predict it anymore. I didn't want to be the person who was saying disaster is coming down the pike, because my pro-abortion predilection was obvious. I felt so overwhelmed by what I saw coming.

**VDT**: But you did see it, in that case. Here's another one I wanted to catch you up on. It's not quite fair, but anyway. Back in the 1965 book of Mindel Sheps and Jeanne Ridley [Public Health and Population Change], you wrote in your chapter ["Demographic Science and the Redirection of Public Policy"] that because of deep-seated familistic norms in Europe--and you'd already claimed that there were these norms in the U.S. too--"it seems unlikely that Europeans are on the road to permanent adjustment of family size at approximately two children." Now, what about that?

I have to admit that the latest [1989] Population Reference Bureau World Population Data Sheet originally had Sweden's fertility rate up to 2.0, but Carl Haub told me at the last moment, "Don't tell anybody out on the West coast, but I think I'm going to lower it to 1.9." Anyway, it's gone up. [Sweden's total fertility rate was 2.0 in the published 1990 Data Sheet.] West Germany has gone up slightly to 1.4. As you know, Italy is now lowest at 1.3. Do you still feel Europeans will not settle at two--in other words, you were assuming [in 1965] they'd go higher?

**BLAKE**: Mainly on the basis of the way social trends were at that time, which was not for there to be very, very high levels of female labor force participation. A lot of this was based on the notion that family structure would stay the way it was, and I'm not apologizing for having some clouded crystal balls.

**VDT**: Okay. Of course, it went up to nearly half of all couples are cohabiting.

**BLAKE**: Right. And not only that, but a very high level of female labor force participation. Very tight labor market there, so it became a viable solution for them, in addition to all the immigration and everything else. It was really more based on the notion that if they're not going to have these women go into the labor force, they're not going to shake up this family structure in some way, then this is what one sees for them. But, indeed, they did.

**VDT**: That also leads to my next question, about U.S. women and their labor force participation. In your September 1974 Scientific American article ["The Changing Status of Women in Developed Countries"], that great issue ["The Human Population"], you doubted that more than a small minority of American women would seek to be high achievers in the work world, because then you still felt that there were these underlying pronatalist constraints--I remember thinking, "She's right," when I read that article. High-achieving women who would seek a status not derived from husbands would risk not marrying and having children, which your research showed most women wanted. Have you changed your views on that?

**BLAKE**: Well, you know, not that high a proportion of women are high achievers even today. I think

in this respect, we haven't really seen the whole picture yet. In a way, I've always felt that people sort of tinker with their fertility behavior and that they look to see what a previous generation has done and in a sense try to learn from that. And women are watching each other very carefully. I think young women are watching how contemporary women are doing things and asking whether this is the route they want to go. And I'm not sure myself that we're not just going to see some reaction against this extremely low fertility. That there's not going to be some generational reaction which in a sense says, "I've seen how those guys did it and I'm not sure that having that few children or one child or risking having no children, even"--which in a sense is what is happening with young women--"is the way I want to go."

So I'm not myself quite sure that things have changed that much for the majority of women, that young women are going to see razzle-dazzle careers for most women as an alternative to the family, because I don't think that's what one sees. I'm just not sure that young women who are really watching the situation could not opt to have two. I don't think we'll have a baby boom situation. But I think this period we're in could be something of an anomaly--not a huge anomaly, but something of an anomaly. Because you talk to a lot of younger women and in part what is influencing them is that the conditions for having children are so difficult. It's not that they don't want them, it's that they're saying, "My God, you can't get a house." When you start having children is when the sex roles start getting very rigid. You can share the dishes and the housework and the vacuuming, but once you have children somehow you get shunted into a situation where you're really kind of back to the 1950s. No matter what anybody says they're going to do, they're not going to do it, because they're going to be the chief wage-earner, by God, and somebody has got to make the money in the family. So the logic all goes in the direction of your getting into a much more rigid female role. And this is one of the things that really bothers them. We've moved close to an anti-natalist environment now, in which it's very penalizing to young women to think of themselves as getting involved with more than one kid or a couple of kids.

So I'm not sure that the score is in yet on this. As I say, I wouldn't predict a baby boom, but I'm wondering whether we're not going to see . . .

**VDT**: Well, the U.S. TFR now rounds off to 1.9, which is more than it's been in the last few years. That's more than it's been since 1973 or 74. It will be interesting to see what happens.

You've written quite often of the only child, another issue, and the issues facing high-achieving professional women. I suppose that reflects your own experience--you have an only child?

**BLAKE**: Let me say this: This interest in the influence of family size on achievement was actually motivated much more by an interest in the effects of high fertility. And other people have just been so interested in the only child that this is all that I can ever somehow get anybody to ask the questions about. I did have one child, but in a sense I brought two children up.

**VDT**: Yes, your stepson [Kingsley Davis's son by previous marriage].

**BLAKE**: Yes. He lived with us during virtually all of my daughter's childhood and young adulthood, so I never thought of her really as an only child, because he was a very material brother to her and it wasn't the same as her really being an only child.

But my interest in this, which I think is important, is much more in trying to say, "Can one look on a disaggregated level, on an individual level, at the effects of high fertility?" Because if one always looks at the aggregated level--the sort of Julian-Simon level--one can say almost anything. And I really felt it was very important to gather data on what at the individual level high fertility does or does not do. I found in this book [Family Size and Achievement], using many more data sets than were in the Demography article [1981 PAA presidential address], that it really has very deleterious

consequences--for IQ, for education, for a lot of things. It's very hard to find any good consequences of high fertility.

Then when somebody is saying, "Oh, rapid population growth is just terrific," you have to say, "Terrific for what?" If it's terrific for individuals, we don't find that. It has to be super-terrific at a macro level to compensate people for this suffering at an individual level.

**VDT**: You extend the field to less developed countries in the book?

**BLAKE**: No, only U.S. data. But it's on kids--a lot more on kids--and a lot more studies developed. And they are really sort of unbelievable in the sense that you never find good things coming from high fertility. You never find people from very large families doing well or better than people from the next size down or something; it's always bad news.

So what I was really interested in was saying, how can we start gathering data that are going to raise a question with this Julian-Simon-like argument, which is to say, "Well, if it isn't good for people, for individuals, what kind of an argument can you make that it's so terrific for society as a whole?"

**VDT**: Great! That's important to know. I'll be interested to see what you think of that recent Atlantic Monthly article I mentioned in which the author [Bernstein: "IQ and Falling Birthrates"] cites a 1984 article by James Flynn which spoke of a massive gain in the mean IQ of Americans from 1932 to 1978. This author, Bernstein, claims that IQs have actually been dropping and that it could be because high-status American women are leaving reproduction to the low-status, lower-IQ women. What accomplishments in your career have given you the most satisfaction?

**BLAKE**: Oh, gosh, I don't know. I've gotten a lot of satisfaction out of teaching. I've greatly enjoyed teaching.

**VDT**: Everybody I know who's studied with you thinks you're a dynamo. That was one expression that John Weeks used when we were talking about you at the recent PAA meeting. Alex von Cube, a good friend of mine at the Population Reference Bureau, said your classes were so stimulating, that occasionally there was controversy, lots of controversy. He said you could be hard on people, and sometimes you were a bit conservative, some of your students felt. You say you have gotten a lot of satisfaction out of teaching?

**BLAKE**: I've never liked teaching huge amounts, because I like to prepare a lot, and I like to use teaching as a way of picking up on issues that I wouldn't ordinarily be starting to write about or do research on and thinking them through. So I've always used it as that kind of a vehicle, for getting into something that I was just starting to think about. It gave me an opportunity to do a lot of reading, a lot of thinking. It has fed into research--not on a day-to-day basis but more to say, "This is a way of beginning to think about something and get input from people and have them criticize you and get a lot of stuff going on something." It's been very creative for me for this reason, and I've enjoyed it enormously.

Interestingly enough, over the years I've seemed far less conservative to people, partly because they have gotten more conservative, I think. So that is rarely mentioned at this point in time, because people have changed their own views about a lot of things. That helped--not that I changed them, but the world has changed.

**VDT**: Indeed. Have you found it an advantage to be a woman in your career? I think probably your enthusiasm--well, frankly, most women teachers I've known have been more enthusiastic than the men teachers.

**BLAKE**: I wouldn't say that when I was coming along it was an advantage. It was a big disadvantage, I think; there's no question about that. Nobody thought of you as being anybody and nobody ever thought of you for anything. When I looked and saw how young men were sort of mentored and coddled and taken under people's wings and pushed and so forth, I didn't feel that during my youth it was an advantage to be a woman at all, in academic life.

On the other hand, I have tried, I think, to make it an advantage for other women. I've felt very strongly about this. I'm not a rabid feminist in the sense that I've joined groups and things of this sort. But I have been very instrumental in pushing a lot of women along. And increasingly so, as opportunities are arising, I think I have helped a lot of people to get good jobs.

**VDT**: Among your graduate students?

**BLAKE**: Yes. And helped them over life-cycle events, like pregnancy, for example, which I think many males have always felt was, "She got pregnant; that's the end of that." I have been very supportive of people who have been in this situation and made them feel that there's no reason at all why they can't continue, through their pregnancy, getting their dissertation done.

**VDT**: It might have come out of your own experience. You had your daughter, presumably, finishing up your Ph.D. dissertation?

**BLAKE**: Yes. Well, being pregnant, they just sort of relegated you to the boondocks in those days. People's attitudes toward all of these things were very rigid. No, I can't say that it was an advantage. I think really in my youth, it was a big disadvantage and it would have been a lot easier not to be a woman. But, on the other hand, it's a good character-builder. [Laughter]

**VDT** [talking about Richard Easterlin, interviewed that morning]: Easterlin comes into his office [at USC] only Tuesdays and Thursdays.

**BLAKE**: He may just know how to live!

**VDT**: Well, this year he and Eileen are both on sabbatical. They have the two small children that have to be gotten to two different places each day, one to school, second grade, and one to a day care center. That makes life more complicated.

**BLAKE**: It really does.

**VDT**: You and I are past that stage! What about some of the questions that I asked you in advance? What have you got there?

**BLAKE**: When I got your letter, I jotted these things down, quite a while ago. You asked me what I felt were some leading issues in U.S. demography. I think there would be a consensus that one is how we're going to handle the volume of immigration that we're experiencing, both legal and illegal, and the resulting fertility of these immigrants. And in a sense, I guess I would regard as a very high priority how we're going to upgrade them and be sure that we're not taking on a permanent underclass in the society, which I feel strongly about. It would be distressing to me--although I won't see it in my lifetime--to feel that we are moving in the direction of taking in people who are going to be low-level workers generation after generation and that we're not going to be able somehow to move them in the direction of education and better jobs.

The fact that they're not doing super-well right now compared to native Americans is not so

significant. They're certainly doing a lot better, obviously, than they would be in their own countries, so you can justify that as a plus. But I wouldn't see that as the criterion of relevance for very long, and I would find it disturbing to think that what we're really doing in taking a mammoth number of people in--I guess we don't have too much choice--is that we're creating a group of people who are just going to be in ethnic ghettos, not speaking English and not able to move--and not have their kids able--to move ahead or have opportunities in the social structure. I see this as a major problem that we're faced with in this country and don't see the end in sight very soon.

**VDT**: Have you done any work on that?

**BLAKE**: No, I haven't; I probably never will. But I certainly feel strongly about it. I feel it's a baleful development, if what we're doing is not going to be able to offer them the opportunity that other immigrants have had in this society.

I'm also very concerned about the effect they're having on black opportunity. It's not only the immigrants' fault; there's a whole restructuring of the economy that is affecting black opportunities. But I think we're in a period about which we should be very concerned in terms of the legitimacy of occupational stratification in this country and how things are moving. As I say, I don't get too upset because people who come to this country from very poor countries are now not suddenly doing super. That's never happened; that's an unreasonable expectation and you can say, "Well, they're probably doing pretty super compared to where they came from." But I wouldn't consider that an answer for the second generation, and I would find that a very disturbing outcome if that is going to happen. So I think this is really a big problem.

I think we also have a problem with how to manage the enormous urban densities that we're accumulating and which I think we're going to continue to accumulate if, as Larry Long and others tell us, the metro-nonmetro turnaround has turned around and we're back to metro growth again. Somehow, unless we can find mechanisms for dealing with the environmental problems, we're going to have some pretty crappy cities to worry about. And living here hasn't changed my views.

**VDT**: Absolutely--the smog and, alas, the freeways.

**BLAKE**: Right. I think we also have a problem of positioning ourselves with respect to the rest of the world, certainly the developing world, and having them perceive the high fertility as <u>their</u> problem and sort of not our mandate. I think this is a very important thing.

**VDT**: Not our . . .?

**BLAKE**: That if they have an average of eight children, that somehow <u>we</u> are the ones who see this as a problem. I think we somehow have to position ourselves so that becomes their internalized problem about which they're interested in doing something. When you see what's happening in Africa and in some North African societies, it's a very distressing thing. And the fact that they're not taking hold vigorously in many cases is still a distressing problem which hasn't gone away.

We certainly have the issue of how to deal successfully with an aging population. I myself--this is my own personal predilection--don't think people should be retiring this soon. I think they're very, very ill-advised to do so--economically, psychologically, and all kinds of different ways.

**VDT**: At 65?

**BLAKE**: Yes--or <u>earlier</u>. We keep saying if you make retirement at age 70, you're never going to get people to retire. That's ridiculous; they're retiring earlier all the time, so it doesn't make any difference

how high you set the retirement age. I feel this is a major problem that we have to concern ourselves with.

We've got plenty to worry about in demography; I don't see these problems disappearing. I think we have a lot of interesting things to do research on and a lot of interesting young people who are coming along to do that work.

**VDT**: That was going to be one of my final questions, what you see as the outlook for demographers in the U.S. You say there are young people coming along and there are still places for them in research and training. What I really wanted to ask was: Do you think all the jobs now are in applied demography--business and state and local government?

**BLAKE**: Heavens, no! If you want an academic department to recruit somebody, you have to fight to get them. And I think this is going to be true more so in the 1990s, when the demography of organizations is such that there's going to be a whole cohort of people, myself included, who will be retiring as the decade wears on. They're almost all going to drop off a cliff like lemmings, because they all got hired in this same period of the 1960s, when they came off the Ph.D. treadmill. All universities are just scared to death that they're going to hit the 1990s and they'll have a terrible time recruiting. Universities have a terrible time recruiting now, because--fortunately, in my opinion--there are so many other alternatives for young people. I'm just delighted. I thought it was terrible when all you could do as a demographer was to get an academic job, or a couple of jobs in places in Washington that hired people. I'm delighted that this is a brisk market today.

Now, one of the things that gives you a hard time recruiting, of course, is the dual-career problem. Every time we get a woman who is all set to come, we can't find a job for her husband, and vice versa. So this is the other feature of it.

But I would say there's going to be a terrific market. But there are some terrific young people coming along. My feeling is you look at this new crop of younger people and sort of mid-level people and it is a very encouraging group of people. I always am thrilled to see the work that's being done. There are just some really interesting people. So I think we're coming right along. But then, I'm always the optimist.

**VDT**: It's wonderful that you are. Let's turn to PAA. Do you remember the first meeting you attended?

**BLAKE**: I certainly do.

**VDT**: What was that?

**BLAKE**: It was down in Virginia, in . . .

**VDT**: University of Virginia, Charlottesville. That was 1954.

**BLAKE**: Yes.

**VDT**: It was the last time they took a photo of all the participants at the meeting, lined up, maybe 150, 200 people. Dan Price gave the archives a copy of that photo. It was on the campus. That must have been a nice one.

**BLAKE**: Oh, it was lovely; it was wonderful; I thought it was heaven!

**VDT**: Do you remember the early meetings you attended, the atmosphere, the luminaries? It must have been wonderful, because there were Irene Taeuber, Margaret Hagood . . .

**BLAKE**: Oh, I know.

**VDT**: And Dorothy Thomas, of course, and all the people who were great names then and became even more so.

**BLAKE**: I used to go very regularly. And the one at Princeton [1955] was the one where we read our initial "Social Structure and Fertility" paper. [Kingsley Davis and Judith Blake, "Recent research and the theory of fertility in underdeveloped areas," presented in a session on "Foreign Studies."]

**VDT**: That was the last one at Princeton.

**BLAKE**: Yes, after that it was not big enough.

**VDT**: That must have been exciting. Was there a reaction to your paper--aware that they were hearing something historic?

**BLAKE**: There was a big reaction all right. Some of it was very controversial, as usual. But when the paper finally came out, it caught on very rapidly, that whole idea, because it solved a very basic problem of how to look at things. It wasn't controversial after that, certainly.

I remember an awful lot of these meetings, looking at the list, because they've all had something special about them. But for me, it was a great thing to be able to go to the meetings--I was still so goggle-eyed--and listen to the greats, just be around. It was a very satisfying experience.

**VDT**: Everyone has said that it was very easy to approach everyone; they were convivial and the high-and-mighties talked to the not-yet-so-high-and-mighties.

**BLAKE**: Yes, and they were very welcoming. You would be introduced to people that, you know, compared to them you were a kid, nobody, and you were always welcomed. They were always very happy that you were in the field. Now, from the vantage point of many years later, I can understand this, because it didn't have many recruits, and every time they saw somebody, they were so happy that at least there were some young people who were coming into demography. At that time, you just felt very much as if this was a good place to be and that these were not people who were unapproachable, as you say. It was a very pleasant experience always; I really used to love that.

**VDT**: You were only the fifth woman president in PAA's history.

**BLAKE**: Is that true?

**VDT**: After 50 years.

**BLAKE**: Awesome! I've never counted up.

**VDT**: Here's the list. There you are: No. 5 woman, 1981. There was a long gap of nearly 20 years between Dorothy Thomas in 1958-59 and Evelyn Kitagawa in 1977. What do you think accounts for that? There were the early women superstars. You probably read Anne Lee's nice vignette in PAA Affairs last fall [Fall 1988] on "Early Women Superstars in PAA"--Dorothy Thomas, Irene Taeuber,

Margaret Hagood, and Hope Eldridge. You mentioned having had your first demography course ever from Hope Eldridge. What are women's positions in the PAA--and in demography? You've already said it was a great disadvantage being a woman--not in PAA.

**BLAKE**: No, I felt that actually it was a very welcoming situation in PAA. I think what happened, perhaps, was that there was just a big gap in the population at risk, you might say. We went through the baby boom and one thing and another and there just weren't that many women who were the Irene Taeubers and Evelyn Kitagawas. So I think that if you were to look at the profession, you wouldn't have seen that many very distinguished women who were available to be a president. This might have been part of the reason for the gap.

**VDT**: That sounds plausible. Were you involved or interested in the issues raised in the early 1970s by the Women's Caucus--more equal rights for women in the profession, at the universities? That was, of course, the time of turmoil at Berkeley and at other universities. But the women's institutions in particular?

**BLAKE**: Well, as I mentioned, I have never been very political, in part because I thought when you got involved with groups that were very political, you never knew which way they were going. You never saw eye-to-eye with them completely, and when they were very radical groups, you weren't sure you were going to feel comfortable for very long in that situation. It's one thing to be involved with a big political party and to say, "Well, it doesn't represent everything I believe in, but I can live with it, because it's never going to do anything much one way or the other." But when you get in with very radical groups who have very strong feelings about a spectrum of things that you may not agree with at all, I think you feel somewhat uncomfortable--at least I always did--throwing your lot in, because you never know which way the cat's going to jump and you see you can't control that very well.

And you have to realize, I was rearing a family and running a household and had a very, very busy husband, and I was just barely hanging in there. So I wasn't exactly a candidate for an awful lot of extra activity. I didn't have very much time. There were years and years when I had no relaxation at all. Life was pretty hairy; I wasn't exactly looking for places to sink my time. That was part of it.

**VDT**: There was one set of issues in PAA that you were involved in, in the mid-1960s, when you were asked to be on the Committee on Organizational Management . . .

**BLAKE**: Yes.

**VDT**: Which was originally set up by Paul Glick [PAA president, 1966-67] and Andy Lunde [secretary-treasurer, 1965-68] because the membership had tripled with Don Bogue's recruitment through Demography and there was just too much work for a non-paid secretary. You were supposed to decide whether or not there should be a paid business manager. But then the committee came up with a report that recommended several other things, which many people took issue with: that there should be an active membership recruitment drive and two classes of membership, formulating professional standards--somewhat like the American Sociological Association has done recently, apparently to everybody's horror--and a small grants program to fund small research projects. Those ideas were shot down by Ansley Coale, who was the incoming president and said he wouldn't put them into effect, and by the membership. Do you recall what your feelings were at the time?

**BLAKE**: I just sort of felt that the field should be more aggressive about recruitment and about its own position, and should have a little opportunity to do something like funding people out of the organization. I felt very much, as I grew older in the Association, that some of the charter members, or

post-charter members, were very proprietary about it. I felt that Ansley was pretty proprietary and had a pretty rigid notion of what the Association should be: it would be what it had always been, in his experience. And I didn't see that that was necessarily true. I thought it should respond to its current problems and try to do something about them.

I didn't feel super strongly about all of this. It was an interesting committee; it brought together people who really didn't agree much on a lot of other things and they agreed on this. Parker Mauldin was on it. [Chair: Forrest Linder; members: Judith Blake, Parker Mauldin, J. Mayone Stycos, Conrad Taeuber; ex officio members: Paul Glick and Anders Lunde.] There was a feeling on the part of a lot of these people that we needed to change some things. I don't think anybody saw it as revolutionary. But I think that Ansley felt that anything that changed the status quo couldn't help but not be in his interest, or in the interest of OPR [Office of Population Research, Princeton], and that things were going so swimmingly for those guys that anything you tried to change was ipso facto going to be somewhat suspect. That was usually his position, that, "I like things the way they are." And he liked them the way they were because they were very much in his interest. I appreciated that; I saw what the situation was. But on the other hand, I felt some other people's interests should be taken into account more.

I think this was a big crisis for PAA, but it weathered it very successfully. It moved from being a kind of sandbox for a very limited group of people, who were benevolent--I'd always felt that they were very benevolent and meant well--but they came to feel, as the field burgeoned out, that things were getting a little out of hand and they lost control. And they did--in a way. But it wasn't a revolution; it was just sort of gradual slippage.

**VDT**: I'm so glad you said that, because that is a little different from any angle that anyone's put on it. I've talked to maybe too many of the old guard, or they weren't involved at that time--Sam Preston, Jane Menken--they were not interested. But you're just the right group to approach. Thank you for that.

**BLAKE**: And I don't say that with any resentment.

**VDT**: No, it's just a slightly different view of it, which I hadn't thought about myself.

**BLAKE**: Well, you know how you feel when you start seeing change in things that you're comfy with. You just looked at it always as a place you understood and there were going to be no unpleasant surprises, or even pleasant ones; everything was going to be just itself. I think that's the way a cohort of those people experienced it, that it was just a nice comfy spot to land. And to have all those people come along and start making changes--I think that was really tough. If you give in on this one, what's going to happen next year?

**VDT**: Good point. Let's skip to the 1981 PAA meeting, which was the important 50th anniversary meeting, when you were president. How did you come to choose the topic of your address: "Family Size and the Quality of Children"? It's true it was growing out of your research. Did you see it as a sort of summing up of a trend of research? No, you said you were embarking on . . .

**BLAKE**: Yes, it was really the beginning. I had been thinking about this for a long time. Some people gave presidential addresses which were very appealing and fun. But I felt that if it was the 50th anniversary, it shouldn't be something that was just topical or light-hearted. I felt it should be something that was serious, or was research, or whatever. And I had been thinking about this for a long time, in part because it stemmed from all the arguments--with the Julian Simon argument--about population growth. I had been thinking how one could zero in on this in a way that would be more

definitive.

I started working on this material and realized that it could be quite interesting. I thought, "Well, I am not going to stop this now in mid-stream and start on something else, and what would the something else be?" I didn't want to talk about abortion, because I didn't want it to be controversial that way; I didn't want it to be inflammatory. So this was the logical thing. It was also practical: I was working on it and I just couldn't switch horses at that point.

I said, "Well, why not? I'll start in and do my first article on this. It's not going to offend people, I don't think." And it didn't, I don't think. I didn't want a presidential address that was going to be inflammatory or offensive to people; I felt I had an obligation to have it just be a scholarly paper. People might disagree, but they weren't going to feel out of joint or anything.

**VDT**: It was a very solid talk. As you say, some presidents have felt they should choose something topical. Ansley Coale did in 1968; he spoke on "Should the United States Start a Campaign for Fewer Births?"

**BLAKE**: Right.

**VDT**: Of course, he pointed out that birth rates were going down anyway and no campaign was needed. But he had decided he should choose a topical issue rather than his research, though others have indeed done their research. Three years before you, in 1978, there was Dick Easterlin's address for which he became famous, "What Will 1984 Be Like?", on the Easterlin hypothesis. Which led to his book. And yours led to a book too!

**BLAKE**: Did that lead to his book?

**VDT**: Yes--Birth and Fortune. He said he had put things in the address, such as the impact of cohort size on homicides, suicides, SAT scores, and so on, that he hadn't done before, and he realized that should become a book. And yours did too--but you were heading toward that book, Family Size and Achievement.

**BLAKE**: I really didn't know that then. As I say, it was something I was working on at the time and I didn't have time to just gin up something else for PAA.

**VDT**: In it there was a nice piece on the only child, again--as you said, that's what people always want. Was that what you were being interviewed on by Cable News Network on that Saturday afternoon after the meeting?

**BLAKE**: I don't remember.

**VDT**: We both can't help recall that. Cynthia Green and I, who were running this press office for the PAA meeting, got you on Cable News Network on that Saturday afternoon, and then less than 48 hours later, just within feet of where we'd been sitting [in the Washington Hilton Hotel], there was the assassination attempt on Reagan.

**BLAKE**: Yes, it was really kind of scary, wasn't it?

**VDT**: Among other special things at that meeting, there was the "PAA at Age 50" session, which Andy Lunde and Con Taeuber organized, with papers by Lunde and Frank Notestein and Frank Lorimer--I'm sure it was the last time they spoke at a PAA meeting--and Clyde Kiser's paper, which

was read by Dudley Kirk. I always regret so much we did not get a photo of that panel. Con Taeuber did do a tape and we've got that in the archives, but we don't have a photo. It should have been there. Then there was the dance, with the piano music by Joe Stycos. Did you get him--old friend and research partner?

**BLAKE**: Yes.

**VDT**: And Lee Bouvier had his trumpet. They'd played together before. And that was the first and only time we've had a press office at a PAA meeting.

**BLAKE**: Not since?

**VDT**: Not a press office like that [but there was one, run by Mary Kent, at the 1991 meeting]. This year [1988] at Baltimore there was a session, chaired by Wendy Baldwin, on should demography reach out more to the press ["Research, Policy, and the Press: A Case Study"], at which Alan Otten of The Wall Street Journal was the press representative. He did write up several of the meeting papers. It appeared in his regular column on the front page of the second section of the Wall Street Journal--one of them was Richard Easterlin--but he never mentioned where he'd heard those papers.

**BLAKE**: Oh, really--just somewhere?

**VDT**: Right. Were you responsible for the press office idea?

**BLAKE**: I can't remember. I have to tell you I had an incredible committee here, who worked like dogs on that meeting. Peter Morrison, as usual, who has for years done very good things for the PAA, worked very hard. We had about six or eight people who worked very hard on putting that meeting together with me and were just incredible.

**VDT**: Some presidents have ended up almost doing it themselves.

**BLAKE**: People were so helpful. I think Bill Butz was involved and Peter and Julie DaVanzo--a lot of the Rand people--and David McFarland was wonderful. We had sociology people, Rand people, and I think we may have had some USC people, and Jorge del Pinal from Public Health, and me. We had a great group of people who took hold on that, because we were doing an awful lot of things that required fine-tuned efforts, like these special programs for some of the older people.

**VDT**: Dudley Duncan said he was there. Did you ask him to chair a roundtable then?

**BLAKE**: I think so.

**VDT**: The last meeting he ever attended, and he hadn't attended many for years till then.

**BLAKE**: We wanted to have this be a real 50th anniversary, with people who younger people might not see again in a long time for a variety of reasons. So we felt that there should be very special things, and things that made some of the older members of the Association feel very special. It was sort of our way of saying thank you to them and making them feel that we appreciated all the things that they've done over the years. So there was a lot of devotion involved in it, a lot of sense of people who we wanted to bring out and have appear and have younger members see them and be sure that they had their day still. There was a lot of very overt thinking on this, that we wanted to be sure that this was

that kind of a meeting, that it wasn't only a scientific meeting. It was kind of a recognition of our forebears in the field. There was a lot of enthusiasm here for doing it. It brought people here together and the committee was full of beans about this and very helpful.

But it's an awful job, because, fundamentally, as you get down to the wire, you get to be the central nervous system for it. You can never delegate completely; ultimately, it's all got to come together. And as you get down to that point, it's murder--it really is.

**VDT**: It also must not have been easy being on the West coast, with the meeting in Washington.

**BLAKE**: No.

**VDT**: Well, it was a spectacular success.

**BLAKE**: Well, it was a fun thing, yes.

**VDT**: Do you regret the changes there have been in PAA over the years? The meetings are now up to 84 sessions, eight overlapping [90 in 1990, 91 in 1991, nine overlapping], sessions on Saturday afternoon, close to 1,200 people at Baltimore this year, which is the record [1,399 at 1991 meeting in Washington], and many split off workshops--business, economic demographers, although the economists didn't meet separately this year--there were several workshops on China this year. We even had workshops Wednesday afternoon and evening, before the beer party.

**BLAKE**: No, of course not. That was never my philosophy. I'm just delighted that they're interested in having all these things go on. I feel that they should feel they can change if they want to, that nobody's saying PAA's got to look like it did 50 years ago. It's their association now, the young people's association, and I think they should be doing what they think it's worthwhile to do. I would never want to be a person who said, "I don't like the way this is going." Because I think they should try those things and have the freedom to do that, and not have people in the older generation sitting on top of them saying, "I don't like the way this went this year," or, "I don't think it's convenient to have it that way."

I think it's great that there is enough good stuff to be in eight overlapping sessions. Not everything's great, but there's a lot of good stuff coming out of this field today, a lot of good research, and I'm feeling very positive toward that. And if they don't change things the way I'd like them, I won't feel I'm losing something. [Laughter] I think that they're taking things in hand and doing them and feeling that they can. I'm very encouraged by that.

**VDT**: Well, you're optimistic.

**BLAKE**: Oh, really?

**VDT**: Quite a lot of the old-timers regret the proliferation in sessions and numbers of people. A final question, about your sister Valerie [Oppenheimer]. How come the two of you chose demography? And, incidentally, I see that she was born in England; how did that come about?

**BLAKE**: Well, of course, I'm older.

**VDT**: Yes, you were born in New York, but you were both raised there, weren't you?

**BLAKE**: Yes. Well, I spent quite a bit of time in England as a child--when she was born in England.

I don't know how she came to choose it, really. I think she was surrounded by it, you might say, and found it very interesting.

**VDT**: You were in the field by the time she graduated?

**BLAKE**: Oh, yes. I'm six and a half years older than she is, so I was sort of well along by that time. And this was what everybody was talking about, so she kind of got into it too.

**VDT**: Ron Lee said that she was at Vassar and she babysat him--although he wasn't into demography at that time!

**BLAKE**: It was really funny. His mother was an anthropologist, a very distinguished anthropologist--Dorothy Lee.

**VDT**: He said both his parents were professors, but I didn't realize who she was. And his father was a mathematician?

**BLAKE**: I think that's what he was. His mother had been a widow for quite a while, and she reared all those kids as well. I'm not sure, but my impression was that she had quite a few children; she was quite a woman. It was a very, very smart family. Valerie was born in England. We lived in England for four or five years and my sister was born there.

**VDT**: Isn't it interesting that you both did turn up in the same field, and now, same university, same department?

**BLAKE**: I know. One reason I'm not more active in the department of sociology is that I have felt that that is basically her department, and I feel somewhat reluctant to get very involved. I don't get very involved on something unless she asks me to. Occasionally, I will do something that's neutral, like being on a recruitment committee or something, that the chair asks me to do. And I do help her students to do things. And we're personally close. I think part of this has to do with not making somebody feel crowded, as I think she would justifiably feel if I were in there with both feet all the time. So I've tried to be sensitive to that, because I do have a place down here [at the School of Public Health].

**VDT**: Well, it's an interesting combination; it probably exists in few other fields.

**BLAKE**: I know--unique. At least my child didn't do that.

**VDT**: I understand your daughter is a physician.

**BLAKE**: No, she's a cell biologist. She got her doctorate at Rockefeller University and she's now got a post-doc at Whitehead, at MIT.

**VDT**: John Weeks thought she had graduated in medicine.

**BLAKE**: No, Kingsley's son graduated in medicine. He's a psychiatrist.

**VDT**: I guess that was it.

**BLAKE**: But Laura isn't. And she's definitely not a demographer.

**VDT**: Well, she's obviously a super-achiever, like her mother. She's had a splendid role model--high-achieving, and yet a very womanly woman. Can I take a picture of you? You look great.

**BLAKE**: Oh, thank you.

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# A BIOGRAPHICAL ESSAY ON JUDITH BLAKE'S PROFESSIONAL CAREER AND SCHOLARSHIP

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KEY WORDS: social demography, population policy, family size, fertility, women sociologists, pronatalist policies



## ABSTRACT

Judith Blake was born and raised primarily in New York City; she received her BS degree magna cum laude from Columbia University in 1951 and her PhD in 1961. She had a remarkable and sustained record of scholarly contributions, which can be divided into five interrelated periods differentiated by a combination of substantive emphases, methodological appraoches, and time periods. Blake was the founding Chair of the Group in Demography at the University of California, Berkeley, which became the first demography department in the United States. She subsequently was the first incumbent of the Fred N. Bixby Chair in Population Policy at the School of Public Health at the University of California, Los Angeles, with a joint appointment in sociology. Blake served on numerous university and professional committees and boards and was elected President of the Population Association of America in 1981, the association's fiftieth anniversary. At the time of her death (1993), Blake was Editor of the *Annual Review of Sociology*. Judith Blake was a dynamic and creative researcher and teacher who left a strong legacy in her research, the students she taught, and the friends and colleagues she influenced.



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# INTRODUCTION

Wide-ranging intellectual rigor combined with passion, enthusiasm, and a concern with major contemporary problems characterized Judith Blake. It permeated her research, her teaching, her professional activities, and her interactions with colleagues. On May 4, 1989, Jean van der Tak interviewed Judith as a part of a series of interviews with past Presidents and Secretary-Treasurers of the Population Association of America. Jean observed that Judith's philosophy for research and writing craves "theoretical relevance and simplicity of conceptual framework.... [which] responds to lucid presentation of investigative problems"; Judith says it best:

> I've always been interested in what I felt to be major problems and not really that interested in just descriptive stuff.... I ... wanted to deal with what I felt were some critical problems in the field .... And ... I wanted to present them clearly if I could, and in an interesting and very lively way, so that people would have access to them.
>
> Also I always had as a goal that people should ... have as much enthusiasm about population as a field as I did. So I wanted to almost shake them and say, "You've got to realize that this can be very interesting and exciting and you should realize this deals with ... big problems, not just little mathematical sums or something. I want you to see that this is something that can be interesting and that you can get steamed up about." ... I would ... try to reach out and grab people and have them suddenly realize, "This is a very interesting thing." And when you read it, you say, "Gee, I didn't realize this, ... I suddenly see this a little differently from the way I did, and it's not that dull after all." (Demographic Destinies, 1991, p. 507)

Judith was never dull. The questions she asked were timely and controversial. The research she conducted was impeccable. The context within which she conducted her research and formulated her research questions was wide-ranging and as likely to draw on history and social anthropology as sociology, math, and economics. And her writing style was pertinent, lively, and accessible. But how and why did she get that way? Where did she get her enthusiasm and her seeming fearlessness in challenging established wisdom? We may never have a complete answer to that question, but after examining her research articles, the interview conducted by Jean van der Tak, the comments of her family, her colleagues, and her friends, I believe that Judith's unique ability both to see the "big picture" and to challenge current interpretations evolved—at least in part—from the unique circumstances of her childhood, the selection, timing, and location of her graduate work, and her experiences as a young wife and mother during a period when women were not readily welcomed into the ivy towers of academia. Rather than taking this as an excuse for failure or complaint, Judith viewed adversity as a challenge. When Judith came to a barrier in her personal life or her intellectual career, she confronted it head-on,

examined it from all conceivable angles, and sailed around it, over it, under it or through it, often redefining it in the process, not only for herself but for others. Judith was a master at breaking psychological set—that tendency that we all have to confront recurring situations with the same old bag of partially successful, but tired, solutions, rather than to consider new approaches that might ultimately be more successful.

In reviewing Judith's career I start with a brief biography of her life and then examine her research. Wherever possible I depend on Judith's own words and those of her colleagues and friends in describing who she was and what she thought.



## CHILDHOOD AND EDUCATION

Judith was born and raised primarily in New York City; she received her BS degree magna cum laude from Columbia University in 1951 and her PhD in 1961. But as her sister Valerie Oppenheimer points out, during Judith's childhood, it was hardly self-evident that she would go to college, let alone become a leading figure in American demography.

> [Judith] came from a family background that was not at all conducive to major academic achievements. In the Great Depression, our father's firm was sort of smallish, and it was ruined, thereby dealing a mortal blow to an already shaky marriage, and while I was still an infant [having been born in England], my mother went back home with us to New York City, where we all crammed into our grandmother's small apartment. Our father remained in California, and neither of us saw him again till we were adults. Hence Judith and I grew up in a three-generation, all-female household, with our mother, grandmother, and maiden aunt, in a state that used to be called "genteel poverty." The adults in our family were all traditional middle-class women with quite modest educational attainments, although reared in a tradition that fostered an appreciation of literature, music, and the arts. None of them evidenced any intellectual leanings that I ever noticed. They believed that ideally young women should marry and be properly supported by their husbands. In fact, I always suspected that in their eyes, the main function of a man in this world was to support a wife in the style to which she aspired.
>
> So how did such a family spawn two professionals? Well, among other things, events led to the recognition that for a variety of reasons, women might find themselves without male support. Here was a group of women, who through widowhood, divorce, and spinsterhood, had to support themselves but were ill-prepared to do so given their background. Hence, it was borne in on everyone that being able to paddle your own canoe, even if only temporarily, would have a decided advantage. However, I don't think any of the adults quite knew how to achieve this.
>
> Consistent with her background, my aunt's approach to the problem was that Judith should have a career as an actress and model, supervised by her aunt, of course. And therefore, Judith was dragged around to one producer's office or another for years, depriving her of a normal adolescence. Although she eventually escaped from this grind, the advantages of some kind of career were brought home

to us. Since she was also very bright, Judith rapidly saw the role of education in accomplishing this. This led to going to Columbia University, where she eventually landed in sociology and then social demography.

However, considering our background, the transformation of a budding ingenue to a professor in social demography was nothing short of a miracle. Judith's pioneering trail and the immediacy of it provided by close association were extremely important in my own academic journey. Judith was the only one in the family that quite seemed to have a head on her shoulders, and always had quite definite career goals—albeit frequently changing ones, but whichever they were at the time, they were definite—that she was working very hard to achieve. She provided me with a constant cafeteria of options to sample. Hence, I aspired to read everything she said was good, and at least thought of trying out every possible career she was considering. The results were not always satisfactory, to be sure; I read many of the literary classics before I was old enough to understand them, because she was my big sister. But that was all right, it led me to discover the section of the local library on science. [When] I ran out of the Thomas Hardy, I got [to the science section]. I positively loathed acting, acting and the whole theater and TV scene; I hated modeling with a passion; and early on realized that a literary career was not for me. Finally, Judith hit the jackpot, from my perspective, at least. She became involved with social demography, a field which I, along with most people in those days, had never even heard of. She naturally recommended I look into it, she always recommended I look into everything she did, especially since demography, early on, provided good career options for women. There were already several eminent women demographers. This was not an unimportant consideration in those days .... Meanwhile, I was just finishing up a bachelor's in anthropology, but getting, I must say, dreadfully tired of the constant menu of village life it offered up. Gradually there had been building up in me a burning desire to be able to say something about a society or country as a whole, and to go beyond the rubric of culture. Judith's encouragement to explore demography, plus Kingsley Davis's book on the population of India and Pakistan, opened up an entirely new intellectual world to me. It is one I've been happily exploring ever since. So, coming from a family background where the sciences, social or natural, were part of the great unknown, Judith managed to carve herself a highly distinguished career, and by her pioneering efforts, also showed me the way to a field with intellectual rewards I treasure today. (Valerie Oppenheimer, *Memorial Service for Judith Blake,* 1993)

Judith's own description of how and why she selected social demography confirms, complements, and expands Valerie's observations.

I guess I was really looking for a sub-field of sociology that was empirical and very broad in its disciplinary scope, international, and that would have an influence on world affairs. I wasn't interested in voting and political participation, for example, which was the focus of most survey research at Columbia. This was Paul Lazarsfeld's focus, really his main interest. Although I was interested in the survey side, I wasn't interested in the voting side. And I wasn't a Marxist. There was a very strong, very distinguished, Marxist tradition at Columbia, with Robert Lynd and Herbert Marcuse and all of that. So that was a whole contingent that was kind of ruled out for me.

So for a while, I was really at sea. Then, strangely enough, I was walking along

Amsterdam Avenue one day and met Alice Taylor, who became Alice Taylor Day, ultimately, but was then just plain Alice Taylor, and told her, "Alice, I'm just not quite focusing in this place and I don't know what to do." Alice was always a dear, sweet person and she said, "Oh, that's no problem; you ought to take a course in demography. I know you're going to be crazy about it; it's just for you." [Judith went straight to graduate school at Columbia after finishing her undergraduate degree.] I was a New Yorker and I really didn't know there was anyplace else; it never occurred to me to go elsewhere. Also I didn't have much money. And it wasn't so dumb, because I knew the territory there, and for me to have uprooted myself to go someplace else without any financial backing or anything would have been a little foolhardy. So I stayed.

One of the things that had happened was I was sort of burned out, because I had taken all these graduate courses for undergraduate credit and by the time I was a graduate student, there really wasn't much else to take. And I had kind of worked my way through a series of enthusiasms which I then worked my way out of, and I was just beginning to feel, "What am I doing here?"

Alice said, "The course is just starting up." So I registered. The course was being taught by Hope Eldridge, who was a wonderful person ....

I've asked myself this over time: What was it that just knocked me for a loop? I think partly it was there were so many data. Now this may not sound important to you, but at the time, generating information was not easy. Sociology had just survived the throes—and was really in the throes—of community studies, studies that were mainly asking how many social classes could you find in a community, and some people found six and some found eight and some found twelve. But fundamentally, generating data, information about whether things were true or not true and whatever, wasn't easy. It was really a tough proposition to process those data as well.

So here was a field where you were awash in data; the stuff was just pouring out. That was wonderful. In the course, you were set to work on lab problems where you were actually looking at census data and manipulating them and there they were for any country you could think of. It was just terrific! Also, you could do comparative analyses and I liked that. I liked the idea of looking at a lot of different countries and comparing.

And surveys also seemed to me to be very applicable. As I started thinking about demographic issues, I got beyond the data side very rapidly and started thinking about why things were happening. I started to realize that the training I'd had in surveys should be very applicable, because what people were asking were analytical questions and with censuses it was hard to zero in on those. By and large, censuses were legal documents and you were very limited as to what you could ask, and vital registration data were the same. So I moved rapidly in my own mind from thinking, "This is a field where you're awash in data," to thinking, "This is a field where you're awash in data, but none of them are quite right for what you're asking, what you're interested in." They were suggestive and you would do these analyses, and then you would be left with kind of an empty pot, because the real questions you wanted to ask you didn't have any control over.

So at the time, I thought that doing surveys was a way of amalgamating training I had had with what I conceived to be the new demography. And I got really enthusiastic about that. So I stayed in the course for the semester and was making very rapid strides and thinking about what I would be interested in doing. Then

> Kingsley came back from Africa and gave the second half of the course. He was full of all of the international comparative stuff and what had been going on in Africa. That just blew me away, I mean the fact that you would be going to the Dark continent and seeing all these things; it was a period of enormous change and colonial problems and so forth.
>
> I got a job at the Bureau for Applied Social Research, which was run by Lazarsfeld and Kingsley and Robert Merton and that crowd. I got a job on the cities' project, which was being done for the Air Force. This project was looking at census data on cities, aggregate data. It didn't take me long to realize that I didn't like dealing solely with very aggregated data. Then I thought, "Oh dear, what am I doing in demography, because demography is mainly aggregated data?" and I was beginning to worry again that perhaps I was in the wrong spot.
>
> Then this Jamaica project came along. The Conservation Foundation funded it and they were interested in the birth rate and family structure and so forth in Jamaica. I just flipped out; I thought this was a terrific idea. Actually, it ended up being the third survey of the topic in a developing country. The first had been Paul Hatt's in Puerto Rico and the second was the one Joe Stycos was doing in Puerto Rico; Joe Stycos had worked with Hatt. Joe was going to be the project leader and I was going to be his sidekick and we were going to go down and do this project in Jamaica. You have to realize we weren't very old; maybe Joe was 27 and I was 26, something like that. (*Demographic Destinies,* 1991, pp. 499–500)

Judith's reminiscences about her graduate education and her introduction to demography set the stage for many of the themes that later would be developed in her research and teaching. Before selecting demography as an area of concentration, Judith already had completed most of the available coursework in the department, which included exposure to some of the major sociological theorists and methodologists of the period. The study on cities, referenced above, resulted in Judith's first publication, a monograph for the Air Force (Foley et al 1953). The Jamaican study formed the basis of her dissertation, which resulted in the sole-authored book *Family Structure in Jamaica: The Social Context of Reproduction* (1961). One of a series of classic studies conducted by demographers during the 1950s, Judith's Jamaican research was one of the first to emphasize the social structural context in which fertility occurs—but let Judith describe it in her own words:

> We went ... to Jamaica and found ... a not overly hospitable environment, which was mainly dominated by anthropologists, who felt that doing a survey in Jamaica was about as loony as you could get .... We didn't pay too much attention to this. Joe is an incredible field person .... And we had the 1953 census we could use as a sampling frame .... So we went out and got ourselves a sample and did all the right things. We had our schedule ready and we went to the Census Bureau and other agencies to get trained interviewers, whom Joe trained some more, with great skill, and went into the field and did the survey.
>
> Those surveys—the Hatt, the Stycos and Hill and Back, and the Jamaican studies—I think really laid the groundwork for people no longer thinking it was loony to do surveys in these countries .... which were much less interested in the causes of fertility behavior, which was our focus, and much more sort of descrip-

> tive knowledge—attitudes and practice (of birth control) types of instruments. [We looked at socioeconomic background], family structure background and incentives and disincentives—what we considered the big questions ... (*Demographic Destinies,* 1991, p. 500)

## RESEARCH CAREER

Judith Blake had a remarkable and sustained record of scholarly contributions, which can be divided into five interrelated periods differentiated by a combination of substantive emphases, methodological approaches, and time periods. Her early career was characterized by research that was heavily theoretical in orientation, with most of the articles addressing major policy issues and the future of social demography. Much of her research adopted a historical perspective and, with the exception of the Jamaican study, did not involve data collection or analysis. When data were collected and analyzed, the methodology used combined social anthropology with survey research techniques. The methodology of Judith's later research combined secondary analysis of existent data sets and data obtained from questions commissioned by Judith as part of ongoing national surveys such as the Gallup Poll. It was in the design and implementation of these later studies that Judith hit her stride as a social demographer who was frequently ahead of her time and who rarely accepted popular or conveniently available explanations of population trends and processes.



### *Early Research, 1951–1964*

The Jamaican study (Stycos & Blake 1954, Blake 1955, Blake 1961) had as one of its goals explaining why Jamaica's birth rate was lower than Puerto Rico's (40 vs. 33) given the greater availability of family planning clinics in Puerto Rico. Data were collected through 100 detailed and highly qualitative interviews with females and about 50 interviews with males selected to represent lower income families. The primary objective was to add to a growing body of scientific knowledge concerning institutional factors and concomitant attitudes relating to fertility in different cultures.... It is therefore in the study of family relationships and the attitudes which arise in and lend support to these relationships that one must begin (Stycos & Blake 1954, p. 349). Judith and her colleagues found that:

> This emphasis on biological paternity, a recurrent theme in the spontaneous replies of our respondents, was not anticipated by the investigators .... We assumed instead that the pater (the sociological father) would be emphasized as it is in many cultures where plural mating and/or matrilineal descent are the norm. In addition, we did not expect that the motivation of the woman would be so explicitly centered on meeting the desires of the man, that the male's desires would be so dominant. (Blake 1955, p. 32)

Judith's important research contributions predated the completion of the Jamaican study, which also served as her dissertation. In 1956, she wrote (with Kingsley Davis) "Social Structure and Fertility: An Analytic Framework," which provided a framework within which much of the social demographic research on fertility continues to this day:

> The present paper represents an attempt to set forth and utilize an analytical framework for the comparative sociology of fertility. It first presents a classification of the intermediate variables through which any social factors influencing the level of fertility must operate. It next tries to show, in broad outline, how some types and elements of social organization, acting through these variables, appear to enhance or depress societal fertility. Our hope is that as more sociological and demographic information becomes available, the theories advanced can be refined further and tested empirically. (Davis & Blake 1956, p. 211)

The paper was originally presented in a session on "Foreign Studies" at the 1955 Annual Meeting of the Population Association of America (PAA). This was only the second PAA meeting that Judith attended. When asked by Jean van der Tak how people reacted at the time, Judith said:

> There was a big reaction all right. Some of it was very controversial ... But when the paper finally came out, it caught on very rapidly, because it solved a very basic problem of how to look at things .... (*Demographic Destinies*, 1991, p. 513)

Although Judith appreciated the impact that this classic paper had on social demography, she was not sure by 1989 that the original ideas were being applied in the ways intended by the authors. Judith was particularly concerned with the way in which the analytical framework had been applied to the World Fertility Surveys (WFS).

> The whole point of the article was to say, "If you want to look at sociocultural influences on fertility, then you have to be aware of what they operate through and that these are the variables they operate through." ... [N]obody would ... have accused us of ... just look[ing] at those variables.
>
> [But] it ends up that something like that provides people with a very mechanical way of looking at things. It's unfortunate .... I don't think they could have done it differently if they wanted to do something at that level. But it's a problem, because I don't think that survey has led to eye-popping theoretical results; we just have an enormous amount of data. (*Demographic Destinies,* 1991, p. 506)

Judith coauthored three other articles with Kingsley Davis during this period. The first paper represented Judith's first published analysis of the divergence between official statements on US contraceptive practices by the Catholic hierarchy and the attitudes and behaviors of Catholic laity (Davis & Blake 1960). This theme would be picked up repeatedly by Judith in later articles (e.g. Blake 1966, Blake 1984).

The second article (Blake & Davis 1963) outlined a research agenda for

social demography that largely operationalized the analytical framework proposed in the 1956 article. Research questions were posed in the areas of population size, migration, marriage and the family, reproduction, labor force, mortality, and population policy.

> [P]opulation trends are the products of human behavior, and human behavior involves motivation....
>
> It would appear then that attitudes and motives are not simply relevant to population study, but essential. They are essential to understanding the causation and possible modification of population trends, as well as to understanding the social and economic consequences of such trends. So long as population is studied apart from the analysis of goals and motives, it is studied inadequately. (Blake & Davis 1963, pp. 24–25)

The third article was a chapter on "Norms, Values and Sanctions" which appeared in REL Faris's *Handbook of Modern Sociology* in 1964. Here the focus was less on social demography per se and more on a review of the pertinent sociological and anthropological literature on the topic. The authors did not, however, recoil from challenging the perspectives of the major sociological lions of the day, including Parsons, Wrong, Dahrendorf, and Mills. While Parsons-bashing was quite prevalent during this period, Davis & Blake's critique of the conflict theorists was perhaps a first. They succinctly suggested that in their eagerness to critique the Parsons school, these writers confused norms with "good" and failed to realize that there are norms which support "bad" as well (Davis & Blake 1964, pp. 466–468).

## *Using Secondary Data to Address Major Contemporary Policy Questions, 1965–1970*

For all practical intents and purposes, the chapter in the Faris *Handbook* represents the last published collaboration between Kingsley Davis and Judith Blake. Starting with a publication in the *Journal of Chronic Diseases* in November 1965, Judith established a format for her articles that was largely followed in all of her research articles until her death, namely, to identify a contemporary population policy issue, review recent political statements and existent research pertinent to the questions raised, and utilize available secondary or national US data sets in examining the questions posed. Her findings frequently, if not always, challenged or contradicted conventional wisdom on the topic in question. From this time until her death she examined American attitudes toward abortion, childlessness, the status of women, federal family planning programs targeted to poor women and teenagers, the only child, and the impact of family size on the quality of children. In "Demographic Science and the Redirection of Population Policy," Judith criticizes the two then-prevalent views that population increase will, on the one hand, decline only in the context of advanced economic and social development and, on the other hand,

decline solely in response to targeted education and communication about birth control (e.g. family planning). In this article, she used available data on European countries both to point out that much can be learned from a study of intended, expected, and completed fertility in European and European-frontier countries and to demonstrate that birth rates did *not* uniformly decline in Europe as a result of industrialization and urbanization but, rather, such declines often preceded the industrial revolution in a geographic region.

In this and succeeding articles, Judith repeatedly makes several points: First, that many explanations for population trends—particularly those that depend on a singular explanation—are far too simplistic and often ahistorical in their simplicity. Second, she argues persuasively for the relevance of studying the population experiences of developed nations in order to better understand and apply data to underdeveloped areas of the world. Third, she describes what can be learned from such studies. Fourth, she repeatedly demonstrates how pronatalist American society and institutions remained. And fifth, she sets the groundwork for what would become another series of classic articles in the field: namely, that economic theories provided insufficient explanations for population shifts.

In a series of articles (Blake 1966a,b), Judith repeatedly demonstrated that between 1930 and 1965 Americans had consistently intended to have 3–4 children *regardless of the number they actually had and regardless of the demographic subgroup with which they were affiliated!*

> No major group in the population wants families small enough to bring about a cessation of population growth even if all other groups were to become similar to it. It is thus unrealistic to believe that any major social grouping constitutes a model for the small family ideal in the United States today. (Blake 1967b, p. 204)

While Catholics generally had larger completed families than did non-Catholics, their expectations regarding family size were more similar to those of non-Catholics than to those of the Catholic hierarchy. Contrary to the expectations of economists, family size did not vary directly with income but did vary inversely with education. Contrary to popular thought of the day (US Senate Subcommittee on Foreign Aid Expenditures 1965, National Academy of Science 1965, US Government Printing Office 1968), Judith succinctly demonstrated that there were not 5 million lower class women who were in need of family planning services! Rather there were perhaps 1.6 million women whose actual completed family size exceeded their desired family size (Blake & Das Gupta 1972).

> Why then is it frequently assumed that there is a numerically important fertility "excess" among the grade-school-educated—an "excess" that more adequate birth-control information and distribution could eliminate? In part, this assumption may be due to a definition of "excess" fertility according to the ideals of the highly-educated, or it may reflect the feeling that fertility is excessive when it has

> nuisance value for the society at large. Relatively large families among the grade-school educated exact public costs through welfare payments, special educational requirements, anti-poverty programmes, etc. The question arises, therefore, whether there is any important sub-group among the lower-educated that does reproduce significantly in excess of its ideals ... It is clear that the range of variation in family size within the grade-school group is far greater than the difference between this group as a whole and any other educational level. Those having no education and one to four years of grade school typically have families close to five children on the average .... [Available data suggest that some number of these women have more children than they desire.] Thus ... it may well be that these women constitute a genuine pocket of fertility excess .... Therefore, although these extremely deprived groups doubtless create a disproportionate share of social difficulty, they are not numerically significant enough to affect the overall grade-school average even if one allows that they are motivated to reduce their family size by one-half to one child on the average.
>
> We must conclude, therefore, that the prospects for narrowing or reversing the inverse family-size differential by education do not seem auspicious if one is hoping to accomplish this end by influencing the behaviour of a major target group with relatively little education.... (Blake 1967a, pp. 173–174)

This series of articles culminated, at least in part, with what subsequently became a classic article, "Are Babies Consumer Durables? A Critique of the Economic Theory of Reproductive Motivation," where Judith concluded:

> Becker's neglect of the social context of reproduction is most evident in four features of his analysis: the analogy of children with consumer durables; the concentration on the "consuming" as against the "producing" role of parents with respect to children; the misapprehension of child costs; and the failure to analyze the utilities involved in having children. (Blake 1968, p. 15)

Ron Lee describes the impact and importance of the article:

> [L]ike her teaching, her research was very forceful, very active, penetrating, masterful. Just as the hour went by in no time at all when you were sitting in her class, so when you read an article of hers, well, it's a very good read, it pulls you along. I remember in particular one article she wrote while I was a graduate student at Berkeley. It was an indignant reaction to work just beginning to be done in the economics of fertility, a paper by Gary Becker.... that article was very critical, in some ways a devastating attack on that approach. But I recently used this article again in a class—some 20 years since it was published—and I realized as I re-read it that, in fact, in her critique she had sketched out much of the research agenda that economic demographers and economists working on fertility followed in the last twenty years. Many of the things she discussed, like putting biology and sociology back into fertility theory and that babies couldn't be purchased at the market have been accepted and studied. It was a very seminal piece and also very lively. (Ron Lee, *Memorial Service for Judith Blake,* June 6, 1993)

## *Attitudes Toward Abortion, 1970–1980*

Judith's interest in understanding institutionalized pronatalist population policy and what differentiated persons who had large families from those who had small ones in combination with the ferment resulting from the advent of the

birth control pill and Roe vs. Wade resulted in Judith's taking a hard look at the American citizenry's attitudes toward abortion and the extent to which the perceptions of national "opinion leaders" failed to reflect those of the general population. The result was a series of articles during the 1970s (Blake 1969, 1971a,b, Blake 1972a, 1977a, 1979) that culminated in the realization that fundamentalist religious groups would ultimately mount a substantial backlash to Roe vs. Wade. Judith's early training at Columbia in survey research and questionnaire design was a pivotal part of the methodological expertise that she brought to bear on this topic during this period.

> In Western countries as well as elsewhere the history of population policy has, with few exceptions, been a chronicle of government efforts to repress birth limitations and reward reproduction.... In most of the United States, state laws on abortion constitute some of the more repressive of our pronatalist policies .... What are the chances of fundamental changes in state laws to remove the legal ban on most kinds of abortion? ...
>
> To answer these questions, I shall analyze the differences and changes in views on abortion among white Americans during the past decade. (Blake 1971a, p. 540)

Judith quickly found an answer to the question she posed.

> Our examination in this article of the opinions of various groups in the population on the legalization of abortion contradicts the conclusions usually drawn by those who argue on a priori ideological grounds that certain groups *should* support legalized abortion in the United States. According to the latter, abortion should be supported most strongly by the less advantaged and by women. Clearly this is not the case. Legalized abortion is supported most strongly by the non-Catholic, male, well-educated "establishment." I have explained this finding in terms of the occupational and familial roles that such men play, in contrast with the roles performed by women in their own class, and by men and women in classes beneath them.
>
> We may conclude, therefore, that changes in abortion laws, like most social changes, will not come about by agitation at the grass roots level, or by the activity of righteously indignant individuals who cannot currently circumvent existing statutes. Rather, it is to the educated and influential that we must look for effecting rapid legislative change in spite of conservative opinions among important subgroups such as the lower classes and women.... (Blake 1971a, p. 548)

In 1971, Judith felt that a powerful elitist minority would be able to effect change in abortion policy at the national level in spite of the fact that 80% of the population disapproved of elective abortion, but by 1973 she was already beginning to sense that a backlash was inevitable. She based much of her reasoning on the realization that much of the rationale for believing that the American populace was in favor of abortion was based on answers to a single question commissioned as part of a Gallup Poll by Planned Parenthood:

> As you may have heard, in the last few years a number of states have liberalized their abortion laws. To what extent do you agree or disagree with the following

statement regarding abortion: The decision to have an abortion should be made solely by a woman and her doctor.

Judith argued that both the wording of the question and its existence as a single question resulted in biased data that failed to tap the full range of attitudes toward abortion (see Blake 1973). To demonstrate her point, she inserted a different question in the August 1972 Gallup survey: "Do you think it should be lawful for a woman to be able to get an abortion without her husband's consent?" Whereas 64% of the Gallup sample answered the first question affirmatively in June 1972, 67% answered the second question negatively. In September 1972, Judith inserted a second question: "Do you believe that there should be no legal restraints on getting an abortion—that is, if a woman wants one she need only consult her doctor, or do you believe that the law should specify what kinds of circumstances justify abortion?" Only 39% of the sample responded that there should be no legal restraints on abortion—quite a drop from the 67% cited by Pomeroy & Landman (1972)!

By 1976 Judith demonstrated that almost half the population favored a constitutional amendment that would prohibit abortions except when a pregnant woman's life was in danger (Blake 1977a) and warned that "If decisions (to legalize abortion] are to be fully implemented on a continuous basis, local organized efforts at collateral deterrence obviously will require equally organized vigilance by those favoring the Court's decisions" (Blake 1977b, p. 53). Judith published her last two papers on abortion in 1980 and 1981 (Blake & Del Pinal 1980a, 1981), leaving the area largely disenchanted. "I quit working on it, because I saw such disaster ahead and I didn't want to predict it anymore. I didn't want to be the person who was saying disaster is coming down the pike, because my pro-abortion predilection was obvious. I felt so overwhelmed by what I saw coming" (*Demographic Destinies,* p. 508).

## *The Pronatalist Stance of American Institutions and Policy, 1960–1993*

Judith's appreciation of the extent to which American society was pronatalist and her ability to verbalize this societal attitude succinctly and pointedly expanded throughout the 1960s and 1970s. Unlike most other sociologists of the period, Judith was not afraid to challenge publicly some of the politically correct sacred cows of the period. One of the particular ironies that she recognized was the essentially pronatalist position of the women's movement. As only Judith could, she succinctly noted in an article on the population crisis

In all of our deliberations it is most important to remember that people do not have birthrates. They have children.... Individuals' willingness to bear children, to expend their human and material resources creating families, cannot be taken for granted. Rather childbearing and child rearing take place in an institutional

context that strongly influences people to do one set of things, reproduce, and not to do another set, activities that compete or conflict with reproduction ...

Perhaps most anachronistic is the pronatalism of our current women's liberation movement. It is often assumed that the present-day women's liberation movement is essentially antinatalist in ideology, and that its effect will be antinatalist as well. Actually, however, the main thrust of the movement is supportive of motherhood for all. [Instead] what is decried is the relative disadvantage that women experience because of childbearing and child rearing. In effect, women's liberation is concerned with lowering the exclusionary barriers for women in the labor force, opening up educational channels, elevating women's awareness of subtle forms of discrimination against them in the outside world, and supporting women's rights to have families as well. Rather than concerning itself with the atypical spinster or childless woman, the movement has gained popularity and a constituency through its recognition of the problems of women who have already made the choice to be mothers, and who are dissatisfied with their impaired occupational chances, or find motherhood less than they expected it to be and wish to switch gears .... Consequently, far from questioning the basic premise that all women should be mothers or, for that matter, that all men should be fathers, the women's liberation movement sets the goal of reproduction for all as a basic good. Childlessness is regarded as an inherent deprivation for all, rather than a socially induced deprivation for some.... [T]he women's liberation movement parts company with antinatalists by failing to recognize that it is not in society's interest to encourage the emergence of a family form in which neither parent is committed to parenthood.... (Blake 1972a, pp. 60, 65–66).

As part of a chapter in Parke & Westoff (Blake 1972b), Judith further developed this theme.

I believe it can be shown not only that there is, in American society, an absence of legitimate alternatives to sex roles having parenthood as a primacy focus, but that change is particularly difficult to effect because those individuals who might aspire to such alternatives are suppressed and neutralized. My thesis is that unless we realize that we have been locking pronatalism into both the structure of society and the structure of personality, the problem of fertility control will appear to be the reverse of what it actually is. We will continue to believe that our principal policy problem is one of instituting anti-natalist coercions instead of lifting pronatalist ones. We will see fertility reduction as involving more regimentation than presently exists, when, in fact, it should involve less, since individuals will no longer be universally constrained to forsake other possible interests and goals in order to devote themselves to the reproductive function. (Blake 1972b, p. 86).

## *Effects of Sibsize on Achievement and Personality*

Although it was often assumed, and Judith herself occasionally suggested, that her interest in the one-child family evolved out of the fact that she herself had a single biological child, in fact Judith's interest in the effect of sibsize on adult achievement and personality was a natural progression from her interest in attempting to understand what motivated people to have children at all and what predicted the number of children desired and had. Whereas her research

during the 1960s treated desired, expected, and actual family size as dependent variables, her research during the last fifteen years of her life examined family size as an independent variable or mediating variable between various socio-demographic characteristics of people and the characteristics of individuals. According to Jean van der Tak's interview with Judith:

> [My] interest in the influence of family size on achievement was actually motivated much more by an interest in the effects of high fertility. And other people have just been so interested in the only child that this is all that I can ever somehow get anybody to ask the questions about .... (*Demographic Destinies,* 1991, p. 509).



An article published in the *Journal of Marriage and the Family* in 1979 provides the first evidence that Judith was turning her attention to the no-child and one-child families.

> As a proponent of the thesis that raising the costs of children, including opportunity costs, will lower fertility, I would join in the expectation that augmented labor-force participation [by women] will have this effect .... This investigation was conducted... to determine whether or not children are, indeed, regarded as *social investment goods* in American society, and whether or not people believe that other types of *consumption goods* are more attractive than children.... There is a high level of consensus that nonparenthood is not an advantaged status and, although offspring are not regarded as economic investments, they are viewed as being socially instrumental—not solely as consumption goods. (Blake 1979, pp. 246, 247, 255)

In describing the research of Blake & Del Pinal (1980a,) Hendershot & Placek (1980) noted that:

> ... [T]he major perceived costs of children are the direct costs of parents' time, money, and effort; the major perceived benefits of children are as a social investment—giving their parents a more recognized and secure social role—and the intrinsic rewards of interacting directly with children. Although nonparents saw fewer benefits and more costs than parents, they did not give strong support to childlessness—a majority of nonparents thought the benefits of children were equal to or greater than their costs. Thus, even to the limited extent that it exists in America today, the preference for childlessness does not appear rooted in strong antinatalist sentiment. (Hendershot & Placek 1980, p. 235)

Blake & Del Pinal conclude that "... people are most likely to see reproduction as being socially instrumental when their alternative means for achieving social goals are the most limited. (Blake & Del Pinal 1980b, p. 249)

Starting in 1985, Judith's research focused almost exclusively on the effects of sibsize, with the various analyses reported in articles providing the background and preliminary work for what eventually became only her second book, *Family Size and Achievement* (1989b) or its sequelae (Blake 1981a, 1985, 1986, 1989a, 1991).

Judith's first research in this area built on the work of William Sewell and

colleagues, focusing in specific on how sibsize mediated the effect that father's education had on son's schooling (Blake 1985, 1986). Using a series of national data sets, she went on to expand her analyses to look at variables such as verbal and cognitive ability, at socio-demographically described subgroups, and at women. In all instances, Judith found that children from small families fared better on all measures considered. The single consistently occurring confounding variable was the fact that children with no siblings had a higher likelihood that their parents would divorce, and the experience of being raised in such a family tended to offset the relative advantage of being a single child.

Judith's last research, which postdated *Family Size and Achievement,* conducted under grants from the National Science Foundation and the National Institute of Mental Health, examined how sibsize influenced personality characteristics. Contrary to conventional expectations Judith found that:

> ... [The] number of siblings is not strongly related to sociability even among men, and it is not at all related among women.... The definitions of need for affiliation and sociability ... embody extremely attractive characteristics, among them warmth, love, cooperation, friendliness, and reciprocity. We would all like to know what makes people sociable. Except for gender effects, we may never find out, but at least there is some value in knowing that lack of sociability does not appear to be the price exacted for the intellectual advantages of small families. (Blake et al 1991, p. 280)

Although it is technically incorrect to consider *Family Size and Achievement* the culmination of Judith's career as a researcher, many of the questions addressed were originally expressed in earlier articles. Certainly the book's themes continued to express Judith's concern with what she perceived to be a strongly pronatalist society in which successful realization of zero population growth would be far more complex and difficult than originally thought. Whether it be abortion or the institutionalization of small or no-child families, these analyses and Judith's presentation of these analyses continued to point out that child-bearing decisions are made within a web of familial relationships and motivations within a societal and institutional culture. The ramifications of these decisions have relevance for both the individual and the society.

*Family Size and Achievement* was awarded the William J. Goode Award by the American Sociological Association in 1990.

## CAREER POSITIONS

Despite her extensive early research accomplishments, like many women of her generation, Judith Blake experienced the nepotism common to academic institutions when she first moved to Berkeley in 1955. Because her then-husband, Kingsley Davis, was on the faculty of the University of California, Berkeley, Judith took a series of positions as Lecturer, first for the School of

Nursing at the University of California, San Francisco (1957–1959) and later for the Departments of Sociology (1957) and Speech (1961–1962) at UC Berkeley. Judith herself described this period of her life in her interview with Jean van der Tak.

> It changed my life a lot to move to Berkeley.... [I] went into a situation at Berkeley that was very alien for me ... not only because it was sort of a suburb [of San Francisco], but the university itself was ... very chauvinist[ic]; there were practically no women on the faculty and one had the feeling that they didn't want any either.... The chairman of the political science department said that a woman would never get a job on his faculty as long as he was chair. People didn't mind saying things like that. [I] felt [I was] totally out of the scholarly community, because I was no longer a student ... and I wasn't anything else ....
>
> I felt it was to my advantage to take lectureships wherever I could so I could get teaching experience and say I had some teaching. I didn't want this long hiatus to develop in my career while I was doing nothing but writing my dissertation. I was very worried about that. I had to have faith that there was going to be a future, which took a lot of faith in the 1950s, because it was not the time when women were doing this sort of thing. I kept saying to myself, "Just take it a step at a time and if somebody offers you a lectureship or this or that, take it. Then take the next step." It wasn't one of these things where you felt, "There's a career out here for me somewhere." You just felt you were going to have to see how it worked out and hope that things would improve.
>
> That's not such a bad philosophy, actually, because life is incremental and I took those jobs and got some experience. I'm sure I was dreadful to begin with and those poor nurses [at the School of Nursing, University of California at San Francisco] probably suffered through quite a few years of boring teaching. But I would have had to do that somewhere. They seemed very appreciative, though. So anytime I got a chance to do something like this I took it, and I gradually got to be, I think, probably a much better lecturer. (*Demographic Destinies,* 1991, pp. 500–501)

## *Graduate Program in Demography*

In 1962 Judith was appointed as an Acting Assistant Professor of Demography in the University of California at Berkeley's School of Public Health, where she quickly advanced to the rank of Professor. A condition of her initial appointment was to develop a program in demography, and in 1965 Judith became the founding Chair of the Group in Demography and a Professor in the subsequently formed Department of Demography, the first such department in the United States. Between 1967 and 1971, a substantial number of the demographers currently working in the United States and abroad were trained in the Department, largely through Judith's efforts. The Department was unable to survive the student uprisings of the early 1970s but was re-established in the late 1970s. Judith described this period in her interview with Jean van der Tak.

> ...Jacob Yerushalmy in the School of Public Health was very interested in demography. He felt that something should be done at Berkeley in demography. So

he talked to Kingsley about it and then they talked to me about it. I thought that was fantastic. Again, I figured you take it one step at a time. Yerushalmy gave me a job and it was to try to start a group, which was the vehicle that Berkeley had at the time for establishing academic programs. The idea was that you would bring people together from all kinds of departments who had genuine jobs in those departments but who were interested in a sub-field like this one and they would form a group and put a curriculum together and get a degree program started. So that eventually was what I started working on.

We got [the Group in Demography] established [in 1965] and then it was clear that we didn't have any say over people's time or anything. I mean, we were accepting [graduate] students, but you get students and then all of sudden you realize that when people want to go on sabbatical or something like that, they just go and you're not able to say, "Well, this is not a convenient time for us this year. We're accepting x number of students, something has to be done with them, there have to be courses." ... So it wasn't long before it was clear that this was going to be an economically non-viable situation. And it was pretty scary because we had students on deck but you couldn't see faculty on deck, at least on a continuous basis. So we went to [Chancellor] Roger Heyns and he said, "Well, there's really nothing else you can do but form a department." ...

So the department got established [in 1967] ... with three faculty positions and never had more. I was sort of beating the bushes, raising money, all the time .... I could keep it patched together on ...[extramural) money, but it was wild, because we kept getting all these students, large numbers of students, and with the doctoral program we had to keep trying to gin up these courses for them to take, people to supervise their dissertations, and one thing and another ....[W]e started an undergraduate set of courses.... They were very popular. This was the time 1965 to 70, of enormous concern for population and younger kids were all excited about this, so you'd have as many as 200 students in an undergraduate course. It was incredible. We even had an undergraduate major that we graduated some people in.

So it looked as if it was going to be a success—in part, I think, because of the great concern for population in the United States.

But, in time, there was an awful lot of political activity on the Berkeley campus, and by the 1970 cohort, I guess, we had many students who were not only interested in population but were very political. They were very concerned about Vietnam—rightly so, of course. We became—it was a very little, obviously not very strong department—we became a big focal point for this activity.... and part of my problem was that I really felt that all one should be concerned about was demography in the university—I mean, that we shouldn't be political. This itself, I think, probably was a very political thing, in the sense of saying, "Well, I don't think a department should go political." But people who thought it should be political felt that I was being very difficult about this ....

Roger Heyns, who had been very supportive of us, left and Bowker came in as the new chancellor and he was importuned very strongly by the students .... So Bowker's feeling was, "Well, here's a set of faculty positions that I can get hold of and I have no commitment to demography; this wasn't my idea to create a department of demography." ... Meanwhile ... people left and really made it possible [for Bowker to close the department.] .... And Kingsley and I were left with a whole big bunch of students to see through dissertations. ... So I went through a period—and Kingsley went through a period—of getting these people

through their dissertations and out into the world. Which was a very strenuous period, and was, I felt, a very difficult way of going about things.... (*Demographic Destinies,* 1991, p. 501–502)

Judith described her view of what a department of demography should look like in an article in *Demography* in 1964 where she argued that demography "like other scientific disciplines... has an internal dynamic.... [Its] scope is influenced by the changing (in this case, growing) significance of its subject-matter for world affairs and by the augmented applicability of demographic knowledge and techniques.... As an essentially quantitative discipline, the study of population involves application of mathematics, probability theory and statistics... [with] important connections [to] biology, economics, geography, history, mathematics, statistics [and] sociology" (Blake 1964, p. 258). Judith felt strongly that it was restrictive for demography programs to be housed in other departments, including departments of sociology.

[O]ver the years, there have been ... groups [that] incorporated... usually about three [different disciplines]. I think it would be a good idea to have that kind of fermentation, because the field I don't think has ever really seen its whole development, especially along biological lines and lines of that sort. It's been very heavily a social science field. I think it would be a very good thing for the field to have more input from, for example .... the sociobiologists, who are out there doing their thing, which in some ways is very demographic .... [There is a] whole group of people who really would benefit—and *we* would benefit—from interaction. Now, I don't agree with them, but that's irrelevant. What's relevant is that they're really thinking very demographically, and it's almost as if they don't know it.

I think the field of demography is very broad. I never agreed at all with Keyfitz' notion that it was just a kind of simple-minded mathematics or something—something that was pretty low-level. I have always felt that it was a field that impinged upon so many areas and that one could, if one could ever realize this in training, have people who brought a demographic perspective to a lot of other fields, which would make people realize what a vital and fascinating perspective this is. I think this may yet happen as time goes by, because we're getting a lot more demographers—a lot of them trained by Berkeley, I'll have to say. We trained an *incredible* number of people in the amount of time we were there. (*Demographic Destinies,* 1991, p. 504)

In the early 1960s, when Judith both outlined and operationalized her ideas about what a program in demography should include and where it should be housed, there were no departments of demography and only the Universities of Chicago and Pennsylvania had a formal concentration in demography. Ron Lee describes what the program at Berkeley looked like from a student's perspective:

Back in the early 1960s, Judith singlehandedly created the program in demography at Berkeley. She invented it, she gave it a structure, she got outside funding for it, she directed it and led the program, and she taught in it. Perhaps it was the

first degree-granting program in demography in the country, certainly it was one of the very few, early programs. And over the years when she was there, it produced a very large generation of demographers contributing to society in many different ways .... I was a student there and I remember how the atmosphere in the program was charged with excitement, and crackling high spirits. There was a constant flow of visitors from throughout the United States and abroad giving seminars. The seminars were very lively affairs. (Ronald Lee, *Memorial Service for Judith Blake,* 1993)

Tom Espenshade went to Berkeley as a post-doc in August of 1970. He reminisces about his experiences at Berkeley under Judith's guidance.

I remember the first time I met Judith Blake. I was totally unprepared for the experience. Sam Preston, who was an Assistant Professor at the time in the Department of Demography, offered to introduce me to Judith, whose office was at the end of a long corridor in one of the temporary buildings on the Berkeley campus. As Sam and I walked down that corridor, I formed a mental picture of Judith. I had just come from graduate school at Princeton, and Judith Blake was already a legend. Her name was everywhere. My memory took me back to the days when I was a junior in college at the London School of Economics. One afternoon I heard a lecture from the distinguished British economist Joan Robinson. When Professor Robinson was introduced, what I saw was a woman in her late 60s or early 70s, plainly dressed in high black tie shoes, and white hair pulled back tightly into a bun. This, I thought, is what Judith Blake probably looked like as well. Sam Preston knocked on the last door on the left at the end of the corridor in building T-8, and when Judith opened it, I practically dislocated my jaw. The person standing in front of me was definitely not Joan Robinson, but rather a strikingly attractive woman in her early 40s, dressed in the latest fashion, with flaming reddish-blond hair, not a strand of which was out of place. That's when I learned to expect the unexpected from Judith Blake!

Judith gave me my first genuine teaching opportunity. It was during the spring semester of 1972 when I was asked to teach baby demography (Demography 100) at Berkeley. It was a wild experience, let me say. There were more than 100 students in the class, some of whom would bring their favorite pets to lecture, others of whom would sit in the back row and read the newspaper. One afternoon in the middle of class, three or four interlopers burst into the room and started pleading with my students to empty out into Sproul Plaza for some demonstration or other. Much to the students' credit (and I suppose partly to mine) the intruders were chased away with cries of "we're here to learn." Teaching that class proved to be a happy experience, and also a productive one because it produced at least two demographers—Greg Spencer and John McHenry.

Judith also gave me my first opportunity to present a paper at a professional meeting. It was the annual meetings of the Population Association of American held in either Montreal or Toronto in the spring of 1972. At the time, I was doing research on the cost of raising children, hadn't quite completed my dissertation, and was petrified at the prospect of delivering a paper in front of a large audience. I prepared solidly for three weeks, going over and over that talk. To this day, I can remember the opening line. The talk was very well received, and the positive feedback that Judith gave provided a big boost to my confidence. (Tom Espenshade, *Memorial Service for Judith Blake,* 1993)

## *Fred N. Bixby Chair, University of California, Los Angeles*

Upon the Department of Demography's closure, Judith moved to the Graduate School of Public Policy at Berkeley, and in 1976 she went to the University of California, Los Angeles (UCLA), School of Public Health as the Fred N. Bixby Chair in Population Policy, with a joint appointment in Sociology. As the first holder of the Fred N. Bixby Chair in Population Policy, Judith Blake was among the first, if not the first woman to hold an endowed Chair in the the University of California system and certainly on the Los Angeles campus. At UCLA Judith took responsibility for developing a Doctor of Public Health (DrPH) program in population and family health. Until shortly before her retirement she chaired the doctoral admissions committee and supervised the doctoral roundtable. In this capacity she served as the primary mentor to the more than twenty doctoral students who started or completed the program during the 15 years she was on the faculty. She developed courses in population policy that were among the most rigorous and highly regarded of those available to students, many of whom sought her out during office hours for both academic and personal advice.

My own memories of Judith date from this period:

> As a student under Kurt Back and Hal Winsborough, I was first introduced to Judith's early work when I was a graduate student, with particular emphasis on her research in Jamaica. However, since I did not pursue demography as an area of concentration, I never saw Judith in action until the Population Association meeting in Seattle in 1975. There, on the first evening of the meeting, I was in the hotel lobby when a group, possibly organized by Linda Moulton, with Judith in the vanguard, sailed into the lobby. I remember asking someone in the area who that was, and being told that it was Judith Blake. Later, during these same meetings, I attended a session for which Judith was the discussant. The session was held in the ballroom, and unlike many such sessions, attracted a large audience. The three presenters gave their papers which, as I recall, were in three quite different substantive areas. Judith then got up as discussant. She carefully examined each paper, using its own basic paradigm and assumptions. She suggested how the analysis might be extended, pointed out flaws in the logic underlying the study, examined the implications for policy and practice, and demonstrated how the papers complemented and contradicted each other. I went away mightily impressed. Judith Blake became my role model for being a discussant. It never occurred to me that she would become a colleague in the not-too-distant future.
>
> When the Bixby foundation provided the School of Public Health with funds for an endowed chair, the late Leo Reeder was appointed to chair the search committee. As an assistant professor, I was not directly involved in the search. But I was, by accident, present when Judith's name was first suggested as a candidate. A group of us were in Leo's suite, and he was suggesting names of people who might be qualified for, and interested in, the Bixby Chair. Someone, possibly Leo himself, suggested the name of Kingsley Davis. Immediately, Leo said, "Well, why Kingsley? Why not Judith?" He immediately picked up the phone and called Berkeley. I don't remember if he actually talked to Judith at the

time, or left a message. But that was, I believe, the time and the way that Judith's name was entered into the search process. (Linda Bourque, *Memorial Service for Judith Blake,* June 6, 1994)

Judith's own memories of her appointment to the Bixby Chair were recorded in her interview with Jean van der Tak.

[I went over to the Graduate School of Public Policy at Berkeley] which turned into ... a five-year period which was a very happy one .... [I]t was my first really friendly, supportive environment in academic life ... .[T]hen around 1976, the School of Public Health ... at UCLA got an endowment fund in population which was for a chair and they approached me. And it really didn't occur to me—I think I gave them six people's names, or something, and said these people would be good and that was it. I never thought about it.

Sometime in that period, Leo Reeder, who was a professor in the School of Public Health, called me ... and said, "Why are you not interested in this job." And I said, "Gee, I don't know." I never felt I had to account for why I wasn't interested in the job; it never occurred to me to leave Berkeley. I said, "I'm very happy where I am now." I just hadn't thought of moving. So he said, "It's a very advantaged situation. The faculty position is paid for out of state funds and the chair is just available to you as a yearly income to do research if you wish. This is a nice situation here. You should at least look into it. Why are you so rigid about this?"

So I began to say to myself, "Why am I so rigid about this?" So I [went to Los Angeles] and a great many of my Northern California prejudices were dissipated quite rapidly .... I found the Los Angeles area very attractive. Partly, I was a New Yorker and the proximity to this very interesting city, which was culturally sort of just bursting out all over, and the proximity to the open spaces, like the beach—40 miles of beach .... I was very impressed [with the university] as being in some ways in social and biological sciences a much more dynamic kind of place, a more interesting place to me, than Berkeley. And I felt less political, which was very appealing to me ....

So I began to see it in a different light and we began negotiations. Part of what I think appealed to me is that they were interested in population, whereas where I was, although they were interested in me and were interested a little bit in population, it was really sort of a concession ... having me in the School of Public Policy; population had never been one of the big things that they were interested in .... I guess I hadn't realized until I left [Berkeley], but it was almost as if something had stopped drilling and I suddenly realized, "There are other things and other places." ... So we negotiated a job here and I got a very substantial promotion and the chair and one thing and another.... So it has been a very happy situation for me. (*Demographic Destinies,* 1991, pp. 502–503)

## *Professional Service*

Judith served on numerous university and professional committees and boards. She was elected President of the Population Association of America (PAA) in 1981, and at the time of her death was the Editor of the *Annual Review of Sociology*. The 1981 PAA meeting was the fiftieth anniversary of the Associ-

ation, and Judith organized a program that featured many of the important demographers of the twentieth century.

> We wanted to have this be a real 50th anniversary, with people who younger people might not see again in a long time for a variety of reasons .... It was ... our way of saying thank you to them and making them feel that we appreciated all the things that they've done over the years .... It was ... a recognition of our forebears in the field. There was a lot of enthusiasm ... for doing it. It brought people ... together and the committee was full of beans about this and very helpful.
>
> But it's an awful job, because, fundamentally, as you get down to the wire, you get to be the central nervous system for it. You can never delegate completely; ultimately, it's all got to come together. And as you get down to that point, it's murder—it really is. (*Demographic Destinies,* 1991, p. 516)

Judith chose to focus her presidential address on emerging research rather than summing up a trend of past research. The topic of her address "Family Size and the Quality of Children" became the basis of her book *Family Size and Achievement* (1981b, 1989b).

> Some people gave presidential addresses which were very appealing and fun. But I felt that if it was the 50th anniversary, it shouldn't be something that was just topical or light-hearted. I felt it should be something that was serious, or was research, or whatever. And I had been thinking about this for long time, in part because it stemmed from all the arguments—with the Julian Simon argument—about population growth. I had been thinking how one could zero in on this in a way that would be more definitive.
>
> I started working on this material and realized that it could be quite interesting. I thought, "Well, I am not going to stop this now in mid-stream and start on something else, and what would the something else be?" I didn't want to talk about abortion, because I didn't want it to be controversial that way; I didn't want it to be inflammatory. So this was the logical thing. It was also practical: I was working on it and I just couldn't switch horses at that point. (*Demographic Destinies,* 1991, p. 514)

## TEACHING

When Jean van der Tak asked Judith, "What accomplishments in your career have given you the most satisfaction?" Judith responded, "Oh, gosh, I don't know. I've gotten a lot of satisfaction out of teaching. I've greatly enjoyed teaching."

Jean went on to say:

> Everybody I know who's studied with you thinks you're a dynamo. That was one expression that John Weeks used when we were talking about you at the recent PAA meeting. Alex von Cube, a good friend of mine at the Population Reference Bureau, said your classes were so stimulating, that occasionally there was controversy, lots of controversy. He said you could be hard on people, and sometimes you were a bit conservative, some of your students felt. (*Demographic Destinies,* 1991, p. 510)

At the memorial service for Judith Blake on June 6, 1993, Ron Lee, Tom Espenshade and Jennifer Frost all commented on Judith's teaching. Ron Lee stated:

> I came to Berkeley as a graduate student in 1965. I'd been an undergraduate major in philosophy. I didn't really know much about what demography was about. I had a lot of reservations about entering a very new and a seemingly very strange program. I went into the first class, which was taught by Judith. The classroom was filled with tables as well as chairs. Every table had one of those massive old calculators, not little electronic things the size of credit cards like we have today; these were big! I thought, "God, I want to hear about ideas, theories; I don't want to do calculations," and so my concerns about demography as a field grew more intense. But then Judith started talking and teaching, and it was only a few minutes before I realized that I was in for a real intellectual treat. Judith's classes weren't really about numbers, they were about ideas and theories. The numbers were all entwined with the ideas, in supporting roles. And so Judith's teachings, as I said, really were about ideas and theories. Classes were intellectual, very rich. The theories and ideas for her were not like books on a shelf or like cans in a supermarket, sitting there in their positions. She didn't offer them to us like a grocer or a librarian, but instead her theories were very vivid, forceful things that were engaged in their own struggles, they had lives of their own. And her role as the teacher and our role as the students was not just to watch this struggle of ideas from the sidelines, but rather to get down there in the pit and argue about them and take part in this intellectual combat that was going on, with Judith leading us through. Well this was a very exciting way to teach .... Certainly, she was the best teacher I ever had, in my graduate or undergraduate years, the most exciting, the most stimulating, and in many ways the deepest. (Ronald Lee, *Memorial Service for Judith Blake,* 1993)

Tom Espenshade commented on Judith's role as a teacher in professional settings outside the classroom.

> An unexpected aspect of Judith Blake was that she had an Assistant Professor's outlook in a Full Professor's body, and by that I mean that she was always prepared; Judith didn't leave anything to chance. The first time I noticed this occurred sometime in the 1970s when my family and I were living in Tallahassee, Florida. Judith had been asked by the population center at Florida State University to come down and give a talk. Because we had known each other at Berkeley I invited her to stay with us. Early in the morning before the sun was even up, I walked past Judith's room and noticed the light coming out from under the door. Several hours later, over breakfast, I commented on this, and Judith replied that she had gotten up early to prepare her talk. One might have expected seasoned pros to talk off the cuff, but not Judith.
>
> A second example occurred when Judith was asked to be a discussant at the PAA meetings for a paper being presented by Paul Schultz. Schultz is an economist schooled in the latest econometric methods. Judith was not daunted by the task, and set about learning two-stage least squares and simultaneous equations models so she would have something intelligent to say. I was tremendously impressed. Judith never stopped learning, never stopped pushing herself to higher

and higher levels, and never stopped expecting anything less from those around her. (Tom Espenshade, *Memorial Service for Judith Blake,* 1993)

Jennifer Frost has similar memories of Judith as a classroom teacher and mentor.

In September 1981, I first walked into Judith Blake's office, a new masters student, assigned an advisor whom I knew relatively little about. Little did I suspect that over the next ten years, that office would become a second home, and the woman, who was so friendly and helpful on that first encounter, would become an inspiration, a mentor, and by far the most influential person in my college years, guiding me by example and by subtle prodding through the maze of academic, professional, and personal decisions which characterize graduate life. In time, as I became somewhat educated in the field of population-related issues, I realized that my teacher was no ordinary professor, but an academic giant, a consummate scholar, a true scientist. My recollections of Judith, like those of many here today, began as a student, and evolved as I came to know, trust, respect, and love this remarkable woman. As a student, I stood in awe of her abilities as a lecturer. If the future should ever find me at the head of a classroom, I know I have an exemplary model from which to draw motivation and guidance. Somehow, even the driest topics took on new meaning in her classroom. She conveyed such a sense of enthusiasm and excitement about the importance of studying population policy, demography, and the sociology of family behavior. Moreover, she taught us to challenge everything. The rhetoric of conservatives and liberals alike often passed before her scientific scrutiny and rarely escaped unscathed. Yet the Judith Blake which I remember most clearly is not the academic scholar or the animated lecturer but the warm, caring human being. A few weeks ago, I was speaking about Judith to one of the editors where I now work at the Alan Guttmacher Institute, and, with real reverence, he declared, "She was a real giant." My response was, "Yes, but even with such distinction she always had the time to give help and support to those of us who studied and worked with her and she always made us feel that our ideas and thoughts were important and worthy of consideration." "That," replied my coworker, "is rare indeed." As a research assistant and an advisee struggling to write a dissertation, I remember both the substantive advice and guidance which Judith dished out, as well as the humor and conversation which characterized her office. Life there was usually a lot of fun, and over the years she gave us encouragement, warmth, and ample suggestions for vacation getaways, movies, and good food. For me, and a relatively high proportion of the female doctoral students under her wing, the discussion invariably turned to the topics of childbearing, child care, and the joys and frustrations of motherhood. Unlike some in the academic world, Judith was also rare in her unequivocal support for those of us who chose to combine graduate studies with childbearing. "For a professional, career-minded young woman, there is no 'good' time to have a baby," she would say, "so one must just make the best of things as they occur." During the early stages of my dissertation, I also remember being impressed by her seemingly insatiable curiosity about the world and all the variety of people in it. When I chose to study culture and fertility in Kerala, she was undaunted. She requested that I provide her with a few references covering Kerala's history, culture, and social structure, and soon she was well versed in the peculiarities of that region, debating with me the relative importance of

corruption in the corporate matrilineal joint-family versus economic change as a stimulus to fertility change. As my research progressed and I began to prepare to orally communicate some of my findings, her advice over and over again was "Rehearse—you can never rehearse too many times." I'm afraid that I never left myself enough time to be as organized, as prepared, as rehearsed, as Judith always seemed to be. But the debt which I owe Judith is somehow deeper and more personal than that of a traditional student-teacher relationship. For some reason she had confidence in me, and her confidence helped me to believe in myself at a time when my own faith was waning. When, unexpectedly, my husband died, leaving me with an unfinished dissertation and an infant son, it would have been quite easy to give up on many of my goals. But Judith was there, providing emotional and financial support, and even without saying anything, I felt her confidence; for her there was no question that I would not only finish, but also excel in all I attempted. At that time, I might have been able to sacrifice my own expectations, but somehow I couldn't let go of the expectations which I knew she held for me. I remember a conversation which I had with Judith some weeks after my husband died, and while most of the words are a blur, one statement stands out in my mind: "Jennifer," she said, "now it's your turn to shine." In June of 1992, a year ago this month, Judith stood next to me and shook my hand as I received my doctorate. It was a shining occasion, and I knew then, as I know today, that much of what I had achieved, and much of what I will achieve, is due largely to the influence of Judith Blake on my life. Her memory will inspire and guide me as I accept new challenges and move on through life. (Jennifer Frost, *Memorial Service for Judith Blake,* 1993)

Judith herself said the following about her teaching:

I've never liked teaching huge amounts, because I like to prepare a lot, and I like to use teaching as a way of picking up on issues that I wouldn't ordinarily be starting to write about or do research on and thinking them through. So I've always used it as that kind of a vehicle, for getting into something that I was just starting to think about. It gave me an opportunity to do a lot of reading, a lot of thinking. It has fed into research—not on a day-to-day basis but more to say, "This is a way of beginning to think about something and get input from people and have them criticize you and get a lot of stuff going on something." It's been very creative for me for this reason, and I've enjoyed it enormously.

Interestingly enough, over the years I've seemed far less conservative to people, partly because they have gotten more conservative, I think. So that is rarely mentioned at this point in time, because people have changed their own views about a lot of things. That helped—not that I changed them, but the world has changed....

[W]hen I was coming along ... [being a woman] was a big disadvantage .... Nobody thought of you as being anybody and nobody ever thought of you for anything. When I looked and saw how young men were sort of mentored and coddled and taken under people's wings and pushed and so forth, I didn't feel that during my youth it was an advantage to be a woman at all, in academic life.

On the other hand, I have tried, I think, to make it an advantage for other women. I've felt very strongly about this. I'm not a rabid feminist in the sense that I've joined groups and things of this sort. But I have been very instrumental in pushing a lot of women along. And increasingly so, as opportunities are arising, I think I have helped a lot of people to get good jobs .... And helped them over

> life-cycle events, like pregnancy, for example, which I think many males have always felt was "She got pregnant; that's the end of that." I have been very supportive of people who have been in this situation and made them feel that there's no reason at all why they can't continue, through their pregnancy, getting their dissertation done. (*Demographic Destinies,* 1991, p. 510)

It is well to note how Judith affected students who did not go on to become demographers. An anonymous student made the following observation in a 1983 teaching evaluation:

> Dr. Blake has provided the most intellectually stimulating challenges in my graduate career. I marvel at the breadth of her knowledge outside demography, from Samuel Johnson to *Le Rouge et le Noire,* absolutely topnotch presentations and rigorous thinking.

## CONCLUSION

Judith Blake was a dynamic and creative researcher and teacher who, nonetheless, never underestimated the value of preparation. Whether presenting to a packed ballroom at the annual meetings of the Population Association of America, lecturing to a classroom of first-year masters students, or participating in a department meeting, she was a dramatic speaker and never dull. When Judith was on the program at the Population Association meetings, the sessions were often scheduled for the largest room available. "People jammed the room, standing out in the hall, and Judith would stand up and be sparkling and entertaining and enlightening and penetrating" (Ronald Lee, *Memorial Service for Judith Blake,* 1993).

> Most of you are aware of her many accomplishments, her extensive writings in many areas.... Also, all of you knew the considerable energy and verve which she brought to everything she did. What is less well known, I think, since Judith, was a very private person, is that she achieved all this despite an adult life characterized by many severe illnesses. She had a bout with kidney cancer which started in her late teens, was not diagnosed and dealt with until her mid-20s. And then there was the onset in her late 20s of the serious lung disease from which she eventually succumbed. In fact, she had an episode where I understand the doctors told her that she probably could only expect to live ten more years, and she outdid them by over 30 years. Despite all this, she managed to build up a long and distinguished career, without most people knowing that she was ill until very recently. And in fact many people didn't even know that she was ill until she died. I hope that it's a sign of the tremendous willpower and true energy that she had despite all this. (Valerie Oppenheimer, *Memorial Service for Judith Blake,* 1993)

While Judith had little tolerance for fools, she was neither mean-spirited nor vindictive. She did expect those with whom she interacted to continually question, challenge, and stretch their intellectual abilities in the same way that

she did. She loved a good intellectual debate, but when she disagreed it was with the ideas expressed, not with the person who expressed the ideas. The force of her personality drew people to her; many stayed to learn from her, exchange ideas with her, and ultimately, to build upon what she gave them. She is sorely missed by her former colleagues and students, but she has left a strong legacy in her research, the students she taught, and the friends and colleagues she influenced.





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# FAMILY SIZE AND THE QUALITY OF CHILDREN*

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*Abstract*—If couples decide to have fewer children in order to achieve higher "quality" offspring, are they correct in assuming that the quality of children bears an important and inverse relation to family size? If they are correct, how does number of children operate to affect individual quality? This research (using U.S. whites primarily) takes educational attainment (among adults) and college plans (among youngsters) as the principal indicators of quality, but also directs some attention to measures of intelligence. The analysis supports the "dilution model" (on average, the more children the lower the quality of each child) and indicates that only children do not suffer from lack of siblings, and that other last-borns are not handicapped by a "teaching deficit." Number of siblings (relative to other background variables) is found to have an important detrimental impact on child quality—an impact compounded by the fact that, when couples are at a stage in life to make family-size decisions, most background factors (however important to the quality of their children) are no longer readily manipulable. A special path analysis of college plans among boys uses a modification of Sewell's Wisconsin Model as its base. The results show that number of siblings is a negative influence on intervening variables affecting college plans. In general, the research documents the unfavorable consequences for individual siblings of high fertility, even in a country that is (at least for whites) as socially, economically, and politically advantaged as the United States.

Many of us have spent the preponderance of our careers studying the determinants of human fertility. In so far as we have been concerned about fertility decision-making within unions in modern societies, we have reasoned that the number or quantity of children competes with two other utilities for the resources at a couple's disposal—the nonchild utilities, cars, houses, vacations, etc. on which the couple could focus its consumption, and the quality of children to be produced. We have debated about the realism of these decisionmaking models and how they should be specified—what the ingredients should be and what goes where in the chain of casuality.

Today, I will not be concerned with whether these models bear much of a relationship to how couples really think in making family-size decisions. Nor, will I discuss whether consumption alternatives to children are what principally drive couples in their family-size decisions. What I do want to deal with is the assumption that people have fewer children in order to have higher *quality* ones. And, I want to turn things around a bit and ask the following question: *If* this is one of the things that people have in mind when they curtail their fertility, can they be shown, on average, to be on the right track? For research purposes, this question can be broken down into three

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subsidiary ones:

1. Is family size *inversely* related to child quality?
2. Is family size an *important* influence on child quality?
3. And, finally, if family size is important, what does it affect? What are the mechanisms by which it operates?

I will take a moment for some conceptual orientation. Why would anyone think that the quality of children is inversely related to their quantity? The obvious answer is some form of the "dilution" model of parental inputs. In considering this, we need to specify, albeit schematically, just what it is that parents put in once the child has been born. We can say that parents provide *environments* or *settings*—types of homes, necessities of life, cultural objects (like books, pictures, music, and so on). They also provide *opportunities*—specific chances to engage the outside world or, as kids say, "to get to do things." Finally, they provide personal attention, intervention, and teaching (either directly or by example)—what has been called *"treatments"* (Spaeth, 1976).

The dilution model says, "The more children, the more these resources are divided (even taking account of economies of scale) and, hence, the lower the quality of the output." It is understood that by child "quality" we mean some objective measure of human capital such as educational or occupational attainment. No judgement is being made about the intrinsic "worth" of one person over another.

We must note as well that the dilution model assumes that the casual arrows all go in one direction—from the parents to the children. The model does not assume any feedback effects from children during the childrearing process. For example, it does not allow for the assumption that parents may create junior executives out of older children—pseudo-parents on whom they can rely. Moreover, the dilution model takes no account of the influence of reproduction on the *parents'* life chances. Parents' socio-economic status is assumed to be uninfluenced by reproduction. It may be *divided* among offspring, but it is not changed by them. An additional possible feedback that the dilution model leaves out is that socialization of siblings by each other may be indispensible to producing high quality children. Siblings—just a few, maybe even just one—may themselves be a *condition* of child quality. We know that most people believe this to be the case—they believe that the only child is disadvantaged because of a lack of siblings with whom to interact (Blake, 1981; Falbo, 1977; Thompson, 1974). However, our concern here is not what people believe, but whether they are correct.

Finally, the parental dilution model has nothing to say about possible birth-order effects. Within any family size, does it matter whether you are first, second, and so on? Logically, the model implies that child quality would go down with each successive child, but the rate of decline would taper off after the second child, because each succeeding one experiences proportionately less of a loss. However, if there are feedback effects—if older children become pseudo-parents, and/or if sib-socialization is a necessary condition of child quality, then quality might not decline with birth order.

I shall now turn to my attempt to answer the three questions raised earlier:

1. Is the relation of family size to child quality inverse?
2. Is family size important for the quality of children?
3. If it is important, what does it affect?

My research on these questions has involved secondary analyses of many large scale surveys that have included number of siblings as a variable. These surveys have never before been analyzed from this point of view. Here I will give you selected results—it is impossible to dis-

cuss all of the surveys in detail, or all of the results of any one analysis. The analyses are for whites only, because of interactions by race. The principal surveys to be discussed are:

1. The National Opinion Research Center, General Social Survey, 1972–1980 (pooled), approximately 8,000 men and women.
2. The 1970 National Fertility Study, approximately 4,000 women, plus data on approximately 4,000 husbands.
3. The 1960 Growth of American Families Study, approximately 2,600 women.
4. The 1955 Growth of American Families Study, approximately 2,350 women.
5. A longitudinal study of 10th-grade boys, *Youth in Transition*, approximately 2,000 boys.

In the studies of adults, the indicator of quality is "total years of education achieved." For the 10th-grade boys, the dependent variable is "college plans." The independent variables, in addition to number of siblings, are indicators of the respondents' parents' socio-economic status, whether they were Catholic or non-Catholic, Southern background, community size, respondent's age, and whether the family was broken or intact when the respondent was growing up. Although these background variables represent important controls, they do not, unfortunately, take full account of potentially negative selective influences on respondents from one-child families. This family-size has not been a popular goal for couples, hence one would expect some disproportionate negative selection into it—some children are singletons because their defects led the parents to decide to stop reproduction, or because the parents themselves had physical or psychological problems inimical to parenthood, or even because the parents did not get along well although they stayed together. Hence, we suspect that any given sample of only children includes some disproportionate effects of negative selection which, although unmeasured, should not be assumed to be an effect of number of siblings per se.

## THE NATURE OF THE RELATIONSHIP

Let us turn first to the nature of the relation between family size and total years of education among adults. From here on out I shall call family size "sibsize" since our point of reference is the offspring, and its quality, as it is affected by the number of its siblings. Does sibsize bear an inverse relation to total years of education? Our regression analyses (Figure 1) of the survey data among both men and women indicate, for each survey, that the relation is, indeed, inverse.

The only child does about equally well as those from two-child families, or just slightly better, but beyond the two-child family, performance starts to drop off. I believe that these results give quite consistent support to the dilution model.

Although, in this research, our "child quality" outcome variable is educational attainment, we have necessarily paid attention to research on the relation of sibsize to cognitive ability or intelligence. This is because intelligence is obviously an important influence on educational attainment, and because a recent challenge to the pure dilution model, by Robert Zajonc, has related to a measure of intelligence (Zajonc and Markus, 1975; Zajonc, 1976).

Considering intelligence for a moment, large-scale studies of school children in France (Institut National d'Études Démographiques, 1973), Scotland (The Population Investigation Committee and the Scottish Council for Reserch in Education, 1949), and England (Douglas, 1964; Eysenck and Cookson, 1970; Marjoribanks, 1974) all suggest that, even within social class levels, an inverse relation exists between sibsize and a variety of measures of cognitive ability. These data thus support, for intelligence, the

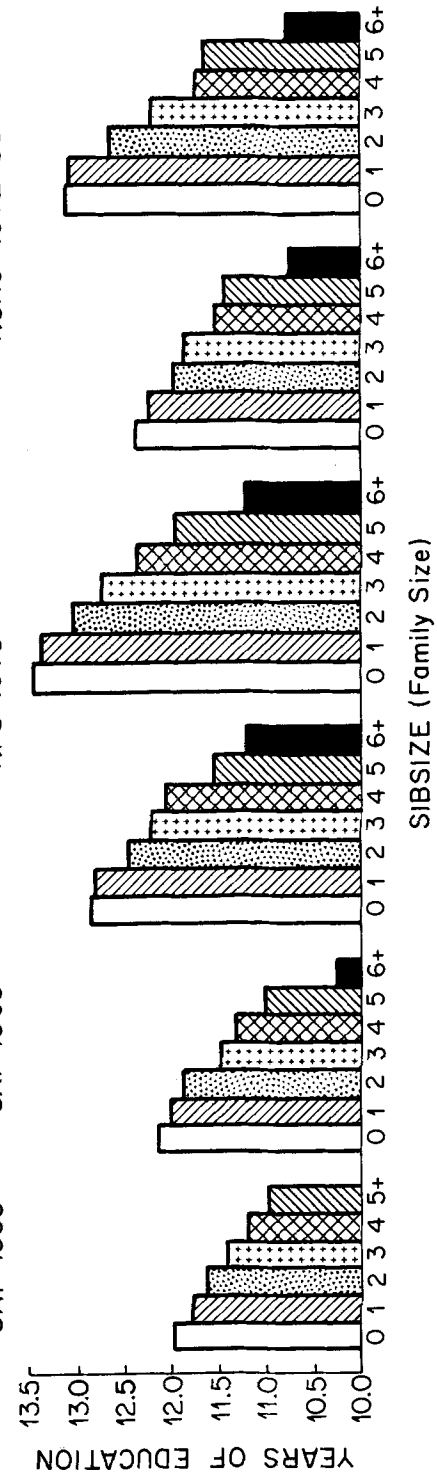


Figure 1.—Total Years of Education by Sibsize (Family Size) among Respondents in the Growth of American Families Studies 1955 and 1960, the 1970 National Fertility Study (Female Respondents and Derived Data on Husbands), and the General Social Survey, 1972–1980, National Opinion Research Center. Results by Sibsize and Birth Order Have Been Adjusted, through Multiple Classification Analysis, for Parents' Socioeconomic Status, Catholic-NonCatholic Religion, Respondent's Age, Southern-NonSouthern Background, Community Size, and Whether the Parents' Marriage was Intact When the Respondent Was Growing Up. Analysis by the Author is Based on Tapes from the University of Wisconsin and, in the Case of the NORC 1972–1980 data, from the Inter-University Consortium for Political and Social Research, see Acknowledgments.

same kind of dilution effect that we have seen to operate further down the chain of causality for educational attainment.

A notable exception to this support has been suggested by Zajonc in his interpretation of intelligence tests on almost 400,000 Dutch males—survivors of the birth cohorts of 1944–47 in Holland (Zajonc, 1976). Zajonc emphasizes that the last child, and, by extension, the only child (who is the last child in a one child family), shows up poorly in those data when they are disaggregated by birth order. The data interpreted by Zajonc are from an article by Belmont and Marolla (1973) which, in turn, represented a reanalysis of a study by Stein, Susser, Saenger, and Marolla (1972; 1975).

As can be seen from Figure 2 the only child does only slightly better than the last child in a three-child family, and within each family size, the results for the last child take a sharp drop.

Zajonc believes that these data point to the existence of a "teaching deficit" affecting last-born children—they have no younger sibling to teach and, hence, their own learning is impaired (Zajonc, 1976; Zajonc and Markus, 1975; Zajonc, Markus and Markus, 1979). In effect, Zajonc is saying that the dilution model, although in the main correct, requires us to take account of sib-socialization if it is to fit the Dutch data. Siblings do not merely dilute parental resources; they are resources for each other.

Before considering the Dutch case in more detail, we should note that the poor showing for the only and last child in those data stands in marked contrast to the results of other large surveys. For example, the French (Figure 3) and Scottish (Figure 4) surveys of school children, disaggregated by birth order, show the only child to be highly favored and, if anything, indicate an advantage for last-born children.

However, these results are for young children—in Scotland the children were 11, and in France they ranged from 6 to 14. Hence, it can be argued that the data suffer from an age-truncation bias. For example, when 11-year-olds are sampled, last-borns will naturally do better than first-borns in large families because last-born children in such families do not necessarily come from close-spaced sibsizes, but first-borns of that age must come from tightly spaced sibs.

For this reason, it is important to introduce additional data that are less subject to such bias. The *Youth in Transition* data set, mentioned earlier, is helpful here. The *Youth in Transition* white, American youngsters are aged 15 and 16. Moreover, if we concentrate on small-to-moderate families, there is no reason to expect particular effects of spacing, since American women have spaced their children relatively closely even when they had only two. The results in Figure 5 come from our Multiple Classification Analysis which controls for parents' socio-economic background and family intactness. It is evident that the same pattern exists as may be seen in the Scottish and French data. Only children perform in a manner similar to those from two-child families, and last-born children also appear to be relatively advantaged.

Another large study of American youngsters by Hunter Breland (1974) is of interest here. Breland analyzed the National Merit Scholarship Qualification Test scores for all participants tested in 1965. Apparently no information was available on parents' SES, race, or intactness of marriage, but it was possible to tabulate the scores by family size and birth order. There is a clear and statistically significant negative relation of scores with family size, except that, as might be expected without controls for race, socio-economic status, or family intactness, the only child does not do as well as first and second-borns from two and three-child families. However, there is no evidence of a sharp drop-off in performance for the last child (Breland, 1974, p. 1013).

Finally, since birth order as well as

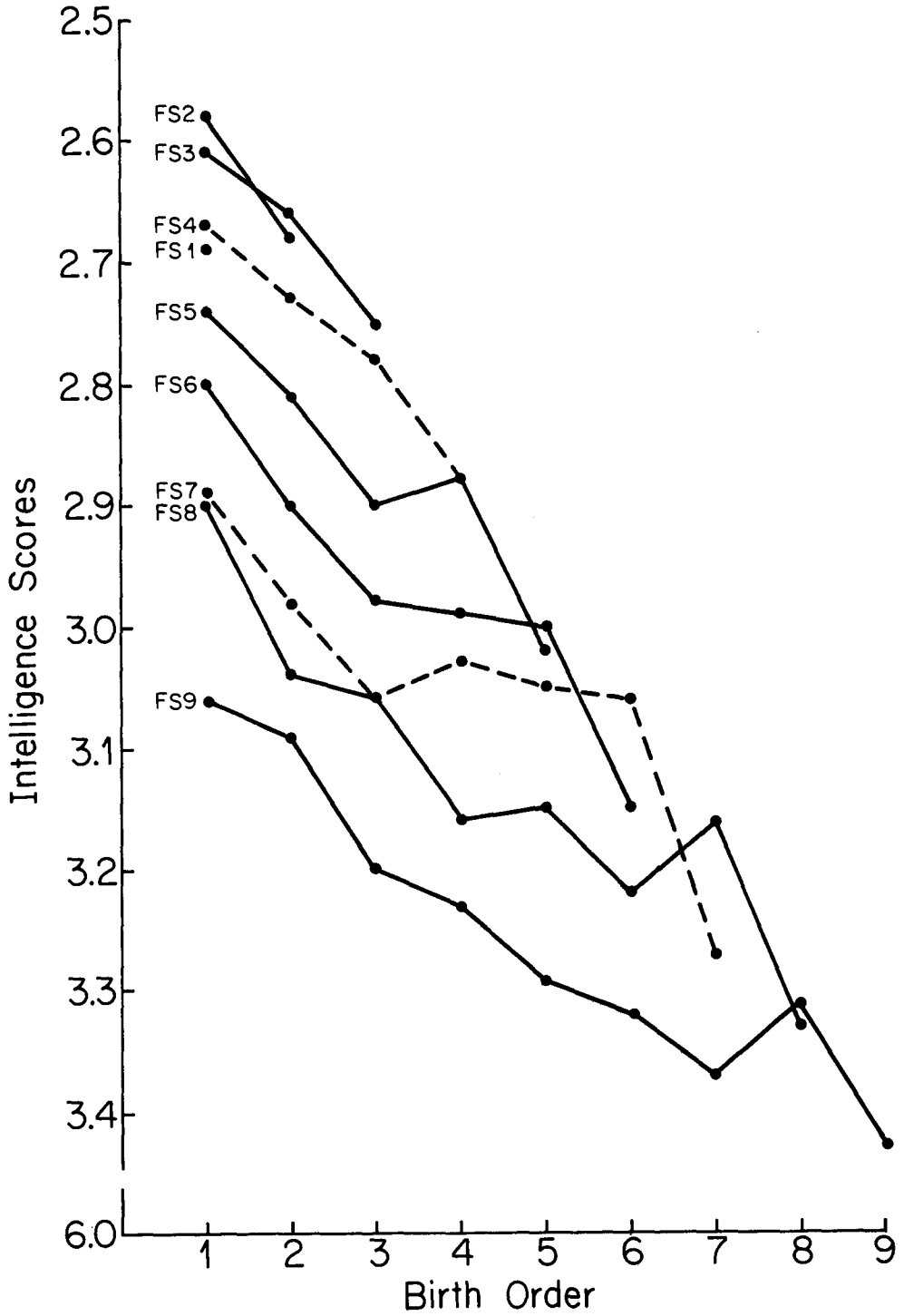


Figure 2.—Scores on Raven Progressive Matrices Test by Sibsize (Family Size) and Birth Order among 386,114 19-Year Old Dutch Males Who Were Survivors of Children Born During 1944–1947. Data from Belmont and Marolla (1973), p. 1098.

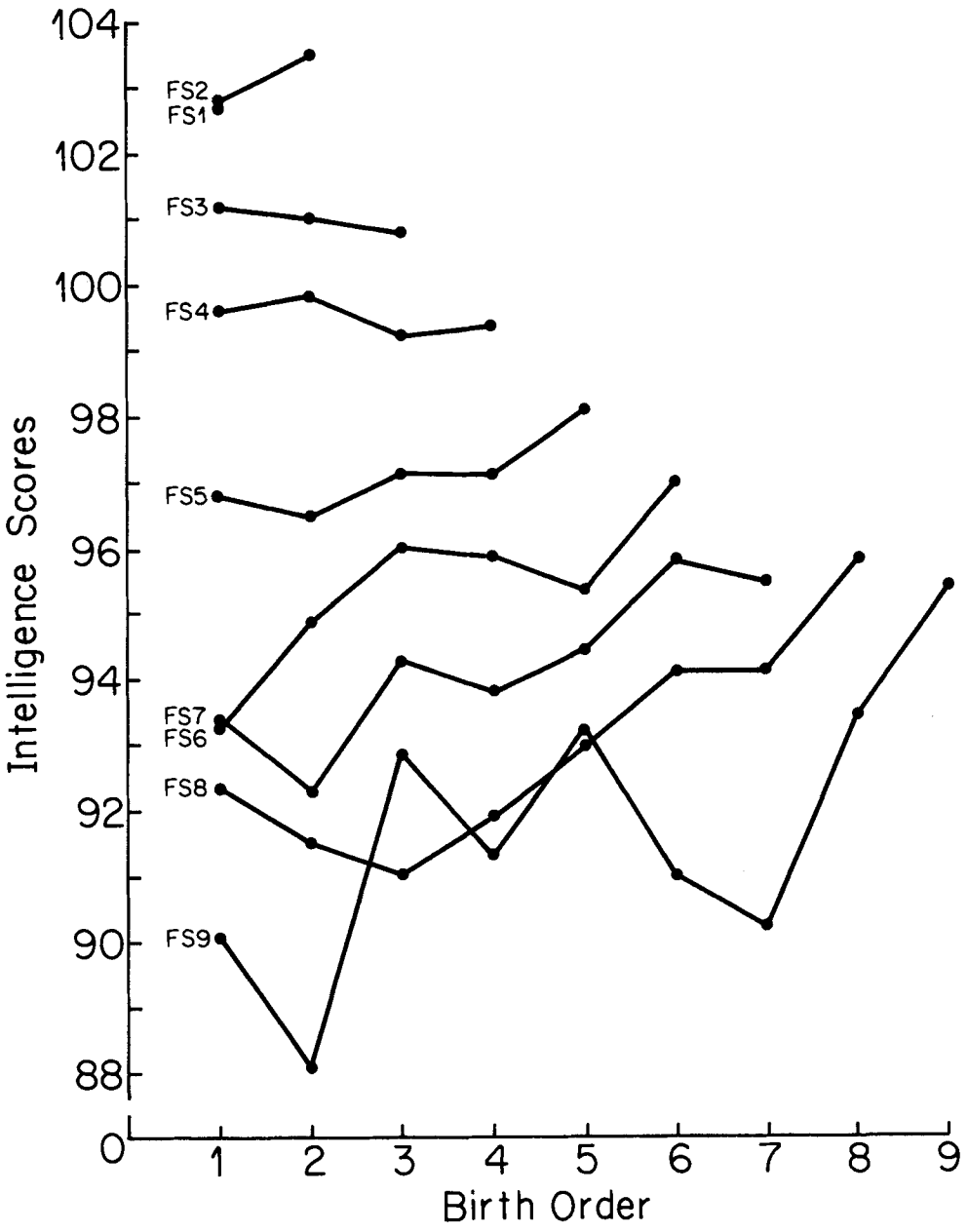


Figure 3.—Scores on Benedetto Group Test of Intelligence by Sibsize (Family Size) and Birth Order among 100,000 French School Children Aged 6–14 in 1965. Data from Institut Nationale d'Études Démographiques (1973), p. 67.

sibsize is available in the 1970 National Fertility Study of adults, my Multiple Classification Analysis of this data set considers total years of education by both sibsize and birth order, controlling for all of the background variables mentioned already. Figure 6 is for white female respondents and Figure 7 is for their husbands.

Again, we see a very different pattern

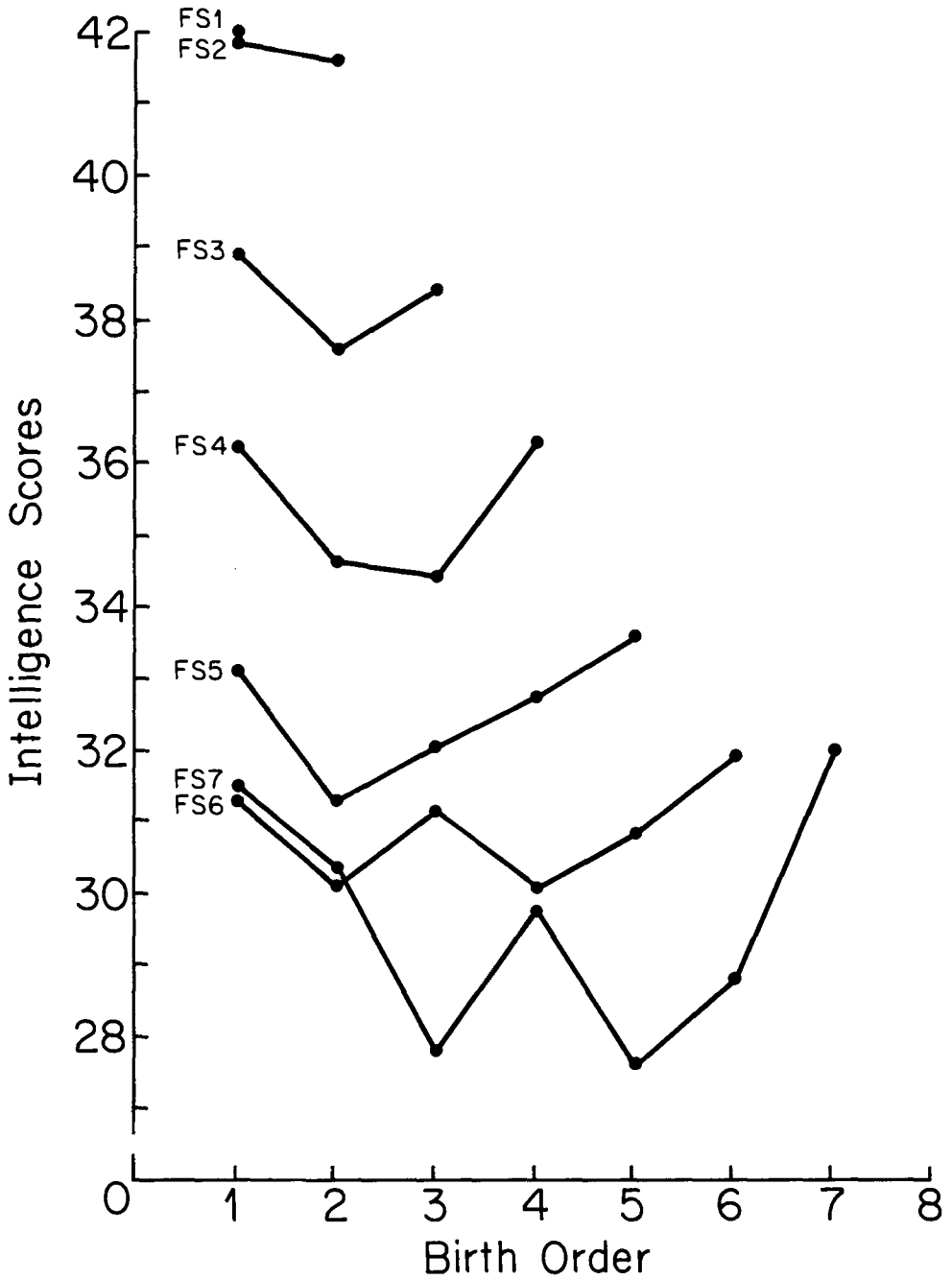


Figure 4.—Scores on Group Test of Intelligence by Sibsize (Family Size) and Birth Order among Scottish School Children Aged 11 in 1947. Data from Population Investigation Committee and the Scottish Council for Research in Education (1949), p. 107.

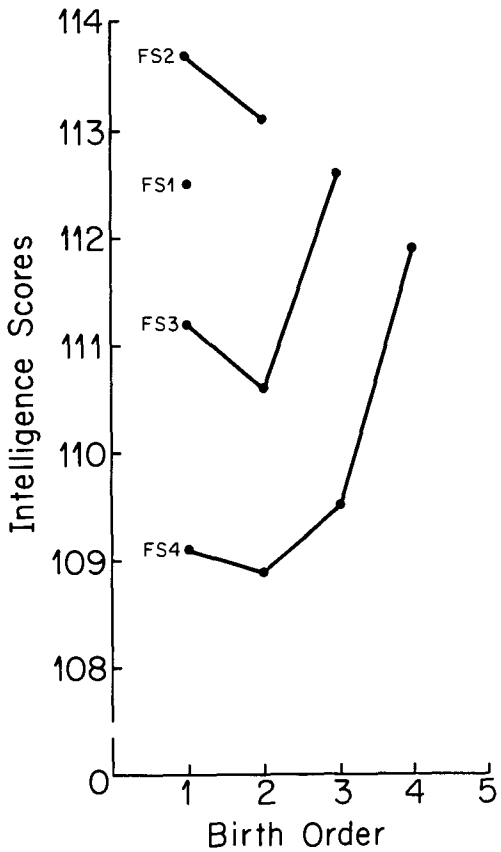


Figure 5.—Scores on Quick Test of Intelligence by Sibsize (Family Size) and Birth Order among 1912 10th-Grade Boys. Results by Sibsize and Birth Order Have Been Adjusted, through Multiple Classification Analysis, for an Index of the Parents' Socioeconomic Status, Catholic-NonCatholic Religion, Community Size, and Whether the Respondent's Parents' Marriage Was Intact. Analysis by the Author is Based on a Tape of the University of Michigan, Youth in Transition Survey, see Acknowledgments.

from the Dutch data, and an analogous one to the results for youngsters. Singletons are close to those from two-child families, and last-borns frequently do better than middle children and sometimes better than first-borns. Birth order is not, however, an important variable in our regressions.

These findings suggest that we need to reconsider the Dutch data insofar as possible, since an analysis of almost 400,000 cases cannot be ignored. In doing so, it is necessary to go back to the original study of the young Dutchmen from which the results used by Belmont and Marolla, and then, derivatively, by Zajonc, were generated.

The original research (Stein, Susser, Saenger and Marolla, 1975) was not concerned with the relation of intelligence to family size and birth order, nor were the data tabulated in this fashion. Rather, it was designed to analyze the effects of maternal malnutrition during pregnancy on the subsequent intellectual development of the children born to mothers who had suffered starvation during the 1944–45 famine in parts of Holland. The study design focused on famine and nonfamine geographical areas of the country, and on whether the children had been carried, or born, during various phases of the famine and post-famine period. The original analysis found no measurable effects of prenatal malnutrition on the intelligence of the surviving Dutch men tested 18 or so years later. Then Belmont and Marolla (and later Zajonc) decided to use the Dutch data for a very different problem—the relation of intelligence to family size and birth order.

However, these data had unrecognized defects for research on intelligence by family size and birth order. This cohort of almost 400,000 men (born between 1944–1947) turns out to be a demographically skewed sample of births and survivors—biases that related *particularly* to being an only and last-born child. The 1944–1945 famine drastically affected conceptions and, thereby, the subsequent birth rate in starvation areas of Holland (Smith, 1947a and 1947b; Stein, Susser, Saenger and Marolla, 1975). Only-child and last-child situa- were thus created *among those couples who were most severely affected by starvation*. Furthermore, very high rates of

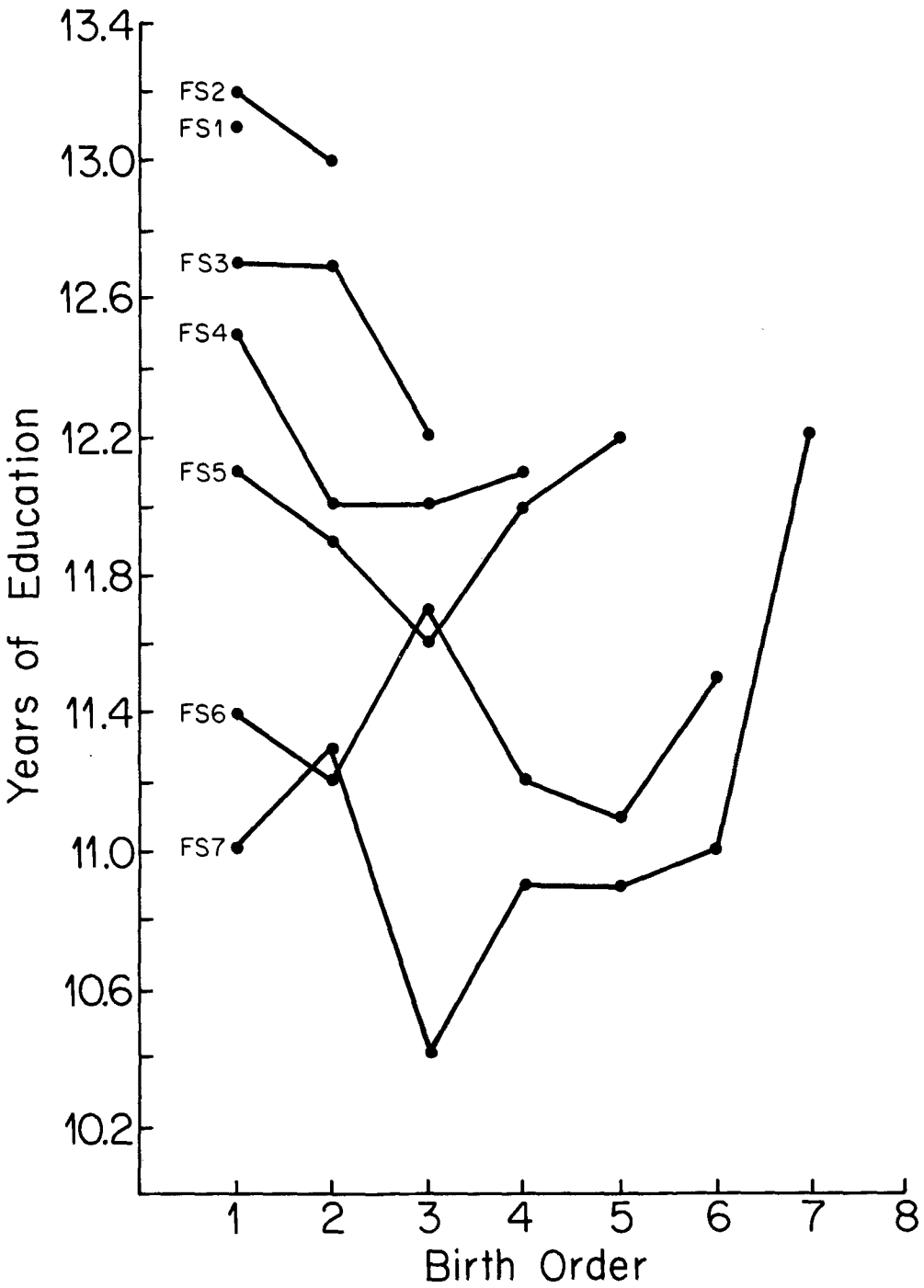


Figure 6.—Total Years of Education by Sibsize (Family Size) and Birth Order among 3868 Female Respondents Over Age 23 in the 1970 National Fertility Study. Results by Sibsize and Birth Order Have Been Adjusted, through Multiple Classification Analysis, for the Respondent's Father's Occupational Prestige Score, Respondent's Age, Religion in Which Reared, Whether She Lived with Both Parents at Age 14, Southern-NonSouthern Background, and Community Size. Analysis by the Author is Based on Tapes from the University of Wisconsin, see Acknowledgments.

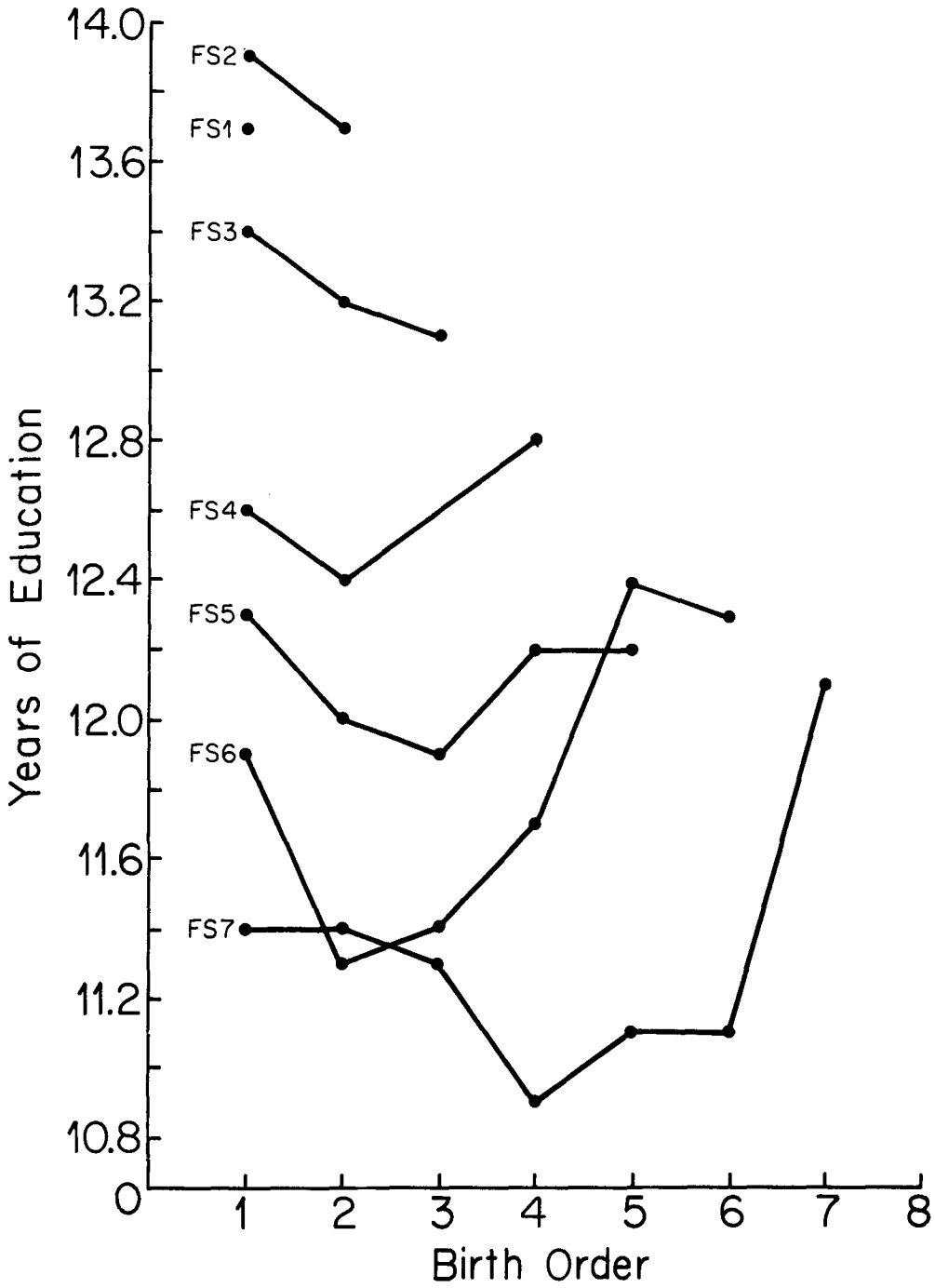


Figure 7.—Total Years of Education by Sibsize (Family Size) and Birth Order among 3782 Husbands (Aged 25 and over) of Respondents in the 1970 National Fertility Study. Results by Sibsize and Birth Order Have Been Adjusted, through Multiple Classification Analysis, for the Husband's Father's Occupational Prestige Score, Husband's Age, Religion in Which Reared, Whether He Lived With Both Parents at Age 14, Southern-NonSouthern Background, and Community Size. Analysis by the Author is Based on Tapes from the University of Wisconsin, see Acknowledgments.

infant and child mortality at this time, together with high stillbirth rates, also helped determine that children would grow up as singletons and last-borns—again particularly in those families worst hit by the famine. Children carried during the famine were selectively subject to mortality, and those born during it were fatally vulnerable (Smith, 1947a and 1947b; Stein, Susser, Saenger and Marolla, 1975). Finally, the joint probability of survival of the parents decreased, since the death rate rose among adults as well. This effect was gravest where people were hit worst by the famine.

In sum, only and last-born children in this cohort of young Dutch men appear to have been negatively selected—on average they were more likely to come from families that suffered the worst. That is how, in part, many of them *became* singletons and last-borns—their parents had acute fecundity problems due to starvation, or their newly born siblings were stillbirths or died in infancy, and/or their parents were widowed, or they themselves were totally orphaned. As a consequence of the selective influence of deprivation on these young people during infancy, they, like other youngsters who have suffered acute starvation, were more prone to cognitive deficiencies. In substantiation of this interpretation, we may note that Belmont documents higher levels of mental retardation and need for psychiatric care among singletons and last-borns of all families in this study, although she does not interpret this finding (Belmont, 1977).

I would conclude, therefore, that the anomaly of marked intellectual deficiencies among singletons and last-borns found in the Dutch data does not require a substantive revision of the dilution model. We do not need to revise it to include a sib-socialization component—at least on the basis of these results. Rather, most studies find only children to be either advantaged, or no different from those in two-child families, and do not suggest that last-borns on average suffer from the lack of siblings to teach, or from anything else that is very influential (Blake, 1981; Claudy, Farrell, and Dayton, 1979; Falbo, 1977; Falbo, 1978).

## IS SIBSIZE AN IMPORTANT INFLUENCE ON CHILD QUALITY?

The family-size decisionmaking model under discussion assumes not only that increasing sibsize is a *negative* influence on the quality of children, but that it is an *important* influence. Does sibsize actually have an important effect on child quality outcomes? An answer to this question also comes from my regression analyses of the surveys of adults discussed so far—the National Opinion Research Center, General Social Survey 1972–1980; the 1970 National Fertility Study; and the Growth of American Families Studies for 1955 and 1960. Total years of education achieved was the dependent variable (indicating "quality") and sibsize, parents' socio-economic status, Catholic-nonCatholic religion Southern-nonSouthern background, respondent's age, community size, and whether the family was intact when the respondent was growing up were the predictors. In all of these studies, sibsize and father's socio-economic status were the principal variables influencing the respondent's educational attainment. In most cases, the relative influence of sibsize, as shown by the beta coefficients, was equal to, in one case greater than, and in one case slightly less than our indicator of father's SES.

In terms of standardized coefficients, what this means is that, other things being equal, every increase in a standard unit of sibsize must be offset by an increase in a standard unit of father's SES, if the person's educational attainment is not to decline. Put in terms of actual metrics instead of standardized coefficients—numbers of children and points on the occupational scale for fathers—in the NORC 1972–1980 data, given an average score on all other variables

in the model, a white male with no siblings would graduate from high school if his father scored 17 on the Duncan Occupational Prestige Scale, but if he had four siblings his father would have needed a score of 37 in order for the son to achieve high-school graduation. It is worth noting that, in the Occupational Changes in a Generation analyses by Featherman and Hauser, the coefficients showing the relative importance of sibsize and father's occupational status (in relation to respondent's educational attainment) are similar to our analyses (Featherman and Hauser, 1978, pp. 242–243).

There thus seems to be little question that, among whites, even when major cultural, period, and socio-economic background variables are taken into account, increasing sibsize is an important negative influence on a person's educatonal attainment. Moreover, our results suggest strongly that there is no family size that is too small for the production of quality. The only child, pronatalist ideology to the contrary notwithstanding, is shown in study after study to be as advantaged, or more so, as children from two-child families, and clearly more advantaged than children from larger sibsizes.

Hence, we seem to have an answer to two of our questions. If people believe that they can trade off child quantity for child quality they are, indeed, on the right track; and sibsize is an important influence on the quality of children. In fact, sibsize is more important than our regression coefficients indicate. This is because, at the time of the onset of childbearing (at the time when family-size decisions are most crucial), a couple's family size is still a decision variable, whereas a major share of the socioeconomic status they will transmit to their offspring has been determined. So, choosing their family size is a way in which all parents can still affect the quality of their children, regardless of the relative importance of their socio-economic position which is, or rapidly on the way to becoming, a fait accompli.

## THE INFLUENCE OF SIBSIZE ON INTERVENING VARIABLES

Although it is important to establish the nature of the relation between sibsize and a child's educational attainment, and it is also necessary to assure oneself that sibsize is not a relatively trivial influence on achievement, a primary focus of our interest is in specifying just what it is that sibsize affects. In what manner does sibsize impinge on variables intervening between parental background and educational outcomes?

Fortunately, there is a distinguished research literature documenting such variables. This work, pioneered by William Sewell and various colleagues (Sewell and Shah, 1967; Sewell and Shah, 1968a; Sewell and Shah, 1968b; Sewell, Haller, and Portes, 1969; Sewell, Haller, and Ohlendorf, 1970; Sewell, 1971; Sewell and Hauser, 1972; Sewell and Hauser, 1975; Sewell, Hauser, and Featherman, 1976; Sewell and Hauser, 1980) has not until very recently (Sewell, Hauser, and Wolf, 1980) included sibsize as a predictor. However, the basic "Wisconsin model" has shown the significance of specifying the influence of parental background (occupational and/or educational achievement) on intervening variables such as perceived parental pressure for academic achievement, child's ability, and child's grades as these impinge on the child's educational aspirations (and, ultimately, on educational attainment).

I have adapted the Wisconsin model to include sibsize and to disaggregate "settings" (a 19-item index of the cultural and physical richness of the home environment) from an overall parental socioeconomic index. The purpose here is to attempt to measure how parents' socioeconomic level is actually expressed in the home environment, and takes advantage of a critique of the Wisconsin model by Joe Spaeth (1976). The parents' level of social status is indicated by their edu-

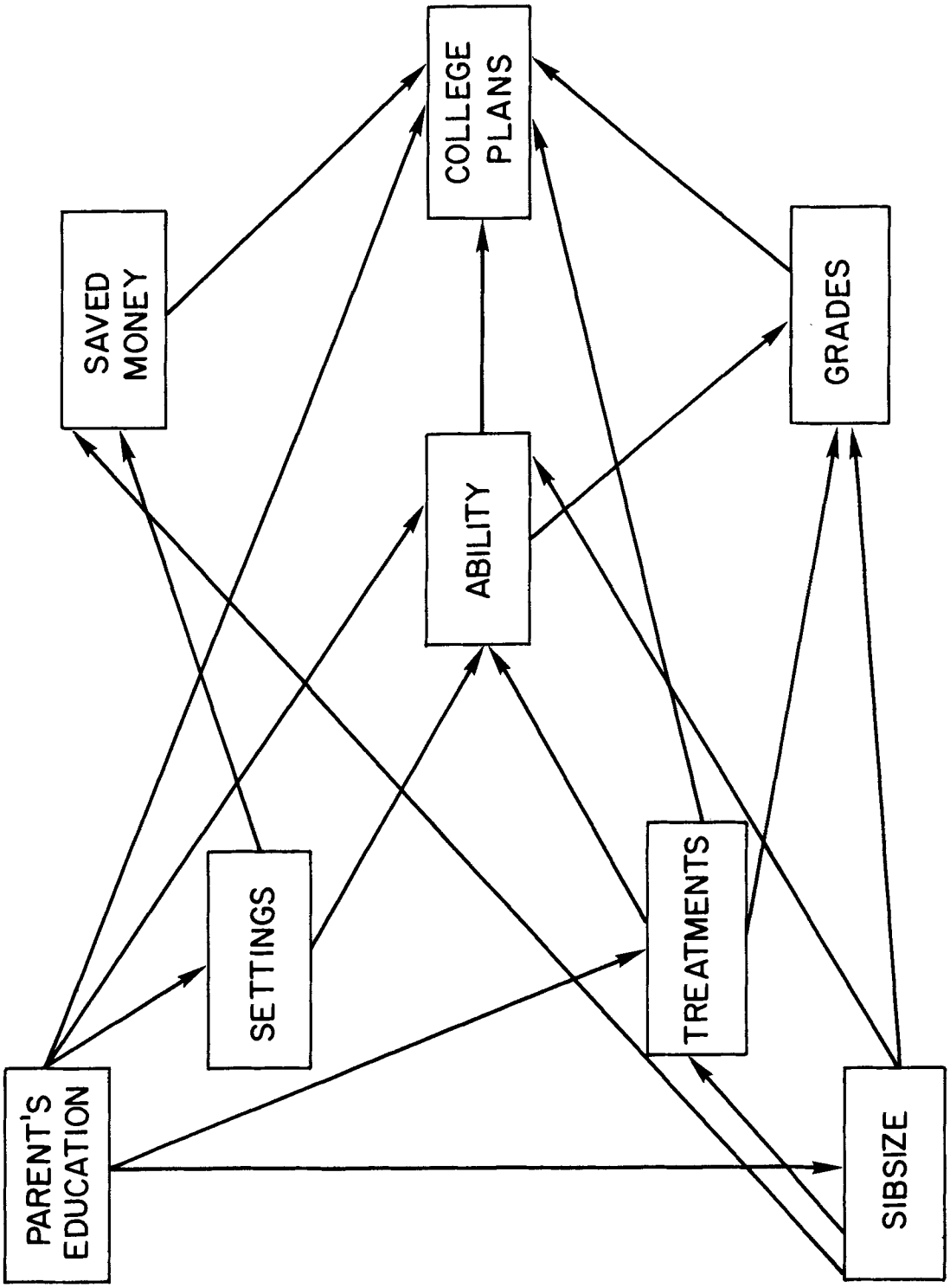


Figure 8.—Schematic Path Diagram of Effects of Selected Variables on College Plans among 10th Grade Boys, University of Michigan, Youth in Transition data tape.

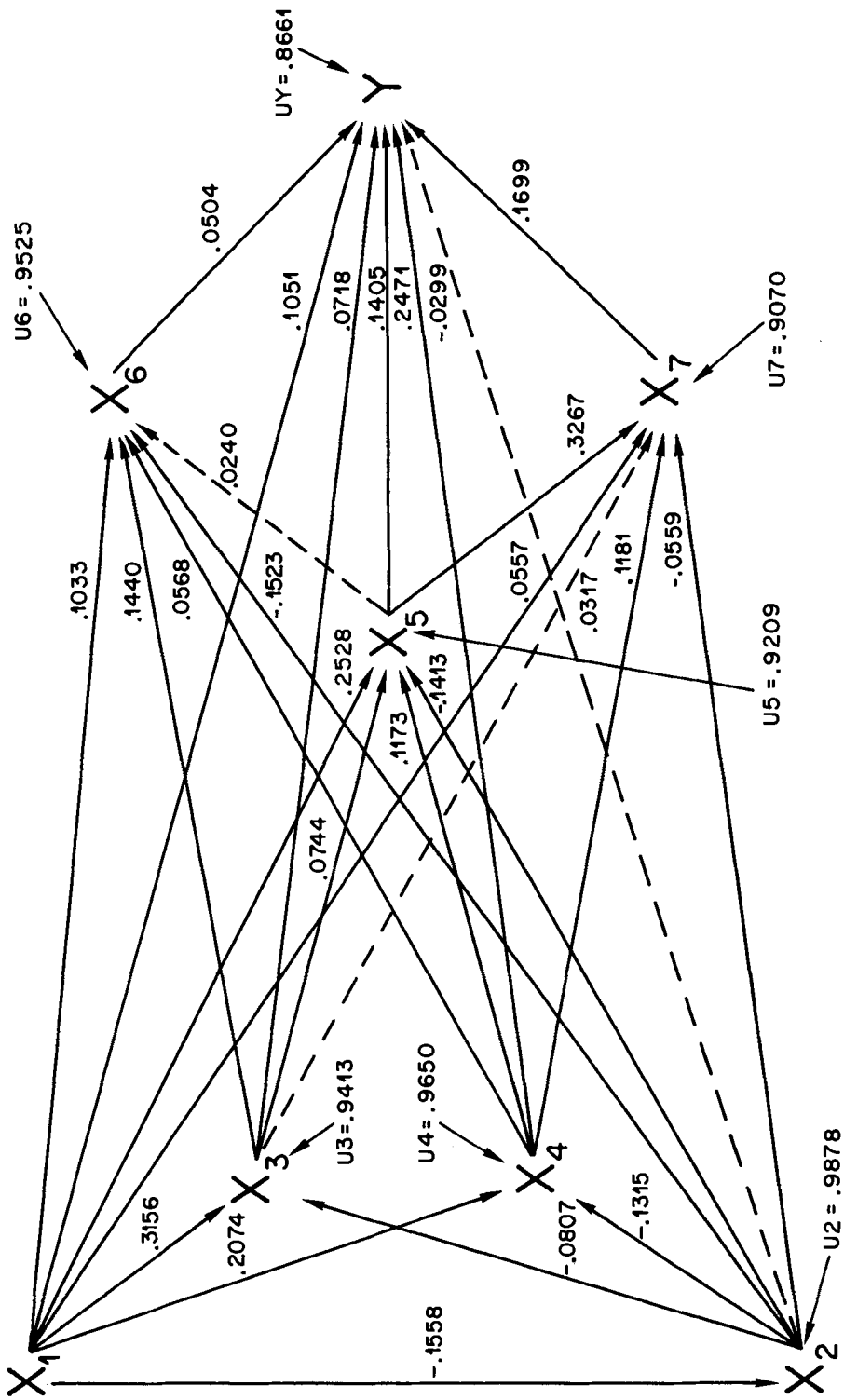


Figure 9.—Path Diagram of the Effects on College Plans (Y) of Parents' Education ($X_1$), Sibsize ($X_2$), Settings ($X_3$), Treatments ($X_4$), Ability ($X_5$), Money Saved ($X_6$), and Grades ($X_7$). Analysis by the Author is Based on a Tape of the University of Michigan, Youth in Transition Survey.

Table 1.—Correlation Matrix. Variables in Path Analysis of Youth in Transition 10th Grade Boys

| | | $X_1$ | $X_2$ | $X_3$ | $X_4$ | $X_5$ | $X_6$ | $X_7$ |
|---|---|---|---|---|---|---|---|---|
| PARENT'S EDUCATION | $X_1$ | 1.0000 | | | | | | |
| SIBSIZE | $X_2$ | -0.1558 | 1.0000 | | | | | |
| SETTINGS | $X_3$ | 0.3282 | -0.1299 | 1.0000 | | | | |
| TREATMENTS | $X_4$ | 0.2279 | -0.1638 | 0.2056 | 1.0000 | | | |
| ABILITY | $X_5$ | 0.3259 | -0.2096 | 0.1998 | 0.2133 | 1.0000 | | |
| SAVED MONEY | $X_6$ | 0.1950 | -0.2014 | 0.2141 | 0.1400 | 0.1305 | 1.0000 | |
| GRADES | $X_7$ | 0.2082 | -0.1565 | 0.1468 | 0.2162 | 0.3881 | 0.2002 | 1.0000 |
| COLLEGE PLANS | Y | 0.2806 | -0.1623 | 0.2248 | 0.3644 | 0.3206 | 0.1792 | 0.3251 |

Table 2.—Decomposition of Effects in a Path Model of College Plans: 10th Grade Boys, Youth in Transition

| DEPENDENT VARIABLE | | PREDETERMINED VARIABLE | | TOTAL EFFECT | INDIRECT EFFECTS VIA $X_2$ | $X_3$ | $X_4$ | $X_5$ | $X_6$ | $X_7$ | DIRECT EFFECT |
|---|---|---|---|---|---|---|---|---|---|---|---|
| SIBSIZE | $X_2$ | PARENT'S ED | $X_1$ | -.1558 | -- | -- | -- | -- | -- | -- | -.1558 |
| SETTINGS | $X_3$ | PARENT'S ED | $X_1$ | .3282 | .0126 | -- | -- | -- | -- | -- | .3156 |
| | | SIBSIZE | $X_2$ | -.0807 | -- | -- | -- | -- | -- | -- | -.0807 |
| TREATMENTS | $X_4$ | PARENT'S ED | $X_1$ | .2279 | .0205 | -- | -- | -- | -- | -- | .2074 |
| | | SIBSIZE | $X_2$ | -.1315 | -- | -- | -- | -- | -- | -- | -.1315 |
| ABILITY | $X_5$ | PARENT'S ED | $X_1$ | .3256 | .0251 | .0234 | .0243 | -- | -- | -- | .2528 |
| | | SIBSIZE | $X_2$ | -.1627 | -- | -.0060 | -.0154 | -- | -- | -- | -.1413 |
| | | SETTINGS | $X_3$ | .0744 | -- | -- | -- | -- | -- | -- | .0744 |
| | | TREATMENTS | $X_4$ | .1173 | -- | -- | -- | -- | -- | -- | .1173 |
| GRADES | $X_7$ | PARENT'S ED | $X_1$ | .2082 | .0198 | .0177 | .0324 | .0826 | -- | -- | .0557 |
| | | SIBSIZE | $X_2$ | -.1272 | -- | -.0046 | -.0205 | -.0462 | -- | -- | -.0559 |
| | | SETTINGS | $X_3$ | .0560 | -- | -- | -- | .0243 | -- | -- | .0317 |
| | | TREATMENTS | $X_4$ | .1564 | -- | -- | -- | .0383 | -- | -- | .1181 |
| | | ABILITY | $X_5$ | .3267 | -- | -- | -- | -- | -- | -- | .3267 |

Table 2.—(Continued)

| DEPENDENT VARIABLE | | PREDETERMINED VARIABLE | | TOTAL EFFECT | INDIRECT EFFECTS VIA $X_2$ | $X_3$ | $X_4$ | $X_5$ | $X_6$ | $X_7$ | DIRECT EFFECT |
|---|---|---|---|---|---|---|---|---|---|---|---|
| COLLEGE PLANS | Y | PARENT'S ED | $X_1$ | .2806 | .0189 | .0314 | .0607 | .0499 | .0052 | .0094 | .1051 |
| | | SIBSIZE | $X_2$ | -.1215 | -- | -.0080 | -.0385 | -.0279 | -.0077 | -.0095 | -.0299 |
| | | SETTINGS | $X_3$ | .0991 | -- | -- | -- | .0146 | .0073 | .0054 | .0718 |
| | | TREATMENTS | $X_4$ | .2931 | -- | -- | -- | .0231 | .0028 | .0201 | .2471 |
| | | ABILITY | $X_5$ | .1973 | -- | -- | -- | -- | .0012 | .0556 | .1405 |
| | | MONEY SAVED | $X_6$ | .0504 | -- | -- | -- | -- | -- | -- | .0504 |
| | | GRADES | $X_7$ | .1699 | -- | -- | -- | -- | -- | -- | .1699 |

cational attainment. "Treatments" is a measure of the youngster's perception of active personal encouragement by parents for college attendance. The child's ability is indicated by a combined index of cognitive achievement tests, and grades are those for the previous year. I have also included a variable relating to whether the youngster said he had or had not saved money, on the assumption that this should have a relationship to the dependent variable, college plans. The data base is the University of Michigan study of 10th-grade boys, *Youth in Transition.*

The schematic path diagram in Figure 8 indicates the main features of the theoretical model. My hypothesis is that the overall importance of sibsize inheres virtually entirely in its indirect effects on the intervening variables. In other words, there seems to be no theoretical reason why sibsize should have a direct effect on educational aspirations (or, for that matter, on educational attainment). In the model, parents' educational level and sibsize are predetermined. The home environment (settings) and the child's perception of parental encouragement to go on to college (treatments) are assumed to be antecedent to ability, money saved, and grades. My reason for placing perception of parental encouragement prior to ability is that I think parental encouragement doubtless materially affects measured ability, and although encouragement is also assuredly *stimulated by* ability, it probably is influenced more by nonobjective factors than by testing results. Hence, in a recursive model, I think it can be argued that one errs less than by assuming that encouragement depends on whether the child is of a certain demonstrable level of intelligence.

Figure 9 shows the path diagram, Table 1 (the correlation matrix), and Table 2 (the decomposition of effects in the overall path analysis). A number of points are worth noting here. First, among the intervening variables, "treatments" has the most important total and direct effect on college plans. Second, of the two predetermined variables assumed to influence treatments (parents' education and sibsize), sibsize has a negative influence on "treatments" that is almost half as large as the positive effect of parents' educational level (coefficient for sibsize is $-.0385$ and for parents' education level, .0607). Third, the next most important intervening variable is ability and, in this case, too, sibsize has a negative effect that is almost half as great as the positive effect of parents' educational level (the coefficient for sibsize is $-.0279$ and for parents' educational level it is .0499). Sibsize also has a negative effect on ability that is slightly larger than the positive effect of "treatments." For the remaining variables, sibsize has some negative effects on every variable in the model.

Interestingly, the analysis suggests that "settings"—cultural and physical advantages in the home—are not nearly as important for academic aspirations as is the child's perception of academic encouragement by parents—"treatments." This indicates that parents cannot very successfully offset the negative effect of sibsize on "treatments" by simply providing "settings." In effect, children are more motivated by parental attention, interaction and encouragement, than by passive environments of cultural and material "advantage." Insofar as increasing sibsize dilutes that uniquely important personal input, it has a major negative consequence for which it is apparently difficult to compensate.

Subsequent to this analysis, William Sewell, in a personal communication, has called my attention to the 18-year follow-up study of the original Wisconsin high school seniors on which most of his elaboration of the Wisconsin model was based (Sewell, Hauser, and Wolf, 1980). This follow-up analysis includes sibsize for the first time and involves a considerable expansion of the original model. Of interest, here, however, is the fact that,

with regard to college plans as a dependent variable, the results are very similar to our path analysis of the *Youth in Transition* data (Sewell, Hauser, and Wolf, 1980, p. 566). Parental encouragement is the most important influence in the Wisconsin data on men, and among the predetermined variables affecting parental encouragement, sibsize ranks close to father's occupational status in importance (the coefficient for sibsize is −.106 and for father's occupational status it is .150).

## SOME IMPLICATIONS OF THIS RESEARCH

Since my discussion is a brief preview of an extensive research undertaking, let me close now with some selected conclusions about and implications of the work outlined here.

First, I think it documents that, insofar as people generally think in terms of a dilution model, they are right. Moreover, at least with regard to the variables studied here, there seems to be, on average, no important positive feedback from the socialization by siblings of each other. That is, neither do last-borns suffer from not having a sibling to teach, nor do they gain in important ways from having older brothers and sisters. This is suggested by the fact that the regressions we did on each sibsize separately, which included birth order as a variable, indicated that it was the least important predictor in the equations and, frequently, not even statistically significant. These results, for intelligence and educational attainment, appear to extend to other dependent variables in our work such as life satisfaction, social adjustment, alienation and occupation achievement (Blake, 1981).

Findings may be different where siblings in the same family are studied. But, the information so far available on unrelated people implies that the effect of sibsize far transcends any effects of birth order, and that unless one had some very cogent theoretical reasons for studying birth order, the empirical results would hardly spur one on. Reviews of some of the problems involved in research on the effects of birth order may be found in Adams, 1972; Clausen, 1965; Schooler, 1972; and Terhune, 1974.

Second, by extension, our research indicates that the only child is not disadvantaged because of the absence of brothers and sisters. Although singletons do not show up in all of the studies as *more* advantaged than children from two-child families, and sometimes they appear to do slightly less well, I personally believe that this is because of selection, as I have already discussed. In all studies (with adequate controls) but the Dutch one, only children do very well indeed and, typically, surpass those from families of more than two siblings. These results thus suggest that, if couples wish to have only one child, it seems reasonable to expect no deleterious consequences. Indeed, my work on educational level is further bolstered by separate analyses of other characteristics of the only child (Blake, 1981; Claudy, Farrell, and Dayton, 1979; Falbo, 1977 and 1978).

Third, these findings imply that people's family-size decisions, in the context of the typical life cycle, are the most important of the background influences on child quality we have studied. Not only can sibsize be shown to compete strongly with parental SES on a statistical basis, but we must remember that, by the time couples are deciding how many children they want, their socio-economic status is typically no longer very readily affected by choice. Equally, if we consider other variables in our model, although one is better off having had Jewish than Catholic parents when it comes to educational attainment, such religious differences are not readily rearranged as a couple is about to embark upon reproduction. A young Catholic couple normally does not have the option of suddenly taking on all the unmeasured characteristics associated with being Jewish. This couple *does* have the choice of not having six children, however.

Fourth, our model of the intervening variables that sibsize affects indicates that it may not be so easy to avoid the negative consequences of large families, even if one is well-off. On average, children from large families have less ability, lower grades, and apparently receive less academic encouragement from their parents, even when parental background and "advantages" in the home are controlled. Large families are considerably more deleterious to a child's educational attainment than are broken homes, suggesting that, on average, a simple nostrum of familistic traditionalism—stable marriages and high fertility—is not the ticket to producing quality offspring.

It is interesting to ask why the effect of broken homes is not greater, if the dilution model is valid. One reason may be that, although a mate (typically a husband, since most children of broken homes were with their mothers) presumably contributes to the personal interventions and treatments that are so important in childbearing, he also makes demands himself. He is a diluter of a mother's attention and personal resources, as well as a contributor to childrearing. Moreover, a certain amount of the theoretical advantage of unbroken homes may be drained off in negotiation and transaction costs between the parents as they attempt to cope with childrearing jointly. It may thus be that the advantages of single parenthood partially offset the disadvantages, whereas on average there appear to be no similarly offsetting compensations in the case of large versus small families.

Finally, although it is inappropriate to extrapolate the conclusions of this research to developing countries where the family structures are so different from our own, it seems inconceivable that the results are totally irrelevant either. The findings I have discussed relate to a period when, for whites at least, the United States was the most open, richest, and sociologically and politically advantaged country in the world. Yet, even under these relatively ideal conditions, coming from a large family had negative effects on the quality of the people we have studied. It is hard to imagine that, in countries much less favored, differences in kinship structure can offset the deleterious impact on human capital of large family size. Hence, one practical advantage of research such as this may be to add to the all-too-modest empirical literature on the individual consequences of high fertility—a literature that will not necessarily influence the bulk of people in developing countries, but may have some effect on their policy-makers. Personally, I doubt whether, on average, any country, or any ethnic or racial group, can meet the human capital demands of the modern world and simultaneously maintain a statistical norm of large families.

## ACKNOWLEDGMENTS

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