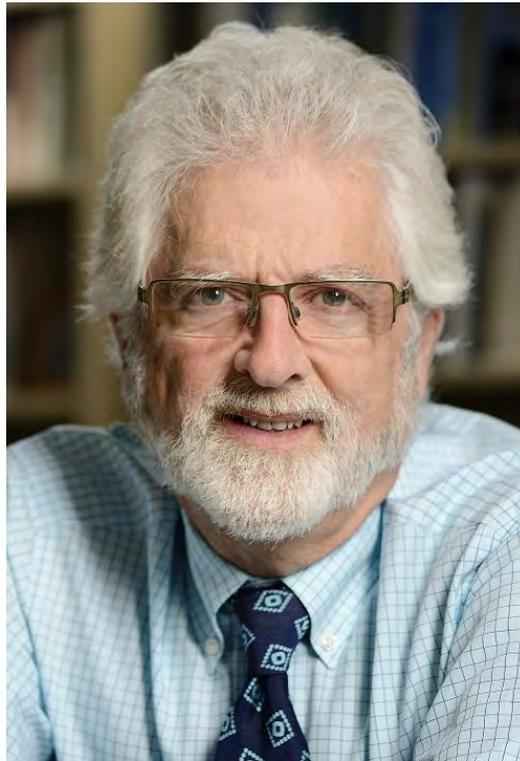


DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Andrew Cherlin PAA President in 1999



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

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

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

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

ANDREW CHERLIN

PAA President in 1999 (No. 62). Interview with Karen Hardee and Dennis Hodgson at the Hilton Hotel, 720 S. Michigan Avenue, Chicago, IL, April 27, 2017.

CAREER HIGHLIGHTS: Andrew J. Cherlin was born in 1948 in Hartford, Connecticut. He received his B.S. in Engineering and Applied Science from Yale University in 1970. After teaching high school for two years he enrolled in graduate school at UCLA, where he received his M.S. in Sociology in 1974 and his Ph.D. in Sociology in 1976. That year he was hired as an Assistant Professor of Sociology at Johns Hopkins University. He was promoted to Associate Professor in 1982, and then to Professor in 1986. In 1993 he was named the Benjamin H. Griswold III Professor of Sociology and Public Policy at John Hopkins University.

Dr. Cherlin is an award-winning family demographer. He has written widely on this topic during a period of intense change in family life in the U.S. and the world. During the first two decades of his career, much of his research and writing focused on union formation and dissolution. In 1978 his paper “Remarriage as an Incomplete Institution” appeared in the *American Journal of Sociology*, which remains one of his most highly-cited publications. In 1981 he published the first edition of *Marriage, Divorce, Remarriage*, an overview of trends in union formation and dissolution since the mid-twentieth century, a volume popular among course instructors as well as scholars and which he revised in 1992. He collaborated with Frank Furstenberg on many publications, including the 1991 book, *Divided Families: What Happens to Children When Parents Part*. In an interdisciplinary study with several collaborators, he published “Longitudinal Studies of Effects of Divorce on Children in Great Britain and the United States” in *Science* in 1991. In 1993 he received a Merit Award from the National Institute of Child Health and Human Development for his research on the effects of family structure on children. His most recent books, as of the date of this interview are: Andrew J. Cherlin. 2009, *The Marriage-Go-Round: The State of Marriage and the Family in America Today*, New York: Alfred A. Knopf; Andrew J. Cherlin. 2013, *Public and Private Families: An Introduction*, New York: McGraw-Hill, Seventh Edition; and Andrew J. Cherlin. 2014, *Labor’s Love Lost: The Rise and Fall of the Working-Class Family in America*, New York: Russell Sage Foundation.

HODGSON: We’re here at the 2017 PAA meetings and this is part of the PAA Oral History Project in which we’ve been interviewing past presidents of the PAA. We go all the way back to 1948. Today, we have the great pleasure of interviewing Andrew Cherlin, our president of the PAA in 1999. Currently, if I’ve got it right, you’re the Benjamin H. Griswold III Professor of Sociology and Public Policy at Johns Hopkins University.

What we’d like to do is start off with a series of biographical-type [questions], in terms of how you got interested in the field and a description of how your career progressed in the context of your interests. So can you tell us a little bit about, say, where you grew up and how you got into an interest in—particularly in your case—family issues?

CHERLIN: I was actually an engineering major as an undergraduate. And I realized just too late to switch my major that I really didn’t want to be an engineer. I got interested in city problems and urban issues. I taught high school for a couple of years, thinking about what I might want to do, and then decided that sociology would be a good way to pursue my interests.

So I went to UCLA for my graduate work in sociology and I got an interest in family issues. They were overlapping with city issues, of course. I worked with Valerie Oppenheimer, my advisor at UCLA, who was later a PAA president. [Correction: she was not PAA President, but in 2009 she was the first recipient of the Harriet B. Presser Award from the Population Association of America, a

biennial award honoring a record of sustained contribution in gender and demography. In fact, her sister, Judith Blake, was PAA President in 1981.] But I never took a demography course. I never really thought of myself as a demographer. I can now admit that I've never actually calculated a life table myself. I've looked at many of them—or a Lexis diagram, which to me is, you know, opaque. So why am I then a demographer?

Well, after I finished my Ph.D., I was interested still in family issues and I was looking around for an intellectual approach that fit the way I was thinking about the issues. I went to the meetings of the family studies association that is called the National Council on Family Relations, and didn't really find anything that I particularly grabbed onto, intellectually. So I wandered into the PAA annual meeting once, and suddenly found a very good group of people, many of them junior people like myself, who were interested in family issues and approached them from what I saw then was a demographic perspective. And I thought, this is for me. Those people were people like Linda Waite and Arland Thornton, who were then also assistant professors who became presidents of PAA afterwards, people like Suzanne Bianchi and Sara McLanahan [both of whom also subsequently became PAA Presidents]. So I found a number of people who were interested in all that. Now, I can continue more but, you know, you can—

HODGSON: Well, can I just go back a speck?

CHERLIN: Yes.

HODGSON: It's kind of interesting, that you graduate from Yale, and then you actually are a high school teacher for several years.

CHERLIN: Right.

HODGSON: Can you come up with why you picked UCLA as your graduate school?

CHERLIN: Yes. I picked UCLA as my graduate school because my wife wanted to go to California, and I was rejected at Stanford and Berkeley. I think I looked very strange. I was an engineering major who was teaching high school, had no background in demography. I think I looked like a big risk to admissions committees. So I went to UCLA, which worked out well because it was the Bill Walton years, and the basketball team was great. And I got to work with Valerie Oppenheimer. So it was a good decision, in retrospect.

HODGSON: Now, Valerie Oppenheimer, she was a young junior faculty at that time. I mean, she wasn't one of the big names in the field at UCLA.

CHERLIN: Yes.

HODGSON: You had Rob Turner, and you had—

CHERLIN: Right.

HODGSON: How quick was it, your finding Valerie Oppenheimer?

CHERLIN: It took me a year or two. There was another professor named Robert W. Hodge, who moved to Chicago, who I worked with. I took a wide variety of courses with the very broad UCLA department but gravitated toward Valerie Oppenheimer, who hired me as a research assistant. She was at that time a junior faculty member. She was not getting a lot of respect from the department or even

yet the profession. As a woman, she had a very hard time converting her non-tenure track position into a tenure-track position and then getting tenure. So she was pretty junior at that point. Judith Treas, another demographer, and I were her first graduate advisees.

HODGSON: And she was just coming out with her big book on the women's labor force.

CHERLIN: She had actually just come out with her big book called *The Female Labor Force in the United States*. It was a hard-to-get monograph published by the Institute for International Studies at Berkeley, but a lot of people in the field were reading it.

HODGSON: Right. And did she teach a family course at UCLA?

CHERLIN: She taught a family demography course, which I did take, yes. She was interested in the labor force, of course. Women's roles in the labor force was the most interesting thing she was into. So that's where I gravitated toward all this.

HODGSON: And how about your dissertation?

CHERLIN: My dissertation was on economic factors in divorce—here again, you know, taking off from Oppenheimer's interest in women's labor force participation and how it affected families. So that was my dissertation. It came out of my interest in what was happening in cities, the rise of single-parent families and so forth.

HODGSON: Right. And that was your first publication I think in *Demography*.

CHERLIN: Right.

HODGSON: It was on kids and divorce.

CHERLIN: Yes. That's right.

HODGSON: What do kids do vis-à-vis divorce. I thought that was interesting. And so there was really one big mentor, Valerie Oppenheimer, that really got you going.

CHERLIN: Yes. And I think to some extent, the PAA was my mentor after I graduated. Because when I started going to the meetings in 1976, my perspective really had not gelled. And as I said, I didn't think of myself as a demographer. But by going to the meetings and keeping in touch with and exchanging information with other people there, I think it helped a lot.

HODGSON: We interviewed Arland Thornton yesterday. And he came up with a very similar story, how he got interested in the family. And he was saying, well, he went to a PAA meeting. And he happened to sit in on a session that Paul Glick gave. And he said, Oh, my God, this is what I really like. And it was that one session that really turned his interest to family [demography]. And you're coming up with kind of a similar type story, except you did have Valerie Oppenheimer.

CHERLIN: Well, here is what was happening with the PAA. In the 1960s and early '70s, the PAA was still about the population problem, how to reduce the birth rates. And a huge proportion of all the people who attended the meeting or were presenting papers were presenting on fertility—some on mortality, because we wanted to get death rates down. Fields like family and household demography and also migration were very underdeveloped. There were a lot of changes happening right at that time

in the American family. And so there was a good group of people who were interested in these changes. And I think that group attracted people like Arland Thornton and me at the time.

HARDEE: And you mentioned those several young academics at the time. How important has that network been to you throughout your career? Have you all stayed in touch, even being in different places, and worked together and exchanged ideas?

CHERLIN: Oh, definitely we have. And the group of family and household demographers has expanded greatly. That is my network. That's why I am a PAA member. And that network has expanded greatly since the early '70s, so that there have been many PAA presidents now who would do family and household demography, and there are lots of sessions on it.

It was not always like this. It was very much dominated by fertility, for very good reasons in the '60s and '70s. It's really only as the huge family changes happened and as the American birth rate declined that the PAA diversified a bit into family and household demography. And I have stayed in touch with these various people who I have known for thirty or forty years and who are my main intellectual colleagues.

HODGSON: Now, if you think about population centers and the funding for population centers, you could see in the context of—particularly when you're doing your graduate school, that you had major population centers. They all had an international focus and they all had a family planning/fertility focus. And you went to UCLA, so in some ways, that's probably nice for you because you weren't funneled into a particular area that wasn't of great interest to you.

CHERLIN: Right.

HODGSON: You were lucky.

CHERLIN: UCLA did not have a population center at that time. They didn't see themselves as population people. I didn't see myself as a demographer. When I got to Johns Hopkins, I started teaching a course in demography. Had to do a lot of reading, but did it well. And when people asked me if I was a demographer, for the first couple years, I said no. And then I got tired of saying no, so I said yes. And then several years after that, I realized I *was* a demographer. Exactly how that happened, I'm not sure, but it was a transformation that did occur.

HODGSON: Now, I think from the first time you appeared in Johns Hopkins, you had a joint appointment, so you were in their public health/population/reproductive health department.

CHERLIN: Yes.

HODGSON: Is that where you taught the demography course?

CHERLIN: Yes. I taught a demography course that had graduate students in public health and some undergraduates in it. It was a social demography course. I taught it with John F. Kantner, who was studying adolescent fertility. And at Hopkins, as everywhere, there was a big emphasis on fertility and bringing down the growth rate.

HODGSON: Phyllis Piotrow was there at the time, I think.

CHERLIN: Yes, yes.

HODGSON: And USAID contracts.

CHERLIN: Yes. Definitely.

HODGSON: Some interesting stuff going on at Johns Hopkins. And you were there from the beginning, but it sounds like you were more focused on the sociology department and family, and this was an additional add-on that you got to teach the demography/population course?

CHERLIN: Right. Well, most of the demographers at Johns Hopkins were in the School of Public Health at the time. And I certainly identified with all of them. I didn't do any research on fertility but did care about it, and taught about it.

HODGSON: Right.

CHERLIN: Yes, being in the arts and sciences school in sociology has been more important to me, but I still retain a joint appointment in public health.

HODGSON: Now, if you think about progression, in terms of topics that really interest you over time, we can look at your books and see a lot of continuity. So that that first one, *Marriage, Divorce, Remarriage*, and I think you did a joint one with Frank Furstenberg on marriage and divorce [*Divided Families: What Happens to Children when Parents Part*]. *The Marriage-Go-Round* is more comprehensive, but [there are] a lot of similarities. And then you've got some new ones, the grandparent book with Frank Furstenberg, too. How did that combination—you and Frank writing on these topics—come about?

CHERLIN: As a graduate student, I had read some work that Frank Furstenberg was doing. I think he's eight years older than me.

HODGSON: Right.

CHERLIN: He was probably an associate professor by the time I met him. I wrote him a letter, which I still have a copy.

HODGSON: So he was at Penn?

CHERLIN: He was at Penn. And I wrote him a letter in '76 or '77. And I said, We really ought to talk. And we arranged to meet at the 1977 or '78 American Sociological Association meetings, found that we had a lot in common, and collaborated for many years after that. This was somebody I identified as doing work I was interested in, and sent a letter to, and things worked out well.

HODGSON: The grandparent book was so interesting because nobody had actually really written much on that particular family role, grandparent. Who thought of that one—let's do a book on grandparents.

CHERLIN: Frank Furstenberg had the idea of doing a book on grandparents. He had done a national survey of children. And he said, "Why don't we see if we can get funds to interview the grandparents of the children that I've just surveyed and write a book about it?" So we applied and did get some funds from the National Institute on Aging to interview the grandparents of the children, interviewed them and wrote a book. It was Furstenberg's idea to do that. And he invited me to collaborate with him, which I was happy to do.

HODGSON: And if you think about your long-term interest in marriage and I'd say divorce and remarriage as well—at the time you began, the domestic big policy issue was a dramatic increase in divorce that was occurring. There was a lot of publicity about that, a lot of concern about that. Now you've been looking at these trends over a long period of time. Here we are in 2017. What's changed in the context of not just your writing, but your thinking about divorce and marriage in the U.S. that has motivated you—

CHERLIN: Here's what's changed about divorce and remarriage in the U.S. In the '60s and '70s when I was in graduate school and an assistant professor, the divorce rate was rising very heavily, very much across the board in the U.S. And I was interested in it in part because the biggest increases were among people with the least education and least income.

In about 1980, the divorce rate peaked. I predicted wrongly, Furstenberg predicted wrongly, that those increases would not be reversed. But in fact the divorce rate has declined. And the heaviest decline has been among the well-educated, so that among people with college degrees, the divorce rate is probably now back to what it was in the 1960s.

What we've seen since 1980, what I did not anticipate in my early career, which I think no demographer anticipated well, was the strong class division that we're now seeing in family life, with lower divorce rates, more children being born in marriage to the college educated, and almost a different pattern of family life among people without a four-year college degree. That's a huge change that nobody really anticipated during the time when I was in grad school and was a young professor.

HODGSON: Okay. Now, if you think about your most recent book [*Labor's Love Lost: The Rise and Fall of the Working-Class Family in America*], where it's basically a history of the working-class family, you can almost see this increasing relevance of social class in your thinking, particularly as it applies to the family. It's obviously present there in the context of divorce. But is that what's driving you a little bit in terms of that new focus on, Well, we've got to take a look at what's going on, particularly in the context of working-class families in the U.S. and—well, I'll let it go at that.

CHERLIN: When I first got interested in families, it was in the context of my interest in low-income people and in urban issues. But then I got very interested in issues that were happening to all classes: the decline in marriage, the rise in divorce. I kind of dropped the cities in the class part for fifteen, twenty years without really thinking about it.

The book I've written recently on the working-class family in a sense gets me back to the perspective on family life I wanted to take as a graduate student, which is a class-based perspective. And the reason I was drawn to it is because there were huge changes in the family lives of working-class Americans, which are making them look more and more like Americans with the least education, making them look less like the college educated. That got me interested in writing a book about what's happened to the working-class family.

HODGSON: Okay.

HARDEE: Can I ask a question?

HODGSON: Sure. Go ahead.

HARDEE: The issues that you've been working on have such policy salience, and continue to have. What role do you feel like you have as an academic to promote evidence-based policy? And you've been interviewed so many times and quoted so many times. Have you testified on the Hill? What do you feel like is the role of an academic?

CHERLIN: I think it's important for academics who work in fields that most demographers work in to make their findings known, to try to influence policy really by informing policy[makers] rather than crafting specific policies. I have a joint appointment as a professor of public policy. And I like to say the kind of research I do is good, important background research for policymakers.

Throughout my career, I've tried to keep one foot or at least one toe in the policy world and one in the academic world. It's kind of a balancing act. It's a bit difficult to do, but it's something that I pursued from the start, and I'm very glad I did. I feel as though my work is taken seriously by people who are in the policy world. That's what I want to do. But I want to always do it from the perspective of an academic researcher, a demographic researcher, and not overstep the boundaries of that, which is the balancing act that I've tried to maintain through the decades.

HODGSON: Now, I noticed that since the beginning, you make a point of doing two or three op-ed type pieces [per year] in either a magazine or newspapers. And that really is unusual, I mean particularly for someone at the early stages in their career, to take the time out, as opposed to trying to get your academic publications and tenure and all that. But you made a real effort right from the beginning. I'm going to inform people about big trends.

I really enjoyed your last one, on the increase in white death rates, in the *Times* [February 22, 2016]. That was last year, I think. And this must have been something that you consciously did: I'm not just going to write articles for my academic colleagues, I'm going to make an effort to more broadly distribute what's going on here.

CHERLIN: Yes. From the start, I have tried to write some general audience pieces. I thought that was important. I did a fellowship year, which was my last year before my Ph.D., at the Russell Sage Foundation in New York City.

One of the other fellows was a man named Victor Navasky, who was an editor of *The Nation* for several years after that. And I talked with him about writing. I decided I would like to try writing. I was not a born writer. English was my weakest subject. I had to teach myself to write. He gave me the name of some good writing books and writing guides and suggested things I ought to read, columnists who wrote well, magazine pieces that were good. And I did. I studied those. I thought about how to write. I did drafts. I worked on writing clearly and then submitted some pieces to various magazines and newspapers.

When I told a senior colleague of mine at Johns Hopkins that I was going to do that, she said, "I think writing for popular places is a great thing. *Scientific American* is a wonderful publication." I said, No, no. That's not really what I'm interested in. I think they were a little scared at Johns Hopkins that I would not concentrate on academic writing or that writing for a general-interest audience would somehow hurt my chances and my career. In fact, it has helped my career, because part of that general audience in settings like the *New York Times* is every demography professor in the country who reads your work.

HODGSON: Exactly.

CHERLIN: And, in fact, it gets your name out there. So I would say to a junior colleague, within limits, you can spend time on this without hurting your career and possibly even helping it.

HODGSON: Now, I've got a related question because you decided to write a textbook on the family. I think it was 1995. We're up to Edition 8. And I've used it in all my family courses right from the beginning.

CHERLIN: Oh, thank you.

HODGSON: It's by far the best textbook on the family.

CHERLIN: Thank you.

HODGSON: What's neat about it is that it does have a definite policy component. The title of it, *Public and Private Families*, sort of sets you—oh, this is going to be intriguing.

CHERLIN: Yes.

HODGSON: Now, you've invested a lot in the context of—it takes a lot of time and effort particularly to keep that updated.

CHERLIN: Yeah. Right.

HODGSON: How did you come up with that way of framing your general textbook on the family—because nobody else has that clear policy-type outlook.

CHERLIN: In the early 1990s, the McGraw-Hill Publishing Company got in touch with me. An editor there asked if I'd like to write a textbook on the sociology of the family. And I thought about it. I thought about the fact that at that time, I had two children who would go to college some day, and I had very little in my banking account. I was very unhappy with all the textbooks that I saw on the sociology of the family, which are all about, at that time, how to have a happy marriage, the secrets of courtship and how to pick a partner—almost nothing on any public issues.

So I said to the McGraw-Hill people, "Listen, I'll write a book for you if you let me write one that also has a more policy-oriented focus. I want to have a chapter on family policy. I want to talk about social issues like non-marital childbearing and divorce. I want to have a focus on what policymakers ought to think about that, in addition to the parts about emotion and romance and marriage." And they said okay, so I did that.

At the time I wrote that textbook, there was not a textbook out there that had a chapter on public policy. There was no textbook that had a chapter on aging, even. There wasn't much focus on other social issues like non-marital childbearing and the growth of single-parent families. It was all kind of help for you in figuring out your life—

HODGSON: And welfare changes. Everything really happened just at that time.

CHERLIN: That's right. And there wasn't much focus on government social policy that affects families. So I wrote the first edition and kept true to my frame. As you noticed, the textbook title, *Public and Private Families*, was a statement that the textbook was going to be about the public dimension of families.

HODGSON: Right.

CHERLIN: That was very unusual at the time I started to write this. But McGraw-Hill, to their credit, let me do it, and it turned out very well. It's been a big success in terms of the number of students who have used it. I'm pleased to see that you have. It's been used in more than 200 colleges and universities. And I found that I liked it more than I thought. I did it at first in part, as I said, because I would like my children to go to college and I didn't have anything in my banking account. So there were some economic reasons at the start, and I didn't know how much I'd like it, but I found I did like it.

After the first edition, it's much less of a job to revise the textbook. And I found I enjoyed the task every three years, of reading through the literature, deciding what students ought to know, and rewriting parts of the textbook. Because I liked it more than I thought, I have kept up with it. Most of the material in that textbook now was not in the first edition 20 years ago, but the perspective is still very much the same, which is, family is something that's about you, your private world, your kinship, your marriage, your partners, but it's also about public issues that affect us all. And we need to study both of them if we're going to have a well-rounded view on the family. That was not the perspective of the family field when I started this.

HODGSON: I did notice, since I used all these editions up to the 7th, that you were able to introduce into the textbook, almost right away, your new, more academic areas of interest. The findings, in terms of significant differences by social class, snuck their way in there and became more and more prominent. The textbook is actually an easy way of finding out what you're really doing with very up-to-date information. It should be more than just undergraduates reading this.

CHERLIN: Oh, thanks very much. You know, looking through all of those editions is a way to see how the field has evolved, because I was drawing on the field and embracing things. In 1996, there was a major reform of the welfare laws. I brought that into the last chapter. And as other issues have waxed and waned, they have been in there.

The textbook editions are now my judgment of what's new that we ought to know and what did we know a little bit too much of and could now be cut as we think about the field from really a social demographic perspective. I mean, I'm at heart a family demographer. And that textbook is at heart a book by a family demographer, looking at U.S. families.

HODGSON: Now, so far, we've only focused on your domestic interest. You do have a more international set of publications as well. Can you tell us a little bit about that? You're not just a U.S. family scholar. I particularly like "A Happy Ending to a Half-Century of Family Change?" that came out in *Population and Development Review* [2016] as a commentary piece about a half century of family change that really questioned what was a very optimistic perception of what's happening. That was all basically international [research] that you were talking about in that context. And you had done continually some international work. Can you tell us how that came about, how that happened?

CHERLIN: In the early 1980s, I had a fellowship at the East-West Center at the University of Hawaii in Honolulu and met demographers doing very interesting work from many Asian countries—turned my attention to that area a bit, did a bit of traveling, wrote some articles, including one in *Demography*, but then really focused on the U.S. for the next maybe twenty to twenty-five years.

But then I increasingly became interested again in what's happened around the world, because there have been enormous changes in family life, not just in the U.S., but elsewhere, as other demographers have shown. So there is increasingly a lot of good work on what had happened to families around the world. And I became interested in saying something about it.

In 1963, William J. Goode, the leading family sociologist at the time, attempted to synthesize all of the changes in the world and family life into a book called *World Revolution and Family Patterns*. He was actually Frank Furstenberg's mentor. No one had really done too much with that book. And as it approached the fifty-year mark, I thought it would be a good idea to take a look at it and see how well its predictions had held, and why they hadn't done well, in cases where they had failed. So, I did a lot of reading while on leave one semester in international family change and wrote an article on looking back fifty years at family change, where did we [predict] we were heading and

where did we go wrong and why? That turned me more toward the interesting area of international family change.

In Europe now, the big change, supposedly, is that a new egalitarian family bargain is being formed, which is a happy ending to a half century of family change. In the U.S., that may be true for college-educated people, but not so much for people without college degrees. But in any case, I am turning more now and have turned more in the last five or ten years to family change in Europe and other developed countries, but also in the fascinating changes in East Asia, the fascinating mixture of continuity and change in India, the lack of change in some ways in Africa, the variety of changes in Latin America that should be of interest to all of us who are interested in family and household demography.

HODGSON: And how about, why do you question the happy ending?

CHERLIN: In Europe, with its generous social welfare programs, there is not as much of an impoverished population or financially challenged population as there is here. And one can make generalizations about society as a whole. One can point to positive trends among the well-educated and assume that the rest of the population will follow along. In Europe, as well as in the U.S., well-educated people are still waiting until after they marry to have children.

In Europe, that looks like it might also work for the less educated. But in the United States, where there's strong class divisions and much less generous social welfare programs, it's harder for people at the bottom to get to the point of the happy, two-income, two-earner family bargain that is the center of this new family life.

It seems to me that if we're going to have the new bargain, if it's going to work anywhere, it's going to work for people who have a decent shot at a good job in the labor force, which is increasingly people with college degrees. So as I look ahead, I see perhaps a neo-traditionalist movement occurring among the well-educated, but much less of that among the not-so-well-educated in the United States. In fact, if there is a social class line, a class division in the United States in 2017, it's between people with a four-year college degree and everybody else. Whether that sticks, I'm not sure. But in the U.S., that's the way our economy, our social welfare system seem to be setting up what is a strong class division in family life.

HODGSON: With a lot of negative consequences.

CHERLIN: Yes.

HODGSON: I do remember that back in 1980, you and Frank Furstenberg made predictions about what was going to happen with divorce. So here we are in 2017. You gave us a little bit of a vision of what you think is likely to happen. You're presenting a future that is somewhat pessimistic, particularly for working-class people. From a policy point of view, what should we as a nation be doing if we want to rectify this somewhat? After looking at these trends for fifty years and studying them closely, are there things we can possibly do to make things better?

CHERLIN: Yes. You know, there has been both economic and cultural change. The recent development that I see as most productive among policy people is an agreement among conservatives and liberals that both economics and culture make a difference.

In the economic realm, people with college degrees are the winners in our globalized and automated economy. And they're the ones who have a marriage-based, stable family life these days. What we need to do is help the people who are not the winners, help them by getting them better

educated, not necessarily college degrees for all, but community college training and other apprenticeship-based programs. That's what we need to do.

On the cultural level, I do think there is a role for stressing the importance of stability in family life. And there is nothing wrong with the liberals doing that. So we need to think about both economic and cultural ways to lessen the class divide that in 2017 seems so strong among American families.

HODGSON: Do you see your future research direction going in any particular way?

CHERLIN: I will continue to be concerned about less fortunate families. I'm working up a project on children in very poor families. I'm also very interested still in the working-class family. The election of Donald Trump has focused everyone's attention on this working class because so much of his support was centered among the white working class. I thought I'd give up on the working class and move elsewhere, but it's certainly still a topic of great interest. And I'm still doing research on that.

HODGSON: Now, can we switch gears—we have some time left—and talk a little bit about the Population Association of America? You already mentioned something interesting, that your initial arrival at a PAA meeting gave you great direction [in terms of] how you would like your career to go. Can you recall when that was? What was that? About when was that first PAA meeting you attended?

CHERLIN: The first PAA meeting was either in 1975 [in Seattle] or 1976 [in Montreal]. I got my Ph.D. in '76.

HODGSON: Right.

CHERLIN: And I wandered into it, not really knowing what to expect, and found a very attractive framework. It was really a social demographic framework on family change. And that's what I found intellectually exciting. That's where I gravitated. So the PAA for me was really the center of my intellectual growth during my early career, as it was for many people. And at the time, we family and household demographers were a minority in a fertility-oriented PAA, but we grew in numbers and in research output. It's become very important for me. I think the PAA has been very influential and remains very influential in deciding the direction that young people's research will go in. I think it still has a reputation of a place to go to as a grad student or early faculty member to find out intellectually what's interesting that's happening.

HODGSON: So here we are, 2017. Your first PAA meeting was 1976. You've been going forty-one years. In the middle, you're president of the PAA, in 1999.

CHERLIN: Yeah.

HODGSON: You've got topical changes that you've mentioned, that we've lost our fertility-control type focus that was quite clearly there, and we broadened a bit. Anything else that's most noticeable to you over that course of forty-one years in which you've been looking at this association? Gender composition—if I remember back then, it was a lot more male than it is today. When you take a look at who comes to a PAA meeting, particularly new arrivals, we seem to have shifted significantly, in terms of sort of the gender composition of who we are as PAA members. Anything that you've noticed along those lines?

CHERLIN: Well, certainly, the gender composition has shifted, but in the part of the PAA I was interested in, which was family and household demography, there were always strong women researchers like Linda Waite early on, Sara McLanahan, Suzanne Bianchi. I could mention many.

We've always been I think a bit more gender-balanced. And now that's true throughout the association.

We still have a strong group of people interested in fertility. We still have a strong group of people interested in mortality. What has changed is the growth of family and household demography and migration as those have become big issues.

Migration was a backwater. I remember being on an NIH review panel with Douglas Massey in the early 1980s. He was trying to bring the study of migration and segregation in demography up to the standards of the rest of the field, which he very aptly succeeded in doing. That was a big change from then to now.

HODGSON: Can you remember your presidency at PAA? What were the things you had to deal with back in 1999?

CHERLIN: Yes, I do remember my presidency in the PAA. And here is what I had to deal with. I had a program that I had to put together. There was no computer program for it. I had thirteen pieces of paper for thirteen different sessions at each time point spread around a rug in my living room. And for a day I had to go around to those pieces of paper and decide who was going to be presenting when, avoiding conflicts of interest, making sure that each time slot had a wide range of sessions. That's how it was done. It was very labor-intensive. It was also a bit smaller than today, so it wasn't quite as bad. But it was a hand-operated scoreboard, so to speak. And the president was the person who did that. You did that in addition to writing a talk for the meeting.

HODGSON: And that was it?

CHERLIN: Yeah.

HODGSON: And we didn't have—I don't recall back then—we didn't have a financial crisis at all in the organization.

CHERLIN: No. I think the organization has always been in good shape financially, as far as I know. It's well supported. It keeps its ambitions in line with its finances, which I think is a good thing, and will continue to do that.

HODGSON: So in terms of the future of the organization, any particular challenges you can see? Or you've given a very optimistic perception of where we're at, how we're about to enter into the future.

CHERLIN: Well, I think that the association is healthy and the demographic perspective is healthy, but in an odd sort of way, we are losing our core, which is people who identify strongly as demographers first and other things second.

That core was always about technical training that you needed to understand fertility. Basically, to understand the birth rate and why it had been going up or going down. Princeton was a classic example of a place that taught people the techniques of demography. And it's still the case that methodological training in the techniques of demography, the life table and so forth, are at the core of demography, but they're no longer at the core of most of the papers that are presented at the PAA. They're no longer at the core of most of the articles in the journal *Demography*.

So our field has expanded methodologically to encompass economists who are using different methods, geographical information systems people, sociologists who are doing surveys, which has been intellectually very positive and very stimulating for us as a field, but has left us with a bit of a hole in the center that we need to think about. That is to say, what does it mean to be a demographer these days?

I was on a group who did an outside review of a leading population center at a leading university this year. And it was striking that some of the people we thought of as the strongest demography types didn't think of themselves as demographers. A couple of them said that. "I think of myself as a sociologist." "I'm into big data and systems analysis."

Who does think of themselves as a demographer these days? And what does it mean to be a demographer? If it no longer means—does it just mean taking the life table course? I think in some places, it has come to that, that if you take the demographic methods course, you're a demographer. It's almost like anthropology, which has nothing holding it together except the ethnographic method. If you do ethnographic methods and you train in it and you go off to the South Seas for two years, you're an anthropologist.

Is that what demography is? If so, it's not intellectually defined, but rather methodologically defined. And I don't think that's healthy for the field. So while we are healthy and while we are diverse, that brings a challenge of thinking of what does this field mean going forward? How does one think about a demographer? When do you become and how do you become a card-carrying demographer is, I think, an issue for us moving forward. And it will be, of course, very interesting to see how the field deals with that challenge.

HODGSON: Now, you have very nicely segued from PAA to the larger issue of demography. And in some ways, it's interesting because you're taking a perspective that tends to divide past PAA presidents. Some people are very, very optimistic about this trend that you just described. They like the idea that we've moved away from this classic definition of what demography is and we've become what they call a multi-discipline.

And it's perfectly all right if we have a context in which most of the people here today are not thinking of themselves as a demographer. "I'm an economist and this is what I like to study." "I'm a sociologist and this is what I like to study. And there are sessions on it, and I can come." They walk around and don't think about it.

I went to a population center as a grad student, so this is Cornell back in 1969, so kind of similar except it wasn't UCLA. We had that international focus. We weren't presented with demography as a multi-discipline. We were presented with demography as—you know, it was the study of population. Kind of like sociologists study society and economists study the economy, anthropologists study culture, we've got this thing where we study population. And we have these primary demographic variables and you can see how they're interrelated. You can describe exactly how population might change by looking at fertility and mortality and migration. And you have life table techniques. It was a set of data and a set of techniques, but all around a central core concept that was presented as something real. There really is something called the American population. And you study it with these techniques.

And what you're describing is a rather dramatic move away from that. In some ways, you're down to life tables as your only distinctive methodology. But we've lost our distinctive data sources as well. I mean, most demographers are not analyzing census data anymore. In fact, many European countries have done away with the idea of going around and taking a census. Everything is basically surveys and sampling, in which case our techniques are not unique. Even our methodologies aren't unique. It's widely shared among quantitative social scientists. And you think that's a problem. Is there something we can do about it if that's problematic, because it does seem to be the direction of the change.

CHERLIN: It's very true that thirty, forty years ago, there was a clear definition of what demography was, in method and in substance. In method, it was the set of techniques that allowed you to analyze population change in various aspects. In substance, it was about bringing the birth rate down, getting death rates down, looking at migration. It was very well defined, also limited.

What's happened is the association has broadened out tremendously. Everybody, myself included, thinks that's terrific, okay? It's still a vibrant, intellectual community because it has moved forward and expanded and encompassed new developments. But to what extent is it about demography or about population?

The word "population" in the PAA, what does that mean now? I know [that] when the institution was founded it meant the population problem, population growth, population change, migration. If it means more than that now, why are we still called the Population Association of America? Why not call it the Applied Quantitative Research Association of America, the Microsocial and Microeconomic Association of America? What do we mean then by population?

I'm not suggesting that's necessarily a problem. It's certainly not a problem for the PAA meetings. We certainly don't want to change the name. But there are people who use the term "demography" as well. And as the core has been lost, as those methods have become less central, one thinks about, what is it that makes you a demographer? Is there something distinctive about the field of demography now that deserves support? I think many of us are having some trouble finding that. And I think many of the more interesting people here think of themselves as another label first and demographer second. Maybe that's okay. But we do have to think about what the core is as we move on.

I think that there still can be and is a core. There are new techniques that are now being used that we need to know about, like geographic information systems, like combining genomic information with social scientific information, with big data and analyzing that. There are new sets of methods around these problems. And we certainly embrace them. But I think we have to realize that the old core demography identification no longer really fits most people, certainly not most younger people. And we might want to pause and consider that at some point as we think about the future of the PAA.

HODGSON: Do you think we need a new population crisis, if we think about environment, climate change, where numbers of people and distribution of people again become a central issue of global concern? We might get refocused on population again?

CHERLIN: We might. We are having a global migration crisis.

HODGSON: Right.

CHERLIN: And that is causing a big increase in migration, which is a traditional subject of the Population Association. So by following the lead of that crisis, we've expanded that. What we *are* doing is responding to the changes in the world around us, that made migration, segregation, family and household issues important. And that's what's happened to the Association, too. It's very healthy.

But it does mean the old core is not any longer at the core, and that the older method sequence, which is really about the kind of fertility-based life table, Lexis diagram, little i_f , little i_g , maximum fertility stuff that the Princeton people always saw, that's becoming less relevant. And I think some places are reluctant to give it up because it's the center of what demography means. But nobody really does much of that anymore after they finish the course. So we have to think then about what is demography or population research these days.

HODGSON: Those are great thoughts. Are there any questions we forgot to ask you that you would like to raise?

CHERLIN: I wanted to get in that last part about the future and what PAA means, because it was striking to go to our review of a department this year and find that the people we thought were best said they weren't demographers.

HODGSON: Now, is there anything, any advice you would give someone just entering the field for whom this is their first PAA meeting and they've got the future, 2030, 2040, 2050 as part of their academic career to look forward to?

CHERLIN: My advice is, first of all, come to the meetings and go to lots of sessions. This is one of the most serious places in terms of people attending sessions, high-quality sessions. You can learn a lot. The second is make some contacts. Contact some senior faculty or senior researchers and ask if they would meet you for a cup of coffee. This is I know hard to do, okay, but I did that when I was a junior faculty member, and nobody ever says no.

There is no senior person who will say no to meeting you for ten minutes or having a cup of coffee. And you can learn a lot from them. And they get to know you. So be more aggressive than you'd like to be. Get a little bit out of your comfort zone, not only come to the meetings, but try to make some contacts. Most of the people in your generation will have the new education and technique mastery that you do, but some of the older faculty members I think you'll find would be pleased to talk to you if you reach out to them.

HODGSON: That's great advice. So I think you did a wonderful job.

CHERLIN: Great.

HARDEE: Thank you. Yes.

HODGSON: Thank you for all the insights. It was really wonderful.

CHERLIN: Sure, sure. Thanks for giving me the opportunity to do this.

HODGSON: Oh, it was great.

HARDEE: And we always enjoy reading about you in the newspaper.

CHERLIN: Oh, thanks. Thanks. I've always tried to—

HARDEE: Thank you for bridging the policy and the academics.

CHERLIN: Thanks. I appreciate it. This was fun.

Videotaped by Brian P. Bruce, Sr.

Transcribed by Patricia Ann Lambros

Transcript audited and edited by Revan Schendler

Reviewed and approved by Andrew Cherlin

GOING TO EXTREMES: FAMILY STRUCTURE, CHILDREN'S WELL-BEING, AND SOCIAL SCIENCE*

ANDREW J. CHERLIN

In this article I argue that public discussions of demographic issues are often conducted in a troubling pattern in which one extreme position is debated in relation to the opposite extreme. This pattern impedes our understanding of social problems and is a poor guide to sound public policies. To illustrate this thesis I use the case of social scientific research examining how children are affected by not living with two biological parents while they are growing up. Over the last decade, I maintain, most of the public, and even many social scientists, have been puzzled and poorly informed by this debate. In particular I consider Judith Wallerstein's clinically based claims of the pervasive, profound harm caused by divorce and, at the other extreme, Judith Rich Harris's reading of behavioral genetics and evolutionary psychology, which leads her to dismiss the direct effects of divorce. Neither extreme gives a clear picture of the consequences of growing up in a single-parent family or a stepfamily.

Beginning in the early 1960s, there occurred a series of events that Samuel Preston, in a memorable phrase from his 1984 PAA presidential address, called "the earthquake that [has] shuddered through the American family in the past 20 years" (Preston 1984:451). The divorce rate in the United States began an ascent in which the risk of divorce doubled by the mid-1970s and has remained at a high plateau (or perhaps in a slight decline) since about 1980. At current rates, about half of all marriages would end in divorce (Cherlin 1992). The proportion of children born to unmarried mothers also increased; unlike the divorce rate, it continued to rise through the 1980s and the first half of the 1990s before reaching the current plateau of just under one-third (U.S. Department of Health and Human Services 1998; U.S. National Center for Health Statistics 1998). Cohabitation, once common only among the poor, became a widespread and acceptable living arrangement; about half of all young adults cohabit with a partner before marrying (Bumpass and Lu 1998). In addition, between 1960 and 1998 the median age at first marriage rose 4.7 years for women and 3.9 years for men (U.S. Bureau of the Census 1999).

*Andrew J. Cherlin, Department of Sociology, Johns Hopkins University, Baltimore, MD 21218; E-mail: cherlin@jhu.edu. This is a revised version of a presidential address presented at the annual meetings of the Population Association of America in New York City on March 26, 1999. My research on the effects of divorce on children was supported by Grant R37 HD29536 from the National Institute of Child Health and Human Development. I thank P. Lindsay Chase-Lansdale, Frank F. Furstenberg, Jr., and J. Richard Udry for comments on a previous version.

As a result of these changes, the proportion of children who spend time in a single-parent family while growing up has increased dramatically: It now stands at about 50% (Bumpass and Raley 1995). That increase has caused concern, and even alarm, among social scientists, social commentators, and policy makers. A lively debate continues about the consequences of these changes for children and about the proper public response; all sides cite the research of social demographers.

Social demographic research is cited about many other controversial topics as well. Demographers study things that are close to people's lives and about which there is great public debate, such as population growth, immigration, adolescent pregnancy and childbearing, racial segregation, the labor market, and gender equity. Consequently the public often pays attention to our findings. Most of us value this aspect of our research. We want our findings to be widely disseminated; we want our research to inform important public discussions.

Too often, however, these public discussions are played out in a troubling pattern in which one extreme position is debated in relation to the opposite extreme. As I will show, that certainly has been the case in the recent debate about family structure and children's well-being. In this article I review that debate and discuss what conclusions we are justified in drawing from the research literature on this subject.

The pattern I am talking about, however, applies broadly to a number of social issues. It passes through three stages. In the first stage, a social scientist presents an extreme view of a particular problem—it is either a total disaster or completely benign—and his or her work receives great media attention. In the next stage, another social scientist, taking a different perspective, presents evidence for the opposite extreme. This viewpoint also receives great attention. And in the third stage, news coverage and public debates lurch back and forth between these extremes as if there were no middle position worth contemplating. I believe that this pattern of going to extremes impedes our understanding of social problems and that it is also a poor guide to sound public policies.

One could argue that extreme statements are useful precisely because they attract so much attention to social issues. One could argue that, in an era of wall-to-wall special interest groups, extreme statements are needed to mobilize a constituency. One could even argue that at a time when hundreds of television channels and millions of web sites compete for people's attention, extreme statements are necessary if one is even to be heard.

I would argue, however, that extreme statements invite counterextremes, with unexpected and often undesirable results. The best-known example in population research occurred during the debates about rapid population growth in the 1960s and 1970s. Some regard that time as the glory period of demography, and indeed demographic research helped to raise public awareness of a pressing global problem. But even in that era, extreme positions sometimes backfired. I would suggest that the exaggerated predictions made by Paul Ehrlich (1968) and others in the 1960s, foretelling widespread famine and soaring mortality rates, contributed to the rise of the opposite extreme in the late 1970s and 1980s: the Panglossian claims of Julian Simon (1981) and others that population growth, far from being a problem, was a positive element. Simon's arguments, I believe, had more force because he could easily refute some of Ehrlich's exaggerated claims (see Tierney 1990).

I see an echo of that pattern in the current literature on the consequences of adolescent childbearing. According to the old wisdom of population research, adolescent childbearing led almost inevitably to poorer outcomes in adulthood. In the 1990s, a newer literature sometimes seems to suggest that teenage childbearing per se is hardly a problem at all—a conclusion sharply at variance with the perceptions of service providers who work with teen mothers (Hoffman, Foster, and Furstenberg 1993).

In this article, however, I want to illustrate my argument by focusing on the example I am most familiar with: the social scientific research on the short- and long-term effects, on children, of not living with two biological parents while they are growing up. On this topic, the past decade has witnessed a grand swing of the pendulum from one extreme to the other, which has left most of the public, and even many social scientists, puzzled and poorly informed.

During the 1980s and early 1990s, data on family structure and on child and adolescent well-being became available to social demographers from high-quality longitudinal surveys such as the Child Supplement to the National Longitudinal Survey of Youth, the Panel Study of Income Dynamics, the High School and Beyond Study, and the National Survey of Families and Households. As these data were analyzed, they yielded substantial evidence that growing up in a single-parent family or a stepfamily is associated with a lower level of well-being and poorer life outcomes than living in a family with two biological parents. The best-known set of studies was conducted by Sara McLanahan and Gary Sandefur, and was reported in 1994 in the book *Growing up With a Single Parent* (McLanahan and Sandefur 1994). These researchers found strong associations between growing up in a single-parent or stepparent family and a higher probability of dropping out of high school, of giving birth as a teenager, and, for young men, of being "idle"—that is, neither employed nor in school in the first few years after high school.

But two important questions remain. First, how much of this association is truly cause and effect, as opposed to merely reflecting unmeasured factors that influence both the likelihood of growing up without two biological parents and

outcomes such as high school graduation? And second, even if the association is causal, what proportion of children in single-parent families experience harmful outcomes? There is still strong disagreement on these questions; let us turn to them now.

PARENTS MAKE ALL THE DIFFERENCE

Over the past decade, the public has been exposed to two extreme positions—two highly publicized treatments of the subject that have reached diametrically opposed conclusions. Both were put forward initially by psychologists but were embraced by scholars from other disciplines. The first treatment came from clinical psychologist Judith Wallerstein. On the basis of a long-term study of 60 families who came to her divorce clinic in northern California, Wallerstein concluded that divorce harms most of the children who experience it, and that the harm is clearly caused by the divorce and by how the divorced parents act.

Wallerstein had been writing insightful clinical case studies for a decade when she suddenly leaped to the conclusion that the families she studied were typical American families. She then coauthored a book with science writer Sandra Blakeslee, in which she described the sorry state of the children 10 years after their parents' divorces. Then she suggested to her readers that their children would likely respond to a divorce in the same way. The book, *Second Chances: Men, Women, and Children a Decade After Divorce* (Wallerstein and Blakeslee 1989), became a best seller and probably the most widely read book on divorce ever published. More recently, Wallerstein has begun to release information from a 25-year follow-up of the youngest children in the families she studied, who were 2 to 6 years old when the study began.

Does divorce actually cause the problems displayed by children in single-parent families? Definitely so, conclude Wallerstein and her coauthors. Moreover, according to the 25-year follow-up study, the effects continue or even worsen over time. In a 1997 paper on the results of that follow-up, Wallerstein and Julia Lewis (1997) wrote that divorce is a cumulative experience for the children. They argued that the effect of divorce gains new strength at late adolescence, when the adolescents, in some cases, don't receive the financial support they need to attend college. Many of the now-grown children in their study had attained less education than their parents. Wallerstein and Lewis claimed that when these children reach young adulthood, many fear that their own adult relationships will fail, as did those of their parents.

What proportion of children are harmed by divorce? Wallerstein's writings clearly imply that most children are harmed. At the 25-year mark, she and Lewis wrote, one respondent after another spoke of their lost childhood. They told of an anxiety about intimate adult relationships that was as severe among those whose parents had undergone a conflicted divorce as among those whose parents had remained cordial—and was as severe among those who had seen their fathers regularly as among those who saw little of their fathers.

But how do we know that the parents' divorce, as opposed to other individual and family problems, caused these difficulties? No comparison group is presented. Moreover, Wallerstein's claims that her families were healthy and typical are unconvincing. To be sure, she screened out families in which the children had seen mental-health professionals before the beginning of the study. Wallerstein and Lewis (1997) asserted that the children were a psychologically sturdy group; they reminded the reader that none had ever been referred for psychological help.

Wallerstein, however, neglects to say that she did not screen out families in which the *parents* had seen mental-health professionals. As Frank Furstenberg and I noted a decade ago (Cherlin and Furstenberg 1989), only the appendix to Wallerstein's first book about the study, published in 1980 (Wallerstein and Kelly 1980), informs the reader that 50% of the fathers and close to half of the mothers were "moderately troubled" individuals when the study began: "Here were the chronically depressed, sometimes suicidal individuals, men and women with severe neurotic difficulties" in personal relationships or with "longstanding problems in controlling their rage or sexual impulses" (p. 328). Furthermore, an additional 15% of the fathers and 20% of the mothers were "severely troubled during their marriages." These individuals "had histories of mental illness including paranoid thinking, bizarre behavior, manic-depressive illnesses, and generally fragile or unsuccessful attempts to cope with the demands of life, marriage, and family" (p. 328).

Typical American families? No, these were largely troubled families—and troubled parents often raise troubled children. This is why Wallerstein's long-term reports are so grim. There is no question that her conclusions exaggerate the harm typically caused by divorce.

PARENTS DON'T MAKE ANY DIFFERENCE

Had I written this article five years ago, I would have spent more time arguing against Wallerstein's doomsday view of divorce. Now, however, Wallerstein is the *old* extremist in this debate. In 1998 a new extremist appeared in print: Her book was touted in a *New Yorker* article (Gladwell 1998) and featured on the cover of *Newsweek* (Begley 1998). She is psychologist Judith Rich Harris, who is influenced not by clinical case studies but by behavioral genetics and evolutionary psychology. In her startling book *The Nurture Assumption: Why Children Turn Out the Way They Do* (Harris 1998), she proposes that what parents do makes little difference in how their children's lives turn out. Rather, she asserts, about half of the variation in children's personalities and behavior is due to genetic inheritance; the other half is due mainly to the influence of children's peer groups. Unlike Wallerstein, Harris argues that growing up in a single-parent family does not actually cause the negative outcomes we see in children from single-parent families. It follows that relatively few children are harmed by living in a single-parent family per se.

Harris is greatly influenced by the subfield of behavioral genetics, which has created a revolution in developmental

psychology but is not well known to most demographers. Its practitioners infer the effects of heredity on personality and behavior mainly from studies of pairs of individuals who differ in genetic relatedness (Plomin, DeFries, and McClearn 1990). The most common design is to compare pairs of identical twins with pairs of fraternal twins. The former have 100% of their genes in common; the latter, like any pair of full siblings, have, on average, 50% in common. The researchers assume, crucially, that the family environments of identical twins while they are growing up are no more similar than those of fraternal twins. This assumption is unlikely to be completely true, strictly speaking, but studies indicate enough similarity to justify taking the results seriously (Kendler et al. 1994). Under this assumption, if identical twins are more similar than fraternal twins in personality or behavior, this must be due to their greater genetic similarity or to the interaction of their genetic tendencies with their environment.

Using this logic, behavioral geneticists have published hundreds of articles that suggest a genetic contribution to personality and behavior. In a study of 1,516 same-sex adult twin pairs in which both twins in each pair had ever married, McGue and Lykken (1992) reported the following: A fraternal twin's odds of divorce were twice as high if his or her co-twin had divorced, but an identical twin's odds of divorce were six times higher if his or her co-twin had divorced. Although the authors reject any simplistic notion of a "divorce gene," they argue that genes contribute to personality traits and behaviors; these, in turn, influence the likelihood that a person's marriage will end in divorce. Commenting on this study, Harris writes, "Heredity, not their experiences in their childhood home, is what makes the children of divorce more likely to fail in their own marriages" (1998:308).

Could it be, Harris argues, that divorce is simply a marker for genetically transmitted characteristics, such as vulnerability to depression, that make parents more likely to divorce and also make their children more likely to suffer unwanted life outcomes? In other words, if children whose parents divorced are more depressed, could that be the case because depression runs in their family and partially caused both the parents' divorce and the children's depressive symptoms? Harris states that more behavioral genetic studies of divorce are needed; these studies, she predicts, will show that "parental divorce has no lasting effects on the way children behave when they're not at home, and no lasting effects on their personalities" (1998:311).

Along the way, Harris trashes much social demographic research for assuming cause and effect in studies that exclude heredity. She subjects the book by McLanahan and Sandefur (1994) to particular criticism, probably, she acknowledges, because it is the leading study of its type. The writer for *Newsweek* was even less kind to social scientists. In the cover story on Harris's book, she wrote:

To reach her parents-don't-matter conclusion, Harris first demolishes some truly lousy studies that have become part of the scientific canon. (Begley 1998:55)

Later she stated:

Even [Harris's] detractors like the way she's blown the lid off dumb studies that can't tell the difference between parents' influencing their kids through genes and influencing them through actions. (Begley 1998:56)

These are fighting words. How should demographers respond? Unfortunately, the major way in which sociology—my discipline—has responded to genetic arguments has been to deny that genetics makes any difference at all for human personality and behavior. The denial comes from two sources. First, sociologists study the ways in which society shapes individuals, and they tend to believe that what they study is the most important source of influences. Consequently they assume that social structure and culture can easily override genetic predispositions. The second source is political: Sociologists are wary of genetic arguments because they have been used in the past to deny equal rights and opportunities to women and minorities. The political concern is understandable: One must use care in stating the implications of behavioral genetic research. Yet these two sources of denial have led to an unfortunate rejection of the importance of genetic inheritance to the study of human society. In view of the accumulating evidence, most sociologists' steadfast refusal to consider heredity is becoming an embarrassment to sociologists who reach out to other disciplines.

The situation is somewhat better among social scientists who identify themselves as demographers. Many listeners agreed with Richard Udry when he argued, in his 1994 presidential address to the Population Association of America, that gender differences have both genetic and environmental components (Udry 1994). The program at the 1999 PAA annual meetings included sessions titled "Evolutionary Perspectives on Fertility" and "The Biodemography of Aging," both of which represent growing areas of inquiry. I would guess that a substantial number of demographers would accept the idea that genetic inheritance plays a role in shaping human behavior.

In any case, I believe that demographers should not react to Harris's critique by denying the importance of genetic inheritance. The more appropriate response is that genetics in fact may be important, but parents' actions still make a difference for outcomes of children in single-parent families. Behavioral genetic studies show clearly that heredity does *not* account for all—or anywhere near all—of the variance in human behavior. In the typical behavioral genetic study of a characteristic such as major depression, heredity appears to account for about half of the variance in the twin pairs studied (Dunn and Plomin 1990).

What accounts for the rest? The most controversial part of Harris's argument is her claim that children's peer groups, rather than treatment by parents, produce the environmental effects on children's development. Harris says, in effect, that half of the variation in children's behavior and personality is due to genes and most of the rest is due to peer groups; what

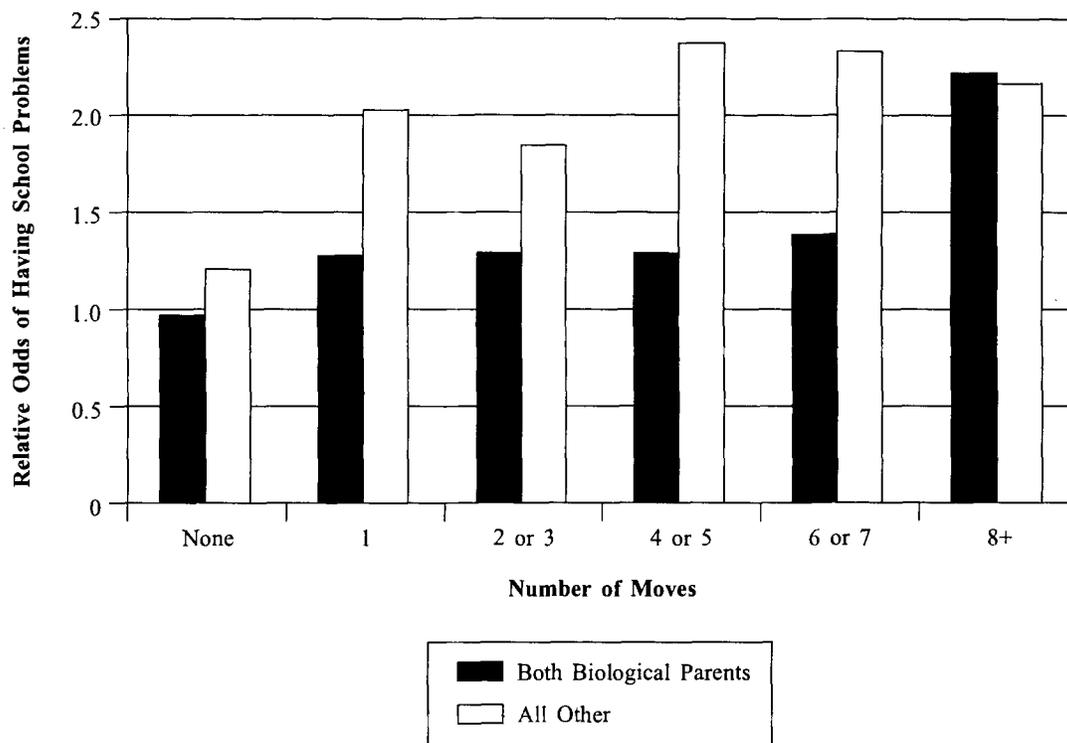
parents do doesn't affect children's development very much. The claim that peers make a big difference and parents don't—not the genetic argument, which many psychologists accept—is drawing the most fire.

To be sure, peer groups have an important influence on children's development, but evidence suggests that what parents do also matters greatly. As I noted above, Harris uses divorce as an example showing why parents don't matter much. Because of her focus on peer groups, however, she claims that one of the few ways in which parental divorce affects children is that the decline in mothers' incomes after separating sometimes forces them to move to a new neighborhood. Harris acknowledges that McLanahan and Sandefur (1994) also discuss post-separation moves, but her own twist is the argument that the move hurts children only because it disrupts their peer groups. They must find and join a new group, which is difficult and which could introduce new, unwanted influences.

What is the evidence? Research suggests that frequent moves *can* lead to poorer adjustment among children in single-parent families. Consider a study by C. Jack Tucker, Jonathan Marx, and Larry Long (1998). These authors used data from the Child Health Supplement to the 1988 National Health Interview Survey to investigate the effects of residential moves on academic and behavior problems in school. As shown in Figure 1, they found that, among children not living with both biological parents, those whose families had moved several times were doing less well in school than children whose families had moved less often. So far, so good for Harris. The authors, however, also found that, among children living with both biological parents, those whose families moved several times were *not* doing significantly worse in school than those whose families moved less often (unless the parents had moved eight or more times). The presence of a second, biological parent in the household seemed to buffer children from the potentially disruptive effects of frequent moves. Apparently the task of finding new peer groups and adjusting to new schools was not a problem for most children in families with two biological parents. These findings suggest that parental divorce or being born to a single parent increases the risk that residential moves will hurt school performance, whereas the risk is reduced by living with two biological parents. In other words, even if peer groups make a difference, parents also make a difference in moderating their effects.

Or consider an article that Lindsay Chase-Lansdale, Christine McRae Battle, and I published (Cherlin, Chase-Lansdale, and McRae 1998) as part of a larger study on the effects of divorce on children. We studied the records of 11,759 British children who were born in 1958 and followed until 1991, when they were 33 years old. All lived in families with two biological parents until at least age 7. Indicators of emotional problems were obtained from the children at ages 7, 11, 16, 23, and 33. We standardized those measures and conducted an analysis of the trajectory of emotional problems from age 7 to age 33, according to whether the children's parents divorced. We used growth-curve modeling, a variant of

FIGURE 1. RELATIVE ODDS OF HAVING SCHOOL PROBLEMS, BY NUMBER OF MOVES AND FAMILY TYPE



Source: Tucker, Marx, and Long (1998).

the models known among demographers as multilevel models and among econometricians as random-effects models.

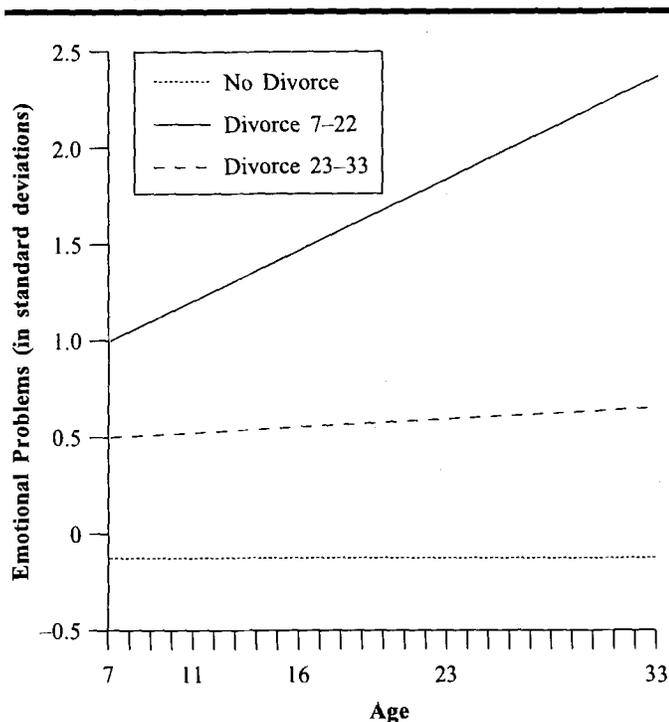
We found that children whose parents would later divorce *already* showed more emotional problems at age 7 than children from families that would stay together. This can be seen in Figure 2 in the gap, at age 7, between children whose parents would later divorce and those whose parents would remain together. This preexisting gap is consistent with the argument that divorce occurs in families that are already troubled. The gap between those whose parents will divorce and those whose parents will not is ignored by the Wallersteinian extreme. In fact, several other researchers have reported evidence that precursors of the difficulties associated with divorce are visible in children, to some extent, years before the breakup (Block, Block, and Gjerde 1986; Doherty and Needle 1991; Elliott and Richards 1991). I would submit that this is now a well-established finding. It suggests that studies that do not take into account the preexisting difficulties of children and their families overstate the effect of growing up in a single-parent family.

That's only part of the story, however. The gap continues to widen between the no-divorce group and the group whose parents divorced when the children were between ages 7 and 22. This finding indicates that, when a divorce did oc-

cur, it coincided with a further increase in emotional problems—a pattern suggesting that family breakup also may affect mental health. In fact, it recalls Wallerstein and Lewis's statement that the effects of divorce are cumulative rather than time-limited. This pattern also was confirmed by a fixed-effects model that we estimated. Even so, partisans of genetic explanations could argue that the widening gap also may be due to differences in inherited characteristics that change over time. The onset of clinical depression, for example, which may have a genetic component, typically does not occur until early adulthood.¹

1. Blankenhorn (1999) cites our article as evidence that researchers are becoming more pessimistic about the effects of family disruption on children. He is correct that our conclusion about the widening gap represents a modification of an earlier article that examined the British cohort only through age 11 (Cherlin et al. 1991). Blankenhorn, however, ignores the initial gap we found at age 7, which indicates the existence of a substantial predisruption effect. In fact, he ridicules predisruption effects, arguing that "this whole exercise of trying to isolate the effects of pre-divorce marital problems from the effects of divorce is largely a waste of time. Often, it's fraud" (p. 8) because the problems are merely part of the divorce process. Yet some of the divorces in the British cohort took place five or 10 years after our initial measures at age 7, and the results were similar. It seems unlikely that the gap at age 7 is due only to the imminent onset of divorce.

FIGURE 2. GROWTH CURVE MODEL OF EMOTIONAL PROBLEMS FROM AGE 7 TO 33, BY AGE AT PARENTAL DIVORCE



Source: Cherlin, Chasc-Lansdale, and McRac (1998).

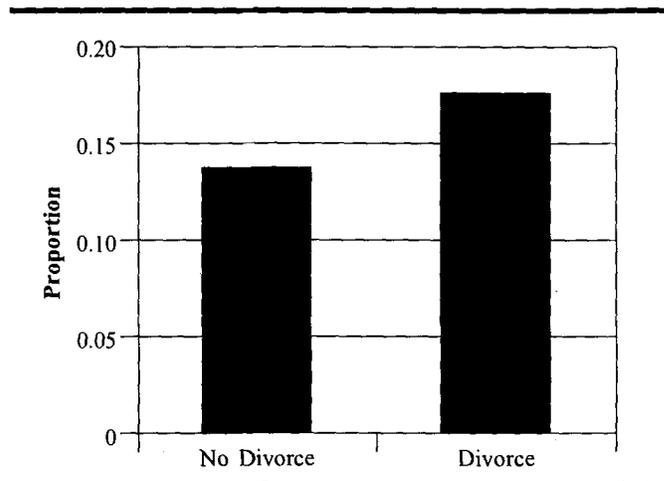
What about evidence from behavioral genetic studies themselves—the kind of study that, according to Harris, should show little or no effect of divorce? I am aware of only one: a study of female twin-pairs published in 1992 (Kendler et al. 1992). The authors examined whether a parental divorce contributed to the variance in major depression (and other mental health outcomes) between identical and fraternal twin-pairs, once the degree of genetic resemblance was taken into account. They did find a statistically significant association: A parental separation or divorce increased the risk of major depression for members of a twin-pair by 42%, even after making allowance for genetic relatedness.² This finding suggests that divorce indeed has an effect on mental health—that the variation is not due only to genes.³

In sum, the evidence suggests that Harris's position is too extreme when she tells parents, in the final paragraph of the penultimate chapter, "Relax. How they turn out is not a reflection on the care you have given them" (Harris 1998:

2. See Kendler et al. (1992), Table 1.

3. The authors note that presumably genetic differences between identical and fraternal twin-pairs accounted for far more of the variance in major depression in the sample than did parental separation and divorce (Kendler et al. 1992). The composition of the sample, however, increased the likelihood of obtaining this result: Only 12% of the twin-pairs had experienced parental separation or divorce, whereas their variation in genetic relatedness (zygosity) was much greater: 57% were identical (monozygotic) and 43% were fraternal (dizygotic).

FIGURE 3. PROPORTION SCORING ABOVE THE CLINICAL CUTOFF AT AGE 23, BY PARENTAL MARITAL STATUS



Source: Chasc-Lansdale, Cherlin, and Kiernan (1995).

349). The danger in the heavy media coverage of Harris's book is that parents will believe this overstatement, and will conclude that an ill-considered divorce will do no harm or that saving to pay for college tuition will do no good. Genes and peers notwithstanding, we have strong evidence that parents still make a major difference.

HOW MANY CHILDREN WILL EXPERIENCE DIFFICULTIES?

This is not to say that all children, or even most, will suffer long-term problems due to a parental divorce. Consider the responses, at age 23, of the British sample we studied. They answered a 24-question index of mental health developed by British psychiatrist Michael Rutter. The yes-or-no questions include "Do you often feel miserable and depressed?" "Are you constantly keyed up and jittery?" and "Do you often get worried about things?" Clinical use of the scale suggests that people who answer "yes" to seven or more questions may need mental health services (Rutter, Tizard, and Whitmore 1970).

Figure 3, which is taken from a 1995 study (Chase-Lansdale, Cherlin, and Kiernan 1995), shows the proportion scoring above this clinical cutoff for the divorce group and the no-divorce group for 23-year-old women, with controls for social class and childhood behavior problems. One can look at this figure in two ways. In an absolute sense, relatively few individuals in either group scored above the cutoff; this finding suggests that the majority of individuals who experienced parental divorce do not experience long-term mental health problems. In a relative sense, however, the risk of experiencing mental health problems was 31% higher in the divorce group (.180 versus .137), which suggests that parental divorce raises the risk of mental health problems. Thus the data suggest that divorce increases the risk of problems but that most people whose parents divorce do not experience those problems.

CONCLUSION

What, then, can we conclude about the effects of growing up in a single-parent family? We know enough to clearly reject one extreme view: that family structure causes all of the problems we see in children who don't live with two biological parents while growing up. Studies of parental divorce show that some of these problems, or at least their precursors, were present before the parents separated—and not merely a year or two beforehand, but sometimes five or ten. These findings strongly suggest (although they do not prove) that some of the children's problems following disruption reflect personal characteristics or family dysfunction which are not simply part of the divorce process. Evidence suggests that these characteristics could be partly genetic in origin, or could involve the interaction of genetic predispositions with environmental influences. Overall this set of findings implies that some of the problems might have occurred even if the biological parents had not divorced.

By scientific standards, we cannot wholly reject the other extreme view: that none of the problems we see were caused by the divorce, that they were caused completely by genetics and peer groups, and that they all would have occurred anyway. Without the possibility of a controlled experiment, it is difficult to reject the null hypothesis that family structure counts for almost nothing. Even so, some of the evidence seems inconsistent with this extreme. According to the one high-quality twin study that has been conducted on this topic, a parental divorce appears to increase the risk of depression, even with controls for inherited characteristics (Kendler et al. 1992). In addition, other social-scientific studies that I have reviewed here have produced patterns of findings that are difficult to reconcile with a "parents make no difference" view. Recall that children seem to handle residential moves more easily when they live with two parents (Tucker et al. 1998).

In summary, the long-term mental health of adults who experienced parental divorce as children or adolescents appears to deteriorate in relation to the mental health of those who grew up with two biological parents, even after the initial gap between the two groups in early childhood is taken into account (Cherlin et al. 1998).

To make further progress in examining the extreme "family structure makes no difference" position, it would be useful to have access to large genetic samples embedded in high-quality longitudinal studies of randomly selected populations. This need is beginning to be met: The National Longitudinal Study of Adolescent Health, for example, which is being conducted by researchers at the University of North Carolina Population Center, includes an embedded genetic sample in its large, longitudinal survey. I hope there will be other studies with similar designs.

Yet on the basis of current evidence, imperfect though it may be, I think there are strong grounds for being nearly as skeptical about the "no difference" extreme as about the "all the difference" extreme. The former view does not fit the facts at our disposal. Rather, the evidence suggests that genetic inheritance and its interaction with the environment are part of

the story but far from the whole story. Thus the lesson I draw is that the actual effect of family structure lies between the extremes. Whether a child grows up with two biological parents, I conclude, makes a difference in his or her life; it is not merely an epiphenomenon. Not having two parents at home sometimes leads to short- and long-term problems, but not all the differences we see in outcomes are the results of family structure. Some of the differences would have occurred anyway. Moreover, parental divorce or being born to unmarried parents does not automatically lead to problems. Many (perhaps most) children who grow up in single-parent families or in stepfamilies will not be harmed seriously in the long term.

Fortunately I see signs that observers, despite Harris and Wallerstein, are turning away from the extremes. Consider Barbara Defoe Whitehead, who stirred up concern in a now-famous article published in 1993 in the *Atlantic Monthly*. The title of the article, "Dan Quayle Was Right," referred to the then vice-president's criticism of the television program *Murphy Brown* for scripting its main character to have a child outside marriage (Whitehead 1993). Drawing heavily on Wallerstein, Whitehead left the impression that divorce badly scars most children and, as the cover line on the magazine warned, that it "dramatically undermines our society."

In *The Divorce Culture*, however, a more recent, book-length treatment of the same topic, Whitehead (1997) apparently has backed away from that position. She seems to accept the evidence that the majority of children whose parents divorce do not suffer serious long-term harm from the divorce. Yet she still argues that divorce is a serious problem even if only a minority of children suffer long-term harm. After all, she argues, if 40% to 50% of all American children are experiencing divorce, then a "minority" is still a lot of kids—and she is right. We have here a troubling social problem that does not "dramatically undermine our society" but nevertheless warrants our attention and concern. Growing up in a single-parent family is not a sentence to life at emotional hard labor, but it sometimes has consequences that parents would not wish upon their children.

As I hope this example shows, backing off from extremes doesn't mean backing away from moral concern or social commentary. Rather, it means helping the lay audience and the media to avoid oversimplifying the causes of complex phenomena such as children's well-being. It means moving away from seesaw debates between those who think a particular social issue is a disaster and those who think it's not a problem at all. For social scientists, it also means recognizing how unlikely it is that a single social science discipline could provide a complete understanding of these changes. Consequently, researchers can avoid extremes by taking an interdisciplinary approach to research. Here social demographers are at an advantage because of the increasingly interdisciplinary nature of the field. They have the inclination and the opportunity to exchange findings across disciplinary boundaries.

One could argue that the tendency to advance extreme arguments is built into the scientific method, which most demographers attempt to follow. The simplification of a complex

problem is essential to a solid scientific theory: Unless your research allows you to simplify reality to some degree, you have not said anything of importance. Other approaches to the social world, however, do not emphasize simplification so strongly. Anthropologists, for example, with their grounded, ethnographic perspective, are much more concerned with thick description and broad understandings. In fact, this difference is a major reason why the introduction of an anthropological perspective into demography has been so beneficial.

But I am not arguing that we should back away from the scientific research enterprise, nor that we should hesitate to identify important pathways when we find them. Rather, I am suggesting that we not overstate the importance of our perspectives. In attempting to learn the origins of complex social phenomena, perhaps the best advice we could follow comes from Albert Einstein, who said, "Everything should be made as simple as possible, but not simpler" (Jones 1996).

REFERENCES

- Begley, S. 1998. "The Parent Trap." *Newsweek*, September 7, pp. 55-59.
- Blankenhorn, D. 1999. "The Shift (Cont.)." 1999. *Propositions* (Winter, No. 3):4-8.
- Block, J.H., J. Block, and P.F. Gjerde. 1986. "The Personality of Children Prior to Divorce: A Prospective Study." *Child Development* 57:827-40.
- Bumpass, L.L. and H.-H. Lu. 1998. "Trends in Cohabitation and Implications for Children's Family Contexts in the U.S." Working Paper 98-15, Center for Demography and Ecology, University of Wisconsin, Madison.
- Bumpass, L.L. and R.K. Raley. 1995. "Redefining Single-Parent Families: Cohabitation and Changing Family Reality." *Demography* 32:97-109.
- Chase-Lansdale, P.L., A.J. Cherlin, and K.E. Kiernan. 1995. "The Long-Term Effects of Parental Divorce on the Mental Health of Young Adults: A Developmental Perspective." *Child Development* 66:1614-34.
- Cherlin, A.J. 1992. *Marriage, Divorce, Remarriage*. Cambridge: Harvard University Press.
- Cherlin, A.J., P.L. Chase-Lansdale, and C. McRae. 1998. "Effects of Parental Divorce on Mental Health Throughout the Life Course." *American Sociological Review* 63:239-49.
- Cherlin, A.J. and F.F. Furstenberg, Jr. 1989. "Divorce Doesn't Always Hurt the Kids." *The Washington Post*, March 19, p. C1.
- Cherlin, A.J., F.F. Furstenberg, Jr., P.L. Chase-Lansdale, K.E. Kiernan, P.K. Robins, D.R. Morrison, and J.O. Teitler. 1991. "Longitudinal Studies of Effects of Divorce on Children in Great Britain and the United States." *Science* 252(June 7):1386-89.
- Doherty, W. and R. Needle. 1991. "Psychological Adjustment and Substance Use Among Adolescents Before and After Parental Divorce." *Child Development* 62:328-37.
- Dunn, J. and R. Plomin. 1990. *Separate Lives: Why Siblings Are So Different*. New York: Basic Books.
- Ehrlich, P.R. 1968. *The Population Bomb*. New York: Ballantine.
- Elliott, B.J. and M.P. Richards. 1991. "Children and Divorce: Educational Performance and Behaviour Before and After Parental Separation." *International Journal of Law and the Family* 5:258-76.
- Gladwell, M. 1998. "Do Parents Matter?" *The New Yorker*, August 17, pp. 54-64.
- Harris, J.R. 1998. *The Nurture Assumption: Why Children Turn Out the Way They Do*. New York: Free Press.
- Hoffman, S.E., M. Foster, and F.F. Furstenberg, Jr. 1993. "Reevaluating the Costs of Teenage Childbearing." *Demography* 30:1-13.
- Jones, A., ed. 1996. *Chambers Dictionary of Quotations*. New York: Chambers.
- Kendler, K.S., M.C. Neale, R.C. Kessler, A.C. Heath, and J.J. Eaves. 1992. "Childhood Parental Loss and Adult Psychopathology in Women: A Twin Study Perspective." *Archives of General Psychiatry* 49:109-16.
- Kendler, K.S., M. Neale, R. Kessler, A. Heath, and L. Eaves. 1994. "Parental Treatment and the Equal Environment Assumption in Twin Studies of Psychiatric Illness." *Psychological Medicine* 24:579-90.
- McGue, M. and D.T. Lykken. 1992. "Genetic Influence on Risk of Divorce." *Psychological Science* 3:368-73.
- McLanahan, S. and G. Sandefur. 1994. *Growing Up With a Single Parent: What Hurts, What Helps*. Cambridge: Harvard University Press.
- Plomin, R., J.C. DeFries, and G.E. McClearn. 1990. *Behavioral Genetics: A Primer*. 2nd ed. New York: Freeman.
- Preston, S.H. 1984. "Children and the Elderly: Divergent Paths for America's Dependents." *Demography* 21:435-57.
- Rutter, M., J. Tizard, and K. Whitmore. 1970. *Education, Health, and Behavior*. London: Longman.
- Simon, J.L. 1981. *The Ultimate Resource*. Princeton, NJ: Princeton University Press.
- Tierney, J. 1990. "Betting the Planet." *New York Times Magazine*, December 2, pp. 52 ff.
- Tucker, C.J., J. Marx, and L. Long. 1998. "'Moving On': Residential Mobility and Children's School Lives." *Sociology of Education* 71:111-29.
- U.S. Bureau of the Census. 1999. "Table MS-2." Retrieved May 11, 1999 (www.census.gov).
- U.S. Department of Health and Human Services. 1998. *Trends in the Well-Being of America's Children and Youth*. Washington, DC: U.S. Government Printing Office.
- U.S. National Center for Health Statistics. 1998. *Births and Deaths: Preliminary Data for 1997*. Washington, DC: U.S. Government Printing Office.
- Udry, J.R. 1994. "The Nature of Gender." *Demography* 31:561-73.
- Wallerstein, J.S. and S. Blakeslee. 1989. *Second Chances: Men, Women, and Children a Decade After Divorce*. New York: Ticknor and Fields.
- Wallerstein, J.S. and J.B. Kelly. 1980. *Surviving the Breakup: How Children and Parents Cope With Divorce*. New York: Basic Books.
- Wallerstein, J.S. and J. Lewis. 1997. "The Long-Term Impact of Divorce on Children: A First Report From a 25-Year Study." Presented at the Second World Congress of Family Law and the Rights of Children and Youth, June 2-7, San Francisco.
- Whitehead, B.D. 1993. "Dan Quayle Was Right." *The Atlantic*, April, pp. 47-84.
- . 1997. *The Divorce Culture*. New York: Knopf.