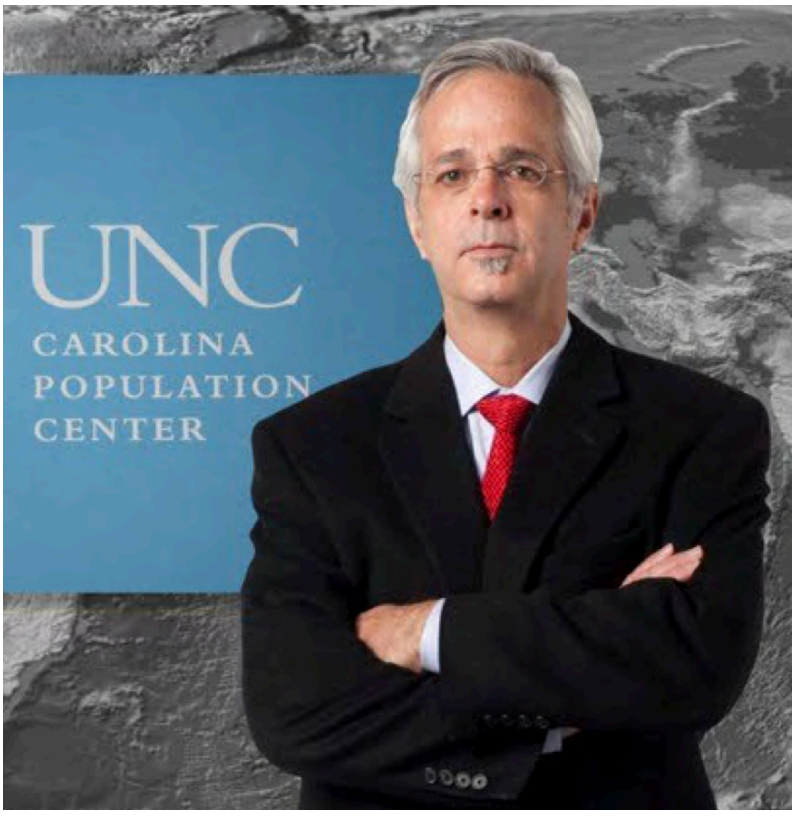
DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with S. PHILIP MORGAN
PAA President in 2003



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), MERCHANT (2016 to present), and Win Brown (2018 to present)

S. PHILIP MORGAN

PAA President in 2003 (No. 66). On 25 June of 2021, during the midst of the COVID pandemic, we were able to have a Zoom interview with Dr. Morgan. The members of the PAA History Committee participating in the interview included John Weeks, Emily Merchant and Win Brown.

CAREER HIGHLIGHTS: S. Philip (Phil) Morgan was born in 1953 in Suffolk, Virginia, and grew up in the small farm community of Corapeake, in Gates County, North Carolina. He received his B.A. in Sociology (with honors) from the University of North Carolina, Chapel Hill in 1976. He then went to graduate school at the University of Arizona in Tucson, where he earned his M.A. in Sociology in 1978 and his Ph.D., also in Sociology, in 1980. He spent three years (1980-1983) as a Post-Doctoral Fellow at the Carolina Population Center at UNC Chapel Hill, and from there spent a year at the University of Georgia in Athens as an Assistant Professor of Sociology. In 1984 he moved to the University of Pennsylvania as an Assistant Professor of Sociology, and was promoted to Associate Professor in 1988, and to Professor in 1992. He served as chair of the Graduate Group in Demography from 1992-1993, and as Chair of the Sociology Department from 1993-1996. In 1998, Dr. Morgan relocated to Duke University, where he then served as Chair of the Sociology Department from 2002-2008 and from 2008-2012 he was the Norb F. Schaefer Professor of International Studies, while also serving as the Faculty Director of the Social Science Research Institute. In 2012, he moved over to Chapel Hill to become Professor of Sociology at UNC, and Director of the Carolina Population Center. In 2014, he was named the Alan Feduccia Distinguished Professor. He retired from UNC in 2018 and now holds the title of Alan Feduccia Distinguished Professor of Sociology Emeritus. He is known especially for his research and publications on fertility and family structure, including a focus on below-replacement fertility.

WEEKS: Phil, are you in Chapel Hill or Durham – Where are you living these days?

MORGAN: I'm in Nags Head, North Carolina. So, I live at the ocean. I live at the beach now. I retired about two years ago, really sort of stepped away from the demography work that I had been doing. About ten years ago, we started a business on the Outer Banks: Seagreen Gallery. My wife's an artist. My older son's an artist. My wife and I and two of our three sons now run a business in Nags Head--Seagreen Gallery--and we have a second store in Duck, North Carolina, which is about fifteen miles north of us.

[their website is: https://www.seagreengallery.com/]

So, I'm a little bit removed from my profession and the discipline and from some of the questions you guys may ask me. But I still have perspectives on these things. The other thing I was going to give you a heads up on is, I don't see you guys. So, you can roll your eyes or whatever and I won't even know. But that means you can't raise your hand and expect me to see you.

WEEKS: Okay. Well, my younger son, who got his doctorate at Chapel Hill, and is now associate dean of the College of Arts and Sciences at UNC Charlotte, he and his family love the Outer Banks. So, I've got to get the information about your business, because I know they'll be going to the Outer Banks this summer. So, when a Weeks family comes in to do business with you, you'll know who sent them.

MORGAN: Oh. Always good to drum up interest in the business. Although, the business is doing fantastic. It exceeds my expectations. We weathered the Covid thing fine. We were closed for a while. But once it was open, I think the Outer Banks is seen as a place that is safe to go to, it's a safe place to be, even during the Covid epidemic. So, lots of people, while they weren't in school, they were working remotely, they just came down and set up shop here at the beach.

WEEKS: Right.

BROWN: Phil, what is it like to be there when the tourist season ends? I guess you count yourself, obviously, as a member of the permanent community, in places that I gather are mostly feeds for tourists. What was the economic life like during the long season when tourists are all gone and those businesses that I guess serve tourists are usually slow? Did Covid have a bad effect on that?

MORGAN: Well, it did initially. We're on basically an island and there's two bridges to get here, and the locals talked the governor into shutting the bridges and they put police up. And they basically didn't let anybody in who wasn't a permanent resident. So that just shut things down and we closed for about two months a year ago--March and April. We love it and hate it. When the tourists aren't here, we don't make much money, but we have an incredible place to live. When the tourists are here, we work a lot. It's crowded. I love it year-round. It's pretty spectacular.

I love working with my family, as well. As I said, I have three sons and my oldest and youngest work here. My wife works here, as well. We make about 25 percent of what we sell in the store. We have 50 or 60 commissioned artists who make another 25 percent of what we sell in the store. And we shop for the rest of it around the world online. It's probably that part of it, which we felt the impact of Covid most. In the past six or seven months, it's just hard to get shipments of anything. We sell some things from Indonesia, India, places like that. And it's hard to get things from there.

WEEKS: Well Phil, just so we have this on the record, because we're recording this like I told you, can you give us the name of the business and maybe even the website so that people can find it.

MORGAN: Sure. So, as I said, my wife and two of my sons and I own and operate a couple businesses on the outer banks of North Carolina. They are Seagreen Gallery and Sea Green Gallery-Duck. The web page is seagreengallery.com. Our store was featured in *Our State Magazine*, which is a North Carolina tourist magazine, almost a year ago for being an innovative and interesting place to visit on the Outer Banks.

WEEKS: Okay, good. Very, very good. Well, we'll go ahead. Karen Hardee, another one of our members of the committee, said she was going to join us. But we'll let her come in when she's able to come in and we'll go ahead and get started with taking you back to the world of demography.

MORGAN: Sure, I look forward to it.

WEEKS: Okay, so for the record, we are here for a past PAA president interview with Dr. Phil Morgan, who is the Alan Feduccia...have I pronounced that correctly?

MORGAN: Yes.

WEEKS: ...Distinguished Professor of Sociology Emeritus at UNC Chapel Hill and former Director of the Carolina Population Center. And President of the PAA in 2003, when in fact you were at Duke University. So, we are very, very happy to have you here with us. I know it's been a while. What's that, 18 years since you were president of the PAA. So, we're going to tap into the brain cells to see what was going on. Now, of course, as good demographers, we want to get as into your personal demographics as we can. We know that you were born in 1953. Right?

MORGAN: Yes.

WEEKS: And where were you born?

MORGAN: I was born in Suffolk, Virginia. I grew up on a farm just across the Virginia line in North Carolina. The closest post office was in a tiny town called Corapeake. The county I grew up in, Gates County, did not have a single stoplight until about ten or fifteen years ago. Very rural!

WEEKS: Okay. But then you, for college, you went off to Chapel Hill, got your bachelor's in sociology in Chapel Hill. Was sociology what you wanted to do, or did you just happen into that?

MORGAN: So, when I went to UNC Chapel Hill, I didn't know what sociology was. Again, I grew up on a farm. My dad worked in the shipyards in Norfolk, Virginia. And we farmed on weekends. My upbringing provided both advantages and disadvantages. The advantages were my parents were really smart, appreciated education. I have two brothers and two sisters. We all have college degrees. Three of us have advanced degrees. But my parents only had high school educations, and my view of the potential jobs in the world, growing up on a farm, I think the number one lesson I learned is I didn't want to be a farmer.

And the only other jobs I saw that respectable people were doing were doctors, lawyers, preachers, and accountants. So, for some reason, I said I wanted to be an accountant when I went off to college. That was primarily because I saw that accountants got to play golf on weekends instead of doing other kinds of work. So, when I got to UNC Chapel Hill, I actually dropped out after a semester. I didn't feel comfortable there. I really wasn't prepared, fully. I transferred to a junior college (Chowan College) and played football for a year. I learned a couple things there.

One, I wasn't nearly fast enough to play big time college football. And second, my feeling of discomfort at UNC-Chapel Hill lay in my being academically and socially unprepared. The time in junior college was valuable. This is where I learned what psychology and sociology were. This was where I took some more serious math courses, so that when I got back to UNC, I was prepared to go to work. And this background is relevant because some of the people that were really important in the evolution of my career introduced me to worlds I had no idea existed. And one was a world of research and graduate school, etc.

WEEKS: Okay, so then you went off to graduate school to Arizona. What took you there? And who were you relating to there?

MORGAN: Well, let me step back just a little bit. As an undergrad at UNC, I did this junior college football stent. I came back to UNC, and I was ready to go to college. I asked people what great courses at UNC I should take. One friend recommended a course taught by a political scientist--Clinton was his last name. I think he was an adjunct professor at UNC at the time. But he was an incredible lecturer. He was a neo-Malthusian. And he's the one who introduced me to

the ideas about the population explosion, Limits to Growth, Garrett Hardin's Tragedy of the Commons. I read all those things that year and was fascinated by them.

I evolved into being a soc major and decided to do a senior thesis in sociology. My primary advisor was Dick Kramer. And he encouraged Amos Hawley to be my thesis advisor. Amos Hawley's one of the early PAA presidents, I believe. [Editor's note--Hawley was the 35th PAA President, in 1971-72.] And he was such a contrast to Clinton's neo-Malthusianism. You may not know, but Amos Hawley wrote a famous book called *Human Ecology*. And it's about how societies adapt. And Otis Dudley Duncan wrote that paper, "POET: Population, Organization, Environment, Technology." It was that kind of approach that warns against the idea that the population explosion was going to lead to a tipping point from which there was no return. So, it was an interesting tension. I wrote a senior thesis on energy and the environment. And I came into it with all these neo-Malthusian ideas. Amos Hawley, an incredibly gentle soul, would listen to me and then give me something to read and tell me to come back after I'd read it. So that's where I really started to struggle with these different ideas about how the world would respond to the kinds of population growth that really were not sustainable under anybody's view, either the Malthusians or the more adaptive people like Amos Hawley.

So, I was not committed to sociology grad school. In fact, Dick Kramer, I mentioned once before. I would have gone to law school if it wasn't for Dick Kramer. He said, law school is fine, but you're really pretty good at sociological research. And what he meant was thinking about social problems, population. Have you ever thought about grad school and sociology? I said no, I can't afford that. So, I was a rising senior at UNC before anyone mentioned to me that you could go to graduate school and have your graduate education paid for. I learned so much growing up on a farm, e.g., being an independent person, but I really had limited views of possible occupations.

So, then I started looking at grad schools and at a number of programs. Again, I was interested in human ecology and demography. Otis Dudley Duncan [PAA President in 1968-69] and Stanley Lieberson were at Arizona. They were people who had done things in the area. When I got to Arizona, I realized that their research interest had moved into other areas. I chose graduate school at Arizona because I was still ambivalent about a PhD versus law school. I planned to get a master's degree and then reconsider my decision about not going to law school. But the more work I did as a master's student and then a PhD student at Arizona, I realized that I really loved social demographic research and that I was pretty good at it.

WEEKS: And was it Otis Dudley Duncan with whom you worked most closely? Or Stanley Lieberson or someone else?

MORGAN: I don't give away all the credit for my career, which I'm quite proud of. I worked hard, but I was lucky. I arrived at Arizona the same year Michael Hout arrived at Arizona as a first-year assistant professor. So, Michael turned out to be both my mentor and my primary thesis advisor. And he was really very important. I sort of looked to Michael. He was closer to my age. I started to imagine maybe a career like his. So, that was very important.

But things at the University of Arizona just revolved around Otis Dudley Duncan. He was a phenomenon, he had this terrific career. He's a famous person. He was also without peer as a teacher. I've never been in a classroom with anyone who was in command of the information the way that he was. Incredibly well-prepared. Worked his students very hard. Returned assignments within 24 hours. Was always available in the office. Again, I've never seen anyone else in the

classroom like him. Perhaps the closest I've seen is Sam Preston at the University of Pennsylvania, who I'll talk about later.

So, the combination of Michael Hout, who shared my research interests (at the time he had done work on fertility and family)--Mike Hout had worked with Philip Cutright at Indiana University--and Stanley Lieberson and Neal Fligstein were also on my dissertation committee. So, I happened into what I think of as sort of the golden years of the University of Arizona sociology department. I mean it was a strong department; it still is one. But I think when Otis Dudley Duncan, Beverly Duncan, Stanley Lieberson were there and then Michael Hout joined and then Neil Fligstein, it was really a very strong group.

WEEKS: Okay, for sure. So, from there, as you finished up your doctorate, you accepted a post-doc back at Chapel Hill, right?

MORGAN: Yeah, another great opportunity. I've had more opportunities than I deserve, I'm sure. I finished my masters and PhD in four years. So, it was a quick finish. I really wasn't ready for a job. I only applied for a few jobs. The UNC post-doc got set up fairly early. Mike Hout suggested to Ron Rindfuss at UNC that I'd be a good post-doc candidate. I actually met Ron at my first or second PAA meeting. We met and discussed the possibility of me doing a post-doc with him at UNC Chapel Hill. Being able to have a post-doc at the UNC Pop Center was an incredible opportunity. I loved the idea of going back to North Carolina, going back to Chapel Hill. I'd actually done some of the library research for my undergraduate senior thesis in the Carolina Population Center library.

It was going home (to UNC-Chapel Hill) for the second or third time, if you count my dropping out as an undergraduate. I love collaborations, but no collaborator is as generous as Ron Rindfuss. We worked incredibly well together. While I love collaborating, it can be frustrating. I think the product almost always turns out better, but sometimes with more pain than others. Ron and I would pass things back and forth and instead of one step forward, one step back, there always seemed to be a step forward. It was a very productive three years.

So, a two-year post-doc at UNC Chapel Hill. And then a third year supported by grants that Ron and I obtained in those first two years. So, I was a post-doc for three years. And I needed every year of it. Also at UNC, I'd like to acknowledge Dick Udry [PAA President in 1994], who I didn't really collaborate with, but he was the director of the Pop Center the entire time I was there. He took the post-docs out to lunch most Fridays. I really looked forward to it. An incredibly interesting person. And genuinely interested in our work. It was really a terrific experience. And I was also fortunate that Charlie Hirschman [PAA President in 2005]. was at Duke at the time, and he was collaborating with Ron Rindfuss [PAA President in 1991]. He would leave soon to go to the University of Washington. But I was on a team with Rindfuss and Hirschman and others who were studying the timing of first births and fertility in general in Asia. Charlie was, and remains, a terrific mentor.

WEEKS: So, you really spent your life from the time you were at Chapel Hill as an undergraduate, hanging around PAA presidents, past and subsequent PAA presidents. So, I guess it was only natural that you would become one yourself.

MORGAN: I'm not sure when I even aspired to be a PAA president. I mean, seriously, the chances of getting a premier job in a premier university are long shots, almost professional athlete kinds of long odds. So, like I said, I think you've got to work really hard. You've got to

have some ability. And you've got to be really fortunate and lucky. And again, I do think, I found myself in positions of resources and support. And I was able to take advantage of it.

But my training, as you might say, I had some serious training up to do. And my UNC undergraduate experience was incredible. My last three years there, were absolutely exciting. Arizona, the same story. The Carolina Population Center, again, was incredible. And I've got to say that it wasn't my—my first job was at the University of Georgia. I was there for one year. Look, it was a good job. I would've signed up for life. This was 1983 or 84. There just weren't any jobs. But the next year, a couple jobs came along that I had to consider. I applied for the position at the University of Pennsylvania and was offered the job. So again, I stepped into another opportunity where the colleagues were incredible, and it really built on that post-doctoral training.

Those first 5-10 years at Penn--they were incredible. Sam Preston [PAA President in 1984], Frank Furstenberg, Doug Massey [PAA President in 1996], Susan Watkins, Etienne van de Walle [PAA President in 1992]--all these names are familiar to you. Over the next few years others arrived: Jane Menken [PAA President in 1985], Herb Smith, Tukufu Zuberi. That was an incredible group of people to be around. And just as the University of Arizona revolved around Otis Dudley Duncan, Penn rotated around Sam Preston. Sam Preston would come out every day for lunch. Well, maybe once or twice a week he had a lunch meeting. But if he wasn't at a lunch meeting, at twelve noon, he came out with a piece of cheese, a bagel, and something to drink and would sit in the atrium. And because he was there, all the other faculty showed up for lunch. It was an incredible opportunity for conversations about our profession, about the work we were doing, about current events. It really was spectacular.

WEEKS: Well just so you know, I do know Sam Preston. He was on my dissertation committee at Berkeley. And you got to Penn just about the time that he had been PAA president then. He was the youngest ever. You were young, at age fifty, being PAA president. But he, I think at 41, he was the youngest ever. But obviously, you were working with him there and you, I think you sort of talk in your PAA presidential address about how he was one of those people that got you thinking about the institutional role, how societies react to demographic change. Am I right about that or not?

MORGAN: Yeah, Sam was incredible. I remember his PAA address very well. It was one of the first and probably second or third or so PAA addresses that I attended. Yeah, it was incredible. Sam's trained as an economist, but he really does think like a sociologist—he was in a sociology department his whole career. So, he may not have had much choice. But he was a real institutionalist all along. What a terrific mentor.

I never took a class with Sam. But he encouraged me to give him my work, so I did. And he always had suggestions for improvement, but he was always supportive. And what's more amazing, I've never given him a freaking paper that he didn't return within a week.

WEEKS: Wow. Okay.

MORGAN: Given the way that Rindfuss and Preston treated me… I aspired to do that well with my students. And I couldn't do it. I did my best, but I couldn't do it. I don't know where Sam and Ron found the time and energy to provide such timely and valuable feedback.

WEEKS: Obviously, Sam Preston's a special individual. I mean, you think about the young age at which he got his doctorate at Princeton. I mean, there are some people you just have to set aside and say, they're exceptional and that's just how it is.

MORGAN: Right.

WEEKS: Well now, you were at Penn for a while and then you went to Duke, well actually then you had some visiting appointments in Australia. What was that about?

MORGAN: I wasn't opposed to taking advantage of opportunities for travel. The Australian National University had a population group, and they were always looking for active faculty to come and spend some time in Australia. Part of the thinking there was, it's really expensive to send Australians to the US. And if you really want to get some outside looks at what you're doing, if you want someone with new ideas and a different perspective, instead of bringing them in for a Friday afternoon talk, why don't you bring them in for a semester or a year. So, they had a history of bringing people in for sabbaticals.

So, those two years that I spent in Australia were probably the best years of my life because my three kids were the right age. And my wife, we packed everybody up. We bought these tickets from the US to Australia with like six stops and a turnaround. My sons, at the time, were like twelve, nine, and six. They went to school in Australia. We flew off the west coast. First place we stopped was Tahiti. Then we went to New Zealand. Then we spent six or eight months in Australia and traveled the coast all the way from Canberra to Brisbane to Cairns. It was just a spectacular opportunity. And the Australians, what a wonderful group of demographers. But when you got beyond the demographers, what a wonderful people, and a magical place to spend time.

We considered not coming back. But we loved the US. I had a great career, great job at Penn. It was too far from our families So, we came back.

WEEKS: Okay now you mentioned your wife and kids. Where, in terms of the familial side of your life, where did you and your wife meet?

MORGAN: We actually met at Nags Head. That's the beach town where I'm speaking from today. We were both working in the same restaurant. These were summer jobs. She was a couple years older than me, and when you're 18 and she's 22, that's a big deal. But a couple years later, when we were still the same distance apart, but not quite so much when it was sort of closer to 21 and 24, we began dating, and she came back to Chapel Hill with me and she did some school there. And she finished her Fine Arts degree in Arizona when I was a grad student there. And we had our first son, Will, the same year of my PhD.

WEEKS: Okay. Very good. All right. So now you were in Australia, came back to Penn, and then the move to Duke – was that just to get back to North Carolina? Or, what was the attraction back to Duke?

MORGAN: Okay, so I've thought a lot about – I'll probably mention this word a few more times. So, like when I made choices in my career, a lot of it was about leverage. Where did I feel like I could make the most difference, have the best colleagues, get the most resources? In some ways, the Duke move doesn't seem to fit because Penn was such a spectacular place. In fact, I loved it so much I'm surprised I left.

But after being there about fifteen years, Sam Preston was Dean. So, I didn't see him at lunch anymore. Tufuku Zuberi, a very productive collaborator of mine wasn't around so much because he was leading initiatives of his own. Doug Massey went to Chicago and came back. But he was gone for a while. So, it just wasn't quite what it was the first eight or nine years that I was there. And Duke University would've probably been my first choice as an undergraduate college if: 1) I could have gotten in; and 2) could have afforded it. So, going to Duke was a real interesting possibility to me.

I also felt that Duke was a place on the rise. It was a very good place, but I felt it could be a lot better in both sociology and demography. So, I considered it both going home, that's true, but also a real opportunity to make a difference there. After all, at Penn I was sitting on the hallway with four past presidents of PAA at the time -- three of them members of the National academy of Sciences. I mean literally Sam Preston, Jane Menken and Doug Massey had offices adjacent to mine. And they were very generous colleagues. I'm not suggesting they soaked up all the air. But when someone thought of Penn, I don't think Phil Morgan was the first person that came to mind.

WEEKS: Now, over time, it seemed to me and maybe this is coincident with your arriving at Duke, that the Carolina Population Center and demography at Duke seemed to start integrating things a bit. Am I right about that or not?

MORGAN: Thank you. Yes. That was one of the strategies I brought with me. Well I thought very highly of UNC. There was tension between the two places because of all kinds of things. But one—there needed to be somebody that was sort of a little less partisan about Duke versus UNC and someone who could do some building of bridges. But Ron Rindfuss was there, he was an old time, long-term collaborator. Barbara Entwisle was there [PAA President in 2007]. Kathy Harris [PAA President in 2009] was a rising star. Kathy was a grad student when I was an assistant professor at Penn. She was a good friend and past collaborator.

So, there were these connections that I think really gave me some credibility with UNC faculty and with my Duke colleagues. And we did start having more activities together, including a yearly event where we shared some of the more exciting, current research that was going on at Duke and UNC. So, I do think I helped bridge the Duke-UNC demographic communities.

Duke's population center was oriented towards aging/mortality. It had aging grants from the National Institute of Aging. Ken Manton was a key person there, a brilliant researcher. Ken Land collaborated with him. But it was a center that was much narrower and much smaller than the Carolina Population Center. It didn't have an NICHD training grant at the time.

WEEKS: Right.

MORGAN: The other attraction of going back to Duke was, I mean you hear this every time Duke and UNC play basketball, that their basketball stadiums are only seven and a half miles apart, which does make collaboration really possible. So, it opened up the possibility for me to work with Kathy Harris again and with Ron Rindfuss. So, it wasn't really a risky move, going to Duke. If things hadn't worked out well at Duke, I felt like I was close enough to UNC that I could rekindle relationships there. But things did work out well and I did draw Duke and UNC population folks together in the meantime.

WEEKS: Yes.

MERCHANT: Can I follow up on that? So, I just have two questions about your time at Duke. You said that there wasn't an NICHD training grant when you got there. Were you able to get one for Duke? Either a training grant or a center grant?

MORGAN: I didn't. I remember having a conversation with Chris Bachrach [PAA President in 2013] about this and she was chastising me a bit about why we hadn't applied for one. And I told her it was because I didn't need one. And it was true.

There are different resource levels at different places. At that particular point in my career, the resources of a Duke faculty member and my personal NIH grants were completely sufficient to carry my research agenda. Maybe that was a little selfish because one thing you can do is, with the training grants and center grants, is you can draw better students and more faculty. But the research triangle is just rich with demographers and the training programs in sociology at Duke were quite willing to take the kinds of students that I wanted to work with. So, frankly, at the time, I didn't see the real need. Now, Duke did win an NICHD training grant after I left. So, other people stepped up and did that. But we did not have one when I was there.

MERCHANT: Thanks. And my other question is you said that when you got to Duke the research there was mostly focused on aging and I was just wondering, it looks like your research is mainly on fertility, whether you were able to apply any frameworks or methods or concepts from the aging research that you were around to your research on fertility.

MORGAN: When I arrived at Duke I didn't really think aging research was all that interesting or important. But I quickly learned that I was very wrong. Aging research in the hands of people like Ken Manton, Ken Land and Jim Vaupel, was fascinating. Vaupel was an especially interesting person, raising incredibly interesting questions about the aging process and the dramatic increases in life expectancy. I was a fertility/family person. That's where I thought all the interesting questions were. That's where I threw myself into. But being at Duke, I listened to my colleagues discuss their work and while I can't say I really did anything that would be considered aging/mortality related in my career, I was influenced by the work in many ways.

Being the Editor of ***Demography*** also helped. Having to read across the discipline made me realize just how viable and interesting a whole set of research questions and problems were. I took a quick look at a couple of the interviews you guys have already done, and one of them was my colleague Doug Massey and he was talking about how Ansley Coale [PAA President in 1967-68] thought migration was incredibly boring. Well, Massey had convinced me way before this time we're talking about, that migration is fascinating. And Preston, on mortality. But the work around aging, I really didn't get right away.

But to answer your question, I didn't pick up and do aging research. At the time, people were moving so quickly into aging, it's where a lot of the new ideas were. It was exciting, incredible new lines of research funding. I made a conscious decision at that point to "stay home" and do fertility research. I felt like somebody should to do it. So that's what I did.

WEEKS: Great. And you did a lot of it. And, of course, that was the focus of your PAA Presidential Address, as well--thinking about low fertility, is this a demographic crisis? I mean, eighteen years later, how do you think your presidential address resonates now?

MORGAN: I think it stands up well. The view in the presidential address is that very low fertility is not inevitable. You can build a 21st century society that is supportive of families and

children, so that fertility doesn't fall to incredibly low levels. But modern societies do have lots of strong anti-natalist forces that can produce very low fertility.

Let me step back a bit. Remember, my initial interest in demography focused on the "population explosion." And if I wanted to do anything about the population explosion, fertility was the place to work. And I think that was the right choice. Fertility decline has been a tremendous and positive change, so my work early was on high fertility in developing countries. So, it was coming full circle to offer a presidential address that was on low fertility. But that high fertility starting point colors my concern about low fertility. What's the alternative? I mean, I didn't say this point blank in the presidential address, but who wanted higher global fertility in 2003 (my presidential address year)? In fact, roughly half the world has got more serious issues with high fertility. So, is low fertility's a problem? I actually used this line in my PAA address. My dad liked to say, "son, that's the kind of problem you want to have." I never liked that. I remember complaining about income taxes and he said, "son, that's a problem you want to have."

Low fertility, that's a problem you want to have. Who has low fertility? Wealthy, educated, highly developed societies. If they can't fix this problem, too bad. The resources are there, I think the know-how is there. It's not the case that low fertility is the result of women wanting to have zero children. It's the result of women not getting around to having the children they say they want. We need to create a society where women/couples can have the number of children they want, and in that situation, I think you'll be getting pretty close to replacement fertility.

WEEKS: You mentioned at the beginning of your presidential address about the fact that half of the world's population is living in low fertility societies, but I did a quick calculation with the UN pop division numbers. And since your presidential address, we've added about another 1.3 billion people to the world's population. So, fertility from that perspective wasn't quite low enough, maybe.

MORGAN: Let me chastise you a little bit because probably half of that growth is not fertility. It's just age structure effects. And I could've said this fifteen years ago. If we went to replacement level of fertility now, they world's population would still grow for another 25 years. I don't fear declining population. I mean, we obviously, don't know the optimum number of humans? That's a question I remember the neo-Malthusian professor Clinton asked when I was in his class– in 1974or so. How many people is the right number?

I don't think we could agree on an optimum population. But would the world really be a horrible place if there were a billion less people in 2100 than now? I don't think so.

WEEKS: Right. Win, you haven't asked any questions. Did you have any specific questions that you wanted to bring up?

BROWN: I am enjoying this interview so much. Phil, it's just great to get a bit of your life story. It's a fascinating one. So, I'm all ears. I did, I always have questions. Why don't I start with one that takes you all the way back to the very bottom of your publications list on your CV. And I noticed the very first paper that shows up on your CV is kind of an anomaly in a way, or maybe it isn't. I'd love to get your thoughts on that. I'm sorry, I don't have the CV in front of me now. But it's something like Prayerfulness in America, which sounds like a very interesting title. I believe it was very early in the 1980's. Where did that come from, and is that a paper that you had going prior to getting into more the demographic content—

MORGAN: Is this the ***Social Forces*** piece, "Are Religious People Nice People?" Or is it about prayer?

MERCHANT: ***Chicago Studies,*** "Prayerfulness in America."

BROWN: That's it.

MORGAN: Look, that's not a – I can't remember where that's published, actually. It's not a major publication. But let me tell you the story. I mean I told you that I've had more opportunities that I deserve. But when I showed up at Arizona, Dudley Duncan taught me in statistics, and I was a quick study. So, I was known as somebody that you want to hire as your research assistant.

About the same time, Andy Greeley took a job in Arizona. Any of you guys know Andy Greeley? He's a famous sociologist and a priest, incredibly prolific. He was at Chicago. Chicago didn't want to give him a tenured job, so he took a job at Arizona. And he would fly in on Monday and teach on Tuesday and Thursday and fly back to Chicago. And this was back in the day when, Hell, a year before this, I was walking over to the computer center with cards to put them in the computer. And Andrew Greeley shows up with the General Social Survey--the whole damn thing on a computer and he could access it immediately.

And he gave that to me and he said, I'm not going to be here much. Look in these data, especially the stuff about religion, and find some interesting stuff. So, I was basically looking around and the first time I saw "frequency of prayer," I said that's kind of interesting. I mean, usually people do "church attendance" or "belief in God," but "frequency of prayer," is a sort of devotional component of religion, not that strongly connected to the institution necessarily. So that's where those papers came from, is doing the work on those data. Andy Greeley was such a fair person. Coming back to Dudley Duncan, every week you had to have progress reports. Every week you'd turn in a progress report. So, I had to do that for Duncan's class. So, I did that for Andy Greeley.

And I did these series of reports on prayer. Who prays, why, "so what" kinds of questions. And he loved them. And he said, write this up. And there's no reason he couldn't have taken a co-authorship, but he didn't. He said go for it. I think he wanted to coax me into the sociology of religion. But I was pretty committed to being a demographer. I loved the measurement advantages of demography. It's hard to tell when someone's religious. I know when someone's born, and someone dies. Those are huge advantages. When I said Duncan was a spectacular teacher, he taught us how to, he taught us the statistics part, but he also taught philosophy of science at the same time. And he has a couple pieces on measurement. He has a book on measurement, actually (**Notes on Social Measurement: Historical And Critical**, Russell Sage Foundation, 1984). And some of it is about population censuses.

And I really appreciate that concreteness of measurement. At least on some demographic variables you have a relatively concrete measurement. What a huge advantage. And people can say, well births aren't perfectly measured. Yeah, but compared to almost anything else, they are. And the same is true of mortality. I have a paper on my vita titled "The Success and Future of Demography." And it really has my views on this. In a lot of ways, demography's golden years may have been the 1970 to 2000 period. Once we start moving away from our core variables that are really measured well, I think we start to struggle—it just becomes more difficult. It's not that the old focus (fertility and mortality) is more important. It's not that we shouldn't study other

things. But there's something valuable about quality measurement on some of your key variables.

BROWN: Phil, can I quickly follow up on that then? I've got a lot of other questions. But just on that point, sort of on the advantage of being a demographer and having precise constructs, I noticed also in your CV that you had written some amount on fertility preferences and intention. So, there you are, in potentially a bit more of a subjective space. I wonder, this goes all the way back to Charlie Westoff [PAA President in 1974-75] and the KAP-Gap and unmet needs. Unmet Need still stands as a really critical part of the family planning part of our field. But it's fraught with the kinds of issues that you're just now talking about in terms of your dependent variable. What's your comment on things like Unmet Need and where should the field go with things like that? If you have any comment on that.

MORGAN: Yeah. Well, just because it's hard to measure, that's no excuse not to try. But even there, being able to compare unmet need with the real measures of fertility and fertility change is advantageous. So, when you really start putting together a set of measures, the Unmet Need comes from the answers people give you on a survey. But, if you see, it's certainly helpful. I'm thinking of Bongaarts' simple framework on proximate determinants. If you've got high levels of unmet need and you put in resources and there's a take-up of contraception, then you should see it in the outcome variable that we can measure well, or at least better.

I don't want to overdo. Even fertility types, like myself, as you say, I have spent a lot of time studying values that people attach to having children, whether they intend to have them or not, but we have a fair number of data sets now where people are asked, do you intend to have a kid. And then we follow them up later and find out whether they did or not. And we can see, there's enough information to get some sense on why they didn't have the ones that they were going to have. And we can also see the flaws in those measures. It's sort of like doing things I really like to do, like playing golf. When am I going to play golf? I love golf. Am I going to play today? No, I'll do it tomorrow. Am I going to do it tomorrow? No, I'm not going to do it tomorrow either. So, a month goes by, and I haven't played golf. Then you can learn these kinds of things from the kinds of longitudinal data we have on intentions and then subsequent behavior. On the unmet need, I mean, that is a tricky concept.

The work that Ryder and Westoff did on unwanted fertility, those are Herculean efforts, trying to get at intendedness of fertility. But it's far from perfect. And all you have to do is read their publications and you'll see it. They acknowledge the difficulty, I think it's actually Ryder in his PAA Presidential Address [Norman Ryder, PAA President in 1972-73] who says, you guys have let me off too easy. These concepts that we constructed around unwanted fertility are a mess. And he challenged us to do better. And if you look at the National Survey of Family Growth, I haven't done this in the last two years, but NSFG now has a battery of questions. As a discussant recently, I said, holy crap. This is almost like an intensive interview with respondents around their intentions. They're asked "do you intend to have another child?" "Would you like to have another child?" And if they were pregnant, "did you intend to get pregnant?" "How did you feel right after you got pregnant?" I'm not doing them justice, but they're impressive sets of questions on both intentions and was the last birth intended.

And again, they're not perfect, but with some kind of triangulation – the way I think about this measurement is like being in quicksand. If you're in quicksand, you just want to find something that's solid. And I think the fertility behavior stuff is solid. You can even sort of grab onto that to

keep from drowning or suffocating in the sand. But it doesn't make all your problems go away. It just gives you a touchpoint, a solid point to try to anchor what you're doing.

BROWN: I think we probably are going to want to use that quicksand quote because that's just about perfect for so many things that we encounter. If, John, it's okay – let me ask one more question. And Phil, this will take you back again to something you mentioned to us earlier, just now, about how you were a quick study, with respect to statistics. When statistics really came at you at that stage, you were a quick study. It all just made sense to you, and I guess you were good at it. And then it takes me back further when you were talking about being a farm boy, essentially, and that you only saw a few ways out and one of them ways accounting. And I guess what that means is that you were probably a pretty good math student back in high school. What would you say about your early experience with math and did that provide you with a special dose of confidence later on when you got into statistics and demography?

MORGAN: I don't want to overestimate the quick study. I'm not sure that I'm, I mean if you gave me one of those standardized tests on math, I'm not sure I'd do so great. But one thing I can say is, looking back into high school, I had great teachers. I was one of the better students in my class. But when I got to college, I quickly had friends who were from places like Chevy Chase, Maryland. I remember my first week of college. We were all having fun. I was accepted or whatever. But they started talking about calculus and how they had taken these two calculus courses in high school and had done well, and you know what? I didn't know what calculus was.

So, I did fine in the math courses that I had, but you know, accounting is pretty much adding, subtracting, multiplying. What I think I was really good at was not advanced mathematics, it was models and data. And really sort of getting very good at handling and being able to use a set of statistics which capture the structure and data. This is what Dudley Duncan taught at the time. And we got a lot of philosophy of science along with it. You may think you have the model, but lots of models fit the same data. I've heard him say that a hundred times. But there are a whole lot of models that don't fit the data. So, you shouldn't jump to: I know the future, I know the answer, the data can only fit this explanation.

People say if you torture the data long enough, it'll give them the answer they want. But that's bullshit. You can torture the data all you want. And it's not going to say yes to some questions. So being able to apply descriptive statistics to data to be able to address important questions was something I was good at. There were people who could do the math better. But the mixing of the substance with the right model test and idea was what I did well.

Another short comment on this is, I got really interested in numbers when there were interesting questions that went with them. Just doing the numbers for the numbers' sake wasn't interesting. But the kinds of population questions that we deal with, questions about whether fertility rates are different, are they changing. When you attach an important question to the enterprise, then the models and the data sort of came to life.

WEEKS: Very good. Phil, before we let you go, we do want to ask a few questions specific about the PAA because you were the PAA president. And obviously, one of the things that we are very aware of on the History Committee is the extent to which the PAA, more than most professional organizations, helps to keep demographers together because there are, I think last time we counted, only two departments of demography in the United States [University of California, Berkeley; and University of Texas, San Antonio]. And so, the PAA, as an association, is very, very crucial in maintaining this sense of demography.

How do you feel that the association itself has progressed over time and what has its role been, not just in your life, but in the life of people that you know?

MORGAN: I'm really indebted to the PAA. I started going, when I was still in graduate school at Arizona. I think the only times I missed a PAA were when I was in Australia, and it was too difficult to get back. Early in my career, every year I had to have a paper. Every year I had to have a new paper for the PAA and one for the American Sociological Association meetings. And if I didn't let those papers die, I knew I'd be productive enough to be successful.

So, I had to have two new papers a year, and don't let them die. And PAA gave me a place to present my work. People were interested in the things I was interested in. I was lucky enough to be nominated for offices and I liked being part of the association as, at least from my experience, an incredibly cooperative group of people, all with a similar interest in seeing the profession and their students succeed. Really a wonderful experience. The highlight of my career was being PAA president, without question. I loved every part of being PAA president. I loved putting the meeting together. It's not that hard because there's a template. The program from year to year doesn't change dramatically. But there are seven, eight, ten sessions that you can put your stamp on.

I remember walking around the PAA that I was in charge of organizing and just seeing all of that stuff happening and it was just thrilling. I mean this was an hour or two before I gave my presidential address. I wasn't nervous in the least. I just, I loved watching all that stuff work. And this is the absolute truth. I must have asked 500 people to do something the year I was PAA President, associated with the meetings. No one ever said no. Not one person ever said no. It's remarkable, absolutely remarkable. You need so much volunteer work. You need discussants. You need organizers. It goes on and on and I made a lot of those calls. And never did anyone say, "too busy."

It's just remarkable and such a good feeling where people not only said yes, but they said yes and thanks for asking. Terrific association, absolutely terrific. I probably let something slip through, so ask a follow up.

WEEKS: Well one of the things that obviously mattered with respect to demography, like with every field, is how funding influences the work that we do. Where the funding comes from and what the funding agenda is, then kind of influences the research that gets done. How do you feel about that? Has that been a major influence in your life?

MORGAN: Again, I feel like I was pretty lucky. The things I was interested in were, for much of my career, were things that were quite fundable. In the US, the first big grant/contract that Rindfuss and I won focused on delayed childbearing. So, I mean, I just sort of walked right into that. Numbers and timing of fertility was what I did. I do think that the funding can shift, and people wanted me to move onto new priorities. And that can be bad when you haven't solved the old problems yet. But still, I think that change has got to be good.

When people want to fund and want to learn about some new issues, how do you pull researchers in that direction? If the funding stays on the old issues, that's where they're going to stay. So yeah, I felt the pull of the money going into the National Institute on Aging. And maybe that was part of my first reaction too, before I learned better and why it was so interesting, I felt that aging research impacted how difficult it was to get research monies. But I actually think it's a healthy thing.

A lot of foundations, they've moved on from population training programs to other things. It's hard to argue those other things they've moved onto are not important. And it's hard to argue they don't deserve to be at center stage a little more. So, yeah, there can be a downside of moving on a little too quickly or not being willing to stay the course and finish off an issue. But the flexibility of funding to move where new problems arise, I think, is a good thing. I started studying fertility when teenage childbearing was hot. Hey, those problems have faded dramatically.

WEEKS: So, Phil, are there things that you wanted to be able to tell us and to have on the record that we haven't asked you?

MORGAN: No, I don't think so. Again, it was a little bit uncomfortable in some ways, giving this interview, seeing as how I'd stepped away, but I don't really feel like stepping away was running away in any respect. I loved my career as a demographer. I'm proud of my career as a demographer. The PAA is a major part of that story, as are the institutions that I was a part of, including the support from NIH.

And I feel like demography's got a wonderful future. I'm sure there's a lot left to do. But, in a lot of ways, I experienced a wonderful and exciting slice of demographic history.

Meeting was recorded on Zoom, and transcribed by GMR Transcription Services (www.gmrtranscription.com)

Edited by John Weeks, 7 December 2021

Reviewed and approved by S. Philip Morgan, 15 February 2022

IS LOW FERTILITY A TWENTY-FIRST-CENTURY DEMOGRAPHIC CRISIS?*

S. PHILIP MORGAN

Nearly half of the world's population in 2000 lived in countries with fertility rates at or below replacement level, and nearly all countries will reach low fertility levels in the next two decades. Concerns about low fertility, fertility that is well below replacement, are widespread. But there are both persistent rationales for having children and institutional adjustments that can make the widespread intentions for two children attainable, even in increasingly individualistic and egalitarian societies.

Like many demographers of my generation, I was attracted to demography by *The Population Bomb* (Ehrlich 1974), *The Limits to Growth* (Meadows et al. 1974) and *The Tragedy of the Commons* (Hardin 1968), the neo-Malthusian classics. The crisis was high fertility and the resulting rapid population growth. The population crisis was dire, seemed intractable, and afflicted the poorest countries.

But an ongoing global fertility transition has made population stabilization by the middle of the twenty-first century a reasonable forecast. Stylized versions of these fertility transitions posit shifts from high to low fertility, with fertility rates eventually oscillating around replacement level. However, observed declines show the total fertility rate (TFR) slicing below replacement levels with little resistance, and many are skeptical about its recuperation (e.g., Lesthaeghe and Willems 1999).

Herein lies a potential demographic crisis: the dynamics of population decline mirror those of population increase. Halving times mirror doubling times, and population momentum can be negative as well as positive (Lutz, O'Neill, and Scherbov 2003). The social problems associated with low fertility are also mirror images of those associated with high fertility: women now have fewer children than they want (see, e.g., Hewlett's (2002) high-profile book about the personal crises of unintended childlessness), and women have children at late ages, with some questioning whether they should have delayed childbearing (e.g., Wolf 2001). At the aggregate level, dramatic population aging threatens the solvency of programs for the old, and country-level population decline could soon become a common reality (Demeny 2003).

But I'm getting ahead of my story. Below, I review the dramatic, ongoing global spread of low fertility. Then I turn to the heart of my argument, which acknowledges the inevitability of low fertility but not of very low fertility. Finally, I answer the question posed in my title: is low fertility a twenty-first-century crisis?

EMERGING GLOBAL LOW FERTILITY

Table 1 shows a year-2000 snapshot of the global fertility transition based on data from the United Nations (United Nations Population Division 2002). Each of the following observations are important and dramatic:

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Table 1. Stages of the Transition to Low Fertility in 187 Countries

	No Transition (1)	Declining With TFR > 5 (2)	Declining With 5 < TFR < 2.1 (3)	Declined to TFR ≤ 2.1 (4)	Early Transition and Baby Boom TFR < 2.1 (5)	Early Transition and 2.1 < TFR < 3 (6)	Total (7)
Number of Countries	16	32	73	23	41	2	187
Percentage of Global Population Living in Countries at Selected Stages of Transition	3.0	9.5	42.6	24.6	19.6	0.7	100.0

Source: United Nations Population Division (2002: table 1.1).

1. The United Nations (U.N.) lists only 16 (of a total of 187) countries as *not* showing clear evidence of a fertility transition (see column 1). Lingering high fertility has become geographically isolated and affects fewer of the world's countries and people. Only 3% of the global population now lives in countries not yet in fertility transition. The U.N. projects that all these countries will soon begin a transition.

2. At the other extreme, there are 64 countries with fertility at replacement level or lower. Twenty-three countries recently made the transition to low fertility (see column 4), and another 41 countries have had several decades of low fertility punctuated by a baby boom and bust (see column 5). Together, approximately 45% of the world's population now lives in countries with low fertility.

3. Between these extremes, 105 countries are said to be experiencing fertility transitions (see columns 2 and 3) because empirical evidence shows that once begun, these declines do not stop until fertility reaches replacement level or below (see Bongaarts and Watkins 1996). For 96% of these 105 countries in transition, their most recent estimate is their lowest recorded fertility.[1]

4. Only two countries have halted their transition at a fertility level that is substantially above replacement (at TFRs of 2.5 to 3.0). Others with currently high TFRs could do so, but evidence clearly suggests that arrested declines will be rare.

Thus, these data and U.N. projections portend a remarkable achievement. In a few decades, high fertility will likely be of historical interest (Bongaarts and Bulatao 2000; United Nations Population Division 2003a).[2]

Replacing a focus on high fertility, concerns about very low fertility have already arrived. Table 2 shows TFRs for six countries. The lowest observed TFRs, for countries like Spain and Italy, provide an empirical basis for concerns about population decline. Fertility in a number of countries is well below replacement. As an example of the consequences of such low fertility, the U.N. projection of the Italian population to 2050 (with

1. Calculations are from United Nations Population Division (2000: tables I.3–I.6).

2. The United Nations Population Division (2003a) forecasts the TFRs of the 49 poorest countries to be low but above replacement level. For these countries, the forecasts are for TFR levels between 2 and 3 in 2050, with the medium variant of approximately 2.5. Thus, fertility is not forecast to be at replacement level or below for all countries.

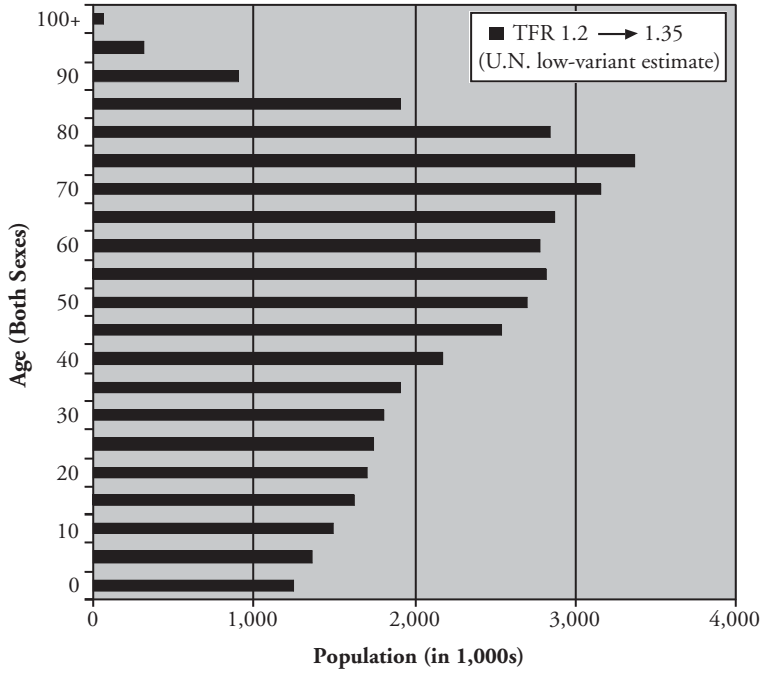
Table 2. Variation in Low Total Fertility Rates (TFR)

Country	TFR
Spain	1.16
Italy	1.20
Greece	1.30
Germany	1.33
France	1.89
United States	2.03
U.S.: White non-Hispanic	1.84

Sources: United Nations Population Division (2002). U.S. data for 2001 are from Ventura, Hamilton, and Sutton (2003).

only a modest increase in its current fertility of 1.2 to 1.35, the U.N. low-variant estimate) produces a 30% decline in total population. Figure 1 shows the projected 2050 age distribution. The old-age dependency burden implied here is dramatic. Such pervasive low fertility and rapidly aging populations could be cited in constructing low fertility as a major twenty-first-century crisis. But there are exceptions to very low fertility; the United

Figure 1. Projected Population by Age: Italy 2050



Source: United Nations Population Division (2003b).

States is the most obvious exception. Also note that the higher fertility of the United States cannot be attributed solely to its minority populations. Non-Hispanic whites have a TFR of 1.84, a high fertility level by European standards.

IS *VERY* LOW FERTILITY INEVITABLE?

Some evidence and arguments suggest that very low fertility is inevitable. For example, in his presidential address, Bumpass (1990) argued that secular trends producing family change, and an imbedded fertility transition, had not run their course. Commenting on classic arguments about family change (e.g., Goode 1963), Bumpass argued that "...the theoretical perspectives of a half-century ago were essentially correct" (p. 483). These theories linked modernization and family change and had essentially two components: a structural component and an ideological one. The structural component acknowledged the changed circumstances of industrial (and postindustrial) economies, which increased the cost of children. Of special importance was the incompatibility of work and familial obligations for women. Citing not only this earlier work but also the contemporary work of Lesthaeghe and colleagues (Lesthaeghe 1983; Lesthaeghe and Surkyn 1988), Bumpass argued that this structural change was accompanied by ideologies that stressed that individuals should assess these costs and act in their self-interest. Bumpass concluded that "there is no reason to think that these processes are exhausted or are likely to reverse" (p. 493).

Another view is that a future with very low fertility is much less certain. I assign this view the title from Campbell's (1974) presidential address, "beyond the demographic transition." Campbell focused on a demographic surprise: the baby boom. This baby boom and subsequent bust called for explanation distinct from the secular decline in high-parity births. An adequate explanation, focusing on the United States, follows from Ryder's (1980) decomposition of trends in the baby boom and bust. Ryder's analysis isolated the dynamic components of change as the timing and number of low-parity births. I argue that by moving beyond the demographic transition and focusing on low-parity births, one can anticipate a floor on low fertility and better understand low-fertility trends and differences. From this view, very low fertility is not inevitable. Of course, this is very different from saying it will never be observed.

To argue against the inevitability of very low fertility, I will first emphasize the distinction between low- and high-parity births and stress that motivations remain for low-parity births. Second, I will suggest that institutional adjustments are possible that would make having small families compatible with contemporary developed-country settings.

Low- Versus High-Parity Births

The distinction between first and second births and higher-order births is crucial. In economically advanced contexts, 75%–90% of births are first or second births. Trends in this direction are long term—secular fertility declines have resulted from a move to small families, not to "no families." Without such a distinction between low- and high-parity births, one might reasonably assume that the average family size could decline to zero. Children are costly in modern contexts: the fewer children one has, the better, and having no children is the best. However, there have not been large increases in the proportion of women who intend to have no children. In fact, the proportion of women who intend to have two children is dominant in most developed countries (Bongaarts 2002).

One explanation for this pervasive intention to have two children comes from studies of the value of children, which have shown that the rationales for having first and second children differ from those for higher-order births. Using data from the mid-1970s for the United States, the Philippines, and South Korea, Bulatao (1981) showed that across contexts, first children were desired for affective reasons, such as to have a child to love and care for, to carry on the family name, and to bring spouses closer together.

Second children were rationalized as "family building"; to provide a sibling for the first child was especially important. Also, second and third births were frequently desired for balancing the sex composition of the family.[3] Higher-order births served primarily economic functions. Bulatao suggested that higher-parity births declined because the rationales for them were undermined by socioeconomic change, whereas factors motivating first and second births were not.

Furthermore, an important ideological shift has encouraged parents to focus attention on just a few children. At the heart of this change is the notion that each child is unique and deserves substantial parental investment, including purchased resources and parental time and attention. Being a good parent is now largely inconsistent with having more than a small number of children. Blake (1972) argued that in the 1960s, there were not strong norms against having large families. This has changed: large families are now viewed as inconsistent with good parenting. Because of this ideological change, low fertility coexists with strong and pervasive desires to be a parent.

Clearly, there have been other relevant ideological changes. A major argument for the continued decline of fertility below replacement levels points to powerful trends toward individualism and self-actualization. But are these trends inevitably antinatalist? Giddens (1991) theorized wide-ranging effects of the rejection of the traditional structured life course. People now have great flexibility in how they structure their lives, making it difficult to find standards against which to judge their own progress or success. Achieving identity through a coherent "narrative of the self" is always a work in progress. Raising a child can bring predictable routines to daily life that promote well-being and provide continuity to one's "narrative" (see Friedman, Hechter, and Kanazawa 1994; King 2000; Morgan and King 2001). In short, beyond adolescence and young adulthood, parenthood may provide a powerful source of connectedness and meaning.

Thus, parenthood can be constructed as a response to increased individualism and self-actualization. Furthermore, greater individualism may undermine traditional constraints on fertility. For instance, continued secularization and individualism could undermine norms against nonmarital childbearing and against parenthood by gay couples. Currently, all western countries that approximate replacement-level fertility have significant levels of nonmarital childbearing (Rindfuss, Guzzo, and Morgan forthcoming).

My final point may be more controversial. I believe that biological predispositions reinforce the desire for parenthood, especially the affective reasons for having children (that parents fall in love with their children), and the willingness of parents to have fewer children to increase their children's life chances (see Lam 2003). It is true that biological predispositions don't "cause anything." As Pinker (1997, 2002) noted, genes associated with risk taking don't make people take risks, but they do increase the pleasure sensation resulting from things like jumping out of a plane or driving fast. Likewise, neural circuitry producing a warm glow when parents hold their helpless, big-eyed infant didn't make them have that baby, but it does help them to fall in love with the baby. In fact, love may be the root of altruism toward one's children. Hrdy (1999) asserted that maternal attachment is conditional, as evidenced most strongly in the coevolution of babies to extract maternal commitment. In short, having few children and heavily investing in them "fits" well with our evolutionary inheritance and, thus, with the neural wiring in our and our children's brains.

In sum, even "large families" in the twenty-first century will be small. Nevertheless, motivations and rationales for first, second, and (sometimes) third children remain relevant in modern contexts. Ideological change, psychological needs, and biological predispositions buttress these motivations and rationales.

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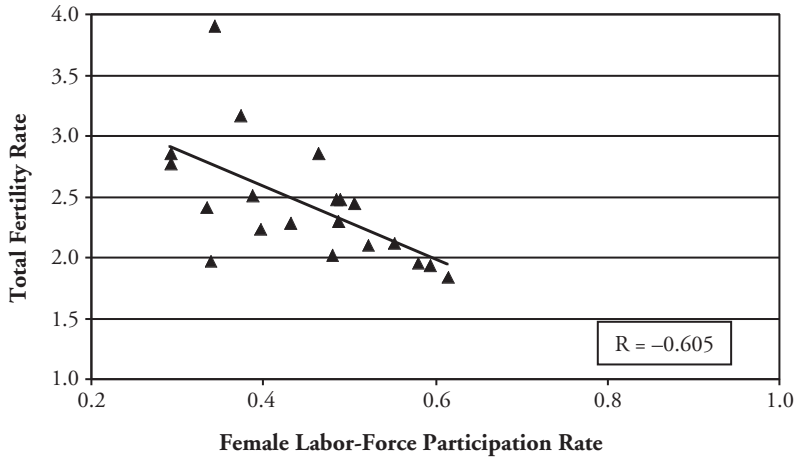
3. Moves toward gender equality may be undermining the importance of balancing the sex composition of the family (Pollard and Morgan 2002).

Figure 2. Total Fertility Rate, by Female Labor-Force Participation Rate, 1960: 22 OECD Countries



Source: Rindfuss et al. (forthcoming).

Institutional Adjustments

The second stage of my argument against the inevitability of very low fertility is that institutional adjustments can make small families feasible in the twenty-first century. Social contexts can support or thwart desires for children. My argument is not new. At the heart of demography is the decomposition of change into that owing to distributional shifts and to changing rates. A common way to think about future trends is in terms of secular change in the distributions (e.g., age or educational distributions) while other distribution-specific rates stay the same. This is a useful counterfactual, but one frequently counter to fact. For example, Preston (1984) noted the increasing proportion of elderly and a decreasing proportion of children over the 1960–1980 period. Such changes (given a Malthusian, fixed-resource constraint) should have advantaged the young and disadvantaged the old. Instead, predictable changes across time in the sizes of America's dependent groups (children and the elderly) were trumped in importance by institutional responses. Key in Preston's account were public policy decisions to provide collectively for the medical care and income maintenance of older persons while maintaining a benign neglect of children.

Let's take this as a template for thinking about the importance of institutions and institutional responses. At the heart of most explanations of recent changes in family and fertility are the changing roles of women and their movement into the labor force. A straightforward application of distributional shifts and constant rates projects lower fertility with greater female labor-force participation.

Rindfuss, Guzzo, and I (forthcoming) showed such an association in cross-sectional data for 22 countries in the Organization for Economic Cooperation and Development (OECD) with multiple decades of low-fertility experience (see Figure 2; the data in the figure are for 1960, but the data for 1950 and 1970 produce the same result). This negative cross-sectional association makes perfect sense when fertility is relatively high. High fertility is incompatible with women's nonfamilial work in most observed or likely contexts.

Figure 3. Total Fertility Rate, by Female Labor-Force Participation Rate, 1990: 22 OECD Countries

Source: Rindfuss et al. (forthcoming).

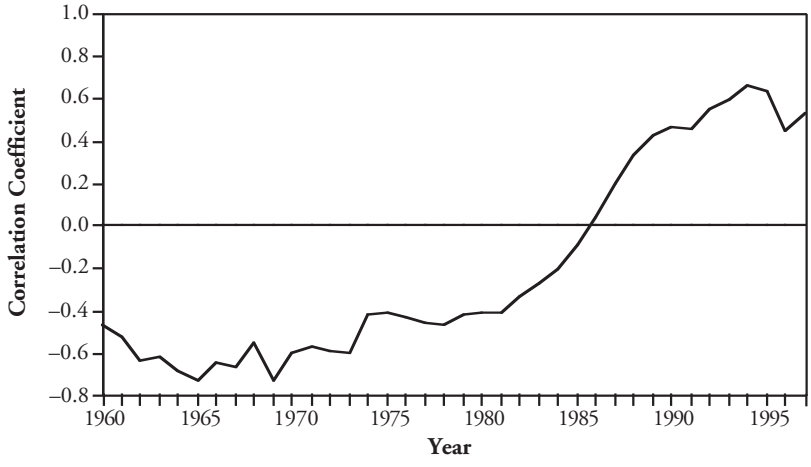
In contrast, many of us who have studied fertility over multiple decades find the scatterplot for 1990, shown in Figure 3, remarkable. By 1990, fertility was much lower than in 1960. But the anomalous finding is the reversal of the country-level association between female labor-force participation and fertility: the negative association is now strongly positive.

Figure 4 plots the simple correlation coefficient, calculated year-by-year, between the TFR and female labor-force participation. This graphic reveals that the previous scatterplots shown in Figures 2 and 3 characterize a dramatic change from a strong negative association to strong positive one.

An adequate explanation for this change requires a move away from cross-sectional analysis. Let us compare two countries that typify this change: the United States, which had the highest 1997 TFR among the 22 OECD countries we examined (Rindfuss et al. forthcoming), and Italy, which had the lowest. As Table 3 shows, both countries have over two decades of replacement-level or lower fertility experience, and both countries experienced increases in female labor-force participation that averaged 0.4% and 0.5% per year in Italy and the United States, respectively. Over this period, the U.S. TFR was insensitive to increasing female labor-force participation. In contrast, a 1% increase in female labor-force participation in Italy was associated with greater than a 3.2% decline in fertility. How could such a pervasive and fundamental social change (increasing female labor-force participation) accompany such divergent fertility trends?

The answer lies in varying institutional responses. Rindfuss (1991) stressed the importance of affordable, quality child care in weakening the incompatibility of work and childbearing and child rearing. Bianchi (2000) stressed gender and technological changes that affected the division of household labor (also see Bianchi et al. 2000; Gershuny 2000). In the United States, these changes allowed women's time with children to remain virtually unchanged over the 1965–1990 period. Esping-Anderson (1999) argued that welfare-state regimes offer different institutional environments for parenting. McDonald (2000) conceptualized very low fertility as an incompatible change in familial and nonfamilial

Figure 4. Year-by-Year Correlation Between the Total Fertility Rate and the Female Labor-Force Participation Rate, 1960–1995: 22 OECD Countries



Source: Rindfuss et al. (forthcoming).

institutions. I find the intuitive appeal of these institutional arguments to be powerful and their overlap to be considerable.

Quantitative tests of institutional arguments provide some evidence of pronatalist impacts, but tests to date are limited by a focus on a few specific features, especially family-related public policy. A broader consideration of the *cumulative* childbearing environment would provide compelling explanations of cross-national differences. Quantitative studies of multiple countries face great challenges in measuring and incorporating this context. Pampel's (2001) interesting and important country-level analysis of welfare-state and institutional arguments suggests that governmental support can attenuate the effects of increasing female labor-force participation on fertility, but his regressions leave the United States as a large outlier. With little public support of families, why is U.S. fertility among the highest in the developed world? The U.S. story clearly does not lie in the relative generosity of welfare systems that were explicitly included in Pampel's analysis; instead, it must lie in the responsiveness of nongovernmental institutions (perhaps consumer markets or changes in the family, especially gender roles).

For example, my colleagues and I (DiPrete et al. forthcoming) compared the costs of a first child in West Germany and the United States. Costs were narrowly defined as the

Table 3. Sensitivity/Elasticity of Female Labor-Force Participation (FLFP) and the Total Fertility Rate (TFR): Italy and the United States

Country	Years Since First TFR < 2.0	Average FLFP Increase	Sensitivity or Elasticity[a]	1997 TFR	Sensitivity Rank
Italy	21	0.4	–3.2	1.19	1
United States	25	0.5	0.3	2.06	22

[a]Sensitivity or elasticity = $\ln(TFR_t / TFR_1) / \ln(FLFP_t / FLFP_1)$, where subscript 1 = the first year in series TFR < 2.0 and year t = 1997 or the last year with observed data.

change in family income that accompanies a birth. Longitudinal data showed that West German women exit the labor force for much longer periods than do American women, with a correspondingly greater decline in earnings for West German women. German government transfers compensate for a substantial part of this difference, but the net costs on this dimension remain greater for West German women. Indeed, the greater cost and longer exits from the labor force are associated with lower rates of first birth in West Germany than in the United States. Apparently, institutional responses other than government transfers, perhaps greater gender equality and labor-market responses, such as flex time, more than compensate for the paltry U.S. government transfers (in women's and couples' decisions to have a first child).

In sum, the context matters, and contexts vary. Covariation of institutional contexts with fertility levels shows that very low fertility is not inevitable. However, this statement leaves us a long way from a precise answer to the question, what mix of institutions produce an environment conducive to replacement-level fertility?

Answering this question is the agenda of the next decade for studies of low fertility. The richness of analyses will be aided by the study of more societies (and more non-Western societies) as low fertility spreads. One way that demography has traditionally contributed to answering such questions is by identifying more precisely what proximate variables produce fertility change and variability.

An Integrative Framework

I recommend Bongaarts' (2001a, 2002) conceptual framework, which views the TFR as resulting from the population's intended family size multiplied by a set of factors that are not subsumed or cannot be subsumed in the respondent's fertility intentions. These factors reflect unanticipated effects. In Table 4, I offer hypothetical values for these proximate determinants that are more fully justified elsewhere (Morgan and Hagewen forthcoming). The values present a stylized view of Italy and the United States. Consistent with substantial evidence suggesting small differences in intended family sizes across developed countries (Bongaarts 2001b, 2002),[4] let us assume an intended family size of 2.0 and 2.2 for Italy and the United States, respectively.

In this framework, a parameter of 1.0 reflects no effect net of intentions, values above 1.0 increase fertility relative to intentions, and values lower than 1.0 decrease fertility relative to intentions. For example, the net effect of unwanted fertility (F_u) pushes the TFR higher than would be predicted by intentions alone: by about 12% in the United States and by 1/3 this amount in Italy. Not having a child of the sex one prefers (F_g) or having a child that dies (F_r) also cannot be anticipated and may increase fertility in a similar way, but these effects are small and do not vary in this illustration.

The last three factors—the tempo effect, infecundity, and competition—are potentially powerful in explaining very low fertility. Bongaarts and Feeney (1998) showed that adjustments for fertility delay (F_t), the postponement of births to later ages (and, necessarily, subsequent years), can explain a substantial part of the very low fertility observed in contemporary settings. The estimates for fertility delay capture the range of values observed circa 1995. Specifically, shifts in the timing of childbearing reduce fertility by about 15% (a factor of .85) in Italy and by 8% (a factor of .92) in the United States. As Bongaarts (2002; Bongaarts and Feeney 1998) has pointed out, these reductions are, by definition, temporary (on a decadal time scale).

Infecundity is the flip side of unwanted fertility. Intended family sizes are inevitably reduced by infecundity, and this impact (F_i) is directly related to the age pattern of

4. Of course, more sophisticated questions and analyses may reveal differences between countries. This perspective should refocus demographers' attention on fertility intentions and levels of certainty respondents attach to their intentions.

Table 4. Bongaarts's Conceptual Model of the Factors Affecting the Period Total Fertility Rate (TFR)[a]: Illustrative Values for Italy and the United States

		Country	
Component	Description	Posttransition Italy	Posttransition United States
IFS	intended family size	2	2.2
F_u	unwanted fertility	1.04	1.12
F_g	gender preferences	1.02	1.02
F_r	replacement effect	1.005	1.005
F_t	tempo effect	0.85	0.92
F_i	infecundity	0.9	0.95
F_c	competition	0.75	0.9
TFR		1.22	1.99

[a]$\text{TFR} = \text{IFS} \times F_u \times F_g \times F_r \times F_t \times F_i \times F_c$.

childbearing. The pattern of later childbearing in Italy reduces fertility by 10%, while the pattern in the United States reduces fertility by half this amount.

Finally, competition (F_c) refers to decisions to revise childbearing intentions, that is, to have fewer (or more) children than anticipated, on the basis of one's life experiences. One might view this as the extent of antinatalist opportunities or the lack of institutional adjustment reducing say, work-family incompatibility. But also included in this variable are reductions in intended family size later in the life course because one does not marry and does not want to have a nonmarital birth or to raise a child without a committed partner. As an illustration of these combined forces, Ginsborg (2003:73–74) suggested the following explanation for low Italian fertility:

> (Italy) offered its own peculiar mixture of transformation and continuity . . . . (T)here were strong forces pushing towards a European model of modernity . . . leading to greater individual choice, the spread of contraception and legalized abortion, the partial emancipation of women and their entry into the labor market. (In addition), tradition weighed heavily in both the public and private spheres: the felt obligation to have children within marriage, the power of the family as an intergenerational collective, the state's disinterest . . . in reproductive politics.

Consistent with evidence I presented earlier, especially with the data presented in Table 3, the competition parameter (F_c) is set to reflect sharp differences in institutional constraints between Italy and the United States.

I find this proximate-determinants model useful. Note how a modest set of hypothetical differences cumulate to produce substantial differences in fertility. Low fertility has multiple causes, and convincing explanations may read like country-specific social histories.

Quesnel-Vallée and I (forthcoming) applied this model to U.S. longitudinal data from the National Longitudinal Survey of Youth for the period 1982–2000. Because we focused on a cohort of women, we were able to eliminate the tempo effect (F_t), an important factor in Bongaarts's period formulation. We showed that only about 38% of women realized their stated intended parity at age 22 by age 40. The individual-level error between intent

and behavior averaged 1.0 birth per woman; the net error was –0.33 birth. Thus, consistent with much prior research (e.g., Westoff and Ryder 1977), the predictive validity of intentions is far from perfect, and errors do not necessarily "balance"; in this case, women more frequently missed low than high. Our prediction is that such an analysis for Italy would show a similar level of error but, following from Bongaarts's framework, with a much larger negative net error. Italians are almost always missing on the low side, that is, they almost always have fewer children than intended.

We (Quesnel-Vallée and Morgan forthcoming) also found that errors between intended and realized family sizes are ordered, as one would expect given the arguments I presented so far. Related to the competition parameter, those who do not marry, or marry late, are likely to have fewer births than intended. This occurs in a context in which roughly one-third of births are nonmarital; marriage would likely play a greater role in Italy, where nonmarital births are rare. Highly educated women, but not highly educated men, are more likely than their less-educated counterparts to have fewer children than intended. We interpreted this finding as an effect of competition between employment and familial options that are felt more intensely by women than by men. Finally, women and men who have children later—say, after age 25—are much more likely to have fewer children than intended (compared with those having children earlier). This finding could be explained in terms of infecundity at later ages, although other interpretations are plausible.

By way of summary and as a proposed working model, variation in low fertility is not produced by a disinterest in having children. Most women want children, and their mean intentions approximate what is needed for replacement-level fertility. But as women age, they are faced with a set of competing demands that are most easily accommodated by a delay in fertility. Postponement is a major reason for contemporary low fertility. Demographers know that shifts in the timing of childbearing affect period fertility rates, but few appreciate that postponement effects can act for three or more decades at substantial levels. The good news is that the underlying quantum of period fertility is substantially greater than the currently very low TFRs.

But this "mechanical" effect of temporal shifts is not the full story. Although postponement is frequently the least costly solution to competing fertility and nonfamilial opportunities, postponement brings risks that women will not have all the children they intend. One reason is the higher levels of infecundity at older ages. In addition, at older ages, competing demands may interact with being "too old," in a social sense, for children (Rindfuss and Bumpass 1978). Thus, women revise their intentions downward at older ages, and continuing delay and competing opportunities translate some delayed fertility into forgone fertility.

Although this general process is occurring everywhere in the developed world, it plays out differently in particular societies. Public policy and institutional responses *can* assist women in realizing their intended fertility by easing the difficulty of combining child rearing, working, and participating in other activities, and by making parenthood practical at younger ages (Lutz et al. 2003). Some countries have contexts that are more conducive to childbearing than do others. Current cross-national fertility differences are largely the unanticipated consequences of current public policy and institutional arrangements. But these differences indicate that lowest-low fertility is not our inevitable destination and demise.

IS LOW FERTILITY A "CRISIS"?

One need not be a pessimist to concede that a future without problems is unlikely. Cherlin (1999), however, warned against "going to extremes" and defining all problems as either a crisis or as trivial. Some problems and challenges are real but are of a second order. Perspective can be gained by identifying some as the kind of problems one would like to have. What characteristics define problems one would like to have? First, these problems

result from solving some bigger, more troublesome challenge. Second, these problems have solutions and befall those with the resources to solve them.

In my view, low fertility is not a twenty-first-century crisis—not yet, anyway. It is a genuine problem, but the kind of problem we want to have. That is, it is the result of solving a bigger, more threatening social problem: the crisis of continued population growth. Low fertility is also a problem that can be addressed through public policy and institutional adjustments. Finally, low fertility is a problem that befalls developed countries that, by and large, have the resources to respond.

At the global level, population growth will slow over the next few decades because of increasingly pervasive low fertility. For those of us attracted to demography by neo-Malthusian concerns and who still believe that there are limits to human population growth, how can this be seen as anything but good news? It is clearly good news compared with the forecasts that primed the interests of many contemporary demographers. Continued attention to this decline and efforts to assure it are crucial. Besides, it was naive to think that fertility would magically stabilize exactly at replacement levels. Thinking globally, I prefer the current low-fertility problem to fertility at 2.5 or 3 births per woman, at least for the first half of the twenty-first century.

Low fertility produces rapidly aging populations and possible country-level population decline. It will reduce the proportion of the global population living in Europe and North America (see Demeny 2003). For whom is this a problem? It clearly depends on one's perspective. An African American colleague recently questioned my research agenda: "so you're studying the disappearance of white folks." I was taken aback; this is not how I prefer to characterize my work. But he had made his point: for many, "fewer white people" does not sound like the greatest crisis of the twenty-first century.

Buchanan (2002), in *Death of the West*, was mightily concerned. He embraced the concept of differential growth rates with adolescent hormonal passion and subtlety. The book's subtitle is its thesis: *How Dying Populations and Immigrant Invasions Imperil Our Country and Civilization*. Buchanan's argument, like Wattenberg's (1989) earlier version, *The Birth Dearth*, is only the latest in a vintage that links population decline, family decline, and the decline of a "valued moral or national order" (Teitelbaum and Winter 1985:132).

Less polemical low-fertility concerns focus on a shortage of warriors, workers, and consumers. But given current technology and the global economy, these concerns seem to be second-order twenty-first-century problems. Fans of U.S. military might are breathless over its speed and technology, not its size. Furthermore, in a global economy, workers and consumers don't need to be homegrown or national coresidents.[5]

Of course, the severity of the problem of low fertility and population decline varies on a country-by-country basis. Effective responses bring to mind the concept of a multiphasic response (Davis 1963). Moderate levels of immigration could easily offset modest below-replacement levels of fertility (say, cohort fertility of 1.7 or 1.8 births per woman). Source populations are numerous and willing (given receptive environments). In addition, short-term concerns about labor-force availability could easily be satisfied by postponing retirement. Retirement ages in some countries are low when compared with healthy life expectancy.

Finally, for societies that cannot even approximate replacement fertility on a decadal time scale, a full-blown crisis exists. For such countries, there is likely much more wrong than low fertility. Societies that can respond to the legitimate needs of their citizens and invest in the next generations will, I believe, approximate replacement-level fertility. These are places where the low-fertility public policy battles should be fought. A coalition of those concerned about low fertility, the welfare of children, the stress of the second shift,

5. For that matter, soldiers need not be citizens. Noncitizen residents are numerous and are targeted recruits for the U.S. all-volunteer military.

and gender equality could press for institutional adjustments that improve the quality of life for mothers, fathers, children, and families. Demographic research can provide evidence regarding which changes would make a difference. If enacted, such changes might just allow women to have the children they want while providing the children that low-fertility societies need.

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