

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with J. Richard Udry PAA President in 1994



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)

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J. RICHARD UDRY

PAA President in 1994 (No. 57). Interview with John Weeks in Chapel Hill, North Carolina, in 2006.

CAREER HIGHLIGHTS: [Joe] Richard (Dick) Udry was born in 1928 in Covington, Kentucky, where he grew up. He then received his undergraduate degree at Northwestern University in Chicago, just prior to being drafted into the U.S. Marine Corps as a result of the Korean War. That took him to southern California, and after his military service, he taught school in the Los Angeles area and then used his GI Bill benefits to enroll in graduate school at the University of Southern California, where he obtained his Ph.D. in Sociology in 1960. After teaching briefly at Chaffey College and California State Polytechnic College at San Luis Obispo, he moved in 1965 to the University of North Carolina at Chapel Hill where he remained for the rest of his career. Dr. Udry was a Kenan Distinguished Professor of maternal and child health in the University of North Carolina, Chapel Hill, Gillings School of Global Public Health and professor of sociology in the College of Arts and Sciences. He also directed the university's Carolina Population Center (CPC) from 1977 to 1992. During his nearly 50-year career, Dr. Udry pioneered research that integrated biological and sociological models of human behavior in the areas of adolescent behavior and health, sexual behavior, and women's gender roles, and developed the National Longitudinal Study of Adolescent Health (Add Health). [Dr. Udry died at his home in Chapel Hill in 2012 at age 83.]

JRW: We are at Top of the Hill restaurant on Franklin Street in Chapel Hill interviewing Dr. J. Richard Udry, Kenan Professor of Maternal and Child Health and Sociology at the University of North Carolina, Chapel Hill. He was President of the Population Association in 1994.

UDRY: You also may not know that I retired in 2005.

JRW: Okay, so you are now Kenan Professor Emeritus.

UDRY: I suppose. One of the things I learned in retiring was once you are retired, you can call yourself any damn thing you want because nobody tells you.

JRW: When was the last time you actually taught a class here?

UDRY: I haven't taught a class since the early 90's.

JRW: So your grants have bought off all of your time since then.

UDRY: Yes.

JRW: And when you were teaching here, were you actually teaching both in the School of Public Health and in Sociology.

UDRY: Yes.

JRW: You taught what, one course each?

UDRY: No, the last course I taught in Sociology was a seminar in gender and the last one I taught in maternal and child health was a graduate seminar where people were working on their dissertations.

JRW: So let's go back and review your career--back to graduate school days or even before that if you want. You got your doctorate at USC in Sociology in 1960.

UDRY: Yep.

JRW: What was it that attracted you to that program at the time?

UDRY: There wasn't anything that attracted me to that program except the fact that I was teaching high school and I was using up my GI Bill just enough to keep it active and so I could go to school at Southern Cal part time and from my house I could drive there in like 40 minutes and I had a fellowship that I could have used at either UCLA or Southern Cal and I chose to use it at Southern Cal because I lived in Orange County and so that's why.

JRW: Had you gotten your undergraduate degree in Los Angeles?

UDRY: Actually, I was already finished with that before I went into the Marine Corps. I came to California to go into the Marine Corps.

JRW: You went through Camp Pendleton?

UDRY: I did boot camp at San Diego.

JRW: The MCRD [Marine Corps Recruit Depot] in San Diego?

UDRY: Right. So that was my introduction to California and having spent the first 10 or 12 weeks in boot camp was my first experience with California and I decided I wanted to live there.

JRW: Turned out that you didn't stay for long though.

UDRY: I stayed there pretty long; I stayed there about 15 years. It only seems short because it's been a long time since.

JRW: You've been in North Carolina for so long, that's right.

UDRY: Right, I've been here 40 years.

JRW: You said you'd already gotten your undergraduate degree when you went into the Marines.

UDRY: Yes.

JRW: Did you go into the Marines as an officer?

UDRY: No, I was drafted and they invited me to go to Officers Candidate School, but if I did that I would had to stay in for four years and so I elected to stay an enlisted man.

JRW: Was this the early involvement in Vietnam.

UDRY: Korea.

JRW: I was trying to remember when we had a draft before Vietnam.

UDRY: The Marine Corps never admitted that it had one. After I was drafted they sent my wife [Janice May, whom he had met in college at Northwestern and they were married in 1950] a letter saying we just want you to know that you should be proud that your husband was selected to go into the Marine Corps.

JRW: Did you see any action?

UDRY: I didn't see any, but the Marine Corps was seeing plenty because I spent the first year of my draft in the Marine Corps Air Squadron, so we had people finishing their tours of duty in Korea and coming back to our squadron, and our band was full of people coming back from 2nd and 3rd combat duty in Korea.

JRW: What instrument do you play?

UDRY: The French horn.

JRW: Do you still play the French horn?

UDRY: No.

JRW: When did you give that up?

UDRY: When I got out of the Marine Corps.

JRW: You learned the French horn as a survival strategy?

UDRY: No, I actually was a pretty proficient horn player. I could have been a symphony musician, but I decided when I went to college that I wasn't going to spend the rest of my life blowing a horn. It didn't seem like something I wanted to do for my life.

JRW: Your teaching high school--what was the subject, social studies?

UDRY: I taught everything from 7th grade to 12th grade. I taught in a new junior high school in Anaheim, which then became a high school and so I taught almost everything they had.

JRW: Well it would be appropriate for somebody coming out of the Marines to teach junior high. I have a niece who teaches junior high and I've always figured she deserved combat pay of some kind. So then you heard about the GI Bill and you just started looking around for a graduate program?

UDRY: Yes. So I really was not driven to academics and graduate school. It was just kind of a gradual thing to do and I finally used up every single instant of my GI Bill and got a Ph.D. out of going into the Marine Corps.

JRW: Not a bad deal.

UDRY: No, it seemed like a pretty good deal at the time.

JRW: Good for the rest of us.

UDRY: Yep.

JRW: Your dissertation was on marriage and the family, and adolescent health topics. You have been around this for a long time, haven't you?

UDRY: Actually, it had to do with adolescent friendship patterns.

JRW: When you finished your doctorate, you didn't immediately go to North Carolina right? Weren't you teaching in Southern California before you came to North Carolina?

UDRY: Right. I didn't have a burning in my belly to become the winner of the Nobel Prize or be a champion researcher so I taught two years in a junior college [Chaffey College] in Ontario [in Southern California] and three years at the polytechnic college at San Luis Obispo. That was my favorite place to have been in California.

JRW: Is that right? I have driven through San Luis Obispo many times, but never actually spent any time there, never even been on that campus.

UDRY: I have been back once since I left California--in 1989--and I can say that time never did anything bad for San Luis Obispo. It was a beautiful place then and they didn't completely mess it up.

JRW: Not too many places you can say that about.

UDRY: Nope. Many of us old time residents of California have lots of places that we can't tell you that about.

JRW: That's exactly right. You were buying ranch property, for example.

UDRY: Yeah, I had a place picked out to buy that was sitting on a mountain.

JRW: How did it happen that you took the job at North Carolina, how did that transpire?

UDRY: Well I loved San Luis Obispo, but it didn't love me and so...

JRW: You decided to get out.

UDRY: It was just a bad fit. I built a house right across the street from the school. I could get on my bicycle and ride to my classroom and ride to my office without pedaling.

JRW: Just like some old movie.

UDRY: Yeah. I started looking for a job and I had a friend from graduate school who was recruiting for jobs at UC Riverside, but I went to UNC before we went to Riverside. So I did and I ended up here. I never even looked at Riverside.

JRW: The job you applied for was a joint appointment in the department of public health and

sociology. Had somebody already decided this was actually with the Carolina Population Center? Was the job designed essentially by the Population Center?

UDRY: The Population Center wasn't formed as yet.

JRW: It was formed in what, '66?

UDRY: Yes '66.

JRW: Right. So how did it become, how did it transpire that you later became the director.

UDRY: Well, after Moye Freymann left, he was replaced by Tom Hall who was another top physician. And that was a time when the population center was on the brink of whether it was going to be closed down by the administration on account of the academic departments didn't like it. And the sociology department actually said "let's close it down," but among the people in the sociology department at that time was Amos Hawley and he volunteered to be in charge of the committee of what should be done to the pop center. He said it should focus on research, not technical assistance, and everybody agreed.

And then he said: I nominate Dick Udry to be the new director and then there was a big silence. Then Amos said, "any more nominations?" And there was more silence, and then he said, "I recommend we appoint Udry." And so they nominated me for it and so I said to them, I said "if you want an academic research center, I know how to build one."

JRW: Right.

UDRY: So that's how I became the center director [in 1977]. I was center director for 15 years.

JRW: During your directorship, the Carolina Population Center then became famous as it is today. As director what were the important things you saw yourself having to do?

UDRY: The most important thing was to identify faculty among those who were already working in the population field and to invite them to join the center and be under my directorship. We designed a method where the fellows of the center could nominate and approve the addition of new fellows and the reason that I succeeded then as director was that I saw to it that promising scholars were nominated as fellows and then put in a position to develop their careers using the center as a mechanism.

JRW: Right. And so you were able to offer them things like summer money and research?

UDRY: Nope, I never did that.

JRW: You never did that?

UDRY: I did that the first year I was the director, but very quickly discovered the problem with giving summer money to people is that they were going to do what they were doing anyhow, so there wasn't any point in wasting any money on them. What I learned was to encourage people who were going to become career scholars and what I was encouraging was the development of programs in research in which scholars would gather together and get money to establish a research program and that's how the center became a research center.

JRW: Okay. So how did you manage to get people to be involved in these things?

UDRY: We had a method of providing service to the faculty research program to help write NIH grant applications. And we taught them to do it and I personally nursed them through the first application and I personally patted them on the shoulder when they didn't get it and said now, we really have to get this one, so I specifically brought in people who were going to be able to develop programs of research and many of them had been working on the same research programs that they began 20 years ago.

JRW: You helped bring Ron Rindfuss along and Barbara Entwisle along? Were they here when you became director?

UDRY: They were already here [Ron Rindfuss came in 1976 and Barbara Entwisle came in 1985]. When I became the director of the center we had research grants by two people.

JRW: Is that right, two people?

UDRY: Rindfuss and me.

JRW: So, you built the center by taking in people and essentially nurturing them cradle to grave getting them involved in research.

UDRY: That's right, and we made the center so attractive as a place to do research that no departments could compete with it.

JRW: How did you do this?

UDRY: We built a staff to do it. We built—this would have been in about the late 70's--we built a computer program which would give them the resources of people and know-how and up-to-datedness that everybody wished they had, but they didn't have and that was a very big help.

JRW: You were probably the first center to get geographers involved in any meaningful way, was that your decision to do that?

UDRY: No. They were already involved and they continued to be involved. So I don't deserve any credit for that and in fact we probably had too many disciplines at that point that I started to direct the center so there were things that we had that we were putting money in at that point that I stopped putting money in, like religion. Would I do the same today, I don't know, but it wasn't contributing anything in those days.

JRW: During this period of time, you had 30 NIH grants on line and you were the director, the focus though was largely United States demography or were you looking at international things?

UDRY: There were programs in several countries around the world.

JRW: Was this the period of time when the Thailand project came on.

UDRY: Yes. This started with funding from the Rockefeller Foundation to build a pop center in Thailand. But after 5 years of fooling around with this, I said this is not the way to do it and if we're going to get another Rockefeller grant, we've got to do it a different way so we did it by holding a

national competition for Ph.D.'s in demography and selected people for those positions and then sent them back there 5 years later with Ph.D.'s and that's the way we got their population center started.

JRW: Very interesting. Very clever idea.

UDRY: Because what they were doing was retreading old faculty and if there was anything that I learned from that experience, and other experience, it is that retreading old faculty is a terrible way to build a research center. So, in case you want to build a research center, I will give a capsule of advice.

JRW: It's good to know. Readers over time will be able to look back at this and know what to do or what not to do. So now as you came up to the end of your period as director of the center, that was the time that you were elected PAA President-Elect, and then you became PAA President.

UDRY: Yes.

JRW: What were the hot button issues in the PAA during that period of time that you were coping with.

UDRY: Well, the first thing we had was that our membership had plummeted to about 2,100 -- something like that [it was actually 2,267 in 1993] from about 3,000 [the earlier peak had been 2,752 in 1990], and that looked like an organization that was about to depopulate. So the first thing we did was to send out a program for encouraging membership and we developed quite an articulated program of several different steps aimed at the same direction and so in the course of that program, which we set it out for 3 years, and in the course of that program we had increased the size of the population center by about 50% and got it back over 3,000 again [it hit 3,007 in 1997].

JRW: And most of it was kind of networking, right? If I recall, wasn't that the case, encouraging people to get others to become involved or renew their involvement?

UDRY: Yes, but any program like that is probably going to succeed because you do more than one thing and we did more than one thing and as far as identifying which ones were successful nobody ever felt the need to do that because if they were all successful, then whatever reasons there were, there the population association was, and that is what matters.

JRW: So, what were the things that you had on tap as a strategy?

UDRY: I actually don't have a very clear picture of how we did it. We talked about it and we put a person in charge of this and a person in charge of that, but exactly what was successful I can't tell you. All I can tell you is immediately the problem was solved, but I noticed in previous and in subsequent periods when I wasn't on the Board of Directors that you get into periods where it can decline and if nobody does anything, it continues down until somebody does something.

JRW: Right, any volunteer organization really needs that reinvigoration.

UDRY: Right.

JRW: As I recall correctly, too, while you were president you also brought up the issue which is, of course, related to membership, but of finances, and didn't you initiate the program to encourage additional donations on the part of people?

UDRY: Yes, but until I gave a good-sized donation to set the pace for it, it wasn't particularly successful, but since then it's continued apace. It's continued to work on a minor level and so I don't think it should be considered a howling success, but every year there are new people who do donate substantial amounts, and a substantial amount I would put in the category of \$1,000, which is what we established as--Great poobah or whatever.

JRW: Right. There's some category of poobahiness in terms of the giving.

UDRY: Right. But that's not a way to increase membership.

JRW: No, but we were just thinking back over the kinds of things that you took on during that period of time of the presidency of the population association. So now to a certain extent, it was almost a set of crisis years while you were involved, because if you are seeing this decline in membership and you don't do something about it, then essentially you had to step up to the plate once again.

UDRY: Yeah, well we never had to do that. There have been times since then that I said somebody better do something about that, but it wasn't my responsibility.

JRW: I was thinking about how, in your life, you were put in the position with the pop center of essentially having it be on your shoulders to make something of this.

UDRY: Right.

JRW: As with the PAA, if something was going to be made of it, it was going to be up to you to do something about that.

UDRY: Right, but other people will only remember the free drinks that they had at Miami. When I finally got to the open cocktail party, they told me all the free drinks were gone!

JRW: I remember that!

UDRY: And I said then, well then open it up, and we sold \$5,000 worth of mixed drinks, which wasn't budgeted and so that was the year we ran over the budget, in fact! Also, I worked to establish the Association of Population Centers, the purpose of which was to make a membership group out of the population centers themselves and that gave that organization a purpose for holding meetings to talk about how to organize population centers and I believe it has been a very successful organization, they have told me.

JRW: Yes, that's always been an interesting thing to me because what you created was a group of cooperating institutions who were really all in competition with one another because every five years you come up for refunding and you kind of compete against one another or any potential newcomer for those monies.

UDRY: Anyway, I started out with 12 organizations for the first meeting and I told them that my purpose, my function was to get the group organized and not to direct it and, therefore, I was going to conduct the first election which would not include me and so the concept is right and the people are working at it. It should be very successful, but that's been a long time ago and I have not kept up with how well it's doing.

JRW: Your presidency of the PAA was also a tough transition time--moving out of the administrative association with the American Sociological Association and becoming administratively independent. You were right in the middle of that transition.

UDRY: Right.

JRW: Also, wasn't that the period of time when the Adolescent Health project [Add Health--the National Longitudinal Study of Adolescent Health] was getting going? So, all this time were you involved in that.

UDRY: Well, Add Health depends on where you want to start it. We started working on a grant in the mid 80's. We got a planning grant from NIH about 1989 and then in 1991 a grant from NIH to do that study and then it hit the fan and if you haven't heard that story, a good place to get a view of that is this book [he hands Weeks a book].

JRW: I haven't read that.

UDRY: You haven't read it, take that with you. You can see I have two of them.

JRW: Very good, I appreciate that. For the record, this is: Morton Hunt, *The New Know-Nothings: The Political Foes of the Scientific Study of Human Nature*; New Brunswick: Transaction Publishers, 1999.

UDRY: Yes. There is a chapter in there about what eventually became Add Health. My own congressman and my own senator were happy when trying to see that we didn't get what we had applied for.

JRW: Yes, I remember Jesse Helms. Wasn't he the one who thought that there just ought to be a barbed wire fence built around Chapel Hill.

UDRY: Yes, he thought that was a very good idea but he was also a budget cutter and he never voted for the barbed wire to go around. That's the history of Add Health. It basically amounts to the fact that the congressmen and senator--both a Democratic Congressman and a Democratic Senator and a Democratic President--voted for killing the adolescent sex project that NIH wanted to do.

JRW: Just a reminder that you never know who your friends are going to be, or aren't going to be.

UDRY: Right, and so there were interesting little stories earlier in the book on how this came about and the legislation that killed it--that study and the Chicago study at the same time, which was an adult study. The policy manager for a congresswoman from Colorado added a paragraph to the legislation that killed these two projects, but which added to this legislation this new paragraph saying that NIH should direct NICHD to make sure that project should be a study of adolescence, which had to do with their general health in which developed a program to determine what was beneficial and what was detrimental to the health of the adolescents and what they should particularly focus on. But the point was that it was supposed to focus specifically on the social determinants of behavior, not on biological issues, and so in essence it killed the main study. It mandated Congress to do this other study and since they mandated this other study done, I called NIH in late June of 1993 and said, would you like to receive a proposal to do the study that you have been mandated in the new legislation, which I just

happened to know about because I had lunch with the lady who wrote the paragraph the week before and she said yes, we'd like to receive an application, and I said you will have one there in a month.

JRW: So it was basically like a Congressional earmark, something on that order?

UDRY: Yeah, well, except they didn't get any money.

JRW: Unfunded mandate.

UDRY: That's right and that's exactly what it was, but we played that for what it was worth, and we got a Congressional mandate to do this study and so that was a successful strategy.

JRW: Of course, I read your article in *Demography* that came out of that [Udry, J. R., 1994, The Nature of Gender, *Demography* 31 (4):561-74] and your ASR article about bringing the role of biology back in to the study of sociology [Udry, J. R., 2000, Biological Limits of Gender Construction, *American Sociological Review* 65:443-457]. It seemed to me that if there's any one person who has helped do that, it would be you. Is that the way you see your own role?

UDRY: I had been influential in that movement to the extent that it's successful, but this is a very difficult change to bring to sociology. They spent their graduate school learning that there wasn't anything to be learned from biology and that everything is social and so whatever success I have had in that field, I consider to be minimal.

JRW: Still you put the agenda out there on the table.

UDRY: I was acting on that agenda. That agenda didn't necessarily come from Add Health. In fact, that article that you saw in the *American Sociological Review* came from a grant that I got in about 1989 and I was never able to get through another study set. It seems that I was working on people at their developmental ages and some of the beginnings of reproduction and that in addition to working on the beginnings of reproduction, I started by focusing on both biology and sociology in the beginning.

JRW: What struck me is that people who teach junior high and high school are always talking about the effect of hormones. You see hormones popping out here and there. People dealing with the kids understand that many of the issues they deal with are hormonal issues. You can't attribute them to anything else and yet it seems as though when we get into the university, we tend to forget that.

UDRY: We forget it because we're sociologists. Because the 7th and 8th grade teachers know that there is something hormonal going on in their classrooms and sociologists don't know that.

JRW: Sometimes unwilling to accept it, I guess. That's what I saw you trying to do--get people to understand that you can't understand how the world works unless you understand that piece.

UDRY: That's my thesis. Clearly, it isn't that biology determines everything. It's that you can't understand what's going on in sociology and the sociology of development and adolescence and reproduction if you don't know the biology.

JRW: Has that perspective been important among other researchers working on the Add Health project or has that been kind of peripheral to the overall research program?

UDRY: No, it hasn't been. I have continued to promote it, but I don't promote that as anything special as compared to the rest of them. We have so many, I mean, our publication list from Add Health is over 1,000 publications and most of them are not biological in nature and most of the writers are sociologists. The project has been successful in highlighting the key elements of adolescence and reproductive development and so now the grant has many other parts, and many different parts of NIH are continuing to fund it, but a key contributor this time is the National Institutes of Aging. If you have a grant that's called adolescent health and you've got over \$1 million in funding for your study from the Aging Institute, you know your study's got a future!

[Note: for more information about the Add Health project, go to: <https://addhealth.cpc.unc.edu>]

JRW: Very good. Well, is there anything else that we should cover, anything else that you had thought about after I mentioned that I was coming to interview you.

UDRY: Actually, I wish I could claim to have thought deeply about this, but when I start thinking deeply about my own contributions, I guess I can easily find lots of people who have contributed more.

JRW: That makes you a nicely modest person, but no less influential.

UDRY: Okay, I'm reading about Bruce Merrifield who received a Nobel Prize in 1984 and he just died at age 99 or something like that. So, I think I will know when I'm honored for my contributions when somebody offers me the Nobel Prize in Sociology.

JRW: Okay! [keeping in mind that there is no such prize!]

UDRY: And nobody has yet.

JRW: You may have to live long as your mother-in-law in order to do that.

UDRY: I don't need to worry about it. My parents both died before they were my age. So that's how I calculate if I have achieved my natural age span, and I've been able to live as long as my parents.

JRW: So now you're living on borrowed time. Interesting perspective. All right, well I appreciate very much the time that you were willing to take for this interview!

UDRY: Well, since we are gathered here for these purposes, before we get our prayer to end the meeting, we should ask: what did I contribute to the Population Association that makes it different now from what it would have been without me? And I have to say it beats the hell out of me!

JRW: That's a very modest thing to say, but, in fact, I guess one of the reasons why in these interviews we don't talk just about your role as PAA president is because all of the things that you have done--in particular the development of the Carolina Population Center and all the scholars that you have influenced--are, in fact, importantly related to the business of the Population Association.

UDRY: That's true, and so I take full credit for everything they do...

JRW: That's what you should do, in fact. Thank you!

J. Richard Udry, esteemed population studies scholar and Carolina Population Center's Director from 1977 – 1992, died at his home on Sunday, July 29, 2012. He was 83.

Originally posted August 2012



Dick Udry joined the UNC faculty as Associate Professor of Maternal and Child Health and Sociology in 1965. He became CPC Director in 1977 when the center was focused on providing family planning technical assistance to developing countries rather than conducting research. Early in his directorship, Udry changed the structure and purpose of the Carolina Population Center and, today, it still closely resembles that structure and purpose. He created the Faculty Fellows program, an elected group of UNC faculty members who choose to conduct population research at CPC.



Udry created CPC's Advisory Council, a group of five CPC Fellows who represent different academic disciplines. In 1987, the group was (left to right): Boone Turchi, Barbara Entwisle, Dick Udry, Barry Popkin, Melinda Meade, and Amy Tsui.

Under his leadership, CPC applied for and received hundreds of grants from NIH. At the time, NIH rarely funded social science research. The CPC History website profiles his first decade as director, titled *Rebirth with Research Focus*. He is also responsible for expanding CPC's research portfolio and impact. Read about that in CPC's History section *Growth and Expansion*.

Udry is perhaps best known for creating the National Longitudinal Study of Adolescent Health, a longitudinal study of U.S. adolescents that started in 1994. It was built on the American Teenage Study which started in the 1980s and ended in 1991. Add Health completed the 4th wave of data collection in 2008. The project is now led by Kathleen Mullan Harris and is still a Carolina Population Center project. The data is used by thousands of researchers worldwide in a wide range of disciplines including sociology, psychology, criminology, education, economics, biostatistics, epidemiology, medicine, genetics, maternal and child health, nutrition, aging, environmental science and geography, among others.

Carolyn Tucker Halpern is Professor of Maternal and Child Health at UNC and a CPC Faculty Fellow. She started working with Udry in 1986 and worked closely with him on the Add Health project. She remembered: "Dick's genius at delegation always reminded me of Captain Jean-Luc Picard on *Star Trek Next Generation*." Like Captain Picard, when Dick (who sometimes referred to himself as the "Grand Pubah") made a decision to do something, he would simply say "make it so," and having assembled such an incredible staff at CPC, it would get done and done well."

CPC's current Director, S. Philip Morgan, was a CPC postdoctoral scholar when Udry was Director. Morgan said: "When I was a CPC postdoc (1980-83), Dick was Director. He took my parking pass away... (Yes in 1980, post docs had parking passes!). When I complained, he said 'you'll get over it... go back to work.'" Both statements were accompanied by a very broad smile; conversation over. Dick was a scientist first. He thought description (especially of understudied phenomena) was very important, but he also collected data to test potential causal mechanisms. His work mattered! There are at least a half dozen of his many articles (over 400) that are fundamental to how I think about social science. His work has been cited over 17,000 times. But most importantly, he took lots of us to lunch early in our careers and expressed genuine interest in our work and



lives. He was generous with his advice, ideas and data. Dick was always up for a good debate, but he insisted that folks take evidence seriously. We miss him and remember him fondly."

In 1994, Udry served as Population Association of America President. Colleagues supported Udry's selection as a PAA Honored Member and a splendid academic biography highlights Udry's contributions to the field. [Read it here.](#)

Ronald R. Rindfuss became CPC Director in 1992, taking the reins from Udry. Rindfuss, Research Professor of Sociology at UNC and a CPC Fellow, offered this tribute:

"Dick will be sorely missed at UNC. His academic achievements are well-documented, but there are so many other aspects of Dick that made CPC and UNC the institutions they are today. He creatively and ambiguously defined 'population' for CPC: 'Population research is like pornography, I know it when I see it' – a broad, welcoming but not engulfing definition that the Population Association of America embraced as well. Meetings were always better if Dick was there. Not only could he cut to the important issue that needed resolution, but his wit – spot on and a tad bit acerbic – made even a meeting on the most tedious topic bearable. He saved his creativity for things that really mattered, and deciding where to eat lunch was not one of them. Dick, and those who joined him, ate lunch at the same place every day – at least while the restaurant was in business. And, of course, Dick was a brilliant research center administrator. He hired talented staff, gave them the latitude to excel, and when they did he promoted them. I still remember how he would describe his responsibility as CPC director: figure out what resources and facilities population researchers need and put them in place before the faculty even know they need them. That's a philosophy tailor made to move population science forward, and that is certainly what has happened at CPC. Thanks Dick."

Udry co-authored more than 400 articles which have been cited by other scientists more than 17,000 times. He was dissertation advisor to many students who have gone on to significant roles in the population field. He was also mentor to many faculty, students, and staff whose careers were influenced by his work.

CPC named its distinguished lecture series to honor Udry's legacy. The 2013 J. Richard Udry Distinguished Lecture will be presented by Amy Tsui on February 28, 2013. Tsui was CPC Director from 1997 to 2001. The lecture will be held in Chapel Hill.

A gathering in remembrance of Udry will be held November 2, 2012 in room 405 of CPC East

(University Square East, 123 West Franklin Street) from 3:30-5:00pm, with a reception to follow.

The Chapel Hill News published Udry's obituary on August 19, 2012.

The Nature of Gender*

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I explain a biosocial model of women's gendered behavior (behavior on which the sexes differ). This model integrates a macro sociological theory with a biological theory derived from primate behavior. The sociological model is designed to explain changes in the relationship between sex and behavior over time or between groups. The biological model is designed to explain individual within-sex variance and between-sex variance in gendered behavior in a cohort. Results from an original study are presented to demonstrate that within-sex variance in women's gendered behavior is explained well by the primate model. I conclude that human nature is gendered. The implications of this conclusion are explored for demographic and other social science research.

I welcome my colleagues of the feminine gender, the masculine gender, and other genders not yet constructed. When I was young, the term *gender* referred to the grammatical inflection of nouns. In those days we had three genders; using *gender* to refer to the male/female classification was a joke. In a comprehensive bibliography of 12,000 titles for marriage and family literature from 1900 to 1964, *gender* does not appear once (Aldous and Hill 1967). Along came John Money; he proposed using the term *sex* to refer to the biological classification of male/female and *gender* to refer to differences in behavior by sex (Money and Ehrhardt 1972). Now everyone uses *gender*, but hardly anyone uses it in the way John Money proposed.

Today, journal editors strike out the word *sex* in my manuscripts and substitute *gender* whenever I mean male and female. *Opposite sex* becomes *the other gender*. I fill out questionnaires, even forms from the PAA, which ask for my *gender*, not my *sex*. I even find the word on my own questionnaires. I now realize that the joke about "being of the feminine persuasion" is a parody on the social science theory of *gender*: the most common use of *gender* in social science today is as a synonym for biological sex. In our urge to be politically correct, we now appear squeamish about using the word *sex*, today's equivalent of the Victorians' substituting *limb* for the more blatant *leg*. But this is only an appearance; today we use *gender* to indicate endorsement of a theory of *gender* as a human social invention.

Before I finish, I want to give you an integrated theory of *gender*. Since current usage is so inconsistent, we need a good definition. *Gender* is the relationship between biological sex and behavior; a *theory of gender* explains that relationship. A *gendered behavior* is one that differs by sex. My theory integrates a macrodimension with a microdimension, a

* Presidential address, presented at the annual meetings of the Population Association of America, Miami, May 6, 1994.

between-sex dimension with a within-sex dimension, and a social dimension with a biological dimension.

When I was in graduate school, I was scarred for life by the slash of Occam's razor. I adopted parsimony as my mandate; I wrote the shortest dissertation in the history of my department; I am one of the shortest persons to hold this high office; someone in the audience is now hoping I will make the shortest presidential address.

I try never to invent a theory when a good one exists. I try never to have a different theory for each group, country, or species. I have been deeply impressed by what animal models in biological research have taught us about human biological functioning. I like the theory of biological evolution because it is panspecies and includes human beings. If there is an established theory about gender in other species, we ought to see how it works on humans.

A BIOLOGICAL THEORY

One well-established gender theory does apply to all vertebrates, from the garter snake to the lower primates. Since most biologists do not use the word *gender*, we will use their term: *sex dimorphism*. This theory of sex dimorphism varies in its details by species, but its overall outline is this: sex dimorphism in behavior is controlled by hormones. The hormones that control sex dimorphism are sex hormones. All mammals have basically similar primary sex hormones. We know how they should work in human biology by studying other mammals, from rats to monkeys. These sex hormones guide the development of sex-dimorphic body structures and control sex-dimorphic reproductive behavior, which lies at the heart of gender.

An important principle from animal research is that the same hormones which cause between-sex differences in a behavior cause within-sex variance in the same behavior. The hormone theory therefore is used to explain both sex dimorphism and within-sex variance on the same behaviors that are sex-dimorphic. Forty years of animal experiments have firmly established this model (Ehrhardt and Meyer-Bahlburg 1981; Goy 1970; Hines 1982; Money and Ehrhardt 1972; Reinisch, Ziemba-Davis, and Sanders 1991).

Social scientists have shown little interest in applying the mammalian model of sex dimorphism to humans; we have our own theories for explaining gender. The first principle of all social science theories of gender is that gender is explained by differences in social experience. Between-sex differences in behavior are explained by differences in the social experience of males and of females, and in the social structures that constrain that experience. Within-sex differences in gendered behavior are explained by the same differences in social experience as explain between-sex differences in that behavior. For example, if males and females differ in "aggressiveness," social science theories explain the between-sex and the within-sex differences in aggressiveness by the same variables, such as differential parental socialization and exposure to different normative structures and opportunities.

Let me clarify the theoretical predicament. A universal theory of sex dimorphism in behavior applies to all vertebrates. Because of the structure of the evolutionary process, all vertebrates share a basic reproductive system controlled by the same hormones. Most of the early progress in understanding human reproduction came from studying animal models. The same hormones control sex-dimorphic behavior as control the development of the reproductive system. But humans are the exception, we social scientists say. Even though they share their evolutionary origins with other vertebrates, humans are exempt from the sex-dimorphic behavior effects of hormones observed in all other species. Humans require a separate theory, we say.

There are important reasons why social scientists do not consider the biological gender theory. First, most do not know about it. Second, it has no place in our disciplinary paradigm. Third, it is politically incorrect; some call it “sexist” and “ideological” (Longino 1990, ch. 6). We believe that accepting a biological foundation for gender logically implies the support of current gender arrangements in society and undercuts motivation for change in gender structure. The idea that *any* behavior has a biological foundation is considered politically conservative. Social scientists imagine that if a behavior is under biological influence, there is nothing we can do about it; this naive notion is held only by social scientists.

A final reason why social scientists do not consider the biological theory is that we already have a theory we think is good, and we too follow the rule of parsimony that says “Don’t have two theories when one will do.”

The social sciences have one explanatory problem that they imagine is not shared with the mammalian theories: how to explain secular change. In humans, gender changes over time in response to environmental change. So does sex-dimorphic behavior in other species (Wilson 1975:19-21). For simplicity, in the remainder of this discourse, I will assume that only social factors affect secular change in gender.

A SOCIOLOGICAL THEORY

Social science explanations of gender depend on three concepts: gender role, socialization, and opportunity structures. I won’t burden you with a detailed development of these ideas, since they are quite familiar to all social scientists, but I’ll give you a once-over-lightly. A gender role is a range of acceptable behavior that differs by sex in a particular behavioral domain (say, parenting) and is supported by gendered norms. The boundaries of acceptable behavior differ by sex; violating these boundaries is accompanied by punishment and is made difficult by structured opportunities. We usually *infer* the existence of sex-differentiated norms from the sex differences in the behavior we are trying to explain by those norms. The reason for this tautology is that we, as social scientists, can’t think of any other way to explain sex differences. So if males and females differ in their interest in and attention to small children, we infer sex-differentiated norms from observations of sex-differentiated behavior, and then explain the sex-differentiated behavior by the norms we have inferred.

And where do the sex-differentiated norms supposedly come from? Here we have a range of theories: technology-driven theories, differences based on differential reproductive roles derived from the irreducible biological basics of reproduction, ideology-driven theories, even pure historical accident.

What determines which behaviors are gendered in a society? Psychologist Sandra Bem (1987) says it is historical accident and entirely arbitrary, but this is a silly idea that no demographer could love. A better starting place is that in mammals, those behaviors related most closely to reproduction and infant survival are the most sex-dimorphic. In humans these behaviors form the foundation for the division of labor by sex.

HYPOTHETICAL GENDER STRUCTURES

Now consider with me some hypothetical societies with different gender structures. Assume with me that we have constructed some kind of composite gender factor whose components are all gendered behaviors. On this composite factor we have a measure of the behavior of each individual in the society. Since we have constructed the measure from

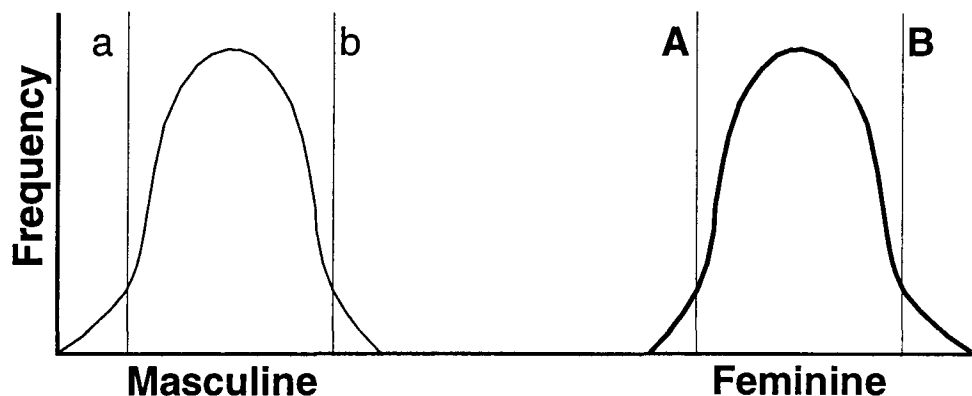
gendered behaviors (those on which males and females differ), we will call the direction in which males exceed females *masculine*, and the direction in which females exceed males *feminine*. In a traditional society, the distributions of males and females might look something like Figure 1. The degree to which the sex means are different is a measure of the importance of gender in the society. Within sex, some males are more masculine than others, and some females are more feminine than others, perhaps because of different socialization experiences or different placement with respect to opportunity structures. Mary is more feminine than Beth, and John is more masculine than Bill, but Bill is still a lot more masculine than Beth. There is little or no sex overlap in this traditional society.

From a sociological perspective, these distributions have the structure they have because of social norms that support different behaviors for each sex, and indicate the tolerance limits for diversity on gendered behaviors within sex. The vertical lines indicate those normative limits. Note that some members of each sex exceed this limit in each direction for their sex: the norms still have some more work to do.

Now imagine that this society undergoes a gender revolution. Transformations occur in childbearing, family structure, and the economy, accompanied by revolutionary changes in norms emerging from a new gender philosophy in the society—an antigender or unisex philosophy. If these new values are strong enough, we can imagine that a society might emerge in which, although there are still males and females, there is no gender—no relationship between biological sex and social behavior (see Figure 2). This society has a no-gender structure. This is a macromodel of social change; we explain such a change by social forces.

COMPARING BIOLOGICAL WITH SOCIAL STRUCTURES

Now let us suspend disbelief and assume that human gender, as in the case of other mammals, has a biological basis. This would mean that because of biological, *not* social, forces, males were predisposed to be more masculine on our composite measure of gender,



Behavior: — Male — Female

Normative limits: a,b - Male A,B - Female

Figure 1. Gendered Behavior in a Traditional Sex-typed Social System

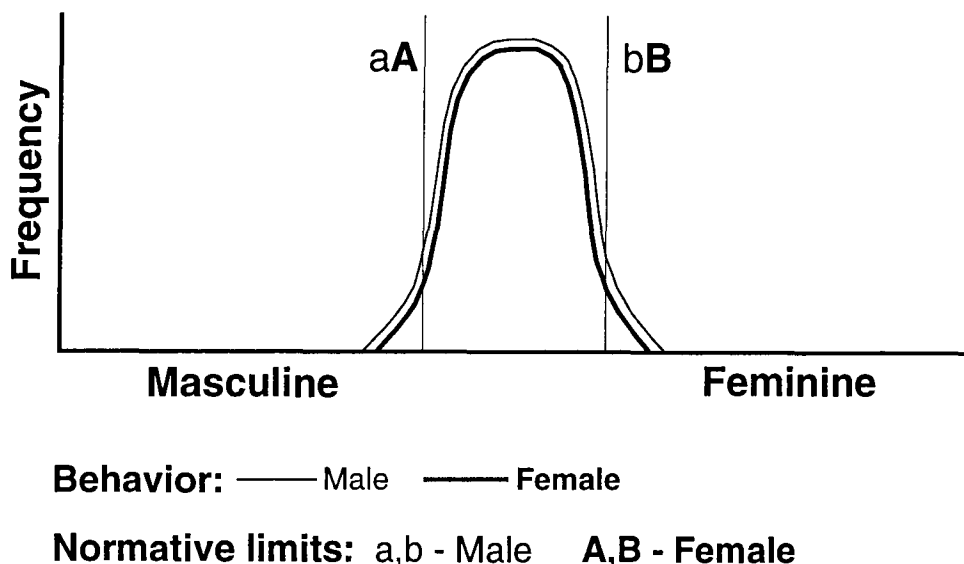


Figure 2. Gendered Behavior in a Unisex Social System

and females were predisposed to be more feminine. Furthermore, suppose that within-sex biological differences predispose some males to be more masculine than others, and some females to be more feminine than others. Assuming that we had the right biological measures, we could measure each person in the population and array their natural gender predispositions on our graphs.

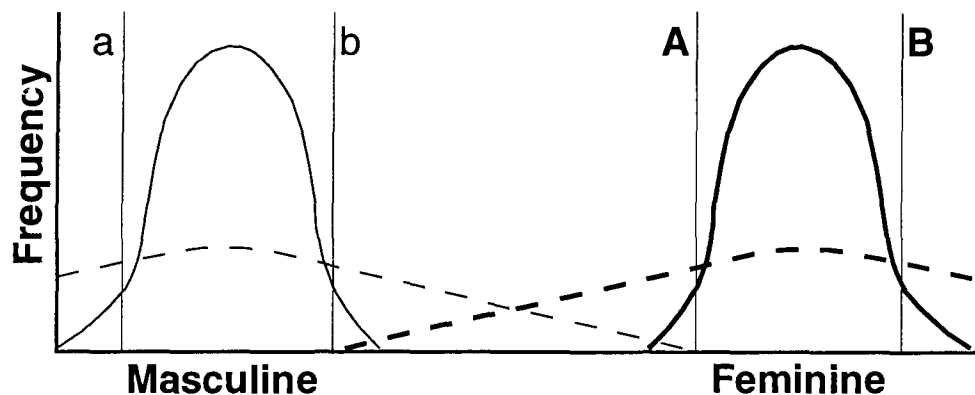
So we return to our diagram of a traditional gender system, but we add broken lines representing hypothetical distributions of gender predispositions for each sex (see Figure 3). We can now compare the behavior distribution with the predisposition distribution and get an idea of the fit between the two. To achieve the behavior distribution of a traditional gender regime, a lot of people have to be pushed far from their natural bent, but even more departure from natural bent is required to fit the unisex distribution (see Figure 4).

Now each of these societies has the same underlying structure of biological predispositions. The difference in the gender structure between societies is explained by differences in norms and in features of social structure. Different gender ideologies undergird these different gender structures.

Suppose that a society developed an underlying ideology which was gender-permissive instead of being antigender or strongly gendered. Norms and social structure were gender-permissive: everyone was encouraged to follow natural predispositions with respect to gendered behaviors. Under these circumstances, we would predict the actual gender structure to follow the distribution of natural predispositions by sex (see Figure 5).

This would be a gendered society, but without gender norms supporting the gender structure. Lacking a biological theory of gender, social scientists would be confident that this society had gender norms and gendered opportunity structures, whether or not they could observe them. This is because they imagine that without gendered norms and opportunity structures, the terminal state would be unisex, or the absence of gender.

Why consider Figure 6? In Figure 6, males have what is now feminine behavior and females have masculine behavior. Now hardly anyone's behavior is aligned with natural predispositions; I show it here because if gender is arbitrary and socially constructed from



Predisposition:

- - Male - - Female

Behavior:

— Male — Female

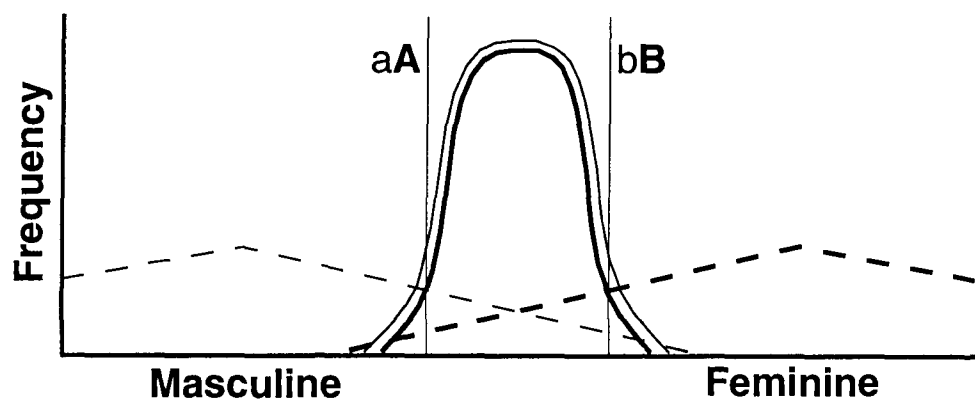
Normative limits: a,b - Male A,B - Female

Figure 3. Gendered Behavior in a Traditional Sex-Typed Social System, with Predispositions

unbiased material, then the social structure represented by Figure 6 ought to be as easy to construct as its opposite.

THE PRIMATE GENDER THEORY

Let us take a closer look at a primate model of sex-dimorphic behavior and its possible application to humans. With this theory I can explain why males and females behave



Predisposition:

- - Male - - Female

Behavior:

— Male — Female

Normative limits: a,b - Male A,B - Female

Figure 4. Gendered Behavior in a Permissive Social System, with Predispositions

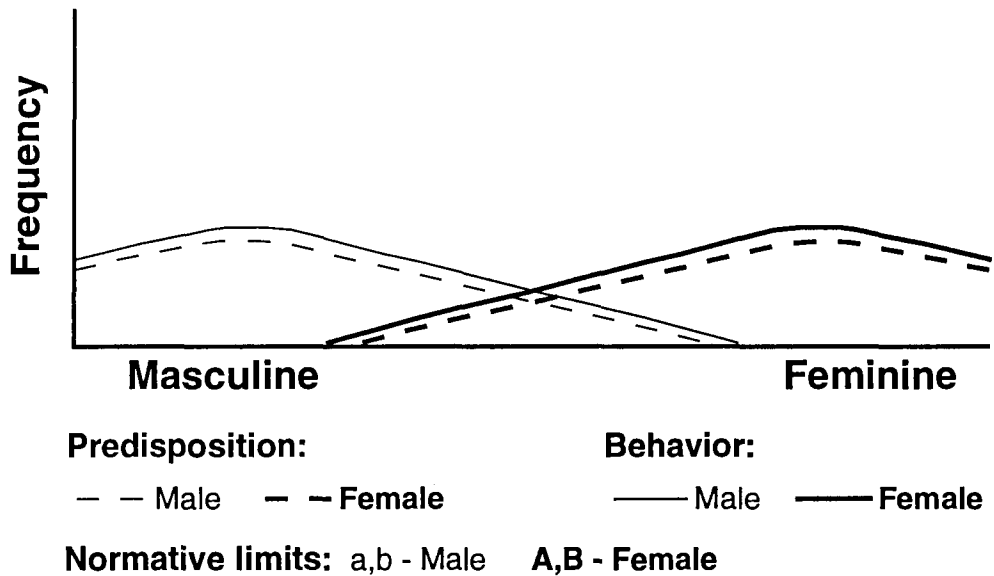


Figure 5. Gendered Behavior in a Permissive Social System, with Predispositions

differently, why these differences have cross-cultural generality, why some males are more masculine than others, and why some females are more feminine than others. I cannot explain secular change; by agreement we stipulate that secular change is to be explained by social science theories alone. The primate model can explain only variance in a cohort. Our theory says that some particular behaviors are sensitive to hormonal influence, and others are not. It is not a historical accident or a random outcome that some behaviors are

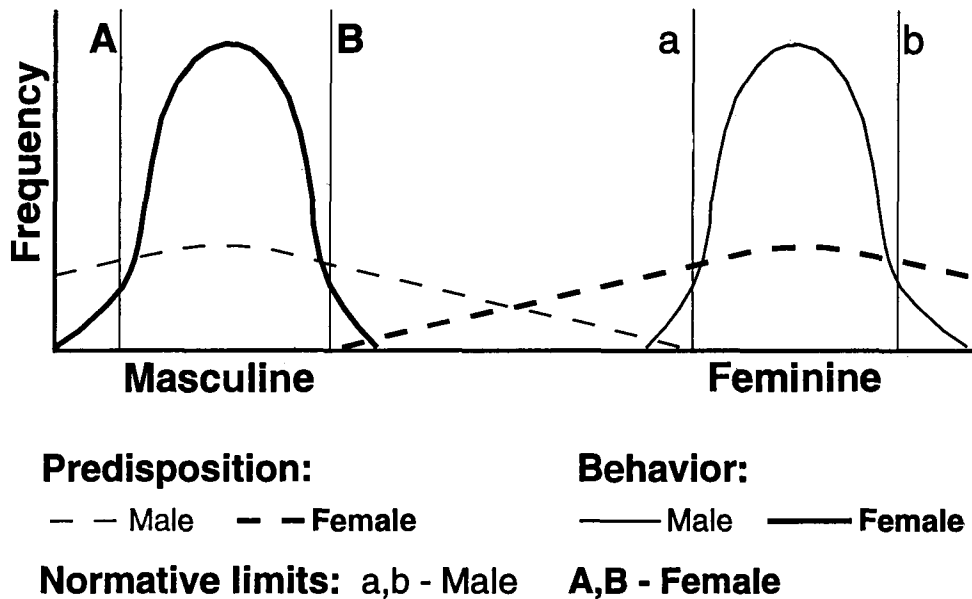


Figure 6. Gendered Behavior in a Reverse-Gendered Social System, with Predispositions

gendered, and others are not. We know which behaviors to examine for the hormone effects: gendered behaviors.

By 1970 a rather clear picture of the hormonal foundation for sex-dimorphic behavior had already been worked out for primates and other mammals (Goy 1970). For primates, the process operates in two stages. The first stage takes place in mid-pregnancy: male fetuses' testicles begin producing large amounts of testosterone early in the second trimester. This not only masculinizes their genitalia, but also masculinizes their brains by affecting the neural structure. By the third trimester, males have a different brain structure from females. This difference in brain structures predisposes males and females to different behavior, given the same environmental stimulus. Females receive very little testosterone fetally; what they receive comes from their mothers' blood, passing through the placenta. In the absence of testosterone, nature makes female genitalia and a female brain. Because the effects of the fetal testosterone reorganize the brain permanently, these are called organizational effects.

The second phase is the development at puberty of further anatomical sex dimorphism, caused by the sex hormones of adulthood. These adult hormones not only cause anatomical changes but also act on the neural structures laid down during the prenatal period to produce adult sex-dimorphic behavior. The degree to which the adult hormones affect sex-dimorphic behavior is contingent on the degree of prenatal exposure to androgens (or male hormones), specifically to testosterone. The primary adult sex hormones of interest are also androgens—testosterone and androstenedione.

APPLYING PRIMATE MODELS TO HUMANS

A body of research on humans tests separate pieces of this theory; most of this work concerns clinical syndromes of hormonal anomaly. To greatly simplify this literature, it shows the following:

1. Human females exposed fetally to abnormally high levels of androgens show distinctly masculinized behavior beginning in childhood and extending through adolescence into adulthood (Ehrhardt and Meyer-Bahlburg 1981; Reinisch et al. 1991).
2. Girls who, because of a genetic anomaly, lack all sex hormones (even female sex hormones) grow up to be unusually feminine (Money and Ehrhardt 1972).
3. Females exposed as fetuses to physician-administered androgenic hormones for the mother's therapy show masculinized behavior in childhood, even though they show no masculinization of anatomy (Reinisch 1977).
4. Women with high adult androgen levels show masculine-skewed behavior, as compared to women with low androgen levels (Purifoy and Koopmans 1979).

MY STUDY

I want to describe a project I conducted (with NICHD support), applying the primate hormonal model to predict within-sex patterns of gendered behavior among women.¹

To test this theory, I needed a sample of adult women for whom I had multiple measures of prenatal hormone exposure, a socialization history in childhood and adolescence, a measure of adult hormone levels, and measures of adult gendered behavior—a set of data seemingly impossible to assemble. Actually, I have found more than one possible data set that meets these conditions rather well. The one I used was the Child Health and Development Study, originally conducted jointly by the Kaiser Research Foundation and the University of California-Berkeley School of Public Health (van den

Berg, Christianson, and Oechsli 1988). In the early 1960s, prenatal patients at Kaiser Plan facilities were recruited into the study. Each woman provided blood samples during each trimester of pregnancy. These samples were stored, and later were made available to researchers. For those giving birth from 1960 through 1963, the children and mothers were followed up with measurement and interviews at children's ages 5, 9-11, and 15-17. These data are now also available for public use (van den Berg 1987).

We reinterviewed about 350 female offspring when they were 27 to 30 years old. From 250 we took blood samples during a controlled period of their menstrual cycles and at a controlled time of day. During this interview, the women completed a self-administered questionnaire in which we obtained measures of their adult gendered behavior. This procedure gave us all the required elements of the needed research design.

Measurement of Gendered Behavior

We measured many different gendered behaviors on our respondents; we tried to tap various domains of life and behavior manifestations. Our measurement technique was to identify a gendered behavior, identify the direction of difference between males and females, and call feminine high. Table 1 lists the measures of gendered behavior we obtained. On a personality test, for example, we scored women low insofar as they answered the personality items more as men answered them than as other women did.

We have 19 measures of gendered behavior, each containing its own unique variance in addition to the measure of gender that it may share with the other measures. Our theory says that the variance these measures share with other measures of gendered behavior is what is relevant to our theory. To identify the vector of shared variance among the gendered behavior components, we used factor analysis. All gender components load on a common superfactor, which we named GENDERED. This finding is important, because it says that there is some overall consistency in the way individual behavior is gendered.

Table 1. Gendered Behavior Components

Ever married to a man
Number of live births
Index of Sex Role Orientation
Importance of career
Importance of children
Domestic division of labor
Sex-typed activities scale
Importance of marriage
Feminine appearance factor
Strong Vocational Interest Inventory
Likes baby care
Proportion female in current occupation
Featherman socioeconomic index
Proportion female in work unit
Bem Sex Role Inventory, feminine score
Bem Sex Role Inventory, masculine score
Adjective Check List
Personality Research Form, masculine score
Personality Research Form, feminine score

Results of My Study

We then constructed multivariate models, including prenatal and adult androgen measures and their interaction in the same equation, to predict GENDERED. To avoid hormone overload I won't burden you with too many tables, but we can look at one example: Table 2 shows a multivariate model predicting GENDERED. This model shows the significant predicted hormone effects for prenatal exposure, adult androgen levels, and their interaction. Androstenedione is an adrenal androgen from which the body makes testosterone. Sex hormone binding globulin is a large protein molecule that binds maternal testosterone and prevents it from passing through membranes and reaching the fetus. In adults it prevents testosterone from reaching the central nervous system. Therefore sex hormone-binding globulin has an expected sign opposite to those for testosterone and androstenedione. The models work only with second-trimester hormones, as predicted. This is a good model; it predicts one-fourth of the variance in Women's GENDERED behavior.

So that you can see how the hormones work in predicting less synthetic gender measures, I offer one model predicting a simple measure. Our interviewers were asked to rate each respondent's appearance and personal demeanor on masculinity-femininity on a seven-point scale, with feminine as high. Interviewers received no further instructions on what to observe for this rating. The same hormone model as in Table 2 predicts 17 percent of the variance in interviewers' ratings of femininity.

These findings are highly consistent with what we would expect from the theoretical foundations we started with. Consider the constraints with which we worked. We had only one maternal blood sample and no fetal blood from which to measure prenatal androgen exposure in each trimester of pregnancy. We found gendered behavior correlations only to second-trimester androgens, not to first- and third-trimester androgens, just as predicted by the theory. We had only one blood sample for measuring adult androgens, from one phase of the menstrual cycle, whereas adult androgens vary from day to day and according to the phase of the menstrual cycle. Behaviors were measured about 30 years after the prenatal blood samples were drawn. Yet we were able to confirm several very specific hypotheses concerning the specific hormones involved prenatally, the trimester of effects of prenatal hormones, the specific hormones involved in adulthood, and the interaction of adult with prenatal hormones. No data dredging was involved. We measured some other, hypothetically irrelevant hormones (such as estrogen) and found them to be irrelevant.

Although this is only one of many studies that successfully applies the primate theory to humans, it is the first to test the organizational hypothesis by the use of prenatal blood samples. It is the first to be able to test the joint effects of prenatal and adult hormones. It is the first to use a broad spectrum of gendered behaviors. It is the first to use a sample from the general population. Obviously we want to replicate these results on other samples of women.

Table 2. Regression of GENDERED on hormonal predictors

Variable	Significance	Sign
Adult androstenedione	***	—
Adult sex hormone-binding globulin (SHBG)	*	+
Prenatal testosterone (trimester 2)	**	—
Prenatal SHBG (trimester 2)	***	+
Prenatal testosterone x adult androstenedione	***	—

R-square = .26

* < .05; ** < .01; *** < .001; one-tailed test.

IMPLICATIONS OF BIOLOGICAL GENDER THEORY

To see what the theory means for social demographers and other social scientists, we invoke the corollary proposition: Those processes which affect within-sex variance in gendered behaviors are the same processes as cause between-sex differences. With increasing confidence we can now say that individual women differ in their biological propensity to sex-typed behavior. We can also infer that males and females differ from one another in their average biological propensity to the same behaviors. In short, we have empirical justification for the dotted lines on my gender structure diagrams.

Once these propositions are admitted, social science gender theories are in big trouble. Gender has biological foundations. We have become so immersed in our own social science theories of gender that we haven't thought seriously about confronting alternative theories. The closest we come to confrontation is to say that it is impossible for a behavior to have biological foundations while experiencing secular change at the same time. Most demographers are accustomed to thinking that the variables which predict individual variance also predict secular change. No such logical deduction can be made, however. A typical example of this reasoning is a recent quote from Troy Duster, who was refuting arguments about the biological foundations of violent behavior:

Violence in our society and its present saturation among young black males is a recent phenomenon that has escalated only in the past 25 years. . . . But do we have reason to believe that things have changed all that much at the biochemical level, or is something else happening? (Touchette 1993:30).

I do not select this quote because it is a particularly egregious example, but because it is typical of my colleagues' reasoning. As far as I know, no one has ever argued that secular change in violence has any biological foundation. Any biological theory of violence is proposed only to explain individual variance in propensity to violence. The secular change must be explained by sociological and other environmental change. Duster and other social scientists are simply confusing the causes of individual variance with the causes of secular change.

What does an admission of a biological basis for individual variance in gendered behavior *not* mean? It does not mean that social forces do not also contribute to individual variance. Social scientists, of all people, often think that if certain behaviors have biological foundations, then those behaviors are foreordained, and there is nothing that society can do about influencing them. I quote sociologist Alan Wolfe on genes and criminal behavior:

If some children are biologically disposed to be criminals, then surely no amount of discipline can help them, unless we believe that social institutions can trump biological instincts (Wolfe 1993:36).

Lay society has always taken it for granted that much undesirable behavior has biological foundations, but society has never believed that there was nothing to be done about it. The whole force of social institutions is designed to "trump" these "biological instincts." Parents have always believed that "natural instincts" produced adolescent sexual behavior, but they never accepted its inevitability. Likewise, laymen have always believed that behavior differences in the sexes were part of the natural order of things.

So now, given a sound understanding of the way in which both biological and social forces affect variance in gender, and given that only social forces may affect secular change in gender, we can ask about the fit between social forces and biological propensities.

When social scientists still believed in human nature, a hot topic was the fit between human nature and social structure. Our hypothetical gender structures are a way of talking about that fit. If our biosocial model is correct, then there is a human nature, and it is

gendered. The permissive society allows a perfect fit to human nature. The traditional society provides a poor fit: it starts with a biological base and constrains humans to fit it. The unisex society starts with an ideology and constrains humans to fit it.

Let me be clear about my views. The future of gender in our society can, should, and will be determined by ideology. If we believe that one type of social structure is evil and another is good, then we must try to achieve the good one. On the other hand, if our theory of gender is not correct, then we will not know how to achieve our goals.

I don't know how far society can differ from nature without encountering difficult problems of social control, but I never said that the goal of society was to make people comfortable. My goal is not to create happiness, but to fulfill our most worthy ideals for humanity. Human dignity may be achieved at the price of happiness. I emphasize that society has never hesitated to encourage behavior it thought unnatural (for example, celibacy), even at the cost of making people miserable. We have not always been happy with our success in controlling what we considered biologically natural but bad, but we have always considered the effort worthwhile, even if it was only partially successful.

Two general types of implications can be drawn from my propositions. The first is for programs of social change; the second, for demographic and social science research on gender.

First, in regard to programs of social change, we can identify two alternative agendas. First, society should provide gender-neutral opportunity structures. Naturally occurring variation in gender predispositions will determine how people take advantage of these opportunities. This is the permissive society that encourages the unfettered flowering of natural endowments and propensities. The second alternative is the degendering of society (Bem 1994). Those in favor of such degendering assume that gender-neutral opportunity structures would degender society, but degendered socialization is impossible because males and females respond differently to the same socialization. Gender-neutral opportunity structures will produce gendered responses and therefore gendered societies. Degendering society will require compensatory gendered socialization and compensatory gendered opportunity structures.

The second type of implications from my propositions affects research. Demographers and social scientists continue to ascribe all gender findings to gendered socialization and gendered opportunity structures. Although this might be attributed to their desire to be politically correct, such attribution is an injustice to social scientists. They merely have an inadequate theory.

With an improved theory, the demographer and social scientist can see gender in new ways.

First, the existence of gendered social structure is not evidence for gendered behavior norms.

Second, gender norms may be consequences, not causes, of sex differences.

Third, the existence of gendered social structure is not evidence of sex discrimination.

Fourth, parental socialization may bear little responsibility for differences in gendered behavior.

Fifth, if demographers and social scientists don't want to tangle with biological predispositions in their models, they can focus on explaining social change and macrocomparative studies.

Now, I should add the warnings. Work on the biology of gender and how it can be integrated with the demography and social science of gender has just begun. My work is

only another step. It needs to be replicated; it needs to be remodeled and tested on males; other implications need to be examined. Demographers are not the most likely people to carry out this work. The empirical support or modification will accumulate only gradually. As we examine the issues further, they will always turn out to be more complicated than our simple models. Even so, we should not be surprised that our own human pattern of gender shares fundamental causes with the sex dimorphism of our animal relatives. The interesting questions will turn out to be not *whether*, but *how much*, and *in what ways*. There is nothing embarrassing about being a primate.

NOTES

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