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If All We Knew About Women Was What We Read in *Demography*, What Would We Know?*

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How do women appear on the pages of *Demography*? This is not a question of authorship, of whether the articles published in *Demography* by women differ from those published by men. Rather, the question is collective: what does *Demography* indicate about the way we as a scientific community, as authors, reviewers, and readers, understand women? Although I focus on women, my question is also about gender, our shared understandings of what it means to be female and what it means to be male, and how these influence our research.[1]

My curiosity was provoked initially by research on marriage patterns in western Europe. It was puzzling that although historical demographers tried to account for distinctive female marriage patterns (relatively late age at marriage, relatively high proportions of spinsters), the dominant explanatory framework emphasized the circumstances of men (whether they could afford to set up a neolocal household, which was believed to be the prerequisite for marriage in western Europe for many centuries). Subsequently I was struck by the paucity of references to women in the index of *The Decline of Fertility in Europe* (Coale and Watkins 1986). Although this 449-page volume describes and attempts to explain a profound change in bedroom behavior in western Europe between 1870 and 1960, the index shows 21 references to modernization and 21 to occupational differentials, but only five to "women's status" and none to either men or sex (although there are seven references to abstinence). Women were thick on the ground in the

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project on which this volume was based; how is it, then, that we learned so little about them? Or about men?

In asking how women are presented in this journal, I reviewed *Demography*, the official journal of the Population Association of America (PAA), from its first issue in 1964 to the present. I read all the articles on fertility (including contraception) and those on marriage and the family. I also read the entire volumes for 1964, 1974, 1984, and 1992, in order to see how women were pictured in articles on other topics. Between 1964 and 1992, *Demography* published 1,232 articles, of which 36% (444) were on fertility and contraception, and 9% (111) were on marriage and the family (Teachman, Paasch, and Carver 1993). An additional 69 articles on other topics were published in 1964, 1974, 1984, and 1992. These 624 articles—approximately half of those published in the past 30 years—are the basis for my conclusions about women in *Demography*. *Demography* is not the same as demography; almost certainly, demographers share a culture that defines some articles as appropriate for *Demography*, others for other journals in the field. Thus my conclusions apply only to this journal. Nonetheless, the status of *Demography* in the field suggests that the understandings of gender I have discovered on its pages are likely to be shared widely in the field.

Some might say that the core of demography is the growth and structure of human populations—that the distinctive thing we do is to provide an accurate accounting of the entries and exits: births, deaths, and migrations. If this were all we did, we would not expect to learn much about women in the pages of *Demography*. Nor would we expect to learn much about gender: after all, stable population theory, the basis of our accounting system, is a single-sex theory.[2] But this is not all we do. Many articles in *Demography* try to understand or predict demographic behavior. To focus our research—to formulate hypotheses, to select or collect data, to interpret our results—we necessarily draw on commonsense understandings of women and men, on what we believe to be true about why they behave as they do. The discipline expresses a high degree of consensus on the substantive issues that are important and how they should be studied, and dense social networks or ties exist among demographic scholars (Guest 1993). This consensus is likely to include agreement about what it means to be a woman or a man; if it did not, reviewers and editors would complain, and readers would be puzzled. Although there is much variety in *Demography*, my topic is this shared culture—what we take for granted as we try to understand or predict demographic behavior.

As I read, I tried to take the stance of an outsider, a naive reader, asking "If all we knew about women was what we read in *Demography*, what would we know?" I formulated a set of questions to keep in mind while reading each article. I asked first whether the introduction to the article made it clear whether the article was about women, about men, or about both. I was surprised to find that occasionally this point was not made explicit until well into the article. It was usually implicit, however, thus suggesting a local scientific culture. Then, for articles based on data about women, I asked what about women in the society (or societies) under discussion was considered important. To answer this question, I looked to the introduction, the theoretical section, and the conclusion, to see whether the authors drew on commonsense understandings about women to focus their research or to interpret their results. I also looked at the table stubs to see what variables were used to describe women. Where both men and women were included in the analysis, I asked whether the variables were the same or different, and, if they were different, whether either the theoretical background or the discussion of the data explained why. I also asked what was *not* in the analysis: if, for example, the article was about married women, where were their husbands? Then I asked a set of more unusual questions (at least for *Demography*) about the rhetoric of the articles. Classical rhetoric has four components: fact, logic, metaphor, and story (McCloskey 1985). Certainly we use fact and logic, but if authors of

articles in *Demography* use metaphors and tell stories, are they gendered metaphors and gendered stories?

Last, I was curious about whether the women's movement or feminism had ruffled the pages of *Demography*. Women have published prominently in *Demography*, they are a substantial proportion of the PAA membership, and the subject matter of demography perhaps is focused more on women than is the subject matter of other social science disciplines.[3] Some feminists expect research by women to differ either in topic or in techniques from research by men. I do not distinguish, however, between male and female authors (or male and female editors) because my familiarity with *Demography* did not lead me to expect any differences.[4] But reviews in other fields show that feminists have raised questions about research topics, theories, and methods, and that in some cases feminist-inspired research had been influential in overturning conventional wisdom in a field. Is there evidence that feminism has influenced us?

The short answer to the question posed by my title is that we learn from *Demography* a great deal about a limited range of women's activities and characteristics. If all we knew about women was what we read in the articles on fertility, marriage, and the family, we would conclude that women are primarily producers of children and of child services; that they produce with little assistance from men; that they are socially isolated from relatives and friends; and that their commitment to the production of children and child services is expected to be rather fragile. We would learn even less about men. The more interesting conclusion is that to a surprising degree, our research draws on what we take for granted about women, men, and the relations between them in order to pose our research questions, to collect our data, and to interpret our results. I identify areas in which I think a more explicit as well as a more informed consideration of gender relations in the societies we study would probably result in a better understanding of demographic behavior, and I give examples from *Demography* that I believe point in productive directions.

WHAT DO WE TAKE FOR GRANTED?

Typically but not invariably, tables in *Demography* show men on top, women on bottom (as in the sex ratio, sometimes called the "masculinity ratio"). When the data are presented in columns, men are usually on the left, women on the right. Similarly, on graphs showing both sexes, it is usual to find that men are shown as solid lines, women as dotted lines. Although it is not obvious that "top" is better than "bottom," or "left" is better than "right," such an interpretation is suggested when the order of other variables presented in the same table shows "employed" above (or to the left) of "unemployed," or when "literate" precedes "illiterate."[5]

In calling attention to the hierarchical presentation of men and women in tables, I do not mean to imply that authors in *Demography* are saying anything about the position of men and women elsewhere. The construction of tables is no doubt conventional: we arrange them as others have done before us. And it is trivial, because the empirical results would not change (although the nuances might) if we put women on top. But drawing on conventional understandings of gender to place men and women elsewhere in our work is not trivial, because we may fail to recognize how such understandings influence our research.

I will begin with a few examples of offhand appeals to common-sense understandings of women and men that have appeared in *Demography*. These casual comments are used to motivate an analysis or to interpret the results. They are not accompanied by citations; this fact suggests that they reflect what "everyone knows." Typically, I believe, they pass unnoticed because they seem so self-evident: I paused only because I was asking how women appeared in our journal. Casual comments about women appear not only where we

might expect them—in articles that set out to explain trends or variations in demographic behavior and thus call for speculations about the actors' motivation—but also in a large class of articles that examine the accuracy of our data. Demographers typically analyze data provided by respondents (e.g., on censuses and surveys), and as a tribe we are quite concerned about the accuracy of that data (Preston, this issue). We do not naively believe, however, that everything the respondents tell us is "true."

The authors of an article comparing eight U.S. fertility surveys note that retrospective reporting of the timing of first birth is likely to be quite accurate because it "is generally accepted as a significant and critical factor in a woman's life" (Swicegood, Morgan, and Rindfuss 1984, p. 21). But we are doubtful about women's veracity in some other areas. For example, the expectation that women may lie about their ages appears in both the first volume of *Demography* and the most recent. Bogue and Murphy (1964) say "A familiar example is the lady who deducts a few years in reporting her age or the man who exaggerates or understates his income" (p. 42); Kestenbaum (1992) says "We hypothesized that women have a greater tendency than men to conceal their true ages" (pp. 578–79). Whether women lie about their ages more than men is not the issue here. Rather, the point is that we believe it is only sensible to expect that women, not men, wish to present themselves as younger, not older, than they are.

We have expectations about the characteristics of women that are associated with misreporting. Single women are expected to report themselves as married or widowed in order to conceal an out-of-wedlock birth (Preston, Lim, and Morgan 1992). Younger women might be reluctant to report having had a baby (Marckwardt 1973). And Jones and Forrest (1992) compare reported abortions to those estimated as actually having occurred according to the mother's age, race, and marital status. Jones and Forrest do not tell us why they expected race and marital status to matter for veracity: presumably they trusted that we would share their expectations.[6]

Women also are expected to wish to conceal a contraceptive failure, thus leading us to underestimate the extent of contraceptive use. The deans of American fertility surveys, Ryder and Westoff (1969), caution that "admissions of failure are likely to have been reduced by embarrassment at confession of inadequacy, and by rationalization, as well as by a genuine change in judgment based on experience with the previously unwanted child" (p. 442). Although one might expect that experiencing contraceptive failure as inadequacy would be more prevalent in a modern industrial society such as the United States than in developing countries, Knodel and Prachuabmoh (1973) appear to think that the situation is much the same in Thailand, where women might be "reluctant" to admit that they desired fewer children than they now have (p. 626).

Articles about marriage often are motivated by appeals to commonsense understandings about why women might want to marry, or about what makes women attractive to men. Corman and Kaestner (1992) think that the added time and money costs associated with a sick child might make the mother less attractive to men, or, alternatively, that if the mother has specialized in child rearing and other home-oriented activities, "she might be a more attractive marriage partner" (p. 391). In a similar vein, Preston and Richards (1975) speculate that whereas a woman's job might induce a male to feel that he could "afford' " to marry, "it could also encourage a woman to feel that she could 'afford' *not* to marry" (p. 210).

Casual comments about women inevitably raise a comparative question: are our expectations the same for men? This question is hard to answer because the articles on fertility, marriage, and the family simply contain much less about men. Yet sometimes gender expectations are made explicit. Thus, as stated above, women are expected to misreport their ages, men their incomes. Ahlburg and Schapiro (1984) find that a slackening labor market predicted male but not female suicide rates. They interpret this difference by

saying "By and large, women hold lower-wage, lower-status jobs, which do not foster job identification as strong as that for men. As a consequence, job loss does not, in general, result in feelings of personal failure and inability to carry out traditional role responsibilities to the same extent that it does for males" (p. 101). Ordinarily I would have passed over this sentence without mental note. Yet because I was looking for presentations of men and women, I observed the implicit assumption that women identify more with their "traditional role responsibilities"—presumably their domestic tasks—and men with their work outside the home. This may be true, but no supporting evidence is cited to show that lower-income-earning women differ from lower-income-earning men: rather, women's lesser identification with their jobs is presented as self-evident.

More typically, comparisons between men and women are implicit: gender is relevant even when men are absent.[7] In an article on age and marital status at first birth that uses data on women from the 1970 National Fertility Survey, Bumpass, Rindfuss, and Janosik (1978) say, "Unmarried childbearing involves the prospect of child care and self-support in the absence of a spouse, as well as social disapproval, whereas early marital childbearing primarily affects the possibilities of pursuing other life course options within marriage" (p. 76). The readers presumably would draw on their knowledge of the United States in that period to understand that sentence. We would assume that the authors mean unmarried childbearing by women, because we too would have observed that in the United States in that period, it was unmarried mothers rather than unmarried fathers who faced the prospect of child care, self-support, and stigma, and that young mothers rather than young fathers would have been inhibited from pursuing other life course options.

More interesting are articles in which the introductory discussion refers to "teenagers," "couples," or "families" but in which I believe we know, before reaching the section that discusses the data, that the data will refer only to women. This expectation, I think, is drawn from our shared culture as readers of *Demography* rather than from the wider culture. Thus we do not find it odd that an article concerned with the repetition of early births across generations looks at the daughters of teenage mothers rather than at the sons or daughters of teenage fathers (Kahn 1992). The absence of males, however, reveals an assumption to which I will return later in discussing articles on fertility more generally: we take it for granted that childbearing and child care are women's issues.

Like the conventional placement of women and men in our tables, some taken-for-granted understandings about women and men can be considered irrelevant to the analyses themselves. Many of them simply motivate an analysis. If women misreport their ages, perhaps it does not matter much why we expect them to do so. They begin, however, to create a picture of expectations about gendered behavior that I think we share not only as members of a particular scientific discipline, but also as members of a wider culture. We expect that women want to appear to be younger than they are, wish to conceal out-of-wedlock births, and are stigmatized by early childbearing; none of these traits are thought to apply to men. Some of these expectations may be more or less correct because the social experience of the people we study is shaped by norms about gender. Some expectations are phrased as testable hypotheses: we find out whether women with jobs are more or less likely to marry than women without jobs. Others, such as the assumption that women identify less with their jobs than do men, are more problematic and are untested by the authors. Our shared understandings of gender become more critical for our practice when they shape our attempts to understand or predict demographic behavior. I now turn to such analyses.

THE POPULATION CRISIS: WOMEN AS OVERPRODUCERS

In the early years of *Demography*, reference to concerns about rapid population growth often signaled why the author believed his or her research to be important. Thus a straightforward analysis of Latin American censuses ends by saying " . . . the obvious conclusion is that the region is running against odds in the race toward economic growth" (Miró 1964, p. 40). Demographers were not alone in their worries. The climate of the time is indicated by Wilmoth and Ball's (1992) analysis of articles on population in American popular magazines between 1946 and 1990: pieces arguing that population growth is "threatening" reached a peak in 1966–1970, when they accounted for nearly 80% of the articles.

Particularly in the 1960s, the reader of *Demography* finds accounts of what appears to be a struggle between the forces of progress—modernization and/or the organizations that distributed modern contraceptives—and the forces of resistance. The latter sometimes are characterized as macro-level attributes (the "weight of custom and tradition"), sometimes as attributes of individuals ("apathy and inertia"; Chandrasekhar 1968, p. 648). A special issue of *Demography* in 1968 provides a convenient starting place for the depiction of women in this literature. The entire issue is devoted to family planning programs around the world, and includes articles by representatives of major funding agencies as well as by senior demographers and others who were young and obscure then but are quite eminent now. This issue was controversial and therefore unrepresentative, but I think it presents, in a particularly clear (because exaggerated) way, the understandings of women that continue to inform our analyses of fertility change.

A sense of the approach taken in this issue is conveyed by the title of editor Bogue's introduction: "Progress and Problems of World Fertility Control." Progress is attributed to family planning programs, characterized by Harkavy, Saunders, and Southam (1968) of the Ford Foundation as "men and money." Bogue uses military metaphors: the family planning movement is an "organized protest" and a "large-scale action"; its efforts are of "truly heroic proportions," a "crusade," a "Holy War"; the articles in this issue are an array of "battlefront reports" (Bogue 1968, pp. 539, 540). Some of the problems are due to inadequate government support, religious opposition, and so on, but others are due to the difficulty of persuading audiences to use modern contraceptives ("insufficient penetration of the audience"; Wilder and Tyagi 1968, p. 774).

Who were the target audiences that were insufficiently penetrated? Bogue's introduction is followed by a set of articles on the efforts of funders of family planning programs. Women seldom are mentioned directly in these articles. Indeed, a discussion of family planning programs in the United States that is conducted almost entirely in terms of "persons" or "couples" might lead the reader to believe that the clients of family planning clinics are as likely to be men as women (Jaffe and Guttmacher 1968). Yet it is clear that the focus is on women's roles rather than men's in solving the problem of uncontrolled population growth. In the article on the Ford Foundation, we see again an elision from "families" or "couples" to "women": "While most of this nation's [U.S.] families have no need for assistance, at least four or five million women need publicly assisted family planning services" (Harkavy et al. 1968, p. 551). Summarizing the Ford Foundation's work in reproductive biology, the authors refer at length to studies of female contraceptives but only briefly to male methods of fertility control. Notestein, then president emeritus of the Population Council, pointed out that new female methods of controlling fertility had been important in convincing leaders of less developed countries that there were methods suitable for their populations (Notestein 1968, p. 556).

It has been claimed that a convenient conjunction occurred between the family planning movement and the women's movement (Jaquette and Staudt 1988). I found some

use of the rhetoric of feminism in this special issue of *Demography*. For example, Ravenholt summarized A.I.D.'s "basic strategy" by saying "It is believed that the world population crisis can be largely solved by the expansion of human knowledge, freedom, and availability of information and means so that women everywhere need reproduce only if and when they choose" (1968, p. 571). Overall, however, the language of this issue gives few hints of exposure to the women's movement. Even when the language seems to empower women by claiming that they have the right and the power to choose to control their fertility, they are expected to use this power to control world population growth.

A reader from outside our demographic culture might expect that because it takes two to tango, some authors would have discussed why the programs were aimed at women rather than men. Insofar as these articles by leaders of the major family planning institutions cited any evidence at all, it was the result of KAP (knowledge, attitudes, and practice) surveys showing that women wanted to limit their fertility (Notestein 1968, p. 558) or that they wanted "too many" children (Ravenholt 1968, p. 572), without comparable information about what men wanted. Yet whatever discussions of women's and men's motivations or marital power may have preceded the establishment of the family planning programs described in that issue of *Demography*, by then the question had been settled so firmly that apparently it required no further attention.[8] In an article on family planning among the rural poor in the United States, the author says "Family planning in the United States to date has been confined largely to women. This makes sense in the same way that the concentration of programs in urban areas makes sense" (Wilber 1968, p. 909).

My point here is not that these prior discussions which ultimately targeted women (rather than men or couples) were necessarily ill-advised, but that they were based on our understandings of gender differences. In the 1960s, before I went to graduate school, I participated in some of these discussions as a volunteer in the first family planning program in the Dominican Republic and later as a volunteer for Planned Parenthood in Washington, DC. It seemed entirely appropriate to me that these efforts were aimed at women. My friends and I believed that women had more reason than men to be concerned with the consequences of childbearing, and we took it for granted that their husbands simply would not cooperate.

We have evidence that people often believe differences between males and females to be far greater than they really are (Bleier 1987; Epstein 1988; Marini 1990). The foregoing review suggests that this may have been the case for demographers in 1968. Some of the evidence available at the time shows that men may have been motivated as strongly as women to limit family size, or even more so. For example, Dow (1967) found that about 40% of urban Kenyan males reported no "best thing" or particular good attached to large family size, compared to 29% of their wives; Caldwell (1968, Table 2) found similar results for Ghanaian urban elite males and females, and for males and females in Lagos. Moreover, Caldwell's (1968) analysis of a variety of surveys showed little difference between the percentage of males and of females who stated "contraception is right" or "contraception is wrong" (Table 9); among the Ghanaian elite, in Lagos, and in an all-India survey, more males than females expressed interest or willingness to use family planning (Caldwell 1968, Table 11; Poffenberger 1968; also see Roberts et al. 1965).[9]

Perhaps the decision to target women was a legacy of the birth control movement in the West, which also focused on women (Hodgson 1991). Also it may be that evidence from KAP surveys, even if correct (there is some distrust of their results), was fundamentally irrelevant; other considerations properly may have carried more weight. These include the more rapid development of coitus-independent techniques of contraception for women and the availability of an infrastructure of maternal and child health clinics. Or, possibly, demographers as well as funders may have concluded rightly that Third World leaders were reluctant to target men, or local clinic workers may have drawn on their knowledge of

gender relations in their own society to conclude that husbands would simply not cooperate. Nonetheless, the lack of explicit justification for focusing on women as targets of these programs, and the disregard of evidence from some KAP surveys, lead me to conclude that this focus almost certainly was influenced by unsupported assumptions about differences between men and women in their willingness to use modern contraceptives.[10]

Later volumes of *Demography* contain fewer articles on family planning programs, and the rhetoric of "population crisis" cools. Women rarely are referred to as targets (but see Nortman and Bongaarts 1975). I found it harder to detect assumptions about women and men, in part because the language becomes more distant and more "scientific"—references to "apathy" or "inertia" largely disappear—and in part because the assumptions are so familiar that I am somewhat deaf to their implications. Even so, I think the legacy of the 1960s can be detected in two major streams of fertility research, on proximate determinants and on fertility preferences. Both continue to focus on women, both depict women as overproducers of children, and both ask what might reduce fertility.

The literature on proximate determinants examines interactions between biology and behavior. At issue is what the maximum production of children would be if women were to produce at their "natural" biological capacity, and to what extent phenomena such as miscarriage, breast-feeding, and coital frequency inhibit women's fertility. In this literature the focus is women's physiology—their bodies. Yet the language draws on metaphors of production and technology, particularly on interference with the production of children. Abortion or miscarriage is "fetal wastage" (Sheps 1964). Extended breast-feeding is a "brake" on fertility (Menken 1979). Spousal separation is "efficient" (that is, it reduces fertility) if it overlaps fecundable rather than gestational or anovulatory exposure (Potter and Kobrin 1982, p. 80). (For other work analyzing the equation of reproduction with production, see Jacobus, Keller, and Shuttleworth 1990). Later we shall see that metaphors of production are not confined to the literature on proximate determinants.

The preference literature of the 1970s, 1980s and 1990s continues in the tradition of the KAP surveys. The National Fertility Surveys, the World Fertility Surveys, and the Demographic and Health Surveys are more sophisticated than the KAPs. Yet because the questions on preferences are asked of women, not men, I think the implicit assumption continues to be that some women produce more children than they want—or than the world needs—and would produce fewer if they had access to modern contraception and could use it properly. That men are rarely surveyed suggests either that men's and women's preferences are assumed to be the same, in which case discrepancies in male and female marital power are irrelevant, or that women are in charge in the reproductive realm.[11]

In our attempts to understand women's behavior we do not rely on their responses to questions about their preferences. We also make assumptions about why women behave as they do, and subject these to empirical examination. Some of these assumptions, I think, are based on commonsense and would be shared by those who are not demographers. Others are governed by theory: they are understood within the local culture of *Demography*, but probably would not be shared by the man or the woman in the street. I turn now to articles on the demand for children, beginning with our data and then examining our theories.

THE DOMESTIC SPHERE

Just as I looked for casual, offhand comments about women to see what our commonsense notions about them were, I looked at the data we use in our analyses, because I expected that the characteristics on which we focus attention would indicate what we consider important about women—and, by omission, what we consider unimportant.

The first generalization about our data is that the population distribution in the articles

on fertility, marriage, and the family is peculiar: primarily women between the ages of 15 and 50. Keyfitz (1964), writing on population projections, characterizes "women and girls" between the ages of 15 and 45 as the "dynamic portion" of the population. Before menarche, females are of little interest; after menopause they disappear from view, as if we regarded postmenopausal women as they are described by the psychoanalyst Helene Deutsch: [At menopause] "woman has ended her existence as a bearer of future life and has reached her natural end—her partial death—as a servant of the species" (quoted in Fausto-Sterling 1985, p. 113). Yet because these articles look beyond the biological basis of reproduction to consider its social and economic determinants, the omission of postmenopausal women is less obviously sensible than it may appear.

A second generalization is that a rather meager range of women's activities is thought to be relevant for their behavior. As Hirschman and Guest (1990) point out, "The major problem for contextual research on fertility has been the selection of macrolevel variables from the almost infinite list of economic, social, cultural and organizational features of ecological areas that could conceivably affect fertility" (1990, p. 372); this statement could apply also to micro-level variables. Yet a simple examination of the tables in each article on fertility shows that the list of variables we use is far from infinite. Typically tables show 1) the women's demographic characteristics—for instance, age, marital status, and parity, 2) education, 3) labor force participation, 4) urban or rural residence, and 5) some measure of income. Especially in articles on the United States, race and religion also are included frequently. In some analyses, the data are aggregated and ungendered: thus, comparative country-level analyses may use national per capita income figures with crude birth rates as the dependent variable (e.g., Ekanem 1972; Janowitz 1971; Kasarda 1971). With the increasing availability of survey data, however, the independent variables are likely to be the woman's education, her labor force participation (but not her occupation), her urban or rural residence, and her husband's income and/or occupation. In recent analyses that include contextual effects, they are likely to be aggregates of these individual characteristics—for example, the proportion of women who have a certain level of education (e.g., Hirschman and Guest 1990). The reader of *Demography* learns a great deal about these characteristics but not much about others: certainly a wide variety of other characteristics have appeared over the years, but they are not included consistently.

This rather short list of characteristics that consistently are assumed to represent women's demand for children suggests considerable consensus in *Demography* about what is important. These variables either are expected to represent the degree to which women are exposed to the risk of childbearing (younger women are more fecund than older, married women more exposed to intercourse than unmarried) or are interpreted as representing activities that compete with domestic activities. Moreover, I was rather surprised to realize that largely the same characteristics of men and women appear in articles on mortality and on migration.

Certainly the aspects of women's lives on which we have chosen to concentrate are those which common sense tells us are promising. Moreover, the research published in *Demography* leads to the conclusion that this common sense is often correct: they are important. We typically find, for example, that women with different levels of education differ in the number of children they have or the age at which they marry.

Our short list of variables, however, reveals some omissions that probably would surprise those who are not part of the culture of *Demography*.

A visiting anthropologist would find it necessary to read *Demography* rather thoroughly in order to find a precise answer to the question "Where do babies come from?" Nearly half of the articles published between 1964 and 1992 concern either fertility and contraception or marriage and family, but in articles about married women, a birth appears to result from an immaculate conception. Sometimes we use euphemisms such as "exposure

to intercourse." Rare, however, is a more nuanced approach to sexual activity, as in Tolnay and Guest (1984) or in an article by Tsui, de Silva, and Marinshaw (1991), where we read that "discouraging a partner's sexual interest, avoiding coitus or coital sex, or creating conditions that are nonconducive to intercourse (such as modifying sleeping patterns)" indeed may lower the risk of conception (p. 115). Our more typical avoidance or abstraction both neutralizes sexuality and masks men's roles in reproduction (and thus in population growth) (Miller and Fowlkes 1987; also see Fox- Genovese 1991; Gagnon 1989; Meyering 1988; Seccombe 1992. For a similar prudishness about sex in the work of social scientists in the late nineteenth and early twentieth centuries, see Rosenberg 1982). Interestingly, sex plays a more prominent role in articles on teenage childbearing (Thornton and Camburn 1987; Udry, Talbert and Morris 1986; Weinstein and Thornton 1989).

An outsider also might wonder about the absence of data on men. In the articles on fertility, marriage, and the family we learn even less about men than about women. If men's characteristics appear at all on our tables, they are most likely to be their income and/or occupation.[12] In contrast, we rarely learn about the wife's income or occupation. When data on men are included, sometimes the same characteristics are assumed to be salient for both men and women. In other cases, however, the data on men are not the same as for women; this fact suggests that we think different things matter for women than for men. The most obvious difference is age, which is usually given for women but not for men, and is interpreted as indicating women's biological capacity to have children. Another is parity: we typically ask how many children a woman has borne, not how many her husband or partner has fathered.

Other differences are farther from biology, and thus more clearly indicate assumptions about gender. As examples, consider two studies that—untypically—gathered data on men. Freedman, Hermalin, and Chang (1975) interviewed Taiwanese husbands as well as wives (Table 8, p. 414). Apparently, however, they asked only men about their aspirations for their children's education and for savings, thus leaving the impression that men's aspirations in these respects were more salient than women's. Similarly, Hogan and Frenzen (1981) analyzed the results of a survey conducted in Thailand. Family background and socioeconomic characteristics were collected from both men and women. Other information was obtained from only one member of the couple.[13] Women were asked about their exposure to mass media (radio, cinema), and men were asked their opinions on the growth rate of the population of Thailand (Table 1, p. 602). Possibly the Thai scientists who collaborated on this survey knew that Thai women, but not men, were exposed to the mass media, or that Thai men, but not women, could respond to questions on population growth. It is also possible, however, that the questions reveal assumptions about more profound gender differences: that women would be influenced by the mass media in ways men were not, or that women's interests did not extend to public issues such as population growth. Also notable is the absence of either conflict or cooperation between men and women. Using only our data as a guide, a naive reader of *Demography* would conclude that women are wholly in charge of the household. Not only do married women appear to bear their children without much help from men; they also appear to rear them alone, without much help from their husbands (except as providers of income; for an exception, see Presser 1987, 1989, who shows that men cooperate in child care by taking on shift work). We do tabulate and try to account for divorce, presumably the outcome of conflict, but this conflict does not seep out into articles on childbearing or child rearing.

Another omission that would probably strike those unfamiliar with *Demography* as surprising are those that might represent a separate women's culture. For example, I think women outside the culture of *Demography* would expect women's networks to influence the extent and duration of breastfeeding. Yet an article on the "household demand" for breast-feeding, based on data from the Sri Lanka World Fertility Survey, uses the

conventional variables: the mother's age, the number and ages of her other children, her education, her work inside or outside the home, urban/rural residence, and her husband's education, plus the woman's ethnicity and the characteristics of the household (whether it has a refrigerator, a toilet, or a landlord, and whether the household "farms own land") (Akin et al. 1981). Perhaps ethnicity, which the authors interpret as representing cultural differences, is meant to indicate the influence of other women. Still, I found myself wondering rather irritably whether it was appropriate to think in terms of a "household demand" for breast-feeding. Ryder (1973) reminds us that we that "we shouldn't forget Granny in the back bedroom" (p. 504). Surely the postmenopausal women who drop out of our tables—not only relatives in the back bedroom but also friends and neighbors outside the household—have forceful opinions about breastfeeding?[14]

What goes into our analyses, and what is left out, is in part a function of the data available. This is the case especially when we use data collected for other purposes, as many of us do. Although the list of influences on fertility may be long, we must resort to what is available. This reliance on data collected for other purposes has forced us to be rather ingenious in our interpretation of available variables. We are less limited when we conduct our own surveys, although here too we face constraints: the World Fertility Survey did not usually ask even about grannies.[15]

What is included or excluded is also a function of our cumulative research record. Thus, we have learned that education is a powerful variable, so it is almost invariably included: the debate centers simply on its interpretation.[16] We have concluded that psychological variables add little, so they have been largely dropped (but see W. B. Miller 1992). Yet the shortness of the list of variables that appear consistently is also a function of our style of work, which leads us to search for generality and parsimony. We want to understand or predict the fertility decline in general rather than its occurrence in a particular time or place, and we do not want our explanations to include the kitchen sink. Our theories point us to some characteristics of women as more worthy of attention than others, and as more likely to apply in the general case. If we are trying to understand women's behavior, obviously it is important that we look in the "right" places. Our theories guide us to these places.

Although most articles in *Demography* are not very explicit about theory, two theoretical frameworks have dominated analyses of fertility, marriage, and the family in *Demography*: modernization and the new home economics (NHE). The former is a capacious but often ill-defined approach to the societal transformations associated with industrialization and the spread of "new ideas," whereas the NHE is focused more narrowly on a central set of issues that concern the efficient allocation of time and other resources inside and outside the household (Becker 1981, 1991).

I think modernization and the NHE share similar understandings of men and women. First, women are primarily producers of children (as in the literature on proximate determinants discussed earlier) and "domestic services." This point is clearest in the NHE. Becker can be credited with turning the economists' attention to the family by explicitly translating economic analyses of the firms' production to the household's production of domestic goods and services (Pollak and Watkins 1993). Second, both frameworks are steeped in notions of gender roles. Men work outside the home, whereas women are responsible for activities associated with the production of children and domestic services.[17] In *Demography*, these domestic services involve primarily care for the children and, increasingly, for the elderly.

Who is in charge of the family? Issues of power are ignored almost completely in *Demography* (for an exception, see Beckman et al 1983). In articles in the modernization framework, the forces affecting behavior are societal-level and impersonal, even when the data concern individuals. Yet because women usually provide the dependent variable (e.g.,

birth rates, fertility preferences, contraceptive use) and because their characteristics either stand alone or predominate on the right-hand side of the equation, I think the implication is that women are in charge. (This is also the implication in the less theoretically motivated literature on fertility preferences.) The NHE is far less opaque about power. An "altruist" is in charge, making decisions for other members of the family: the pronoun "he" refers to the altruist, and "she" refers to the beneficiary (Becker 1981, p. 173). Pollak (1985) describes the altruist as having the characteristics of a "husband/father/dictator/patriarch" (also see England forthcoming; Folbre 1983). Thus, "household demand" is his demand.

The core issue in research on fertility differentials and trends is what leads women to abandon their domestic activities such that they delay marriage, do not marry at all, or bear fewer (or no) children. In both theoretical frameworks, the stories we tell emphasize activities that take them out of the household: as young girls to school, or as adult women to work. In the modernization framework, both men and women are expected to be influenced by their education, but the effects are expected to be quite different. If education leads a man to seek wider horizons, the articles in *Demography* do not typically assume that he will abandon his role as the family breadwinner, but they do assume that education will erode his wife's performance of her role. In the NHE, the effect of education is much simpler: it increases the amount the wife could earn if she were to produce for wages, thus raising the opportunity costs of her staying at home. The gap between the two theoretical frameworks is even less when we consider female labor force participation. In both, women's work outside the home is considered to compete with her domestic activities. Both also assume that work outside the home is likely to be more attractive than work inside the home.[18]

More speculatively, our dominant theoretical frameworks and the variables that we use in *Demography* to characterize men's and women's activities create a picture of men's and women's behavior that is surprisingly similar to the gender conventions contained in the doctrine of separate spheres. This doctrine expresses the family aspirations of middle-class families in the United States in the nineteenth century (Cott 1977; Degler 1980). In this doctrine, as in *Demography*, men are breadwinners, whereas women produce in the domestic sphere. The ideal household is nuclear, and, as novels of the time make clear, both sex and conflict are expected to take place behind closed doors. In the modernization version of separate spheres, women's domestic responsiblities appear to include decisions about contraceptive use. In the NHE version, these decisions are made by the husband.[19]

Although I am sure many readers will not agree, I think both theoretical frameworks reflect an understanding that pervades the culture of *Demography*: that a woman's commitment to the domestic sphere will continue only so long as she does not receive too much education or make a better deal for herself (or for the altruist) by working outside the family. In this sense I believe we view women's commitment to the production of children and domestic services as rather fragile. When women become educated or work outside the home, we expect them to be less likely to marry, less likely to bear many children, and more likely to look for substitutes for breast-feeding. Despite great changes in our methods and our sources of data over the past 30 years, this view of women is rather consistent over these decades.[20]

As Thornton (1991) pointed out, many of our views about population have deep roots; this appears to be no exception. Consider, for example, Spengler's explanation for the decline of fertility in the West:

> The arch-woman, the peasant woman, is a Mother. Her whole destiny desired from childhood, lies in this world. But now there is the Ibsen-woman, the comrade-wife, heroine of cosmopolitan literature from nordic drama to Parisian novel. She has mental conflicts instead of children, marriage is a problem in Arts and Crafts, the main thing

> is 'to understand one another.' It does not matter whether an American lady cannot find sufficient reasons for having children, because she does not want to miss a single 'season', a Parisienne, because she fears her lover will leave her, or an Ibsen heroine, because 'her body belongs to herself.' They all belong to themselves, and are all infertile. (Spengler 1924, quoted in Eversley 1959, p. 161).

Spengler, an archconservative, was dour about the consequences of teaching women to read because he thought declining fertility (particularly in some segments of the population) was a problem. In contrast, we interpret increasing levels of females' education as a sign of progress, liberating women from repetitive childbearing and domestic drudgery and helping to solve the population crisis. Yet some ambivalence about this liberation, I believe, remains in the pages of our journal: Who will take care of the children? Who will cook dinner?[21] Thus, although today's list of temptations for women is shorter and less misogynistic, I think the view that problems arise when women desert the domestic sphere is an enduring one in western culture, and in the culture of *Demography* as well.

GENDER AND SCIENCE

In the previous sections I have pointed to ways in which our understandings of gender influence what we do as demographers. This is no more than pointing to one of the ways, among many, in which we as scientists are influenced by the cultures in which we live.

> Science, since people must do it, is a socially embedded activity. It progresses by hunch, vision, and intuition. Most of its change through time does not record a closer approach to absolute truth, but the alteration of cultural contexts that influence it so strongly. Facts are not pure and unsullied bits of information; culture also influences what we see and how we see it. Theories, moreover, are not inexorable inductions from facts. The most creative theories are often imaginative visions imposed upon facts; the source of imagination is also strongly cultural (Gould 1981, pp. 21–22).

One of the cultures on which we draw is discipline-specific: this culture influences our paradigms—the problems we choose to attack, the methods we use, the interpretations that are considered satisfying (Kuhn 1970). The paradigms themselves, however, are influenced by our lives outside the office (Harding 1986; Jacob 1988; Mendelsohn et al. 1977; Rosenberg 1988). Feminists writing on science build on this literature in the history and philosophy of science, adding to it an interest in gender (Nielsen 1990).

One of the goals of the feminist movement has been to change cultural understandings of gender, both outside and inside academia. In a review entitled "A Lab of One's Own," Clifford Geertz writes,

> The intrusion, advance, spread, import, insinuation—word choice is important here, exposing world views, projecting fears—of feminist thought into just about every aspect of contemporary cultural life is by now entirely general. Literature, philosophy, sociology, history, economics, law, even linguistics and theology, are engulfed in fierce and multisided debates over the relevance of gender difference, gender interest, and gender prejudice to this or that issue or to the shape of the enterprise overall (Geertz 1990, p. 19).

In the early years of the feminist movement in academia, the focus was on breaking down the barriers against women and on a call for research using data on women. Subsequently there developed a more sophisticated literature on gender and science that raised questions about the relevance of gender for methods and research topics, and, ultimately, for science.

This literature is both voluminous and diverse. I will try to summarize briefly what I have learned from it, because I think some of this literature is pertinent to thinking about what we do as demographers.

Some feminists have written about the methods of science (for a bibliography of feminism and science, see Wylie et al. 1989; the citations below are illustrative rather than comprehensive). Some claim that the "unfeeling" distance between analysts and subjects hinders our ability to understand (Keller 1983; Laslett 1992). Although there are those who claim that this distance can only be overcome by ethnographic methods, acknowledged feminists use all existing methods (Reinharz 1992) and most argue for a diversity in methodological approaches. Many feminists point out that their disciplines focus on topics that are of greater interest to men than to women. Dorothy Smith (1987), for example, criticizes sociologists for concentrating on public activities that primarily engage men (and for overlooking women's contributions to these activities) while ignoring the activities that primarily engage women. And feminists have called attention to paradigms of human behavior in their field which they believe are more compelling to men than to women, such as an emphasis on hierarchy, competition, control, and self-interest, and which thus overlook characteristics more usually associated with females, such as connection and cooperation (Gilligan 1982; Nelson forthcoming; Mansbridge 1990).

More generally, some feminists argue that theories that attempt to state universal relations cannot adequately capture realities that are context-specific (Ross 1991). Although they believe that reality is knowable through science, they stress that such knowledge is always partial and influenced by the "standpoint" of the observer (Haraway 1988; Hartsock 1987). This leads directly to the critiques I found most unsettling, those that characterize attempts to formulate universal theories of human behavior as typically masculine, particularly to the degree that they are expressed in attempts to "control" variables or to "dominate nature" since nature is often conceptualized as female (Ortner 1974; Keller 1985; Strathern 1980). Feminists do not argue that men and women are "essentially" different: they emphasize changes in gendered behavior over time and the diversity within each gender in specific times and specific cultures. But since men and women typically have different social experiences, and perhaps differ in their psychological development as well (Keller 1985), many feminists find it plausible that the way that men and women do science could differ. This is not to say that it does, in practice, differ notably: because of the consensus within fields expressed through peer review, women are likely to publish articles quite similar to those published by men.

Despite the diversity in feminists' critiques of science, I think most share a view that science, as well as gender, is socially constructed; scientists can thus never be completely objective and value-free (Folbre forthcoming). How communities of scientists think science ought to be done has changed over time, and bears the imprint of the historical contexts in which it has taken shape, including the understandings of gender that prevailed in a particular time and place (Bordo 1987; Ross 1991).

In some fields research has been influenced by the feminist call to be vigilant in distinguishing between beliefs about gendered behavior and reality, although an outsider finds it difficult to evaluate the consequences. Primatologists, for example, have shown that because of the presumption that male competition rather than female choice was significant in sexual selection and in reproductive success, researchers were unable to see that females also have dominance hierarchies, that they select their partners, and that often they do not select dominant males (Bleier 1987, p. 115; also see Haraway 1989). Research by biologists on slime molds contradicts the conventional view that the aggregation of cells is orchestrated hierarchically by a special "pacemaker" or "founder" cell—much like a patriarch, or perhaps an altruist—that emits periodic signals to the other cells (Keller 1985, pp. 150–57).

The example most striking to me was Emily Martin's (1991) article "The Romance of the Egg and the Sperm" because it shows the influence of gendered understandings in a "hard" science and, moreover, in an area relevant to demographers' interests in women's physiology. In reviewing textbooks on reproductive physiology, Martin found that the story of the egg and the sperm is typically told in gendered terms. Eggs are passive: they are "transported," they are "swept," or they "drift." In contrast, multitudes of "streamlined" sperm with "strong tails" compete fiercely to burrow through the egg's coat and "penetrate" it. New research, however, has revised this story. This research gives the egg a more active role, and presents the sperm as less competent than had been thought. In one study, the researchers discovered that the forward thrust of the sperm is extremely weak, and that their strongest tendency is to escape by attempting to pry themselves off the egg. If the egg did not trap the sperm and prevent their escape, few if any would reach the egg (Martin 1991).[22]

Closer to home are critiques of the NHE that are not made only by scientists identifying themselves as feminists, but nonetheless are consistent with some of the strands of feminist critiques of science. Birdsall (1976) argues that men's and women's interests may diverge. Sen (1990) calls for more attention to the interaction between culture and household production in order to "clear the fog of ambiguity in which the roles of different types of laboring activities are hidden by stereotyped social perceptions" (pp. 129–30). Some scholars have shown that the evidence does not support the assumption, inherent in NHE models, that women specialize in housework because they have a "comparative advantage" in household production (Bergmann 1987; Brines 1993). Moreover, the failure of the NHE models of household behavior to explore power relations within the family renders the approach incomplete on its own terms (Folbre 1983; Pollak 1985).[23] Yet another feminist insight, and one that I found particularly compelling, is that neoclassical models are inadequate because they are based on an assumption of "separative selves": that humans are autonomous and impervious to social influence (England forthcoming).

My reading of *Demography* between 1964 and 1992 suggests that echoes of what Geertz termed "fierce and multisided debates" are muted in our journal. Perhaps we found the early feminist agenda largely irrelevant because women were represented relatively well among authors of *Demography* and in the PAA, and because much of our work was based on data about women. And perhaps on the whole, the *Demography* community found the feminist critiques of research topics "too political" for a scientific discipline, and viewed qualitative methods as inappropriate or problematic.[24]

The women's movement and/or feminism, however, seem to have had some impact. A few articles include the influence of the women's movement as part of their research question, or mention it as an influence on the behavior in question (see, for example, Blake 1974; McLaughlin 1974; Pebley and Westoff 1982). And although calls for improvement in women's position are rare in *Demography*, they exist (Preston and Richards 1975). Demographers addressed major changes in women's labor force participation: Teachman et al. (this issue) show an increase over the last 30 years in articles on marriage and the family, including those on female labor force participation and child care.[25] Perhaps even more important for *Demography* has been feminist pressure to collect a wider variety of data. In the 1970s a feminist group, Social Scientists in Population Research, persuaded the Census Bureau to collect data on child care and on child support, thus enhancing the potential for research on these issues. They also pressured the Census Bureau to drop the census category of household head: they argued that if the Census Bureau wanted it as a measure of authority structure (as was claimed), they should "ask an authority question" rather than a household-head question (interview with Harriet Presser in van der Tak 1992).

Power is a central issue for feminists. The term *women's status* was used in the early years of *Demography* (Berelson 1964), then dropped out, and recently has reappeared

(Hirschman and Guest 1990; Poston and Gu 1987), perhaps at the prompting of feminism. Mason (1987) considers the status of women to be an issue of men's and women's relative power. In *Demography*, however, *women's status* is a new label for several of our conventional variables, particularly women's education and nondomestic labor force participation. The distribution of power thus remains a central question that we still must address explicitly. If women's interests in more or fewer children can be overruled by their husbands, by others in the family, or by their friends and neighbors, we need to know more about the characteristics of those others. To address this question requires expanding our collection of data beyond surveys of individuals: the distribution of power in the family and in the community is institutional and thus is not represented adequately by information on individuals (Caldwell 1982; Greenhalgh 1990; Johansson 1991; Mason 1987; McNicoll 1992; H. L. Smith 1989).

WHAT NOW?

Before I began this review, I believed that as a field we had made great strides in our understanding of demographic behavior, and particularly of fertility change, the area I know best. In 1964, for example, when *Demography* was first published, we knew little even about levels of fertility in many parts of the world, and much less about trends and how to explain them. Now we know a great deal. Much of this progress is due to our commitment to "normal science," by which I mean the development of formal demographic models, precise quantitative description, and the rigorous testing of hypotheses. These conclusions remain unchallenged, and even amplified.

Still, I was surprised to see just how much we draw on taken-for-granted understandings of what women are like, and of the differences between men and women. These appear in the casual comments that are used to motivate analyses of misreporting of age and marital status, or to justify our suspicions about the accuracy of reporting of contraceptive failure or abortion. Even when what we take for granted is not explicit, it is implicit—as in the focus on women and the virtual exclusion of men in articles motivated by concern about high rates of population growth. Of greater consequence for our research, understandings of gender appear in our theoretical frameworks—the stories we tell about why people behave as they do.

In the articles in *Demography* on fertility, marriage, and the family, we clearly expect the men that we study to behave like men, the women like women. Some of these expectations are based on biology. Fertility is one of our disciplinary interests, and only women bear children. Yet the pages of our journal show that we understand that much of the behavior that precedes and follows birth is social. The social construction of gender in *Demography* has two layers. One is the social construction of gender that influences the behavior of the men and women we study. We expect them to have internalized notions of what it is to behave like a man and what it is to behave like a woman, and we expect that they will behave more or less in these ways. Thus we consider it common sense that women rather than men will worry about the stigma of early childbearing, and that both men and women will consider child care the woman's responsibility. The second layer is displayed in our local scientific culture. Our theoretical frameworks are necessarily more parsimonious and more general than commonsense understandings of gender. As now formulated, these frameworks, I believe, lead us to expect men and women to behave as if they had internalized a particular social construction of gender, the nineteenth-century western doctrine of separate spheres. In this doctrine men bring home their income; women are embedded primarily in the domestic sphere, producing (at times overproducing) and rearing children. In the version of this doctrine that appears on our pages, women fulfill their

domestic responsibilities with little help from husbands or postmenopausal grannies, and rarely chat with relatives or friends. Perhaps precisely because this is such an unappealing picture, we found it intuitively plausible that women's commitment to their domestic responsibilities would be rather fragile: that they would reduce the time spent on these domestic responsibilities when tempted away by higher education or paid work, or that they would turn their back on marriage when they could.

If I have painted a reasonably accurate picture of the way women appear in the pages of *Demography*, so what? What are the implications for our research? It may be that this picture of separate spheres captures reasonably well the way most men and women live in the societies we study. But it also may be that what we take for granted about gender gets in our way. The literature on gender and science that I reviewed earlier points to the need to be vigilant in distinguishing between beliefs and reality, especially when the reality that emerges seems to be associated with the social construction of gender. The challenging of conventional understandings of gender has led researchers to interesting new knowledge: the romance of the egg and the sperm no longer resembles the stories in *True Romances*.

Such caution is especially relevant with respect to our theoretical frameworks, because these determine the data we collect or select. From the viewpoint of parsimony and generality, it is all to the good that our view of what it means to be a male and what it means to be a female is rather stark, and thus that our list of consistently important influences on behavior is rather short. Yet our theoretical frameworks are built on maintained hypotheses, the assumed stories we tell about human behavior that we do not test. These tell us not only what to "see," but also what to ignore. We can find out whether the characteristics we expect to be important are indeed important: the problem is with what we have swept under the rug. If our theoretical frameworks lead us to expect conflict or cooperation to be unimportant, for example, we will not look for data to find out whether they are important.

In this review I have emphasized consensus among the authors, reviewers, and readers of *Demography*. Yet some examples of articles show the fruitfulness of challenging that consensus. Below I have chosen three such examples, which I think show what can be done when we don't take for granted our own local culture's expectations about gender.

The first concerns conflict. As I noted earlier, articles about the determinants of fertility are usually based on surveys of married women and typically disregard the possibility that men's preferences differ from women's. Reliance on the wife's report is presumably justified by early analyses based on surveys of both men and women, which showed that "fertility preferences of husbands and wives exhibit similar enough correlations so that it would be redundant to present both sets of relationships" (Westoff et al. 1961, p.140, cited in Thomson, McDonald, and Bumpass 1990; also see Fried, Hofferth, and Udry 1980; Morgan 1985). Relying on the report of only one member of the couple is efficient; it saves time and money. Interviewing only the wife rather than the husband is most efficient because information on women is indispensable in many analyses of fertility (e.g., those which require measurement of exposure time).

In "Fertility Desires: Hers, His and Theirs" (Thomson et al. 1990), the authors propose that excluding men's fertility desires and attributing joint preferences to the wife rather than to the husband is inadequate because it attributes to wives not only all the variance explained by shared desires but also all the variance due to the resolution of conflicts in the wife's favor (for a similar argument, see Coombs and Fernandez 1978; Sorenson 1989; Thomson 1983). Thomson et al. reanalyzed the Princeton Fertility Surveys, the first wave of which (in 1967) interviewed husbands. Distinguishing between shared and conflicting desires raised questions about defining "unwanted" births on the sole basis of the desires of wives. It also revealed that the desires of husbands affect couples' fertility, that disagreement reduces the likelihood that either spouse achieves his or her desired fertility, and that

ignoring husbands distorts our accounts of social and economic variation in the achievement of desired fertility (Thomson et al. 1990, pp. 579, 586).

My second example also concerns both men and women. Typically we expect that women's commitment to childbearing will be weakened by nonfamilial activities such as education and labor force participation. But what about the effect of non-familial activities on men's interest in childbearing? A recent article by Axinn (1992) shows what we would have missed if information about only women's nondomestic activities, and not men's, had been collected. Following a line of research articulated most clearly in the pages of *Demography* by the work of Freedman and his collaborators on demographic change in Taiwan (see, for example, Freedman et al. 1975; Thornton 1986), a collaborative project by Thornton, Fricke, and Axinn used survey and ethnographic methods to collect data on the nonfamilial experiences of both husbands and wives among the Tamang in Nepal (see, for example, Fricke, Axinn, and Thornton 1993). These included work outside the family, residence away from family and peers, moviegoing, and participation in the Small Farmers Development Project.

Axinn's knowledge of the context in which the Tamang live leads him to expect that husbands will have as much influence on fertility decisions as will their wives, and indeed that men's extrafamilial experiences may be more influential than those of their wives. Separate regressions—each with the wife's contraceptive use as the dependent variable, but distinguishing between husbands' and wives' experiences—support his expectations that extrafamilial activities matter, but that they matter more consistently for the husbands than for the wives (Axinn 1992).

A third example concerns social interaction. Women and men typically are seen in *Demography* as isolated. They do not interact with others: rather, we expect them to conform to the "separative" selves of neoclassical economic models. My own work on historical fertility transitions, however, led me to think that women's gossip—I didn't study men—may have been important both in spreading information about techniques of fertility control and in altering fertility preferences (Watkins 1991; Watkins and Danzi 1991; Watkins, Goldstein, and Spector 1992). I also found intuitively appealing the idea that women's gossip affects their behavior; it conforms to my personal observations that even when women are primarily performing their traditional roles, they have a life outside the domestic sphere that influences their behavior within it.

The early years of *Demography* give some indication that informal conversations matter for fertility behavior. Palmore, Hirsch and Marzuki (1971), for example, report that three-fourths of Malaysian women living in metropolitan areas had discussed family planning with friends, relatives, or neighbors, and they suggest that the opinions of others were influential. (For similar findings, see CELADE 1965; Concepçion and Flieger 1968; Duby and Choldin 1967; Hawley and Prachuabmoh 1966; Palmore 1968; Poffenberger 1968; Povey and Brown 1968). I found no comparable studies in *Demography* for males; Entwisle et al. (1991), however, conclude on the basis of focus group interviews and interviews with village leaders that women in the Thai villages that they studied not only talked a great deal about contraception, but also discussed it much more than did the men.

Thus the lessons of my first two examples concern the advantages of bringing men into our analyses of fertility: the lesson of the third is the relevance of what may be, at least in some contexts, a distinctive but nondomestic women's sphere. The more general moral, however, is that the gender which is constructed in our theoretical frameworks may hinder our understanding of demographic behavior.

One recommendation that follows from these examples is obvious: that we simply be more alert to ways in which the social construction of gender shapes not only the behavior of the people we study, but also the theoretical frameworks that lead us to collect data about some behaviors or characteristics and not about others. A second is perhaps somewhat less

obvious because it concerns our style of work. In general, when we seek to explain behavior, we prefer to assume what people want rather than to ask them. To formulate our theories, we combine casual observation of the world in which we live with introspection.[26] To make obvious the limits of casual observation and introspection, I offer the following *cri de coeur* from a Sicilian woman, taken from a book by Ann Cornelisen (1976), who lived in Sicily for 20 years:

> I had nine children in that room, back there, and I suppose I'll die there the same way—with all the men in the family sitting around the fire, muttering, 'Why doesn't she hurry up about it.' When my time came and the pains started, I'd send my husband to call my mother—after she died, he called my sister—whichever it was would tell the midwife, and they'd come and stand by the bed and wait to see how far apart the pains were. I'd hear the shuffling in the room and know the men were arriving, one by one. My father, my brothers, my husband's brothers, they all sat there by the fire and drank wine and waited. . . . I suppose I'll die the same way. The men used to say, 'She's a brave one, she is.' But I'll never forget the pain. I remember all nine times, just how they felt—and every one is different, I can tell you—and you just lie there and bite the towel and never let out a sound. Not once. So many times it was all for nothing too. Six of mine died. I could have wailed then—that's all right—but there are some hurts that stay inside. Every time one of my babies was about to be born I'd think to myself, You're going to die! this time you're going to die! Then it'd come out (pp. 130, 132).

The point is not that this passage demonstrates that the fear of death in childbearing motivates women, but not men, to control childbearing, although that may be the case. The point is rather that without listening to Sicilian women talking, we would not know whether fear of death in childbearing motivates *them*. And without talking with both Sicilian women and men, it would be difficult to appreciate the extent to which the women's views differed from those of their husbands, sitting by the fire, drinking wine, and waiting. The economic costs (and benefits) of childbearing might well be quite similar for men and for women, but it seems likely that the noneconomic costs (and benefits) might be rather different. Moreover, without paying attention to the distribution of power in this society, we would not know who is in charge of reproduction, whose calculations are most salient. Because the social construction of gender is variable across time and culture (Laslett and Brenner 1989), only hubris could lead us to think that an analyst's construction of gender, drawn from observation of a particular slice of the world, can be exported without modification for the local experience of gender.

This point in turn suggests that we would benefit from challenging our intuitions. One way to do this is by doing more ethnographic work—either in the field or in the library—at least at early stages of our investigations, and especially when we conduct research in contexts quite different from our own.[27] We need not believe uncritically what the people we are studying tell us, just as we do not naively believe that respondents always admit to contraceptive failure. And we can treat our revised intuitions and observations in just the same way as we have treated the theories of others: by formulating testable hypotheses, by collecting appropriate data and rigorously analyzing them with statistical techniques, and by carefully evaluating the results.

A more general implication is that we should be more attentive to the fit between gender as it is constructed in the pages of *Demography* and gender as it is constructed in the societies we study. Women may lie about their ages in some times and places but in other societies they may conceal something about which we expect them to tell the truth. If our expectations about gendered behavior sometimes lead us astray in studying a society we know well, where we can take so much for granted, it is plausible that our intuitions and

observations are even less likely to guide us well in studying societies that are unfamiliar, where we can take less for granted.

It would be hard to proceed with research on fertility, marriage and the family unless we shared some understandings of gender. The pages of our journal demonstrate that we believe that the experiences of men and women are constrained by the social construction of gender in the societies in which they live. Even so, we could be more attentive to the ways in which our expectations, and particularly our theoretical frameworks, attach importance to some aspects of women's (and men's) lives but ignore others. Relaxing the reins on our curiosity—stepping outside our local cultures to observe the interactions of men and women—surely can provide us with information that otherwise we might miss, about those whose experience of the social construction of gender may be quite different from our own.

NOTES

[1] To define gender, I follow Joan Scott (1988), who emphasizes the social construction of the relationship between the sexes. "In its most recent usage, 'gender' seems to have first appeared among American feminists who wanted to insist on the fundamentally social quality of distinctions based on sex. The word denoted rejection of the biological determinism implicit in the use of such terms as 'sex' or 'sexual difference.' 'Gender' also stressed the relational aspect of normative definitions of femininity . . . according to this view, women and men were defined in terms of one another, and no understanding of either could be achieved by entirely separate study" (p. 29).

[2] For attempts in *Demography* to solve the difficult mathematical problems inherent in modeling a two-sex population, see Keyfitz (1964); Pollak (1986); Schoen (1981).

[3] Between 1964 and 1992, 28% of the articles in *Demography* had at least one female author, 16% had a female first author, and 11% had a single female author (Teachman, et al., this issue). Over time, the percentage of articles involving at least one female author has increased from 14 percent to 47% (Teachman, et al., this issue). Women constituted about 17% of the membership of the Population Association of America in 1965, 34% in 1984 (Teachman, Paasch, and Carver 1993), and approximately 39% in 1993 (PAA office, personal communication).

[4] In any case, a peer-reviewed journal such as *Demography* (in a field with alternative publications) would not be an appropriate source of data for evaluating that claim.

[5] Lakoff and Johnson (1980) proposed that in the United States, "up" is better than "down." Happy is "up," sad is "down"; someone is at the "peak of health," or "dropped dead"; having control is up ("I am on top of the situation" vs. "He is under my control"); rational is up, emotional is down ("We put our feelings aside and had a high-level intellectual discussion of the matter") (Lakoff and Johnson 1980, pp. 15–17). Also see Nelson (1992), who adds connotations of "superior" and "masculine" to the "up-in-center-control-rational" metaphors, and "inferior" and "feminine" to the "down-out-periphery-submission-emotional" metaphors.

[6] In the conclusions, however, they speculate about patterns of misreporting by age.

[7] This comment is a paraphrase of Barbara Laslett: "As a characteristic of the division of labor, as a source of personal identity and a motive for action, and as an element of institutional cultures, gender is a feature of social structure, of social institutions, and of social actions. It is therefore relevant to historical analysis *even when women are absent* (1992, p. 188; emphasis is Laslett's).

[8] How funders reached this decision would be an interesting research topic; so would the more general topic of the effects of funding priorities on the topics that demographers research (Preston, this issue).

[9] One step toward a clearer understanding of differences between men and women in knowledge, attitudes, and practices in the early stages of the fertility decline would be a reanalysis of the KAP surveys with today's more sophisticated techniques (Hermalin, Entwisle, and Myers 1985).

[10] The targeting of women by family planning programs remains an issue for some feminists. For a review of feminist opinions on population control, see Dixon-Mueller 1993.

[11] Both the World Fertility Survey and the Demographic and Health Survey have included husbands in a few countries. Westoff (1975) notes problems with interviewing only women.

[12] Indeed, sometimes the mother's income is specifically excluded: for example, in an article on the effect of mother's employment outside the home on the children's academic achievement, Desai, Chase-Lansdale, and Michael (1989) operationalize the family's socioeconomic status (SES) with a measure of income that ignores the mother's earnings. For this point I am grateful to Herb Smith's extended (unpublished) critique of this article.

[13] Dennis Hogan, personal communication.

[14] Although it was not reported, there was apparently no effect of grandparents in this study (Barry Popkin, personal communication).

[15] For a cogent critique of the data collected by the WFS, see Caldwell (1985).

[16] Surprisingly often, however, education (and other variables) are not interpreted. Sometimes it is noted that a variable is included because it proved important elsewhere, but at other times variables are included with no justification at all.

[17] In the NHE, this specialization is based explicitly on biological differences in comparative advantages between the sexes (Becker 1981, p. 23).

[18] In the modernization framework it is not clear what makes the jobs outside the home attractive. In the NHE, a job is more attractive if it permits the woman to contribute more to the household economy than she would by specializing in household production.

[19] Nelson (forthcoming) observes that the NHE has been accused of formalizing and reinforcing outdated assumptions about male and female roles.

[20] This is less the case in the early years of *Demography*, when women were more likely to be regarded as apathetic and inert (e.g., Basu 1964).

[21] In *Demography* women rarely are seen actively caring for their husband or partner, although the term *domestic services* presumably includes cooking his dinner in addition to a variety of other more culturally variable benefits such as ironing his shirts.

[22] The goal of this research, done in a biophysics lab at Johns Hopkins University, was to develop a contraceptive that worked topically on sperm. Martin reports two other similar research findings. Examining the language in which this research was published, however, she finds that "Even though each revisionist account gives the egg a larger and more active role, taken together they bring into play another cultural stereotype: woman as dangerous and aggressive threat" (Martin 1991, p. 48).

[23] One apparent result of such critiques has been an interest in models of within-household bargaining (see, for example, Lundberg and Pollak 1992).

[24] The use of ethnographic methods in *Demography* is rare (for exceptions see Axinn 1992; Fricke, Syed, and Smith 1986; van de Walle 1992).

[25] Articles on female labor force participation in general, like articles on men's labor force participation, were coded by Teachman et al. (this issue) under "population characteristics." If female labor force participation had been associated with either fertility or child care, it would have been coded under either "fertility and contraception" or "marriage and the family" (Teachman, personal communication).

[26] By introspection, I mean asking "How would I behave in these circumstances?"

[27] In this regard I echo Hammel's (1990) recommendations for studying culture.

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