

This commentary discusses theoretical and methodological issues that arise in the articles by Brines and Fiorentine. It commends Brines's careful tests of three models of the household division of labor but criticizes some of the principal tenets of the economic model of the household division of labor that Brines tests. It also criticizes a number of Fiorentine's generalizations about the state of the sociological literature on gender stratification although noting that there are intriguing issues to be pursued in the area of the differential attrition of women from male-dominated fields before and after they enter the labor force. The commentary concludes with a call for more rigorous development of sociological theories of gender inequality.

Economic and Sociological Explanations of Gender Inequality

JERRY A. JACOBS

University of Pennsylvania

I accepted the invitation to comment on these two articles in the hopes of identifying common theoretical concerns and research directions in the areas of paid work and housework. Below I discuss each article in turn, along with a few concluding thoughts regarding future avenues of inquiry.

BRINES'S STUDY OF HOUSEWORK

Professor Brines's article is a serious effort to explore the implications of three models of the division of housework while proposing the need to develop a fourth. Although the similarities between the human capital, bargaining, and dependency approaches have been noted, Brines identifies points of difference that allow for tests among them. Among her key findings are the following:

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1. Wives spend 17 hours more per week on housework than did their husbands. Among the sex-role atypical group (husbands working the fewest hours with wives working the most hours) a 10-hour gap remained.
2. The housework/paid work trade-off was much sharper for wives than for husbands. Wives cut housework by about 20-30 minutes for each hour of paid work whereas husbands substituted only a few minutes of housework for each hour of paid work.
3. These trade-offs were generally linear in form, not curvilinear as Brines expects.
4. Curiously, men earning high wages did more chores (after their education, hours worked, and many other factors were controlled), whereas the reverse was true for women.

The results, then, point to several items consistent with all three models and several results that are inconsistent with all three. Brines stresses the latter in criticizing these gender-neutral models of household production that assume the only differences between husbands and wives reflect their efforts in the paid labor force. This conclusion seems hard to dispute with the cross-sectional data at hand. The most straightforward evidence regarding this conclusion is that wives with the same education, hours worked, wages, and labor market experience as their husbands still work more at home. Brines instead points to sex differences in elasticities; that is, how much paid work substitutes for housework for wives and husbands under particular conditions.

Brines's thoughtful analysis is a distinguished example of this genre of scholarship. She scrutinizes the assumptions of each model and, in some cases, extends the underlying reasoning. Her data analysis represents a creative effort to test the implications of these models. Yet I have a number of reservations with the genre itself. Assumptions of the various theories are delineated without critical comment. Although this is undoubtedly the right approach when setting the stage for an empirical test, I feel uncomfortable with the unquestioning repetition of unrealistic assumptions. Let me cite three examples.

Brines deftly summarizes Becker's (1991) arguments regarding investments in marketable skills versus household skills. Yet is "investment" in household skills really the right metaphor? The plausibility of viewing formal schooling decisions as investments rests heavily on the notion of opportunity costs: When one is in school, one forgoes earning opportunities, not to mention the direct cost of tuition. Yet household skills can be learned in the course of everyday routines without requiring major "investment" decisions in organized study. In other words, perhaps housework is more analogous to "on-the-job training" than to educational investments.

The abstract language of comparative advantage obscures the simple nature of many of the tasks at issue. How long does it take to learn how to wash dishes? To use a washing machine? A clothes dryer? To make up a bed? (Recall that the article is about housework, not child care, some facets of which arguably require more skill. But even with child care, many basics, like changing a diaper, are not college-level subjects.) Hill's 1985 data (cited by Coverman 1989) indicate that wives spent 2 more hours per week washing dishes, 2 more hours doing laundry, and 4 more hours cleaning than their husbands did. The gender gap in housework would be reduced by about half if men shared these simple tasks and would be eliminated completely if husbands assumed full responsibilities in these areas, in exchange for wives' greater responsibilities for cooking and child care. I submit that research could determine whether men know how to perform these tasks and whether lack of knowledge or perceived lack of facility with these tasks inhibits men's performance of them. Indeed, some evidence presented by Brines and others suggest that men assume some of these responsibilities when there are small children at home, only to forgo them as the children grow older. This evidence would suggest that men know how these tasks are performed, or can easily learn them as needed. Thus the pertinence of the notion of "investments in skill" to most of the sex gap in housework (as opposed to child care) is arguable on face terms.

A second example is the assumed "complimentarity" between child care and housework. Brines notes assumed synergies between childbearing and child care, but the synergies between child care and housework remain unspecified. My own view is that the key overlap is time at home. For example, one can easily start a load of laundry when a child is taking a nap. But for couples who work similar hours, I see no reason to believe that the spouse who specializes in child care would be in a better position to perform other housework. In this case, hours at home are equally available to husbands and wives for housework; if anything, husbands would likely have more time for housework if wives are spending more time attending to the children. Here again, the applicability of economic reasoning warrants closer scrutiny.

Third, a more general objection concerns the application of economic analysis to family behavior. The analogy between families and firms breaks down in certain key areas. In particular, it is unrealistic to expect families to exhibit optimizing behavior. One crucial difference is that firms are under pressure to maximize their productivity, whereas families are not. Although some might expect marriage markets to create pressure on families similar to the pressure of labor markets or commodities markets, this analogy does

not hold. Marriage markets are not cleared by a price mechanism. They are more like queues than bazaars and are highly structured by social institutions like networks and social classes. In any event, entry into and exits from marriage are costly, so that married individuals are not directly pressured to conform to the example of other marriages (a point that Brines notes in a connection with the "bargaining model"). Thus only the dubious assumptions regarding self-interested utility maximization are in place to lead families to behave as predicted by human capital theory.

These caveats are not meant to discount the importance of economic behavior in marriages. They are meant to reinforce Block's (1990) warning about overextending the analogy between market and nonmarket behavior. Indeed, Block makes the point that even labor markets resemble ideal spot markets less than is often assumed.

Brines concludes by proposing symbolic exchange as a factor in the distribution of housework. In so doing, she suggests that gender itself is fundamental to the process of dividing housework. This suggestion parallels Acker's (1990) argument that gender is inextricably related to the essential features of organizations and work. I have two main reactions to this conclusion. First, the evidence she cites for this conclusion is the deviation of the findings from economic predictions. Although Brines's findings certainly suggest that noneconomic considerations are at work, I wonder whether it may eventually be possible to develop more direct evidence for the symbolic exchange approach. Second, the analysis of symbolic exchange is not developed as rigorously as the economic analysis. Consequently, Brines's intriguing suggestions along these lines would require greater specification in order to stimulate further research. Again, this is not so much a complaint against Brines as it is a common pattern in sociological research. It seems typical of current sociological research in this area to pay much more attention to showing deficiencies of the economic analysis than to developing precise specifications of alternatives. We continually demonstrate the limits of economic models (not that economists pay much attention) and suggest the need for the development of sociological alternatives. Yet study after study returns to demonstrate the limitations of economic models while devoting less attention to advancing alternatives. Moreover, nationally representative survey data sets often lack measures of key sociological concepts. Thus, even if she tried, Brines would be hard-pressed to locate measures of symbolic exchange in the data she analyzed. Developing sociological insights in this area will require more specific theoretical development along with gathering original sociological data.

FIorentine's REVIEW OF GENDER STRATIFICATION

I must confess that I found Fiorentine's article disappointing. I discuss in turn Fiorentine's review of the literature, several theoretical and methodological issues he raises, and Fiorentine's own theory, the "normative alternatives" hypothesis.

Fiorentine's review of the vast literature on sex differences in employment is replete with flawed assertions and does not effectively highlight key contributions. I have five specific complaints. First, the frame-setting assertion of his article is completely unfounded. In the first paragraph, Fiorentine claims that "there has been no attempt to contrast" sociological, economic, and psychological theories of gender stratification. How can anyone make this claim? Virtually every significant sociological contribution in this area considers potential gender differences in economic motives and preferences in the course of demonstrating the limitations of these approaches. I submit that specific research reports (Bielby and Baron 1986; England, Farkas, Kilbourne, and Dou 1988; Strober 1984), among many others, and reviews (England and Farkas 1986; Marini 1989; Reskin 1984; Reskin and Hartmann 1986) belie this assertion. (These references, along with Fiorentine and Cole 1992, cited below, are all found in Fiorentine's bibliography.)

Second, the review of the literature in the area of "structural barriers" is particularly spotty. Fiorentine attempts to impartially characterize a wide range of studies by labeling the results "mixed." He does this by lumping together a range of different approaches and selectively citing dated research. Does this really advance the debate? I must note that of the 54 references cited in connection with the "structural barriers" approach, 32 are from the 1970s and only 7 date from 1986 to the present. One would get the impression from Fiorentine's review that this was a hot research area in the 1970s that has since cooled down. Yet the reverse is true.

Third, Fiorentine's review is oddly ahistorical. It juxtaposes extremely active research areas with others that have lapsed. For example, the structural functional approach was ascendant in the 1950s (see, e.g., Friedan's 1963 comments on Parsons), whereas the fear-of-success (Fiorentine's "cognitive" category) hypothesis drew much public attention but was never intellectually ascendant even in its heyday in the 1970s. This amalgam obscures the history of the field by failing to acknowledge how later approaches displaced earlier ones. By treating all views equally, it gives no clear sense of which areas are closer to the "cutting edge" of contemporary research. Moreover, Fiorentine does not take history into account in reconciling discrepant outcomes. Thus, in discussing research on the social psychology of attributions, Fiorentine cites contradictory findings from the 1960s and the 1980s without consider-

ing the possibility that changes in behavior in the interim may explain these disparities.

Fourth, I find it odd that Fiorentine treats human capital theory as a "cultural" theory. As Brines makes clear, Becker (1991) maintains that women "invest" more in household skills than in labor market skills in order to maximize household productivity, not as a result of norms about gender roles.

Last, a sustained discussion of some of the leading contributions to gender stratification would be much more rewarding than his article. In my view, some of the most insightful and provocative studies in this area in recent years have been Barbara Bergmann's (1986) powerful synthesis of economic approaches to gender inequality, including an impressive review of evidence on discrimination against women; Reskin and Roos's (1990) exemplary study of occupational resegregation; Goldin's (1990) remarkable historical analysis of changes in women's economic position; and Baron and Bielby's outstanding series of articles on gender and the structure of jobs. Some comparative work has also been thought provoking (e.g., Brinton 1988; Rosenfeld and Kalleberg 1990). It is significant to me that none of these seminal works is seriously discussed here—indeed most are not even cited! Thus, in my view, the remarkable work that has made gender stratification one of the most active and productive research frontiers in sociology in recent years is either missing or is not seriously examined. Somehow, in Fiorentine's review, all recent progress is rather summarily dismissed as producing "mixed results." I would love to see a thoughtful assessment of the current state of the literature and a serious attempt to integrate the insights of these and other notable recent studies. Unfortunately, his article does not fit the bill. Fortunately, an attractive alternative will soon be available (Reskin forthcoming).

Let us now turn to issues of theory and methods. Fiorentine organizes much of the discussion around the distinction between structure and agency, yet this dichotomy is overdrawn. Sociologists at least since Goffman (1952) have recognized that what is taken for individual choices is often the result of subtle social control processes. Clark (1960), for example, showed how guidance counselors at community colleges were able to convince students that they were not likely to succeed in transferring to a 4-year college. The "cooled out" student blames himself and considers the outcome the result of his own choices. The process obscures the role of the community college, and the policies which limited the number of slots for transfer students, in the decision-making process. In my book (Jacobs 1989a), I suggested that similar processes may well contribute to the maintenance of sex segregation throughout the life course. In contrast to most economists, who assume that

all pre-labor-market decisions reflect individual choices and tastes, I have argued that the social controls individuals encounter before entering the labor market may resemble those experienced by men and women at work.

Moreover, it should be noted that Fiorentine's discussion of agency focuses just on women. Where are the men in all of this, as Reskin (1988) wondered? A related point is that Fiorentine follows a well-worn tradition of focusing on women's deficits, whether they be ability, ambition, or effort. Yet, as Bergmann (1986) points out, men bring various liabilities with them to the workplace: Men are more likely than women to abuse drugs and alcohol, crash motor vehicles, engage in theft or embezzlement, and attempt to unionize (surely a liability from management's point of view). Would not even an evenhanded assessment of occupational inequality assess men's strengths and weaknesses along with women's?

Fiorentine makes a number of methodological claims that are either commonplace or wrong. For example, he asserts that "whereas ambition and ability can be measured directly, opportunity cannot" (p. 345). I find this an odd claim because ambition and ability are notoriously difficult to measure. (We sometimes have measures of aspirations and performance, which of course are quite different.) Moreover, it is wrong to elevate this lacuna to a methodological principle because there is occasionally direct evidence of discrimination. Goldin (1990) reports that "on the eve of American entry into World War II, fully 87 percent of school districts would not hire a married woman and 70 percent would not retain a single woman who married" (p. 162). She presents additional evidence, based on a survey of 328 firms in three large cities in 1940, indicating that more than 50% of women worked in firms that either barred employment of married women or allowed for their dismissal. In a simple but convincing study conducted in 1974, Levinson (1982) directed a team of men and women to call employers in response to classified job advertisements. In about one third of the cases, Levinson concluded there was "clear cut" evidence of discrimination: Employers refused to interview a sex-inappropriate applicant just minutes after agreeing to interview a sex-appropriate applicant. In more than one in four of the remaining cases, there was ambiguous evidence of discrimination. More contemporary evidence culled from court cases is compiled by Bergmann (1986), who notes, "We do not have to depend solely on indirect statistical evidence" in order to conclude that opportunities for women are blocked (p. 83).

It is true that discrimination is often measured indirectly as the residual that remains after productivity-related measures are controlled. Fiorentine points out that this approach would overstate discrimination if the analysis incompletely controlled for differences in ability or ambition. He also points out that discrimination may be understood because "lower levels of human

capital, as well as other aspects of ambition and ability, are, to some extent, consequences of blocked opportunity” (p. 346). Both points are correct, and both points have been made innumerable times before.

Fiorentine further claims that evidence against supply-side explanations should not be construed as support for demand-side explanations. Why not? One also wonders why the intellectual burden of proof should be on discrimination explanations: Should we assume the world is perfectly fair and efficient in the absence of definitive evidence to the contrary?

On pages 345-346 Fiorentine claims that

sex segregation research presents frequently detailed data on the sex composition of an occupation with almost no attention given to the possible causes of these outcomes. . . . Sex segregation research consistently confuses descriptions of occupational outcomes with measures of a process that could conceivably, but not invariably, lead to these outcomes. Confusing outcomes for a process imposes a single-variable causal model in which opportunity completely, and invariably, explains why women are underrepresented in some occupations and overrepresented in others.

Later on, Fiorentine acknowledges that several studies (England 1982; Corcoran et al. 1984; England and Farkas 1986) have attempted to explain occupational sex segregation and have shown that a human capital model of lifetime income maximization could not account for women’s choice of female-dominated fields. It seems to me that this point directly undercuts his earlier assertion to the contrary.

Because my book (Jacobs 1989a) is cited in connection with Fiorentine’s claim, I must protest that this conclusion fundamentally mischaracterizes my work. First of all, I explicitly argue that career aspirations, educational decisions, and market discrimination work together to produce occupational sex segregation. The “single-variable causal model” charge is either a profound misreading of my work or a careless generalization to research not read. Second, trends in segregation by themselves do not prove anything: They are data that need to be explained. In a study of long-term trends in segregation (Jacobs 1989b), I presented evidence that occupational sex segregation remained roughly constant in the United States between 1900 and 1970, after which time it declined slowly but steadily throughout the 1970s and 1980s. I then noted that “these results do not constitute definite support for any theory of sex segregation” (p. 171). In my book, I tracked the decline in sex segregation in higher education, in career aspirations, and in the workplace. The descriptions of change constitute the minority of the analysis and the search for explanations the preponderance of the study. For example, I maintained that sex role socialization by itself is insufficient to

account for the long-term persistence in sex segregation because there is only a weak link between aspirations and outcomes. In other words, young women initially aspiring to female-dominated occupations were only slightly more likely to maintain such preferences 5 to 10 years later and only slightly more likely to be employed in female-dominated fields. One may disagree with my methods or my conclusions, but one cannot credibly argue that I give "no attention to the possible causes of these outcomes" (p. 345).

Students of gender inequality sometimes do assume that the gender gap in social rewards is illegitimate, but they do so not because of some "logical fallacy" but rather because they assume that women would be as able, ambitious, and successful as men given equal opportunities and social support. Fiorentine's claim regarding the confusion of outcome with process attempts to create a methodological principle out of a disagreement over initial assumptions.

After Fiorentine has impaled competing theories on several methodological petards, a reader might assume that his own research would be well protected against similar thrusts. What is the evidence for the normative alternatives approach? Women premedical students at SUNY Stony Brook during the 1980s were less likely to enter medical school than were their male counterparts. Fiorentine discounts the effects of grades, perceived discrimination, and expected conflict between career and family as explanations of this gender gap, based on evidence derived from retrospective surveys. In short, the "evidence" for the normative alternatives approach is that direct tests of alternative hypotheses are not supported. Does this sound familiar? Why is Fiorentine so critical of such indirect evidence when it suggests the presence of discrimination, and so willing to credit it when it supports his theory? Indeed, the only direct evidence on social support presented by Fiorentine and Cole (1992) suggests that both male and female undergraduates received substantial encouragement to pursue their careers, which hardly suggests that norms regarding the salience of careers differed by gender.

Although I am not persuaded that the "normative alternatives" explanation is correct, I nonetheless think that the differential attrition of women from premedical programs poses an interesting puzzle. I am doubly intrigued because a dissertation I supervised on engineering students was similarly unable to explain the higher attrition rates of female students with reference to grades or social support (Adams 1988). Yet this case study of one field of study at one college during one period needs to be understood in a larger context. The process of sex-typing of career choices begins well before college, and college freshmen bring with them sex-stereotyped choices of major and career already in mind. Thus, although preferences to enter medicine apparently were not skewed by gender at Stony Brook in the cohort

that Fiorentine studied, male freshmen remain slightly more likely to be pre-med students than were their female counterparts at most schools. Indeed, in 1989, freshmen's intended majors were sufficiently sex-stereotyped that about 35% of women would have had to change fields in order to be distributed in the same manner as men (measured across 43 occupational choices). Similarly, 45% of freshmen women would have had to change their intended majors in order to be distributed in the same manner as men (measured across 80 fields of study). (These calculations are based on an analysis of data reported in *The American Freshman: National Norms* 1989, 17-19, 33-35.) Second, changes in career goals involve women's entry into male-dominated fields, along with the attrition that Fiorentine focuses on. Indeed, the former has predominated over the past two decades (Jacobs 1989a). As a result, senior degree recipients are less segregated by field than they were as entering freshmen (measured in terms of their intended majors). Fiorentine's focus on attrition in women from male-dominated fields might lead one to assume that sex segregation increased during college. Finally, gender inequality does not end with the selection of a professional degree program. For example, medical specialties remain segregated by gender and women physicians earn less than their male counterparts. More attention to the dynamic processes that reproduce gender inequality at each stage of the life course would be welcome.

CONCLUDING THOUGHTS

Reflecting on these two articles leads me to three general points. First, gender roles are changing in our society and elsewhere, yet much gender inequality remains. Sociologists need to develop more specific theoretical predictions in order to account for both stability and change. Otherwise, we will be "testing" economic hypotheses for many years into the future and continuing to speculate on the significance of the differences between the data and these predictions. For example, sociologists of the labor market need to do more than insist on the existence of discrimination, namely, to specify where we can expect it to be most virulent and to identify the conditions that might lead to its attenuation. Similarly, sociological studies of the gender gap in housework need to account not only for the existence of gender inequality but also to specify why it is lower in some groups than in others and to indicate what factors might precipitate its decline. Might we attempt to develop a theory of values and preferences, so that these considerations would lose their post hoc quality and become salient theories worthy of direct measures and tests? In each area, such theorizing will not be easy because of multiple and

complex relationships between economic, political, institutional, and cultural factors, but proceed we must.

Second, we need more longitudinal and cross-cultural studies. I increasingly find analyses of individual and historical change more persuasive than cross-sectional analyses. An analysis of the trends in the housework gap in a number of different countries would be most interesting. Similarly, the repeated analysis of men's and women's entry into and exit from sex-typed fields over several decades would give us insight into the changing dynamics of gender inequality in the labor force.

Finally, sociological analyses need to connect individual motives and actions with larger social structures. If we seek to explain individual behavior, we must remember that individuals respond in part to available options that are structured by dominant institutions. Whether or not these structures can in turn be explained by individual choices, they need to be a part of the story of individual explanation. The interrelationships between individuals' actions and social, political, cultural, and economic arrangements also require that attention be given to historical context. I suspect that cross-cultural analyses would be especially likely to bring these factors into sharper relief.

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