

REASSESSING THE UNEVEN GENDER REVOLUTION AND ITS SLOWDOWN

PAULA ENGLAND
Stanford University

In my recent status report on the gender revolution in *Gender & Society* (England 2010), I argued that change in the gender system has been asymmetric, with women changing more than men. I attributed this to the devaluation of and low rewards offered to those in roles associated with women, giving women an incentive to leave them (even when they find them meaningful) and men little incentive to enter. For example, large numbers of women jettisoned full-time homemaking to enter paid employment, and many entered “male” occupations and fields of study. In support of the unevenness of the gender revolution by class, I pointed to higher employment rates of college-educated than other women and the fact that women have entered male-dominated professions and managerial jobs much more than the largely male blue-collar trades. In this issue, several able critics comment on my piece but do not dispute these points.

My critics take issue with two parts of my account. Some believe that I understated continuing progress toward gender equality. Evoking the most criticism was my supply-side hypothesis about why few women integrated male-dominated blue-collar jobs, while by contrast, male-dominated professional and managerial fields were substantially integrated. While considerations of space prohibit a response to all of the points made by my critics, I respond to some of their main critiques below. I also suggest types of research I believe we need to better understand these issues.

IS THE GENDER REVOLUTION STALLED IN THE PAID WORKPLACE?

I argued that the gender revolution has stalled, at least in the United States. I meant that progress on a number of fronts has either slowed or stopped. Some of the points I made in support of this were not disputed by my critics.

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For example, the sex segregation of college majors decreased between 1970 and the mid-1980s but has not budged since. As another case in point, the proportion of women who are employed grew dramatically for decades but then largely stabilized after 1990, even turning down slightly after 2000 before turning back up again. While the term used in the press for this—an “opt out revolution”—was a gross exaggeration of a tiny dip in employment, arguing against media exaggeration should not blind us to the reality that women’s employment in the population as a whole has increased little since 1990. An interesting exception that I did not discuss is that single mothers did increase their employment in the 1990s (Hoffman 2009).

I also mentioned that the pace of occupational desegregation slowed after 1990. Reskin and Maroto point out that trends in segregation vary across data source and subpopulation. For example, they point out that desegregation among those younger than 25 years of age continued in dramatic fashion past 1990. While I do not dispute any of their specific sources or facts, I continue to believe that the preponderance of the evidence suggests that a slowdown of occupational desegregation has occurred since 1990. For example, a briefing paper issued since my 2010 article by the Institute for Women’s Policy Research (Hegewisch et al. 2010) uses the index of dissimilarity to examine trends in sex segregation of occupations from 1972 to 2009 for all workers of age 25 to 64. They find dramatic declines in segregation until 1990, much slower declines from 1990 to 2000, and virtually no decline from 2000 to 2009 (Hegewisch et al. 2010, 5). Among those without a college education there has been virtually no decline since 1990 (Hegewisch et al. 2010, 5). The slowing of the decline from 1990 to 2009 occurred for all age groups considered (25-34, 35-44, 45-54, 55-64), and the cessation of desegregation after 2000 occurred for all ages except those 55 to 64 (Hegewisch et al. 2010, 6). Moreover, this virtual flattening of the trend in sex segregation occurred within whites, Blacks, and Latinos. Only Asians experienced substantial desegregation continuing after 1990. In an analysis using another kind of data, “job” categories that specify workers’ employers, Tomaskovic-Devey et al. (2006) also find that the rate of desegregation by sex has slowed; segregation declined at an average pace of 1.8 percent per year between 1966 and 1980 but at a slower pace of 1.5 percent per year between 1980 and 2003.

I mentioned that progress in closing the gap between men’s and women’s median earnings has slowed as well (England 2010, 163). But McCall points out that despite this, the proportion of women who earn incomes in the top 10 percent of the individual earnings distribution has continued to grow unabated. McCall’s amendment to what I said reminds us that central tendency is not the only relevant metric for assessing progress.

WHAT IS THE NATURE OF THE GENDER REVOLUTION IN HETEROSEXUAL UNIONS?

Graf and Schwartz argue that I understated the change in heterosexual relationships. I pointed out how little change has occurred in the expectation that men propose marriage, wives take their husbands' surnames, and men are taller than their female partners. Moreover, the double standard of sexuality persists; women are judged more harshly than men for nonrelational sex. And the double standard of aging continues; older men feel entitled to younger women partners. Graf and Schwartz grant this as well as my hypothesis that the persistence of these interpersonal inequalities may be because incentives for change are less in these matters than in the world of jobs and income.

I concluded that there has been much more change in the world of paid work than in personal, heterosexual relationships, but Graf and Schwartz make the compelling argument that change in women's employment, occupations, and earnings spills over into personal relationships for the vast majority of women who marry men. They argue that marriages or cohabitations must necessarily be transformed as more men are partnered with employed women and with women whose education and earnings approximate or even exceed their own. As they say, I gave too short shrift to this point. I agree that it is not just that men are married to women who are more their economic equals than previously but also that men's and women's attitudes toward the propriety of these patterns have shifted. Perhaps attitudes followed behavior, as they often do, but it is a big change when few people think it remarkable or stigmatizing that spouses have similar earnings. Men accept, often even expect, that women will be earners too. There is little evidence that it is hurting women in the heterosexual marriage market to have serious careers, and this is, as Graf and Schwartz say, a change in the family as well as in paid employment. We all agree, however, that a limitation of this change is scattered evidence that men are uncomfortable when women's achievements exceed their own.

One part of Graf and Schwartz's argument that I would put in "maybe" rather than "confirmed" status is their claim that women's more equal earnings lead to more equal power in their relationships with men. This may seem an odd critique for me to make as I am, myself, in print several places saying exactly this—that relative earnings affect relative power in male-female relationships. Exchange theory in sociology, game theory-based "bargaining" theories in economics, and folk wisdom all suggest that money talks in relationships. There is indeed evidence that when women earn more, at least up to a point, it lowers their housework burden, which suggests that money yields power in male-female relationships (Bittman et al. 2003; Gupta 2007).

But other evidence casts doubt on whether women translate earnings into power in marriage. Individual women's earnings do not translate into much increase in their male partners' housework (Bittman et al. 2003; Evertsson and Neramo 2004). This is not to deny that men's housework has increased in recent decades (Bianchi, Robinson, and Milkie 2006). But the increase is as large in homemaker/breadwinner families as in those where women are employed, and the reduction in women's housework is even greater in homemaker/breadwinner families than in those where women are employed (Sayer et al. 2009, 542). This suggests a more generalized cultural change rather than money-based power dynamics in individual families.

Moreover, we have little evidence about whether who earns the money affects power in relationships on matters other than housework. One reason housework has been used to get at power is that the tacit assumption that most people would prefer to do less of it seems reasonable. By contrast, with other outcomes measured on surveys (e.g., relocation, major purchases, activities spouses do together, what sexual practices are engaged in and how often), it is harder to know which partner is getting more of what he or she wants and thus harder to surmise who has more power. Tichenor (2005) has provided rich qualitative analysis of power within marriages where women earn more, suggesting that gendered norms, displays, and identities get in the way of women's earnings translating into equal (let alone greater) power. We need more creative qualitative and quantitative work on the question of how and under what circumstances money gives partners power in couple relationships.

DOES GENDER SOCIALIZATION CREATE A SUPPLY SIDE TO OCCUPATIONAL SEX SEGREGATION?

Introducing a volume showcasing new work on culture and poverty, Small, Harding, and Lamont (2010, 10) note that cultural explanations of poverty have been the "third rail" of American sociology, avoided because they were seen to blame the victim. Among sociologists of gender, a similar aversion exists to explanations that assert a causal role for socialized preferences on the supply side of labor markets. I respectfully disagree; I believe that continuous gendered socialization affects taken-for-granted assumptions (e.g., which jobs we even consider), identities, and preferences. Outside social forces change our insides. Rather than eschewing socialization explanations in fear that they will be used to blame the victim, I believe we should point out that people did not choose the constraining social forces that formed their preferences, identities, and assumptions (Browne and England 1997)

and that even if they chose their jobs, they were not always aware of and certainly do not prefer the low pay in those jobs.

Because of my broad view of the social forces affecting our gendered responses, I disagree with Crawley's apparent adoption of the "doing gender" perspective as the whole microsociological story. I agree that gender is sometimes "done" because we feel accountable to make sense to others, as argued in the ethnomethodological view. But gender is also sometimes internalized into preferences or taken-for-granted assumptions about courses of action. Much of the evidence adduced to support the doing gender view is also consistent with other theories of gender. This too is an area where we need creative new research with designs allowing us to tell whether gendered behavior is motivated by a need to be accountable to others to make sense to them, by constraints and incentives, or by internalized preferences stemming from previous social conditions.

Given the views I have just detailed, I have long disagreed with the common aversion to socialization-based explanations of gender inequality among sociologists of gender. Much of my earlier work focused on demand-side discrimination, focused on the underpayment of female-dominated relative to male-dominated jobs, and argued against one particular supply-side answer to occupational sex segregation—that women choose jobs that penalize interruptions least. Despite this, I have long believed that the jobs in which men and women end up reflect both gendered preferences on the supply side and discrimination in hiring and placement on the demand side (England and Folbre 2005). The particular supply-side story I offered in my 2010 piece was not meant to deny that there are both supply and demand aspects to gendered outcomes in labor markets.

WHY HAVE FEWER WOMEN ENTERED MALE BLUE-COLLAR TRADES THAN PROFESSIONS AND MANAGEMENT?

In my 2010 article, I was interested in how uneven the gender revolution has been by class, and I thus took up the question of why women integrated professional and managerial jobs to a much greater extent than blue-collar jobs. I suggested an admittedly oversimplified model of occupational choice: People want to move up relative to a reference group of members of their class and gender in the previous generation, and there is a strong tendency to choose jobs traditional for one's sex. I argued that it is when these things conflict—that one cannot move up without transgressing the gender system—that desegregation is most likely to occur. In this view, college-educated

women integrated the male professions (medicine, law, academia) and management because to move up *vis-à-vis* their class-and-gender-based reference group from the generation of women before them, they had to enter male jobs. This is because college-educated women in the previous generation, if employed, were already in the highest-status, most-lucrative, predominantly female jobs such as teaching, nursing, and social work. By contrast, working-class women have the option to move up *vis-à-vis* women in their reference group in the previous generation by getting more education and moving, for example, into largely female-dominated jobs such as nursing and teaching. None of my critics dispute the point that blue-collar male jobs have been slow to integrate, but several dispute what I said about why.

In their critique of my supply-side argument, Reskin and Maroto review evidence that employer practices matter in which jobs women get. I grant their point that employer's practices matter mightily. But they offer no explanation of the question I posed—why there has been so little desegregation in male blue-collar jobs relative to the substantial entry of women into professional and managerial jobs. They object to occupations I classified as working or middle class in the graph I used to show that desegregation was much steeper in middle-class jobs. But as I stated (p. 157), if instead we simply use individuals' education as an indicator of class, we also find dramatically more desegregation for college graduates than others (Cotter, Hermsen, and Vanneman 2004, 12-14).

Bergmann's critique is more helpful in that it offers a plausible reason for why it was harder for women to integrate male blue-collar trades. Taking me to task for relegating to a footnote the possibility that there may be more discrimination in blue-collar jobs, Bergmann explains why she believes this is so. In professional and managerial jobs, she argues, one is trained largely by getting college or professional school degrees. When women hold these degrees, it is relatively difficult to argue that they are not qualified for entry-level jobs. In the blue-collar crafts, in contrast, training is often obtained on the job itself, so women have to get the jobs to get training. Even worse, women often have to receive on-the-job training from male workers who resent their presence. Bergmann argues that employers often acquiesce to male worker resentment and do not hire women.

Crawley's piece provides another clue to why women are less able to enter blue-collar than other male-dominated occupations. Crawley locates blue-collar men's resistance to women in their jobs in the fact that the body is a site for masculinity construction. I agree. Since being strong and able to do hard labor is of symbolic importance to men, she argues that those in jobs wherein masculinity can be displayed this way are especially loathe

to admit women. How can ability to do the job denote masculinity if women can do the job? Thus are men strongly motivated to keep women out of “their” blue-collar jobs.

While I have no doubt that discrimination in hiring and placement exists, as these authors assert, I am struck by how little relatively direct evidence we have on the prevalence of hiring discrimination. One possible form of relevant evidence would be quantitative analyses of outcomes for job applicants, to see whether comparably qualified men and women are treated the same. Sometimes defendants are required to provide such data for analysis in lawsuits, but outside of lawsuits, employers will typically not give such data to sociologists (except occasionally those in business schools), so there are few opportunities to use this strategy. I recommend two methods for future research. One approach is the audit study; this approach was used by Pager (2007) to examine racial discrimination and by Correll, Benard, and Paik (2007) to see whether applicants to high-level jobs advertised in a newspaper were treated differently depending on whether their resumes revealed them to be mothers, women without children, fathers, or men without children. To apply this method to gender, pairs of confederates posing as real applicants, one male and one female, would apply for jobs, with virtually identical resumes, applications, or answers to interview questions. This would illuminate the degree of sex discrimination in hiring in various types of jobs. Another approach I recommend is qualitative-interview or ethnographic studies carefully designed to unearth discriminatory actions by male coworkers and employers; such studies could compare the treatment received by men and women in the same work setting while also uncovering the meanings actors give to their behaviors.

McCall’s critique of my account was not that it contained a supply side view; she too believes that gendered preferences affect agency. Her general critique is with my lack of a model in which gender, class, and race all affect outcomes. Her specific critique of my explanation of the limited desegregation of blue-collar jobs is that it is inconsistent with research on intergenerational mobility. I argued that women all seek upward mobility and that they are more likely to seek entrance into male jobs if their reference group—women in their class in the previous generation or cohort, such as their mothers—were already in the highest-ranking female jobs. She says that if this were the case, intergenerational mobility tables would show that fathers’ characteristics add no information in predicting daughters’ outcomes once we know mothers’ characteristics. But she points to a recent study by Beller (2009) showing that both parents’ characteristics matter. I agree with Beller and McCall that cumulative class resources from either parent

undoubtedly affect the class category to which one aspires, whether one gets a college education, and network connections, all of which make it more likely one will end up in professional or managerial jobs. I am less sure than McCall how relevant Beller's findings are to predicting the sex composition of the occupations in which women end up, given that Beller's dependent variable comprises class categories that often contain a mix of male- and female-dominated occupations.

McCall offers yet another possible demand-side explanation (one that I had buried in a footnote)—that working-class women did not enter blue-collar jobs because those jobs were shrinking. By contrast, managerial and professional jobs have expanded. The structural constraint of occupational demand may be binding in yet another way: Given dramatic growth in women's educational attainment, it might have been impossible for all the new female college graduates to find employment in traditionally female professions (teaching, nursing, social work), and this may have "forced" some movement of women into male professions, not to achieve upward mobility, as I argued, but to avoid downward class mobility.

In retrospect, I see that my account was one-sided in emphasizing a supply-side argument over demand-side factors when the former had no more evidence favoring it than the latter. Given the existence of these plausible demand-side explanations, is there, then, any reason to retain my supply-side hypothesis? I believe so. It would be fruitful for both sets of hypotheses to inform future research. While I provided no direct evidence for my assertion that the extent to which women aspire to male jobs varies by class, I present some here.

NEW EVIDENCE ON CLASS DIFFERENCES IN GENDERED ASPIRATIONS

Figure 1 shows what young women respondents to the National Longitudinal Survey of Youth reported in 1979, the first wave of the survey, when asked about the occupation in which they would like to work at age 35. These young women were 14 to 21 years old when answering the question. To minimize the extent to which they had already experienced discrimination by employers and thereby had changed their aspirations, I limited the analysis to those 14 to 19 years of age, although results are very similar for the whole sample. To see if my generalization fits both Black and white women, I provide separate statistics by race. Figure 1 shows what proportion of young women, queried in 1979, aspired to male jobs, defined as occupations that were at least 67 percent male in 1970. These proportions are given separately

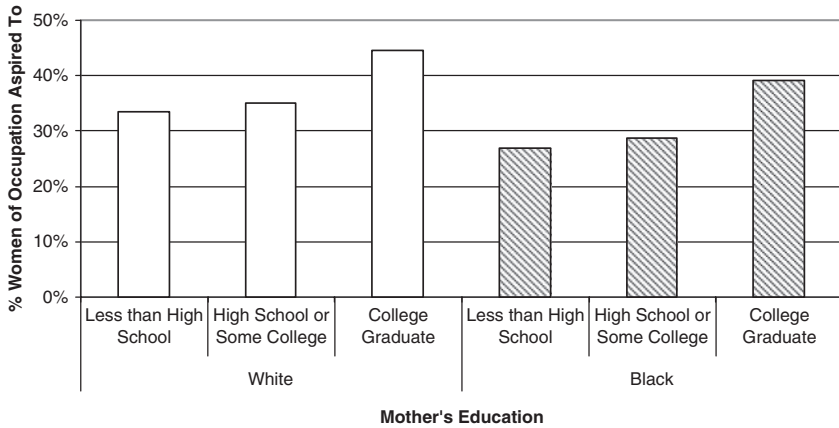


Figure 1: Percentage of Young Women Aspiring to “Male” Occupations, by Their Mother’s Education, for White and Black Women Younger than 20

SOURCE: Author’s computation from the 1979 wave of the National Longitudinal Survey of Youth.

NOTE: For each level of respondent’s mother’s education, the graph provides the regression-predicted percentage of young women aspiring to male occupations (at least 67 percent male in the 1970 census) from a multinomial logistic regression model also controlling for respondent’s age and the Duncan Socioeconomic Index score for the occupation. Results are similar if the Duncan Socioeconomic Index score is excluded from the model.

by the education of the respondents’ mothers (denoting class background). Both white and Black women are more likely to aspire to male occupations if their mothers had more education, with more gender-traditional choices among those whose mothers had less education. In results not shown, I looked also at the average “percent female” of the occupations to which the respondents aspired, and here too we see that the higher one’s mother’s education, the lower is the percent female of the occupation to which the respondent aspired. These results are consistent with the thesis I offered, although they do not prove that the mechanism was the one I suggested—that gender boundaries are more apt to be transgressed if this is necessary to achieve upward mobility by class.

CONCLUDING THOUGHTS

I’ve tried here to answer specific criticisms of my 2010 article. Let me end by highlighting two more general conclusions I draw from my exchange

with my critics. First, I realize that my previous tacitly held view was that gender-equalizing change, once started, proceeds relatively evenly across groups and inexorably over time. That was naïve. My current view sees unevenness across groups to be a function, in part, of different incentives across social locations. I also see more clearly the powerful path dependence that creates inertia around institutions and ideas, closing off certain kinds of change. Second, this debate reminds me what a contentious issue gendered preferences remains for sociologists. The average person believes that gender differences in preferences are huge and explain many inequalities, so it is accurate and strategically appropriate for gender scholars to point out that gender differences are smaller than most people believe them to be and that many other forces are at work producing inequality. Nonetheless, I believe that it serves neither our role as scholars nor our role as change-promoting feminists to ignore how much women's and men's preferences are affected by the gender order and are part of its perpetuation.

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Paula England is a professor of sociology, and an affiliate of the Clayman Institute for Gender Research, at Stanford University. Her current projects include studying how gender structures college sexual and romantic relationships and the role of class differences in unintended pregnancies and family patterns.