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EMPIRICAL CONSEQUENCES OF COMPARABLE WORTH

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ALTHOUGH some efforts to implement comparable worth have taken place in the private sector, the major push has occurred in state and local governments, a sector of the economy in which union membership is growing and a large proportion of the employees are women. Starting with a 1974 state of Washington study, several states have undertaken formal job evaluation studies to see how their compensation systems mesh with the principle of comparable worth, and several state and local governments have begun to implement comparable worth either through the legislative or collective bargaining process (see Ehrenberg and Smith 1987a, tables 10.1 and 10.2).

Although proponents and opponents of comparable worth continue to debate the legitimacy of the concept, to some extent events have passed them by. Protestations of economists to the contrary, the concept of comparable worth has become widely accepted in the public sector of some states, raising the policy question of whether the concept should be extended to other public employees in the state and local sectors and to the federal and private sectors. While debate on this issue will undoubtedly continue to be both emotionally charged and politically motivated, rational decision making must include an

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evaluation of what the empirical consequences of comparable worth are likely to be. Decision makers in both the legislative and collective bargaining processes need to know, for example, whether implementation of comparable worth can be expected to improve the female-male earnings ratios significantly, whether it would lead to a decline in the employment of women, whether it would induce more women to enter the labor force, whether it would help or hinder the occupational mobility of women and reduce occupational segmentation, and who would "win" and who would "lose."

To help focus subsequent debate, this paper presents a nontechnical survey of the small but growing empirical literature by economists on the consequences of comparable worth. I discuss in turn studies of the consequences of comparable worth on the male-female earnings gap, of its potential to affect adversely the employment of women, of its effects on the labor supply and occupational mobility of women, and of its effects on women *and* their families as a group. The survey is critical in nature and points to areas in which research is needed.

There are several important empirical issues relevant to future policy debate that I do not discuss. These include the existence of sex bias in describing or evaluating jobs, the difficulty (some would argue impossibility) of devising a single evaluation scheme that can meaningfully compare the "worth" of all employees in a single firm, and the problem of rater reliability; these are all issues that have been, and will be, addressed by noneconomists. I also do not discuss a key theoretical issue of concern to economists, namely, whether it makes any sense to speak of the worth of a job independent of labor market conditions. Rather, my focus is solely on empirical studies of the consequences of implementing a comparable worth policy.

Effect on Earnings Gap

Estimates of whether implementing comparable worth would have a significant effect on the gap between the average earnings of females and the average earnings of males have been both ex ante and ex post in nature. Ex ante studies (Ehrenberg and Smith 1987a; Sorensen 1986, 1987a, 1987b; Johnson and Solon 1986; Aldrich and Buchele 1986; and Smith 1988) use cross-section data to estimate how much women's wages would increase if comparable worth were implemented in a way the authors specify. Ex post studies (Kahn 1987; Killingsworth 1987a, 1987b; and Orazem and Matilla 1987) try to infer what has happened to the earnings of males and females

after the *actual* implementation of comparable worth–type pay adjustments in the public sector. I discuss each type of study in turn.

Ex Ante Studies. Most, but not all, states have conducted job evaluations for their employees based on the factor point method (Treiman 1979). The characteristics of jobs are described, and raters then assign point scores to each job on a number of dimensions. In the widely used Hay point system, for example, developed by Hay Associates, these dimensions are know-how, problem solving, accountability, and working conditions. In another widely used system, developed by Norman D. Willis and Associates, the dimensions are knowledge and skill, mental demand, accountability, and working conditions. The points a job receives for each category are then summed to get a total score, or measure of worth for the job.

Assuming that the principle of comparable worth requires that jobs of equal worth be paid equal wages, one can compute a comparable worth wage gap (CWWG), or estimate of how much, on average, wage levels in female-dominated jobs (typically taken to be those that employ at least 70 percent females) would have to be increased to achieve equal wage levels with equally rated male-dominated jobs (taken to be those that employ at least 70 percent males): First, estimate a wage equation in which a measure of the occupational wage (e.g., the starting wage scale, the mid-range wage scale, or the maximum wage scale in the occupation) in male-dominated jobs is specified to be a function of only the occupation's total factor point score. Next, compute, in percentage terms, how much the actual wage in each female-dominated job lies below this estimated male wage equation; this is an estimate of the magnitude of the comparable worth wage adjustment required in each occupation. Finally, weight each of these individual occupational wage adjustments by the share of employees in each occupation and then aggregate across the female-dominated occupations to come up with the CWWG.

This was the approach followed by Ronald G. Ehrenberg and Robert S. Smith (1987a) and Elaine Sorensen (1987a), who together studied pay systems for state government employees in five states and local government employees in one municipality *prior* to any implementation of comparable worth in the jurisdictions. Of course, in implementing the methodology described above, the researchers had to decide which wage measures to use (Sorensen used a single measure; Ehrenberg and Smith experimented with starting, mid-range, and maximum salaries), which functional form to use to describe the wages of males (Sorensen used a linear equation, Ehrenberg and

Table 5.1. Estimates of Comparable Worth Wage Gaps (CWWG) for State Employees in Selected States and Municipal Employees in San Jose[a]

Study	*Jurisdiction*	*Evaluation System*	*Estimated CWWG*
Ehrenberg & Smith (1986)	Minnesota (1981)	Hay	14.6–20.0%
	Washington (1974)	Willis	21.9–23.9
	Connecticut (1980)	Willis	15.4–20.2
Sorensen (1987a)	Iowa (1983)	Arthur Young	15.9
	Michigan (early 1980s)	Arthur Young	17.5
	Minnesota (1981)	Hay	21.4
	San Jose (1982)	Hay	25.5
	Washington (1983)	Willis	33.5

[a]The larger estimate observed by Sorensen for the state of Washington than those obtained by Ehrenberg and Smith may reflect the latter's use of data from different years (1983 versus 1974).

Smith experimented with linear and loglinear functional forms), and whether to enter the four individual factor point scores rather than the total score as predictors in the male wage equation (Ehrenberg and Smith experimented with the four-factor point scores because this allowed the existing male-dominated occupational structure to determine the marginal value the state placed on an additional point in each of the four categories, rather than assuming that only total factor points affected wages).

The results of the two studies are summarized in table 5.1. CWWGs in the range of 15.4 to 33.5 percent were found for the six jurisdictions. The range of estimates for each state in the Ehrenberg and Smith study occurred because of all the experimentation they did. In each case, however, their estimates were fairly robust to the methods used.

Given these estimates, one can compute the effect of making such comparable worth wage adjustments on the relative earnings of men and women by computing hypothetical wages for all female and male employees after such adjustments (assuming these adjustments are made only in female-dominated occupations and are given to employees of *both* sexes employed in these occupations) and then contrasting the ratio of average female to average male wages after the adjustments to the ones that existed before. This was the procedure followed by Sorensen: the unweighted average (across the six jurisdictions) earnings ratio observed before the hypothetical adjustments was 76 percent, whereas it was 87 percent after the adjustment. Sorensen thus concluded that, on average, such comparable worth wage

adjustments would reduce the female-male earnings gap for *government employees* in these jurisdictions by about 45 percent (11/24).

For several reasons, one must be cautious in drawing conclusions from these numbers about the likely effects of implementing comparable worth for state employees. First, in some of the states (e.g., Washington), the job evaluations covered only a sample of state employee occupations; the results may not generalize to other state employee groups. Second, such wage adjustments raise total labor costs (on average Sorensen computes this increase to be 8 percent of payroll), which, along with the changing relative costs across occupations, may cause the level and composition of employment across both male-dominated and female-dominated occupations to change. Sorensen implicitly assumes no such changes would occur. Finally, given the political nature of both the collective bargaining and legislative processes and the constant pressure by groups to improve their job evaluation scores (see Ehrenberg and Smith 1987a, n. 17), there is reason to believe that actual comparable worth wage adjustments would not approach the magnitudes described above. Indeed, as we shall see below, several studies suggest that in practice such adjustments have been much smaller in several states.

Private-sector ex ante studies, such as those by George Johnson and Gary Solon (1986), Sorensen (1987b), Mark Aldrich and Robert Buchele (1986), and Robert S. Smith (1988), have adopted somewhat different approaches. Johnson and Solon use a large national sample of both private- and public-sector workers taken from the May 1978 Current Population Survey and estimate wage equations for males and females as functions of the individuals' personal characteristics (e.g., age), industry dummy variables, occupational characteristics variables (developed by the National Research Council), and a variable that measures the percentage of the workers in an occupation who are female. In such wage equations, a larger share of female employees in an occupation is associated with lower wages.

Johnson and Solon then *interpret* the concept of comparable worth to mean that it would be illegal to have this share influence wages and simulate how much the average female-male wage gap would be reduced if the coefficients of the female share were set at zero. Depending on the specification they use, an overall female-male wage differential of roughly 41 percent is estimated to decline by 3 to 8 percent when this restriction is imposed. Johnson and Solon thus estimate that comparable worth would reduce the overall wage gap by at most one-tenth to one-fifth.

Aldrich and Buchele, who used a different sample of data, the National Longitudinal Surveys, undertook a similar calculation and

found that comparable worth would "reduce the male-female wage gap by 15 to 20 percent" (Aldrich and Buchele 1986, 148). So their estimate and that of Johnson and Solon are fairly consistent.

Finally, Sorensen (1987b) extends and replicates Johnson and Solon's analysis, using more recent data from the May and June 1983 Current Population Surveys, which permit her to include additional variables (e.g., firm size) in her estimated wage equations. Like Johnson and Solon, Sorensen concludes that a comparable worth policy would reduce the overall wage gap by at most one-fifth. Quite strikingly, however, the potential effect of such a policy is seen to vary widely across sectors of the economy. While she estimates that the policy might reduce the female-male earnings gap by about one-third in the public sector and by one-quarter in the nonmanufacturing private sector (defined by her to include all industries except for manufacturing and the public sector), it would reduce the gap in manufacturing by at most only 6 percent.

Of course, none of these authors' concepts of comparable worth really corresponds to the definition that proponents expound, namely, equal wages within a firm for jobs of equal value. The authors control for interindustry wage differentials and (in Sorensen's case) for wage differentials because of firm size, but these are incomplete controls for firm-specific wage differentials. Other studies suggest that the magnitude of the coefficient of the variable percentage female (that is, the percentage of workers in an occupation who are female) is sensitive to the variables that are included in the wage equation, so that more controls reduce the magnitude (see, for example, Filer 1987). Moreover, as Johnson and Solon and Sorensen note, their estimated comparable worth effects would be diminished if coverage of comparable worth was incomplete. Formal job evaluations tend to be conducted only by large firms, and Johnson and Solon conjecture that only 40 percent of workers, namely those employed by the government and by large private firms, would be affected. Assuming that the magnitude of the female-male wage gap does not depend on whether an individual employer would be covered by comparable worth, they further estimate that the overall effect would be to reduce the wage gap by only 1.4 to 3.2 percent, far less than one-tenth of the overall gap.

Sorensen's (1987b) results are relevant to this point. While coverage of workers under comparable worth might be large in manufacturing, where many workers are employed in large establishments (U.S. Bureau of the Census 1985a) her evidence cited above suggests that comparable worth would have a small effect in this sector. In contrast, in the nonmanufacturing private sector, where she estimated compa-

rable worth to have the potential to reduce the wage gap by one-quarter, only a small fraction of the employees would likely be covered. If coverage was restricted to workers in firms with at least one hundred employees, for example, only about 49 percent in the service industry and 48 percent in retail trade would be covered (U.S. Bureau of the Census 1985b, table 5, and U.S. Bureau of the Census 1985c, table 5). If the minimum size for coverage was set at five hundred employees, these numbers would fall to about 29 percent and 38 percent respectively.

Of course, these crude calculations assume that females and males are distributed across firms of different sizes in the same manner and that all employees, not just those in occupations that are predominantly female, would be eligible for comparable worth wage adjustments. Smith (1988) made more refined calculations using the May 1979 Current Population Survey data, which have information on individuals' industry, occupation, and size of employer, to estimate the maximum percentage of women who might have their wages adjusted as a result of a comparable worth policy. He assumes that only females employed in jobs that are at least 60 percent female and are either nonteaching jobs in the public sector (since it is hard to envision other jobs "comparable" to teachers in education) or private-sector jobs in firms that have at least five hundred employees, would be eligible for such wage adjustments. Using these criteria, he concludes that only 23 percent of all female workers would likely be covered by a comparable worth policy and that they would tend to be higher-paid women. So, overall, the effects of comparable worth on women's wages might be even smaller than Johnson and Solon estimate.

Ex Post Studies. After a well-publicized strike over the issue, San Jose, California, was the first city in the United States to implement comparable worth for its employees via the collective bargaining process. Five wage adjustments to achieve comparable worth took place during the July 1981–July 1984 period. Two studies (Kahn 1987 and Killingsworth 1987a) provide estimates of what the effects of these adjustments were. Both these studies try to make inferences based on before and after comparisons, which require them to infer what would have happened in San Jose in the post-1981 period in the absence of the adjustments. As the discussion will indicate, this is not a simple task.

Shulamit Kahn focuses on the wage increases for those San Jose city jobs that were targeted to receive comparable worth increases and contrasts them to the wage increases in nontargeted city jobs. She

finds that during the July 1980–July 1986 period the wage increases in targeted jobs averaged 74 percent. In contrast, the wage increases for other jobs in the city (*not* just those that had been part of an original pay equity survey) averaged about 50 percent during the period. Because a similar pattern of relative wage changes was not observed for jobs in other nearby local governments, she concludes that the observed difference in San Jose may have been due to the comparable worth efforts. I say "may" here because, although the other job wage scales in San Jose were roughly equal in 1980 to those in the surrounding areas, the wage scales in the jobs targeted to receive comparable worth increases were somewhat lower in San Jose. Hence some of the observed difference in wage increases may simply have been responses to market forces, although Kahn does note that in 1980 wages in San Jose in the targeted public-sector occupations (clerical) were higher than the wages in these occupations in the private sector.

Mark R. Killingsworth (1987a) focuses his analysis on the 170 full-time job classifications that were part of the original pay equity survey. He finds that between October 1981 (*after* the implementation of the first comparable worth wage adjustment) and July 1986, mean pay increased by 30.5 percent and 38.1 percent in the male-dominated and female-dominated jobs respectively, which, like Kahn's analysis, suggests that comparable worth may have had an effect (smaller in his case) on wages in female-dominated occupations.

To model more formally whether comparable worth adjustments affected wages in both the female-dominated and male-dominated occupations in the city, Killingsworth conducted both cross-section and longitudinal econometric analyses. As he notes, the longitudinal analyses, in particular his fixed and random effects models, are preferable.

In both cases, Killingsworth used salary data by occupation for eight points in time (July 1980, October 1980, October 1981, January 1983, August 1983, March 1984, April 1985, and July 1986); the first two dates preceded the implementation of comparable worth, while the latter six were during and after implementation. The logarithm of the salary in occupation i at time t is specified to be a linear function of a time trend term (the number of days between July 1980 and the date), a dummy variable that takes on the value of one once comparable worth is implemented (the last six dates) and zero otherwise, and an occupation-specific effect that is assumed to be either fixed or random. The models are estimated separately for the male-dominated, and female-dominated occupations, and in each case

the coefficient of comparable worth is interpreted as indicating by how much, on average, comparable worth increased wages in these occupations.

Killingsworth found that, on average, comparable worth caused the wages of males to increase by about 9 percent more than would have been the case and the wages of females to rise by about 12 percent more. As such, he concludes that comparable worth in San Jose increased women's wages by about 3 percent relative to those of men during the period; this difference was statistically significant from zero in the fixed effects model but not in the random effects model. He thus finds much smaller effects for comparable worth than Kahn did.

Killingsworth's findings, however, raise two questions. First, why should comparable worth wage adjustments in female-dominated jobs cause wages to rise faster than would otherwise be the case in *male*-dominated jobs? Indeed, one fear of critics of comparable worth is that comparable worth wage increases would be financed by restricting wage increases in other public-sector jobs; one might thus expect comparable worth adjustments to reduce wage increases in male-dominated jobs.

This leads to the second question: why should one assume (as Killingsworth's model implicitly does) that in the absence of comparable worth adjustments, wages would have increased at a constant rate in San Jose during the July 1980–July 1986 period? In fact, average hourly earnings growth varied considerably for the economy as a whole during this period, falling from more than 9 percent in 1980 and 1981 to less than 4 percent in 1984, 1985, and 1986. The effects he attributes to comparable worth may reflect only underlying nonlinear trends in earnings growth in San Jose.

Killingsworth (1987b) uses essentially the same methodological approach to estimate the effects of three sets of comparable worth wage adjustments that were legislatively enacted for Minnesota state employees between 1983 and 1986 (these became effective in July 1983, July 1984, and July 1985). He analyzes data for a random sample of one thousand white male and one thousand white female employees who were present and active in state employment during the entire October 1981–April 1986 period and asks whether, after holding constant changes in personal characteristics and allowing for long-term pay trends, salary increases were larger for women than for men after the comparable worth wage adjustments. He concludes that the women's wages grew cumulatively by about 7 percent *more* and the men's wages by about 1.4 percent *less* than they would have in the

absence of comparable worth. One must, however, again question his assumptions of constant trend growth rates in the absence of the comparable worth adjustments.

Peter F. Orazem and J. Peter Matilla (1987) used a different approach to estimate the impact of a comparable worth policy on the wage gap of Iowa state employees. Based on a job evaluation study conducted by Arthur Young and Associates, a pay equity program was proposed in 1984 for these employees. The proposal, which called for wage decreases for about 40 percent of the covered employees, was subject to considerable political debate, and eventually a "compromise" program was adopted in 1985 that moderated the wage increases "winners" received and eliminated all the proposed reductions.

Orazem and Matilla used data on a random sample of state employees and estimated wage equations for them using as explanatory variables individual characteristics, job evaluation point scores, and several other variables, including whether the employee was a woman. Three different wage outcomes were analyzed: the employee's actual wage scale as of December 1983 (prior to the comparable worth plan), the employee's wage scale as proposed under the Arthur Young plan, and the employee's wage scale after the implementation of the political compromise. Focusing on how the coefficient of the "female" variable changed with the wage outcome used enabled the authors to estimate the effects of the original comparable worth proposal and the compromise that was adopted on the male-female wage differential.

The precise estimates the authors obtained are somewhat sensitive to the explanatory variables they included in their equations. Some of their specifications included private-sector market wage rates for occupations, as measured by an annual wage survey conducted by the state—presumably proponents of comparable worth would prefer to see this variable excluded. Some specifications included the job evaluation point scores, while others did not. Nonetheless, all tended to suggest that the pay equity policy that was actually implemented reduced the unexplained (by the wage equations) wage gap by about one-quarter, whereas the gap would have been almost completely eliminated by the proposed policy.

In an absolute sense, their estimates suggest that the policy that was implemented increased the wage scale of the average female state employee in Iowa by about 1 to 4 percentage points relative to the wage scale of the average male. These numbers should be contrasted to the average 8 percentage point gain that they estimate would have been

produced by the original Arthur Young pay equity proposal. Comparable worth policies implemented through the political process do not necessarily lead to "comparable worth." Indeed, using analyses similar to those of Orazem and Matilla (1987), Killingsworth (1987b) reaches this same conclusion.

Effect on Employment Levels

As with studies of the effect of comparable worth on earnings, studies of the effect of comparable worth on levels of employment have been both ex ante and ex post in nature. Among those who have conducted studies in the former category are Ehrenberg and Smith (1987a, 1987b) and Ehrenberg, Smith, and Stratka (1986), who simulated the effect of imposing comparable worth on the employment levels of women in the state and local sectors, and Aldrich and Buchele (1986), who performed similar simulations using economy-wide private-sector data. Included in the latter category are analyses by Robert G. Gregory and Ronald C. Duncan (1981) of how comparable worth–type wage adjustments influenced female employment in Australia and by Kahn (1987) and Killingsworth (1987a, 1987b) of how comparable worth wage adjustments in San Jose and Minnesota, respectively, affected municipal and state employment levels in these jurisdictions.

Comparable worth wage adjustments (CWWA) would tend to increase the wages of female employees relative to those of males within any major occupational group (e.g., clerical) in that females are more likely to be employed in female-dominated detailed occupational groups (e.g., secretaries) that would receive CWWA increases. Similarly, CWWAs would tend to increase the average wage costs in those major occupational groups that contain many female-dominated occupations (e.g., clerical) relative to those major occupational groups (e.g., blue-collar workers) that contain fewer female-dominated occupations. As such, one might expect to observe decreases in the employment of women, both because of male-female employment substitution away from female-dominated detailed occupational groups within major occupational groups and because of substitution away from female-dominated major occupational groups. To the extent that CWWA increases for female-dominated groups are not "paid for" by smaller wage increases for male-dominated groups, average wages would rise, which would further depress employment levels for both men and women.

Ex Ante Studies. Ehrenberg and Smith (1987a, 1987b) used data from the 1980 Census of Population grouped by state (for state employees) and SMSA (for local government employees) to simulate the likely effect on female employment rates of a 20 percent wage increase for all female employees in these sectors. Their simulations are based on estimates of within-occupation male-female substitution elasticities obtained from a constant elasticity of substitution production function specification and on estimates of across-occupation substitution elasticities obtained from a translog cost share specification. Because the estimated elasticities they obtained were quite small, they concluded that a 20 percent increase for all female employees in the sector would reduce female employment levels by only 2 to 3 percent.

Aldrich and Buchele (1986) applied Ehrenberg and Smith's approach to private-sector data, using three-digit industries rather than geographic areas as units of analyses. They obtained very similar employment effects, from which they predicted that private-sector comparable worth wage increases in the range of 10 to 15 percent would reduce female employment levels by about 3 percent in that sector.

Although the loss of female employees in each of these studies seems small and should allay critics' fears that comparable worth wage increases in the United States would lead to large losses of female employees, it should be emphasized that these estimates are based on cross-section demand elasticities that use broad occupational groups (four in all) and that do not control for area-specific (in the case of Ehrenberg and Smith) or industry-specific (in the case of Aldrich and Buchele) variables that might influence either male-female employment ratios within occupations or the occupational distribution of employment. Ehrenberg, Smith, and Stratka (1986) used longitudinal data on local government employment and wages from the Equal Employment Opportunity Commission's EEO-4 data to try to control for such omitted area-specific variables. They also used a larger number of occupational categories (eight). These modifications did not fundamentally alter any of the conclusions, however.

Ex Post Studies. All the authors acknowledge that serious data problems limit the usefulness of the above studies for public policy simulations and that the simulations are often based on statistically imprecise estimates of parameters. As such, it is useful to turn to the ex post studies. Gregory and Duncan's (1981) time series study of the Australian employment experience after the institution of comparable worth–type wage adjustments found that relative (by sex) employment demand elasticities with respect to relative (by sex) wages were suffi-

ciently small and that the substantial relative wage increases for women that occurred between 1975 and 1978 in Australia reduced employment growth for women by only about 1.5 percentage points a year. The estimated slowdown in employment growth was smallest in the public (close to zero) and service sectors and largest in manufacturing. Of course, whether 1.5 percentage points per year is a small effect should be judged in the context of an overall female employment growth rate of 3 percentage points per year more than the male growth rate during the period. Viewed in this context, the Australian policy reduced the employment growth rate advantage of females vis-à-vis males by one-third (1.5/[3.0 + 1.5]). As noted above, employment of males is also likely to be affected by comparable worth policies; Gregory and Duncan did not analyze this effect.

In later work, Robert G. Gregory, R. Anstie, A. Daly, and V. Ho (1987) present analyses of the Australian data that cover the period from 1966 to 1984. Although they conduct no formal econometric analyses in their paper, they note that women increased their share of hours worked in Australia during the period and that the growth rate of this group of women was dominated by a trend in which no sharp slowdown occurred after the large (in the range of 20 percent) comparable worth–type wage adjustments were implemented. From this they conclude that any effects of the policy on the relative employment of women must have been very small, although they note that they did not analyze the effects of the policy on total employment. In fact, Ehrenberg and Smith (1987a, 1987b) found in their simulations that the potential adverse effect of a comparable worth policy on employment of women in the United States would be primarily through its effect on total employment.

The two studies of the San Jose experience reach conflicting conclusions. Kahn (1987) finds that municipal employment grew more rapidly during the 1981–86 period in the public sector of San Jose than in other neighboring cities, that employment in the municipal jobs targeted for CWWA in San Jose grew more rapidly than municipal employment in nontargeted jobs, and that the percentage of female workers in these targeted jobs actually increased. From this evidence Kahn concludes that comparable worth had no adverse effects on employment, that higher wages in the targeted jobs induced more females to apply, and that affirmative action, or an increase in the labor supply of women in general, led to the increases in the employment of women.

It is hard to evaluate the validity of Kahn's findings because they are all based on simple comparisons of trend increases in employ-

ment across occupations in San Jose and/or across local governments in the San Jose area. Put another way, implicitly, she is assuming that, in the absence of comparable worth, municipal employment would have grown at the same rate for all occupations in San Jose and that this rate would have equaled the growth rate of municipal employment in neighboring cities. As such, she does not allow for the possibility that conditions other than comparable worth influence employment growth and labor supply across occupations and areas.

Killingsworth (1987a) estimates a fixed effects model using data for six points in time and the 170 full-time job classifications that were part of the original San Jose job evaluation study. The logarithm of employment in an occupation at each time is specified to be a function only of the logarithm of the occupational wage at that time, a time trend term to control for general growth in employment, and occupation-specific dummy variables. Separate equations are estimated for male- and female-dominated jobs, and he concludes that negative wage elasticities of demand, in the range of minus one, exist for both the male- and female-dominated occupations. Killingsworth attributes the increases in employment that Kahn observes to his time trend term (which is about 9 percent per year for both male- and female-dominated jobs). Given his estimate that the CWWA increased males' wages by about 9 percent and females' wages by about 12 percent, Killingsworth concludes that these wage adjustments actually "cost" San Jose's male municipal employees one year's employment growth and female municipal employees more than one year's growth.

Killingsworth (1987b) performs similar analyses for Minnesota using data for 876 male-dominated state jobs and 203 female-dominated state jobs over nineteen quarters during the October 1981–April 1986 period. He finds wage elasticities in the range of minus one for both males and females when starting wage scale data are used. Coupled with his estimated wage effects reported earlier, these data suggest that the comparable worth wage adjustments in Minnesota *decreased* female employment levels by about 7 percent and *increased* male employment levels by about 1.4 percent during the 1981–86 period. The estimated effect on women is equivalent to a loss of about one year's employment growth.

Of course, Killingsworth's results for both San Jose and Minnesota are contingent first on his estimated CWWA effects on the wages of males and females in these jurisdictions; as noted above, I believe there are problems with these estimates. Second, his employment equations do not permit interoccupational substitution (an occupa-

tion's wage influences its employment level only) and assume that omitted time-specific factors influence all occupations in a gender group identically and at a constant rate over time. Indeed, no thought is given to the possibility that comparable worth per se may have influenced the trend rate of growth of employment (one of Kahn's points) independent of its effects via wage rates. Although my own preference is for rigorous econometric modeling, such as Killingsworth's, the jury is still out on the effects of comparable worth on municipal employees in San Jose and on state employees in Minnesota.

General Equilibrium Considerations: Who Will Win and Who Will Lose

The studies discussed in the previous section ignore the partial coverage aspect of any comparable worth policy that is likely to be implemented in the United States. If comparable worth has adverse effects on employment rates in the covered sector, displaced workers may seek jobs in the uncovered sector, resulting in downward pressure on wages there. Even if the number of jobs lost by female employees in the covered sector is low relative to the wage gains induced by comparable worth there, it is not obvious that women as a group would gain. Women in the low-paid uncovered sector might find, for example, that their wages are lowered even more by the "crowding" of displaced workers into that sector (see Smith 1988).

Alternatively, increased wages in the covered sector might induce some displaced women to remain "attached" to the covered sector in the hope of obtaining a higher-paying job in the future. Thus the policy might lead to "wait unemployment" among females. As is well known, in this case the increase in the number of females who are unemployed might exceed the number displaced because of the increase in covered-sector wages caused by the CWWA policy, and the direction that female wages in the uncovered sector would move would depend on demand elasticities in both sectors (see Ehrenberg and Smith 1988, chap. 12, for a more extended discussion of wait unemployment).

Of course, in addition to influencing the allocation of women employees between the covered and uncovered sectors, CWWA may also influence the labor force participation rates and occupational choice of women. Higher wages in some female-dominated occupations might induce more women to enter the labor force and increase the supply of women to occupations in which the adjustments took place (Kahn 1987). Higher wages in these occupations might increase their

attractiveness to incumbents and new entrants and thus reduce the mobility of women into traditionally male-dominated occupations. Finally, higher wages in traditionally female-dominated occupations might increase the supply of males to these occupations, thereby reducing occupational segregation.

Empirical research related to these topics has been surprisingly slim. As noted above, Kahn (1987) found that CWWAs were associated with an increased representation by women in targeted occupations in San Jose. Perry C. Beider, B. Douglas Bernheim, Victor R. Fuchs, and John B. Shoven (1988) simulate in a computable general equilibrium model some of the effects of a policy (like comparable worth) that raises the wages of females. Although their simulations likely overstate the effects of comparable worth, because the policies they simulate eliminate all gender differences in earnings within major occupational groups, they are the only authors who analyze comparable worth empirically in a general equilibrium framework.

Beider et al. find that comparable worth would induce more married women to enter the labor force and that increased employment for these women would be at the expense of employment for males and single women. Despite the loss of employment to these groups, they would gain in a distributional sense because of their increased wages. In contrast, married couples would lose (increased employment of married women would be offset by decreased employment for some of their husbands), and single men as a group would be the big losers. Beider et al. also present estimates of efficiency and employment losses under a variety of assumptions about, for example, coverage of comparable worth (partial or total), the nature of utility functions for married couples, the elasticities of supply and substitution, and employer hiring rules (applicant fraction or historical fraction), and are careful to stress the sensitivity of their results to changes in assumptions. Nonetheless, to keep their model "computable," they are forced to limit it to only two occupational groups (skilled and unskilled). This restriction prevents them from addressing a number of the issues described above.

Conclusions

As this survey has shown, we know very little about what the true "general equilibrium" effects of comparable worth are likely to be. The research described above has concentrated heavily on estimating what the direct effects of comparable worth might be on the female-male wage gap and what the likely direct effects of comparable worth

wage changes might be on the employment of women in the covered sector. Although the authors of the various studies might disagree, the evidence appears to be fairly consistent. The studies surveyed above suggest that one of the direct effects of comparable worth will be to reduce modestly the overall female-male wage gap and that this reduction would be achieved at the cost of only small losses of female employees.

What is missing, however, is much discussion of the true general equilibrium, or second-round effects, that comparable worth would be likely to induce. Would the altered wage structure affect the occupational choices of males and females in the covered sector and/or employers' hiring decisions? Would the changing wage structure in one sector of the economy lead to alterations in the wage structure in the rest of the economy? Would higher mandated wages in female-dominated jobs lead to higher implicit hiring standards, or would employers compensate by providing less on-the-job training? Would these higher wages reduce occupational mobility for women over the life cycle and their earnings growth rates? (See Hashimoto 1982 for evidence that minimum wages affect earnings growth rates.) Analyses of issues such as these should be on the agenda of comparable worth researchers.