ECONOMETR) ` ANALYSES OF THE EMPIRICAL CONSEQUENCES
OF COMPARABLE WORTH: WHAT HAVE WE LEARNED?

Ronald G. Ehrenberg

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ABSTRACT

This paper presents a survey of the small, but growing, empirical liter-
ature by economists on the consequences of comparable worth. It discusses in
turn studies of comparable worth's effects (or potential effects) on the
male/female earnings gap, on female employment, on female labor supply and
occupational mobility, and on women and their families as a group. The
survey is critical in nature and areas in which further research is needed
are indicated.

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I.   Introduction

Some two and a half decades after the passage of the Equal Pay Act of 1963 and Title VII of the Civil Rights Act, it is still common to observe that on average females earn less than males, that females are distributed across occupations in a manner quite different from that of males, and that earnings in female dominated occupations tend to be lower than earnings in male dominated occupations, even after one controls for traditional proxies for productivity. The frustrations generated by these outcomes have led to pressure for the adoption of the principle of comparable worth, or pay equity, a principle that at least one participant in the debate has called "the women's issue of the 1980s."

In simplest terms, proponents of comparable worth assert that jobs within a firm can be valued in terms of the skill, effort, and responsibility they require, as well as the working conditions they offer. Two jobs would be said to be of comparable worth to the firm if they were comparable in terms of these characteristics. The principle of comparable worth asserts that within a firm jobs of comparable worth should receive equal compensation.

While some efforts to implement comparable worth have taken place in the private sector, the major push has occurred in the state and local government sector; a sector of the economy in which both union membership is growing and a large proportion of the employees are women. Starting with a 1974 state of Washington study, a large number of states have undertaken formal job evaluation studies to see how their compensation systems mesh with the principle of comparable worth and a number of 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. Economists' protestations to the contrary, the concept of
comparable worth has become widely accepted in the public sector of some
states and the real policy issue is whether the concept should be extended
to other public employees in the state and local sector and to the federal
and private sectors. While debate on this issue will undoubtedly continue
to be both emotional and political, rational decision making must include
an evaluation of what the empirical consequences of comparable worth are
likely to be. Decision makers either in the legislative or collective
bargaining processes need to know things such as whether implementation of
comparable worth can be expected to improve female/male earnings ratios
significantly, whether it would lead to a decline in female employment,
whether it would induce more women to enter the labor force, whether it
would help or hinder female occupational mobility and reduce occupational
segmentation, and who would "win" and who would "lose" from the
implementation of comparable worth.

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 for male/female earnings gaps, of its
potential to affect female employment adversely, of its effects on female
labor supply and occupational mobility, and of its effects on women and

their families as a group. The survey is critical in nature and I point out areas in which research is needed.

Before embarking on this survey, however, I should stress that there are a number of important empirical issues relevant to future policy debate that I do not discuss. These include the existence of sex bias in describing or evaluatir ; 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 with empirical studies of the consequences of implementing comparable worth.

II. Comparable Worth and the Male/Female Earnings Gap

Estimates of whether implementing comparable worth has 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, which include 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 female wages would increase if comparable worth were implemented in a way the authors specify. Ex post studies, which include Kahn (1987), Killingsworth (1987a) (1987b) and Orazem and Matilla (1987) try to infer what has happened to male and female earnings after the actual implementation of

comparable worth type pay adjustments in the public sector. I discuss each type of study in turn.

A. **Ex Ante Studies**

Most, but not all, states have conducted job evaluations for their employees based upon the factor point method (Treiman (1979)). The characteristics of jobs are described and then raters assign point scores to each job on a number of dimensions. For example, in the widely used Hay Point System developed by Hay Associates, these dimensions are "know-how", "problem solving", "accountability" and "working conditions", while in the also 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 whose employment is at least 70 percent female) would have to be increased to equal wage levels in equally rated male jobs (taken to be those whose employment is at least 70 percent male) as follows: 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 only of the occupation's total factor point score. Next, compute, in percentage terms, how much the actual wage in each female-dominated job lie 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 Ehrenberg and Smith (1987a) and 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 male wages (Sorensen used a linear equation, Ehrenberg and 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 equation (Ehrenberg and Smith experimented with the four factor point scores because this allows the existing male-dominated occupational structure to determine the marginal value the state places on an additional point in each of the four categories, rather than assuming that only total factor points should affect wages).

Their results are summarized in Table 1. CWWG in the range of 15.4 to 33.5 percent were found for the six jurisdictions. The range of estimates for each state from the Ehrenberg and Smith study occurred because of all the experimentation they did. However, in each case 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 she observed before the hypothetical adjustments was 76 percent, while after the adjustment the average earnings ratio was 87 percent. So, she 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 a number of reasons, however, 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) and this increase in labor costs along with the changing relative costs across occupations may cause the level and composition of employment across occupations of males and females to change. Sorensen implicitly assumes no employment changes in these calculations. 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), footnote 17), there is reason to believe that actual comparable worth wage adjustments that are implemented would not approach the magnitudes described above. Indeed, as we shall see below, several studies sugges' that in practice they have been much smaller in several states.

Private sector ex ante studies, such as Johnson and Solon (1986), Sorensen (1987b), Aldrich and Buchele (1986) and 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 measuring the fraction of an occupation nationwide that is female. In such wage equations, a larger share of female employment 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 equal to zero. Depending upon 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. So comparable worth is estimated to reduce the overall wage gap by at most one-tenth to one-fifth by them.

A similar calculation was undertaken by Aldrich and Buchele who use a different sample of data, the <u>National Longitudinal Surveys</u>, and find that "comparable worth would reduce the aggregate female/male wage gap by one-fifth. So their estimate and those 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 <u>Current Population Surveys</u> that permit her to include additional variables (e.g., firm size) in her estimated wage equations. Like them, she 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 the policy might reduce the female/male earnings gap by about one-third in the public sector, and one-quarter in the nonmanufacturing private sector (defined by her to include all industries except for manufacturing and the public sector), at best it would reduce the gap in manufacturing by only six percent.

Of course, none of these authors' concept of comparable worth really corresponds to the definition of comparable worth that proponents expound, namely equal wages within a firm for jobs of equal value. The authors control for interindustry and (in Sorensen's case) firm size wage differentials, but these are incomplete controls for firm-specific wage differentials. Other studies also suggest that the magnitude of the coefficient of the female occupational employment share variable is sensitive to the variables that are included in the wage equation, with more controls reducing 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 sector and large private firms, would be affected Assuming that the magnitude of the female/male wage gap does not depend upon whether an individual employer would be covered by comparable worth, they further estimate that the overall effect of comparable worth 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 employees are employed in large establishments (U.S. Bureau of the Census, 1985a) her evidence (cited above) suggests that comparable worth's likely effects would be small in this sector. In contrast, in the nonmanufacturing private sector, where she estimated comparable worth to have the potential to reduce the wage gap by a quarter, only a small fraction of employees would likely be covered. For example, if coverage was restricted to employees employed in firms with at least 100 employees, only about 49 percent of the employees 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 instead the minimum firm size for coverage was set at 500, 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 different sizes of firms 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. More refined calculations are made by Smith (1988) who uses the May 1979 Current Population Survey data, which have information on individuals' industry, occupation, and size of employer to estimate the maximum fraction of women who might have their wages adjusted by a comparable worth policy. He assumes that only females employed in jobs that are (a) at least 60 percent female and (b) 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 whose employment was at least 500, would be eligible for such wage adjustments. Using these criteria he concludes that only 23 percent of females would likely be covered by comparable worth and that these would tend to be higher paid women. So overall, comparable worth effects on women's wages might be even less than Johnson and Solon estimate.

B. Ex Post Studies

After a well-publicized strike over the issue, the city of 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 in San Jose 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 upon before-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.

Kahn focuses on the wage increases for 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 increases in targeted job wages averaged 74 percent. In contrast, wage increases for other jobs in the city (not just those that had been part of an original pay equity survey) averaged roughly 50 percent during the period. Since a similar pattern of relative wage changes was not observed for the 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, while the other job wage scales in San Jose were roughly equal to those in the surrounding areas in 1980, 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 for the two sets of jobs in San Jose may simply have been responses to market forces, although Kahn does note that public sector wages in San Jose in the targeted (clerical) occupations were higher than private sector wages in these occupations in San Jose in 1980.

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 of 1981 (after the implementation of the first comparable worth wage adjustment) and July of 1986, mean pay grew by 30.5 percent and 38.1 percent in the male-dominated and female-dominated jobs respectively, which like Kahn's analysis suggests comparable worth may have had an effect (smaller in his case) on wages in female occupations.

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

In both cases, Killingsworth uses salary data by occupation for 8 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 occur during and after the 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 had begun to be 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 and female occupations and in each case he interprets the coefficient of comparable worth as indicating by how much, on average, comparable worth increased wages in these occupations.

Killingsworth finds that on average comparable worth caused male wages to increase by about 9 percent more than otherwise would have been the case and female wages to rise by about 12 percent more. As such, he concludes that comparable worth in San Jose increased female wages by about 3 percent relative to male wages 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 jobs cause wages to rise faster than would otherwise be the case in *male* 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 jobs.

This leads to the second question, why should one assume (as his model implicitly does) that in the absence of comparable worth adjustments, wage increases would have increased at a constant rate in San Jose during the July 1980-July 1986 period. In fact, for the economy as a whole, average hourly earnings growth varied considerably during the period, falling from over 9 percent in 1980 and 1981 to under 4 percent in 1984, 1985 and 1986. The effects he attributes to comparable worth may reflect only nonlinear underlying trend rates of growth of earnings 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 1,000 white male and 1,000 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

and Smith (1987a; 1987b) and Ehrenberg, Smith and Stratka (1986), who simulate the effect on female employment in the state and local sector of imposing comparable worth in the sector and Aldrich and Buchele (1986), who perform similar simulations using economy wide private sector data. Included in the latter category are Gregory and Duncan's (1981) analyses of how comparable worth type wage adjustments influenced female employment in Australia, and Kahn (1987) and Killingsworth (1987a) (1987b) analyses of how comparable worth wage adjustments in San Jose and Minnesota, respectively, affected municipal employment and state employment levels in these jurisdictions.

Comparable worth wage increases would tend to increase the wages of female employees relative to male employees within any major occupational group (e.g., clerical) as females are more likely to be employed in female-dominated detailed occupational groups (e.g., secretaries) that would receive CWWA increases. Similarly, CWWA increases 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 female employment, both because of within major occupational group male/female employment substitution away from female-dominated detailed 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 should further depress both female and male employment.

allowing for long-term pay trends, salary changes were larger for women than for men after the comparable worth wage adjustments. He concludes that women's wages grew cumulatively by about 7 percent _more_ and men's wages by about 1.4 percent _less_ than they would have grown during the period, in the absence of comparable worth. However, again, one must question his assumptions of constant trend growth rates of earnings for men and women in the absence of the comparable worth adjustments.

Orazem and Matilla (1987) use a different approach to estimate the impact that a comparable worth policy had on the wage gap for Iowa state employees. Based on a job-evaluation study conducted by Arthur Young and Associates, a pay equity program was proposed in 1984 for Iowa employees. However, 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 use data on a random sample of state employees and estimate wage equations for them using individual characteristics, job evaluation point scores, and several other variables, including whether the employee was a woman, as explanatory variables. Three different wage outcomes are 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 changes with the wage outcome used allows the authors to estimate what the effects of the original comparable

worth proposal and the compromise that was adopted were on the male/female wage differential.

The precise estimates the authors obtain are somewhat sensitive to the explanatory variables they include in their estimating equations. Some of their specifications include 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 include the job evaluation point scores, while others do not. Nonetheless, all tend to suggest that the pay-equity policy that was actually implemented reduced the unexplained (by the wage equations) wage gap by about a quarter, while the proposed policy would have almost completely eliminated the gap.

In an absolute sense, their estimates suggest that the policy that was implemented increased the average female state employee's wage scale in Iowa by about 1 to 4 percentage points relative to the average male's wage scale. These numbers should be contrasted to an average 8 percentage point gain that they estimate the original Arthur Young pay equity proposal would have produced. Comparable worth policies implemented through the political process do not necessarily lead to "comparable worth". Indeed, using analyses very similar to Orazem and Mattila (1987), Killingsworth (1987b) reaches this same conclusion.

III. **Comparable Worth and Employment Levels**

As with studies of the effect of comparable worth on earnings, studies of the effect of comparable worth on employment have been both ex ante and ex post in nature. Included in the former category are Ehrenberg

A. **Ex Ante Studies**

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

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

While female employment losses in each of these studies seems small,
and should allay critics' fears that comparable worth wage increases in
the U.S. would lead to large female employment losses, it should be
emphasized that these estimates are based on cross-section demand
elasticities that use broad occupational groups (4 in total) and that do
not control for area specific (in the case of Ehrenberg/Smith) or industr
specific (in the case of Aldrich/Buchele) factors that might influence

either male/female employment ratios within occupations or the
occupational distribution of employment. Ehrenberg, Smith and Stratka
(1986) use 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 and also use a larger number of
occupational categories (8). However, these modifications did not
fundamentally alter any of the conclusion that only small employment
effects would result.

B. Ex Post Studies

There are serious data problems (acknowledged by all the authors)
that limit the usefulness of the above studies for public policy
simulations and the simulations are often based on statistically imprecise
parameter estimates. 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 sufficiently small, that the
substantial relative wage increases for women that occurred between 1975
and 1978 in Australia reduced female employment growth by only
approximately 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 the context of
this number, the Australian policy reduced the employment growth rate

advantage of females vis-a-vis males by one-third (1.5/(3.0+1.5)). As noted above, male employment is also likely to be affected by comparable worth policies; this was not analyzed by them.

In later work, Gregory, Anstie, Daly and Ho (1987) present analyses of the Australian data that cover the 1966-1984 period. While no formal econometric analyses are conducted in this paper, they note that women increased their share of hours worked in Australia during the period and that the growth rate of this share was dominated by a trend with no sharp slowdown occurring after the large (in the range of 20 percent) comparable worth type wage adjustments occurred. From this they conclude that any relative employment effects of the policy must have been very small, although they note that they again 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 female employment 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 San Jose public sector 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 share of female employment in these targeted jobs actually increased. From this evidence she concludes that comparable worth had no adverse employment effects, that higher wages in the targeted jobs induced more female applicants, and that it was

affirmative action, or increased female labor supply in general, that led to the increases in female employment.

It is hard to evaluate the validity of Kahn's findings because they are all based on simple comparisons of trend increases in employment 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 equalled the growth rate of municipal employment in neighboring cities. As such, she does not allow for factors other than comparable worth to 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. He attributes the actual employment increases that Kahn observes to his time trend term (which is roughly 9 percent per year for both male and female dominated jobs). Given his estimate (see the previous section) that the CWWA increased male wages by about 9 percent and female wages by about 12 percent, he concludes that these wage adjustments actually "cost" San Jose male municipal employees one year's,

and female municipal employees more than one year's employment growth during the period.

Killingsworth (1987b) performs similar analyses for Minnesota using data for 876 male-dominated state jobs and 203 female-dominated state jobs over 19 quarters during the October 1981-April 1986 period. He again finds (when starting wage scale data are used) wage elasticities in the range of minus one for both males and females. These, when coupled with his estimated wage effects reported earlier, suggest that the comparable worth wage adjustments in Minnesota decreased female employment by about 7 percent and increased male employment by about 1.4 percent during the 1981-1986 period. The estimated female effect is equivalent to a loss of about one year's employment growth for women.

Of course, his results for both San Jose and Minnesota are contingent first on his estimated CWWA effects on male and female wages in these jurisdictions; as noted in section II, I believe there are problems with these estimates. Second, his employment equations do not permit interoccupational substitution (an occupation's wage influences its employment level only), and assume that omitted time specific factors influence all occupations (in a sex 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. While my own preference is to prefer rigorous econometric modelling, such as Killingsworth's, the jury is still out on what comparable worth's effects have been on municipal employment in San Jose and state employment in Minnesota.

IV. <u>General Equilibrium Considerations: Who Will Win and Who Will Lose</u>

The studies discussed in the previous section ignore the partial coverage aspect (see section II) of any comparable worth policy that is likely to be implemented in the United States. If comparable worth has adverse employment effects in the covered sector, displaced workers may seek jobs in the noncovered sector and downward pressure on wages there would result. Even if female employment losses in the covered sector are low relative to the wage gains induced by comparable worth there, it is not obvious that women as a group would gain, for women in the low-paid noncovered sector (see Smith (1987)) might find their wages lowered even more by the "crowding" of displaced workers into that sector.

Alternatively, increased wages in the covered sector might induce some of the displaced females to remain "attached" to the covered sector in the hope of obtaining a now 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 female unemployment may exceed the number of females displaced by the increase in covered sector wages caused by the CWWA policy, and the direction that the female wages in the noncovered sector would move would depend upon demand elasticities in both sectors (see Ehrenberg and Smith (1988), Chapter 12, for a more extended discussion of wait unemployment).

Of course, in addition to influencing the allocation of female labor between the covered and noncovered sectors, CWWA may also influence female labor force participation rates and occupational choice. 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 relating to these topics has been surprisingly slim. As noted above, Kahn (1987) found that CWWA were associated with increased female representation in targeted occupations in San Jose. Beider, Bernheim, Fuchs and Shoven (1987, henceforth BBFS) simulate some of the effects of a policy that raised female wages (like comparable worth) in a computable general equilibrium model. While their simulations likely overstate the effects of comparable worth, because the policies they simulate eliminate all within major occupational group gender differences in earnings, their results are of interest because they are the only authors who analyze comparable worth formally in a general equilibrium framework empirically.

BBFS 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 latter's loss of employment, in a distributional sense as a group they would gain 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. BBFS 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 married couples' utility functions, 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.

V.   Concluding Remarks

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 female employment in the covered sector. Although the authors of the various studies might disagree, my evaluation of the evidence is that it is fairly consistent. The studies surveyed in sections II and III suggest to me that comparable worth's direct effects will be to reduce the overall female/male wage gap modestly and that this reduction will be achieved at the cost of only small female employment losses.

What is missing, however, is much discussion of the true general equilibrium, or second round effects, that comparable worth is likely to induce. Will the altered wage structure affect the occupational choices of males and females in the covered sector and/or employers' hiring

decisions? Will the changing wage structure in one sector of the economy lead to alterations in the wage structure in the rest of the economy? Will higher mandated wages in female-dominated jobs lead to higher implicit hiring standards, or employers compensating by providing less on-the-job training? Will these higher wages reduce female occupational mobility over the life-cycle and female earnings growth rates (see Hashimoto (1982) for evidence that minimum wages affect earnings growth rates)? Analyses of issues like these should be on the agenda of comparable worth researchers.

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Table 1

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

Study	Jurisdiction/Year	Job Evaluation System	Estimated Percentage CWWG
Ehrenberg and Smith (1986)	Minnesota/1981	Hay	14.6 to 20.0
	Washington/1974	Willis	21.9 to 23.9
	Connecticut/1980	Willis	15.4 to 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 their using data from different years for this state (1983 versus 1974).