

Theoretical and Measurement Issues in the Study of Sex Segregation in the Workplace: Research Note

JERRY A. JACOBS

In the United States, research on occupational sex segregation has relied principally on the index of dissimilarity (D) as a measure of the level of segregation. Several investigators (Charles, 1992; Semyonov, 1992; Grusky and Charles, 1992) advocate the use of odds ratios with highly aggregated occupational or industry data. However, because researchers generally prefer more detail rather than more aggregation when studying sex segregation, the use of odds ratio measures has not become a common tool. Given the broad consensus on D as a metric in the United States, I was intrigued to learn of the vigorous debate in Europe and elsewhere regarding the best way to study sex segregation.

This essay makes three principal points. First, researchers should select indices of segregation that match the concepts they seek to investigate. Second, because no single measure adequately captures all theoretically relevant aspects of segregation, investigators should employ multiple indices of segregation. Occupational segregation is at least as complex and multifaceted as income inequality, where no universally accepted index has been developed that meets all measurement desiderata (Atkinson, 1987; Jenkins, 1991). Crowding, unevenness and intergroup contact are three separate aspects of segregation that require separate measures. Third, given D's wide use, easy computation and simple interpretation, I recommend continued reliance on D unless a clear and compelling alternative emerges.

Why study sex segregation? Edgeworth (1922) argued that women earn less than men because

they are crowded into a limited number of fields, thus artificially creating a large pool of women for a restricted set of employment channels. (The issue of crowding is also discussed by Bergmann, 1986, and Parcel, 1989.) D does not tap the degree of crowding *per se*, nor does the WE index (OECD, 1985), the Sex-Ratio Index, either adjusted (Siltanen, 1990) or unadjusted (Hakim, 1992), various size-standardized indices or the K-M Index (Karmel and MacLachlan, 1988). The case of complete sex segregation can be used to illustrate this point. It is possible for all women to be crowded into a single occupational title, but it is also possible for women to be found in 10 or 20 or more perfectly segregated occupations. Indeed, it is theoretically possible for women to be found in all but one occupation with men crowded into the remaining single field. (On the other hand, complete integration implies no crowding of women relative to men.) Thus, the level of crowding and the level of segregation are only partially related.

Most studies that have specifically addressed the issue of crowding have found that women are indeed restricted to a far smaller set of fields than are men, but there are exceptions. In my research on college students in the United States, I found that by 1980 women's choices of college majors were more dispersed across fields than their male counterparts, a marked switch from the situation twenty years earlier. Most researchers who have considered the issue of crowding employ ad hoc measures which describe some portion of the tail of the distribution—eg., 50 per cent of women were crowded into

20 occupations—but more comprehensive measures are available (Jacobs, 1989; Massey and Denton, 1989). Thus, crowding is an important feature of sex segregation that requires its own index.

One caution for prospective students of crowding. If official statistics do not report female-dominated fields in as much detail as male-dominated fields, women will appear more crowded than they actually are. Researchers familiar with occupational data recognize that the number of categories tabulated in government publications does not always parallel the degree of task differentiation. In some cases, fine distinctions among male-dominated occupations seem more common than among female-dominated ones. For example, in the United States, detailed occupational data classify machine operators into forty-nine different specialties, based on the type of machinery they operate, while the two headings for nurses note only educational credentials and reflect none of the increasing specialization of nurses in American hospitals. The occupation 'truck driver' is reported as three specialties: 'Truck drivers, heavy,' 'Truck drivers, light,' and 'Industrial truck and tractor equipment operators,' while secretaries are reported in a single classification and are not differentiated by industry or responsibility level. Reliance on such data will inflate the occupational crowding of women that no doubt exists in the United States economy.

The principal dimension of segregation that is the focus of most research is the degree to which men and women are distributed unevenly across occupations. This concept is typically measured by D, but other measures have been proposed. Watts (1993) complains that D is faulty because it indicates how many women would have to change occupations to be distributed in the same manner as men, with no replacement. 'No replacement' means that the resultant distribution will not correspond to the occupational structure we currently observe. His alternative is to use the Karmel-MacLachlan index, which factors in replacement when measuring the distance to be moved to achieve complete integration.

Are Watts' criticisms of D sufficiently compelling and is his alternative sufficiently

attractive to abandon D? I think not. Watts's criticism of D is not a serious objection, and his alternative creates more problems than it solves. Watts's basic objection is that he finds intuitively unappealing the procedure of constructing a counterfactual distribution against which to measure the currently observed level of segregation. However, as we will see, any index of segregation will involve measuring observed distributions against hypothetical alternatives. 'Moving' women or men to create an equal distribution, as D does, is a sensible way for measuring how much segregation one currently observes, and has the attractive property of symmetry: the same proportion of men or women would have to be moved to create an equal distribution.

Watts's choice, the Karmel-MacLachlan index, is sensitive to the proportion of men and women in the labour force, since the proportion of men or women that need to be moved depends in part on the size of the group that needs to be replaced. This is a crippling limitation for an index of occupational segregation by sex, limiting both temporal and cross-sectional comparisons. Because the proportion of women employed has increased over time in most countries, the K-M index will tend to rise, not because segregation *per se* has increased but rather because the numbers of women who need to be reallocated has grown. (Karmel and MacLachlan (1988) themselves were well aware of this property of their index, although they do not seem to fully appreciate the significance of this drawback.) In contrast, D is independent of the proportion of women in the labour force. If a researcher wants to answer the question 'Does segregation rise as women enter the labour force in increasing numbers?' he or she will need a measure that does not grow as women's numbers in the labour force grow.

Watts argues that changes in the K-M index can be decomposed into the proportion of change due to the increase in the representation of women and the increase due to other factors. I have three reactions to this claim. First, the approach Karmel and MacLachlan propose for decomposing the K-M index can be applied to D and most other indexes as well. I would rather decompose a conceptually attractive index rather

than a conceptually unattractive one. Second, decomposing changes in the K-M index requires creating counterfactual distributions, purged of changes in gender composition and occupational size. Why are these hypothetical distributions any more intuitively appealing than the ones used to calculate D? Third, Watts shows that for three examples, change over time as measured by the K-M index, when purged of the gender effect, 'is approximately equal to the total percentage change of the ID (index of dissimilarity) . . .' (1992: 483). In other words, once the appropriate adjustment in the K-M index is made to remove the effect of the changing gender composition of the labour force, one obtains the same results as produced by D. Why not just stick with the easy interpretability of the index of dissimilarity?

Watts makes the case for the K-M index by criticizing other measures, often successfully, but also by criticizing other students of segregation. In this regard, I must object to two charges Watts levels at me. First, he writes 'The use by Jacobs (1989) of the figure of 50% as some sort of target (or ideal) embraces a strong normative element. There is no convincing argument that women should comprise 50% of employees.' I have two reactions to this comment. First, by introducing this idea in a discussion of measures of segregation, Watts implies that I recommend a 'fixed standard' approach for measuring changes in the level of sex segregation over time. This is not the case. I did use a fixed set of occupations in studying career mobility between male-dominated, sex-neutral and female-dominated occupations. The study of occupational mobility involves different issues from those involved in measuring trends in segregation over time. In studying mobility, it is clear that one should separate the case of individuals moving across a fixed set of occupations from a situation in which the occupations themselves changed their sex composition over time.

Second, Watts's extrapolation from my measure to a notion of social ideals is unwarranted. It is possible to use a fixed standard without any normative implications. One perfectly reasonable definition of a mixed occupation is one in which men and women are

more or less equally represented. This statement does not imply anything about how many women should be working. Similarly, one can say with confidence that most occupations in the United States are predominantly white, since blacks represent only twelve per cent of Americans. Saying this does not imply anything about how many blacks there should be in the country.

There are theoretical reasons for being interested in the absolute representation of men and women in a given situation, whether it is in a firm, a job or an occupation. For example, Kanter (1977) argues that the proportions of men and women in a situation matter for social behaviour. Kanter is not concerned with women's representation versus their labour force representation but rather their representation versus men. To test Kanter's ideas regarding the relative size of dominant and subordinate groups, one ought to examine sex composition unadjusted for the sex composition of the labour force. To summarize, using the proportion of women employed in occupations with some fixed value, say 70 per cent or more women, is not a useful way of studying trends in the level of sex segregation, although it might be appropriate given particular theoretical questions.

His second claim, in footnote 5, is that I, along with others, 'have misinterpreted the meaning' of the index of dissimilarity. Given the placement of this footnote, I can only assume that the charge is that I do not use the words 'without replacement' in my discussion of this index. I do not think this qualification is necessary or important, nor do I believe it justifies the claim that I misinterpreted this measure.

Another proposed measure is the sex-ratio index that groups detailed occupations into two groups, a male-dominated sector and a female-dominated sector (Siltanen, 1990). The difficulty with this approach is that the shape of the distribution is lost in the process of aggregation. It is like studying income inequality by collapsing all income data into two scores, below average and above average. And if I were to aggregate in this manner, I would prefer to use an odds ratio as my statistic.

A third aspect of segregation is the degree of intergroup contact, in other words, the chances of men and women sharing the same occupation. This aspect of segregation reflects the numbers of women and men in the labour force, and studying it requires violating James and Taeuber's (1985) principle of composition invariance. I borrowed the idea of studying the probability of men and women sharing an occupation from studies of the probability of sharing a residential neighbourhood. Intergroup contact measures (designated P^* by Lieberson, 1980) reflect both the level of segregation and the representation of each group. Moreover, women's chances of sharing an occupation with men differ from men's chances of sharing an occupation with women. One striking result of the growth in women's labour force participation is that women's chances of sharing an occupation with men have declined (as working women's numbers increase, women's chances of sharing an occupation with another woman increase) while men's chances of sharing an occupation with women have increased markedly (Jacobs, 1989; Jacobs and Lim, 1992). The two groups thus differ in how they experienced the same changes, an aspect of segregation revealed by measures of inter-group contact.

What of the need to separate vertical and horizontal segregation? Most researchers examine sex segregation because it is related to the gender gap in wages, whether or not they rely on crowding as the cause of this disadvantage. But why not simply measure the average occupational status of men and women? If women earn less than men because they are employed in less desirable occupations, these occupations should have lower social standing. Why not simply describe the average occupational status score of men and women?

The answer is that occupational standing, as it is measured by the prestige index and by Duncan's socio-economic status scores, does not adequately capture gender inequality (see Jacobs and Powell, 1987, for a more detailed discussion of this issue). In the United States, women have approximately equal occupational status to men (they tend to score a few points higher than men on the SEI index and a few points lower on the prestige index). Men and women are highly

segregated by sex within all socio-economic strata. In my view, occupational segregation by sex is a type of social stratification which is orthogonal to the socio-economic stratification studied by most social mobility researchers. Furthermore, I have shown that the process by which sex segregation is reproduced is fundamentally different from that observed for socio-economic stratification (Jacobs, 1989).

In this specific sense, sex segregation taps a 'horizontal' aspect of stratification, in that it is not principally related to the vertical dimension captured by socio-economic and related measures. It is not horizontal, of course, in that female-dominated occupations have lower authority, autonomy, and income than male-dominated occupations with similar overall social status.

Hakim (1981) correctly points out that segregation measures do not distinguish between horizontal and vertical segregation. But that is just the point: researchers turned to segregation as a concept because a simple vertical ranking failed to do the job. Brinton and Ngo (1993) make a similar point in comparing sex segregation in Japan and the United States. They seek to explain the puzzle of lower sex segregation in Japan despite the dramatically restricted opportunities for Japanese working women. Their convincing resolution of this puzzle is that blue-collar occupations are more integrated by sex in Japan than in the United States. In a companion paper (1992), they recommend a segregation measure weighted by occupational status to adjust for this difference. My problem with this particular solution is that if the status scores were not biased with respect to gender inequality then we would have little need for the segregation measure.

My own recommendation in this area for both temporal and international comparisons is to compute more than just a single summary measure of segregation. I advocate following Gross's approach (1968) of providing measures of segregation within each of 10 major occupational strata along with a summary measure. But keep in mind Semyonov's observation that women may be losing representation in a strata—craft work, for example—even though there is no change in their distribution within

the strata. For measuring segregation across broad strata, odds-ratio are a logical choice (Semyonov, 1992; Charles, 1992).

A few concluding thoughts. Firstly, the more detailed the units of analysis, the better. Recent research indicates that in the U.S. job-level segregation explains much more of the sex gap in wages than occupational-level segregation (Petersen and Morgan, 1992; Tomaskovic-Devey, 1993). Approaches oriented to examining 10 major occupational categories are likely to be informative for only the crudest comparisons and as a most basic indicator of the direction of change over time (Jacobs and Lim, 1992). Secondly, for extremely detailed data, such as job level data, investigators should consider following the lead of Baron and his colleagues (Baron, Mittman, and Newman, 1991) in using the Theil index. When there are only a handful of cases per category, the index of dissimilarity will be somewhat inflated by random variation, a difficulty the Theil index avoids. Thirdly, I believe that studies of trends over time within countries are more likely to be informative than cross-sectional comparisons across countries. Cross-national comparisons are fraught with difficulties in occupational measurement, and in compositional differences in the labour force. Fourthly, multiple measures for a country, stratified by broad occupational strata and full-time versus part-time employment, help to avoid most of the apparently anomalous international comparisons produced by the index of dissimilarity. Short of having a more satisfactory measure of 'vertical' occupational inequality, this is the most appropriate way to pinpoint the contours of segregation within the occupational structure. Employing alternative indices on labour force aggregates is unlikely to correct these difficulties. Fifthly, WE index used in the 1985 OECD report has little to recommend it. On statistical grounds it is far less attractive than the index of dissimilarity. Similarly, studies that examine the per cent of women in occupations with more than 70 per cent women (or some other fixed percentage) may be appropriate for particular theoretical questions but are not appropriate as a comparative measure of sex segregation. Finally, more attention should be paid to the dynamic processes which reproduce

or attenuate sex segregation rather than to developing new measures of the static level of segregation.

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AUTHOR'S ADDRESS

Population Studies Center, University of Pennsylvania,
3718 Locust Walk, Philadelphia PA19104-6298, USA.

Manuscript received: June 1993.