

women move as freely in urban space as most men, and we worry about the strategic implications of claims to this effect.

References

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Reply to Susan Hanson and Geraldine Pratt

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The collaborative work of Susan Hanson and Geraldine Pratt has contributed a great deal to our understanding of the gendered nature of local labor markets in the United States. I feel particularly fortunate to have had them write a commentary on my paper and welcome the invitation to engage in the ongoing debate about the geographies of women's paid employment. In many instances, Hanson and Pratt and myself are in general agreement, but in others I think that they have misinterpreted my argument. It seems appropriate that in my reply I make explicit what I sought to achieve in my paper.

The purpose of my paper was *not* "to rebut a well-established empirical regularity (that women's work trips are shorter than men's)" (Hanson and Pratt 1994:500). Nowhere in my paper did I make this claim. Indeed, as Hanson and Pratt rightly point out, my data do not support this. Although I did discuss gender differences, most of my attention focused on differences *among* women. I did not reject the spatial-entrapment thesis out-of-hand, but, in fact, called for a more finely tuned, nuanced,

and theoretically informed consideration of it. I argued that "spatial fixity *does* lie at the heart of women's (and men's) experiences of paid employment, but the spatial-entrapment-of-women thesis, as it presently stands, is an over-generalization and over-simplification (England 1993:239). I approached my task by attempting to tack back and forth between empirical and theoretical inquiry, so my paper "considered a certain amount of theorizing alongside the presentation of empirical results" (p. 239). It was within this framework that I offered "an alternative conceptualization of the spatial-entrapment thesis grounded in the acceptance of people as knowledgeable agents" (p. 239). This alternative conceptualization involved exploring "the links between women's commutes and the extent to which they are enmeshed into a pre-existing and evolving web of localized relations [because] women have varied reasons for their choices of paid work-place and residence, such that multiple roles need not imply a short journey-to-work" (p. 239–240). In short, my aim was to refine, not reject, the spatial-entrapment thesis.

The empirical portions of my paper were tightly focused on a critical examination of the spatial entrapment of suburban women as an explanation for the changing geography of office employment in Columbus (and so my investigation was embedded not only in the literatures of gender and commuting and the geographies of women's paid employment, but also in the literature on the suburbanization of office work). As such, I find it curious that Hanson and Pratt are troubled by my choice of samples. Given my research question, I do not believe that it would have been appropriate to collect data that were "representative of the population of the Columbus metropolitan area or even Columbus suburbs" (Hanson and Pratt 1994:501). The samples "suffer from selectivity bias" because that is *precisely* what I intended. It would have made no sense to target Columbus' blue-collar, manufacturing suburbs (except as a contrasting case study, which might have yielded some interesting results). The research question dictated my choice of data and methods. So in order to adequately explore my research question, I was compelled to pick those suburbs that were popular destinations for suburbanizing offices as well as for firms that employed large numbers of clerical workers. Indeed, if anything, my samples are "biased" in favor of the spatial-entrapment thesis. The case-study suburbs are inhabited by women that, according to census measures, constitute the preferred pink-collar labor supply for offices.

I intended my paper to be a theoretical engagement with my empirical study as one of a number of studies. Accordingly, let me make explicit a goal that was implicit in the original paper and that, I think, underlies Hanson and Pratt's misinterpretation of my argument. There are a range of approaches to research, a variety of research questions to be asked, and an array of methods to choose from (as I indicated in my paper, I am a proponent of triangulated data and methods). Interwoven throughout my paper was a consideration of what might be the most appropriate manner in which to conduct research on the geographies of women's paid employment. Is it through generalizations based on supposedly representative samples? Or, through conceptualization, aided by intensive interviews, of how women, in light of contemporary gender identities and relations, go about constructing their daily geographies? My

commuting studies added to the bewildering range of findings regarding commuting differences among women (I outlined the contradictory nature of previous findings on page 230 of my paper). In thinking about this dilemma, I drew on intensive interviews to argue that "women's journeys-to-work should be re-conceptualized as an effort to juggle a multiplicity of overlapping and often contradictory roles and spatial factors. Essentially, the sociospatial system that a woman in the paid labor force operates is not a closed one. Her work-place location is affected by more than just her place of residence and vice versa. There are other important spatial factors and these may be manipulated so as to realize all her roles, but a short journey-to-work is not necessarily the only possible spatial outcome of this" (p. 237). What I was getting at here was: 1) a short commute is only one possible strategy that women may employ to deal with multiple roles; and 2) there is no necessary relationship between multiple roles (or the lack thereof) and the length of a woman's commute. Or, as Sayer (1982) pointed out over ten years ago, empirical regularities do not necessarily indicate the causes of those regularities.

Following on from this, it seems to me that Hanson and Pratt and I have a difference of opinion about exactly *what* the spatial-entrapment thesis is. For me, it is more than simply that women, in comparison to men, have shorter commutes and more spatially limited job search areas. In addition, I see the spatial-entrapment thesis as relating to the spatial implications about a set of widely held assumptions and expectations about women's roles as well as the realities and complexities of women's everyday lives. It should now be clear that it was this broader interpretation of the spatial-entrapment thesis that formed the nucleus of my paper. Moreover, I do not see the spatial entrapment of women as merely the length of their commute (relative to men, or even other women). In my paper I was attempting to promote an interpretation of entrapment that went beyond women's commutes to consider the *totality* of women's experience. Perhaps Hanson and Pratt's concern that I am promoting a "politically dangerous" position is that they seem only to consider spatial-entrapment as absolute commuting distance (or time). Conventionally, a short commute has been basically viewed as a reflection

of the oppression of women. Oppression (like capitalism) is metamorphic and so is constantly changing shape. Simply because some women have lengthy commutes does not mean that the oppression of women has been eliminated; indeed they may feel even more oppressed.

The general aim of my paper was to alert geographers to the taken-for-granted nature of the spatial-entrapment-of-women thesis as it presently stands. I certainly appreciate Hanson and Pratt's kind words regarding my efforts to "describ[e] the real-life complexities behind the mean, median, and modal travel distances" (Hanson and Pratt 1994:501), and I am delighted that they find my "discussion of the spatial-entrapment-of-women . . . thoughtful and valuable" (Hanson and Pratt 1994:501). In closing, let me reiterate that I am grateful to them for providing me with the opportunity to clarify and sharpen my original argument, and to make explicit the aims of my paper. I think that despite our differences, Susan Hanson and Geraldine Pratt and myself do agree that "what

is true for the population as a whole is not true for distinct sub-populations," "that generalizations must be made with care, that all women are not the same, and that differences among women's experiences must be carefully unravelled," and that none of us believe that "most women move as freely in urban space as most men" (Hanson and Pratt 1994:501-502).

References

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