The Effects of Job and Housing Location on Race/Gender Wage Differentials in Milwaukee: Testing the `Network Hypothesis'

by

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ABSTRACT

This study considers the joint role of job and housing location as they affect the earnings of different race and gender groups. Building on Wial's (1991) case study of networks in Boston which distribute good jobs, the `network hypothesis' suggests that the channels which distribute high wage jobs inside Milwaukee county are connected to individuals who are predominantly white males and live outside of the county, thereby limiting access for others partly on the basis of race and gender and partly due to job and housing location. Data from the 1990 U.S. census are used to test this hypothesis. The results are consistent with the network hypothesis.

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As the sun rises over Lake Michigan, a portion of Milwaukee's inner city is shadowed by the buildings of the central business district. Yet even the most casual observer could not fail to notice that the individuals coming to work in these buildings do not resemble the inner city residents living nearby. Skin color marks the workers as commuters from outside the community, and their attire suggests the commuters are wealthier than local residents. Implicit in this observation is the possibility that differences in earnings by race and gender may have dimensions which are specific to job and housing location. The analysis here uses data on Milwaukee from the 1990 U.S. Census to analyze this issue.

Until recently, there was little in the way of theory or evidence to suggest a link between discrimination and job or housing location. In the voluminous literature on discriminatory wage differentials, only a handful of papers consider the effects of housing location, finding that blacks and whites experience positive returns to suburban residence.[1] Surprisingly, where this finding has been unearthed, no compelling explanation emerges from the standard theory (Price and Mills, 1985, p. 9). One might think that the literature on labor segmentation, initiated in the hope of explaining the adverse economic circumstances of urban blacks in the U.S., might offer more, but both job and housing location have historically been ignored in this literature (e.g., Edwards, Reich and Gordon, 1975; Gordon, Edwards and Reich, 1982). This situation changed with the publication of Wial's (1991) study of mobility among young males in three ethnic neighborhoods of Boston.

In interviews with both employees and employers, Wial finds evidence of employment networks which funnel young men from `bad' to `good' jobs, which he views as movement from secondary to primary segment jobs. In these networks, individuals strive to use contacts with current employees holding good jobs either to obtain timely or otherwise valuable information about particular

job openings or to gain a personal recommendation to the boss. Employers support these networks by either hiring directly through the network or, more commonly, by hiring the first qualified applicant (who is often both first and qualified by virtue of network membership).

Wial's study explicitly suggests that job networks are specific to *ethnic* groups and to *residence*. By excluding women, the study implies these networks are *gender* specific as well. Further, Wial finds individuals in his sample limit their job search to employers already connected to the network, hinting that *job location* may also be specific to each network. It is the role of these four potentially relevant characteristics of segmented labor markets which drive the analysis below.

Building on Wial's findings, I hypothesize that white males, living outside of Milwaukee county while holding good jobs inside of the county, maintain a network which distributes these jobs. This possibility is labelled the `network hypothesis.'[2] For a stereotypic example, suppose we found a whites-only country club located in the suburbs, excluding women from desirable tee-times, having members who are executives in industry, banking or government within the city, and where jobs are discussed during golf games. If such discussions occur, we would not be surprised if white, males residing in the suburbs tended to fill openings for good jobs in the city.

This stereotype, however, misses the far broader channels through which such networks can develop. For example, individuals holding good jobs in the city and commuting from the suburbs may meet through activities relating to school, church, or community organizations in the suburbs, or during lunch or other activities surrounding the workplace. In such settings, either the organization or activity may be segregated or individuals might segregate themselves within the activity by gender and race for informal discussions during which information about jobs may be shared or recommendations of specific individuals for jobs may be given. More directly, a study of employment in downtown Milwaukee conducted in 1991 and 1992 finds the vast majority of new high wage jobs in the revitalized downtown area being held by white suburbanites and the corpus of new low wage jobs being provided

to city residents.[3] While education and experience may provide part of the explanation for these findings, the network hypothesis may also be salient.

If the network hypothesis is indeed relevant, this raises the empirical question of how we expect race, gender, and job and housing location to interact in a segmented labor market. Stated in polar terms, are women and people of color locked out of good jobs by virtue of who they are, where they live or where they work?

Fortunately, a well-developed literature based on the work of Oaxaca (1973) exists which is intended to permit the decomposition of such effects while controlling for a variety of individual characteristics which may also influence wages. In what follows, we first discuss these techniques, including the recent approach suggested by Neumark (1988), and general methodological issues such as sample selection bias. The proceeding section describe the data, variables for the analysis, and the Milwaukee context. In the results section, we test whether housing and job location have distinct or joint roles in the wage determination process. The network hypothesis suggests a joint effect will be identified. We then analyze the race/gender component of these effects, basically asking whether job and residence location effects are distinct for white males as opposed to all individuals in the labor market. The conclusion provides a summary and discussion of policy implications from the study.

Methodology

Here we discuss the Oaxaca decomposition, Blinder's use of the decomposition, and Neumark's approach, focussing particularly on how these relate to segmentation theory, concluding with some technical considerations.

To understand Oaxaca's (1973) method, suppose there are two major sources of earnings variation: differences in individual productivity, and differences in how individuals with given productivity are treated. Further assume that differences in individual productivity reflect human capital (Becker, 1975), and that we can proxy human capital by variables included in standard earnings

equations (e.g., years of schooling, experience, occupation). If we then use those variables in a log-linear wage regression, we can ascertain what the returns to human capital would be in the absence of discrimination. The average difference between this hypothetical wage and actual wages, or the residual, for particular race/gender groups can in turn be attributed to discrimination.

To use this technique requires some judgement about the state of a necessarily hypothetical `discrimination free' labor market. That is, what returns to human capital would exist if discrimination disappeared? Oaxaca personally resolved this issue by positing that the elimination of discrimination would yield a labor market where either all are treated as members of the privileged group, or where all are treated as the less privileged group (or somewhere in between). In the first case, wages for the low wage group are projected to rise to current levels for comparable high wage employees. In the second case, wages for the high wage group fall to current levels for comparable low wage employees. The resulting estimates of discriminatory wage differences are respectively what Oaxaca considers upper and lower bounds to the extent of discrimination. Blinder (1973), however, suggests that it is more reasonable to posit that the elimination of discrimination would result in all labor market participants being treated as if they were in the privileged group (this yields Oaxaca's upper bound figures).

Recently, Neumark (1988) has argued that if employer preferences are the sole source of discrimination, then a single figure can be obtained for the discriminatory component in wage differentials. He posits that the wage bill for the high wage group will fall by the same amount the wage bill will rise for the low wage group until each receives the current average return on human capital. A feature which has attracted interest in the Neumark approach is that it can yield discriminatory wage figures which lie outside of Oaxaca's `lower' and `upper' bound figures. For present purposes, however, it is more important to note that the Neumark approach assumes that productivity and hence the total wage bill would remain unchanged if discrimination were eliminated: discrimination is a zero-sum game.

Since the network hypothesis derives from segmentation theory, while these decomposition techniques are based on human capital theory, the preferred technique for testing the network hypothesis is not obvious. For two distinct sets of reasons, we use the Neumark and Blinder approaches, ignoring Oaxaca's lower bound. The first set of reasons concern how segmentation theorists view the productivity effects of segmentation. Many argue that segmentation is profitable but not productive and hence that wages for *all* groups of workers would rise if segmentation were eliminated (Reich, 1981). If this argument is correct, then even Blinder's method will tend to understate wage losses due to discrimination. Since Blinder's approach, however, yields the largest losses, it is reasonable to use this to generate `upper bound' estimates here. Other researchers argue that segmentation confers concrete benefits on white males, benefits which would be lost in the absence of segmentation (Schulman, 1990; Amott and Matthaei, 1991). This argument is close to Neumark's assumption that discrimination is a zero-sum game, so its elimination would benefit some groups and make others worse off. We therefore take the Neumark approach as providing our `lower bound' estimates here.[4]

A second set of reasons for using the Blinder and Neumark approaches concerns what they actually do with the data. The Neumark approach uses data on all groups to calculate the returns to various characteristics, while the Blinder approach uses only data from the most privileged group (in our case, white males). In considering the network hypothesis, the Neumark method can then inform us as to the average returns to job and housing location, while Blinder's approach yields information on the average returns to these characteristics for white males. By comparing these results, we can therefore gain insight into the question of how job and housing location interact with race and gender. On the one hand, if race and gender are all that really matter, we would expect that wage differences attributable to job and housing location for the entire sample will not appear in the white, male subsample. On the other hand, if race and gender are irrelevant, the returns to job and housing location will be identical in our comparison of results for the entire sample and for white males alone.

Within the context of this general strategy, we treat the data conservatively. The reason for doing so is that the major variables related to the network hypothesis -- race, gender, job and housing location -- are likely to be both closely associated with each other and correlated with human capital controls. In the absence of a conservative approach, it could therefore be argued that any results attributed to the network hypothesis are in fact due to omitted variables. In response to the possibility for collinearity with omitted variables, we include a very extensive set of control variables. It is generally believed that reliance on a virtual laundry list of controls leads to underestimation of discriminatory differences (Cain, 1984), in which case the results will indeed be conservative. Additionally, to control for the possibility that wage earners systematically diverge from others in the sample population, we follow Reimers (1983) and employ a sample selection correction to the wage regressions (Heckman, 1979; Greene, 1981). Whether our extensive controls or the sample selection procedure in fact yield conservative results is later checked by rerunning the regressions after removing various controls and (separately) without the correction for sample selection.

The Data and Milwaukee Context

This section describes the sample used for the analysis, discusses the variables used in the analysis, then outlines conditions in Milwaukee at the time of the 1990 Census.

As part of the Decennial U.S. Census of Population and Housing, the Bureau of the Census collects extended information from 16% of the population using the `long-form' census. The Public Use Microdata Samples (henceforth PUMS) data employed here represent five-sixteenths of long-form respondents to the 1990 census, so provides a 5% sample from the population (Bureau of the Census, 1992a).

The main sample used in the wage analysis is limited to respondents at least 16 years of age who self-reported as a civilian wage or salary employee with positive earnings in the previous year. Following Reimers (1983), we exclude respondents who are self-employed, unemployed, out of the

labor force, members of the armed forces and retirees.[5] Out of 32,418 respondents in the Milwaukee area, 20,650 or around 64%, meet the criteria for inclusion in the sample of wage earners. The excluded group of 11,768 respondents are accounted for in the sample selection procedure.[6] Note further that all figures reported below are weighted.[7]

As is standard, the dependent variable takes the form of the natural log of hourly wages which, for Census type data, is calculated as the log of the ratio of usual weekly earnings to usual weekly hours (e.g., Reimers, 1983; Neumark, 1988; Belman and Heywood, 1991; Leigh, 1991).[8]

Regarding variables for testing the network hypothesis, the PUMS data contains two geographic variables, one concerning residence, and another concerning job location. For the prior, the Milwaukee metropolitan region is broken into six PUMS areas, two covering three counties outside of Milwaukee (Ozaukee, Washington and Waukesha), one for the area inside of the county but outside of the city of Milwaukee, and three more for areas inside of the city, one of which can be characterized as capturing the inner city, although the indicator is not perfect. For job location, the data set is limited to information on whether one holds a job inside or outside of the county of Milwaukee. Given the nature of the data, we classify the metro region external to Milwaukee county as the suburbs, and further separate out the inner city of Milwaukee for residence, ignoring the other geographic distinctions permitted by the data. An interaction term is added later for the joint characteristic of residing outside of, but working inside of, Milwaukee county.

As mentioned earlier, we employ a large number of control variables. Indeed, the controls include virtually all of those covered across a variety of relevant studies excepting those not salient to a cross-sectional study of a single city (e.g., city population).[9] Standard human capital controls included here are potential experience (years of age less years of schooling less 6, entered in quadratic form) and years of formal schooling. To account for potential sheepskin effects, we supplement the years of schooling variable with splines for holding a high school diploma and for a bachelor's degree. Relatedly, although we do not know whether individuals are enrolled in school full-time, we do include

a dummy variable for current school attendance, and we include dummy variables for whether the individual usually works less than 35 hours per week and for individuals who were unemployed or out of the labor force for part of the previous year. Information on potential experience is supplemented by a dummy variable for individuals who moved to Wisconsin since 1985, and a dummy variable indicates non-U.S. citizenship. We also introduce dummies for whether a language other than English language is always or sometimes spoken in the home, for military veteran status, and for government employment. Additionally, the individual's marital status is indicated by dummies for presently married (not separated) and for never married, leaving separated, divorced and widowed individuals as the residual group. Relatedly, we include a variable for the number of dependent children living at home. Control dummies are also included for five of six possible occupational groups, and for eight of nine possible industries. While this list of control variables is extensive, union status is not provided in the data, even though evidence strongly suggests that unions affect wages (Freeman and Medoff, 1984; Leigh, 1991).

To identify the sample selection procedure, we include two variables only in the probit for whether one is a wage earner or not. These are the amount of family income exclusive of wage and salary earnings for the respondent, and a dummy variable for whether the respondent received public assistance during the previous years (actual amounts of public assistance are subsumed in family income). While each variable could conceivably influence earnings, it seems reasonable to believe their main effects are on employment *per se*.

Turning to race/gender categories, while gender divisions are clear in the PUMS data, racial classification is not. The latter information is contained in two separate questions, one on racial or ethnic heritage, and another on whether the respondent has a latin background.[10] Following Reimers (1983), we respond to this problem by defining all respondents with a latin background as a distinct racial category, and exclude these respondents from alternative race categories. The remaining observations are classified by gender as white, black, asian, or native american. While these categories

are not all inclusive, they come very close, covering 99.96% of respondents in the sample.[11] Further note there is a fair amount of heterogeneity within these groups (Sowell, 1985; Amott and Matthaei, 1991). Nonetheless even these splits result in fairly small samples, particularly for asians and native americans (see Table 1), suggesting finer divisions should not be applied to this sample.

Limited information on the sample of 20,650 wage earners, according to race/gender categories, is provided in Table 1. While the figures and our analysis concern Milwaukee, most figures are at least loosely linked to the general patterns one would expect to see in broader samples.

Looking at the distribution of employees by race/gender (the weighted percentage figures listed under each group's titles, these are reasonably close to roughly comparable figures for the U.S. using data on all long-form Census respondents.[12] For example, white males make up 45.5% of the Milwaukee area's body of civilian employees, while the comparable national figure is 44.1%. Similarly, black males are 4.1% of civilian employees in the Milwaukee area, and 4.7% nationally. Other figures are also quite similar except that the representation of asians and latin employees (for both genders) in Milwaukee is less than one-half that for the nation, and white women are more highly represented in Milwaukee (41.7% of local employees versus 36.8% nationwide). Educational attainment also reflects a pattern similar to the for the entire nation, with Asian males holding the highest levels, followed by white males, with white females and asians females close behind, and with all remaining race gender groups holding lower levels (Amott and Matthaei, 1991). Divergence in potential experience largely reflects the greater age of whites relative to others in the Metropolitan area.

There undoubtedly is, however, a substantive difference between the geographic distribution of residence and jobs in Milwaukee relative to the rest of the nation. As suggested by race/gender figures for location of residence in Table 1, Milwaukee is highly segregated at present. These figures reflect a historical pattern. Indeed, as early as 1968, Milwaukee had earned a sufficiently strong reputation as a segregated and racially troubled city to warrant coverage in the Kerner Commission's Report (National Advisory Committee on Civil Disorders, 1968). More recently, Massey and Denton (1993, p. 77) find

sixteen major U.S. cities warrant the label `hypersegregated'. Milwaukee is not only among these cities, but is also more segregated than the average hypersegregated city according to four of their five segregation indices.

If Milwaukee is more segregated than other cities in the U.S., then we might expect to find stronger evidence for the type of network linkages in this data relative to what a broader study would reveal. Unfortunately, to the extent other U.S. are generally becoming more segregated, as Massey and Denton (1993) suggest, then results using Milwaukee data may tell us where other cities are headed at present.

Results

Initial regressions of both the Neumark and Blinder type included all independent variables and employed the sample selection correction. As expected, using either method, the results showed that inner city residence negatively influences earnings, suburban residence has a positive effect, and employment inside Milwaukee county has a positive effect (results not reported here).

As our first test of the network hypothesis, we inserted an interaction term for whether the employee both lived in the suburbs and worked in Milwaukee county. Log-likelihood Chi-squared statistics for the additional explanatory power of the interaction are significant at the one percent level for both the Neumark and Blinder equations, and the variable attracts a positive coefficient, consistent with the network hypothesis.[13]

Results for regressions including the interaction terms are shown in Table 2, with results for industry and occupational variables excluded for brevity. Interpreting the coefficients as the percentage change in the dependent variable for a one unit change in the causal variable, the results are generally typical for this type of study, showing around a 4.5% wage return for each additional year of education, positive (though not uniformly significant) returns to high school and college graduation. Potential experience also demonstrates standard curvilinear effects, with wages first rising with

experience up to a maximum of around 30 years for the Neumark method and around 37 years for the Blinder method. Given the Blinder type equation includes only white males, this finding suggests that returns to experience may rise over a much longer period for white males than for other wage earners. As also expected, returns to intermittent or part-time work are negative, and we find a large and significant bonus for marrying at some point for white males (the negative never married coefficient under Blinder Method), consistent with evidence suggesting white males often gain economically from marriage (Folbre, 1982).

Considering the critical variables of interest for testing the network hypothesis, we find that living in the inner city consistently reduces earnings by between 15% and 19%. The fact that this result holds in the Blinder type regression implies that even white males suffer adverse effects from inner city residence, a result reported earlier by Price and Mills (1985). Neither living in the suburbs nor working in Milwaukee generate significant coefficients, but the interaction term for both characteristics is large and positive. For the overall sample, there is an approximate 17.5% gain from this status, while for white males the gain is estimated to be an even larger 25.3%.

We next make the role of race and gender explicit through simulations using the regression results reported in Table 2. Column (1) in Table 3 reports the average hourly wage of the ten race/gender groups addressed here.[14] Quite typically for this type of data, the pattern suggests white females earn around two-thirds as much as white males, with black males being slightly above and black females being slightly below the figures for white females. Even lower wages by gender are located for native latins and for native american women. Surprisingly, however, we find asian males earning less than white males and similarly asian females earning less than white females, while national data usually finds asian earnings (by gender) above those for whites (Amott and Matthaei, 1991, pp. 342-4). A check of overall earnings data suggests that the poor economic circumstances of four sizable asian groups in Milwaukee -- Laotian, Hmong, Pakistani and Hawaiian individuals -- account for this disparity.[15]

Column (2) of Table 3 reports the hypothetical wage the race/gender groups would receive if all were treated equally, allowing their average characteristics to remain as at present and using Neumark's assumptions regarding the workings of a non-discriminatory market. For white males, wages are projected to fall by over $1 per hour, and wages for black and native american males are expected to fall by much smaller amounts. For all other groups, wages are predicted to rise in the absence of discrimination, with projected increases of under $1 per hour for white, black and latin females as well as asian and latin males, and larger increases for asian and native american females. Looking at traditional comparison groups, the figures suggest that of the $3.92 difference between white males and females, $1.93 is accounted for by different characteristics, and the remaining $1.99 per hour differential (50.8%) is attributable to discrimination. Stated differently, $.84 of the white gender gap is due to discrimination against white women, and an additional $1.15 is due to discrimination favoring white males. Comparing white and black males, $2.42 of the $3.26 per hour average differential can be accounted for by objective differences, suggesting that around 26% of the differential may be due to discrimination.

If the network hypothesis is correct, however, then what are labelled `objective' differences above include differential access to job networks. To ascertain the potential effects of these networks, we simulated the effect of giving each race/gender group the locational attributes (residence and job location) of white males in a setting with non-discriminatory wages. The results for these simulations are reported in Column (3) of Table 3. Not surprisingly, projected gains for white females are minimal because their locational distribution is quite similar to that for white males. For other race/gender groups, however, estimated returns are substantial, ranging from $.60 an hour for latin males to a low of $.14 for native american males, with most groups falling in the range of $.30 to $.45 per hour. Simulations (not shown) for all non-white males reveal an average gain of $.175, which translates into a $400 loss of wage and salary income per year for each non-white, non-male employee.[16] Of course, that figure is dominated by the presence of white women in the Milwaukee area labor market. For the

average non-white, the average gain is $.513, or over $1,000 of wage and salary income per year on the basis of job and housing locational disadvantages alone.

The simulation results just discussed relied upon Neumark's model wherein discrimination is a zero-sum game. Table 4 reports comparable figures using results from the Blinder method regression (see Table 2) wherein we assume that the elimination of discrimination would yield labor markets like those for white males at present. Column (1) of the table again reports average actual wages. In column (2) we find non-discriminatory wages are projected to rise for every group except white males; of course, the method does not permit the white male wage to change. As expected, these results are generally less conservative than those for the Neumark method. Instead of attributing $1.93 of the white gender gap to different characteristics, only $.93 is here associated with characteristics *per se*. Further, the asian female wage is projected to double in the absence of discrimination, while wages for white, native american, and latin females, and for asian and latin males, the simulated wage is projected to rise by over 30% in the absence of discrimination. Contrary to this pattern, however, instead of tracing 26% of the black-white male wage gap to discrimination, only 8% of the difference is attributed to discrimination using Blinder's method.

The crucial issue for the present analysis lies in comparing simulated changes in the non-discriminatory wage for white/male job and housing location across the Blinder and Neumark methods. Comparing the difference between columns (2) and (3) of the relevant tables reveals that for all groups, the projected wage increase is larger under the Blinder method. Indeed, the increases almost double for each race/gender group excepting black males (for whom the increase `only' rises from $.56 to $.83). Indeed, for all non-white males, the average gain from switching to white male locational attributes is here projected to be $.25, or $518 per year, while comparable figures after excluding white women are $.75, or over $1,500 per year.

Recall from our earlier discussion that the difference between the Neumark and Blinder results ultimately derives from divergence in the way the labor market treats the average person regardless of

race and gender (Neumark) as opposed to the functioning of the labor market for white males (Blinder). Given this distinction, we interpret these findings to mean that networks for good jobs indeed have job and housing location-specific dimensions, but that race and gender nonetheless play a critical role in determining who gains access to these networks.

As a check on the validity of the findings, we reran both the Neumark and Blinder type regressions after dropping the sample selection correction and again after excluding the industry and occupational dummy variables (though including sample selection, see Appendix Tables). For the Neumark method, results after omitting the sample selection term are very similar to those reported in Table 3, while the discriminatory gaps and those for housing and job location were generally larger absent controls for industry and occupation, suggesting as expected that the regression approach employed here is conservative. For the Blinder method, results following the exclusions were quite close to those reported in Table 4, except the returns to white male housing and job location are larger for seven of the nine relevant race/gender groups after excluding industry and occupational dummies. It therefore seems reasonable to conclude that the results reported here provide relatively conservative estimates of the quantitative effects associated with the network hypothesis, and that these results are robust with respect to alternative specifications.

Discussion

This paper stems from the idea that good jobs are distributed through networks which exhibit race/gender as well as residential and job location dimensions. The `network hypothesis' suggests these facets of job networks are interrelated, so that the wages one receives depend partly on who you are, and partly on where you live and work. Using data on the metropolitan Milwaukee area from the 1990 U.S. census, we examined different decompositions of wage differentials to ascertain the respective roles of these various characteristics while controlling for other aspects of wage determination.

Probably the most important finding from the study is that there is a joint positive and significant effect from living outside while working inside of Milwaukee county. This finding alone is consistent with a much simpler argument not developed here: that individuals who obtain good jobs inside the city use the resulting high earnings to purchase a house in the suburbs. However, this alternative argument cannot explain the fact that whites provide a disproportionate share of suburbanites, nor can it explain the finding that white males, *ceteris paribus*, receive higher wages than members of other race/gender groups.

The latter findings suggest discrimination in wages, employment and housing markets exists, in which case the network hypothesis provides an attractive explanation for the maintenance of discrimination over time, just as Wial (1991) suggests.

The other novel finding from our analysis is that the returns to working inside while living outside of Milwaukee county are greatest for white males. For the average non-white in the sample, after controlling for both a host of potentially relevant human capital characteristics and direct wage discrimination, the yearly return to exhibiting the housing and job location patterns of white males are estimated to be over $1,000. This figure, however, is derived using data on the average returns to location for everyone in the labor market. When we focus exclusively on average returns to location for white males, this figure rises to over $1,500. For women and particularly minorities, housing and job location seem to be associated with barriers to entry into the networks which distribute high-wage jobs, but even if these barriers are overcome, the hurdle of race and gender discrimination remains.

Considering this phenomenon in a slightly different light, whether one is a white male or not, residence in the inner city is associated with a severe wage penalty which may be due to exclusion from the relevant job networks (see Table 2). Movement to suburban residence in tandem with a job inside the county improves everyone's chances of entering the network, but the possibilities are much richer for white males.

The policy implications of this study are broad. Although not discussed explicitly here, education and human capital play a substantial role in explaining race/gender wage gaps in the data. For example, even in the absence of discrimination, and with the job and residential location of white males, we estimate that the hourly wage differential between white males and blacks of either gender would remain at around $2, as would the gap between white males and native american men and women (see Tables 3 and 4). Therefore, this study suggests there is a place for education and job training policies in efforts to remedy the race/gender wage gap.

The central focus of our study, the `network hypothesis', however, suggests that improved education will not by itself eliminate race/gender wage disparities. The fact that location of jobs and housing are important aspects of these disparities implies that efforts to integrate housing may be warranted, while the additional contribution of race and gender are consistent with affirmative action employment policies.

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Footnotes

Table 1:

Average Characteristics of Race/Gender Groups

Group (# Observations) [Weighted % of total sample]	Years of Schooling (1)	Potential Experience (2)	Live in Inner City (3)	Live in Suburbs (4)	Work in Milwaukee County (5)	Both (4) and (5) (6)
White Males (9571) [45.5%]	14.12	17.56	3.1%	41.0%	62.6%	15.7%
White Females (8758) [41.7%]	13.98	17.21	3.0%	38.5%	64.1%	11.1%
Black Males	12.82	16.90	31.8%	1.2%	87.6%	0.6% (697) [4.1%]
Black Females (862) [4.9%]	13.20	16.87	30.3%	1.2%	90.5%	0.9%
Asian Males (94) [0.5%]	15.05	13.11	19.6%	20.2%	78.7%	10.5%
Asian Females (83) [0.4%]	13.95	15.56	17.7%	21.8%	78.0%	9.3%
Native American Males (45) [0.2%]	12.78	14.43	8.4%	19.5%	77.6%	10.1%
Native American Females (50) [0.2%]	12.96	17.32	20.8%	21.1%	79.6%	7.7%
Latin Males (292) [1.5%]	11.48	16.89	35.1%	15.8%	69.5%	3.1%
Latin Females (198) [1.0%]	12.34	14.99	31.1%	14.8%	80.7%	1.0%

Note: Sample of wage earners only. Number of Observations is number in actual data, weight for weighted number of observations is normalized to mean one for all descriptives and regressions in the analysis.

Table 2:

Log-Wage Equations Used for Simulations in Tables 2 and 3
(Absolute Value T-statistics in Parentheses)

Independent Variables	Neumark Method	Blinder Method
Constant	1.141 (4.613)***	1.711 (3.958)***
Years of Schooling	.045 (4.984)***	.044 (2.555)**
High School Diploma	.049 (1.271)	.078 (1.132)
Bachelor's Degree	.124 (2.424)**	.082 (.877)
Years of Potential Experience	.030 (10.590)***	.044 (7.913)***
Potential Experience Squared	-.0005 (7.570)***	-.0006 (5.258)***
Worked Part of Last Year Only	-.067 (2.728)***	-.075 (1.636)
Usually Work < 35 Hours Per Week	-.155 (5.244)***	-.152 (2.181)**
Number of Dependent Children	-.011 (.955)	-.002 (.097)
Married	.051 (1.604)	-.044 (.687)
Never Married	-.067 (1.726)*	-.214 (2.845)***
Citizen of Country Other Than U.S.	.261 (3.117)***	1.009 (5.695)***

Non-English Language Sometimes or Always Spoken in Home	.052 (1.167)	.094 (1.009)
Currently Enrolled in School	-.035 (1.031)	-.026 (.401)
Veteran of U.S. Armed Services	.193 (6.167)***	.014 (.304)
Migrated to Wisconsin During Last Five Years	-.003 (.144)	-.059 (1.588)
Public Sector Employee	.062 (1.541)	-.058 (.717)
Live in Inner City	-.158 (3.671)***	-.185 (1.827)*
Live in Suburbs	-.043 (.769)	-.140 (1.435)
Work in Milwaukee	-.037 (.480)	-.206 (1.494)
Live in Suburbs and Work in Milwaukee	.175 (2.732)***	.253 (2.328)**
Sample Selection Correction (λ)	.054 (.537)	-.145 (.771)
Adjusted R^2	.076	.068
Number of Observations	20650	9571

Notes: Where unit of measurement is not explicit, variables are all 1-0 dummies. Each equation includes eight industry and five occupational dummy variables, coefficients not reported. Significance levels: * for .10, ** for .05, and *** for .001.

Table 3:

Returns to Race, Gender, and Housing/Job Location: Neumark Method

Group	Average Wage (1)	Non-discriminatory Wage (2)	(2) plus White/Male Job and Housing Location (3)
White Males	$12.06	$10.91	$10.91
White Females	$8.14	$8.98	$9.05
Black Males	$8.80	$8.49	$9.05
Black Females	$7.44	$7.79	$8.29
Asian Males	$10.36	$11.42	$11.79
Asian Females	$6.99	$10.02	$10.35
Native American Males	$9.33	$8.85	$8.98
Native American Females	$5.94	$7.69	$8.00
Latin Males	$8.48	$9.08	$9.68
Latin Females	$6.86	$7.40	$7.89

Notes: Simulation results from log-wage equation on entire sample of wage earners, corrected for sample selection.

Table 4:

Returns to Race, Gender, and Housing/Job Location: Blinder Method

Group	Average Wage (1)	(2) plus Non-discriminatory Wage (2)	White/Male Job and Housing Location (3)
White Males	$12.06	$12.06	$12.06
White Females	$8.14	$11.13	$11.26
Black Males	$8.80	$9.06	$9.89
Black Females	$7.44	$9.33	$10.21
Asian Males	$10.36	$18.82	$19.74
Asian Females	$6.99	$15.84	$16.63
Native American Males	$9.33	$9.64	$9.88
Native American Females	$5.94	$9.05	$9.61
Latin Males	$8.48	$11.70	$12.55
Latin Females	$6.86	$9.47	$10.35

Notes: Simulation results from log-wage equation on white male sample of wage earners, corrected for sample selection.

Appendix Table 1:

Returns to Race, Gender, and Housing/Job Location:
Alternative Specifications Using Neumark Method

	Results from Regression Without Sample Selection		Results from Regression Absent Industry/Occupation Dummies	
		(1) plus		(3) plus
Group	Non-discriminatory Wage (1)	White/Male Job/Housing Location (2)	Non-discriminatory Wage (3)	White/Male Job/Housing Location (4)
White Males	$10.91	$10.91	$10.69	$10.69
White Females	$8.98	$9.05	$9.15	$9.26
Black Males	$8.49	$9.07	$8.42	$9.15
Black Females	$7.79	$8.31	$8.02	$8.72
Asian Males	$11.44	$11.83	$11.61	$12.12
Asian Females	$10.02	$10.37	$10.26	$10.73
Native American Males	$8.85	$8.99	$8.47	$8.67
Native American Females	$7.69	$8.02	$7.72	$8.16
Latin Males	$9.08	$9.66	$8.83	$9.45
Latin Females	$7.39	$7.91	$7.51	$8.16

Notes: Simulation results from log-wage equation on entire sample of wage earners, each regression replicates those for Table 2 except for single difference noted above.

Appendix Table 2:

Returns to Race, Gender, and Housing/Job Location: Alternative Specifications Using Blinder Method

	Results from Regression Without Sample Selection		Results from Regression Absent Industry/Occupation Dummies	
		(1) plus		(3) plus
	Non-discriminatory Wage	White/Male Job/Housing Location	Non-discriminatory Wage	White/Male Job/Housing Location
Group	(1)	(2)	(3)	(4)
White Males	$12.06	$12.06	$12.06	$12.06
White Females	$11.14	$11.24	$11.03	$11.16
Black Males	$9.07	$9.83	$9.29	$10.21
Black Females	$9.35	$10.14	$9.46	$10.42
Asian Males	$18.78	$19.61	$18.96	$19.95
Asian Females	$15.86	$16.55	$16.08	$16.93
Native American Males	$9.65	$9.85	$9.60	$9.86
Native American Females	$9.06	$9.55	$9.05	$9.65
Latin Males	$11.73	$12.63	$11.82	$12.75
Latin Females	$9.50	$10.31	$9.45	$10.39

Notes: Simulation results from log-wage equation on sample of white male wage earners, each regression replicates those for Table 2 except for single difference noted above.

1. See Price and Mills (1985) and works cited therein.

2. Consistent with our analysis, though rarely dealing with issues of job and housing location *per se* is the business literature on `networking' (see, e.g., Boe and Youngs, 1989).

3. See Levine and Zipp (1994). The precise figures are that 79% of jobs at new firms paying over $40,000 per year go to suburbanites, and 98.3% of the relevant job holders are white, while 74.5% of jobs at new firms paying less than $10,000 annually go to city residents, and 94.8% of all black and latin employees in these new firms earn less than $20,000 per year.

4. There is a sleight of hand in assuming that increases in productivity would translate into higher returns to human capital. If segmentation theorists had derived a model for wage determination in the absence of segmentation, we could use whatever assumptions appeared there instead. However, to my knowledge, however, no such model currently exists.

5. These exclusions are identical to those in Reimers (1983) except we cannot identify full-time students explicitly, so they are not excluded. We are, however, able to control for current school enrollment (see variable description below).

6. Since the idea of sample selection refers to potential labor force participants, we excluded the 2,701 observations on individuals who are either institutionalized or report handicaps such that they are unable to work. An additional 14 respondents did not fit the race categories described below. None of these observations are counted in the 32,418 figure reported above.

7. The relevant PUMS variable is PWGT1. The Bureau of the Census imputes responses for individuals who do not complete the census, thereby eliminating all missing data. While flag variables exist to discern non-responses, the weighting variable is constructed under the assumption that all

observations will be used, a procedure followed here. See Bureau of the Census (1992b, pp. 3-16, 3-17).

8. While we exclude individuals who characterize their main job as self-employment, we do include any earnings from self-employment for our sample of wage and salary earners because the usual hours data refers to all market-related work.

9. That is, closely related independent variables are found in at least one of the following studies: Reimers (1983), Price and Mills (1985), Neumark (1988), Belman and Heywood (1991), Leigh (1991), Gyimay-Brempong and Fichtenbaum (1993), Holtman and Idson (1993), and Wood, Corcoran, and Courant (1993). Excepting union status and various interaction terms, virtually all relevant variables incorporated in those studies are included here.

10. Following current usage, we avoid the Census term `hispanic' here. We also avoid the words `latino' and `latina' because they are gender specific.

11. See Footnote 6.

12. National data is from the Bureau of the Census (1992c). That data set does not break out latin respondents explicitly. For the national figures below, we therefore assumed that half of latin respondents were also claiming to be white, and that all respondents in the census `other' race category were latin. This rule, while crude, is consistent with the Milwaukee area PUMS data which found approximately half of latin respondents (419 of 887) classified themselves as white, while only a small fraction (41 total) reported as black, asian, or native american, with the remainder falling into the `other' category.

13. For the Blinder type regression, the statistic is 7.46 (1% s.l., 1 d.f.). For the Neumark type regression, the equivalent term is 12.68 (1% s.l., 1d.f.)

14. All figures reported in Tables 3 and 4 were calculated in logarithmic form, then we applied the anti-log to the average.

15. The data yields average yearly earnings in 1989 of $20,552 for the estimated 7,316 asian labor force participants in Milwaukee. Average yearly earnings for the estimated 735 Laotians, 383 Hmong, 312 Hawaiians, and 244 Pakistanis, however, are $11,684, $8,340, $10,311 and $9,892, respectively.

16. All yearly figures refer to individuals employed 40 hours per week for 52 weeks per year.