Neighborhoods, Economic Self-Sufficiency, and the MTO Program

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DESPITE THE SUBSTANTIAL decline in the degree of racial segregation in the U.S. housing market reported in the 2000 census, most African Americans still reside in communities that are geographically separate from those of white Americans.[1] Continued racial disparities in income, education, and employment mean that housing segregation is accompanied by the concentration of poverty and high rates of joblessness in predominantly black neighborhoods. The concentration of black households in older, predominantly inner-city neighborhoods, coupled with the continuing decentralization of employment within metropolitan areas, reduces the access to jobs of low-skilled inner-city residents. Lack of access is compounded by public transit systems that do not facilitate reverse commuting and by low rates of automobile ownership among poor minority households. This "spatial mismatch" between the locations of low-skilled jobs and the residences of low-skilled workers has been a focus of labor economists since the late 1960s.[2]

During the 1980s, concern with the employment effects of residential segregation was subordinated to a more general concern with the external effects of economic and racial segregation on social outcomes—for example, rates of school completion, teenage pregnancy, crime, and disease. These "neighborhood effects" were thought to contribute to the pathology of an urban "underclass."[3] The spatial concentration of the poor declined during the 1990s, and the number of "underclass" census tracts declined by one-third.[4] Never-

We thank Gary Burtless, David Card, Ingrid Gould Ellen, Jeffrey Kling, Helen Ladd, Jens Ludwig, Janet Rothenberg Pack, Lisa Sanbonmatsu, Michael Stoll, and Bruce Weinberg for their valuable input.

1. See Jargowsky (2003).
2. Kain (1968).
3. Jencks and Peterson (1991).
4. Jargowsky and Yang (2006).

theless, the 2000 census documented the fact that more than 3.5 million poor Americans lived in neighborhoods where the poverty rate exceeded 40 percent.

Assessing the importance of these issues for economic welfare is complicated. The explicit causal mechanisms are hard to define, and it is difficult to measure influences. Making any assessment based on non-experimental data is more difficult because individuals sort across neighborhoods for reasons that are almost certainly correlated with the determinants of the social outcomes studied.[5] For example, in interpreting cross-sectional data on the isolation of low-income workers from job locations, it is likely that those with a weaker attachment to the labor force will have chosen to locate in places where employment access is low, simply because monthly rents are lower in those places.

Thus, the experimental evidence provided by the Moving to Opportunity (MTO) program undertaken by the U.S. Department of Housing and Urban Development (HUD) during the 1994–98 period is potentially quite valuable—in understanding the importance of neighborhood externalities on social outcomes, in general, and the importance of spatial isolation on employment outcomes, in particular.

The MTO experiment sought to document the effect of neighborhood conditions on a broad set of social outcomes for households with children residing in poor, socially isolated neighborhoods. The experiment recruited more than 4,600 low-income households residing in public housing in high-poverty neighborhoods in five central cities: Baltimore, Boston, Chicago, Los Angeles, and New York. Program participants were assigned to one of three groups: a control group; an experimental treatment group that was given housing vouchers that could be used only in neighborhoods with relatively low poverty rates; and an additional treatment group that received identical vouchers but with no neighborhood or geographical restrictions attached.

Families assigned to the two treatment groups were exposed to significant declines in neighborhood poverty rates. Experimental evaluations of the program during the five-year period following random assignment found some significant positive effects on mental and physical health and personal safety for adults and female youth and adverse behavioral effects for male youth.[6] The evaluations, however, found no evidence at all of an experimental impact on adult self-sufficiency as measured by employment or earnings. Kling, Liebman and Katz conclude that “housing mobility by itself does not appear to be an effective antipoverty strategy—at least over [the] five-year horizon [of the

5. Manski (1999).
6. Kling, Liebman, and Katz (2007); Orr and others (2003).

experiment].”[7] More generally, the experimental findings suggest that nonspatial factors such as poor skills and racial discrimination in labor markets are more important in explaining racial inequality than are structural geographical barriers arising from the operations of local housing markets.

In this paper, we consider the implications of the findings from the MTO experiment for adult self-sufficiency. Our evaluation of the MTO results is that, while the experimental treatment certainly reduced a household’s exposure to concentrated poverty, the magnitude of that treatment was very small. It is hard to see how a treatment of this magnitude could offset the spatial disadvantages experienced by low-skilled African Americans. In that regard, therefore, the experiment is uninformative.

The effect of treatment under the MTO program was, on average, to move households in the five MTO metropolitan areas from neighborhoods at roughly the 96th percentile of the neighborhood poverty distribution to neighborhoods at the 88th percentile. Over the five-year period following random assignment, members of the experimental group resided in neighborhoods that were nearly identical along many observable dimensions to the neighborhood of the *average poor black* resident in these metropolitan areas. The treatment (that is, the exposure to new neighborhoods) fell far short of moving experimental subjects to neighborhoods comparable to those of the *average poor white* resident in metropolitan areas. Moreover, essentially none of the treatments affected the subjects’ access to employment opportunities. Finally, given the small intent-to-treat effects of MTO on access to employment and the standard errors of the estimated employment effects, the magnitude of any employment effect implied by the existing body of non-experimental research lies well within the confidence intervals of the MTO estimates.

An assessment of this experimental evidence on labor market outcomes and adult self-sufficiency—estimated effects that are insignificantly different from no effect at all—clearly depends on prior expectations about the magnitudes involved. The discussion below helps to confirm the magnitude of this prior. We present and estimate a simple model of employment and wage determination that assumes that within metropolitan areas, blacks and whites have access to different subsets of employment opportunities. We use this model to characterize the conditions that give rise to a spatial mismatch between residence and job locations. More important, the model provides a range of non-experimental estimates of the employment effects of the spatial mismatch that accords with the existing body of non-experimental research. These magnitudes can be compared with the treatment effects of the MTO for the five

7. Kling, Liebman, and Katz (2007, p. 108).

metropolitan areas in which the treatment was applied. We then discuss the impact of treatment under MTO on neighborhood quality and on physical access to employment opportunities, as well as the precision of the estimates relative to the range of non-experimental estimates.

Wage and Employment Determination: The Importance of Space

We develop a simple model of wage and employment determination that illustrates the mechanism through which the mismatch between jobs and residences affects the relative employment rates of blacks. The model is based on aggregate data from 241 metropolitan statistical areas (MSAs) and designed to answer this question: how large an effect on employment could be expected from treatment under MTO if the experimental treatment eliminated completely the difference in labor demand and supply conditions faced by black and white workers?

We extended the factor shares model presented by Card (2001) to describe the relationship between differential access to employment and differential concentrations of labor supply on employment outcomes. This model was used by Card to analyze the effects of immigrants on native wages and employment, but it is easily adapted to the case in which effective labor demand and supply vary within metropolitan areas due to housing segregation by race and to the uneven spatial distribution of employment.

The Basic Model

Consider an aggregate production function that varies by city c and is differentiated by race r. Race-specific production functions reflect the geographic dissimilarity within cities between the residential and workplace locations of members of different racial groups. Production takes place according to the relationship

$$Q_{cr} = F(K_{cr}, L_{cr}), \tag{1}$$

where K_{cr} is a vector of nonlabor inputs for city c and race group r, and L_{cr} is an aggregation of different quantities of labor N_{jcr}, distinguished by skill level j. The aggregation takes the convenient constant elasticity of substitution (CES) form

$$L_{cr} = \left[\sum_{j=1}^{J} (e_{jcr} N_{jcr})^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}, \tag{2}$$

where J is the number of different skill groupings, σ is the elasticity of substitution between any two grades of labor, and e_{jcr} is a productivity factor, which may vary by skill group, city, and race.

The wages and employment rates for workers in each skill category must satisfy two standard conditions: the marginal revenue product of each grade of labor must equal the wage paid to the workers and the quantity of labor demanded must equal the quantity of labor supplied. The first condition implies that

$$(3) \qquad F_{L_{cr}} L_{cr}^{\frac{1}{\sigma}} e_{jcr}^{\frac{\sigma-1}{\sigma}} N_{jcr}^{-\frac{1}{\sigma}} = w_{jcr},$$

where the price of output is unity and w_{jcr} is the wage paid to a worker in group *jcr.* With a slight rearrangement, the natural log of total employment of each skill group can be expressed as a linear function of the natural log of the wage:

$$(4) \qquad \ln N_{jcr} = X_{cr} + (\sigma - 1)\ln e_{jcr} - \sigma \ln w_{jcr}.$$

where $X_{cr} = \sigma \ln[F_{Lcr} L_{cr}^{1/\sigma}]$ varies by city and racial group but not by skill group.

Let P_{jcr} be the resident population of group *jcr* and assume that labor supply takes the log-linear form

$$(5) \qquad \ln \frac{N_{jcr}}{P_{jcr}} = \varepsilon_j \ln w_{jcr} \Rightarrow \ln N_{jcr} = \varepsilon_j \ln w_{jcr} + \ln P_{jcr},$$

where ε_j is the labor supply elasticity for members of skill group j.[8] Equating the right hand side of equation 4 (the demand condition) and equation 5 (the supply condition) and solving for w_{jcr} yields the equilibrium wage:

$$(6) \qquad \ln w_{jcr} = \frac{1}{\varepsilon_j + \sigma}\{X'_{cr} + (\sigma - 1)\ln e_{jcr} - \ln(P_{jcr} / P_{cr})\},$$

where $X'_{cr} = X_{cr} - \ln P_{cr}$ and P_{cr} is the total population of racial group r in city c. The equilibrium wage in equation 6, in conjunction with the labor supply function in equation 5, yields the employment rate for group j:

8. A more general specification would allow the labor supply elasticity to vary with all three dimensions of the data (that is, by *jcr*). The constraint that the supply elasticity is constant across racial groups and cities but varies across skill groups facilitates the difference-in-difference model that we estimate below. This empirical specification suggests that employment and wages should increase with access to jobs and decrease with the degree of labor market competition, two fairly straightforward propositions.

$$\ln(N_{jcr} / P_{jcr}) = \frac{\varepsilon_j}{\varepsilon_j + \sigma}\{X'_{cr} + (\sigma - 1)\ln e_{jcr} - \ln(P_{jcr} / P_{cr})\}. \tag{7}$$

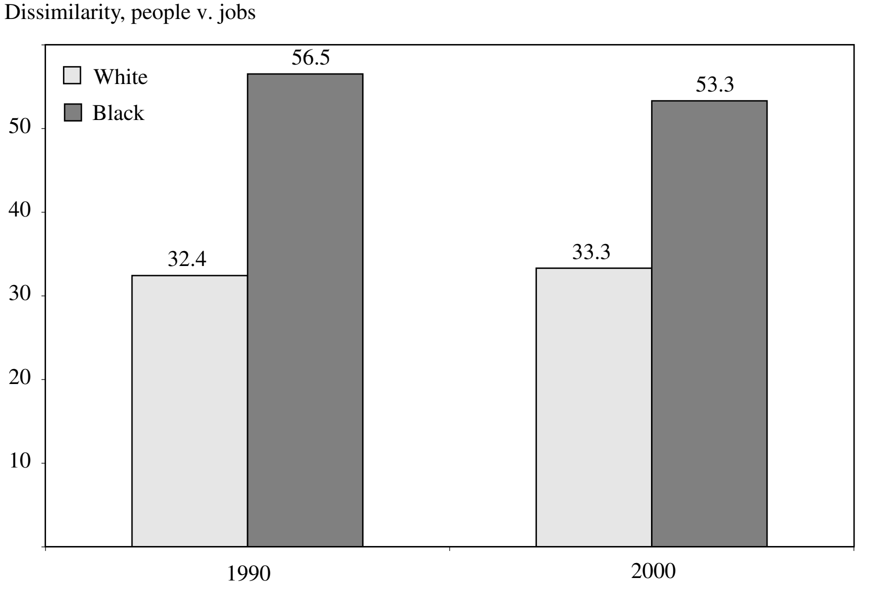
Equations 6 and 7 summarize the causal mechanisms through which a spatial mismatch between workplace and residence may affect the relative employment and earnings of black workers. Wages and employment by race are affected by the term $X'_{cr} = \sigma \ln[F_{L_{cr}} L_{cr}^{1/\sigma}] - \ln P_{cr}$. But X'_{cr} is an increasing function of the marginal product of the labor aggregate. A higher employment density in white neighborhoods relative to black neighborhoods is merely a greater endowment of nonlabor inputs K_{cr} (that is, more capital is located in white neighborhoods). Other things being equal, the relatively large capital endowment increases the marginal product of labor in white neighborhoods and, in turn, employment and wages. Thus, the differential effect of employment decentralization on black employment outcomes is measured by the race-specific demand factor. The impact of a positive demand shock on the wages of any given group will be smaller if the labor supply elasticity is larger and if the elasticity of substitution between labor grades is larger. On the other hand, the effect of a demand shock on a specific group's employment rate will increase with the labor supply elasticity and decrease with the elasticity of substitution.

Equations 6 and 7 also indicate that wages and employment of members of group *jcr* decline in the share of the regional population in group *jcr*. The negative wage effect of a supply shift (for example, an increase in P_{jcr}/P_{cr}) is smaller if the group-specific supply elasticity is larger and if the elasticity of substitution between skill groups is larger. The negative effect on employment is larger if the supply elasticity is larger; the effect is smaller if the substitution elasticity is larger. With the sizable racial disparities in educational attainment, racial segregation mechanically concentrates low-skilled workers in black neighborhoods while reducing the factor shares of high-skilled workers. This relationship between segregation and factor proportions works to the detriment of low-skilled black workers and to the advantage of high-skilled black workers.

How Different Are Demand and Supply Conditions in Black and White Neighborhoods?

Is there a difference between the labor demand functions faced by black and white workers within the same metropolitan area? Answering that question requires reference to a quantitative measure of demand conditions—access to jobs. Measures of job access used in the past include the average commute

Figure 1. Weighted Average Dissimilarity Index Values (x100) between Residential Distributions and the Distribution of Total Employment for Blacks and Whites



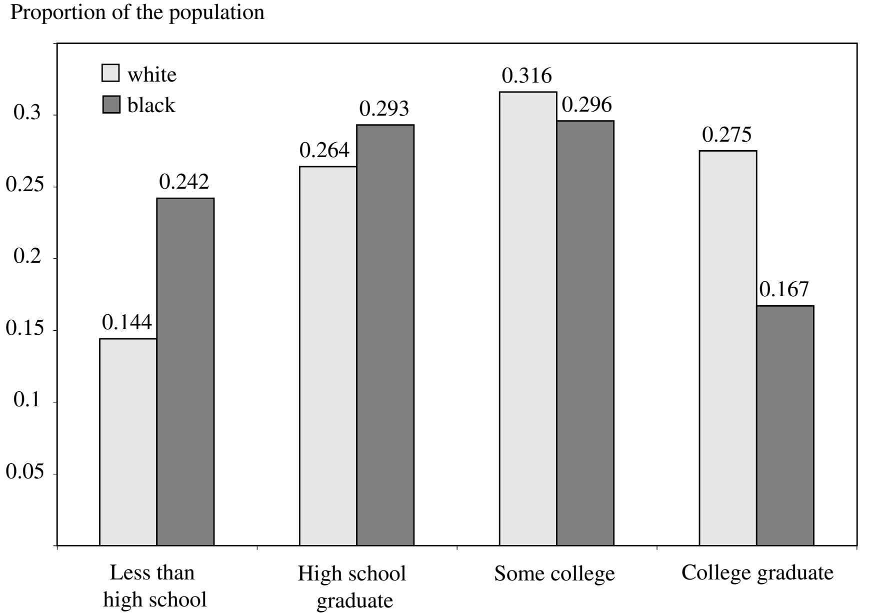
Source: Raphael and Stoll (2002).

times of different types of workers (Ihlanfeldt 1992), ratios of jobs to residents (Stoll, Holzer, and Ihlanfeldt 2000; Hellerstein, Neumark, and McInerney 2007), distance-weighted estimates of proximity to employment clusters (O'Regan and Quigley 1996), and proximity to employment growth (Raphael 1998). Here we use a simple metric employed by Raphael and Stoll (2002) to characterize racial disparities in effective labor demand—namely, the disparity between the residential and workplace distributions of whites and blacks.

Figure 1 presents the average dissimilarity between the residential distributions of blacks and whites and the distribution of total employment for the years 1990 and 2000. The figures are weighted averages of values of the Taueber index calculated by postal code for each of 241 metropolitan areas, where the weights are the metropolitan area population of each racial group.[9] The index is interpreted as either the proportion of the population or the proportion of jobs that would have to be relocated to yield a uniform job-residence distribution across the geographic units of analysis.

9. These dissimilarity indices are measured by using zip code–level employment data from the 1994 and 1999 economic censuses as well as zip code–level population data from the 1990 and 2000 Census of Population and Housing; see Raphael and Stoll (2002) for details.

Figure 2. Educational Distribution of Adults in the Neighborhoods of the Average White and Black Residents of U.S. Metropolitan Areas



Source: Authors' tabulations from the 2000 Census Summary Files 3.

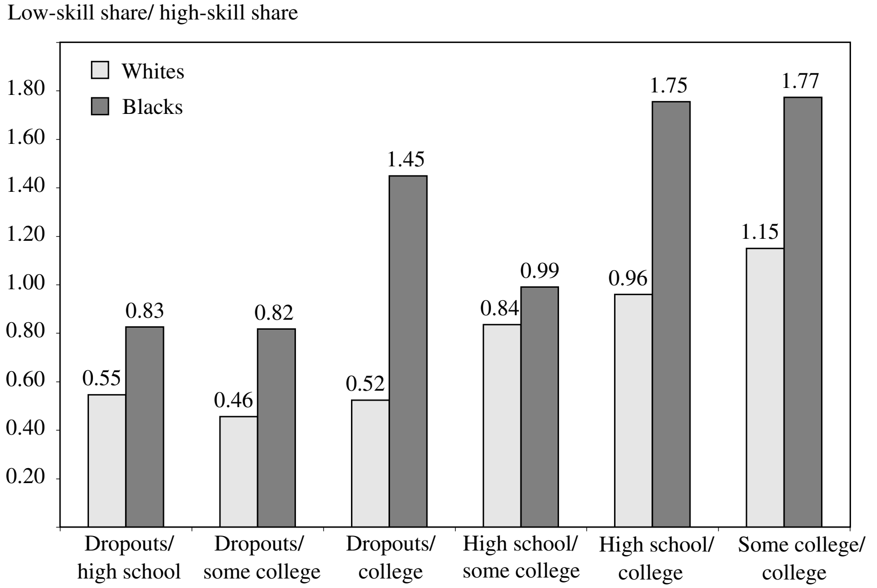
The figure illustrates the large interracial disparities in the jobs-people dissimilarity index. While roughly 33 percent of the white population residing in U.S. metropolitan areas would have had to move in 2000 to yield an even ratio of jobs to white workers by postal code, the comparable figures for black metropolitan area residents is 53 percent. To the extent that these disparities segment the effective labor demand for workers of different racial groups, black and white workers face different demand conditions.

Do supply conditions differ in black and white communities? A simple measure of the effect of racial segregation on available factor shares is presented in figure 2, which reports the educational distribution of adult residents in the neighborhoods of the average black and white resident.[10]

There are clear disparities between the educational attainment of adults in typical white neighborhoods and typical back neighborhoods. For example,

10. For the census tract of the average black or white resident in all MSAs, we calculate the proportion of adults 18 to 65 years of age who have less than a high school education; who are high school graduates; who have attended some college; and who have graduated from college. Those calculations are based on data from the 2000 Census of Population and Housing Summary File 3 (SF3) using all 67,000 tracts located in MSAs.

Figure 3. Comparison of Factor Proportions in the Neighborhoods of the Average White and Black Resident of U.S. Metropolitan Areas



Source: Authors' tabulations from the 2000 Census Summary Files 3.

roughly 24 percent of adults in black neighborhoods are high school dropouts.[11] In contrast, only 14 percent of adults in typical white neighborhoods have less than a high school education. At the other end of the spectrum, the difference between the percent of adults in white neighborhoods with college degrees and the percent in black neighborhoods is a full 11 percentage points. Figure 3 compares low-skilled to high-skilled factor proportions in black and white neighborhoods; in all comparisons, the ratio of less-skilled to more-skilled labor is considerably higher in the average black neighborhood.

How Do Differences in Demand and Supply Conditions Relate to Black Employment Rates?

Do the observed differences by race in demand conditions and factor supplies matter? Answering that question requires estimating the wage and employment equations 6 and 7. Here we focus on estimating the employment equation.[12]

11. This number would certainly be even higher if one accounted for the nearly 20 percent of adult black men in this educational category who are incarcerated on any given day (Raphael 2007).

12. For low-skilled blacks, an unusually high proportion of non-institutionalized working-age

We impose two restrictions that permit estimation of equation 7 using cross-sectional data from the 2000 census. First, we assume that the demand shifter X'_{cr} is a function of the degree of dissimilarity between the residential distribution of race group r and total metropolitan area employment, allowing for an intercept and a slope specific to skill groups. Specifically, we assume that

$$X'_{cr} = \alpha_j + \beta_j D_{cr}, \tag{8}$$

where D_{cr} is the degree of dissimilarity (the Taeuber index) between the spatial distribution of employment and the spatial distribution of the residences of group r in city c. If demand is decreasing in the geographic imbalance between people and jobs, β_j is negative. Second, we assume that the productivity coefficient is constant across cities and racial groups but varies across skill groups:

$$\ln e_{jcr} = \gamma_j. \tag{9}$$

Substituting these two restrictions into equation 7 yields the reduced-form equation

$$\ln(N_{jcr} / P_{jcr}) = \theta_j + \delta_j D_{cr} + \xi_j \ln(P_{jcr} / P_{cr}), \tag{10}$$

where

$$\theta_j = \frac{\varepsilon_j}{\varepsilon_j + \sigma}[\alpha_j + (\sigma - 1)\gamma_j],$$

$$\delta_j = \frac{\varepsilon_j}{\varepsilon_j + \sigma}\beta_j$$

and

$$\xi_j = -\frac{\varepsilon_j}{\varepsilon_j + \sigma}.$$

With a positive labor supply elasticity, both δ_j and ε_j are negative.[13] Equation 10 can be estimated separately by skill group by using data on employment rates,

adults are not employed (nearly 60 percent), rendering the participation selection-bias problem in the wage equation especially difficult (Raphael 2007). However, estimating the employment relationship, equation 7, does not require addressing this selection bias. Of course, if one wishes to uncover the structural parameters of this model—that is, the labor supply elasticities and the elasticity of substitution—both the wage and employment equation would have to be estimated. Nonetheless, the model does provide clear predictions regarding the likely signs of the effects of the demand shifter and supply shifts on employment rates.

13. Since an increase in wages induces offsetting income and substitution effects on labor supply, the sign of the supply elasticity is ambiguous. However, estimates of labor supply elasticities

factor shares, and the employment dissimilarity measure for a given demographic group.[14]

Table 1 presents estimates of various specifications of equation 10 for black workers in four skill groups: high school dropouts, high school graduates, those with some college, and college graduates. Within each skill group, we estimate models using three different dependent variables: the overall employment rate for black adults in the group, the employment rate for black women, and employment rate for black men. Employment rates are calculated for each of 241 metropolitan areas using data for the 2000 Five Percent Public Use Microdata Sample (PUMS) from the census.[15] Metropolitan area jobs-people dissimilarity indices are computed following Raphael and Stoll (2002); factor shares in black neighborhoods are estimated using data from the SF3 files as discussed above.

For high school dropouts, the simplest model shows a significant negative effect of the spatial mismatch in employment on the employment rates of all black high school dropouts, slightly larger effects for black males, and slightly smaller yet significant effects for black women. In addition, a greater proportion of adults in black neighborhoods who are high school dropouts leads to a lower overall black employment rate and a lower employment rate for each gender. In specification 2 in table 1, we add the residential dissimilarity index between blacks and whites to the specification.[16] The results are robust to inclusion of the residential segregation measure, yet the coefficients on the mismatch index are attenuated, especially for black men.

Table 1 also reports that the dissimilarity between black residents and jobs exerts significant negative effects on the employment rate of black high school graduates and of blacks with some college education. Again, the results are fairly similar whether we use overall employment rates, male employment rates, or female employment rates. The effects for college graduates are generally insignificant or small. The effects of neighborhood factor shares decline

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in the United States tend to be positive, with higher elasticity estimates for men than women. See the estimates in Raphael (2007) and the research reviewed in Juhn and Potter (2006).

14. Several factors may bias simple cross-sectional estimates of the coefficients of equation 10. For example, African Americans in metropolitan areas where the mismatch between people and jobs is lowest may be more productive relative to those in metropolitan areas with a high degree of mismatch, even within defined educational groups. These unobserved differences in productivity would bias upward our estimate of the effects of variation in demand conditions. As noted below, however, if that were true, it would make our comparison with the MTO findings even more conservative.

15. Employment rates pertain to non-institutionalized adults between 18 and 65 years of age.

16. We tabulate the degree of residential dissimilarity by metropolitan area using data form the 2000 SF3 files.

Table 1. Estimated Effects of Variation in Labor Supply and Demand Conditions in Black Neighborhoods on Employment Rates for Black Workers[a]

Educational level	*Jobs-people dissimilarity*	*Black-white dissimilarity*	*$ln(P_{jcr}/P_{jc})$*	*R^2*
A. High school dropout				
Specification (1)				
All	-0.489 (0.067)	. . .	-0.124 (0.048)	0.259
Women	-0.406 (0.081)	. . .	-0.157 (0.058)	0.175
Men	-0.546 (0.078)	. . .	-0.112 (0.056)	0.239
Specification (2)				
All	-0.245 (0.115)	-0.340 (0.131)	-0.119 (0.047)	0.280
Women	-0.296 (0.141)	-0.154 (0.160)	-0.154 (0.058)	0.178
Men	-0.164 (0.133)	-0.531 (0.151)	-0.104 (0.055)	0.278
B. High school graduate				
Specification (1)				
All	-0.373 (0.036)	. . .	0.083 (0.039)	0.332
Women	-0.351 (0.039)	. . .	0.109 (0.042)	0.285
Men	-0.375 (0.045)	. . .	0.073 (0.047)	0.247
Specification (2)				
All	-0.216 (0.072)	-0.206 (0.084)	0.129 (0.043)	0.349
Women	-0.163 (0.079)	-0.247 (0.090)	0.165 (0.046)	0.307
Men	-0.243 (0.090)	-0.174 (0.104)	0.111 (0.053)	0.256
C. Some college				
Specification (1)				
All	-0.103 (0.029)	. . .	-0.119 (0.041)	0.067
Women	-0.112 (0.029)	. . .	-0.079 (0.041)	0.061
Men	-0.084 (0.037)	. . .	-0.179 (0.052)	0.055
Specification (2)				
All	-0.159 (0.052)	0.078 (0.061)	-0.111 (0.041)	0.073
Women	-0.125 (0.053)	0.017 (0.062)	-0.077 (0.062)	0.062
Men	-0.205 (0.067)	0.171 (0.078)	-0.162 (0.053)	0.074
D. College graduate				
Specification (1)				
All	0.024 (0.022)	. . .	0.051 (0.014)	0.052
Women	0.014 (0.024)	. . .	0.032 (0.016)	0.019
Men	0.054 (0.035)	. . .	0.083 (0.023)	0.058
Specification (2)				
All	-0.082 (0.041)	0.146 (0.048)	0.063 (0.015)	0.087
Women	-0.053 (0.044)	0.093 (0.052)	0.040 (0.016)	0.033
Men	-0.140 (0.065)	0.268 (0.076)	0.104 (0.024)	0.105

Source: Models estimated with data from the 2000 Census Public Use Microdata and the 2000 Summary Files 3.

a. Equation $\ln(N_{jcr} / P_{jcr}) = \theta_j + \delta_j D_{cr} + \xi_j \ln(P_{jcr} / P_{cr})$.

Standard errors are in parentheses. All models are weighted by the metropolitan area black population. Results in panels B, C, and D are based on models estimated with 241 MSA-level observations; results in panel A are based on 237 observations. For models labeled "All," the overall black employment rate is the dependent variable. For models labeled "Women" or "Men," the dependent variable is the gender-specific employment rate.

with educational attainment, a result consistent with existing research.[17] We do find an unexpected positive effect of own factor shares for high school graduates, significant negative effects for those with some college, and small positive effects for college graduates.

A simple extension of the model specified in equation 10 permits the productivity coefficient to vary both by city and by educational attainment group:

$$\ln(N_{jcr} / P_{jcr}) = \theta_{jc} + \delta_j D_{cr} + \xi_j \ln(P_{jcr} / P_{cr}). \tag{11}$$

(Note that θ_{jc} has been substituted for θ_j.) Equation 11 cannot be estimated with data for one racial group; however, with data on two racial groups, this city-/education-group productivity component can be eliminated by differencing across groups. Let $r = (b,w)$ indicate blacks and whites, respectively; then let

$$\begin{aligned} &\ln(N_{jcb} / P_{jcb}) - \ln(N_{jcw} / P_{jcw}) \\ &= \delta_j (D_{cb} - D_{cw}) + \xi_j [\ln(P_{jcb} / P_{cb}) - \ln(P_{jcw} / P_{cw})], \end{aligned} \tag{12}$$

where the common city-occupation component has been differenced away.[18]

Table 2 presents estimation results of this alternative specification of the model. For black high school dropouts, the effects of the geographic imbalance between people and jobs remain significant and negative, though in these models the coefficient estimates are somewhat smaller. Similarly, the estimated effects of own factor shares are smaller by comparison. For the other three educational attainment groups, the estimates of the mismatch effect using the specification in equation 12 are quite similar to those from equation 10. Again, the effect sizes are comparable for the models using the overall employment rate differentials and the models using employment rate differentials by sex.

Thus, the correlation between the employment rates of less skilled black workers and a simple measure of geographically-induced variation in demand conditions is fairly robust. Controlling for the degree of residential dissimilarity between blacks and whites and transforming the data into interracial differences to account for city–education group productivity effects does attenuate this relationship; nevertheless, the measure of mismatch is associated with large and highly significant effects in almost all models, especially for less skilled workers. The geographic proximity variable exerts comparable effects on male and female employment rates. The results concerning the supply concentration effects, however, are less robust.

17. See, for example, Hellerstein, Neumark, and McInerney (2007).

18. The regression results based on equation 12 can also be viewed as a test of whether factor prices equalize across white and black communities within the same metropolitan area.

Table 2. Estimated Effects of Relative Variations in Labor Supply and Demand Conditions in Black and White Neighborhoods on Relative Employment Rates of Black and White Workers[a]

Educational level	*Jobs-people dissimilarity*	*Black-white dissimilarity*	*$ln(P_{jcr}/P_{jc})$*	*R^2*
A. High school dropout				
Specification (1)				
All	-0.351 (0.071)	. . .	-0.086 (0.053)	0.164
Women	-0.387 (0.104)	. . .	-0.114 (0.077)	0.114
Men	-0.381 (0.080)	. . .	-0.046 (0.059)	0.139
Specification (2)				
All	-0.229 (0.101)	-0.198 (0.117)	-0.054 (0.056)	0.174
Women	-0.471 (0.148)	0.134 (0.171)	-0.136 (0.082)	0.116
Men	-0.118 (0.111)	-0.428 (0.129)	0.022 (0.061)	0.181
B. High school graduate				
Specification (1)				
All	-0.374 (0.035)	. . .	0.159 (0.058)	0.314
Women	-0.358 (0.042)	. . .	0.187 (0.069)	0.233
Men	-0.388 (0.042)	. . .	0.152 (0.069)	0.259
Specification (2)				
All	-0.262 (0.056)	-0.156 (0.061)	0.163 (0.057)	0.332
Women	-0.282 (0.067)	-0.106 (0.073)	0.189 (0.069)	0.239
Men	-0.250 (0.067)	-0.193 (0.072)	0.156 (0.068)	0.281
C. Some college				
Specification (1)				
All	-0.166 (0.021)	. . .	0.019 (0.035)	0.201
Women	-0.141 (0.029)	. . .	0.039 (0.047)	0.087
Men	-0.179 (0.027)	. . .	0.019 (0.043)	0.159
Specification (2)				
All	-0.101 (0.034)	-0.091 (0.037)	0.025 (0.034)	0.222
Women	-0.103 (0.047)	-0.053 (0.051)	0.043 (0.047)	0.091
Men	-0.117 (0.043)	-0.088 (0.047)	0.026 (0.043)	0.172
D. College graduate				
Specification (1)				
All	-0.067 (0.030)	. . .	0.008 (0.023)	0.039
Women	-0.084 (0.035)	. . .	-0.029 (0.027)	0.024
Men	-0.025 (0.047)	. . .	0.052 (0.035)	0.024
Specification (2)				
All	-0.009 (0.038)	-0.113 (0.047)	-0.021 (0.025)	0.062
Women	-0.056 (0.045)	-0.056 (0.055)	-0.044 (0.030)	0.028
Men	0.027 (0.059)	-0.104 (0.073)	0.025 (0.039)	0.033

Source: Models estimated with data from the 2000 Census Public Use Microdata and the 2000 Summary Files.

a. Equation $\ln(N_{jcb} / P_{jcb}) - \ln(N_{jcw} / P_{jcw}) = \delta_j (D_{cb} - D_{cw}) + \xi_j [\ln(P_{jcb} / P_{cb}) - \ln(P_{jcw} / P_{cw})]$.

Standard errors are in parentheses. All models are weighted by the metropolitan area black population. Results in panels B, C, and D are based on models estimated with 241 MSA-level observations; results in panel A are based on 237 observations. For models labeled "All," the overall black-white employment rate difference is the dependent variable. For models labeled "Women" or "Men," the dependent variable is the gender-specific employment rate difference.

How Big an Effect Might We Expect?

How should we interpret these magnitudes? The regression estimates can be used to predict how the employment rates of low-skilled blacks would change if blacks confronted the same economic geography as whites with respect to job availability and neighborhood factor shares.

Table 3 shows the estimated effects on the employment rates of black high school dropouts, including overall employment rates (panel A), female employment rates (panel B), and male employment rates (panel C). The first column reports the employment rate for black high school dropouts in the five metropolitan areas included in the MTO experiment. The last row within each panel provides the average employment rate across MTO sites, where the representation of MTO subjects in each metropolitan area is used in weighting. The employment rates of black high school dropouts are extremely low, with an average rate of 0.36 overall, 0.33 for black females, and 0.40 for black males.[19] Columns 2 and 3 characterize the differences in labor supply and demand conditions between blacks and whites in each city (which are assumed not to vary by sex, supposing that black men and women live in the same neighborhoods). Column 2 reports the large differences in each metropolitan area between the dissimilarity index for blacks and the dissimilarity index for whites (ranging from 0.15 to 0.35). Column 3 reports the large disparities in the natural log of the proportion of adults who are high school dropouts between black and white neighborhoods in the five metropolitan areas.

The characteristics reported in columns 2 and 3 are used to estimate the joint effect of employment mismatch and supply concentration on the employment rates of black high school dropouts, shown in columns 4 and 5. Specifically, we estimate the increase in employment rates that would occur if the disparities in columns 2 and 3 were eliminated.[20] The upper-bound estimates in column 4 (based on the model specification from tables 1 and 2 with the largest mismatch coefficient) indicate a joint mismatch–supply concentration effect on overall employment rates ranging from 0.05 for Los Angeles to 0.11 for Boston, with a weighted average estimate of 0.08. The upper-bound estimates are slightly lower for women (with a weighted average of 0.07) and larger for men (with a weighted average of 0.09).

The lower-bound estimates in column 5 (based on the model specification with the smallest mismatch coefficient) yield estimated employment effects

19. These employment rates are calculated from the 2000 Five Percent PUMS.

20. We use the parameter estimates in tables 1 and 2 to estimate the effect on the natural log of the employment rate, add that to the log of the employment rate for the metropolitan area, and exponentiate.

Table 3. Implied Effects of Employment Mismatch and Supply Concentration on the Employment Rates of Black High School Dropouts in MTO Metropolitan Areas[a]

Black high school dropouts	*(1) Employment fraction for black high school dropouts*	*(2) Black-white difference in the mismatch index*	*(3) Black-white difference in the log of neighborhood residents who are high school dropouts*	*(4) Effect of differences in (2) and (3) on black high school dropout employment level (HIGH)*	*(5) Effect of differences in (2) and (3) on black high school dropout employment level (LOW)*
Panel A: All black high school dropouts					
Baltimore	0.38	0.15	0.68	0.07	0.03
Boston	0.46	0.30	0.51	0.11	0.05
Chicago	0.32	0.35	0.56	0.09	0.04
Los Angeles	0.29	0.24	0.34	0.05	0.02
New York	0.37	0.26	0.54	0.08	0.03
Weighted average[a]	0.36	0.27	0.52	0.08	0.03
Panel B: Black female high school dropouts					
Baltimore	0.36	0.15	0.68	0.06	0.06
Boston	0.42	0.30	0.51	0.10	0.08
Chicago	0.29	0.35	0.56	0.08	0.06
Los Angeles	0.27	0.24	0.34	0.05	0.04
New York	0.33	0.26	0.54	0.07	0.06
Weighted average[a]	0.33	0.27	0.52	0.07	0.06
Panel C: Black male high school dropouts					
Baltimore	0.41	0.15	0.68	0.07	0.00
Boston	0.52	0.30	0.51	0.13	0.04
Chicago	0.35	0.35	0.56	0.10	0.03
Los Angeles	0.32	0.24	0.34	0.06	0.02
New York	0.41	0.26	0.54	0.09	0.03
Weighted average[b]	0.40	0.27	0.52	0.09	0.03

Source: Authors' tabulations from the 2000 Census Summary Files 3.

a. The high estimates in column (4) are based on the regression model in either table 1 or table 2 with the largest (in absolute value) coefficient on the mismatch index. The low estimates in column (5) are based on the regression model in table 1 or table 2 with the lowest coefficient on the mismatch index. The employment-level effects are the joint implied effect of the geographic concentration of supply and the mismatch between black residential distribution and labor demand. Separate models by gender are based on the gender-specific employment rate models presented in tables 1 and 2.

b. The averages in this row use the MSA proportional representation among MTO subjects as weights.

ranging from a low of 0.02 for Los Angeles to 0.05 for Boston, with a weighted average estimate of 0.03. Here, the estimates for women tend to be higher, with a weighted average effect of 0.06, while the estimates for men tend to be lower, with a weighted average of 0.03. The 3-to-8 percentage-point range of the average effect (reported in panel A) is roughly 25 to 58 percent of the black-white employment rate differential among high school dropouts.[21]

The upper-bound estimates from a regression with few controls are perhaps too high, while the lower-bound estimates derived from models that hold constant the level of black-white dissimilarity are perhaps too low. However, the results do provide a benchmark (or a prior) against which to compare the experimental estimate from the MTO. We take an estimate of 6 to 6.5 percentage points as the benchmark non-experimental estimate of the predicted effect of eliminating the mismatch on the employment rates of low-skilled black men and women.

Moving to Opportunity: Employment Results

The MTO experiment was conducted in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. Experimental households were drawn from public housing residents living in census tracts with very high poverty rates. Between 1994 and 1997, 4,248 households were randomly assigned to one of three groups: a control group, which received no new assistance but which continued to be eligible for public housing assistance; a Section 8 group, which received a traditional Section 8 housing voucher with no geographic restrictions on the units eligible for rental; and an experimental group, which received a Section 8 housing voucher, restricted for one year for use in a census tract with a poverty rate of less than 10 percent (the last group also was provided with mobility counseling).[22] After the initial one-year period, experimental group households were permitted to use their housing voucher to move from their new location with no further geographic restrictions attached. Thus, after the first year, the experimental group and the Section 8 group faced the same behavioral rules, but the former group was eligible for mobility counseling.

Table 4 summarizes the mobility outcomes for the three MTO groups. For the control group, the table provides cross-tabulations of households by their post-assignment mobility decisions. It reports the average census tract poverty rates for movers, stayers, and for all members of the group. The table provides similar figures for the experimental group and the Section 8 group, with additional tabulations indicating whether the households complied with the treatment

21. This range of estimates is consistent with those provided in other non-experimental studies (reviewed in Ihlanfeldt and Sjoquist 1998), and they are somewhat larger then the more recent estimates of Hellerstein, Neumark, and McInerney (2007).

22. Orr and others (2003).

Table 4. Summary of Mobility Outcomes for Three MTO Assignment Groups and Poverty Rates by Residential Location in 2002

Group	*Number of households*	*Percent of assignment group*	*Mean neighborhood poverty rate in 2002*[a]
Panel A: Control group			
Stayed in place	343	30	51.1
Moved	793	70	33.6
Total	1,136	100	38.9
Panel B: Experimental group			
Did not lease	785	53	39.6
Stayed in place	267	18	49.1
Moved	518	35	34.6
Leased	701	47	20.0
Did not move again	245	16	12.6
Moved again	456	31	24.0
Total	1,486	100	30.4
Panel C: Section 8 group			
Did not lease	408	39	38.3
Stayed in place	166	16	46.8
Moved	242	23	32.5
Leased	641	61	28.6
Did not move again	215	20	29.1
Moved again	426	41	28.4
Total	1,049	100	32.4

Source: Figures in this table come from exhibit 2.5 in Orr and others (2003).
a. Based on census tract poverty rates from the 2000 census.

(leased a Section 8 rental unit or did not); for those that did, the table reports whether they moved again after their first move.

Several patterns are clear from table 4. First, nearly 70 percent of the control households moved after random assignment. Moreover, the mover households were exposed to substantial declines—more than 18 percentage points—in average neighborhood poverty rates (from 51.1 to 33.6 percent). Among households in the experimental group, only 47 percent complied with treatment and leased a Section 8 dwelling in a designated neighborhood. Of that 47 percent, roughly two-thirds moved again after their initial move; most of those who moved again ultimately selected neighborhoods with relatively high average poverty rates. Among the 53 percent of the experimental group households that did not lease a unit, nearly two-thirds moved subsequently, most to lower-poverty neighborhoods.

Among the Section 8 group, 61 percent of households used the voucher offered at random assignment, with two-thirds moving again after the first program-induced move. Among those that did not lease a unit (39 percent of the group), nearly 60 percent moved since random assignment. In general, mobility rates are high among low-income renters, and the households participating in the MTO program were no exception.

With the exception of the post-assignment moves of compliers in the experimental group, the post-assignment moves of all of the subgroups listed in table 4 were toward neighborhoods with lower poverty rates. Nonetheless, a comparison of the neighborhood poverty rates does demonstrate notable intent-to-treat effects on this variable. In particular, in 2002 the average census tract poverty rate for control group households stood at 39 percent. By contrast, the neighborhood poverty rates for the experimental and Section 8 groups were 30 and 32.4 percent, respectively.

Table 5 summarizes the estimated employment effects reported for the five MTO cities.[23] The first column presents the mean values of outcomes for the control group. The second column presents estimates of the intent-to-treat (ITT) effect of the offer of an MTO voucher. These effects are estimated by a simple regression of the outcome on assignment group indicator variables and a vector of observable human capital and demographic covariates. The third column presents estimates of the effect of the treatment on those who complied, or the treatment-on-the-treated (TOT) effect. Here, the key explanatory variable is an indicator of using an MTO voucher; the effects are estimated by employing group assignment indicator variables as instruments for whether a household actually used an MTO voucher.[24]

The table provides results for a number of outcomes—including self-reported employment in 2002 and employment indicators from state administrative records for the year 2001—for the five-year period following random assignment and for year 5 following random assignment. None of the estimates are statistically significant. All of the Section 8 ITT and TOT point estimates are positive, yet insignificantly different from zero. Half of the TOT point estimates for the experimental group are negative (including two of the three estimates derived from state administrative data), and half are positive. The difference from zero is statistically insignificant for all groups. Therefore, there is no evidence of an impact on employment rates arising from the MTO program.

23. Kling and others (2004).

24. The TOT estimate is simply the ITT estimate divided by the regression-adjusted proportion of either the experimental group or the Section 8 group that complied.

Table 5. Summary of Employment Effect Estimates from the Moving to Opportunity Experiment Five Years after Randomization[a]

Effect versus control group	*Control group mean*	*Intent-to-treat effect*	*Effect of the treatment on the treated*	*N*
Self-reported employment rate in 2002				
Experimental	0.520	0.015 (0.021)	0.033 (0.044)	2,525
Section 8	0.520	0.024 (0.023)	0.040 (0.038)	2,068
Fraction of quarters employed in 2001 (administrative data)				
Experimental	0.508	-0.017 (0.017)	-0.036 (0.035)	2,910
Section 8	0.508	0.014 (0.017)	0.022 (0.028)	2,411
Fraction of quarters employed in years 1 through 5 after random assignment (administrative data)				
Experimental	0.422	-0.006 (0.013)	-0.012 (0.028)	2,455
Section 8	0.422	0.001 (0.014)	0.001 (0.023)	2,039
Fraction of quarters employed in year 5 after random assignment (administrative data)				
Experimental	0.499	0.002 (0.018)	0.005 (0.039)	2,455
Section 8	0.499	0.008 (0.020)	0.013 (0.032)	2,039

Source: Figures in the table are reproduced from tables 3 and 4 in Kling and others (2004).
a. Standard errors are in parentheses.

What Explains the Difference between the MTO Employment Results and the Non-Experimental Research Results?

The non-experimental estimates of the effect of a spatial mismatch on employment and the experimental employment results from MTO stand in stark contrast to one another. While the empirical research on spatial mismatch suggests that eliminating the relative disadvantage that African Americans face in terms of the demand and supply conditions characterizing their local labor markets would narrow interracial differentials in employment outcomes, the only experiment that provides certifiably exogenous variation in residential mobility fails to find *any* impact on the relative employment outcomes of treated subjects. What explains this difference in results?

Two aspects of the MTO experiment limit its effectiveness as a test of the effects of neighborhood on adult self-sufficiency: the magnitude of the treatment in terms of the types of neighborhoods to which those treated in the program were exposed, and the statistical power of the MTO estimates relative to the magnitudes commonly reported in the non-experimental literature. Here we discuss each in turn.

Table 6. Average Census Tract Characteristics for MTO Control, Treatment, and Section 8 Groups and for Poor Black and White Residents of the Five MTO PMSAS[a]

	MTO groups				
Average census tract characteristic	*Control*	*Section 8*	*Experimental*	*Poor blacks*	*Poor whites*
Poverty rate	0.45	0.35	0.33	0.32	0.17
Poverty rate > 30 percent	0.87	0.62	0.52	0.51	0.15
Share on public assistance	0.23	0.17	0.16	0.14	0.05
Share of unemployed residents 16 and over	0.38	0.44	0.46	0.46	0.57
Share of workers in professional and managerial occupations	0.21	0.23	0.26	0.24	0.37
Share minority	0.90	0.87	0.82	0.89	0.40

Sources: All figures with the exception of the employment rates come from Kling and others (2007, table 1). The employment shares for individuals 16 years old or older are calculated from Kling and others (2004, table 2).

a. Average characteristics for the MTO groups describe the traits of the sequence of an individual's addresses between randomization and 2001, weighted by duration. The figures in the final two columns pertain to the five PMSAs containing the MTO cities and are average tract characteristics from the 2000 census, using either poor blacks residing in the tract or poor whites as weights.

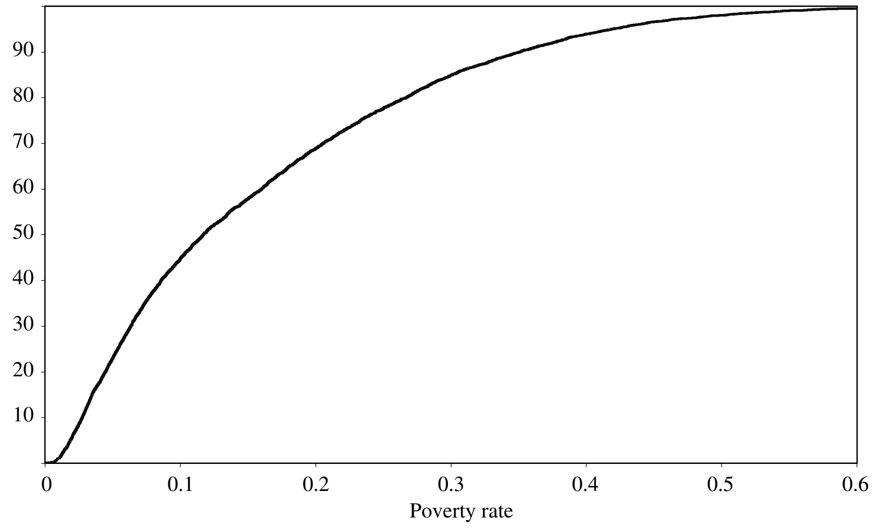
How Big Was the MTO Treatment?

The hypothesis tested above posits that the disparity in demand and supply conditions characterizing the neighborhoods of low-skilled whites and low-skilled blacks helps explain the disparity in employment and earnings between the two groups. The magnitude of non-experimental effects are based on a simple counterfactual: black high school dropouts are relocated to neighborhoods in which demand conditions and labor factor shares are similar to those encountered by white high school dropouts. The extent to which MTO provides a test of variations in these neighborhood conditions depends on whether treatment under MTO did in fact move poor inner-city minority families to neighborhoods comparable with those of low-skilled whites.

Table 6 presents the average characteristics of the census tracts where members of the MTO control, Section 8, and experimental groups resided between randomization and 2001. Census tract characteristics are estimates from the 1990 and 2000 censuses; the figures are averages weighted by the duration of residence in a given census tract.[25] There are notable differences among the three groups, with the Section 8 and experimental groups residing in neigh-

25. For the years between 1990 and 2000, tract characteristics are based on linear interpolations of the 1990 and 2000 values. These results are reported in Kling, Liebman, and Katz (2007) and in Kling and others (2004).

Figure 4. Empirical Cumulative Density Function of 2000 Census Tract Poverty Rates Weighted by Tract Population for the Five MTO PMSAs



Source: Authors' tabulations from the 2000 Census Summary Files 3.

borhoods with lower poverty rates; lower proportions of households on public assistance; higher employment rates; higher proportions of adult workers in professional and managerial occupations; and lower shares of minority residents. However, the average neighborhood of an experimental group household is still quite poor and largely minority. For example, 52 percent reside in neighborhoods with poverty rates that exceed 30 percent, and the minority residents in the census tract of the average experimental group households make up 82 percent of the residents.

Figure 4 indicates how these changes compare with the distribution of poverty concentration across the five MTO metropolitan areas, presenting the empirical cumulative density function of census tract poverty rates weighted by the total census tract population for the areas. This distribution is calculated by using data from the 2000 census SF3 files. A move from a census tract that is 45 percent poor (the rate for the average control group household) to a census tract that is 33 percent poor (the rate for the average experimental group household) constitutes a move from the 96th percentile to the 88th percentile of this distribution.[26]

26. In the unweighted cumulative distribution of census tract poverty rates, rates of 0.45 and 0.33 correspond to the 95th and 86th percentiles, respectively.

Table 6 also provides comparisons of the characteristics of the neighborhoods of the average experimental household with those of other subpopulations in the metropolitan areas. From the SF3 files of the 2000 census, we calculated the values of these neighborhood characteristics for the average poor black person and the average poor white person for the five primary metropolitan statistical areas (PMSAs) within which the MTO experiment was implemented.[27] The characteristics of the neighborhood of the *average* poor black person are nearly identical to the average characteristics of experimental household neighborhoods. In other words, it appears that MTO moved extremely poor minority households from extremely poor neighborhoods to the type of neighborhood in which the average poor black person lived. While that certainly was an improvement, it falls far short of eliminating the racial disparity in neighborhood quality measures that exists in metropolitan areas throughout the country.

This point is further illustrated by the tabulations for poor white people in MTO metropolitan areas presented in the last column of the table. There are very large disparities between the neighborhood of the average poor white person and the neighborhood of the average poor black person. For example, the average census tract poverty rate is 32 percent for poor blacks (at the 87th percentile of the cumulative density function in figure 4) and 17 percent for poor whites (at the 62nd percentile). Fully half of poor blacks but only 15 percent of poor whites reside in neighborhoods where more than 30 percent of the residents are poor. The proportion of households receiving public assistance in poor black neighborhoods is nearly three times that in poor white neighborhoods. Employment rates and the proportion of residents employed in professional and managerial occupations are higher in poor white neighborhoods. Finally, there is an enormous difference—of 49 percentage points—in the proportion of residents who belong to a minority group.

Given the marginal changes in the neighborhood characteristics induced by MTO, what was the effect of treatment under the program on subjects' physical access to employment opportunities? The residential mobility achieved certainly did not integrate these households into their respective PMSAs, given the large share of minority poor households observed for experimental group

27. These are weighted averages of tract characteristics for the five MSAs in which the tract count of the population (either poor black or poor white) is used as the weight. We also tabulated these figures so that each metropolitan area contributes to the weighted average in proportion to the representation of each MSA among MTO households. These alternative results suggest that poor black households lived in neighborhoods that were slightly better than those listed above, indicating that the MTO experimental group resided in neighborhoods that were not as high quality as those of the average black poor person.

households. Therefore, the observed mobility was unlikely to bridge the racial disparities in demand and supply conditions discussed above. Moreover, the conditions for compliance with treatment involved moving to neighborhoods with low poverty rates, not neighborhoods with better proximity to employment opportunities. While poverty concentration and access to employment as commonly measured are certainly negatively correlated, that correlation is far from perfect; there are many wealthy neighborhoods in urban areas with poor access and poor neighborhoods in suburban areas with relatively better access to employment.

In the web appendix to Kling and others (2004), the authors provide estimates of employment growth in the post-assignment zip codes of the three MTO groups.[28] Raphael (1998) and Mouw (2000) both demonstrate a strong partial correlation between black employment outcomes and access measures based on proximity to employment growth. Thus, neighborhood employment growth does provide one previously used gauge of mismatch that is demonstrably positively associated with employment rates.

Table 7 presents these tabulations. The table presents for various time periods the average change in the natural log of employment and the ITT effects on that variable for the experimental group and the Section 8 group. Panel A presents estimates using residential distributions one year after random assignment. Panel B presents figures using the residential distribution of MTO households in 2002. There are very few significant differences in neighborhood employment growth for the experimental group and the Section 8 group relative to the control group. For the period 1994 through 1998 in panel A, experimental group households typically resided in zip codes where employment growth was near zero or slightly negative.[29] The neighborhoods of experimental group households one year after random assignment did experience employment growth over the longer period from 1994 through 2001, but the observed change was nearly identical to that observed for the neighborhoods of the average control group member. The results in panel B using the residential distributions for 2002 are essentially the same.

Therefore, while MTO certainly did induce moves to less poor neighborhoods, the observed changes in neighborhood conditions were relatively small. There is little evidence that the program improved access to employment oppor-

28. Jeffrey R. Kling and others, "Moving to Opportunity and Tranquility," Princeton IRS Working Paper 48, April 2004, web appendix tables A4–A21 (www.nber.org/~kling/mto/481a.pdf [May 12, 2008]).

29. This statement is based on adding the ITT effect for the experimental group to the control group mean.

Table 7. Estimates of Employment Growth in Zip Codes of the MTO Control, Experimental, and Section 8 Groups, One Year after Random Assignment and Residence in 2002[a]

Period	*Control mean*	*Experimental group intent-to-treat effect*	*Section 8 group intent-to-treat effect*
Panel A: One year after random assignment			
Δln employment (1994–95)	-0.008	0.010*	0.013*
		(0.003)	(0.003)
Δln employment (1994–96)	-0.023	0.005	-0.000
		(0.005)	(0.006)
Δln employment (1994–97)	-0.028	0.015*	-0.002
		(0.007)	(0.007)
Δln employment (1994–98)	-0.011	0.007	-0.006
		(0.007)	(0.008)
Δln employment (1994–99)	0.015	0.005	-0.012
		(0.008)	(0.009)
Δln employment (1994–2000)	0.056	0.001	-0.029*
		(0.009)	(0.010)
Δln employment (1994–2001)	0.065	0.001	-0.032*
		(0.009)	(0.010)
Panel B: Residence in 2002			
Δln employment (1994–95)	0.005	0.004	0.012*
		(0.003)	(0.005)
Δln employment (1994–96)	-0.009	-0.006	0.005
		(0.007)	(0.007)
Δln employment (1994–97)	-0.014	0.004	0.005
		(0.008)	(0.009)
Δln employment (1994–98)	0.001	0.003	0.001
		(0.009)	(0.009)
Δln employment (1994–99)	0.024	0.002	-0.003
		(0.010)	(0.010)
Δln employment (1994–2000)	0.050	0.002	-0.007
		(0.010)	(0.011)
Δln employment (1994–2001)	0.050	-0.001	-0.006
		(0.011)	(0.011)

Source: Figures in the table are reproduced from table F14 of Jeffrey R. Kling and others, "Moving to Opportunity and Tranquility," Princeton IRS Working Paper 48, April 2004, web appendix tables A4–A21 (www.nber.org/~kling/mto/481a.pdf [May 12, 2008]).

a. Standard errors are in parentheses. *Significant at the 5 percent confidence level.

tunities or bridged the gap in neighborhood quality between poor blacks and poor whites.

Did the Experiment Have Enough Power to Rule Out Non-Experimental Effect Sizes?

The discussion above suggests that receiving treatment under MTO probably did not eliminate the disadvantages related to access and competition from other workers faced by residents of isolated inner-city neighborhoods. Nonetheless, given the mobility induced by the experiment, how big an employment effect might we have expected, given the results from the non-experimental work? Most important, does the MTO experiment have sufficient power to rule out such magnitudes?

Roughly half of treatment group households leased units in neighborhoods designated by the experiment. The resulting moves had modest effects on neighborhood poverty rates and no measurable effect on physical access to employment. For the sake of argument, however, assume that treatment under the program eliminated half of the relative proximity disadvantage of program participants assigned to the treatment group.

The non-experimental empirical estimates presented above suggest that the effects of mismatch on the employment rate of black high school dropouts on the order of 6 percentage points. The upper-bound estimate for female black high school dropouts is 7 percentage points. Coupled with the observed lease rate and the assumption of elimination of half of the proximity disadvantage, this estimate for women suggests a likely intent-to-treat effect on the order of 1.75 percentage points and an effect of the treatment on the treated of roughly 3.5 percentage points.

To gauge whether the experimental estimates have sufficient power to discriminate against effects of these magnitudes, table 8 presents the upper and lower bounds of the 95 percent confidence intervals for the employment effects listed in table 5. The ITT and TOT effects implied by the upper-bound mismatch effect lie solidly within these confidence intervals for every outcome, with the exception of the confidence interval for the fraction of quarters employed in 2002, where the upper-bound effect lies on the edge of the interval. However, even for this exception, the assumptions that we have made in this thought experiment are generous with regard to the power of this estimate. To start, we are basing our prior and the simulated effect sizes for black female high school dropouts, while 38 percent of MTO participants have high school degrees.[30] Our non-experimental estimates indicate smaller mismatch and supply concentration effects for high school dropouts. Moreover, we have assumed that treatment eliminated half of the spatial proximity disadvantage experienced by low-skilled blacks among program compliers, despite the results discussed in the previous section demonstrating little effect of treatment on access to employment for compliers.

30. Kling, Liebman, and Katz (2007).

Table 8. Estimates of the 95 Percent Confidence Intervals around the MTO Intent-To-Treat and Treatment-on-the-Treated Employment Effect Estimates[a]
Versus control group

	Intent-to-treat confidence interval		*Treatment-on-the-treated confidence interval*	
Period	*Lower bound*	*Upper bound*	*Lower bound*	*Upper bound*
Self-reported employment rate in 2002				
Experimental	-0.026	0.056	-0.053	0.119
Section 8	-0.021	0.069	-0.034	0.114
Fraction of quarters employed in 2002 (administrative data)				
Experimental	-0.050	0.016	-0.107	0.035
Section 8	-0.019	0.047	-0.033	0.077
Fraction of quarters employed in years 1 through 5 after random assignment (administrative data)				
Experimental	-0.031	0.019	-0.067	0.043
Section 8	-0.026	0.028	-0.044	0.046
Fraction of quarters employed in year 5 after random assignment (administrative data)				
Experimental	-0.033	0.037	-0.071	0.081
Section 8	-0.031	0.047	-0.050	0.076

a. Tabulated from effect size estimates and standard errors reported in table 4.

Thus, the experiment did not have sufficient power to reject mismatch effects implied by our non-experimental model results. With regard to the importance of the spatial mismatch hypothesis, the MTO experiment is uninformative.[31]

Conclusion

The MTO experiment represented a bold attempt to study the effects of residing in poverty on an individual's economic, health, and other sociological outcomes. Treated households experienced substantial reductions in neighborhood poverty and improvements in other measures of the average health of their neighborhoods. As we have noted in our review, the experiment was gen-

31. A similar point is made in appendix G of the interim report on MTO in Orr and others (2003). In a very careful and thorough analysis of the initial findings, the authors calculated minimum detectable effect sizes for all outcomes where the interim report did not find statistically significant effects. The authors also calculated 95 percent confidence intervals for those outcome variables. While they were able to conclude that large effects were unlikely for several youth behavioral outcomes, they concluded that moderately large effects on the remaining variables (including post-treatment earnings) were certainly possible and that the experiment had insufficient power to rule out effect sizes for these outcomes that may have been sufficiently large to be important to public policy.

erally unable to reject the null hypothesis of no effects of neighborhood poverty on employment. However, our evaluation of this evidence is that the relatively small mobility effects of the program and the variance of the effect-size estimates cannot rule out neighborhood effects of the range implied by the existing non-experimental literature. The ultimate intent-to-treat effect on neighborhood poverty indicates that most of the net mobility was from extremely poor neighborhoods to the average poor minority neighborhood. Moreover, the existing MTO research indicates that there was little impact on access to employment. Therefore, the absence of employment effects is not especially surprising.

Nonetheless, MTO did reveal significant effects for the mental and physical health of adults and several behavioral outcomes for girls. In addition, experimental group families resided in safer neighborhoods and were happier as a result. Given the relatively modest moves experienced by these households, these findings are quite remarkable. In fact, structural estimates of the effects of poverty and various outcomes from MTO indicate poverty effects in line with non-experimental estimates.[32]

The low compliance rate in the experimental group coupled with the subsequent mobility patterns of that group clearly point to the difficulty of achieving real poverty reduction by relying on residential mobility programs. The low compliance rate is consistent with housing market discrimination against poor minority households in neighborhoods that are less poor, a lack of affordable rental units in those neighborhoods, or a reluctance on the part of the experimental households to abandon familiar neighborhood surroundings. All of these mechanisms are likely at play, and they can explain the post-assignment moves of experimental households back toward poorer neighborhoods quite easily. Together, these findings indicate how difficult it is to counter the social and economic forces that lead to racial and socioeconomic segregation in U.S. cities.

The existence of a spatial mismatch in labor market conditions by race is predicated on the unobserved mechanisms that maintain racial segregation despite incentives for lower-skilled, inner-city minority workers to move to areas of the metropolitan region with more favorable labor market conditions. One of the most problematic aspects of existing non-experimental research on the question is the fact that most studies simply assume that segregation reflects geographically constrained housing choices and that low employment densities are caused by barriers (physical and political) to capital formation in urban neighborhoods—that is, observable variation in mismatch conditions within and/or between metropolitan areas is exogenous. As we have argued, the one

32. Kling, Liebman, and Katz (2007).

recent social experiment did not provide enough variation in underlying neighborhood conditions to resolve this identification problem. Future non-experimental research on the topic should focus on identifying sources of exogenous variation, but that is no substitute for additional experimentation.

Comments

Lisa Sanbonmatsu: In their very interesting paper in this volume, John Quigley and Steven Raphael explore the apparent contrast between the literature on the spatial mismatch hypothesis and recent findings from the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) program. The spatial mismatch hypothesis, first suggested by John Kain in 1968, posits that employment levels for blacks are lower than for whites because there are fewer jobs close to black residential areas than to white areas.[1] Empirically testing the spatial mismatch hypothesis is complicated by the fact that non-experimental studies are potentially affected by bias due to the selection of different types of people into different neighborhoods. In contrast, MTO, a randomized study, provides an exogenous source of variation in the neighborhood environments of otherwise similar low-income families. MTO randomly assigned public housing families with children to either a control group, a group offered a standard housing voucher, or an experimental group offered a restricted housing voucher that could be used only to move to a low-poverty neighborhood.

Part of the interest in mobility programs stemmed from results of the Gautreaux program, which, although not a randomized study, suggested that moves to new neighborhoods might be associated with large gains in employment.[2] However, findings from the interim MTO evaluation do not show any statistically significant impacts of MTO-induced mobility on employment or earnings outcomes measured five years after randomization.[3] The interim results have been interpreted by many people as suggesting that spatial mismatch can-

This comment reflects the views of the author and does not necessarily reflect the views or policies of the U.S. Department of Housing and Urban Development or of the U.S. government.

1. Kain (1968); Ihlandfeldt and Sjoquist 1998). Kain did not use the term "spatial mismatch," and different authors seem to use the term slightly differently. I define it to be consistent with Ihlandfeldt and Sjoquist's summary: "A simple statement of the SMH is that there are fewer jobs per worker in or near black areas than white areas."

2. Popkin, Rosenbaum, and Meaden (1993).

3. Orr and others (2003); Kling, Liebman and Katz (2007).

not be an important explanation for the low levels of labor market participation in many of the nation's most disadvantaged urban areas. Quigley and Raphael help demonstrate why the MTO interim findings cannot be interpreted in that manner, and it is useful to note that their interpretation is consistent with the interpretations of the MTO evaluation teams.[4] As a member of one of the MTO evaluation teams, I appreciate the opportunity to comment further on three important questions that the authors address:

—Is MTO an effective test of spatial mismatch?

—How big was the MTO treatment?

—What can MTO tell us about mobility programs?

Is MTO an Effective Test of Spatial Mismatch?

Quigley and Raphael conclude that the MTO intervention is a weak test of spatial mismatch because it does not appear to have improved access to jobs and does not have the statistical power to rule out small or moderate effects on employment. They take the latter point a step further, illustrating that the employment effects one might expect to observe due to spatial mismatch are within the confidence bounds of the MTO findings. I would add that MTO still has the power to rule out some of the larger estimates in the literature.

IMPACT ON JOB ACCESS AND OTHER PROXIES FOR SPATIAL MISMATCH. The MTO intervention induced families to move to neighborhoods that were substantially less poor and had higher levels of employment and lower crime rates; however, there is little to suggest that MTO had a substantial impact on spatial mismatch. Proxies for spatial mismatch include commute time, distance to jobs, place of residence (central city or suburbs), ratio of jobs to workers, and job growth.[5] Kling, Liebman, and Katz (2007) examined MTO's effects on job growth (at the zip code level) and found few differences between the residences of treatment and control groups. Similarly, on other measures, such as commute time and access to transportation, the MTO survey data show no gains for the experimental (restricted voucher) group.[6] In fact, in qualitative interviews, some program movers indicated that they found transportation services to be lacking in their new communities.[7] The transportation challenges are perhaps not surprising given that only about 17 percent of MTO families had

4. See, for example, Kling, Liebman, and Katz (2007).
5. Ihlanfeldt and Sjoquist (1998); Hellerstein, Neumark, and McInerney (2007).
6. Kling and others (2004).
7. Popkin, Harris, and Cunningham (2002); Turney and others (2006).

a car at baseline and families moved away from highly urban neighborhoods (my calculations).

EFFECT SIZES. Orr and others (2003), the main interim MTO evaluation report for the U.S. Department of Housing and Urban Development, considers the question of how large a program effect would need to be in order for the MTO interim evaluation to have detected it. Quigley and Raphael, in a slight twist on that question, ask how big of an effect one would *expect* for MTO under the spatial mismatch hypothesis, and then they explore whether MTO would have had the power to detect such an effect. They used non-experimental models of metropolitan area employment rates to estimate the expected effect size and two measures of the residential gap between blacks and whites: a jobs-people dissimilarity index and the neighborhood "supply" of high school dropouts. The dissimilarity index compares the distribution of jobs and people across zip codes; it can be thought of as the fraction of people of a specific race who would need to move to be distributed in the same manner as jobs.[8] Quigley and Raphael's models suggest that larger racial gaps on both of these measures are associated with lower employment rates for blacks than for whites.

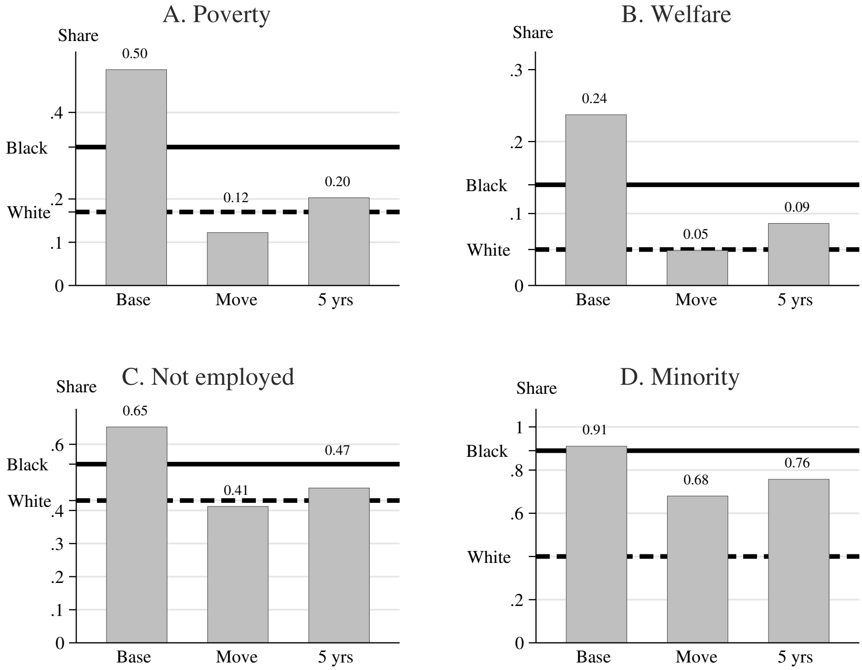
Using the coefficients from their models, Quigley and Raphael estimate an expected treatment-on-the-treated (TOT) effect on employment of about 3.5 percentage points for MTO, assuming hypothetically that the intervention had eliminated half of the spatial mismatch for experimental group treatment compliers. The assumption that MTO had eliminated half the black-white difference in spatial mismatch is an upper-bound estimate as there is little evidence (as discussed above) that MTO increased access to jobs. The authors show that the upper-bound estimate of 3.5 percentage points for MTO is within the confidence intervals of the MTO interim findings. They help illustrate that MTO cannot rule out modest employment effects; however, it is important to note that MTO does have enough statistical power to rule out the larger effects that some non-experimental studies of neighborhood effects suggest.

How Big Was the MTO Treatment?

While I agree that MTO was a weak test of spatial-mismatch, I disagree with Quigley and Raphael's more general conclusion that MTO's "observed changes in neighborhood conditions were relatively small." The authors examined average neighborhood characteristics of the experimental group and concluded that "it appears that MTO *moved* extremely poor minority households from

8. Raphael and Stoll (2002).

Figure 1. Average Neighborhood Characteristics of Experimental Compliers Compared with Those of Poor Blacks and Poor Whites[a]



Source: Author's calculations using data from the U.S. Department of Housing and Urban Development.
a. Sample includes adult experimental compliers who were interviewed in 2002 (n = 694). Mean neighborhood characteristics are my calculations based on 2000 census tracts. The horizontal lines in the graphs represent the average neighborhood for poor whites (dashed line) and poor blacks (solid line) as reported by Quigley and Raphael.

extremely poor neighborhoods to the type of neighborhood in which the average poor black person lived [italics added]." However, because not all families complied with the treatment and the baseline neighborhoods of MTO participants were extremely disadvantaged, it is perhaps not surprising that when one averages the characteristics of the neighborhoods of both experimental noncompliers and compliers they look disadvantaged. When one actually looks at the neighborhoods that the compliers initially moved to using a program voucher, the story is quite different: it is clear that MTO initially moved families to neighborhoods that, although still predominantly minority, were on many socioeconomic and educational dimensions comparable to or better than the neighborhoods of poor whites.

Figure 1 shows the average neighborhood characteristics of the residential addresses of experimental group compliers at three points in time: prior to treatment (base); after the initial move under MTO with a restricted voucher (move);

and at the time of the interim evaluation (approximately 5 years after compliers entered the program), using data from HUD on adult participants interviewed in 2002. For comparison, the graphs include lines representing Quigley and Raphael's estimates of the average neighborhood characteristics for poor blacks (solid line) and poor whites (dotted line). The initial new neighborhoods of MTO experimental compliers, represented by the middle bars in each graph, were actually similar to or better than the average neighborhood of poor whites on measures such as share of poor residents, residents not employed, welfare recipients, and residents with less than a high school education (not shown).

Although subsequent moves by compliers tended to be to less advantaged neighborhoods, estimates of the treatment-on-treated effects are still substantial 5 years after random assignment. For example, the program appears to have lowered the average neighborhood poverty rate for experimental compliers by 17 percentage points.[9] This is larger than Quigley and Raphael's metric of the gap in neighborhood poverty of poor blacks and poor whites, which is a 15 percentage point gap.[10]

The intent-to-treat effects (ITT) capture the effects of the program for everyone offered a voucher regardless of whether they used it. Due to the compliance rate of the program, ITT effects are about half the size of the TOT effects, but they still show that the experimental voucher offer led otherwise similar families to live in neighborhoods that some five years after the initial voucher offer were better off than those of the control group. The ITT estimates from the five-year evaluation show that the neighborhoods of the experimental group had higher proportions of neighbors who had incomes of at least twice the poverty level (ITT of .10, or 28 percent higher than the control mean of .37), who were college educated (.038, or 25 percent higher), who were owners of their housing unit (.095, or 41 percent higher), and who were two-parent families (.067, or 17 percent higher).[11]

Another way to think about the magnitude of the neighborhood changes is from the perspective of the participants. At entry into the program, more than three-quarters of participating families indicated that getting away from gangs and drugs was their primary or secondary reason for moving.[12] Families signing up for the program reported very high levels of victimization, with almost a quarter reporting that a household family member had been threatened with a gun or knife or had been beaten or assaulted in the previous six months.[13]

9. Orr and others (2003, exhibit 2.8, p. 42).
10. Quigley and Raphael, table 6.
11. Orr (2003, exhibit 2.10, p. 45).
12. Orr and others (2003, p. 17).
13. Orr and others (2003, exhibit C1.3, p. C-3).

Approximately 5 years after randomization, participants in the experimental group were 20 percent less likely than controls to report the victimization of a family member during the previous six months (control mean = .21), 26 percent less likely to report seeing drug transactions in the previous month (control mean = .45), 26 percent more likely to report feeling safe at night (control mean = .55), and 29 percent more likely to be satisfied or very satisfied with their neighborhoods (control mean = .475).[14] The experimental group's reports of greater safety are supported by administrative data on local area crime rates, which show lower rates for the neighborhoods of the experimental group than for those of controls.[15] In light of these neighborhood improvements in safety and victimization rates, it is perhaps not so surprising that the interim evaluation found large benefits in the self-reported mental health of adults and important reductions in violent crime by youth (although some lower crime benefits were partly offset by negative behaviors for boys).[16]

What Can MTO Tell Us about Mobility Programs?

Ihlanfeldt and Sjoquist (1998) categorizes the policy options for addressing spatial mismatch as those that move jobs closer to workers through inner-city development, move workers closer to jobs through residential desegregation, and make it easier for workers to get to existing jobs. The very fact that MTO itself provides a weak test of spatial mismatch theory means that MTO is quite informative about the potential of residential mobility programs to move workers closer to jobs. MTO moves were successful in substantially changing the socioeconomic composition of neighborhoods for families as well as safety, mental health, and a range of other outcomes. But MTO moves for some reason did not have a large impact on proximity to jobs.

Determining why MTO families did not move to areas with better access to jobs—and for that matter why only around half of the families assigned to the MTO experimental treatment relocated through the MTO program—are important questions for housing policymakers and for social policy more generally. Quigley and Raphael note that candidate explanations include housing market discrimination, the difficulty of leaving behind familiar neighborhoods, and the limited supply of low-income housing units. Shroder's research on the factors affecting whether MTO families leased a unit using a program voucher sug-

14. Orr and others (2003, exhibit 3.5, p. 66).
15. Ludwig and Kling (2007).
16. Kling, Liebman, and Katz (2007); Kling, Ludwig, and Katz (2005).

gests additional supply factors as well as demand side factors, including a preference for living in the suburbs (or beyond), dissatisfaction with their current neighborhood, and uncertainty about liking a new neighborhood.[17] It is difficult to isolate the independent contributions of each of these plausible hypotheses from the MTO data. This is an area where non-experimental research approaches may generate considerable value for future policy design.

Summary

In summary, Quigley and Raphael's paper is helpful in understanding what MTO can and cannot tell us about spatial mismatch. MTO is a weak test of spatial mismatch because it does not appear to have increased job access for participants, and the interim estimates, ex post, do not have the statistical power to rule out small to moderate effects on employment. MTO helps highlight the limitations of and obstacles involved in trying to move people closer to jobs using residential mobility strategies.

But to say that MTO did not have much impact on available measures of spatial mismatch is not to say that MTO is a weak intervention. MTO had substantial impacts on many economic dimensions of neighborhoods as well as on safety and a range of other outcomes, such as health, that have important implications for social welfare and consequently for benefit-cost analyses of mobility programs. Moreover, spatial mismatch is only one of several hypothesized links between mobility and employment outcomes. For example, numerous theories rely on social interactions as a mechanism for explaining why neighborhoods might impact labor market outcomes. It is possible that these types of effects could be more pronounced over time if MTO families became more socially integrated into their new neighborhoods, although it is also possible that MTO would not have any impact on labor market outcomes even over the long term if neighborhood environments converged over time for the treatment and control groups. That is one of the key empirical questions that our team at the National Bureau of Economic Research will be addressing as part of our ongoing long-term evaluation of the MTO demonstration.

17. Shroder (2002).

Bruce A. Weinberg: Economists have considered the effects of neighborhoods on employment at least since the publication of Kain (1968). Experimental and quasi-experimental studies often have been taken to be the standard for estimating neighborhood effects. In pointing out the power limitations of these studies, Quigley and Raphael's paper makes a valuable contribution to the literature. This comment provides some additional power assessments, especially of the social effects of neighborhoods, and situates Quigley and Raphael in the literature.

The literature has considered at least two broad classes of explanations of the effects of neighborhoods on employment. The first dates back to Kain, focusing on the role of job proximity. Job proximity is believed to matter because it is less costly for people both to commute to jobs and to search and interview for jobs that are closer to where they live. The second class of explanations focuses on the social effects of neighborhoods. Here it is argued that in a neighborhood where employment is low, being unemployed is regarded as socially acceptable, unlike in a neighborhood with high employment. In addition, insofar as people find out about jobs from employed acquaintances, having employed neighbors may improve access to information about job opportunities. Rightly or wrongly, the literature, which originally focused on job proximity, has shifted much of its focus to the social arguments.

The literature in both classes of explanations has gone through at least two generations. A typical first-generation study in either class related an individual's employment, earnings, or hours worked to some measure of job proximity or to the social characteristics of his or her neighborhood.[1] Such studies frequently found strong relationships between neighborhood characteristics and labor market activity. An obvious concern with the studies is that the neighborhoods that people "choose" are likely to be endogenous. The people who live on Chicago's Gold Coast are probably different from those living on Chicago's South Side, and while researchers may be able to measure and control for many of those differences, they will not observe many others.

These issues have been understood for a while, and a substantial second-generation literature that tries to address selection into neighborhoods has now developed. Many such studies employ experiments or quasi-experiments to obtain variations in the neighborhoods (or other social groups) in which people are located, and, in contrast to other estimates, they typically obtain small and statistically insignificant estimates of the effects of neighborhoods on employment (and frequently on other variables). One of the most visible and ambitious attempts to obtain exogenous variations in neighborhoods is the

1. See references in Weinberg, Reagan, and Yankow (2004).

Moving to Opportunity program, which is the focus of Quigley and Raphael's paper.

Quigley and Raphael focus on the power of Moving to Opportunity (MTO) to rule out conventional estimates of the effects of neighborhoods on employment through job access. They argue that estimates of the employment effects of neighborhoods from the project, while small, are not statistically distinguishable from those that might be expected based on reasonable non-experimental estimates of the effects of job proximity. Essentially the noise in the estimates is too large to rule out a wide range of reasonable estimates.

While Quigley and Raphael focus on neighborhood effects on employment due to job access, Moving to Opportunity is much more oriented toward the social effects of neighborhoods. People in the experimental group were offered vouchers to obtain housing in relatively low-poverty neighborhoods. A study that focused on job access in particular might have moved people to neighborhoods that were closer to jobs or assisted them in paying for cars or car insurance. But because a program that helped people move to neighborhoods with better social conditions may well have improved their job access too, it is important to assess the implications of MTO for employment due to improvements in job access.

Given the MTO program's emphasis on the social effects of neighborhoods, I provide some assessment of the power of the program to rule out non-experimental estimates of the social effects of neighborhoods. I also provide some additional evidence on the power of MTO to reject conventional estimates of the effects of neighborhoods through job access that is broadly consistent with that of Quigley and Raphael. Studies of the effects of job access on labor market outcomes have generally taken one of two approaches. They may look within metropolitan areas to study, for instance, whether people living in neighborhoods with better job access have higher employment rates than people living in neighborhoods with worse job access. Or they may exploit cross–metropolitan area variations in job access between, for instance, blacks and whites. Quigley and Raphael take the latter approach. For my assessment of the power of Moving to Opportunity, I exploit variations within metropolitan areas. This source of variation is more comparable to that in MTO, which involves relocating people within metropolitan areas rather than between metropolitan areas.

The analysis requires estimates of the effects of neighborhoods on employment. I draw those estimates from Weinberg, Reagan, and Yankow (2004), which provides a broad range of non-experimental estimates using data from the 1979 National Longitudinal Survey of Youth. The estimates have some advantages and disadvantages for the present purposes. The sample is large, with 27,313 observations on 2,352 individuals. The panel nature of the data permits a variety of specifications, which cover many approaches in the non-experimental literature, from ordinary least squares estimates (with and without a wide range of controls), to fixed effects (within-person) estimates, to estimates that allow for individual fixed effects and for individual-specific experience profiles. While the sample is young and contains oversamples of blacks and Hispanics, it focuses on men.[2] In general, I expect women to have higher labor supply elasticities than men, but women in the groups studied here tend to have relatively strong labor force attachment. Weinberg (2000, 2004) finds little systematic differences between men and women in the effects of job access on the employment of young, less educated people.

While Weinberg, Reagan, and Yankow (2004) studies a variety of dependent variables, none are directly comparable with those reported in the MTO studies. The closest is annual hours. The mean of annual hours in the sample is 1,885 hours, with a standard deviation of 915. Thus, someone who is 1 standard deviation beneath the mean would work 970 hours. The MTO sample likely has a lower number of hours than respondents to the National Longitudinal Survey of Youth. To convert changes in annual hours to employment rates for comparability with the MTO studies, changes in hours are divided by 1,000 hours. Assuming fewer hours also raises the estimates, enhancing MTO's ability to reject them. The conversion between changes in annual hours and changes in employment rates also generates some slippage, but again it likely serves to overstate MTO's ability to reject estimates. In particular, if people reduce hours without changing weeks (or quarters) worked, employment rates will be unchanged.

The employment rate of men, the main independent variable used in Weinberg, Reagan, and Yankow (2004) to measure neighborhood social conditions, is quite close to that available in the MTO studies.[3] The study also contains measures of job access, although they are not comparable with the little data on job access reported in MTO studies. Given the various sources of slippage, the analysis should be regarded as an initial analysis. I nonetheless hope that it will provide suggestive evidence on the power of Moving to Opportunity to reject non-experimental estimates of the effects of neighborhoods.

Table 1 reports estimates. The top panel studies the effect of employment rates. The first row reports the point estimates from three of Weinberg, Rea-

2. The sample had a mean education level of 11.99 years and an average of 9.75 years of potential experience, implying an average age of 27.74 years.

3. Weinberg, Reagan, and Yankow (2004) obtains similar results when women's employment rate is taken as the relevant neighborhood social variable.

Table 1. Comparison of Moving to Opportunity Estimates to Non-Experimental Estimates from the 1979 National Longitudinal Survey of Youth

	1979 National Longitudinal Survey of Youth estimates			*Moving to Opportunity estimates and 95 percent confidence intervals*							
				Employed (self-reported)		*Share of quarters employed in 2001 (administrative data)*		*Share of quarters employed in years 1 through 5 (administrative data)*		*Share of quarters employed in year 5 (administrative data)*	
Effect	*OLS*	*Fixed effects*	*Fixed effects and individual-specific experience profiles*	*Estimate (standard error)*	*Lower / upper bound*	*Estimate (standard error)*	*Lower / upper bound*	*Estimate (standard error)*	*Lower / upper bound*	*Estimate (standard error)*	*Lower / upper bound*
Effect of employment rate of adult men on annual hours	835.497	243.049	148.058								
	(76.24)	(57.356)	(62.402)								
Effect of a 1 standard deviation (.148) change on annual hours	123.654	35.971	21.913								
Effect of a 1 standard deviation (.148) change on employment rate assuming 1,000 hours a year	0.124	0.036	0.022								
ITT experimental control: effect of .074 change in neighborhood employment	0.062	0.018	0.011	.015	-.026	-0.017	-0.050	-0.006	-0.031	0.002	-0.033
				(.021)	.056	(.017)	0.016	(.013)	0.019	(.018)	0.037
ITT section 8 control: effect of .056 change in neighborhood employment	0.047	0.014	0.008	.024	-.021	0.014	-0.019	0.001	-0.026	0.008	-0.031
				(.023)	.069	(.017)	0.047	(.014)	0.028	(.020)	0.047
TOT experimental control: effect of .159 change in neighborhood employment	0.133	0.039	0.024	.033	-.053	-0.036	-0.105	-0.012	-0.067	0.005	-0.071
				(.044)	.119	(.035)	0.033	(.028)	0.043	(.039)	0.081
TOT section 8 control: effect of .093 change in neighborhood employment	0.078	0.023	0.014	.040	-.034	0.022	-0.033	0.001	-0.044	0.013	-0.050
				(.038)	.114	(.028)	0.077	(.023)	0.046	(.032)	0.076
Effect of job density on annual hours	-13.308	8.943	10.095								
	(9.652)	(6.780)	(7.439)								
Effect of a 1 standard deviation change (1.033) on annual hours	-13.747	9.238	10.428								
Effect of a 1 standard deviation change (1.033) on employment rate assuming 1,000 hours a year	-0.014	0.009	0.010								

Source: Moving to Opportunity estimates come from Kling and others (2004, tables 2, 3, and 4). National Longitudinal Survey of Youth Estimates come from Weinberg, Reagan, and Yankow (2004, tables 2 and 4). For intent to treat and treatment on the treated, the effect of the program on neighborhood employment is shown in the first column.

gan, and Yankow's specifications—ordinary least squares estimates with a wide range of explicit control variables (first column), fixed effects (second column), and fixed effects with individual-specific experience profiles (third column). Beneath each coefficient is its standard error and the implied effect of a 1 standard deviation change in the variable. The next row reports the implied effect of a 1 standard deviation change on the employment rate, assuming that the respondent works 1,000 hours annually. The estimates and the implied effects of a 1 standard deviation change fall dramatically from the ordinary least squares specification to the fixed effects model and fall somewhat further when individual-specific experience profiles are included.[4]

The next four rows report the implied effect of MTO's intent to treat (ITT) and treatment on the treated (TOT) for both the experimental group and the Section 8 group compared with the control group. The implied effects for the fixed effects estimates with or without individual-specific experience profiles are quite small. According to my calculations, the largest effect—the treatment on the treated for the experimental group—would have raised employment by less than 4 percent (fixed effects only) or less than 2.5 percent (fixed effects with individual-specific experience profiles).

The next eight columns report the point estimates and standard errors for the MTO estimates and the lower and upper bounds of the implied 95 percent confidence intervals for a variety of employment measures. Comparison of Weinberg, Reagan, and Yankow's estimates with the upper bounds of the 95 percent confidence intervals of the MTO estimates indicates that the ordinary least squares estimates exceed the upper bounds of the confidence intervals in twelve of the sixteen comparisons (each of the four implied effects compared with the four confidence intervals). Of the sixteen comparisons for the fixed effects (only) estimates, only two of the point estimates exceed the upper bound of the 95 percent confidence interval. None of the estimates that include individual-specific experience profiles are greater than the upper bound on the 95 percent confidence interval. These results suggest that the Moving to Opportunity program has the power to reject naive estimates, which do not control for selection into neighborhoods, but that it has considerably less power against estimates that control for neighborhood selection.

The estimates for self-reported employment are especially striking. Here, the MTO point estimates, though statistically insignificant, are actually *larger*

4. Note that the ordinary least squares estimates may exceed the estimates with fixed effects, in part because the ordinary least squares estimates capture the effect of living in better neighborhoods as a child while the fixed effects estimates capture only the contemporaneous effect of neighborhoods.

than those implied by the non-experimental study once fixed effects are included. The upper bounds of the confidence intervals exceed the estimates from the fixed effects (only) models by a factor of 3. The upper bounds of the confidence intervals exceed the estimates from the models with fixed effects and individual-specific experience profiles by a factor of 5 (experimental versus control) to an order of magnitude (Section 8 versus control).

The bottom panel of the table shows estimates for the effects of job access, whose structure is similar to that for neighborhood employment. Because the few job access measures reported in MTO studies are quite different from those used in Weinberg, Reagan, and Yankow (2004), the table does not report the implied effects of Moving to Opportunity through job access. Even though a direct comparison is not possible, it is worth noting that in all but one case, Moving to Opportunity raises the employment rate in a person's neighborhood by less than 1 standard deviation. If the same holds true for the effects of Moving to Opportunity on job access, then the effect of a 1 standard deviation change in job access will overstate the effects of Moving to Opportunity on job access. Viewed in this light, it is noteworthy that Weinberg, Reagan, and Yankow's estimates of the effects of a 1 standard deviation change from models with fixed effects (with or without individual-specific experience profiles) are smaller than the upper bounds of *all* the 95 percent confidence intervals of the MTO estimates. These estimates corroborate Quigley and Raphael's assessment of the power of the Moving to Opportunity program to reject non-experimental estimates of the effects of job access.

For the reasons discussed above, these comparisons should be taken with caution. In addition to gender differences in responsiveness to social interactions, Weinberg, Reagan, and Yankow (2004) looks at the relationship between employment and specific aspects of neighborhoods. The MTO estimates give the total effect from changing neighborhoods, which incorporate changes in all neighborhood characteristics. Given that the correlation between various characteristics is less than 1, any single dimension will capture only a portion of the variation. Still, if one were to assume that the true effects of Moving to Opportunity are double those implied by the calculations above based on Weinberg, Reagan, and Yankow's analysis, the stronger estimates would exceed the upper bounds of the 95 percent confidence intervals of the MTO estimates in only two of the sixteen cases for the effects of job access and in only four of the sixteen cases for the social effects of neighborhoods.

There is another way of assessing the power of Moving to Opportunity to reject estimates of the social effects of neighborhoods. One can estimate the implied endogenous effects and multipliers from MTO studies. If one thinks

Table 2. Endogenous Effects for Employment Implied by Moving to Opportunity

	Employed (self-reported)	*Share of quarters employed in 2001 (administrative data)*	*Share of quarters employed in years 1 through 5 (administrative data)*	*Share of quarters employed in year 5 (administrative data)*
Estimate				
ITT experimental control change	0.203	-0.230	-0.081	0.027
ITT Section 8 control change	0.429	0.250	0.018	0.143
TOT experimental control change	0.208	-0.226	-0.075	0.031
TOT Section 8 control change	0.430	0.237	0.011	0.140
Upper bound				
ITT experimental control change	0.757	0.221	0.263	0.504
ITT Section 8 control change	1.232	0.845	0.508	0.843
TOT experimental control change	0.748	0.205	0.270	0.512
TOT Section 8 control change	1.226	0.827	0.495	0.814

Source: Estimates are author's calculations based on estimates in Kling and others (2004, tables 2 and 3). The estimates divide the point estimates reported in table 1 above by the effect of Moving to Opportunity on neighborhood employment (.074, .056, .159, .093). The upper-bound figures divide the upper bounds reported in table 1 by the effect of Moving to Opportunity on neighborhood employment.

of a person's employment as being influenced by the employment of his or her neighbors, the endogenous effect measures how much a person's probability of employment and hours worked if employed would increase (in percentage points) if his or her average neighbor's employment increased by 1 percentage point. If endogenous effects are present, a change (government policy change, movement of people in or out of the neighborhood, and so forth) that affects one person's behavior will generate a feedback process. The multiplier gives the total effect (in percentage points) that arises from a change that would raise employment by 1 percentage point in the absence of any feedback process.

To estimate the endogenous effects, I assume that the entire effect of neighborhoods comes from an endogenous effect from neighborhood employment, which, while unlikely, would be consistent with the approach taken in much of the empirical work in this area.[5] I also assume that the changes in the various employment measures used as dependent variables in the MTO studies represent, at an individual level, the same construct and are scaled in a way that is similar to that of the neighborhood employment measure.

The estimates are reported in Table 2. The first set of columns shows the mean of the implied endogenous effects. The lowest estimates are negative, but

5. While theoretical work often distinguishes endogenous effects from exogenous and correlated effects, empirical work rarely does. Very little work, either theoretical or empirical, considers multidimensional social effects (that is, whether one person's employment is influenced not only by his or her neighbors' employment but also, for example, by the amount that they spend on status symbols).

the highest are more than .4. The average of the implied endogenous effects is .095, which implies a social multiplier of 1.104. That is not a large multiplier, but it is not trivial either, implying that an exogenous shift that would otherwise raise employment by 1 percentage point would, because of social interactions, raise employment by 1.1 percentage points.

The next set of columns reports the endogenous effect implied by the upper bounds of the 95 percent confidence intervals. Here the estimates range from .2 to more than 1. The high end of this range is surely too high—if employment were not bounded, a multiplier of more than 1 would imply that no finite equilibrium exists! The mean of these endogenous effects is .642, which implies a multiplier of 2.792. Although they constitute the high end of the estimates that are consistent with Moving to Opportunity, social effects of neighborhoods of this magnitude would be regarded as quite strong.

In principle more precise estimates could be obtained by averaging the various estimates. Doing so would reduce the upper-bound estimates of the social effects of neighborhoods because the standard errors for the averaged estimates would be lower than those for the individual estimates. That cannot be done using the available data, because the various estimates that I have are not independent and there is no information available on the covariance between them.

Again, while the analysis is only suggestive, it does raise questions about the power of Moving to Opportunity to rule out even relatively strong effects of neighborhoods. These results corroborate the value of additional studies of the power of Moving to Opportunity to reject non-experimental estimates of the effects of neighborhoods arising from both job access and social effects.

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