

UCLA

UCLA Previously Published Works

Title

Employment proximity and outcomes for Moving to Opportunity families

Permalink

<https://escholarship.org/uc/item/6qm4638r>

Journal

Journal of Urban Affairs, 39(4)

ISSN

0735-2166

Authors

Lens, Michael C
Gabbe, CJ

Publication Date

2017-05-19

DOI

10.1080/07352166.2016.1255528

Peer reviewed



HHS Public Access

Author manuscript

J Urban Aff. Author manuscript; available in PMC 2017 October 03.

Published in final edited form as:

J Urban Aff. 2017 ; 39(4): 547–562. doi:10.1080/07352166.2016.1255528.

Employment proximity and outcomes for Moving to Opportunity families

Michael C. Lens and C. J. Gabbe

University of California, Los Angeles

Abstract

The Moving to Opportunity for Fair Housing Demonstration (MTO) randomly assigned housing vouchers to public housing residents in an experimental test of the effect of neighborhood and location on household outcomes. In terms of adult employment outcomes, the 2 treatment groups did not significantly differ from the control group. We use MTO data to examine whether spatial proximity to jobs and job growth explains this lack of treatment effect. We first estimate differences in access to jobs and job growth for the 3 MTO groups. We then use 2-stage least squares models to test relationships between employment accessibility and 2 key outcomes: employment status and earned income. We find that employment accessibility declined for all groups, and these declines were strongest for the 2 treatment groups. However, our results show essentially no effect of employment proximity on earnings or employment status for MTO participants.

Launched by the U.S. Department of Housing and Urban Development (HUD) in 1994, the Moving to Opportunity for Fair Housing Demonstration (MTO) randomly assigned housing vouchers to public housing residents in an experimental test of the effect of neighborhood and location on household outcomes. Policymakers were optimistic that moving to low-poverty neighborhoods would help families access areas of metropolitan job growth and gain entrance into middle-class social networks, leading to new job opportunities (Orr et al., 2003).

Although there were important positive outcomes, including safer neighborhood locations, improved adult health, and, for child participants, increased future college attendance and earnings (Chetty, Hendren, & Katz, 2015; Sanbonmatsu et al., 2011), the adult employment outcomes for the two treatment groups did not significantly differ from those of the control group (Kling, Liebman, & Katz, 2007; Sanbonmatsu et al., 2011). Explanations include the lack of attention to human capital-based barriers to employment, reduced transportation access, high work rates for the control group in the context of welfare reform and a strong economy, and decreased social ties with similarly low-skilled workers (Kling et al., 2007; Leventhal & Brooks-Gunn, 2003; Sanbonmatsu et al., 2011; Turney et al., 2006).

CONTACT: Michael C. Lens, mlens@ucla.edu, Department of Urban Planning, UCLA Luskin School of Public Affairs, University of California, Los Angeles, 3250 School of Public Affairs Building, Box 951656, Los Angeles, CA 90095-1656.
C. J. Gabbe is now at Santa Clara University.

We use the interim wave of MTO data to estimate the role of another potential explanation for the lack of a treatment effect: spatial proximity to jobs and job growth. Specifically, we address two key questions. First, did MTO participants use their housing vouchers to reach areas near more jobs? Second, is proximity to jobs associated with positive employment outcomes for MTO participants? The latter question allows us to use the MTO experiment to test the spatial mismatch hypothesis (Kain, 1968), which contends that low-income and minority populations are spatially isolated from jobs and that this isolation contributes to unemployment.

Given the MTO experiment centered on changing residential location, the dearth of research on the role of spatial proximity to employment on MTO outcomes is an important research gap. Existing research on the role of employment accessibility in MTO outcomes (de Sousa Briggs, Popkin, & Goering, 2010; Kling et al., 2007) uses coarse geographic and employment measures. Using more spatially sophisticated measures of employment accessibility, we first estimate differences in access to jobs and job growth for the three MTO groups. We then use two-stage least squares (2SLS) models to test relationships between employment accessibility and two key outcomes: employment status and earned income. Our two-stage models utilize random assignment to address the potential selection bias inherent in a simple association between employment status or earnings and housing location.

The first finding is that employment accessibility declined for all groups. Though all groups had worse job accessibility at the MTO interim evaluation than at baseline random assignment, these declines were strongest for the two treatment groups. In some ways, this is not surprising, given that some participants used vouchers to move farther away from the central city, the traditional employment center, perhaps to more “at-risk” suburban areas with minimal job opportunities (Imbroscio, 2012). However, we also find that moves away from job growth did not specifically impact economic outcomes—our models suggest that there is essentially no effect of employment proximity on earnings or employment status for MTO participants.

These findings suggest that we should be less concerned about spatial mismatch for low-skilled workers. Rather than addressing poor employment outcomes through housing mobility programs and the like, more consequential policy interventions would focus on human capital, employment networks, employment options for people with disabilities, and transit or automobile access. However, although we do not find that spatial mismatch is particularly consequential for earnings and employment status for households receiving housing assistance, we suggest that housing policymakers should be cautious when promoting housing mobility for subsidized households. In particular, given the emphasis on low-poverty neighborhoods with good schools and other amenities, suburban areas are often targeted. However, low-income households can typically only afford housing in job-poor suburban areas (Raphael & Stoll, 2010). For people who struggle to find work, it makes little sense to actively promote moves away from centers of employment.

Empirical evidence

Housing mobility programs

In recent decades, housing mobility programs—most notably the Gautreaux program in Chicago and the MTO demonstration—have improved our understanding of the role of residential location in determining household outcomes. The Gautreaux program was created in Chicago in 1976 as a result of a series of lawsuits against the Chicago Housing Authority and HUD. Gautreaux offered Black families in Chicago Housing Authority housing the opportunity to move to mostly White neighborhoods (Rubinowitz & Rosenbaum, 2002). The program moved more than 7,000 families between 1976 and 1998 (Keels, Duncan, Deluca, Mendenhall, & Rosenbaum, 2005). After moving, program participants experienced positive employment outcomes and their children had substantial schooling improvements. In particular, suburban movers had greater gains in employment than urban movers, although there were no effects on hours worked or wages (Popkin, Rosenbaum, & Meaden, 1993; Rosenbaum, 1995). However, Gautreaux participants were not randomly assigned, meaning that selection bias limits the strength of causal conclusions scholars can make about neighborhoods and these important outcomes from this study. Regardless, the results sparked optimism in the potential benefits of poverty deconcentration and inspired further research.

MTO was the first randomized experiment to study the effects of residential location through a housing mobility program. In 1993, Congress created MTO to test some of the results from Gautreaux and to reduce the concentration of inner-city families in poverty, address extreme racial segregation in public housing, and employ a randomized social experiment to test the effects of housing policy (Goering, 2005). Families in public housing applied to participate and were randomly assigned to one of three groups: the *experimental group* received a voucher that could only be used in a low-poverty neighborhood, relocation counseling, and search assistance; the *comparison group* was given counseling and a standard Section 8 subsidy that could be used in any neighborhood; or the *control group*, in which the family was given no assistance to leave its public housing unit (de Sousa Briggs, Comey, & Weismann, 2010).

At baseline, the MTO participants were severely economically disadvantaged; the majority were unemployed, most did not have high school diplomas, and a high percentage received Aid to Families with Dependent Children and food stamps (Orr et al., 2003; Sanbonmatsu et al., 2011). Additionally, many had serious health problems (Orr et al., 2003). The typical family was living well below the poverty line, with an average household income of \$12,827 (2009 dollars; Sanbonmatsu et al., 2011). MTO participants who were employed were predominantly in low-wage jobs in the health care, retail, and social service sectors (de Sousa Briggs, Popkin, & Goering, 2010). Despite these disadvantages at the outset of the program, policymakers were hopeful that the MTO intervention would lead to positive employment and economic outcomes (Sanbonmatsu et al., 2011).

MTO was predicated on the notion that a combination of access to jobs and areas of job growth, safer neighborhoods, more and better educational opportunities, and new job networks and different social norms would combine to improve participants' employment

outcomes (Orr et al., 2003; Sanbonmatsu et al., 2011). Though researchers lack data to directly test the role of social networks on participants' employment outcomes, data are available showing the extent to which MTO households resided in neighborhoods with low-skill job-holders or unemployed persons. In this article, we focus on access to jobs and areas of job growth. This focus is rooted in Ellen and Turner's (1997, p. 842) observation that "the most straightforward impact of neighborhood is physical proximity and accessibility to economic opportunities, particularly jobs."

At the interim evaluation, there were few or no economic benefits for MTO participants (Kling, Liebman, Katz, & Sanbonmatsu, 2004; Orr et al., 2003). These disappointing results were reinforced at the final evaluation. Sanbonmatsu et al. (2011) found that employment outcomes or the types of jobs held by participants were not significantly different between groups. Possible explanations include the timing of MTO with major welfare reform and national economic growth—each of which may have contributed to higher work rates for the control group as well as the experimental group—the lack of transportation access for participating families, and potential disruptions in existing social and job networks from moving (Kling et al., 2007; Leventhal & Brooks-Gunn, 2003). Additional explanations include most participants' lack of education and limited previous work experience, physical and mental health challenges, and concentration in the retail and health care sectors, which often rely on word-of-mouth and weak social ties to learn about job opportunities (Turney et al., 2006). Importantly, however, recent research by Chetty et al. (2015) finds that children who participated in MTO earlier in life (and spent more time in higher opportunity neighborhoods) have had higher earnings in adulthood and were much more likely to attend and graduate from college.

Spatial mismatch hypothesis

The spatial mismatch hypothesis, which contends that low-skilled, low-income, and minority households are clustered in central city neighborhoods with low job prospects (Kain, 1968), is another potential explanation for the lack of effects. Spatial mismatch partially explains high rates of unemployment among African American households as a result of housing market discrimination, which has separated racially segregated central city neighborhoods from suburban areas of employment growth (Ihlanfeldt & Sjoquist, 1998; Kain, 1968, 1992). Elaborating on spatial mismatch is the concept of modal mismatch, which refers to the idea that beyond distance to employment, one must also consider an individual's ability to access that job; for example, jobs farther away may be readily accessible by auto but not by public transit (Blumenberg & Pierce, 2014; Grengs, 2010).

Although theoretically the MTO experimental group would have been expected to move to areas closer to more job opportunities, Kling et al. (2004, p. 15) found evidence that "the neighborhoods the experimental group moved into may have been experiencing job loss instead of the job growth that we had hypothesized would occur." However, Kling et al. (2004) measured neighborhood employment access simply by dividing the number of employees working in the census tract by the number of residents, which ignores the fact that very few people live and work in the same census tract. Such a measure essentially captures whether a household lives in a mixed-use (residential and commercial)

neighborhood. Three years later, Kling et al. (2007) updated this analysis and found no differences between the three MTO groups in terms of job accessibility but again used limited measures—aggregate employment growth at the ZIP code level. Though ZIP codes are larger, meaning that more individuals live and work in the same ZIP code, this again primarily captures land use features of residential ZIP codes and says nothing about the neighboring ZIP codes where individuals are very likely to seek employment.

In another study, de Sousa Briggs, Popkin, and Goering (2010) calculated the number of entry-level jobs and growth in those jobs within 1, 5, 10, and 20 miles of MTO program participants. Though not perfect, this was an improvement over past research, because they explicitly account for distance. They reported findings for the Los Angeles and Chicago sites. In Chicago, they found no significant differences in job accessibility between the three MTO groups. In the Los Angeles area, the experimental group moved to areas that were less accessible to jobs and job growth within 5 and 10 miles (de Sousa Briggs, Popkin, & Goering, 2010). This suggested that moving to a “low-poverty census tract outside the inner city did not *necessarily* mean relocating to a job-rich zone, at least on average” (de Sousa Briggs, Popkin, & Goering, 2010, p. 207). One explanation from qualitative research is that MTO households were forced to manage trade-offs between a spatial match for employment, housing and child care (de Sousa Briggs, Popkin, & Goering, 2010).

As the short literature on employment proximity for MTO households illustrates, job proximity presents a thorny measurement issue in spatial mismatch research. Several authors have suggested that it is more important to study access to employment growth and competition for these jobs, rather than use measures of total jobs (Ihlanfeldt & Sjoquist, 1998; Raphael, 1998; Shen, 2001). In a recent paper, Lens (2014) used expanded employment accessibility measures to test associations between employment growth, competition, and the locations of different types of subsidized households. On one hand, he found that many subsidized households, and particularly public housing households, were located near major employment centers and job growth. On the other hand, he found that these same households were also in fierce competition from other low-skill workers for these job openings. In this article, we employ a variation on Shen’s (2001) and Lens’s (2014) employment accessibility methodology, described further below.

Blumenberg and Pierce (2014) analyzed associations between auto ownership and neighborhood transit access and an MTO participant’s likelihood of gaining or maintaining employment. The researchers used a multinomial logistic model to regress the change in employment status between the MTO baseline and interim evaluation on the change in auto ownership status or relocation to areas with improved public transit, controlling for individual sociodemographic and economic characteristics, neighborhood characteristics, and dummy variables for each of the metropolitan areas. Owning a car or procuring one was associated with gaining or keeping employment. Additionally, better access to transit was tied to keeping employment, though not significantly related to gaining employment.

Existing research on MTO has only made cursory attempts to examine the role of employment proximity in participant employment outcomes. Further, no existing study to our knowledge has used data from a randomized experiment to assess the validity of the

spatial mismatch hypothesis. This article uses MTO data to address two questions. First, did MTO participants reach areas proximate to more jobs (low-skilled or otherwise)? Second, in an experimental test of the spatial mismatch hypothesis, is proximity to employment associated with positive employment outcomes for MTO participants?

Data description, measuring job accessibility

In this section, we first describe the data sources used in this analysis, including MTO, the Census Transportation Planning Package (CTPP), the decennial census, and the census's longitudinal employer—household dynamics (LEHD) database. We then explain the methodology for calculating employment accessibility measures. Lastly, we define the regression models used to compare job accessibility between groups, and the relationships between job accessibility and economic outcomes.

We use MTO data from the study's baseline interviews—which occurred between 1994 and 1998—and the first follow-up interviews 4 to 7 years after random assignment. The full MTO program included a total of 4,608 participants in Baltimore, Boston, Chicago, Los Angeles, and New York City (Orr et al., 2003). The interim evaluation only included the 4,248 families that were randomly assigned by January 1, 1998, because the tenure of the 356 families assigned after that date was deemed too short to be included in the first follow-up study (Orr et al., 2003). Of the 4,248 families in the interim data, 636 families were from Baltimore, 959 from Boston, 894 from Chicago, 678 from Los Angeles, and 1,081 from New York City.

As mentioned previously, many participants in MTO had difficulty finding a suitable housing unit; only about 47% of the experimental group and 62% of the Section 8 group were able to lease-up (Orr et al., 2003). However, we report intent-to-treat estimates, where we compare the three groups as they were randomly assigned, consistent with other large-scale evaluations of MTO (Orr et al., 2003; Sanbonmatsu et al., 2011). The intent-to-treat results are less likely to suffer from selection bias, because participants in both groups were randomly assigned from the same overall pool. Unless otherwise noted, we use the same set of covariates used by Orr et al. (2003) to control for baseline characteristics of MTO participants. The covariates capture MTO participants' demographic characteristics, family size and structure, educational background, work experience, past and present income from public assistance, crime victimization and perceived safety, auto ownership, motivations for moving, and local social capital. Appendix Table A1 includes summary statistics of the full list of participant covariates.

Our methodology for estimating tract-level employment accessibility is derived from Lens (2014) and Shen (2001).¹ We lead with a static measure of jobs (not estimated openings)

¹Specifically, both authors subtract the number of jobs in time 1 from the number of jobs (j , in tract t) in time 2 to estimate the growth rate in time 2. Then, they assume a turnover rate of 3% and express total job openings (O_{jt}) as the sum of openings resulting from growth ($O_{jt}(G)$) and turnover ($O_{jt}(T)$):

$$(FN1) O_{jt} = O_{jt}(G) + O_{jt}(T)$$

using the U.S. Census Bureau's CTPP data. The CTPP was conducted as part of the decennial census until 2000, and we have one year of data (2000) overlapping the MTO interim period.² We then estimate job openings using 2002 and 2004 data from the U.S. Census Bureau's LEHD files as a robustness check.³ Our results using the LEHD measures are shown in this article's Appendix. We use the 2000 CTPP data because the LEHD files are missing reliable jobs measures for two of the MTO sites: Baltimore and Boston. Therefore, the primary set of results utilizes the full five-city set of MTO sites and 2000 CTPP data.

Next, we create a distance-decay model to measure the number of jobs proximate to each residential tract location. This model discounts jobs farther away—up to 50 miles—based on the Euclidean distance between tract centroids. We express this model as

$$A_{it} = \sum_{j=1}^N O_{jt} \exp(\gamma d_{ij}). \quad (1)$$

Here, A_{it} gives us the distance-weighted jobs for each census tract, d_{ij} is the distance between the centroid of that tract and every tract within 50 miles, O_{jt} is the number of jobs in every one of those tracts, and γ is a distance decay parameter calculated for a similar population by Parks (2004).⁴

Models of job accessibility and economic outcomes

Using these measures, we begin by regressing job accessibility measures on dummy variables for experimental and Section 8 group assignment and the set of baseline covariates. The model is as follows:

$$A_{it} = \alpha + \beta_1 \text{Experimental}_{it} + \beta_2 \text{Sec8}_{it} + \beta_3 X'_{it} + e_{it}, \quad (2)$$

²We assign CTPP attributes from the year 2000 to each MTO participant's tract at baseline (between 1994 and 1998) and at the interim evaluation (2002). We note that if a family remained in the same tract, they did not experience a change in job accessibility based on our methodology, because the jobs measures do not change.

³The advantage of the LEHD data is that they are available annually, which allows us to more strictly adhere to the methodology of Lens (2014) and Shen (2001) and estimate job openings rather than jobs. However, the LEHD data are not available for Massachusetts and the District of Columbia. Consequently, we cannot use these data to analyze MTO participants at the Boston or Baltimore sites. Our results using the LEHD measures for the other three MTO sites (Chicago, Los Angeles, and New York) are shown in this article's Appendix and are very consistent across the two sources of data.

⁴Parks (2004) empirically estimated this parameter using household-level data on employment and residential locations for low-skilled females—the precise population that MTO participants were drawn from—and arrived at an estimate of -0.058 . Her research was conducted in Los Angeles, which is one of the MTO sites, and though the urban form of the other five sites differs from that of Los Angeles, population densities are similar across the five cities. Using her distance decay parameter, we weigh jobs at k distance from tract i by: 0 minutes = 1, 5 minutes = 0.75, 10 minutes = 0.56, and 20 minutes = 0.31. Using national surveys, we estimate that the distance-to-time ratio for commuting is approximately 3 to 1. That is, roughly the same proportion of people work 15 minutes away as those who work 5 miles away, 30 minutes corresponds to 10 miles, etc. Thus, we arrived at a decay parameter of $-0.058 * 3 = -0.174$, where 0 miles = 1, 3 miles = 0.59, 5 miles = 0.42, 15 miles = 0.07, 30 miles = 0.005, and 50 miles = 0.0002. Only jobs within 50 miles are included. In order to test the sensitivity of our models to the distance-decay assumptions, we created two new sets of employment accessibility measures with steeper distance-decay gradients. We ran each regression model with the new employment measures and we found that all results were robust to the different distance-decay assumptions.

where the employment accessibility of participant i in tract t is regressed on that participant's MTO group (Experimental $_{it}$ and Sec8 $_{it}$) and the baseline covariates defined in Note 1.

Our second set of models tests whether differences in accessibility to jobs are associated with MTO participants' employment outcomes, as measured by employment status and annual earnings. Ordinary least square (OLS) models do not address the fact that for MTO participants moving to a neighborhood with better employment accessibility is endogenous to employment status and earnings. Although MTO is a randomized experiment and the likelihood of a move was much greater for the experimental and Section 8 groups, the randomization process did not determine participants' precise subsequent residential locations. Individuals who locate in more job-accessible neighborhoods may have been more motivated, had larger social networks, had better knowledge of the metropolitan area, or were more persistent, and these are all characteristics that can help in finding work. We take a two-stage approach using experimental or Section 8 assignment as instrumental variables for employment accessibility (Equations 3 and 4). We can thus better account for differences between those who live in job-rich and job-poor neighborhoods. Given that the experimental and Section 8 groups moved away from jobs, we are testing whether *reduced* job accessibility may have contributed to the lack of statistically significant differences in employment outcomes between the treatment and control groups.

In the first stage of each model, we regress employment accessibility on MTO group assignment and the baseline covariates to obtain a predicted value of employment accessibility for each participant, identical to Equation 2. In the second stage of the models, we regress predicted employment accessibility on earnings (or employment status).⁵ For the employment status outcome, we follow Katz, Kling, and Liebman (2000) and specify linear probability models (LPMs) with robust standard errors.⁶ As shown in Equation 3, in the second stage we regress the binary employment status variable on predicted employment accessibility (\hat{A}_{it}) and baseline covariates:

$$Empstatus_{it} = \alpha + \beta_1 \hat{A}_{it} + \beta_2 X'_{it} + e_{irt}. \quad (3)$$

For the earnings outcomes, in the second stage we regress earnings in 2001 on predicted employment accessibility and baseline covariates (Equation 4). Following standard economic analysis, we transform earnings using the natural logarithm to account for the nonlinear nature of the earnings data (in this case, 1,372 participants had zero earnings):⁷

$$\ln(\text{Earnings})_{it} = \alpha + \beta_1 \hat{A}_{it} + \beta_2 X'_{it} + e_{irt}. \quad (4)$$

⁵We use several tests to check the validity of our instrumental variables. The F-statistic in the first stage is well above 10, the typical threshold below which instruments are considered weak, for all of the results shown in Tables 3 to 6 and Tables A5 to A8 (Staiger & Stock, 1994). Additionally, we conduct the Sargan's test of overidentifying restrictions. In our results, the p -value of every Sargan's test is well above .05, suggesting the likely validity of our instruments.

⁶Katz et al. (2000) found that their results were not sensitive to whether they used LPM or probit estimates.

⁷Given that $\ln(0)$ is undefined, we calculated $\ln(\text{earnings} + 1)$.

Effects of MTO participation on job accessibility

Our first research question is whether experimental or Section 8 groups live in areas with greater or lesser job access than the control group. We use descriptive statistics to assess participants' changes in employment accessibility between the baseline and the interim evaluation. We report these statistics for the control, Section 8, and experimental groups in Table 1. We observe here that all groups declined in job accessibility, regardless of the type of job (all jobs or low-skilled jobs⁸). However, the decline in access to jobs was about twice as high for the experimental group than the control group, and the Section 8 group job accessibility declines were about 1.5 times higher than that for the control group. Although all three groups moved away from jobs, the experimental and Section 8 groups did this at much greater rates.

Table 2 provides comparisons between the three groups in a regression framework, where we can control for baseline characteristics.⁹ We see that controlling for these characteristics, the experimental group was located in areas proximate to nearly 70,000 fewer total jobs and over 16,000 fewer low-skilled jobs than the control group. To put these numbers in perspective, this accounts for about 10% of the job accessibility measures of the average census tract in the study sample. For the Section 8 group, those numbers are essentially halved, meaning that though they located nearer fewer jobs than the control group, those differences were about half as large than those between the experimental and control group.

Even controlling for extensive baseline characteristics, the differences between the two treatment groups and the control group are statistically significant (at the 1% level for the experimental group and the 5% level for the Section 8 group). For the sake of brevity, we do not include the full set of covariates in the table, but we note here that several variables were significantly associated with participants' employment accessibility. The participant's age, whether the participant has never been married, whether the participant heads a large household, and Hispanic participant variables were positively associated with better employment accessibility. In addition, several other variables were negatively associated with employment accessibility, including a variable indicating whether a participant was confident about finding an apartment elsewhere, whether a participant was enrolled in school, whether the participant had no family in the neighborhood, and whether the participant had been robbed, assaulted, or threatened with a weapon within the 6 months prior to the survey. Notably, the MTO group differences in job accessibility at the interim survey are consistent for all five cities in the study.

We also measure access to employment *growth* as a robustness check, utilizing LEHD data for Chicago, Los Angeles, and New York City. The results using the job growth measures in Table A2 are similar to the results using the 2000 job measures (although on a different scale because job growth numbers are much smaller than total jobs numbers). Given the findings from Lens (2014), we are not surprised by the differences between the three MTO groups

⁸The low-skilled job category includes the following North American Industry Classification System sectors: Agriculture, Construction, Manufacturing, Retail, Administrative and Support and Waste Management, Accommodation and Food Services, and Other Services.

⁹Table A2 shows the results of a similar model using the 2002–2004 job growth figures.

that we observed here. Lens (2014) found that public housing is located closer to job growth in the urban core than housing voucher households. Thus, given the value of a subsidy (for both the Section 8 and experimental groups) and an incentive to move to low-poverty areas (for the experimental group), we would expect the Section 8 and experimental groups to be more likely to locate away from the urban core, in areas less accessible to jobs. Our findings corroborate this and conclude that the MTO program actually reduced participant employment accessibility. The greater intensity of differences between the experimental and control groups (compared to the differences between the Section 8 and control groups) can be explained by the fact that this group could only use their vouchers in low-poverty neighborhoods—those with 10% poverty rates or less. In many cases, this necessitated movement to the suburbs, where concentrations of jobs are often unevenly distributed across large geographic spaces.

Effects of job accessibility on earnings and employment status

In Tables 3 and 4 we present the results of two-stage models that address the probable endogeneity of employment accessibility and employment status and earnings.¹⁰ We first specify a 2SLS model to analyze the relationship between employment accessibility and earnings, controlling for baseline covariates, and instrumenting in the first stage with the treatment group assignment variables. Table 3 presents these results, where we display only the key independent variable coefficients although we control for baseline characteristics. To account for the huge numerical differences between the number of jobs (where the mean value is 670,753 total jobs) and the natural log of earnings (average value 5.5 or \$252), we divide the jobs numbers by 1,000. What we see is that access to total jobs has no statistically significant relationship to earnings at the follow-up interim evaluation. The same is the case for access to low-skilled jobs. In both cases, the coefficients are negative; however, the standard errors are virtually the same size as the coefficients, meaning that we can be confident that there is no significant relationship between employment accessibility and earnings.

In Table 4, we specify two-stage linear probability models to see whether employment accessibility is related to employment status, using the same controls and instruments as above. Consistent with our results on earnings, we find no significant relationship. In Tables A5 and A6, we use estimates of job openings derived from 2002 and 2004 job numbers in Chicago, Los Angeles, and New York City (not available in Boston and Baltimore), and we again find no effect on earnings (Table A5) or employment status (Table A6).

Across our models, several participant characteristics are consistently associated with both positive earnings and employment status outcomes, including working at baseline, educational attainment of a GED or high school diploma, and enrollment in school. A variable representing whether the participant would likely tell a neighbor if they saw the neighbor's child getting into trouble is positively associated with earnings but not associated with employment status. Other characteristics are consistently associated with both negative earnings and employment status outcomes, including age, having a disability at baseline,

¹⁰We show the results using ordinary least squares models in Tables A3 and A4.

having lived in the neighborhood for five or more years, and participants self-reporting that they were very dissatisfied with the neighborhood. Having an automobile at baseline is negatively associated with being employed at the interim and not associated with earnings.

In Tables 5 and 6, we break out the results by city and observe that in none of the cities do we observe a statistically significant relationship between either employment accessibility and earnings or employment accessibility and employment status. In both tables, the coefficients are positive in Boston and Chicago and negative in Los Angeles and New York (in Baltimore, the coefficients are positive for earnings and negative for employment status) but, again, these coefficients are statistically equal to zero. These findings hold in Tables A7 and A8, when we utilize the job openings measures.

Discussion

Our analyses tell us two clear things. First, MTO participation was associated with movement away from jobs. This is not surprising, given prior research showing that public housing is mainly located in urban locations near major job centers, albeit clustered among other low-skilled workers who likely serve as competition for those jobs. If participants used these vouchers to move to suburban areas that were more job-poor than most, then this would further explain these findings.

Second, our results show that the movement away from jobs may not have mattered much for employment and earnings. Our regression results suggest that moving to areas with better or worse access to jobs leads to no significant differences in earnings or employment status. Human capital characteristics—rooted in a person's long-term education, experience, and health—make a larger difference, according to our models. The biggest positive predictors of a participant's interim employment status were having been employed at the baseline, being enrolled in school, and having a GED or high school diploma. Conversely, having a disability at baseline was most strongly negatively associated with the likelihood of employment at the interim evaluation. This illustrates larger, more fundamental barriers to employment greater than spatial proximity to jobs.

There are other possible explanations for our null findings. First, job accessibility may not be a very good predictor of employment outcomes because some employed people choose to live away from jobs, and this biases estimates toward zero (Ihlanfeldt & Sjoquist, 1998). Second, there may be conflicting forces that also attenuated the effects of job accessibility. Other benefits of moving to low-poverty neighborhoods—increased safety, better schools, access to better connected social networks, and the improved mental health effects that were found in other MTO analyses—may have drowned out the negative effects of moving away from jobs. On the other hand, existing social networks may have been disrupted the most for the people who made moves of greater distance. In these ways, we can think of residential mobility as involving a series of tradeoffs that make it difficult to connect a particular neighborhood attribute to economic mobility.

Accordingly, our research suggests that if the goal is to improve economic outcomes, the most important strategies probably have little to do with residential location: help people

consume education and training, build job networks, obtain occupational therapy to overcome a disability, and, perhaps, as Blumenberg and Pierce (2014) and others have suggested, help people gain access to cars. Although we are skeptical that spatial mismatch is the determining factor in joblessness for MTO participants, the results from this article suggest that we should be cautious about using mobility to address such joblessness. We find that mobility programs that incentivize moves to the suburbs are likely to decrease spatial access to employment. Though this may not be determinant for employment and earnings outcomes, it makes little sense to actively promote moves away from centers of employment for these populations.

These results do not settle questions about whether housing mobility programs are the best way to improve the fates of low-income households and neighborhoods. MTO participants' movement away from jobs was an unintended consequence of this housing mobility experiment, but our analyses find that these moves did not reduce employment outcomes. Given the important positive outcomes for MTO and other housing voucher participants across several (though not nearly all) domains, including accessing safer neighborhoods and higher performing schools (Horn, Ellen, & Schwartz, 2014; Lens, Ellen, & O'Regan, 2011; Rubinowitz & Rosenbaum, 2002), improved adult health, and increased future college attendance and earnings for child participants (Chetty et al., 2015; Sanbonmatsu et al., 2011), we do not advocate for throwing out housing mobility efforts wholesale, as others have done (Imbroscio, 2012). If housing mobility programs can be designed to limit moves away from jobs while improving neighborhood consumption across other domains that appear to have a greater effect on outcomes, then this would be the ideal. We stress that some destination neighborhoods are better than others when considering several dimensions of neighborhood opportunity. This is particularly true given that housing mobility programs often encourage moves to the suburbs, suburban poverty is increasing (Garr & Kneebone, 2010; Howell & Timberlake, 2014), and low-income suburbs are often those with poor job accessibility (Raphael & Stoll, 2010). Thus, we need to avoid encouraging moves to high-poverty suburbs with poor access to jobs, which often contain the only suburban properties that housing voucher households can afford.

Acknowledgments

We gratefully acknowledge the comments of three anonymous reviewers. We also thank attendees of the 2014 Association of Collegiate Schools of Planning conference for their feedback. Lastly, we thank UCLA colleagues Evy Blumenberg and Greg Pierce for several insightful conversations about this research. All errors are our own.

Funding

We thank the UCLA Academic Senate Council on Research for generous funding.

Appendix

Table A1

Descriptive statistics of all variables.

| Variables | Obs | Mean | SD | Min | Max |
|------------------|-------|----------|-----------|-----|-----------|
| Earnings in 2001 | 3,496 | 8,417.72 | 10,442.96 | 0 | 78,040.00 |

| Variables | Obs | Mean | SD | Min | Max |
|---|-------|--------|--------|------|----------|
| Earnings in 2001 (ln) | 3,496 | 5.53 | 4.54 | 0.00 | 11.26 |
| Employment status in 2001 | 3,700 | 0.51 | 0.50 | 0 | 1 |
| Total jobs in 1,000s (2000) | 3,816 | 670.75 | 459.77 | 0.16 | 2,395.98 |
| Low-skill jobs in 1,000s (2000) | 3,816 | 182.96 | 115.77 | 0.05 | 584.61 |
| Total job growth in 1,000s (2002–2004) | 2,808 | 9.79 | 6.54 | 0.15 | 31.24 |
| Low-skill job growth in 1,000s (2002–2004) | 2,808 | 3.62 | 2.08 | 0.09 | 10.08 |
| Current age of sample adult | 4,431 | 39.30 | 9.40 | 22 | 94 |
| Hispanic | 4,402 | 0.29 | 0.45 | 0 | 1 |
| Black | 4,402 | 0.64 | 0.48 | 0 | 1 |
| Other race | 4,402 | 0.04 | 0.20 | 0 | 1 |
| Sample adult is male | 4,418 | 0.02 | 0.13 | 0 | 1 |
| Sample adult had a GED | 4,431 | 0.18 | 0.38 | 0 | 1 |
| Sample adult was a high school graduate | 4,431 | 0.40 | 0.49 | 0 | 1 |
| Missing variable for sample adult was a high school graduate | 4,431 | 0.06 | 0.24 | 0 | 1 |
| Sample adult was enrolled in school | 4,431 | 0.16 | 0.36 | 0 | 1 |
| At baseline, sample adult had never been married | 4,431 | 0.59 | 0.49 | 0 | 1 |
| Sample adult was under age 18 at birth of first child | 4,431 | 0.25 | 0.42 | 0 | 1 |
| Sample adult was working | 4,431 | 0.25 | 0.42 | 0 | 1 |
| Any household member was disabled | 4,431 | 0.15 | 0.36 | 0 | 1 |
| Core household did not contain any teen children (ages 13 to 17) at baseline | 4,431 | 0.59 | 0.49 | 0 | 1 |
| Baseline respondent was receiving Aid to Families with Dependent Children/Temporary Assistance for Needy Families | 4,431 | 0.76 | 0.43 | 0 | 1 |
| Baseline respondent had a car that runs | 4,431 | 0.20 | 0.40 | 0 | 1 |
| Core household size is two or fewer | 4,431 | 0.21 | 0.40 | 0 | 1 |
| Core household size equals three | 4,431 | 0.34 | 0.47 | 0 | 1 |
| Core household size equals four | 4,431 | 0.22 | 0.41 | 0 | 1 |
| Any householder had been robbed, assaulted, or threatened with a weapon with the 6 months prior to the survey | 4,431 | 0.40 | 0.49 | 0 | 1 |
| Baseline respondent had lived in neighborhood for five or more years | 4,431 | 0.63 | 0.48 | 0 | 1 |
| Baseline respondent stopped to chat with a neighbor in the street or hallway at least once a week | 4,431 | 0.50 | 0.50 | 0 | 1 |
| Baseline respondent was very dissatisfied with neighborhood | 4,431 | 0.49 | 0.50 | 0 | 1 |
| Baseline respondent was very likely to tell neighbor if saw neighbor's child getting into trouble | 4,431 | 0.53 | 0.50 | 0 | 1 |
| Baseline respondent had no family in neighborhood | 4,431 | 0.66 | 0.47 | 0 | 1 |
| Baseline respondent reported not having any friends in the neighborhood | 4,431 | 0.43 | 0.49 | 0 | 1 |
| Baseline respondent considered streets near home very unsafe at night | 4,431 | 0.52 | 0.50 | 0 | 1 |
| Baseline respondent reported being very sure he or she would find an apartment in a different area of the city | 4,431 | 0.44 | 0.50 | 0 | 1 |
| Adult respondent had moved more than three times in 5 years prior to baseline | 4,431 | 0.09 | 0.28 | 0 | 1 |
| Baseline respondent's primary or secondary reason for wanting to move was to get away from gangs or drugs | 4,431 | 0.78 | 0.41 | 0 | 1 |

| Variables | Obs | Mean | SD | Min | Max |
|--|-------|----------|--------|------|----------|
| Baseline respondent's primary or secondary reason for moving was to have access to better schools for children | 4,431 | 0.46 | 0.49 | 0 | 1 |
| At baseline, respondent had already previously applied for a Section 8 voucher or certificate | 4,431 | 0.44 | 0.49 | 0 | 1 |
| Baltimore site | 4,431 | 0.14 | 0.35 | 0 | 1 |
| Boston site | 4,431 | 0.26 | 0.44 | 0 | 1 |
| Chicago site | 4,431 | 0.20 | 0.40 | 0 | 1 |
| Los Angeles site | 4,431 | 0.15 | 0.36 | 0 | 1 |
| Total jobs in 1,000s (2000—Baltimore) | 557 | 313.98 | 63.97 | 8.48 | 746.65 |
| Low-skill jobs in 1,000s (2000—Baltimore) | 557 | 85.07 | 16.27 | 3.91 | 252.78 |
| Total jobs in 1,000s (2000—Boston) | 1,023 | 516.58 | 150.19 | 4.03 | 1,459.18 |
| Low-skill jobs in 1,000s (2000—Boston) | 1,023 | 121.27 | 29.56 | 1.44 | 371.76 |
| Total jobs in 1,000s (2000—Chicago) | 815 | 468.24 | 166.35 | 0.16 | 1,545.78 |
| Low-skill jobs in 1,000s (2000—Chicago) | 815 | 137.37 | 41.77 | 0.05 | 392.41 |
| Total job growth in 1,000s (2002–2004—Chicago) | 810 | 6.33 | 2.13 | 0.46 | 19.89 |
| Low-skill job growth in 1,000s (2002–2004—Chicago) | 810 | 2.46 | 0.73 | 0.20 | 6.62 |
| Total jobs in 1,000s (2000—Los Angeles) | 597 | 582.53 | 199.60 | 0.46 | 874.78 |
| Low-skill jobs in 1,000s (2000—Los Angeles) | 597 | 214.96 | 73.03 | 0.25 | 298.42 |
| Total job growth in 1,000s (2002–2004—Los Angeles) | 593 | 8.81 | 2.97 | 0.15 | 13.67 |
| Low-skill job growth in 1,000s (2002–2004—Los Angeles) | 593 | 3.90 | 1.20 | 0.09 | 5.34 |
| Total jobs in 1,000s (2000—New York City) | 824 | 1,367.55 | 494.81 | 5.52 | 2,395.98 |
| Low-skill jobs in 1,000s (2000—New York City) | 824 | 347.63 | 118.78 | 1.98 | 584.61 |
| Total job growth in 1,000s (2002–2004—New York City) | 817 | 17.74 | 6.27 | 0.23 | 31.24 |
| Low-skill job growth in 1,000s (2002–2004—New York City) | 817 | 5.91 | 1.99 | 0.10 | 10.08 |

Table A2

Intent-to-treat estimates of employment accessibility (2002–2004 measure) OLS results.

| | (1) | (2) |
|----------------|----------------------|-------------------------------|
| Variables | Job growth (1,000s) | Low-skill job growth (1,000s) |
| Experimental | -1.035*** (0.220) | -0.336*** (0.0728) |
| Section 8 | -0.531** (0.239) | -0.172** (0.0789) |
| Observations | 2,679 | 2,679 |
| R ² | 0.644 | 0.613 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

Table A3

Employment accessibility (2000 measure, CTPP files) and earnings OLS results.

| Variables | (1) | (2) |
|-------------------------|-------------------------|-----------------------|
| | Earnings (ln) | Earnings (ln) |
| Total jobs (1,000s) | -0.000392 (0.000313) | |
| Low-skill jobs (1,000s) | | -0.00158 (0.00123) |
| Observations | 3,387 | 3,387 |
| R^2 | 0.223 | 0.223 |

Note. Robust standard errors in parentheses.

Table A4

Employment accessibility (2000 measure, CTPP files) and employment status using a linear probability model.

| Variables | (1) | (2) |
|-------------------------|-------------------------|-------------------------|
| | Employment status | Employment status |
| Total jobs (1,000s) | -1.99e-05 (3.39e-05) | |
| Low-skill jobs (1,000s) | | -7.64e-05 (0.000133) |
| Observations | 3,586 | 3,586 |
| R^2 | 0.175 | 0.175 |

Note. Robust standard errors in parentheses.

Table A5

Employment accessibility (2002–2004 measure) and earnings 2SLS results.

| Variables | (1) | (2) |
|-------------------------------|--------------------|-------------------|
| | Earnings (ln) | Earnings (ln) |
| Total job growth (1,000s) | -0.0557 (0.199) | |
| Low-skill job growth (1,000s) | | -0.173 (0.617) |
| Observations | 2,499 | 2,499 |
| R^2 | 0.167 | 0.167 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Table A6

Employment accessibility (2002–2004 measure) and employment status using a two-stage linear probability model.

| | (1) | (2) |
|-------------------------------|---------------------|---------------------|
| Variables | Employment status | Employment status |
| Total job growth (1,000s) | -0.0187 (0.0223) | |
| Low-skill job growth (1,000s) | | -0.0579 (0.0691) |
| Observations | 2,655 | 2,655 |
| R^2 | 0.127 | 0.126 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Table A7

Employment accessibility (2002–2004 measure) and earnings, by MTO site, 2SLS results.

| | (1) | (2) | (3) |
|---------------------------|-----------------------|---------------------------|-----------------------------|
| Variables | Chicago—Earnings (ln) | Los Angeles—Earnings (ln) | New York City—Earnings (ln) |
| Total job growth (1,000s) | 0.281 (1.755) | -0.238 (0.492) | -0.0690 (0.158) |
| Observations | 725 | 544 | 710 |
| R^2 | 0.185 | 0.156 | 0.198 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Table A8

Employment accessibility (2002–2004 measure) and employment status, by MTO site, using a two-stage linear probability model.

| | (1) | (2) | (3) |
|---------------------------|---------------------------|-------------------------------|---------------------------------|
| Variables | Chicago—Employment status | Los Angeles—Employment status | New York City—Employment status |
| Total job growth (1,000s) | 0.0624 (0.189) | -0.00446 (0.0520) | -0.0272 (0.0176) |
| Observations | 774 | 568 | 751 |
| R^2 | 0.079 | 0.171 | 0.094 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Biographies

Michael C. Lens is an assistant professor of urban planning in the University of California, Los Angeles (UCLA) Luskin School of Public Affairs. He conducts research on low-income housing subsidies, neighborhood effects, and segregation by income and race.

C. J. Gabbe is an assistant professor of environmental studies and sciences at Santa Clara University. His research focuses on the political economy of zoning and the effects of land use regulations on housing markets.

References

- Blumenberg E, Pierce G. A driving factor in mobility? Transportation's role in connecting subsidized housing and employment outcomes in the Moving to Opportunity (MTO) program. *Journal of the American Planning Association*. 2014; 80:52–66. DOI: 10.1080/01944363.2014.935267
- Chetty, R., Hendren, N., Katz, LF. The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. Cambridge, MA: National Bureau of Economic Research; 2015. Working Paper No. 21156 Retrieved from <http://www.nber.org/papers/w21156>
- de Sousa Briggs X, Comey J, Weismann G. Struggling to stay out of high-poverty neighborhoods: Housing choice and locations in moving to opportunity's first decade. *Housing Policy Debate*. 2010; 20:383–427. DOI: 10.1080/10511481003788745
- de Sousa Briggs, X., Popkin, SJ., Goering, J. *Moving to Opportunity: The story of an American experiment to fight ghetto poverty*. New York, NY: Oxford University Press; 2010.
- Ellen IG, Turner MA. Does neighborhood matter? Assessing recent evidence. *Housing Policy Debate*. 1997; 8:833–866. DOI: 10.1080/10511482.1997.9521280
- Garr, E., Kneebone, E. The suburbanization of poverty: Trends in metropolitan America, 2000 to 2008. 2010. Retrieved from <http://www.brookings.edu/research/papers/2010/01/20-poverty-kneebone>
- Goering, J. Expanding housing choice and integrating neighborhoods: The MTO experiment. In: de Sousa Briggs, X., editor. *The geography of opportunity: Race and housing choice in metropolitan America*. Washington, DC: Brookings Institution Press; 2005. p. 127-149. Retrieved from <http://books.google.com/books?hl=en&id=FWSmlW2kcdoC&oi=fnd&pg=PA127&dq=o%09Expanding+Housing+Choice+and+Integrating+Neighborhoods:+The+MTO+experiment+&ots=GasFvcpIDL&sig=BHcStc5WF1jQIBvnB-gQo8xJ-fU>
- Grengs J. Job accessibility and the modal mismatch in Detroit. *Journal of Transport Geography*. 2010; 18:42–54. DOI: 10.1016/j.jtrangeo.2009.01.012
- Horn KM, Ellen IG, Schwartz AE. Do housing choice voucher holders live near good schools? *Journal of Housing Economics*. 2014; 23:28–40. DOI: 10.1016/j.jhe.2013.11.005
- Howell AJ, Timberlake JM. Racial and ethnic trends in the suburbanization of poverty in U.S. metropolitan areas, 1980–2010. *Journal of Urban Affairs*. 2014; 36:79–98. DOI: 10.1111/juaf.12030
- Ihlanfeldt KR, Sjoquist DL. The spatial mismatch hypothesis: A review of recent studies and their implications for welfare reform. *Housing Policy Debate*. 1998; 9:849–892. DOI: 10.1080/10511482.1998.9521321
- Imbroscio D. Beyond mobility: The limits of liberal urban policy. *Journal of Urban Affairs*. 2012; 34:1–20. DOI: 10.1111/j.1467-9906.2011.00578.x
- Kain JF. Housing segregation, negro employment, and metropolitan decentralization. *The Quarterly Journal of Economics*. 1968:175–197.
- Kain JF. The spatial mismatch hypothesis: Three decades later. *Housing Policy Debate*. 1992; 3:371–460. DOI: 10.1080/10511482.1992.9521100
- Katz, LF., Kling, JR., Liebman, JB. *Moving to Opportunity in Boston: Early results of a randomized mobility experiment*. Cambridge, MA: National Bureau of Economic Research; 2000. Retrieved from <http://www.nber.org/papers/w7973>
- Keels M, Duncan GJ, Deluca S, Mendenhall R, Rosenbaum J. Fifteen years later: Can residential mobility programs provide a long-term escape from neighborhood segregation, crime, and poverty. *Demography*. 2005; 42:51–73. DOI: 10.1353/dem.2005.0005 [PubMed: 15782895]
- Kling JR, Liebman JB, Katz LF. Experimental analysis of neighborhood effects. *Econometrica*. 2007; 75:83–119. DOI: 10.1111/j.1468-0262.2007.00733.x

- Kling, JR., Liebman, JB., Katz, LF., Sanbonmatsu, L. Moving to Opportunity and tranquility: Neighborhood effects on adult economic self-sufficiency and health from a randomized housing voucher experiment. Princeton, NJ: Princeton University, Woodrow Wilson School of Public and International Affairs, Center for Health and Wellbeing; 2004. Working Paper No. 247 Retrieved from http://ideas.repec.org/p/pri/cheawb/kling_mto481.html
- Lens M. Employment accessibility among housing subsidy recipients. *Housing Policy Debate*. 2014; 24(4)doi: 10.1080/10511482.2014.905966
- Lens MC, Ellen IG, O'Regan K. Do vouchers help low-income households live in safer neighborhoods? Evidence on the Housing Choice Voucher program. *Cityscape*. 2011; 13:135–159.
- Leventhal T, Brooks-Gunn J. Moving to Opportunity: An experimental study of neighborhood effects on mental health. *American Journal of Public Health*. 2003; 93:1576–1582. DOI: 10.2105/AJPH.93.9.1576 [PubMed: 12948983]
- Orr, L., Feins, J., Jacob, R., Beecroft, E., Sanbonmatsu, L., Katz, LF., Kling, JR. Moving to Opportunity: Interim impacts evaluation. 2003. Retrieved from <http://scholar.harvard.edu/lkatz/publications/moving-opportunity-interim-impacts-evaluation>
- Parks V. Access to work: The effects of spatial and social accessibility on unemployment for native-born black and immigrant women in Los Angeles. *Economic Geography*. 2004; 80:141–172. DOI: 10.1111/j.1944-8287.2004.tb00305.x
- Popkin SJ, Rosenbaum JE, Meaden PM. Labor market experiences of low-income black women in middle-class suburbs: Evidence from a survey of Gautreaux program participants. *Journal of Policy Analysis and Management*. 1993; 12:556–573. DOI: 10.2307/3325306
- Raphael S. The spatial mismatch hypothesis and Black youth joblessness: Evidence from the San Francisco Bay Area. *Journal of Urban Economics*. 1998; 43:79–111. <http://doi.org/10.1006/juec.1997.2039>.
- Raphael, S., Stoll, M. Job sprawl and the suburbanization of poverty. 2010 Mar 30. Retrieved from <http://www.brookings.edu/research/reports/2010/03/30-job-sprawl-stoll-raphael>
- Rosenbaum JE. Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program. *Housing Policy Debate*. 1995; 6:231–269. DOI: 10.1080/10511482.1995.9521186
- Rubinowitz, LS., Rosenbaum, JE. Crossing the class and color lines: From public housing to White suburbia. 1st. Chicago, IL: University Of Chicago Press; 2002.
- Sanbonmatsu, L., Ludwig, J., Katz, L., Gennetian, L., Duncan, G., Kessler, R., Lindau, ST. Moving to Opportunity for Fair Housing Demonstration Program—Final impacts evaluation. Washington, DC: U.S. Department of Housing & Urban Development, Office of Policy Development and Research; 2011.
- Shen Q. A spatial analysis of job openings and access in a U.S. metropolitan area. *Journal of the American Planning Association*. 2001; 67:53–68.
- Staiger, DO., Stock, JH. Instrumental variables regression with weak instruments. Cambridge, MA: National Bureau of Economic Research Cambridge; 1994. Retrieved from <http://www.nber.org/papers/t0151>
- Turney K, Clampet-Lundquist S, Edin K, Kling JR, Duncan GJ, Ludwig J, Scholz JK. Neighborhood effects on barriers to employment: Results from a randomized housing mobility experiment in Baltimore [with comments]. *Brookings-Wharton Papers on Urban Affairs*. 2006:137–187.

Table 1

All employment measures at baseline and interim follow-up by group and percentage change in access to employment (2000 measure, CTPP files) and employment growth (2002–2004 measure).

| | Obs | Mean | SD | Min | Max |
|--|------------|-------------|-----------|------------|------------|
| Baseline—Access to all jobs in thousands (2000) | | | | | |
| Control group | 1,290 | 813.91 | 467.76 | 309.10 | 2,239.75 |
| Section 8 group | 1,199 | 883.80 | 501.77 | 267.89 | 2,201.57 |
| Experimental group | 1,715 | 821.99 | 476.64 | 306.88 | 2,201.57 |
| Interim follow-up—Access to all jobs in thousands (2000) | | | | | |
| Control group | 1,315 | 688.31 | 441.01 | 0.16 | 2,310.80 |
| Section 8 group | 1,025 | 712.48 | 487.59 | 4.03 | 2,261.55 |
| Experimental group | 1,476 | 626.13 | 452.72 | 0.46 | 2,395.98 |
| Baseline—Access to low-skill jobs in thousands (2000) | | | | | |
| Control group | 1,290 | 221.06 | 118.61 | 90.46 | 539.91 |
| Section 8 group | 1,199 | 234.54 | 125.41 | 80.28 | 536.08 |
| Experimental group | 1,715 | 220.37 | 119.97 | 83.78 | 536.08 |
| Interim follow-up—Access to low-skill jobs in thousands (2000) | | | | | |
| Control group | 1,315 | 185.90 | 113.21 | 0.05 | 564.06 |
| Section 8 group | 1,025 | 192.82 | 121.73 | 1.44 | 547.93 |
| Experimental group | 1,476 | 173.49 | 113.11 | 0.25 | 584.61 |
| Percentage change access to all jobs (2000) | | | | | |
| Control group | 1,114 | -0.09 | 0.21 | -0.99 | 1.04 |
| Section 8 group | 1,018 | -0.13 | 0.22 | -0.95 | 1.49 |
| Experimental group | 1,465 | -0.17 | 0.24 | -0.96 | 0.86 |
| Percentage change access to low-skill jobs (2000) | | | | | |
| Control group | 1,114 | -0.07 | 0.18 | -0.99 | 1.17 |
| Section 8 group | 1,018 | -0.11 | 0.20 | -0.95 | 1.79 |
| Experimental group | 1,465 | -0.14 | 0.22 | -0.95 | 0.75 |
| Percentage change access to all job growth (2002–2004) | | | | | |
| Control group | 831 | -0.08 | 0.20 | -0.97 | 0.80 |
| Section 8 group | 779 | -0.14 | 0.22 | -0.96 | 1.61 |

Author Manuscript

Author Manuscript

Author Manuscript

Author Manuscript

| | Obs | Mean | SD | Min | Max |
|--|------------|-------------|-----------|------------|------------|
| Experimental group | 1,148 | -0.17 | 0.24 | -0.97 | 0.89 |
| Percentage change access to low-skill job growth (2002-2004) | | | | | |
| Control group | 831 | -0.07 | 0.18 | -0.96 | 0.63 |
| Section 8 group | 779 | -0.12 | 0.20 | -0.95 | 1.35 |
| Experimental group | 1,148 | -0.14 | 0.22 | -0.96 | 0.82 |

Table 2

First-stage model: Intent-to-treat estimates of employment accessibility (2000 measure, CTPP files).

| Variables | (1) | (2) |
|--------------|-----------------------|-------------------------|
| | Jobs (1,000s) | Low-skill jobs (1,000s) |
| Experimental | -69.89 *** (13.55) | -16.55 *** (3.407) |
| Section 8 | -36.50 ** (14.89) | -8.332 ** (3.755) |
| Observations | 3,626 | 3,626 |
| R^2 | 0.659 | 0.657 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

Author Manuscript

Author Manuscript

Author Manuscript

Author Manuscript

Table 3

Employment accessibility (2000 measure, CTPP files) and earnings 2SLS results.

| Variables | (1) | (2) |
|-------------------------|-----------------------|---------------------|
| | Earnings (ln) | Earnings (ln) |
| Total jobs (1,000s) | -0.00258 (0.00251) | |
| Low-skill jobs (1,000s) | | -0.0106 (0.0106) |
| Observations | 3,387 | 3,387 |
| R^2 | 0.205 | 0.204 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Author Manuscript

Author Manuscript

Author Manuscript

Author Manuscript

Table 4

Employment accessibility (2000 measure, CTPP files) and employment status using a two-stage linear probability model.

| Variables | (1) | (2) |
|-------------------------|-------------------------|-----------------------|
| | Employment status | Employment status |
| Total jobs (1,000s) | -0.000350 (0.000284) | |
| Low-skill jobs (1,000s) | | -0.00145 (0.00121) |
| Observations | 3,586 | 3,586 |
| R^2 | 0.143 | 0.139 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Author Manuscript

Author Manuscript

Author Manuscript

Author Manuscript

Table 5

Employment accessibility (2000 measure, CTPP files) and earnings, by MTO site, 2SLS results.

| Variables | (1) | (2) | (3) | (4) | (5) |
|---------------------|-------------------------|----------------------|-----------------------|---------------------------|-----------------------------|
| | Baltimore—Earnings (ln) | Boston—Earnings (ln) | Chicago—Earnings (ln) | Los Angeles—Earnings (ln) | New York City—Earnings (ln) |
| Total jobs (1,000s) | 0.00487 (0.0145) | 0.00258 (0.00583) | 0.00693 (0.0264) | -0.00455 (0.00710) | -0.000633 (0.00214) |
| Observations | 495 | 898 | 730 | 548 | 716 |
| R ² | 0.271 | 0.442 | 0.133 | 0.152 | 0.200 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.

Table 6

Employment accessibility (2000 measure, CTPP files) and employment status, by MTO site, using a two-stage linear probability model.

| Variables | (1) | (2) | (3) | (4) | (5) |
|---------------------|-----------------------------|--------------------------|---------------------------|-------------------------------|---------------------------------|
| | Baltimore—Employment status | Boston—Employment status | Chicago—Employment status | Los Angeles—Employment status | New York City—Employment status |
| Total jobs (1,000s) | -0.00177 (0.00171) | 0.000336 (0.000681) | 0.000649 (0.00313) | -0.0000747 (0.000755) | -0.000357 (0.000240) |
| Observations | 536 | 942 | 779 | 572 | 757 |
| R ² | 0.193 | 0.300 | 0.097 | 0.174 | 0.094 |

Note. Models include MTO baseline covariates, consistent with Orr et al. (2003), listed in Note 1. Robust standard errors in parentheses.