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Employment Proximity and Outcomes for Moving to Opportunity Families

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 two treatment groups did not significantly differ from the control group. We use MTO data to examine if 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 three MTO groups. We then use two-stage least squares models to test relationships between employment accessibility and two key outcomes: employment status and earned income. We find that employment accessibility declined for all groups, and these declines were strongest for the two treatment groups. However, our results show essentially no effect of employment proximity on earnings or employment status for MTO participants.

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Introduction

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).

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 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 (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 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. While 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 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 policy makers 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 that 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 Moving to Opportunity (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 (CHA) and HUD. Gautreaux offered black families in CHA 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 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 to 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 (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 (AFDC) 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 $12,827 (2009$) (Sanbonmatsu et al., 2011). MTO participants who were employed were predominantly in low-wage jobs in the health care, retail, and social service sectors (Briggs, Popkin, et al., 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). While 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 paper, 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 Raj Chetty and colleagues (Chetty et al., 2015) finds that children that 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, farther jobs 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. 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. While zip codes are larger, meaning 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, Briggs et al. (2010) calculated the number of entry-level jobs and growth in those jobs within 1, 5, 10, and 20 miles of MTO program participants. While not perfect, this was an improvement over past research, as 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 (Briggs, Popkin, et al., 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" (Briggs, Popkin, et al., 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 (Briggs, Popkin, et al., 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 paper, 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 socio-demographic 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 paper 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, Census Transportation Planning Package (CTPP), decennial Census, and Longitudinal Employment-Household Dynamics (LEHD). 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 four to seven 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, since 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 percent of the experimental group and 62 percent of the Section 8 group were able to lease-up (Orr et al., 2003). However, we report intent-to-treat (ITT) 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 ITT 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 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) using U.S. Census Bureau's Transportation Planning Package (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 Longitudinal Employer Household Dynamics (LEHD) files as a robustness check.[3] Our results using the LEHD measures are shown in this paper'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:

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[1] Specifically, 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 percent and express total job openings (O_{jt}) as the sum of openings resulting from growth ($O_{jt}(G)$) and turnover ($O_{jt}(T)$):

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

[2] We 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.

[3] The 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 paper's appendix, and are very consistent across the two sources of data.

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

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:

$$(2)\ A_{it} = \alpha + \beta_1 Experimental_{it} + \beta_2 \mathrm{Sec8}_{it} + \beta_3 X'_{it} + e_{it}$$

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

Our second set of models test 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

[4] Parks (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 while 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 = .75, 10 minutes = .56, and 20 minutes = .31. Using national surveys, we estimate that the distance to time ratio for commuting to be approximately 3 to 1. That is, roughly the same proportion of people work 15 minutes away that 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 = .59, 5 miles = .42, 15 miles = .07, 30 miles = .005, and 50 miles = .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.

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, 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 that 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 et al. (2000) and specify linear probability models (LPM) 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.

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

[5] We 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 A-5 to A-8 (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 0.05, suggesting the likely validity of our instruments.

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

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]

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

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 jobs[8]). 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 experienced this phenomenon at much greater rates.

[Insert Table 1 about here.]

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

[7] Given ln(0) is undefined, we calculated ln(earnings + 1).

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

[9] Table A-2 shows the results of a similar model using the 2002-04 job growth figures.

16,000 fewer low-skilled jobs than the control group. To put these numbers in perspective, this accounts for about 10 percent 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 while 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 percent level for the experimental group and the 5 percent level for the Section 8 group). For 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, if participant has never been married, if the participant heads a large household, and Hispanic participant variables were positively associated with better employment accessibility. Meanwhile, several other variables were negatively associated with employment accessibility including a variable indicating if a participant was confident about finding an apartment elsewhere, if a participant was enrolled in school, if the participant had no family in the neighborhood, and if the participant had been robbed, assaulted, or threatened with a weapon within the six 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.

[Insert Table 2 about here.]

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 A-2 are similar to the results using the 2000 job measures (although on a different scale as 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 that we observed here. Lens 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 (as 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 percent 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 1000. What we see is that access to total jobs has no statistically significant relationship to earnings at

[10] We show the results using ordinary least squares models in tables A-3 and A-4.

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 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 if 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 A-5 and A-6, 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 A-5) or employment status (Table A-6).

[Insert Tables 3 and 4 about here.]

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, 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 A-7 and A-8, when we utilize the job openings measures.

[Insert Tables 5 and 6 about here.]

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 was 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 that 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 paper suggest 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. While 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) and improved adult health, and increased future college attendance and earnings for child participants (Chetty, Hendren, & Katz, 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 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.

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TABLES

Main Paper Tables

Appendix Tables

Table 1: All employment measures at baseline and interim follow-up by group, and percent change in access to employment (2000 measure, CTPP files) and employment growth (2002-2004 measure)

	Obs	Mean	Std. Dev.	Min	Max
Baseline - Access to all jobs in thousands (2000)					
Control group	1290	813.91	467.76	309.10	2239.75
Section 8 group	1199	883.80	501.77	267.89	2201.57
Experimental group	1715	821.99	476.64	306.88	2201.57
Interim follow-up - Access to all jobs in thousands (2000)					
Control group	1315	688.31	441.01	0.16	2310.80
Section 8 group	1025	712.48	487.59	4.03	2261.55
Experimental group	1476	626.13	452.72	0.46	2395.98
Baseline - Access to low skill jobs in thousands (2000)					
Control group	1290	221.06	118.61	90.46	539.91
Section 8 group	1199	234.54	125.41	80.28	536.08
Experimental group	1715	220.37	119.97	83.78	536.08
Interim follow-up - Access to low skill jobs in thousands (2000)					
Control group	1315	185.90	113.21	0.05	564.06
Section 8 group	1025	192.82	121.73	1.44	547.93
Experimental group	1476	173.49	113.11	0.25	584.61
Percent change access to all jobs (2000)					
Control group	1114	-0.09	0.21	-0.99	1.04
Section 8 group	1018	-0.13	0.22	-0.95	1.49
Experimental group	1465	-0.17	0.24	-0.96	0.86
Percent change access to low skill jobs (2000)					
Control group	1114	-0.07	0.18	-0.99	1.17
Section 8 group	1018	-0.11	0.20	-0.95	1.79
Experimental group	1465	-0.14	0.22	-0.95	0.75
Percent 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
Experimental group	1148	-0.17	0.24	-0.97	0.89
Percent 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	1148	-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) Jobs (1000s)	(2) Low skill jobs (1000s)
Experimental	-69.89***	-16.55***
	(13.55)	(3.407)
Section 8	-36.50**	-8.332**
	(14.89)	(3.755)
Observations	3,626	3,626
R-squared	0.659	0.657

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table 3: Employment accessibility (2000 measure, CTPP files) and earnings 2SLS results

VARIABLES	(1) Earnings (ln)	(2) Earnings (ln)
Total jobs (1000s)	-0.00258	
	(0.00251)	
Low skill jobs (1000s)		-0.0106
		(0.0106)
Observations	3,387	3,387
R-squared	0.205	0.204

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table 4: Employment accessibility (2000 measure, CTPP files) and employment status using a two-stage linear probability model

VARIABLES	(1) Employment status	(2) Employment status
Total jobs (1000s)	-0.000350	
	(0.000284)	
Low skill jobs (1000s)		-0.00145
		(0.00121)
Observations	3,586	3,586
R-squared	0.143	0.139

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table 5: Employment accessibility (2000 measure, CTPP files) and earnings, by MTO site, 2SLS results

VARIABLES	(1) Baltimore - Earnings (ln)	(2) Boston - Earnings (ln)	(3) Chicago - Earnings (ln)	(4) Los Angeles - Earnings (ln)	(5) New York City - Earnings (ln)
Total jobs (1000s)	0.00487	0.00258	0.00693	-0.00455	-0.000633
	(0.0145)	(0.00583)	(0.0264)	(0.00710)	(0.00214)
Observations	495	898	730	548	716
R-squared	0.271	0.442	0.133	0.152	0.200

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table 6: Employment accessibility (2000 measure, CTPP files) and employment status, by MTO site, using a two-stage linear probability model

VARIABLES	(1) Baltimore - Employment status	(2) Boston - Employment status	(3) Chicago - Employment status	(4) Los Angeles - Employment status	(5) New York City - Employment status
Total jobs (1000s)	-0.00177	0.000336	0.000649	-7.47e-05	-0.000357
	(0.00171)	(0.000681)	(0.00313)	(0.000755)	(0.000240)
Observations	536	942	779	572	757
R-squared	0.193	0.300	0.097	0.174	0.094

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Appendix Tables

Table A-1: Descriptive statistics of all variables

VARIABLES	Obs	Mean	Std. Dev.	Min	Max
Earnings in 2001	3496	8417.72	10442.96	0	78040.00
Earnings in 2001 (ln)	3496	5.53	4.54	0.00	11.26
Employment status in 2001	3700	0.51	0.50	0	1
Total jobs in 1000s (2000)	3816	670.75	459.77	0.16	2395.98
Low skill jobs in 1000s (2000)	3816	182.96	115.77	0.05	584.61
Total job growth in 1000s (2002-04)	2808	9.79	6.54	0.15	31.24
Low skill job growth in 1000s (2002-04)	2808	3.62	2.08	0.09	10.08
Current age of sample adult	4431	39.30	9.40	22	94
Hispanic	4402	0.29	0.45	0	1
Black	4402	0.64	0.48	0	1
Other race	4402	0.04	0.20	0	1
Sample adult is male	4418	0.02	0.13	0	1
Sample adult had a GED	4431	0.18	0.38	0	1
Sample adult was a high school graduate	4431	0.40	0.49	0	1
Missing variable for sample adult was a high school graduate	4431	0.06	0.24	0	1
Sample adult was enrolled in school	4431	0.16	0.36	0	1
At baseline, sample adult had never been married	4431	0.59	0.49	0	1
Sample adult was under age 18 at birth of first child	4431	0.25	0.42	0	1
Sample adult was working	4431	0.25	0.42	0	1
Any household member was disabled	4431	0.15	0.36	0	1
Core household did not contain any teen children (ages 13 to 17) at baseline	4431	0.59	0.49	0	1
Baseline Respondent was receiving AFDC/TANF	4431	0.76	0.43	0	1
Baseline Respondent had a car that runs	4431	0.20	0.40	0	1
Core household size is 2 or fewer	4431	0.21	0.40	0	1
Core household size equals 3	4431	0.34	0.47	0	1
Core household size equals 4	4431	0.22	0.41	0	1
Any householder had been robbed, assaulted, or threatened with a weapon with the six months prior to the survey	4431	0.40	0.49	0	1
Baseline Respondent had lived in neighborhood for 5 or more years	4431	0.63	0.48	0	1
Baseline Respondent stopped to chat with a neighbor in the street or hallway at least once a week	4431	0.50	0.50	0	1

Baseline Respondent was very dissatisfied with neighborhood	4431	0.49	0.50	0	1
Baseline Respondent was very likely to tell neighbor if saw neighbor's child getting into trouble	4431	0.53	0.50	0	1
Baseline Respondent had no family in neighborhood	4431	0.66	0.47	0	1
Baseline Respondent reporting not having any friends in the neighborhood	4431	0.43	0.49	0	1
Baseline Respondent considered streets near home very unsafe at night	4431	0.52	0.50	0	1
Baseline respondent reporting being very sure he/she would find an apartment in a different area of the city	4431	0.44	0.50	0	1
Adult respondent had moved more than 3 times in 5 years prior to baseline	4431	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	4431	0.78	0.41	0	1
Baseline respondent's primary or secondary reason for moving was to have access to better schools for children	4431	0.46	0.49	0	1
At baseline, respondent had already previously applied for a Section 8 voucher or certificate	4431	0.44	0.49	0	1
Baltimore site	4431	0.14	0.35	0	1
Boston site	4431	0.26	0.44	0	1
Chicago site	4431	0.20	0.40	0	1
Los Angeles site	4431	0.15	0.36	0	1
Total jobs in 1000s (2000 - Baltimore)	557	313.98	63.97	8.48	746.65
Low skill jobs in 1000s (2000 - Baltimore)	557	85.07	16.27	3.91	252.78
Total jobs in 1000s (2000 - Boston)	1023	516.58	150.19	4.03	1459.18
Low skill jobs in 1000s (2000 - Boston)	1023	121.27	29.56	1.44	371.76
Total jobs in 1000s (2000 - Chicago)	815	468.24	166.35	0.16	1545.78
Low skill jobs in 1000s (2000 - Chicago)	815	137.37	41.77	0.05	392.41
Total job growth in 1000s (2002-04 - Chicago)	810	6.33	2.13	0.46	19.89
Low skill job growth in 1000s (2002-04 - Chicago)	810	2.46	0.73	0.20	6.62
Total jobs in 1000s (2000 – L.A.)	597	582.53	199.60	0.46	874.78
Low skill jobs in 1000s (2000 – L.A.)	597	214.96	73.03	0.25	298.42
Total job growth in 1000s (2002-04 – L.A.)	593	8.81	2.97	0.15	13.67
Low skill job growth in 1000s (2002-04 – L.A.)	593	3.90	1.20	0.09	5.34
Total jobs in 1000s (2000 - NYC)	824	1367.55	494.81	5.52	2395.98
Low skill jobs in 1000s (2000 - NYC)	824	347.63	118.78	1.98	584.61
Total job growth in 1000s (2002-04 - NYC)	817	17.74	6.27	0.23	31.24
Low skill job growth in 1000s (2002-04 - NYC)	817	5.91	1.99	0.10	10.08

Table A-2: Intent-to-treat estimates of employment accessibility (2002-04 measure) OLS results

VARIABLES	(1) Job growth (1000s)	(2) Low skill job growth (1000s)
Experimental	-1.035***	-0.336***
	(0.220)	(0.0728)
Section 8	-0.531**	-0.172**
	(0.239)	(0.0789)
Observations	2,679	2,679
R-squared	0.644	0.613

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table A-3: Employment accessibility (2000 measure, CTPP files) and earnings OLS results

VARIABLES	(1) Earnings (ln)	(2) Earnings (ln)
Total jobs (1000s)	-0.000392	
	(0.000313)	
Low skill jobs (1000s)		-0.00158
		(0.00123)
Observations	3,387	3,387
R-squared	0.223	0.223

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A-4: Employment accessibility (2000 measure, CTPP files) and employment status using a linear probability model

VARIABLES	(1) Employment status	(2) Employment status
Total jobs (1000s)	-1.99e-05	
	(3.39e-05)	
Low skill jobs (1000s)		-7.64e-05
		(0.000133)
Observations	3,586	3,586
R-squared	0.175	0.175

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A-5: Employment accessibility (2002-04 measure) and earnings 2SLS results

VARIABLES	(1) Earnings (ln)	(2) Earnings (ln)
Total job growth (1000s)	-0.0557	
	(0.199)	
Low skill job growth (1000s)		-0.173
		(0.617)
Observations	2,499	2,499
R-squared	0.167	0.167

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table A-6: Employment accessibility (2002-04 measure) and employment status using a two-stage linear probability model

VARIABLES	(1) Employment status	(2) Employment status
Total job growth (1000s)	-0.0187	
	(0.0223)	
Low skill job growth (1000s)		-0.0579
		(0.0691)
Observations	2,655	2,655
R-squared	0.127	0.126

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table A-7: Employment accessibility (2002-04 measure) and earnings, by MTO site, 2SLS results

VARIABLES	(1) Chicago - Earnings (ln)	(2) Los Angeles - Earnings (ln)	(3) New York City - Earnings (ln)
Total job growth (1000s)	0.281	-0.238	-0.0690
	(1.755)	(0.492)	(0.158)
Observations	725	544	710
R-squared	0.185	0.156	0.198

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.

Table A-8: Employment accessibility (2002-04 measure) and employment status, by MTO site, using a two-stage linear probability model

VARIABLES	(1) Chicago - Employment status	(2) Los Angeles - Employment status	(3) New York City - Employment status
Total job growth (1000s)	0.0624	-0.00446	-0.0272
	(0.189)	(0.0520)	(0.0176)
Observations	774	568	751
R-squared	0.079	0.171	0.094

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: models include MTO baseline covariates, consistent with Orr et al. (2003), listed in footnote 1.