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Neighborhood Effects on Barriers to Employment: Results from a Randomized Housing Mobility Experiment in Baltimore

MOVING THE POOR OUT of inner-city neighborhoods of concentrated poverty (where jobs are scarce), and into low-poverty suburban neighborhoods (where jobs may be more plentiful) has been suggested by Wilson's (1987) theory of social isolation and Kain's (1968) theory of spatial mismatch to lead to greater

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employment and earnings. Between 1994 and 1997, the U.S. Department of Housing and Urban Development (HUD) launched the Moving to Opportunity for Fair Housing Demonstration Program (MTO) in an attempt to examine the effects of housing mobility on various factors including economic self-sufficiency. The MTO demonstration gave families living in distressed public housing in Baltimore, Boston, Chicago, Los Angeles, and New York the opportunity to relocate to private market housing in low-poverty suburban and city neighborhoods. MTO applicants were randomly assigned to one of three groups: an experimental group, with members receiving a voucher to be used in a census tract with a poverty rate of less than 10 percent; a Section 8 group,¹ with members receiving a voucher to move anywhere; or a control group. In 2002 all participating families, regardless of their MTO start date, were surveyed. Pooling data from all five cities, a recent study finds no significant effects on employment or earnings of adults in the experimental group, suggesting that receiving a voucher to move to a low-poverty neighborhood does not increase the economic self-sufficiency of poor families.²

In this paper we use data from an embedded in-depth qualitative study of MTO families in Baltimore to explore the social processes that might underlie these results. We present survey data from Baltimore that estimate the effect of the MTO vouchers on employment and earnings of adults, compared with the results from all five MTO cities. The difference in employment rates for the experimental and control groups is positive and of moderately large magnitude in Baltimore (larger than in the five cities combined), but statistically insignificant. The experimental group in Baltimore had lower average earnings than the control group. The lack of a large positive effect on employment and earnings is puzzling. In 2003 and 2004 we conducted in-depth interviews with a random sample drawn from all the Baltimore MTO families. Although the qualitative sample is relatively small, the in-depth nature of the data allows us to derive hypotheses that can be used to guide further qualitative work and the next round of survey work with the MTO population, scheduled for 2007.

We find that though experimentals and controls have similar rates of employment and earnings, both at the time of the survey (2002) and qualitative interview (2003–04), the nature of respondents' relationship to the labor force does differ by program group, at least in the qualitative sample. Additionally, we identify three barriers to employment that are common across program groups. Using these data, we generate hypotheses about why the MTO

1. Housing Choice Vouchers are commonly referred to as *Section 8* vouchers.

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

intervention may not have as strong an effect on the employment or earnings of Baltimore participants as originally projected.

First, many of the MTO experimentals had significant human capital barriers—including lack of adequate education and work experience, as well as mental and physical health problems—before moving to a low-poverty neighborhood. The MTO demonstration was not designed to address these deficits. In addition, employed respondents in both groups are heavily concentrated in retail and health care jobs. To get and keep jobs, many of these respondents relied heavily on a particular job search strategy—informal referrals from weak social ties (work contacts, acquaintances, or casual associates) who already held entry-level jobs in these sectors. Though experimentals were more likely to have employed neighbors, few of their neighbors held jobs in these sectors and therefore were not providing such referrals. Controls have fewer employed neighbors overall, but they were more likely to come across these useful weak ties in the course of their daily routines. Finally, the configuration of the Baltimore metropolitan area's public transportation routes in relationship to the locations of most jobs, in particular hospitals and nursing homes, posed special transportation challenges for experimentals as they searched for employment or tried to retain their jobs.

Background

Existing empirical studies that try to explain the employment problems of the urban poor usually focus on the influence of individual-level factors such as human capital or structural factors such as social isolation and the geographic accessibility of jobs. We look at how hypotheses and empirical support surrounding these three themes—human capital, social isolation, and spatial mismatch—are used to explain barriers to employment.

There is a strong positive connection between individual human capital and socioeconomic outcomes such as employment status and earnings.³ Many argue that the unemployment problems of the poor are due to a mismatch between their education and skills and the demands of a changing economy.⁴ For example, about 75 percent of entry-level jobs now require a high-school diploma, references, and general work experience.⁵ Human capital barriers

3. Becker (1975).

4. See Handel (2003) for a review of the literature. See also Kasarda (1985); Holzer and Danziger (1998).

5. Holzer (1996).

are most detrimental to employment when they appear in conjunction with other additional barriers to employment. For example, welfare recipients with a combination of educational deficits and mental and physical health problems have worse employment outcomes than individuals who only lack education or who only have health problems.⁶

The social isolation of those poor who live in high-poverty inner-city neighborhoods may contribute to their employment difficulties. One seminal study defines social isolation as “the lack of contact or of sustained interactions with individuals and institutions that represent mainstream society.”⁷ This theory implies that individuals’ actions are shaped by the actions of those who live around them. The neighborhood creates a normative climate that defines acceptable and unacceptable behaviors. Neighborhoods with high employment rates have a certain rhythm to daily life that may be beneficial to all residents. On a more practical level, employed neighbors can pass along job information to the unemployed. Additionally, communities with high employment rates will have more resources to invest in institutions that benefit all residents.⁸ Conversely, those living in neighborhoods with low employment rates may be isolated from a normative climate that promotes work, job information and referrals, and community resources.⁹

Another relevant line of research is in how most workers acquire their jobs. The majority of Americans find employment through social ties rather than help wanted advertisements or other formal methods.¹⁰ Neighborhood poverty may interact with how effective local social ties are in obtaining a well-paying job. In a study using the Atlanta Multi-City Study of Urban Inequality (MCSUI) data, researchers find that, controlling for individual-level characteristics, increases in neighborhood poverty lower the odds of having a social tie who has steady employment.¹¹ Even the few job contacts within these poor neighborhoods may not prove to be helpful in terms of social mobility. Another analysis with the same data finds that for African Americans, using a neighborhood job contact depresses annual income by \$3,214, whereas there is no effect for whites.¹² Similarly, African American residents of poor and racially

6. Danziger (2000).

7. Wilson (1987, p. 60).

8. Wilson (1996).

9. See also Massey and Denton (1993).

10. Granovetter (1974); Lin, Ensel, and Vaughn (1981); Lin and Dumin (1986); Fernandez and Weinberg (1997); Reingold (1998); Stoloff, Glanville, and Bienenstock (1999); Kleit (2001); Chapple (2002); Mouw (2002).

11. Tigges, Brown, and Greene (1998).

12. Green, Tigges, and Browne (1995).

segregated neighborhoods who use social ties to find their jobs usually work with predominantly African American coworkers.¹³ This racial composition within the job, in turn, has a negative effect on their annual earnings.

Granovetter (1973) argues that the most successful job searches are those that use weak ties (casual acquaintances), not strong ties (close friends or immediate family members). Residents of high-poverty inner-city neighborhoods are less likely to have access to the kind of social ties most effective for a job search, that is, extensive, varied, spatially dispersed, nonkin ties.¹⁴ Perhaps because of this, some analyses find that low-income workers tend to rely on strong (rather than weak) ties when seeking employment.¹⁵ One study of scattered-site public housing tenants in Maryland found that respondents used strong network ties when looking for jobs rather than neighbors, even though many of their neighbors were employed and had considerably more economic means than their close friends and family.¹⁶ Mendenhall (2005) examined the neighborhood networks of a sample of twenty-five Gautreaux housing assistance participants, African American women who were given the opportunity to move out of Chicago's public housing and segregated neighborhoods. Mendenhall found that among adult suburban movers, female neighbors in the higher-resource communities served as a valuable source of job networks for the least-educated women. But for women with somewhat higher levels of education, such as those certified for clerical work, their suburban neighbors were less helpful in the job search process.¹⁷

Another structural explanation for labor market disparities between inner-city and suburban job seekers is Kain's (1968) spatial mismatch hypothesis, which argues that the spatial location of jobs vis-à-vis inner-city workers may account for their low employment rates. According to this line of reasoning, the suburbanization of jobs, when combined with increasing residential segregation by class, has exacerbated the employment problems of the urban poor. Similarly, Wilson (1987 and 1996) argues that the decline of manufacturing jobs has left inner-city neighborhoods bereft of employers, while the rise of service sector employment has occurred mainly in the suburbs. Thus

13. Elliott (1999).

14. Granovetter (1995); Reingold (1998); Green, Tigges, and Diaz (1999); Elliott (2000).

15. Elliott (1999); Kleit (2001).

16. Kleit (2001, 2002).

17. Although early studies of Gautreaux found that the adult suburban movers experienced a modest gain in employment compared to those who stayed in the city (Popkin, Rosenbaum, and Meaden 1993), a recent analysis of a more representative sample, using administrative data, does not find a city versus suburb difference in the proportion of calendar quarters with positive earnings (Mendenhall, DeLuca, and Duncan, forthcoming).

many urban residents have the education or experience to fill these jobs but not the means to get to them. Research has shown that urban residents also suffer from a lack of information about suburban job openings and experience greater levels of hiring discrimination in the suburbs than in the city.¹⁸

Methods

This paper uses quantitative and qualitative data collected from individuals who signed up to participate in the MTO demonstration in Baltimore. We are in the unique position of having experimental data from a large quantitative sample of all Baltimore individuals who signed up for the mobility program by 1997 ($N = 636$), and a smaller, stratified, random qualitative sample ($N = 124$). The methodological problem of self-selection plagues most studies of neighborhood effects, as individuals have a certain amount of choice in deciding what neighborhood they live in and how long they remain in that neighborhood.¹⁹ Individual-level factors, within structural constraints of housing availability and financial resources, influence these decisions. The randomized design of MTO allows us to isolate the effect of neighborhood context on individual outcomes, since it encouraged otherwise similar groups of individuals to live in different types of neighborhoods.

We first use the quantitative data to estimate the effect of the MTO treatment on employment and earnings outcomes in Baltimore. We then use the qualitative data and methods of analytic induction to examine the processes by which these outcomes occur and generate hypotheses about the relationship between residential mobility and employment.

Quantitative Methods

The quantitative data for this paper come from a 2002 survey designed to test the effects of moving from public housing and some of the nation's poorest neighborhoods to low-poverty neighborhoods. These data contain information on individuals at two points in time, at baseline and in 2002.

When public housing residents enrolled in the MTO program between 1994 and 1997, the head of the household completed a baseline survey. Although

18. Kain (1968, 1992); Wilson (1987, 1996); Holzer (1996); Ihlanfeldt and Sjoquist (1998).

19. Tienda (1991); Brooks-Gunn, Duncan, and Aber (1997).

data exist for MTO participants in all five cities, this analysis primarily focuses on Baltimore respondents.²⁰ Of the Baltimore MTO participants, 97 percent of household heads are African American and 99 percent are female. These Baltimore families had high rates of unemployment, low educational attainment, and were likely to be receiving governmental cash assistance; 74 percent of respondents were unemployed at baseline, 43 percent did not have a high-school diploma or General Equivalency Diploma (GED), and 80 percent received cash welfare payments.

In addition to the baseline survey, respondents participated in a survey four to seven years after families were randomly assigned to one of the three groups. Data were collected from January to September 2002 and the sample includes all families randomly assigned through December 31, 1997. The overall response rate was 89.6 percent across the five cities.²¹ In Baltimore, the response rate was 89.3 percent.²² Fieldworkers conducted in-person surveys with adults, and the sample includes 2.6 members per family, including 1.6 children. The interviews took place primarily in the respondents' homes, using Computer-Assisted Personal Interviewing (CAPI) on laptop computers.

The experimental design of MTO allows us to draw conclusions about the effect of a low-poverty housing mobility policy on individuals, beyond individual and family-level characteristics. In this paper we look at the effects of living in a low-poverty neighborhood on employment and earnings outcomes by comparing average outcomes of adults assigned to the experimental and control groups. Because we have data from two points in time, and because of the experimental nature of the study, we are able to make inferences about causal mechanisms. This intent-to-treat (ITT) coefficient in our regression analyses estimates the causal effect of offering families the services—including the voucher to move to a low-poverty neighborhood, housing counseling, and budget counseling—made available through the experimental treatment. Although

20. The appendix shows how Baltimore participants compare with all MTO participants at baseline.

21. During fieldwork, a three-in-ten subsample of hard-to-locate families was taken in order to focus resources on difficult-to-find cases. Observations from the subsample receive greater weight in the analyses. Accounting for the fact that subsample observations are used to represent observations that were not in the subsample, we calculate an effective response rate (ERR) based on the phase one response rate (R1) and the subsample response rate (R2). $ERR = R1 + (1-R1)*R2$.

22. See Orr and others (2003) for a detailed description of the data collection and analysis of the survey data.

only 58 percent of Baltimore experimental group members used the voucher to make a low-poverty move (compared to 47 percent of experimental group members in the five cities combined), all still received some form of treatment if they attended the counseling sessions.

We calculate this ITT effect using ordinary least squares (OLS) regression with a set of covariates (\mathbf{X}) representing prerandom assignment baseline characteristics.²³ All of the models are computed using sample weights.²⁴ Although all three groups (experimental, Section 8, and control) are in the data, we omit adults in the Section 8 group from our analyses.²⁵ This leaves us with a sampling universe of 3,039 across all five cities and of 449 in Baltimore.

We use regression analyses to estimate the control mean of seven employment and earnings outcome variables. We first look at these seven outcomes across all five MTO sites, and then use the same models to analyze Baltimore outcomes. Let Y be the outcome of interest and Z be membership in the experimental group. Equation (1) shows a simple regression model used to estimate the control means (β_{10}) and the ITT differences between the experimental and control groups (β_{11}):

$$(1) \quad Y_i = \beta_{10} + Z_i \beta_{11} + \varepsilon_{1i}.$$

In order to increase precision of the estimates and control for any small sample differences in baseline covariates (\mathbf{X}), the primary quantitative analyses in this paper use regression-adjusted ITT effects, as estimated using equation (2):

$$(2) \quad Y_i = \beta_{20} + Z_i \beta_{21} + X_i \beta_{22} + \varepsilon_{2i}.$$

23. Means of these covariates can be found in the appendix.

24. These weights have three components, and they are described in detail in Orr and others (2003, appendix B). Three-in-ten subsample members receive greater weight since they represent individuals who were not contacted during this subsampling phase. For child and youth outcomes, youth from larger families receive greater weight. Since two children were randomly sampled from each household, they represent a larger fraction of the population. Finally, weights are used to take into account a change in the ratio of individuals randomly assigned to treatment groups.

25. For this reason, our coefficients are different than the results reported in Orr and others' (2003) analysis. We compare experimentals to controls, and Orr and others estimate experimental and Section 8 effects simultaneously.

Qualitative Methods

The qualitative data consist of transcripts and field notes from in-depth, semistructured interviews with a stratified random subsample of families who volunteered to participate in the MTO experiment. We sampled among all three program groups and evenly among three household types: (1) households with children ages 8–13 years only, (2) households with children ages 8–13 years and 14–19 years, and (3) households with children ages 14–19 years only. Of the 149 families sampled in Baltimore, we interviewed 124 adult respondents in the experimental, Section 8, and control groups (for an 83 percent response rate).²⁶ Reasons for nonresponse include inability to locate the respondent, death of the respondent, and respondent refusal.

The in-depth interviews with adult respondents took place between July 2003 and June 2004. Intensive locating and tracking efforts were followed by interviews usually lasting from two to five hours. The respondents were asked questions about their neighborhood, social status, employment, focal child (ages 8–13 years), focal youth (ages 14–19 years), and physical and mental health. Interviewers were instructed to ask specific questions, although the wording and timing of the questions often varied so that the interview felt like a conversation. Adult respondents were paid from \$50 to \$85 for their time, depending on whether we asked them about one or two children. These interviews were tape recorded, transcribed, coded thematically, and entered into a database by theme. Subsequent coding and analysis allowed us to take an inductive approach that is traditional in qualitative work, exploring the relationship between neighborhood characteristics and employment and earnings across the program groups. The extensive effort and cost required to obtain and process each interview limited the total number of families we could interview for this study.

Not all households assigned to the experimental group used the MTO voucher to make a move.²⁷ Among the fifty-one Baltimore respondents in the qualitative sample assigned to the experimental group, 62 percent used their voucher to move to a low-poverty neighborhood. Of these thirty-three *compliers* (the terminology used to describe respondents who moved with their MTO

26. We also interviewed a stratified random subsample of sixty-four families in Chicago, but this paper focuses solely on Baltimore families.

27. MTO participants had a limited period (typically 120 days) to use the voucher, and sometimes reported difficulty finding a unit in a low-poverty neighborhood or finding a landlord who would accept Section 8 housing.

voucher), only four were living at their placement address at the time of the qualitative interview. The rest had moved to different units, often in different neighborhoods.

We focus our qualitative analysis on the experimental compliers and a set of control group respondents who likely would have moved through the MTO demonstration had they been assigned to the experimental group. We use a matching procedure to determine those controls that likely would have moved through the program. We select nineteen likely control noncompliers to be the counterparts of the eighteen experimental noncompliers, with the reasoning that there should be the same fraction of adults in the control group who would not have complied as there is in the experimental group.²⁸ We select 100,000 random samples of nineteen from all Baltimore controls and then compare the average values of the eighteen experimental noncompliers to these nineteen controls on fourteen demographic, neighborhood, and employment variables.²⁹ Each of the 100,000 samples is given a similarity score and the most similar of the 100,000 constitute the nineteen likely control noncompliers. Similarity is defined as the sum of the difference in means for each variable divided by the control group standard deviation for that variable—essentially, the sum of the difference between groups in the average z-scores for the fourteen variables. Each variable receives equal weight in the calculation. Based on this matching procedure, we select a group of control noncompliers that are similar, on average, to the experimental noncompliers (as shown in appendix table A-2). For our qualitative analysis, we exclude the experimental noncompliers and control noncompliers. We use data from the experimental compliers—those who moved to a low-poverty neighborhood through the MTO program—and their likely control complier counterparts to explore the relationship between neighborhood mobility and employment in Baltimore.³⁰ For simplicity, we refer to experimental compliers as *experimentals* and likely control compliers as *controls* when discussing our qualitative sample.

Although the sample sizes are small, these qualitative data are very useful for exploring the processes by which neighborhoods may affect employment

28. We attempted to interview sixty-two Baltimore adults in both the experimental and control groups. We completed fifty-one interviews with experimental group participants (including eighteen of the twenty-two noncompliers) and fifty-three interviews with control group participants.

29. See the appendix for a description of the variables.

30. This paper does not look at adults assigned to the Section 8 group, although these families are included in the qualitative sample.

and earnings outcomes in a manner that cannot be captured by survey data. Interviewers systematically asked respondents about both human and social capital, so it is possible to look at how these factors interact with employment and earnings outcomes in the context of a housing mobility program. Examples of interview questions include, "Tell me the whole story about how you got [this/your last] job," and "Tell me about the events that led you to leave your last job." The matching of the experimental noncompliers with the likely control noncompliers allows us to take full advantage of the study design; those who used the MTO voucher are different than those who did not move, and lumping all of the experimental and control respondents together would not allow us to separate out these differences.³¹

Quantitative Results

We use data from the Interim Survey to look at the types of neighborhoods in which the MTO participants are living. We then examine employment and earnings outcomes for Baltimore respondents, and find that the MTO intervention did not have a significant effect on the economic self-sufficiency of these individuals.

Neighborhood Characteristics

Table 1 shows descriptive neighborhood information for the five-city survey sample and Baltimore survey sample. We define neighborhood by the census tract the individual lived in at each point in time and use data from the 2000 Census. We look at neighborhood socioeconomic disadvantage in the following four ways: poverty rate, percent of residents with college diplomas, percent employed among the civilian population, and percent African American. The percentage of African American residents does not directly estimate neighborhood quality, but serves as an indicator of racial residential segregation, which perpetuates the notion of the African American underclass and has implications for economic well-being.³²

31. Although our qualitative sample comes from a random sample of MTO participants in Baltimore, our small sample size prohibits these results from being representative of all Baltimore respondents or generalizable to the entire MTO population.

32. Massey and Denton (1993).

Table 1. Neighborhood Characteristics for Five-City and Baltimore Surveys, 2002^a

| <i>Characteristic</i> | <i>Five cities^b</i> | | <i>Baltimore</i> | |
|--|--------------------------------|------------------|-------------------------------|------------------|
| | <i>Control group mean</i> | <i>E – C</i> | <i>Control group mean</i> | <i>E – C</i> |
| Household income below poverty line in tract | .392 | –.085* (.008) | .355 | –.066* (.018) |
| College degree in tract among those over age 25 | .142 | .043 (.005) | .123 | .048* (.013) |
| Employed in tract | .410 | .054* (.005) | .411 | .063* (.014) |
| African Americans in tract | .562 | –.014 (.010) | .840 | –.049* (.028) |

Source: Authors' calculations based on U.S. Department of Housing and Urban Development's Interim Survey data on the Moving to Opportunity program.

E – C = experimental – control (intent-to-treat) difference.

* $p < 0.05$.

a. Estimates are based on equation (2) in main text. Sample size is 2,501 for five cities and 376 for Baltimore.

b. Five cities include Baltimore, Boston, Chicago, Los Angeles, and New York.

Across nearly all measures of neighborhood quality in Baltimore, experimentals were living in higher-quality neighborhoods than their control-group counterparts at the time of the 2002 survey (four to seven years after random assignment). These neighborhoods are a substantial improvement to the poor-quality neighborhoods that respondents were living in at baseline, where about half of the residents were living below the poverty line.

Employment Results

We use quantitative data from the 2002 survey to predict employment and earnings outcomes for Baltimore respondents. For these models, we use the full experimental and control sample so the coefficients are valid estimates of the MTO treatment. Consistent with previous employment and earnings findings on the MTO intervention,³³ we look at seven employment and earnings outcomes: currently employed; employed with health insurance; employed full time (thirty-five or more hours a week); weekly earnings above poverty; annual earnings in 2001; weekly earnings at main job; and employed at job greater than one year.

The dependent variable in each of our models is one of these seven employment or earnings outcomes. The independent variable is a dummy variable

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

Table 2. OLS Regression Models for Employment and Earnings Outcomes^a

| <i>Outcome</i> | <i>Five cities^b</i> | | | <i>Baltimore</i> | | |
|---------------------------------------|--------------------------------|---------------------------|-----------------|--------------------|---------------------------|----------------|
| | <i>Sample size</i> | <i>Control group mean</i> | <i>E – C</i> | <i>Sample size</i> | <i>Control group mean</i> | <i>E – C</i> |
| Currently employed | 2,525 | .520 | .016 (.021) | 379 | .577 | .061 (.050) |
| Employed with health insurance | 2,499 | .293 | .024 (.019) | 373 | .390 | .080 (.054) |
| Employed full-time (35 or more hours) | 2,501 | .389 | .001 (.021) | 372 | .481 | .012 (.054) |
| Weekly earnings above poverty | 2,386 | .321 | –.006 (.020) | 351 | .386 | .033 (.055) |
| Annual earnings in 2001 | 2,386 | 8,839 | 130 (448) | 353 | 10,047 | –142 (1059) |
| Weekly earnings at main job | 2,386 | 178 | 0 (9) | 351 | 192 | 15 (23) |
| Employed at job greater than one year | 2,496 | .359 | .031 (.021) | 374 | .401 | .085 (.053) |

Source: Authors' calculations based on U.S. Department of Housing and Urban Development's Interim Survey data on the Moving to Opportunity program.

E – C = experimental – control (intent-to-treat) difference.

a. Estimates are based on equation (2) in main text, using covariates described in the appendix. Robust standard errors shown in parentheses. All data weighted to adjust for sampling design.

b. Five cities include Baltimore, Boston, Chicago, Los Angeles, and New York.

for experimental group status (with control group status as the reference category), and we include thirty-eight covariates for baseline adult characteristics. These covariates allow us to account for any slight differences between the experimental and control groups at baseline and add precision to our models. We first look at the effect of the MTO treatment among all participants, and then restrict our models to Baltimore respondents. Table 2 displays our results.

These results are consistent with previous quantitative analyses that find the MTO intervention did not have any significant effect on the employment or earnings of participants.³⁴ In Baltimore, on which we base our qualitative sample, those assigned to the experimental group do not have employment and earnings outcomes that are statistically different from their control group counterparts. However, the treatment may have had a bigger impact in Baltimore than across all five cities. Compared to the control group, for example, the employment rate rose 6.1 percentage points in the experimental group.

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

Additionally, the number of respondents holding jobs with health insurance rose 8.0 percentage points, and the number holding their job for greater than one year rose 8.5 percentage points. Although the results in Baltimore are statistically insignificant, the magnitudes suggest some economically meaningful differences between the experimental and control groups. On other measures such as average earnings, however, there was no evidence of a meaningful difference.

Qualitative Results

The effects on adult economic self-sufficiency outcomes were not large or significant, which raises various questions. Do these results provide evidence that a housing mobility intervention does not greatly affect employment and earnings? Or is there a more complex story underlying these results—a story about how a very disadvantaged group of Americans who have spent years, perhaps an entire lifetime, in some of the nation's most distressed public housing projects, go about seeking, obtaining, and maintaining jobs? We turn to in-depth interviews with a random subsample of experimental and control group members in Baltimore to develop a set of hypotheses about some of the social processes that might be at work. Keep in mind that when we refer to experimentals in this section, we are referring to compliers, those who actually used their MTO voucher. When we refer to controls, we mean the matched set of control-likely compliers.

Profile of Baltimore Qualitative Respondents

Table 3 compares the demographic characteristics of experimentals and controls in the qualitative study, and the groups are fairly similar across these measures. Though there are differences in educational attainment, these are not statistically significant.³⁵ All respondents are women, most live with dependent children, and all but one are African American.³⁶ At the time of the qualitative interview, the respondents were, on average, about thirty-eight years old (ages ranged from twenty-six to fifty-seven). Twenty-eight percent of experimentals and 47 percent of controls had neither finished high school nor obtained a GED. On the other end of the spectrum, only 5 percent had gradu-

35. Although our overall number of cases is relatively small, we present percentages throughout the paper to preserve a sense of proportion.

36. One respondent identifies as multiracial.

Table 3. Descriptive Characteristics of Baltimore Qualitative Respondents^a

| <i>Characteristic</i> | <i>Compliers</i> | |
|---|---------------------|----------------|
| | <i>Experimental</i> | <i>Control</i> |
| Demographic | | |
| Female | 1.00 | 1.00 |
| African American | 0.970 | 1.00 |
| Age | 38.39 | 37.53 |
| Education | | |
| High-school dropout | 0.281 | 0.471 |
| High-school diploma/GED | 0.594 | 0.529 |
| College degree | 0.094 | 0.000 |
| Number of children | 2.82 | 3.09 |
| Number of people in household | 3.42 | 4.47 |
| Housing | | |
| Lived in public housing as child | 0.476 | 0.480 |
| Current housing | | |
| Public housing | 0.091 | 0.212 |
| Subsidized housing | 0.576 | 0.424 |
| Unsubsidized private housing | 0.152 | 0.182 |
| Homeowner | 0.121 | 0.182 |
| Other | 0.000 | 0.030 |
| Poverty rate < 10 percent | 0.242 | 0.059 |
| Poverty rate 10 to 20 percent | 0.333 | 0.294 |
| Poverty rate > 40 percent | 0.242 | 0.294 |
| Employment and public assistance receipt | | |
| Employment status | | |
| Full time | 0.455 | 0.324 |
| Part time | 0.212 | 0.235 |
| Unemployed | 0.333 | 0.441 |
| Receives Temporary Assistance for Needy Families (TANF) | 0.094 | 0.242 |
| Receives food stamps | 0.406 | 0.393 |
| Receives medical assistance | 0.563 | 0.576 |
| <i>N</i> (sample size) | 33 | 34 |

Source: Authors' calculations based on qualitative interview data from the Moving to Opportunity program.

a. Experimental compliers used an experimental voucher to move to a low-poverty area. Control compliers are control group members not selected as likely noncompliers on the basis of matching average characteristics of experimental noncompliers, as described in the text.

ated from a two- or four-year college program, obtaining an associate's or bachelor's degree, or are licensed practical nurses or registered nurses.

Approximately half each of experimentals and controls said they lived in public housing as a child. Of course, all were living in Baltimore public housing when they enrolled in the MTO program. Although the MTO demonstration did not provide the controls with a voucher to move, the residential mobility

of both groups after random assignment is high. By the time we conducted the in-depth interviews in 2003, six to nine years after random assignment, the majority of experimentals had moved from their low-poverty, MTO placement neighborhoods. This was possible because the MTO voucher reverted to a standard Housing Choice Voucher (Section 8 voucher)—that is, it lost its geographic restrictions—after one year of use in the placement neighborhood. Similarly, as a result of the widespread demolition of Baltimore public housing, most controls were not living at their baseline address by 2003 either. Indeed, those whose units had been demolished were offered the option of a standard Housing Choice Voucher.

At baseline, both experimentals and controls were living on the east and west sides of the center of Baltimore—neighborhoods characterized by very high rates of poverty, unemployment, and labor force detachment, as well as a host of other neighborhood distress indicators. Subsequently, the experimentals who took up the MTO offer and moved were dispersed to the outskirts of Baltimore City, the inner suburbs in Baltimore County, and several more distant suburbs. Most experimentals moved to neighborhoods that, while low in poverty, were still predominantly African American or mixed in their racial composition.

Over time, the experimentals moved somewhat closer toward the center of Baltimore City, though few moved back to their baseline neighborhoods. Meanwhile, controls usually moved to and remained within the city, often in quite close proximity to their origin neighborhoods. Despite high rates of residential mobility in both groups, the experimentals are still more likely than controls to be living in neighborhoods with poverty rates 20 percent and under (about 58 versus 35 percent), somewhat more likely than controls to live in subsidized private housing (about 58 versus 42 percent), and less likely to be living in public housing (about 9 versus 21 percent) than controls.

Labor Market Context

Experimentals and controls exhibit many similarities in their employment status and job quality, and nearly all respondents say they value work and that having a job is an important goal, in part because they believe it boosts self-worth. But the unemployed controls have less consistent work histories and have been unemployed longer than their unemployed experimental counterparts, who typically have been cycling between employment and unemployment.

Among experimentals, about 46 percent are employed full time, another 21 percent are employed part time, and 33 percent are unemployed by 2003. In the six to nine years since these respondents signed up for MTO, their

overall employment rate has increased dramatically. Two-thirds are currently working, as opposed to only 15 percent at baseline. This is presumably due, in part, to the more stringent work requirements mandated by welfare reform, implemented in Maryland in 1996.³⁷ The increase in employment is also presumably due to a booming economy (unemployment in Baltimore County dropped from 5.3 percent in 1995 to 3.7 percent in 2000),³⁸ and other factors that substantially boosted the work rates of low-income single mothers across the country.³⁹ Respondents' children also got older, reducing the potential costs of working (for example, child care) and enabling them to balance work and parenting more easily. The changes that resulted in increased employment for low-income women in general set an extraordinarily high bar for individuals assigned to the experimental group to show a significant difference from a control group experiencing the same trends. The experimental participants also had to negotiate neighborhood environments that were very different than those they had been used to, and they had to compete with other similarly skilled individuals to find a job.

However, many former welfare recipients who have left welfare for employment have been funneled into particular types of jobs, namely jobs in health care or retail establishments. These patterns are readily evident in our data, as half of the employed experimentals are working in health care either as nursing assistants, medical billing clerks, or as housekeepers, prep cooks, and other nonhealth care jobs within hospitals. Cheryl,⁴⁰ for example, a 29-year-old mother of four, works full-time as a prep cook for a Baltimore hospital, and Quresha, a 40-year-old mother of three, works full-time as a housekeeper at a hospital in a Baltimore suburb. Overall, just more than half of those working in health care work in these pink-collar jobs in hospitals. Nearly four in ten experimentals (37 percent) work in other service sector jobs, either in retail establishments, as janitors, in food service, or as child care workers. A few (18 percent) work as administrative assistants or in other office jobs, in blue-collar jobs (9 percent), or in the informal economy (9 percent).

Six of the eleven unemployed experimentals are only temporarily unemployed. These six women have steady work histories and are actively

37. National Health Policy Forum, George Washington University, "Welfare Reform in Maryland: Flexibility in Action" (www.nhpf.org/pdfs_sv/SV_MD02.pdf [April 25, 2002]).

38. U.S. Bureau of Labor Statistics, "Labor Force Data by County, 1995 Annual Averages" (<ftp://ftp.bls.gov/pub/special.requests/la/laucnty95.txt>); and "Labor Force Data by County, 2000 Annual Averages" (<ftp://ftp.bls.gov/pub/special.requests/la/laucnty00.txt> [accessed April 25, 2006]).

39. Meyer and Rosenbaum (2000).

40. We use pseudonyms throughout the paper to protect the confidentiality of the respondents.

searching for jobs, and some even have jobs lined up. The MTO survey, conducted four to seven years after random assignment and two years before our qualitative interviews, did not attempt to measure the nature of unemployment. Sadie, for example, lost her job as a housekeeper at a hotel one month before the interview. She was terminated from her job because she needed to take a week off to devote all of her energy to getting her son, Kevin, age seventeen, out of jail. He spent a month in jail on a robbery charge, only to be released after the police conceded it was a case of mistaken identity. Sadie, who has a steady work history, spent six months as a custodian at her son's school before working at the hotel (she was fired from the school job because she needed to deal with another child's asthma), tended bar for four years, and worked as a cashier at a convenience store. Now that Sadie's son is out of jail, the 41-year-old mother of three is again actively seeking work, looking daily in the want ads, and has an interview lined up for a job at a warehouse. LaShonda, a 40-year-old mother of two, has worked as a unionized welder for the past two years, and was laid off from her welding job two months ago. This job paid LaShonda \$17 an hour and included health and retirement benefits. When she worked overtime, she made \$25.50 an hour. She collects unemployment now, but anticipates working again soon.

In contrast to the experimentals, only one of the unemployed controls can be considered temporarily unemployed. Overall, unemployed controls have less consistent work histories—it has been several years since most of them have last worked—and most do not have concrete plans for future employment. This stands in stark contrast to the employment aspirations of the experimentals.

In our qualitative subsample, a somewhat smaller proportion of controls are currently working, compared to experimentals (about 56 versus 67 percent). Although half of employed experimentals are working in hospitals or health care jobs, only a few (16 percent) controls hold similar jobs. Mercedes, age thirty-three, is the only control employed as a nursing assistant, a job she has held for four years. Two other respondents, 37-year-old Sharon and 40-year-old Jane, work as customer service representatives at hospitals in Baltimore. More than four in ten (42 percent) employed controls work in service sector jobs outside of the health care sector as custodians, housekeepers, or in retail. Nearly a third (32 percent) do secretarial work, a few (16 percent) work blue-collar jobs (as meter maids or low-level municipal employees) and one respondent is self-employed, making gift baskets out of her home.

The fact that experimentals are more likely to be employed in health care jobs may give them an important advantage over the controls over time, as health care jobs are more likely to have medical benefits and career ladders than

jobs in other sectors. In addition, the demand for such workers should increase significantly in the future; one analysis projected an increase of 36 percent between 2000 and 2010.⁴¹ Tisha, a 32-year-old woman employed full-time as a medical billing team leader at a local hospital, says, "When you get into the medical field, it's nonstop growth there. You know, it's just nonstop [opportunity]. So I just feel like I'm just gonna take this and go all the way as far as I can until I just get burned out." On the other hand, though, many entry-level health care jobs are physically demanding and have high rates of turnover, posing additional barriers for those individuals in the profession.

Human Capital Barriers to Employment

Experimentals and controls have similar barriers to employment, such as low educational attainment and poor mental and physical health, which may in part result from years of exposure to concentrated poverty neighborhoods. These barriers were not explicitly addressed through the MTO demonstration. While many experimentals and controls were able to overcome such limitations, these barriers still pose difficulties.

At the time of the qualitative interview, a substantial minority (28 percent) of experimentals have neither graduated nor earned a GED, though some (9 percent) have a two- or four-year college degree. A handful of experimentals told us that MTO not only encouraged them to move out of their neighborhood, but also to further their education because their experiences were broadened. Lisa, a 38-year-old mother of three employed in a federal government office, attributes completing her bachelor's degree at Coppin State University, located in the Baltimore suburbs, to MTO. "That is how I took advantage of the [Moving to Opportunity] program. Where most people took advantage of it as far as maybe, well, it was still a better environment, but I wanted a full package. I wanted a better environment, a better education," says Lisa, who has plans to go back to school to earn her master's degree. Peaches, a 34-year-old experimental, began taking classes at Baltimore City Community College just before moving through the program, but completed her associate's degree after moving and was motivated to do so by her MTO move. "You know, it just opened up a whole another world for me. And, it was like a big change and I was like 'wow.' I was missing out on this whole experience, you know."

41. Harmuth (2002).

In addition to having more traditionally reported educational certifications such as GEDs or high-school diplomas, about 30 percent of experimentals have graduated from other short-term programs offering credentials for occupations such as home health aides or pharmacy technicians. Many of these respondents have three or four different certificates of this kind. Certificates that credential respondents for health care jobs are the most common, which probably explains in part why such a large proportion of experimentals work in such jobs.

One way housing mobility programs may benefit participants is from contact with new, employed neighbors who will offer job information and referrals. But the educational credentials of many respondents limit the usefulness of drawing upon these resources from those in their new environments. Experimentals rarely activate neighborhood social networks to search for jobs, as we discuss in more detail below. But this is partly due to the large differences between their own human capital and the education and skills of their new neighbors. Many respondents, especially those still living in very low-poverty neighborhoods at the time of the qualitative interview, say their neighbors all work in white-collar or professional jobs. Keona, a 30-year-old woman living in a low-poverty neighborhood, says that all of her neighbors have office jobs rather than the kind of work she seeks. "Office. I can see the way they dress. I can tell it's for an office. Not for a factory, you don't see, not even in nursing. . . . You know how you see more briefcases, suits." Terry, who also lives in a low-poverty neighborhood, says that many of her neighbors are lawyers or other highly educated professionals. Tina, a 32-year-old woman still residing in a low-poverty neighborhood, says her neighbors mostly work as doctors, police officers, and at the naval academy.⁴² Although it is unlikely that all of the experimentals' neighbors are doctors or lawyers, some of them probably are working in these occupations. More important, the fact that experimentals perceive their neighbors to be working in these jobs means that since they lack these credentials themselves, they usually do not even attempt to approach neighbors for job information or referrals. Even if they tried, it is unclear whether these ties would generate more or higher quality jobs than they are already getting through other means.

In addition to low education, experimentals also report a number of physical and mental health conditions that militate against finding work and staying

42. Experimentals not living in low-poverty neighborhoods talk about their neighbors' being employed, for example, as custodians, corrections officers, teacher's aides, and informal and formal child care providers.

employed. Living in high-poverty, economically depressed neighborhoods has a negative effect on one's health,⁴³ and as indicated earlier in this paper, past MTO research has shown significant physical and mental health gains for those who moved through MTO relative to controls.⁴⁴ In fact all five of the unemployed experimentals who are not cycling in and out of the labor market report debilitating physical or mental health barriers.

Roneesha, a 44-year-old mother of two and grandmother of two, is one example. Although Roneesha is HIV-positive, she managed to remain stably employed for many years until she began to suffer from panic attacks on the job. She had worked at her most recent job—a data entry position paying \$11.49 with full benefits—for seventeen years, but left the job after being hospitalized after her first panic attack. She explains, “I couldn’t get myself together for nothing, then I was really panicking out and I started shaking and rocking and shaking and rocking and so then finally recognized it was a panic attack and not no heart attack or nothing.” In addition to suffering from HIV and panic attacks, Roneesha suffers from diabetes and depression.

Thirty-nine-year-old Rochelle and 32-year-old Sonya both suffer from serious mental health problems as well. Rochelle, who had a nervous breakdown several years ago and receives disability payments for her depression, says she has never had a job and has no plans to search for one. Because of her mental health problems, interviewers had a difficult time constructing an employment profile for Sonya. Our fieldworker wrote after the interview, “The respondent has clearly some pretty serious mental illness issues . . . there were lots of stream of consciousness associations to strange objects to coat hangers and rattles.” Obviously, these conditions would not only influence respondents’ ability to sustain employment but their capacity to forge and maintain social connections as well—connections they might have relied on to secure a job.

Experimentals and controls had a similar mix of educational credentials at baseline, and there is no statistically significant educational attainment difference between the two groups at the time of the qualitative interview. In addition, about one-third of respondents in both groups have completed at least one short-term training program that certifies them for a job, usually low-level health care jobs.

43. Ross and Mirowsky (2001); Boardman and others (2001).

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

Like their experimental counterparts, unemployed controls demonstrate an acute awareness of the importance of education and experience for employment and pay. Rachel, a 37-year-old unemployed control who has worked in the past as a nursing assistant and an addictions counselor, says that these jobs now require a certificate or degree, and her lack of either is the reason for her lengthy recent spell of unemployment. "Everybody wants you to have a degree now, you know, and before it wasn't a big issue. I could get a job in addictions just like that. And now they want you to have a degree." Rachel, who dropped out of high school and never received a GED, is currently enrolled in a program in which she can earn her high-school diploma, and she believes this credential will help her find a job. Missy, a 36-year-old unemployed mother of three, says that her lack of work experience is holding her back. "Some of 'em, I have the experience, but I don't have the working experience [in recent] years, I don't have that kind," says Missy, whose last job as a housekeeper was five years ago. She says she tried to enroll in a training program recently but was not allowed to participate because of a conviction for marijuana possession.

Experimentals and controls also have similar physical and mental health barriers that sometimes prohibit them from getting a job or staying employed. Depression is the most common problem. Kenya, a 30-year-old mother of two who has been unemployed most of her adult life, has trouble sleeping through the night because of stress related to her cousin's fatal drug overdose. As a result, she falls asleep unexpectedly throughout the day. When we interviewed her, in fact, she fell asleep several times and we had to wake her. Kenya, a control, points to other sources of stress as well. "What stresses me out? My children's fathers, they ain't no good. Life itself stresses me out. The trials and tribulations that I've been through. Stresses me out thinking about it." Wendy, a 35-year-old mother of five who has been unemployed for two years, has severe arthritis, which prohibits her from jobs requiring her to stand all day on her feet or do physical labor. "Each job I just couldn't work; it was my legs swelling up," says Wendy. "Cramping, aching. . . . It just get me, oh God, my hands too."

As indicated above, analyses of the MTO 2002 survey, occurring four to seven years after random assignment, found mental health gains for experimentals relative to controls.⁴⁵ These results are encouraging, and suggest that moving to a low-poverty neighborhood can reduce psychological distress and depression. Among those in the qualitative sample, however, there are no

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

noticeable differences in respondents' reports of depression, stress, or other mental health problems as they relate to employment, though we did not use formal measures of depression and stress.

Social Connections and Residential Mobility

We now turn to the processes by which respondents search for jobs. In particular, we examine the extent that neighbors versus other members of respondents' social networks (close family, friends, and acquaintances) influence the job search process. Experimentals and controls find employment through similar channels and, when activating social networks to find a job, mostly rely on a particular type of weak tie, acquaintances who have similar jobs to those they seek and similar job credentials. Usually, these are not one's immediate neighbors. For experimentals, too few neighbors have such qualifications, and for controls, too few neighbors have jobs at all. Rather, both rely on casual encounters with acquaintances they have met on the job, in training, or in other venues over the years.

The employed experimentals used three job search strategies: formal methods, agency-based methods, and social networks.⁴⁶ When we asked these respondents how they got their current job, only a small number (14 percent) used formal methods, such as help wanted ads and direct application, to find their current job. Nearly a third (32 percent) used a temporary agency or local social service agency to find their job. Yet more than two-thirds (68 percent) relied on social networks (some respondents used more than one method, so these numbers total more than 100 percent). Of those who used social connections, about four in ten used a friend, though rarely a close friend. Other referral sources include current or past coworkers or other casual associates from school, church, or elsewhere. Only rarely did family members play this role.

Thus consistent with Granovetter (1974 and 1995), but not all prior research, the majority of experimentals who found their current job through social connections used a weak tie, not a strong tie. For example, 46-year-old Jacqueline found her part-time job as a crossing guard through a friend's father. "I used

46. Formal methods of job search include the following specific strategies: direct application, newspaper search, Internet search, Yellow Pages search, and responding to a flier. Agency-based methods of job search include using a social service agency, going through a temporary employment agency, participating in a job training program, and attending a career fair.

to check back and forth down at civil service . . . and I also know someone that used to work with the city, and she helped me. Matter of fact, her father, he used to work [down there], and he knew someone. Sometimes you have to know somebody to get a job.”

Yet none of the experimentals say they found their current job through a neighbor. “A lot of neighbors, they don’t, they don’t tell you too much about a job,” says 37-year-old Renee. Though neighbors did not play a direct role in job searches, several respondents describe how a neighbor’s example or encouraging words have motivated them when searching for a job. Amy, who spends weekends and some evenings as an evangelist traveling from church to church in the Baltimore area, says her neighbors have encouraged her to be persistent in her job search. Sheila, a 38-year-old nursing assistant still living in her MTO placement neighborhood, says she often talks to her neighbors about her job search, but these conversations and the tips they have shared have never led to employment.

Though no current jobs flowed through neighbor referrals, one experimental did find a past job through a neighbor and another says a neighbor helped her secure a job at a grocery store. This respondent, Cookie, is thirty-nine years old and lives in a low-poverty neighborhood. “As a matter of fact, one day I went to the store and after [my neighbor] had told me about [a job opening in the store she owned], I said, no, I don’t want to do it. . . . Then I got to the store—as a matter of fact I went there to get something. We was planting the flowers out in the back and I was all dirty. Went up there and I just happened to see Gail and I said, you know, let me fill out an application. And talked to her right there and she was like, ‘Well, don’t you wanna start next week?’ I was like, ‘All right, OK, I can do that.’ So I start next week.” These are the exceptions, however. “I don’t really do a lot of interacting with my neighbors other than just speaking, you know, just small conversation,” says Joyce, a 41-year-old mother of two. Roneesha says, “No, ’cause like I say, I don’t associate with [my neighbors]. Not a lot.”

Although many experimentals have not forged close ties with neighbors, most have noted their neighbors’ employment status and have some notion of what kind of work they do. Further, most eagerly point to the benefits of living in a community where most residents work as opposed to those where workers are few. Experimentals are proud that their neighbors are working. Tina brags that mostly everyone in her low-poverty neighborhood has a job where they wear uniforms, such as police officers. “You rarely catch anybody in their regular civilian clothes,” says this 32-year-old woman who is employed full time as a hospital laundry aide.

Neighbors who are working, many experimentals say, “take care of themselves” and do not “get into others’ business.” Peaches, who lives in a low-poverty neighborhood and is employed as a hospital help desk technician, tells us, “It makes a big difference when you have people focused on a goal or focused on something positive. It changes the whole environment where you live at. Because you know they are going somewhere. They are doing something positive with their life.” Unemployed residents, on the other hand, are not viewed as beneficial to the community. Lisa, the respondent employed at a Social Security office who has been living in her current neighborhood for five years, says, “[When people don’t have jobs], it brings the community down. What if you are a working person, and you have all this noise and you have to get some sleep and everybody in the neighborhood is just having a party, having a good time?”

There are no striking differences in the job search strategies of experimentals and controls. The two groups report using formal and agency-based methods to find their current job in relatively similar proportions. Among both groups, respondents who used social networks to secure a job are much more likely to report using a weak tie as opposed to a strong tie; 60 percent of experimentals and 79 percent of controls report using a casual acquaintance with similar skills and credentials for job referrals. Controls, however, find it easier to use these connections, because sheer proximity brings them into contact with such individuals more often. The low-poverty move limits access to individuals working in occupations similar to the ones the respondents usually seek. Getting a job not only requires that respondents have the required educational credential or relevant work experience, but that they learn of a given job opening promptly. Here, respondents believe, informal channels work best because such jobs fill rapidly, and by the time the job is listed in the newspaper or through other formal means it is generally too late to apply.

The controls mirror the experimentals in that none of them report getting information about their current job through an actual neighbor, but for a different reason than experimentals. For the controls, too few of their immediate neighbors are working. Baltimore survey results also show no effect of the MTO program on the probability of using a neighbor to find a current job. However, due to their residential location, controls’ daily routines do bring them into contact with others who can provide the relevant information. Experimentals have to work harder in order to make these sorts of contacts. They try to make up for this deficit by attempting to draw on their close ties—family members and close friends who may still live in or near their origin neighborhoods. However, since such ties are redundant (the

parties know many of the same people), they are rarely effective in linking individuals to social networks outside of their own.⁴⁷

Controls have more ability to use social connections to find work. Their neighbors are less likely to be employed overall, but they are more likely to have neighbors employed in occupations similar to their own. More importantly, because of their residential locations, they are more likely to make contact with acquaintances who work in such jobs in the course of their daily routines. Unlike the experimentals, none of the controls say their neighbors work in white-collar or professional jobs. Instead, they say their neighbors work in health care jobs, such as nursing assistants, in service sector jobs at retail establishments, and in blue-collar jobs, such as security jobs or guards at correctional facilities.

When we examine what controls say about their neighbors' employment (or lack of employment), we find they echo themes of the experimentals. The controls speak positively about the benefits of having neighbors who work, as employed neighbors do not have time to meddle in others' business and foster a desire to work. "If it's more working people, then you know it's less trouble. Everybody is focused on what their agenda is for the next day or whatever," says 37-year-old Cathy, who is employed in the informal labor market. Tammy, a 37-year-old woman employed part-time as a custodian, says that having employed neighbors motivates her. "If I don't work, and most people do, that's going to encourage me to work. And if it's the other way around, I may not be able to keep up with working," she says. Controls often lament the fact that many of their neighbors do not work. Only half live in neighborhoods where they perceive that most people work, and about a quarter (26 percent) say almost none of their neighbors are workers. Yet the perceived gains to experimentals of having working neighbors seems to have been diminished by more limited access to ties best able to provide useful job referrals.

Space and Residential Mobility

Next, we look at how moving to a low-poverty neighborhood changes the spatial dynamics of these families' lives. Experimentals are still living geographically farther from their baseline neighborhoods than controls are by the time we interview them, six to nine years after random assignment. Not only does this place them farther from the social ties that are so crucial to getting a job in their field, in this city at least, it places them farther from (not closer to) many of the jobs for which their education and skills qualify them. Both experimentals and controls say transportation is a problem when

47. Granovetter (1973).

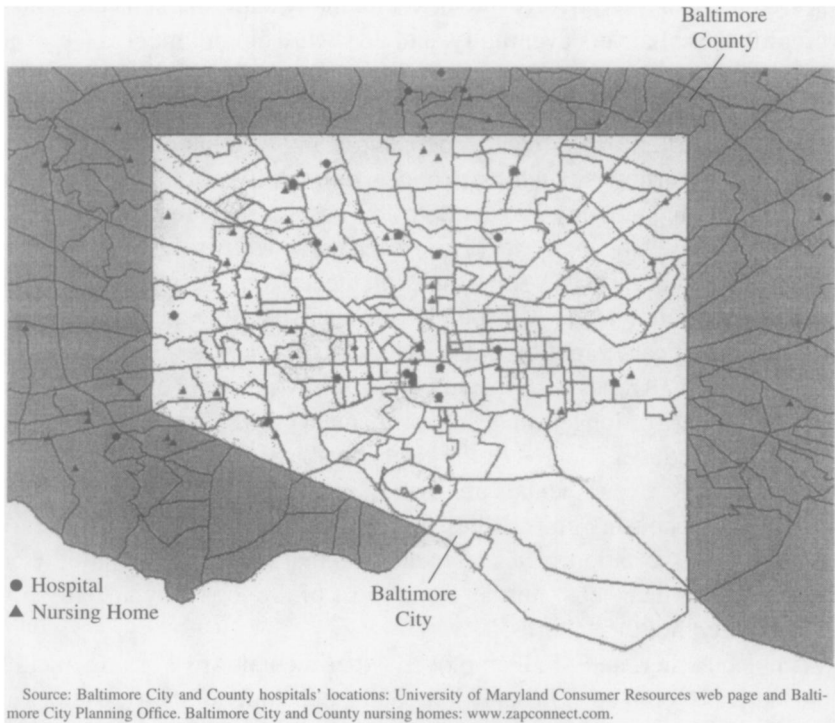
it comes to getting and keeping a job, but transportation problems are often exacerbated by a low-poverty move. While many experimentals are able to overcome these barriers eventually, and do secure employment at a reasonable distance from their homes, the barriers impede attempts to get jobs or search for better jobs.

As noted earlier, experimentals perceive many benefits of living in low-poverty neighborhoods, such as stronger community norms supportive of work. But housing mobility also comes with a cost for some families. When the experimentals used their voucher to move to a low-poverty neighborhood, most moved to neighborhoods a substantial distance from their public housing developments, on average 8.46 miles from their baseline address, although there was significant variation in the distances that families moved (ranging from 2.49 to 20.30 miles). After living in their low-poverty neighborhood for the required year or longer, many experimentals chose to move on to neighborhoods in the inner suburbs or the city's outskirts. At the time of the qualitative interview, experimentals are living, on average, 5.82 miles from their baseline public housing units. Unless they are lucky enough to live close to a job, public transportation routes often demand that workers take multiple bus routes to get to their jobs. Furthermore, most of the jobs they actually hold are in the city, not the suburbs.

As noted earlier, half of all employed experimentals are working in either health care jobs or in other hospital jobs, and the majority of these positions are in the city. Since such a large proportion of respondents are employed in health care occupations, we mapped the location of all hospitals and nursing homes in the metropolitan area (Baltimore City and Baltimore County) (see figure 1). Although we do not capture all possible health care jobs in the Baltimore area, we were able to gain address information for these larger health care employers. While there are twenty-two hospitals and fifty-seven nursing homes in Baltimore City, there are only eleven hospitals and fifty-one nursing homes in the geographically larger area of Baltimore County.

Ironically, then, many of the health care jobs for which many MTO participants are qualified are actually closer to where respondents were living at baseline than the neighborhoods they moved to through the program. This is not consistent with notions of spatial mismatch, which partly attributes the unemployment of urban residents and the persistence of urban poverty to the out-migration of jobs to the suburbs.⁴⁸ A substantial number

48. Kain (1968, 1992); Wilson (1987, 1996); Holzer (1996); Ihlanfeldt and Sjoquist (1998).

Figure 1. Hospitals and Nursing Homes in Baltimore City and County, 2005

of experimentals nonetheless have managed to secure health care jobs, and are more likely to have done so than controls despite an equal level of credentialing. However, this is in part because once the voucher became a standard Housing Choice Voucher, many moved closer to the city, where so many of these jobs are located.

Not only did experimentals' initial move place them farther from job referral networks, network ties provide two other crucial benefits for some—the child care and transportation that often enable single mothers to work. In the course of our interviews, we asked respondents to name their three closest friends and provide other descriptive information about them. Only 30 percent of experimentals say they have a friend in their neighborhood. In fact, many explicitly say they are not friends with their neighbors. Of the eight experimentals still living in a very low-poverty neighborhood, all but one fail to name a single close friend or family member in their neighborhood. This finding is consistent with Clampet-Lundquist's (2004) examination of social ties

among Philadelphia public housing tenants who moved through the HOPE VI initiative.⁴⁹

While 37 percent of experimentals own a car, only one experimental still living in a very low-poverty neighborhood does so. Car ownership is clearly important for employment among this group, as half of employed experimentals but only one unemployed experimental (the unionized welder who is only temporarily laid off), has a car. Of course, respondents who work may be more likely to own cars because they can afford them. However, having to rely on public transportation, which many deem unreliable, is commonly named as a barrier to finding work or sustaining employment, especially in the suburbs where buses run less frequently. Cheryl, the respondent employed full-time at a hospital in Baltimore, says she had to quit a previous job because she did not have reliable transportation to work. Cheryl, who does not own a car, says that “something was going on with the [Maryland Transit Administration] buses” and that she could not get to her job working in a medical records office. Terry, a 33-year-old experimental, discusses how transportation issues often result in her being late to her job as a school nurse at an elementary school in Baltimore. “The bus driver, she was late one day and then the next day she didn’t come at all. I be out there looking for another bus to catch. I am at the point where I am ready to buy a car,” she says, but gets depressed because she cannot afford car insurance.

Although not having a car presents a barrier to employment for some respondents, others demonstrate incredible perseverance in navigating the metropolitan area’s public transportation system. Roneesha, the respondent whose current health conditions prohibit her from working, has had a steady employment history despite never owning a car. She discusses her long commute to her administrative assistant job when still living in her MTO placement neighborhood. Roneesha says that she did not have to be at work until 8:00 a.m., but had to leave her house at 5:30 a.m. for her two bus, hour-long commute because of the irregular and often unreliable bus schedule.

Tina’s case is an exception to the transportation problems faced by respondents living in low-poverty neighborhoods. This 32-year-old respondent has a four-minute walk to her job as a laundry aide at a convalescent center in

49. HOPE VI, administered through HUD, is a public housing redevelopment program that frequently relocates tenants in the process of redevelopment. Clampet-Lundquist (2004) found that in the short term families who moved through the program stay to themselves because they did not care to make new friends and they wanted to avoid potential conflict with neighbors.

Anne Arundel County. Tina has lived at her placement address for eight years and has gotten by without a car, but says that transportation poses a problem in other aspects of her life. She talks about how she used her social connections to help navigate the new neighborhood. "When I first came out here, I was lost about everything. I used to catch a cab back to Baltimore before I knew the bus line was only down the road from me. As far as finding stores, [my neighbor who] I didn't know . . . someone that lived above me . . . took me to the stores out here. [My] transportation is basically the bus or [rides from] friends that I have met out here."

Transportation problems were also a motivation for some experimentals to move on from their MTO placement neighborhoods to somewhat more disadvantaged neighborhoods on the outskirts of the city, where buses and trains ran more frequently. Tisha, for example, did not have a car when she moved through the MTO program to a Baltimore County suburb. "I had to get back into the city where more buses run on a frequent basis than in the County. . . . If you miss the [bus], if you missed it, go back home, sit down at the table, whatever. 'Cause the next bus comes an hour and a half to two hours later. So that was ridiculous and there was a lot of stress and when I moved back to the city, I told my sister, I said, 'I feel so good. And much as I hate the bus I was never so happy to be back in the city where I could catch the bus to get anywhere I needed to go.' "

It is important to note, though, that transportation concerns also weigh heavily on lives of the controls. Nearly half of controls own cars (53 percent of employed controls and 40 percent of unemployed controls), but many of these vehicles are unreliable. Kenya, who is currently unemployed, describes how she used to miss work or be late because of her unreliable car: "I wouldn't go in. I'd call [my boss] and tell him, 'I can't make it. I don't have a car.' Or I'd borrow my friend's car every now and then." Nevertheless, among the qualitative respondents, the employed controls have shorter commute times than the experimentals, regardless of whether they drive or take public transportation to work. Additionally, since the controls are living closer to the center of the city, where buses and trains are more frequent, they have fewer complaints about access to public transportation.

Due to the widespread demolition of public housing in Baltimore, most of the controls have moved from their baseline addresses, too. Yet most are still living significantly closer to their baseline neighborhoods than the experimentals (3.11 miles versus 5.82 miles).⁵⁰ This seemingly small difference in

50. One control, who moved to North Carolina, is excluded from this analysis.

distance is still important, especially because before MTO, many respondents in both groups had been living in their baseline neighborhoods for a significant portion of their lives, sometimes their entire lives, and had virtually no exposure to a low-poverty neighborhood. Moving away from what is familiar can bring a host of challenges such as navigating new public transportation routes, finding child care and after-school programs, and locating an accessible coin-operated laundry, grocery store, or health clinic. Additionally, though all who moved from their baseline unit described suffering some severing of social ties, the disruption was much more severe among experimentals. Even six to nine years after random assignment, both experimentals and controls still talk fondly of the neighbors they had in their public housing projects, and lament about the amount of time it takes to build new neighborhood connections. The greater proximity of controls to their baseline neighborhoods is perhaps part of the reason why only 30 percent of experimentals mention having a close friend or family member in their neighborhood while nearly half (47 percent) of controls report a close neighborhood social connection.

In sum, the experimentals talk repeatedly about how they benefited from living in their low-poverty neighborhoods. But these neighborhoods posed unique challenges to them—they had to navigate transportation concerns and develop new social networks—that could create difficulty when searching for and maintaining employment.

Discussion and Conclusion

Previous analyses of the 2002 survey, conducted four to seven years after random assignment, find virtually no significant effects on employment or earnings of adults who moved to low-poverty neighborhoods through the MTO program.⁵¹ Given past theory and research, the lack of a larger effect of the MTO program on employment warrants further examination.

Our analysis of in-depth interview data from a random subsample of Baltimore MTO experimentals and controls explores the job search strategies and other social processes that may underlie the survey results. First, we find important differences in the labor market attachment of those experimentals and controls who are currently unemployed. The unemployed experimentals are cycling in and out of jobs with low wages and high turnover, and report considerable job stress. However, while their work experiences have been

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

far from ideal, they express a strong commitment to ongoing labor force participation. In contrast, more of the unemployed controls are permanently detached from the labor force, and fewer have been recently employed or are currently seeking employment.

Experimentals and controls have similar barriers to employment, such as low educational attainment and poor mental and physical health. These barriers, most of which predate their participation in MTO, may be a result of years of exposure to neighborhoods that are among the most distressed in the nation. Furthermore, these barriers were not explicitly addressed by MTO. Even many low-wage service sector jobs now require employees to possess a high-school diploma or GED. And those employers who do not require this credential will presumably choose a high-school graduate over someone without such credentials. Similarly, respondents who are battling depression or other mental and physical health problems have a more difficult time sustaining employment—and forming and maintaining social connections that can lead to employment—than healthy respondents.

Additionally, the move to their low-poverty neighborhood may have decreased experimentals' access to a particular type of social tie that has proved particularly effective in the job search process for the employed respondents in both program groups, namely acquaintances with similar skills and credentials who work in jobs similar to those the respondent is seeking. In respondents' views, such jobs typically fill quickly, often before they are posted in the newspaper or by other formal means. Thus insider tips about upcoming or recent openings are crucial to successfully securing a job. Ironically, though experimentals are more likely to have working neighbors, take pride in that fact, and sometimes credit the presence of these neighbors for providing them the motivation to get and keep jobs, no experimental was referred to her current job through a neighbor. Meanwhile, experimentals' residential locations may have decreased the probability of a chance encounter with these crucial social ties. Controls had fewer employed neighbors and rarely got job information and referrals from neighbors either, but their residential locations led to more of these chance encounters in the course of their daily routines. Since the majority of respondents in both groups tend to rely on social ties to find jobs, MTO may have simultaneously increased the motivation to work while inadvertently making the process of job search more difficult.

Transportation poses an additional challenge to MTO participants. Many experimentals are employed in the expanding health care sector, which bodes well for their future employment, as there is an increasing demand for health care workers. But in the Baltimore metropolitan area, these jobs are more

likely to be located in the city or on the city-suburban fringe than in the suburbs. Suburban residents who rely on public transportation must often commute to the city center first, and then on to their job. Suburban buses also run less frequently, increasing commute time. Though most experimentals eventually find work that does not involve an onerous commute, in the qualitative sample at least, this is often because they have rejected jobs that are too difficult to get to or because they have made a subsequent move in order to be closer to their job or to more reliable public transportation.

The respondents in our qualitative sample were randomly chosen from the entire MTO population in Baltimore, but our sample size is small. Thus we cannot generalize these results to all MTO participants. Nor do we intend to make causal claims of any kind. Rather, we deploy these data to develop hypotheses about the array of complex social processes that may underlie the MTO survey results. Basing our hypothesis development directly upon the experiences of MTO families has helped identify which of many potential theories are most grounded in the particular context in which the families live. We hope that these results will help guide questionnaire design in future waves of the MTO survey and future quantitative analyses of housing mobility programs.

APPENDIX

Table A-1. Means of Covariates Used in Regression Analyses

| <i>Covariates (at baseline)</i> | <i>Five cities^a</i> | | <i>Baltimore</i> | |
|--------------------------------------|--------------------------------|----------------|---------------------|----------------|
| | <i>Experimental</i> | <i>Control</i> | <i>Experimental</i> | <i>Control</i> |
| Male | 0.01 | 0.02 | 0.01 | 0.02 |
| Black | 0.67 | 0.66 | 0.98 | 0.99 |
| Other nonwhite race | 0.26 | 0.27 | 0.02 | 0.01 |
| Hispanic | 0.29 | 0.29 | 0.02 | 0.02 |
| Age 19–29 | 0.36 | 0.37 | 0.33 | 0.38 |
| Age 30–39 | 0.42 | 0.41 | 0.48 | 0.43 |
| Age 40–49 | 0.15 | 0.16 | 0.13 | 0.14 |
| GED | 0.18 | 0.21 | 0.14 | 0.17 |
| High-school diploma | 0.41 | 0.38 | 0.45 | 0.39 |
| Enrolled in school | 0.16 | 0.16 | 0.14 | 0.17 |
| Never married | 0.62 | 0.62 | 0.73 | 0.74 |
| Under age 18 at birth of first child | 0.25 | 0.24 | 0.27 | 0.27 |
| No teen children in household | 0.59 | 0.62 | 0.62 | 0.67 |
| Employed | 0.29 | 0.25 | 0.22 | 0.30 |

continued on next page

Table A-1. Means of Covariates Used in Regression Analyses (continued)

| <i>Covariates (at baseline)</i> | <i>Five cities^a</i> | | <i>Baltimore</i> | |
|--|--------------------------------|----------------|---------------------|----------------|
| | <i>Experimental</i> | <i>Control</i> | <i>Experimental</i> | <i>Control</i> |
| Received AFDC/TANF | 0.74 | 0.75 | 0.78 | 0.79 |
| Had car that runs | 0.17 | 0.15 | 0.05 | 0.03 |
| Any household member disabled | 0.16 | 0.16 | 0.12 | 0.08 |
| Core family size = 2 | 0.23 | 0.20 | 0.26 | 0.28 |
| Core family size = 3 | 0.30 | 0.32 | 0.36 | 0.35 |
| Core family size = 4 | 0.23 | 0.22 | 0.21 | 0.23 |
| Previously applied for Section 8 | 0.41 | 0.45 | 0.52 | 0.58 |
| Moved 3+ times in past five years | 0.08 | 0.11 | 0.10 | 0.13 |
| Lived in neighborhood 5+ years | 0.61 | 0.62 | 0.51 | 0.52 |
| No family in neighborhood | 0.65 | 0.65 | 0.74 | 0.72 |
| No friends in neighborhood | 0.40 | 0.41 | 0.42 | 0.49 |
| Chatted with neighbor in street or hallway at least once a week | 0.52 | 0.55 | 0.63 | 0.56 |
| Very likely to tell neighbor if saw neighbor's child getting into trouble | 0.53 | 0.56 | 0.64 | 0.64 |
| Streets near home very unsafe at night | 0.48 | 0.49 | 0.57 | 0.54 |
| Very dissatisfied with neighborhood | 0.46 | 0.46 | 0.59 | 0.49 |
| Primary or secondary reason for moving was drugs and gangs | 0.77 | 0.78 | 0.83 | 0.81 |
| Primary or secondary reason for moving was better schools | 0.47 | 0.48 | 0.37 | 0.39 |
| Very sure would be able to find an apartment in a different area of city | 0.45 | 0.45 | 0.55 | 0.59 |
| Household member had been victimized within six months | 0.42 | 0.41 | 0.50 | 0.40 |
| Boston | 0.22 | 0.21 | ... | ... |
| Baltimore | 0.23 | 0.22 | ... | ... |
| Chicago | 0.16 | 0.16 | ... | ... |
| Los Angeles | 0.25 | 0.25 | ... | ... |
| <i>N</i> (number of cases) | 1,453 | 1,080 | 213 | 168 |

Source: Authors' calculations based on U.S. Department of Housing and Urban Development's Interim Survey data on the Moving to Opportunity program.

... Not applicable.

AFDC = Aid to Families with Dependent Children; TANF = Temporary Assistance for Needy Families.

a. Five cities include Boston, Baltimore, Chicago, Los Angeles, and New York.

Table A-2. Means of Variables Used to Match Noncompliers

| Variable | Experimental noncomplier | Control likely noncomplier ^a |
|--------------------------------------|-----------------------------|--|
| Age | 42.0 | 40.6 |
| Number of children | 3.08 | 3.16 |
| Full-time employment | 0.61 | 0.63 |
| Part-time employment | 0.11 | 0.11 |
| Job through formal search | 0.17 | 0.26 |
| Job through agency | 0.17 | 0.16 |
| Job through network | 0.33 | 0.32 |
| Dropout | 0.29 | 0.32 |
| Poverty rate in tract | 0.65 | 0.63 |
| High-school graduation rate in tract | 0.40 | 0.38 |
| College degree rate in tract | 0.11 | 0.12 |
| Employment rate in tract | 0.37 | 0.39 |
| Living in initial unit | 0.17 | 0.16 |
| Government assistance | 0.37 | 0.37 |

Source: Authors' calculations based on qualitative interview data from the Moving to Opportunity program.
a. Selected based on matching of these characteristics, as described in the text. See below for variable descriptions.

Variables Used to Select Control Noncompliers

- Age:** Age of respondent.
- Number of children:** Number of children in household.
- Full-time employment:** 1 = respondent employed full-time; 0 = respondent not employed full-time.
- Part-time employment:** 1 = respondent employed part-time; 0 = respondent not employed part-time.
- Job through formal search:** 1 = respondent found current job through formal search (newspaper, Internet, Yellow Pages, direct application); 0 = respondent did not find current job through formal search or unemployed.
- Job through agency:** 1 = respondent found current job through agency (temporary employment agency or social services agency); 0 = respondent did not find current job through agency search or unemployed.
- Job through network:** 1 = respondent found current job through social network connection; 0 = respondent did not find current job through social network connection or unemployed.
- Dropout:** 1 = respondent did not graduate from high school; 0 = respondent graduated from high school.
- Poverty rate in tract:** Percentage of neighbors living below the poverty level at time of qualitative interview (2000 Census data).

High-school graduation rate in tract: Percentage in neighborhood with high school diploma or GED at time of qualitative interview (2000 Census data).

College degree rate in tract: Percentage in neighborhood with college degree at time of qualitative interview (2000 Census data).

Employment rate in tract: Employment rate among civilian population at time of qualitative interview (2000 Census data).

Living in initial unit: 1 = respondent living in reference unit at time of qualitative interview; 0 = respondent not living in reference unit at time of qualitative interview. Reference unit is placement unit if respondent moved through MTO. If respondent did not move through MTO, reference unit is baseline unit.

Government assistance: Average of **Medicaid** (1 = respondent receives Medicaid; 0 = respondent does not receive Medicaid), **HCV** (1 = respondent receives housing assistance; 0 = respondent does not receive housing assistance), **TANF** (1 = respondent receives TANF; 0 = respondent does not receive TANF), **Food stamps** (1 = respondent receives food stamps; 0 = respondent does not receive food stamps).

Comments

Jens Ludwig: There is widespread belief that a person's neighborhood of residence affects labor market outcomes, particularly for low-skilled minority workers living in central cities. This view stems from the results reported in a large body of theoretical and empirical research from across the social sciences. Yet the conclusion that neighborhoods matter for labor market outcomes seems to stand in sharp contrast to research on the Moving to Opportunity (MTO) randomized mobility experiment, which shows little impact on work or earnings measured four to seven years after random assignment.¹

I consider ways of reconciling these conflicting strands of research as well as the implications for public policies designed to improve the labor market prospects of disadvantaged workers. I focus on three of the leading explanations that have been offered to explain why findings from MTO conflict with most people's reading of the existing literature on neighborhood effects: 1) the possibility that MTO did not generate large enough differences in neighborhood environments to affect outcomes; 2) whether estimates of neighborhood effects on the MTO population, which consists of the subset of public housing families who volunteered for the demonstration, generalize to other groups; and 3) the possibility that the effects of mobility on labor market outcomes become more pronounced over time.

This paper by Turney and her colleagues provides useful information on these candidate explanations in the form of detailed, qualitative accounts of MTO families' experiences in the Baltimore demonstration site. After discussing previous hypotheses to reconcile MTO with existing research in light of findings from this work and other studies, I consider the evidence on another explanation that seems to have received less discussion—the possibility of bias with the previous nonexperimental research.

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

Did MTO Change Neighborhoods Enough?

It is natural to wonder whether MTO actually changed neighborhoods enough to plausibly affect labor market or other outcomes. After all, of those families assigned to the MTO experimental group, only a fraction moved through the MTO program (58 percent in the Baltimore demonstration site). Experimental-group families were only required to live in their new low-poverty neighborhoods for one year, at which point they were free to use their vouchers to relocate to higher-poverty areas, which many chose to do. In addition, some control-group families wound up moving to neighborhoods with lower poverty rates over time on their own or as a result of HUD demolitions of public housing projects.

Nevertheless, there are at least three reasons to believe that MTO generated important changes in the neighborhood environments of program participants, and therefore has something useful to say about the neighborhoods' role on labor market outcomes. First, across all five MTO cities, assignment to the experimental (rather than control) group reduced poverty rates by about 15 percent of the control group average in the tracts in which families were living four to seven years after random assignment (see table 1). In the Baltimore MTO site the experimental-control difference is more like 20 percent of the control mean for tract poverty, almost as large (17 percent) for tract employment rates, and more than twice as large (42 percent) as a share of the control mean for the presence of affluent (college-educated) adults in the neighborhood. These across-group differences pool together the experiences of families in the experimental group who did and did not move through MTO. The impact on those families who actually moved through the experimental MTO treatment in Baltimore will be about 1.7 times as large as the overall across-group differences.² The one exception to this general pattern of MTO-induced changes in neighborhood attributes is for racial integration, which was more modestly affected by the MTO experimental treatment.

A second reason to believe that MTO generated important changes in neighborhood environments is that MTO participants themselves perceive important differences in their neighborhood environments, as suggested by

2. Bloom (1984) demonstrates that if the treatment compliance rate is below 100 percent, so long as treatment assignment has no effect on those who are assigned to the treatment group and random assignment is in fact random, we can infer the effects of the treatment on those who are treated as the overall difference between the treatment and control group in some outcome (in this case postassignment neighborhood environments) divided by the treatment compliance rate (in this case, $1 / 0.58 = 1.72$).

the qualitative interviews described by this paper's authors. Families in the experimental group notice that a large fraction of their neighbors are employed (including some neighbors who are in higher-status, more-skilled office jobs) and these families think this has beneficial effects on the quality of community life in general. Other survey data from MTO reveal large experimental-control differences in overall reported satisfaction with neighborhoods measured four to seven years after random assignment, including pronounced changes in community safety and disorder.³

Finally, the possibility that MTO-induced neighborhood changes are large enough to change labor market outcomes is suggested by the fact that these neighborhood changes had some effect on other outcomes. Assignment to the MTO experimental rather than control group leads to detectable improvements in: adult mental health; some measures of physical health (reductions in obesity); and a wide variety of outcomes for female youth, such as risky behavior and mental health. There are even improvements in violent criminal behavior of male youth, at least in the short run.⁴ One might wonder whether particularly large neighborhood changes are required to affect labor market outcomes, if work and earnings are somehow harder to influence than other behaviors or subject to some sort of tipping phenomenon. But a recent study finds little evidence for nonlinearities in the effects of neighborhood socioeconomic composition on labor market or other outcomes in MTO.⁵

How Do MTO Results Generalize to Other Populations?

MTO families were drawn from some of the country's worst public housing projects, located in some of the nation's most disadvantaged neighborhoods. This paper notes that many of the MTO adults were themselves brought up in public housing, and more generally have low levels of schooling and high rates of mental health problems. The qualitative interviews of Baltimore families highlight the implications of these factors for employment outcomes. So what can be learned about neighborhood effects more generally from studies of MTO?

Suppose that families need some minimum level of human capital to take advantage of the opportunities afforded by living in a lower-poverty neighborhood. The MTO findings in this case are still interesting in their own right

3. Orr and others (2003).

4. Orr and others (2003); Kling, Ludwig, and Katz (2005); Kling, Liebman, and Katz (2007).

5. Liebman, Katz, and Kling (2004).

because so many low-income families share similar human capital barriers to those facing the MTO program population. For example, in 1994 (the year MTO began), about 45 percent of poor American adults had less than a high-school education, and survey data around this time for a sample of less-educated women find that nearly two in five report poor mental health.⁶ Moreover, since MTO families volunteered for the demonstration, it might be expected that the families who signed up are the ones who expect to benefit the most from moving. There is a plausible argument to be made that MTO provides upper-bound estimates for the effects of neighborhood mobility on similarly disadvantaged families in the population as a whole.

Will MTO Effects Become More Pronounced over Time?

The MTO interviews reported in this paper by Turney and her colleagues provide several reasons to believe that the program's effects on labor market outcomes could potentially increase over time. Adults in the Baltimore MTO experimental group are more likely than controls to work in the health sector, which MTO participants at least believe provides greater opportunities for upward mobility in the future. Some of the experimental families describe efforts to improve their educational credentials, a process that might take some time given these adults are likely to be limited to part-time study. In addition, many experimental families seem to not take full advantage of the social networks available in their new neighborhoods, in part because they are not yet comfortable interacting with their new neighbors. Put differently, this last problem may be one of demand rather than supply for helpful social interactions in these new neighborhoods, which in principle could change over time as MTO families become more comfortable in their lower-poverty communities.

Selection Bias

Implicit in any discussion of MTO is that the results may differ from previous studies because the latter are plagued by self-selection bias—that is, bias from hard-to-measure individual or family attributes that are associated with both neighborhood selection and labor market or other outcomes of interest. Yet in practice this possibility does not seem to be taken as seriously as it should by many analysts who currently believe that neighborhoods are extremely important for labor market outcomes.

6. For the first point, see Blank (1997, p. 17); for the second point, see Kaestner and Tarlov (2003).

One should be cautious about investing too much faith in most previous nonexperimental studies of neighborhood effects. Consider, for example, what happens when one analyzes the MTO data nonexperimentally, for example by relating variation in neighborhood attributes within, rather than across, randomly assigned MTO groups to variation in outcomes for MTO participants, controlling for a variety of individual and family baseline attributes. This type of standard nonexperimental method generates estimates that are not only of the wrong magnitude compared to experimental findings, they often are of the wrong sign.⁷ These findings are important in part because the set of control variables available with the MTO data is relatively rich by the standards of this research literature.

Summary

This paper provides interesting descriptive information about how MTO changes the neighborhood environments and life experiences of program participants in Baltimore. These experiences seem relevant to understanding neighborhood effects on poor families more generally, since MTO induces substantial changes in neighborhood characteristics for those families who move and many low-income families in America have human capital barriers similar to those for the MTO population.

In about three years, when the final long-term results for MTO are available, we will know more about whether the difference in findings from MTO versus earlier studies is explained by differences in the residential duration of study samples in their current neighborhoods of residence. In the meantime there are reasons to suspect that differences in residential duration are unlikely to explain away the entire difference in results between MTO and previous studies, since residential mobility is a prevalent phenomenon among American families more generally, particularly among low-income minority families.⁸

Although the long-term MTO results have yet to be published, the findings available to date from the MTO demonstration still provide very useful information about the effects of different housing policy options on the labor market outcomes for disadvantaged families living in distressed public housing communities. Findings reported here and elsewhere suggest efforts to move public housing families into private-market housing through expanding voucher programs is very likely to improve the well-being and physical safety of these

7. Liebman, Katz, and Kling (2004); Ludwig and Kling (2006).

8. South and Crowder (1997); Briggs and Keys (2005).

families and may be worth supporting for that reason alone. However, expanding residential mobility for disadvantaged families alone is unlikely to generate detectable changes in work or earnings for many of these families, at least for a period of up to five years.

John Karl Scholz: Moving to Opportunity (MTO) is one of this generation's most important randomized social experiments. Like the negative income tax experiments of the 1970s, or major welfare reform evaluations of the 1990s (such as the Canadian Self-Sufficiency Project, Minnesota Family Investment Program, and Milwaukee's Project New Hope), the MTO demonstration examines factors that many believe fundamentally affect the lives of low-income Americans. But unlike most previous experiments (including those mentioned above), MTO does not focus on human capital acquisition and labor market incentives. Instead, it was designed to provide evidence on the ideas that:

—residence in a distressed community can limit an individual's economic prospects;

—inner-city, low-skilled minority workers are disadvantaged because job opportunities are disproportionately in suburban areas;

—housing market discrimination, commuting costs, and other barriers make it difficult to reach those suburban jobs.¹

MTO provides evidence on these ideas by offering housing vouchers to randomly selected households in high-poverty public housing projects in five U.S. cities, and by comparing their experiences to a control set of households, also from high-poverty public housing projects in five U.S. cities.

MTO's results to date are unexpected and striking. Kling, Liebman, and Katz (2007), in a wide-ranging analysis of the experimental data, find the intervention succeeded in altering the neighborhoods in which treated households lived.² Namely, families that were offered vouchers lived in safer neighborhoods with lower poverty rates than families in the control group that were not offered vouchers. Despite the change in neighborhood quality, however, there is no significant evidence of beneficial treatment effects on earnings, welfare participation, or the amount of government assistance received after an average of five years following random assignment. This result leads Kling, Liebman, and Katz to conclude "housing mobility by itself does not appear to be an effective anti-poverty strategy—at least over a five-year period."

1. See Wilson (1987); Kain (1968).

2. This paper is available at www.nber.org/papers/w11577.

MTO had some measurable, experimental effects. Adult mental health improved for the experimental group relative to the control group across several specific measures, including distress, depression, anxiety, calmness, and sleep. The intervention did not have a statistically significant effect on overall physical health of adults, however. There are positive effects on mental health and risky behaviors for female youth and negative effects on mental health and risky behaviors for male youth. The findings on mental health and gender differences in some youth outcomes will fuel social science research in the years to come.

I view the results showing no beneficial effects of better neighborhoods on employment-related outcomes as being remarkable.³ Hundreds of social science papers have been written examining the deleterious effects of bad neighborhoods on various outcomes, or examining harmful effects of spatial mismatch on employment outcomes. Kling, Liebman, and Katz (2007) cite a comprehensive survey, for example, that concludes the empirical evidence overwhelmingly supports the spatial mismatch hypothesis.⁴ Given the surprising outcome of the MTO experiment, particularly given the extensive body of social science research that led me (and presumably many others) to expect different MTO outcomes, further work probing the MTO results would be valuable.

This paper by Turney and her coauthors is a nice step in that direction. The paper focuses on Baltimore, one of the five MTO cities (the others were Boston, Chicago, Los Angeles, and New York), and combines statistical evidence on the MTO sample, with completed, in-depth qualitative interviews of 104 Baltimore participants (fifty-one in the experimental group, fifty-three in the control group). This paper nicely illustrates the insights that qualitative research can provide in better understanding factors that may lie behind statistical, or quantitative, analysis. The authors also aspire to use the qualitative work to derive hypotheses that can be used to guide further qualitative work and guide the next rounds of survey work with the MTO population, scheduled for 2007.

The Baltimore MTO sample is overwhelmingly African American and female, which differs somewhat from the populations of other MTO cities. But the quantitative analyses of the Baltimore sample are similar to the results for the broader five-city MTO sample. Across nearly all measures of neighborhood

3. Kling, Liebman, and Katz (2007) are more understated, writing "it is somewhat surprising that the MTO intervention . . . had no discernable overall effects on unemployment."

4. See Ihlandfeldt and Sjoquist (1998).

quality, households in the treatment group were living in higher-quality neighborhoods than their control group counterparts, four to seven years after random assignment. Thus the MTO intervention successfully altered the feature of household environments that it was designed to affect. Nevertheless, across seven different outcomes, those assigned experimental group status do not have employment or earnings outcomes that are statistically different from their control group counterparts.⁵

When the quantitative evidence mentioned above is combined with the qualitative evidence, which is the heart of Turney and her coauthors' paper, it is useful to consider differences in the samples used for the two approaches. The authors show the quantitative results for Baltimore are consistent with the broader MTO results reported in Kling, Liebman, and Katz (2007). But it might also be interesting to confirm the quantitative results hold in the 124-household subsample selected for the qualitative study. With such a small sample, fewer of the appendix covariates can be used in estimating the regression-adjusted treatment effects. But when considering the qualitative evidence, I would like to know there are no statistically significant employment differences in the subsample used in the qualitative analysis.

More importantly, the regression work in the paper by Turney and her coauthors examines the "intent to treat." That is, the results measure differences between all treatments and controls. A substantial fraction (42 percent) of those given an offer of treatment chose not to accept the voucher to make a move to a low-poverty neighborhood. Yet these households are included in the intent-to-treat estimates. In contrast, much of the qualitative analyses focus on the "treatment on the treated," dropping treated households who refuse the MTO offer and a matched set of control group households.

It is not clear to me why, given the available data, the qualitative work focuses on the treatment-on-the-treated parameter. Doing so reduces already small samples. The authors drop eighteen treatment households who did not take up the MTO offer and a matched set of nineteen control group households.⁶ The observable characteristics of the matched sample closely mirror the characteristics of the treatment sample that did not participate in the MTO pro-

5. Point estimates of treatment effects for the Baltimore subsample are larger than for the broader MTO sample. The treatment group have employment rates 5.7 percentage points higher, health insurance coverage is 7.9 percentage points more likely, and the number having a job more than one year is 8.5 percentage points higher than the corresponding rates for the control group, but none of these effects are statistically significant at the usual confidence levels.

6. Twenty of the 124 households in the sample for the qualitative analysis also did not complete their interviews.

gram. Because the characteristics of those who did not take up the MTO offer were so carefully matched, the characteristics of the treated sample and the remaining control group households, which is the sample used for the qualitative analysis, differ quite sharply on some important observable characteristics. For example, 47 percent of the control group sample are high-school dropouts, but only 28 percent of the treated sample are (0 percent of the control group sample have a college degree, but 9 percent of the treated sample have one). The control sample has one more person per household (4.5) on average than the treated sample (3.4). These differences in observable characteristics raise a concern that the treatment and control group samples differ in unobservable ways that may be relevant to the hypothesis-generating spirit of the qualitative analysis.

It would be interesting to see two further extensions to the analysis. First, I think it would make more sense to do the matching analysis to balance the observable characteristics of the treated who did take the MTO offer and control group households. Put differently, since the authors choose to explore the treatment-on-the-treated parameter, their matching exercise could balance the observable characteristics between the treated sample who did take the MTO offer and control group households (rather than balancing the observable characteristics between the treated sample who did not take the MTO offer and control group households). Second, it would also be interesting to learn whether the qualitative conclusions from the treatment-on-the-treated sample differ in any important ways from the intent-to-treat samples. Given the general nature of the results of the qualitative analysis, my suspicion is that it is not necessary to restrict the sample to the treatment-on-the-treated subsample.

The qualitative results focus on three sets of factors—human capital shortcomings, social isolation, and spatial mismatch—that consistently are raised in the in-depth interviews. Because I do not do qualitative research, it seems that a fundamental challenge for the authors is to assess the relative importance of various themes or common factors that arise from the detailed interviews. The ethnographic work highlights many themes. Households have low levels of human capital and find this inhibits labor market success. There are treatment-control differences in the sectors in which people work, with members of the treatment group being much more likely to work in the health care sector. There are treatment-control differences in the nature of unemployment, with unemployed members of the treatment group expecting to have brief periods of unemployment, while unemployed control group members believe they will be out of the labor market for longer periods. Treatment group households appear to be less well-integrated into the communities in

which they live, and face greater transportation challenges than control group households. The challenge for the authors (and for readers of the paper) is to get some idea of the relative importance of these different experiences.

The experimental design and results of MTO impose constraints on the qualitative analysis that often are not present in other qualitative studies. Specifically, one knows there are no aggregate treatment-control employment effects, at least in the intent-to-treat parameters. So if the qualitative sub-sample is representative of the entire Baltimore MTO sample, there are two possibilities. The first is that none of the treatment-control differences raised in the qualitative analysis are important in understanding employment. The second is that the differences are important, but coincidentally they are offset, so that the aggregate effect is zero. Offsetting effects are possible—treatment households have shorter periods of unemployment and better neighbors, both of which should improve employment outcomes. At the same time, they have more transportation difficulties and disrupted social networks that might make it more difficult to find jobs. The net effect (or the “complex story” to which the authors refer) might be that these effects indeed are important, but offset one another. I am skeptical of this, however, since (to my knowledge) there were no significant treatment-control differences in adult employment outcomes (broadly defined) across any of the five MTO sites. It strikes me as being unlikely that the treatment-control differences unearthed by the qualitative interviews were important in understanding employment outcomes and exactly offset each other in each of the five MTO locations.

There is abundant high-quality statistical evidence from a variety of experimental and nonexperimental analyses that human capital substantially affects employment outcomes. Evidence on the other factors is, in my view, more difficult to interpret. Common sense suggests that transportation difficulties or lack of access to informal networks that are important in securing jobs would inhibit employment prospects. But understanding the empirical effects of these barriers on employment is very difficult. Unobserved characteristics correlated with having access to unreliable transportation or having less-than-ideal job networks likely bias efforts to understand the effects of transportation difficulties, job networks, or the effects of neighborhoods on economic outcomes. The ability to account rigorously for such unobservables is what makes the MTO experiment so valuable. An example is highlighted in Turney and others’ paper when they write, “living in high-poverty, economically depressed neighborhoods has a negative effect on one’s health.”⁷ But the MTO intervention

7. Ross and Mirowsky (2001); Boardman and others (2001).

finds no statistically significant treatment-control differences in physical health, calling into question the assertion about neighborhoods and health. I suspect that failure to account appropriately for selection explains the link between neighborhoods and health in many nonexperimental settings, though other factors, of course, may come into play.

I have similar suspicions about the role of what the authors (and the literature) call “weak” and “strong” social networks in finding jobs. Would employment outcomes differ appreciably for workers if their social networks change, holding constant their skills, experience, mobility, and all other relevant characteristics? My guess is that human capital considerations dominate employment relationships and that there are enough sources of information about jobs through newspapers, posted ads, the Internet, radio, television, jobs centers, as well as through word of mouth, that social networks in fact are less important than one might infer from talking to people. The selection problems in studying this in a nonexperimental setting are formidable. Factors that result in people’s having broad, rich social networks are presumably related to characteristics that are beneficial in the labor market. Hence it is possible that the emphasis placed on social networks in the qualitative study is misplaced.

I am puzzled by the first result highlighted in the final section of the paper, namely, that the authors “find important differences in the labor market attachment of those experimentals and controls who are currently unemployed.” It is hard for me to believe that this difference is a treatment effect. If the MTO intervention caused greater labor market attachment of unemployed treatment group members (relative to controls), we would presumably see some significant employment-related treatment effects across sites (such as in annual earnings). But we do not see these differences. The authors are careful in the conclusions to say that they “. . . do not intend to make causal claims of any kind.” But the beauty of a randomized social experiment is that one may be able to make well-grounded causal inferences from treatment-control differences. It seems, however, that the item highlighted first in the conclusions is unlikely to be a result of the MTO intervention. Hence it perhaps should get less emphasis from the authors.

A first-order question as one draws lessons from MTO is the degree to which the absence of beneficial employment effects is a result of the experiment’s being implemented during a period of sustained economic growth, low unemployment, and a sharp change in the administration and rhetoric associated with Aid to Families with Dependent Children and Temporary Assistance for Needy Families (TANF). As the authors note, employment rates of control group households increased sharply in the MTO sample, which may reduce,

to some extent, the likelihood of observing treatment-control differences. One might be able to learn more about this issue by examining whether there are cross-site differences in economic performance (and the work orientation of TANF programs) and then correlating differences in treatment effects with observed geographic differences.

In closing, the authors have produced a nicely written, well-reasoned paper. Employment issues facing low-skilled Americans are of first-order social, economic, and policy importance. This paper puts a spotlight on the role of neighborhoods and geography. Methodologically, it is very interesting to combine qualitative and quantitative approaches. The extensive interview work raises the possibility of learning more than what is revealed by regression coefficients from empirical models. The paper provides much to think about and I look forward to reading subsequent qualitative work with the MTO sample.

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