

Guest Editor's Introduction

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Residential segregation of America's neighborhoods by income has been increasing over the past 40 years, with nearly 9 million people now living in census tracts with poverty rates of 40 percent or more (Kneebone, Nadeau, and Berube, 2011; Watson, 2009). Because housing policy affects the geographic concentration of poverty in a variety of ways, policymakers have long been concerned about the possibility that living in a distressed neighborhood could have some harmful effects on the life outcomes of adults and children. The list of plausible reasons why neighborhood poverty might adversely affect people's well-being and behavior is long and includes limited exposure to peers and role models who support prosocial behaviors such as schooling and work; neighbors who are willing and able to cooperate and work together to improve community life; high-quality local public institutions such as schools, police, health care, and housing; and elevated exposure to risk factors like pollution or crime.¹

Empirically isolating the independent effects of neighborhood environments on the life outcomes of residents turns out to be quite challenging in practice, because most people have at least some degree of choice regarding where they live. A large body of research dating back to the 17th century shows that people who live in relatively more distressed neighborhoods tend to have worse life outcomes than do those people living in less disadvantaged areas, even after statistically adjusting for characteristics of the individuals and their families. What remains unclear is the degree to which these patterns reflect true *neighborhood effects*—that is, the causal influence of neighborhood environments on the life outcomes of residents—or instead reflect the influence of hard-to-measure characteristics of people that lead them to wind up living in different types of neighborhoods—or what social scientists call *selection bias*.

To overcome concerns with selection bias and help isolate neighborhood effects on low-income families, in the early 1990s the U.S. Department of Housing and Urban Development (HUD)

¹ For excellent reviews of the theoretical and empirical literatures on neighborhood effects, see Ellen and Turner (1997), Jencks and Mayer (1990), Kawachi and Berkman (2003), Leventhal and Brooks-Gunn (2000), and Sampson, Morenoff, and Gannon-Rowley (2002).

launched one of the most ambitious social experiments ever carried out by the agency—the Moving to Opportunity (MTO) for Fair Housing demonstration. Via random lottery, MTO offered some public housing families, but not others, the chance to use a housing voucher to move from high-poverty to lower poverty neighborhoods. Some of the families who were offered the opportunity to relocate received special vouchers that initially could be used to move into only very low-poverty areas, although, after 1 year, families could use the vouchers to move again (including to higher poverty places).

This issue of *Cityscape* focuses on the long-term followup that measured outcomes of MTO families 10 to 15 years after random assignment. This long-term followup study was carried out by a research team assembled by the National Bureau of Economic Research (NBER) and was supported by a contract with HUD and additional grants to NBER from other agencies and private foundations. In this guest editor's introduction, I provide some basic background about MTO that frames all of the articles that follow, and I offer some thoughts of my own about what lessons we might take from MTO for both social science and public policy.

In the second section, I briefly review the motivation for the MTO demonstration and the specifics of its design. A more detailed discussion of MTO's rationale and design is in the article in this symposium by Mark D. Shroder and Larry L. Orr. The symposium article by Jennifer Comey, Susan J. Popkin, and Kaitlin Franks shows that MTO was successful in helping families move into higher quality housing units. The article by Edgar O. Olsen in this symposium notes that the cost to taxpayers of providing higher quality housing units to MTO voucher holders might actually be zero or negative, in the sense that previous research suggests that the cost of providing a given level of housing quality might be lower with vouchers than public housing. Olsen notes, however, that there would be great value in exploiting the MTO platform to learn even more about these cost-effectiveness issues.

In the same section, I also show that MTO was successful in getting families to move initially into very low-poverty areas. One year after randomization, the difference in tract poverty rates between the control group and those who were offered housing vouchers to move into low-poverty areas was about 35 percentage points, or fully 2.8 standard deviations in the nationwide census tract poverty rate distribution. Previous housing mobility programs have found that families initially relocated into low-poverty areas tend to “stick” (Keels et al., 2005). An open empirical question is whether the same would be true for MTO families.

In the third section, I review the evidence showing that the very large initial differences in average neighborhood conditions between the two MTO treatment groups and the control group narrowed over time. This convergence is commonly attributed to the tendency of families who move with MTO vouchers to make additional moves back to higher poverty areas and has led to calls for the government to provide additional supports to voucher recipients to help them stay in low-poverty areas once they have moved there. I show that, somewhat surprisingly, most of the convergence over time between MTO treatment and control groups in neighborhood poverty rates is actually due to improvements over time in the neighborhoods of the control group.

In the fourth section, I consider the key question of whether MTO generates enough sustained variation in neighborhood conditions to provide a useful test of the “neighborhood effects”

hypothesis. When we look across the entire 10- to 15-year followup study period, moving with an MTO voucher reduces average census tract poverty rates by about 18 percentage points, equal to nearly one-half of the control group's average tract poverty rate of 40 percent. This is about as much variation in neighborhood poverty as we see in studies of African-American families in leading observational data sets like the Project on Human Development in Chicago Neighborhoods (PHDCN). MTO generates less pronounced differences across randomly assigned groups in racial segregation, although as Shroder and Orr discuss in their article, much of the discussion leading up to MTO was about neighborhood-effect theories that emphasized adverse effects from economic segregation more so than from racial segregation.

MTO also had large, sustained impacts on more subtle neighborhood attributes that are not readily measured with existing administrative data sources, such as social networks and neighborhood social processes and safety, and that require original in-person data collection from the MTO participants to measure. Because families were followed up over such a long time (10 to 15 years), and because low-income families tend to be very residentially mobile and hence difficult to track, no one would have been surprised if the long-term survey effort had wound up with a low response rate. As Nancy Gebler and her co-authors note in their article in this symposium, however, the team from the University of Michigan tasked with carrying out the surveys achieved remarkably high response rates to preserve the key strength of MTO's experimental design: 90 percent for adults and 89 percent for youth, which were very similar across randomly assigned MTO groups. Gebler et al.'s article includes some useful lessons for future researchers about how to track similar populations, and it presents some interesting results about what we would have found in the MTO data had we run out of time and money and been forced to stop the data collection at a lower response rate.

The articles in this symposium by Lisa Sanbonmatsu and co-authors and by Lisa A. Gennetian and co-authors summarize the mixed pattern of impacts that MTO had on the outcomes of adults and youth over the long term. In the fifth section of my introduction, I offer my own thoughts about what these results mean for social science hypotheses about neighborhood effects on adults and children. The MTO findings reject the hypothesis that "neighborhoods always matter," because we did not find detectable effects on schooling or labor market outcomes across the five demonstration sites in either the interim (4- to 7-year) or long-term (10- to 15-year) followup studies. Because MTO enrolled a very disadvantaged set of families living in severely distressed areas, these findings may not generalize to less disadvantaged samples and settings. These are exactly the sorts of disadvantaged families who have commanded (for good reason) a disproportionate share of media and policy attention, however, and there is little in the existing research literature that would have predicted that the most disadvantaged families should be less affected by their neighborhood environments than are other types of families. MTO teaches us that neighborhood effects are more contingent than we had thought.

The fact that MTO moves had impacts on several important outcome domains—physical and mental health—that are to my way of thinking quite large in size also lets us reject the overly sweeping conclusion that neighborhoods don't matter. What remains something of a puzzle is why neighborhood environments seem to matter much more for health than for other outcomes. I speculate about some answers to this question, which are motivated by some suggestive evidence that changes in neighborhood safety could be one of the key reasons behind MTO's impacts on health outcomes.

In the final section, I consider the implications of MTO for public policy. Many people have concluded that mobility programs that are more intensive than MTO in terms of achieving changes in neighborhood or school environments of families may be necessary to change those outcome domains like schooling and employment that were not affected by MTO. This is, for example, the spirit of the articles in this symposium by Philip Oreopoulos, by Margery Austin Turner, and by Kathryn Edin, Stefanie DeLuca, and Ann Owens. My own reading of MTO and other research suggests this need not be the case. I also consider what we might learn from MTO about the design of community-level interventions, with a focus on safety, given the role this might play in driving the MTO impacts on health and the importance of safety to the MTO families themselves.

The MTO Experiment

The MTO story begins in 1966 on the South Side of Chicago, actually not very far at all from my office at the University of Chicago. The first quasi-experimental evidence to support the idea that neighborhoods may exert large effects on poor families arose from a discrimination lawsuit against the Chicago Housing Authority (CHA) filed on behalf of an African-American public housing resident named Dorothy Gautreaux (Rubinowitz and Rosenbaum, 2000). As a result, starting in the 1970s, a total of 7,100 families were moved either into different parts of Chicago that were poor and segregated, but improving, or else into low-poverty, racially integrated suburbs (Keels et al., 2005).

A 1988 followup survey by Northwestern University sociologist James Rosenbaum found that moving to the suburbs instead of other parts of Chicago was associated with better job outcomes for mothers and schooling outcomes for children (Rosenbaum, 1995; Rubinowitz and Rosenbaum, 2000). Rosenbaum's findings were interesting and provocative, but left open the question of whether at least part of the difference in outcomes between Gautreaux suburban versus city movers might be due to other differences in the characteristics of the families themselves. Followup research has provided some support for this concern and has also tended to find smaller impacts on family outcomes (DeLuca et al., 2010; Mendenhall, Duncan, and DeLuca, 2006; Votruba and Kling, 2009).

The initial Gautreaux findings were nonetheless important enough to motivate HUD to sponsor the first true randomized experimental test of what happens to families when they move into very different neighborhood environments—the MTO demonstration. Eligibility for MTO was limited to low-income families with children living in selected distressed public housing or project-based housing in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. The housing projects from which MTO families came were among the most distressed in the country, with an average tract poverty rate of fully 53 percent. These projects were also extremely racially segregated. Almost all MTO participants from the Baltimore and Chicago sites are African American, whereas the other three sites are split about evenly between African-Americans and Hispanics. There were very few White families in these housing projects, and as a result there are very few Whites in the MTO study sample.

Between 1994 and 1998, MTO enrolled 4,604 families. Surveys collected at baseline (exhibit 1) show just how disadvantaged those families were when they signed up for the MTO program. The average annual household income was \$12,827 (in 2009 dollars). Fewer than two of five MTO household heads had a high school diploma, whereas three-quarters were on welfare.

Exhibit 1**Baseline Characteristics**

| | Experimental Group | Section 8 Group | Control Group |
|---|---------------------------|------------------------|----------------------|
| | N = 1,456 | N = 678 | N = 1,139 |
| Female | 0.988 [~] | 0.978 | 0.978 |
| Age as of December 31, 2007 | | | |
| ≤ 35 | 0.145 | 0.132 | 0.143 |
| 36–40 | 0.212 | 0.236 | 0.229 |
| 41–45 | 0.236 | 0.223 | 0.234 |
| 46–50 | 0.184 | 0.203 | 0.175 |
| > 50 | 0.223 | 0.207 | 0.219 |
| Race and ethnicity | | | |
| African American (any ethnicity) | 0.651 | 0.635 | 0.664 |
| Other non-White (any ethnicity) | 0.281 | 0.279 | 0.267 |
| White (any ethnicity) | 0.068 | 0.086 | 0.069 |
| Hispanic (any race) | 0.314 | 0.340 | 0.304 |
| Other demographic characteristics | | | |
| Never married | 0.623 | 0.624 | 0.637 |
| Parent before age 18 | 0.249 | 0.277 | 0.246 |
| Working | 0.271 | 0.269 | 0.245 |
| Enrolled in school | 0.161 | 0.174 | 0.167 |
| High school diploma | 0.381 | 0.347 | 0.361 |
| Certificate of GED | 0.159* | 0.183 | 0.199 |
| Receiving AFDC | 0.763 | 0.736 | 0.763 |
| Site | | | |
| Baltimore | 0.134 | 0.140 | 0.135 |
| Boston | 0.201 | 0.207 | 0.205 |
| Chicago | 0.205 | 0.209 | 0.205 |
| Los Angeles | 0.233 | 0.214 | 0.226 |
| New York | 0.227 | 0.231 | 0.229 |
| Neighborhood characteristics | | | |
| Household member was crime victim in last 6 months | 0.434 | 0.414 | 0.416 |
| Streets unsafe at night | 0.493 | 0.517 | 0.512 |
| Very dissatisfied with neighborhood | 0.478 | 0.477 | 0.467 |
| Lived in neighborhood 5 or more years | 0.599 | 0.616 | 0.606 |
| Very likely to tell neighbor about child getting into trouble | 0.556 | 0.521 | 0.555 |
| Primary or secondary reason for moving | | | |
| To get away from gangs and drugs | 0.786 | 0.749 | 0.779 |
| Better schools for children | 0.491 | 0.553* | 0.481 |
| To get a bigger or better apartment | 0.441 | 0.438 | 0.457 |
| To get a job | 0.063 | 0.050 | 0.069 |

AFDC = Aid to Families with Dependent Children. GED = general equivalency diploma.

* = $p < .05$. ~ = $p < .10$.

Notes: An omnibus F -test fails to reject the null hypothesis that the set of baseline characteristics reported above is the same across Moving to Opportunity sample random-assignment groups (p -value for the experimental-to-control group comparison is $p = .90$; p -value for the Section 8-to-control group comparison is $p = .69$). All values represent shares calculated using sample weights to account for changes in random assignment ratios across randomization cohorts and for subsample inter-viewing. The baseline head of household reported on the neighborhood characteristics listed here.

Source: Baseline surveys for adult respondents to the long-term survey

Perhaps the most striking result from exhibit 1 is that over 40 percent of MTO applicants had someone in the household victimized by a crime during the 6 months before the baseline survey. It is perhaps not surprising, then, that far and away the most important reason families signed up for MTO was safety. Three-quarters of MTO applicants said getting away from gangs and drugs was the first or second most important reason they signed up for MTO.

The families who volunteered for MTO were then randomly assigned them to one of three conditions.

The experimental group was offered the chance to use a housing rent-subsidy voucher² to move into private-market housing in lower poverty areas. As part of the MTO design, the vouchers offered to families in this group could only be redeemed in census tracts with a 1990 poverty rate under 10 percent. Families had to stay in these neighborhoods for 1 year. If they moved before the year was up, they would lose their voucher. After their initial 1-year lease was up, they could use their housing voucher to move again, including moves into a higher poverty area. Families in this group also received housing search assistance and relocation counseling from local nonprofit organizations.³

The Section 8 group was offered a traditional housing voucher to move into private-market housing, with no special MTO-imposed constraints on where they move. Families in this group also did not receive any special housing mobility counseling beyond what is normally provided to voucher holders.

The control group did not receive access to any new services through MTO, but did not lose access to any housing or other social services to which they would otherwise have been entitled.

Random assignment in MTO helps overcome the self-selection concerns with previous observational (nonexperimental) studies by creating groups of families who are comparable in all respects but differ in the housing and neighborhood conditions that they experience. As a result, any differences across groups in their average outcomes can be attributed to the MTO mobility intervention itself.

Not all of the families who were offered an MTO housing voucher used them. Around 47 percent of those families offered an experimental group voucher and 63 percent of those offered a Section 8 group voucher relocated through MTO. Although many people outside the housing policy research community have been surprised that these takeup rates are not higher, the voucher utilization rates observed in MTO are generally similar to what has been found in previous studies of other housing voucher programs (Olsen, 2003; Rubinowitz and Rosenbaum, 2000). One reason some families do not move is because they cannot find a unit that is affordable under the voucher program rules, within the time limit that the voucher program allows families to search for housing. The difficulty of finding affordable housing in the allowable time may have been particularly challenging for

² Housing vouchers provide families with a subsidy for their private-market rent, equal to the difference between the local area Fair Market Rent (set to equal between the 40th and 50th percentile of the local metropolitan area's rent distribution, depending on the city and year in question) and 30 percent of the family's adjusted income (see Jacob and Ludwig, 2012, and Olsen, 2003, for details). The family's required rent contribution is the same for public housing and housing vouchers and so receipt of a voucher does not free up any extra disposable income to families by enabling them to change their own out-of-pocket spending on rent.

³ The interim (Orr et al., 2003) and long-term (Sanbonmatsu et al., 2011) HUD technical reports summarizing the MTO results describe the three groups using the same terminology I use here: experimental, Section 8, and control groups. In some of our research team's other writings (for example, Ludwig et al., 2011), we used instead the more descriptive terms low-poverty voucher group, traditional voucher group, and control group.

families in the experimental group, who were restricted to looking in low-poverty census tracts. Some families in the experimental group did not relocate because they did not attend all of the life-skills counseling sessions that the local nonprofit organizations assisting with the housing search required them to complete before looking for housing. It is worth keeping in mind that many of the proposals to increase voucher take-up rates that have been suggested may create some difficult tradeoffs for policymakers.⁴

The fact that only some of the families who are offered MTO housing vouchers actually use them does not introduce any selection bias into our analyses (for additional discussion, see Ludwig et al., 2008). Families who are assigned to a voucher group who use a voucher are surely different from those who do not. The analyses presented in this *Cityscape* issue show two types of estimates: (1) the effect of being offered a housing voucher through MTO, known as the “intention to treat” (or ITT) effect and calculated as the difference in average outcomes of all families assigned to one of the treatment groups with all families assigned to control; or (2) the effect of actually moving with a housing voucher in MTO, known as the “effect of treatment on the treated” (or TOT), which is calculated using a method that preserves the strength of the MTO experimental design.⁵

It is also important to keep in mind when reading the MTO findings that the control condition in the MTO demonstration does not correspond to a situation of “no mobility.” Families in the MTO control group were allowed to move on their own, even if they did not receive any assistance through MTO to move. In addition, many of the public housing projects in which MTO families were living at baseline were demolished through HUD’s HOPE VI and other programs (see, for example, Katz, 2009), which further contributed to control group mobility.

Finally, we should be clear about what policy questions MTO can and cannot answer. MTO compares the effects of being offered a housing voucher with the chance to stay in public housing, which leads to sizable changes in neighborhood conditions (as I describe in the following section) but no change in out-of-pocket household spending on rent. This comparison helps answer the policy question of what would happen if we changed the mix of means-tested housing programs to include a larger share of housing vouchers and a smaller share of project-based units.

⁴ For example, one potential way to improve voucher take-up rates is to provide families with a longer window of time to search for units. This, however, creates the risk of reducing the share of vouchers that are being used by low-income families at any given point in time and increasing the share of voucher subsidies that are idle while families continue to search for housing. As an alternative, we could spend more money on housing-mobility counseling assistance for voucher recipients or efforts to encourage landlords to accept housing vouchers. Even if these efforts were successful in increasing voucher lease-up rates, spending more on these types of activities necessarily comes at the cost of diverting money that could have gone to providing actual housing subsidies to the two-thirds of income-eligible households in America who are not enrolled in means-tested housing programs (Olsen, 2003).

⁵ We do not try to estimate the effects of moving with an MTO voucher by doing something nonexperimental, such as comparing just the experimental group movers with the control group, because the families in the experimental group who move with a voucher are a self-selected subset of families assigned to that group—and so this self-selected subset cannot be compared with all the families assigned to the control group, which would be an apples-to-oranges comparison. Instead, we estimate the TOT in a way that exploits the experimental design of MTO, as follows. If we are willing to assume that being assigned to the experimental (or Section 8) group does not have much effect on families who do not use an MTO voucher to move, then the TOT effect will equal the ITT effect divided by the share of families assigned to the experimental (or Section 8) group who use an MTO voucher to relocate (H. Bloom, 1984). Because no control group families can use an MTO voucher by construction, the TOT estimate for some outcome of interest is basically the ratio of two ITT effects that are fully experimental—the ITT effect on the outcome divided by the ITT effect on use of an MTO voucher.

MTO does *not* tell us anything about the effects of giving housing vouchers to people who are already living in the private housing market but without any sort of government subsidy. For that population, voucher receipt leads to large gains in disposable income for families because they can now spend much less out-of-pocket on rent, but it generates relatively little change in neighborhood conditions (see Jacob and Ludwig, 2012; Mills et al., 2006). The comparison of vouchers with living in the private housing market without a subsidy is relevant for the policy question of what happens when we expand the share of families receiving means-tested subsidies, which is important in its own right given that less than one-third of income-eligible families are in means-tested housing programs (Olsen, 2003).

MTO's Effects on Neighborhood Conditions

The logic model behind MTO is that assignment to the experimental or Section 8 group leads families to change their living conditions, which in turn leads to changes in their behavior and well-being. For there to be any value at all in looking at MTO impacts on behavioral outcomes, we need to first establish that the MTO demonstration did actually change the environments in which families were living. So that the articles in this *Cityscape* issue do not have to repeatedly replicate this material, I summarize MTO's impacts on neighborhood conditions of participating families here. MTO also changed the housing conditions of families as well, which are carefully presented and discussed in the article in this symposium by Comey, Popkin, and Franks.

Exhibit 2 shows that, 1 year after the time of random assignment, even the ITT effects of MTO on neighborhood conditions were very large, despite the fact that many families who were offered MTO vouchers did not use them. The ITT estimates in exhibit 2 show that, 1 year after baseline, families assigned to the experimental or Section 8 group were living in census tracts with poverty rates that were 17 and 14 percentage points lower than the average census tract of the control group, which was 50 percent poor. (In what follows, I focus on the experimental-versus-control group contrast, which winds up providing the strongest test of “neighborhood effects,” although the contrast between the Section 8 and control groups is also of interest for what it can tell us about providing vouchers to public housing families and other key housing-policy questions about the right mix of housing program services.)

Over time, MTO's effect on neighborhood poverty rates diminishes. Exhibit 2 shows that the ITT effect on census tract poverty rates from being assigned to the experimental rather than control group was 10 percentage points measured 5 years after baseline, and about 5 percentage points measured 10 years after baseline.

What has not been widely appreciated is that most of this convergence in neighborhood conditions across randomized MTO groups is caused by improvements over time in the neighborhoods of control group families rather than by subsequent mobility (or “secondary moves”) by the experimental or Section 8 group families. The average census tract poverty rates for families assigned to the experimental group declined over the period from 1 to 10 years after baseline by 5 percentage points (from around 33 to 28 percent). The convergence in tract poverty rates between the experimental and control groups occurs because the control group experienced an even larger decline in tract poverty rates over this period, equal to fully 17 percentage points (from 50 to 33 percent).

Exhibit 2

MTO Effects on Housing and Neighborhood Conditions, 10 to 15 Years After Random Assignment (1 of 4)

| Outcome | Control Mean | Experimental vs. Control | | Section 8 vs. Control | | Respondents (N) |
|---|--------------|--------------------------|--------------------|-----------------------|--------------------|-----------------|
| | | ITT | TOT | ITT | TOT | |
| Number of moves after random assignment | 2.165 | 0.561* (0.073) | 1.157* (0.151) | 0.629* (0.096) | 1.014* (0.155) | 3,273 |
| Tract poverty at baseline | | | | | | |
| Share poor | 0.531 | -0.004 (0.005) | -0.009 (0.009) | -0.004 (0.006) | -0.007 (0.009) | 3,227 |
| Share poor, z-score on U.S. tracts | 3.172 | -0.034 (0.037) | -0.070 (0.076) | -0.034 (0.046) | -0.054 (0.074) | 3,227 |
| Share poor, z-score on MTO controls | 0.000 | -0.028 (0.031) | -0.058 (0.063) | -0.028 (0.038) | -0.045 (0.061) | 3,227 |
| Tract poverty 1 year after random assignment | | | | | | |
| Share poor | 0.499 | -0.170* (0.008) | -0.351* (0.016) | -0.140* (0.009) | -0.224* (0.014) | 3,224 |
| Share poor, z-score on U.S. tracts | 2.916 | -1.377* (0.062) | -2.847* (0.128) | -1.133* (0.072) | -1.818* (0.116) | 3,224 |
| Share poor, z-score on MTO controls | 0.000 | -1.047* (0.047) | -2.164* (0.097) | -0.861* (0.055) | -1.382* (0.088) | 3,224 |
| Tract poverty 5 years after random assignment | | | | | | |
| Share poor | 0.399 | -0.099* (0.007) | -0.203* (0.015) | -0.070* (0.009) | -0.114* (0.015) | 3,208 |
| Share poor, z-score on U.S. tracts | 2.109 | -0.803* (0.060) | -1.646* (0.124) | -0.571* (0.076) | -0.921* (0.123) | 3,208 |
| Share poor, z-score on MTO controls | 0.000 | -0.602* (0.045) | -1.234* (0.093) | -0.428* (0.057) | -0.690* (0.092) | 3,208 |
| Tract poverty 10 years after random assignment | | | | | | |
| Share poor | 0.329 | -0.045* (0.007) | -0.092* (0.014) | -0.040* (0.009) | -0.065* (0.014) | 3,196 |
| Share poor, z-score on U.S. tracts | 1.540 | -0.361* (0.056) | -0.745* (0.116) | -0.326* (0.071) | -0.528* (0.116) | 3,196 |
| Share poor, z-score on MTO controls | 0.000 | -0.276* (0.043) | -0.569* (0.089) | -0.249* (0.055) | -0.404* (0.088) | 3,196 |
| Duration-weighted census tract characteristics | | | | | | |
| Share poor | 0.396 | -0.089* (0.006) | -0.184* (0.012) | -0.069* (0.007) | -0.111* (0.011) | 3,270 |
| Share poor, z-score on U.S. tracts | 2.082 | -0.722* (0.046) | -1.487* (0.094) | -0.556* (0.055) | -0.897* (0.088) | 3,270 |
| Share poor, z-score on MTO controls | 0.000 | -0.708* (0.045) | -1.458* (0.093) | -0.546* (0.054) | -0.880* (0.086) | 3,270 |
| Share minority | 0.880 | -0.061* (0.007) | -0.125* (0.014) | -0.018* (0.009) | -0.029* (0.015) | 3,270 |
| Share minority, z-score on U.S. tracts | 1.798 | -0.195* (0.022) | -0.401* (0.045) | -0.057* (0.029) | -0.092* (0.047) | 3,270 |

Exhibit 2**MTO Effects on Housing and Neighborhood Conditions, 10 to 15 Years After Random Assignment (2 of 4)**

| Outcome | Control Mean | Experimental vs. Control | | Section 8 vs. Control | | Respondents (N) |
|---|--------------|--------------------------|----------------------|-----------------------|----------------------|-----------------|
| | | ITT | TOT | ITT | TOT | |
| Share minority, z-score on MTO controls | 0.000 | -0.375* (0.042) | -0.772* (0.086) | -0.110* (0.056) | -0.178* (0.091) | 3,270 |
| Concentrated disadvantage index | 1.389 | -0.238* (0.016) | -0.490* (0.032) | -0.189* (0.018) | -0.305* (0.029) | 3,270 |
| Concentrated disadvantage index, z-score on MTO controls | 0.000 | -0.644* (0.042) | -1.326* (0.087) | -0.511* (0.049) | -0.824* (0.080) | 3,270 |
| Share college graduates | 0.161 | 0.042* (0.004) | 0.087* (0.008) | 0.018* (0.005) | 0.029* (0.008) | 3,270 |
| Local area violent crime rate (per 100,000 residents) | | | | | | |
| Duration-weighted | 2,462.5 | -416.2* (43.3) | -860.4* (89.5) | -315.7* (51.9) | -508.8* (83.7) | 3,271 |
| At baseline | 4,040.0 | -70.8 (90.5) | -146.7 (187.5) | -30.4 (114.7) | -49.1 (185.2) | 3,215 |
| 1 year after random assignment | 3,547.2 | -994.4* (84.9) | -2,174.3* (185.7) | -717.8* (99.8) | -1,177.4* (163.7) | 3,111 |
| 5 years after random assignment | 2,478.5 | -428.8* (60.2) | -929.1* (130.4) | -257.9* (70.8) | -421.5* (115.8) | 3,112 |
| 10 years after random assignment | 1,671.5 | -152.2* (38.4) | -327.8* (82.6) | -49.3 (57.9) | -80.6 (94.7) | 3,053 |
| At beginning of interim fielding (December 31, 2001) | 2,635.5 | -537.9* (67.8) | -1,152.1* (145.2) | -351.9* (78.5) | -571.2* (127.5) | 3,105 |
| At beginning of long-term fielding (May 31, 2008) | 1,494.1 | -100.3* (35.8) | -216.3* (77.2) | -36.3 (51.5) | -59.8 (84.9) | 3,031 |
| At long-term evaluation | 1,447.0 | -88.8* (35.5) | -189.3* (75.7) | -90.7* (45.1) | -149.2* (74.3) | 3,019 |
| Local area property crime rate (per 100,000 residents) | | | | | | |
| Duration-weighted | 4,883.6 | -157.1 (103.1) | -324.8 (213.1) | -143.2 (108.1) | -230.8 (174.2) | 3,271 |
| At baseline | 6,898.8 | 196.2 (236.3) | 406.4 (489.5) | 83.5 (223.5) | 134.8 (360.9) | 3,215 |
| 1 year after random assignment | 6,234.0 | -544.7* (242.6) | -1,172.9* (522.4) | -355.4* (204.8) | -582.6* (335.8) | 3,148 |
| 5 years after random assignment | 5,121.0 | -183.6 (127.8) | -394.8 (274.8) | -57.8 (162.5) | -94.1 (264.5) | 3,134 |
| 10 years after random assignment | 3,823.8 | -37.8 (94.1) | -81.2 (202.2) | -8.4 (126.1) | -13.7 (205.4) | 3,088 |
| At beginning of interim fielding (December 31, 2001) | 5,198.4 | -229.8* (120.9) | -487.1* (256.2) | -191.0 (148.4) | -308.9 (240.0) | 3,130 |
| At beginning of long-term fielding (May 31, 2008) | 3,471.0 | 39.1 (74.4) | 83.9 (159.6) | 126.9 (116.5) | 208.0 (191.0) | 3,067 |
| At long-term evaluation | 3,370.7 | 0.3 (74.4) | 0.6 (158.0) | -63.9 (93.6) | -104.6 (153.1) | 3,058 |

Exhibit 2

MTO Effects on Housing and Neighborhood Conditions, 10 to 15 Years After Random Assignment (3 of 4)

| Outcome | Control Mean | Experimental vs. Control | | Section 8 vs. Control | | Respondents (N) |
|--|--------------|--------------------------|---------------------|-----------------------|---------------------|-----------------|
| | | ITT | TOT | ITT | TOT | |
| Housing self-reports and interviewer observations from long-term survey | | | | | | |
| Condition excellent | 0.213 | 0.003 (0.018) | 0.006 (0.037) | 0.030 (0.024) | 0.048 (0.038) | 3,267 |
| Condition excellent or good | 0.570 | 0.053* (0.021) | 0.109* (0.044) | 0.031 (0.029) | 0.050 (0.046) | 3,267 |
| Number of housing problems (0–7) | 2.051 | – 0.349* (0.081) | – 0.720* (0.166) | – 0.442* (0.104) | – 0.706* (0.166) | 3,267 |
| Number of housing problems, z-score | 0.000 | – 0.176* (0.041) | – 0.363* (0.084) | – 0.223* (0.052) | – 0.356* (0.084) | 3,267 |
| Interviewer rated building condition on respondent's block as fair | 0.836 | 0.021 (0.016) | 0.044 (0.032) | 0.036~ (0.020) | 0.057~ (0.033) | 3,199 |
| Interviewer rated building condition on respondent's block as well kept | 0.348 | 0.024 (0.021) | 0.049 (0.044) | 0.007 (0.028) | 0.011 (0.045) | 3,199 |
| Interviewer observation of neighborhood problems, raw value | 0.316 | – 0.018 (0.011) | – 0.037 (0.022) | – 0.004 (0.014) | – 0.007 (0.022) | 3,207 |
| Interviewer observation of neighborhood problems, z-score | 0.000 | – 0.070 (0.043) | – 0.146 (0.090) | – 0.018 (0.056) | – 0.028 (0.090) | 3,207 |
| Received any housing assistance | 0.620 | 0.026 (0.021) | 0.054 (0.043) | 0.045~ (0.027) | 0.072~ (0.044) | 3,273 |
| Total monthly housing cost | \$678.73 | 19.50 (23.30) | 39.66 (47.38) | – 6.26 (30.73) | – 10.00 (49.10) | 3,180 |
| Household is rent burdened | 0.676 | 0.011 (0.020) | 0.022 (0.041) | 0.020 (0.027) | 0.032 (0.043) | 3,169 |
| Household is severely rent burdened | 0.426 | – 0.004 (0.021) | – 0.007 (0.043) | 0.017 (0.029) | 0.027 (0.046) | 3,169 |
| Collective efficacy: very likely/likely to report | | | | | | |
| Kids spraying graffiti | 0.589 | 0.076* (0.021) | 0.156* (0.043) | 0.042 (0.028) | 0.067 (0.045) | 3,255 |
| Kids skipping school | 0.346 | 0.029 (0.021) | 0.059 (0.043) | 0.075* (0.028) | 0.119* (0.045) | 3,250 |
| Social networks | | | | | | |
| One or more friend with college degree | 0.532 | 0.071* (0.021) | 0.145* (0.044) | 0.007 (0.029) | 0.010 (0.046) | 3,203 |
| No close friends | 0.145 | – 0.018 (0.015) | – 0.038 (0.030) | 0.042~ (0.022) | 0.066~ (0.034) | 3,265 |
| Medical care | | | | | | |
| Place to go for routine care (not ER) | 0.935 | – 0.012 (0.011) | – 0.024 (0.022) | 0.010 (0.012) | 0.015 (0.020) | 3,264 |

Exhibit 2

MTO Effects on Housing and Neighborhood Conditions, 10 to 15 Years After Random Assignment (4 of 4)

| Outcome | Control Mean | Experimental vs. Control | | Section 8 vs. Control | | Respondents (N) |
|--|--------------|--------------------------|--------------------|-----------------------|--------------------|-----------------|
| | | ITT | TOT | ITT | TOT | |
| Safety | | | | | | |
| Feel unsafe during day, z-score | 0.000 | -0.090* (0.041) | -0.186* (0.085) | -0.113* (0.054) | -0.181* (0.086) | 3,262 |
| Feel unsafe during day | 0.196 | -0.036* (0.016) | -0.074* (0.034) | -0.045* (0.021) | -0.072* (0.034) | 3,262 |
| Feel unsafe during night, z-score | 0.000 | -0.087* (0.042) | -0.179* (0.087) | -0.149* (0.056) | -0.238* (0.088) | 3,246 |
| Feel unsafe during night | 0.404 | -0.043* (0.021) | -0.088* (0.043) | -0.073* (0.027) | -0.117* (0.043) | 3,246 |
| Police don't respond | 0.420 | -0.067* (0.021) | -0.138* (0.044) | -0.075* (0.028) | -0.118* (0.045) | 3,146 |
| Saw drugs used/sold, last 30 days | 0.310 | -0.062* (0.019) | -0.128* (0.039) | -0.057* (0.025) | -0.090* (0.040) | 3,249 |
| Household crime victimization (last 6 months) | | | | | | |
| Any crime | 0.184 | -0.022 (0.016) | -0.046 (0.033) | 0.025 (0.022) | 0.040 (0.035) | 3,241 |
| Assault | 0.074 | -0.014 (0.011) | -0.030 (0.022) | -0.005 (0.014) | -0.008 (0.023) | 3,238 |
| Break-in/attempted break-in | 0.043 | -0.003 (0.009) | -0.007 (0.019) | 0.015 (0.013) | 0.025 (0.021) | 3,242 |
| Snatched purse/wallet/jewelry | 0.077 | -0.005 (0.011) | -0.010 (0.023) | 0.008 (0.014) | 0.012 (0.023) | 3,234 |
| Stabbing/shooting | 0.029 | -0.006 (0.007) | -0.013 (0.015) | -0.008 (0.008) | -0.013 (0.013) | 3,241 |
| Threatened with knife/gun | 0.066 | -0.008 (0.010) | -0.016 (0.021) | 0.001 (0.013) | 0.001 (0.021) | 3,236 |

ER = emergency room. ITT = Intention to treat. MTO = Moving to Opportunity. TOT = Treatment on the treated.

* = $p < .05$. ~ = $p < .10$.

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates and field release, weighted, and clustering on family. Housing problems include peeling paint, broken plumbing, rats, roaches, broken locks, broken windows, and broken heating system. Interviewer-observed neighborhood problems include abandoned buildings, cigarette or cigar butts on the sidewalk or in the gutter, "For Sale" signs, metal bars on windows above the basement level, fair or poor street conditions, and moderate to heavy amount of litter on the streets. Households are defined as rent burdened if their monthly housing costs are greater than or equal to 30 percent of their monthly household income. A household is severely rent burdened if monthly housing costs are greater than or equal to 50 percent of their monthly household income. The concentrated disadvantage index is a weighted combination of census tract percent poverty, on welfare, unemployed, female-headed family households, and younger than 18, with loading factors developed using 2000 census tracts in Chicago by Sampson, Sharkey, and Raudenbush (2008), but it does not include percent African American.

Source: Adult long-term survey

Regardless of the cause, it is clearly true that the neighborhood conditions of the experimental and control groups became more similar over time. Rather than look at MTO's impacts on tract poverty rates at a particular point in time, we can also average over the entire followup study period. Looking at MTO's effects on average neighborhood conditions that families experience over the entire followup study period also fits with the common view that behavioral change may require accumulated exposure to neighborhood environments (see, for example, Clampet-Lundquist and Massey, 2008). Exhibit 2 presents results that average the neighborhood conditions over all of the different addresses families report during the study period, giving more weight to those addresses at which people spent relatively more time. Over the course of the study period, the average control group family lived in a census tract that was 40 percent poor, compared with an average tract poverty rate for families assigned to the experimental group equal to 31 percent, for an ITT effect of 9 percentage points.

I have intentionally focused so far on the ITT effects of MTO on neighborhood environments to make it easier to see how much the changes over time in the control group neighborhoods are contributing to the convergence in neighborhood conditions between the experimental and control groups. As mentioned previously, however, it is also possible to calculate the effects of MTO on the neighborhood conditions of those who actually move through the program, or the TOT effects. Exhibit 2 shows the TOT effect on duration-weighted tract poverty rates was fully 18 percentage points, nearly one-half of the control group's average tract poverty rate over the study period of 40 percent.

Exhibit 2 also shows that MTO had large impacts on an index of neighborhood disadvantage that Sampson, Sharkey, and Raudenbush (2008) argue may provide a better measure of the extent of neighborhood conditions compared with just looking at poverty alone. The index is a weighted average of census tract share poor, unemployed, share of households headed by a female, share receiving welfare, and share of the tract population that is under age 18.⁶ The logic behind this index is that some neighborhoods are considered low income because they are composed of two-parent families who are mostly working but have low earnings, whereas other neighborhoods are considered poor because they have a large share of single-parent households that are disconnected from the formal labor force. These two types of neighborhoods may have similar poverty rates but the social conditions in these two types of places will be quite different, which will be reflected in different values of the concentrated disadvantage index. Exhibit 2 shows that the average duration-weighted tract disadvantage level of the control group in MTO over our study period was about 1.39. Those who move with an experimental group voucher experience a decline of 0.49 on this index, equal to around 35 percent of the control mean.

Although MTO focused explicitly on reducing economic rather than racial segregation for participating families, one might have expected there to be important changes in neighborhood racial segregation as a byproduct of the MTO moves, given that residents of high-poverty neighborhoods are very disproportionately likely to be Hispanic or African American (Jargowsky, 2003, 1997).

⁶ Whereas Sampson, Sharkey, and Raudenbush (2008) calculated the index using share African American as an additional component, we discuss MTO impacts on tract minority share separately and so do not include that variable in our own calculation of the index. The weights we use in exhibit 2 are based on a principal components analysis that Sampson, Sharkey, and Raudenbush (2008) calculated using tract-level data for Chicago from the 2000 decennial census and equal .90 for tract share receiving welfare, .88 for tract share poor, .86 for tract share unemployed, .87 for tract share households headed by female, and .73 for tract share under age 18.

Exhibit 2 makes clear, however, that MTO's impacts on racial segregation for participants were fairly modest. The average control group family spent the study period in a census tract that was 88 percent minority. The tract share minority for those who moved with an experimental voucher was lower by a statistically significant amount, but the TOT effect of about 13 percent means that, over the study period, even the experimental group movers were living in census tracts in which fully three-quarters of all residents were members of racial and ethnic minority groups.

Despite the lack of major MTO impact on neighborhood racial composition, MTO moves led to sizable changes in neighborhood social processes that a growing body of sociological research suggests might be particularly important in affecting people's life outcomes (Sampson, Morenoff, and Gannon-Rowley, 2002; Sampson, 2012). Note that exhibit 2 focuses on the self-reports of MTO adults about their social networks and neighborhood social processes measured 10 to 12 years after random assignment—that is, after the convergence in neighborhood poverty rates between the two treatment groups and the control group has occurred.

Exhibit 2 shows that, 10 to 12 years after baseline, the experimental group TOT effect on the likelihood of having at least one college-educated friend was nearly 15 percentage points, or about one-third of the control mean of 53 percent. The experimental TOT effect on the likelihood that neighbors would do something if local youth were spraying graffiti (intended to measure what Sampson, Raudenbush, and Earls, 1997, call “collective efficacy”) was over 15 percentage points, about one-quarter of the control group's value of 59 percent.

MTO also delivered in terms of changing the neighborhood condition that was the main reason most MTO families signed up for the program originally—safety. Moving with an experimental group voucher reduces the local violent-crime rate (as measured by police data) by 876 violent crimes per 100,000 residents, equal to more than one-third the control group average of 2,420 violent crimes per 100,000.⁷ Self-reported data about neighborhood safety from MTO participants show similarly large effects. The experimental TOT effect on the likelihood that adults report feeling unsafe in their neighborhood during the day equals 7 percentage points, over one-third of the control group's rate of 20 percent, and reduces the likelihood of having seen drugs used or sold in the neighborhood over the past month by 13 percentage points, over two-fifths of the control group value of 31 percent.

What Can MTO Tell Us About Neighborhood Effects?

If it had turned out that there were few differences in average neighborhood conditions between the two treatment groups and the control group in MTO, then the MTO demonstration will not have much useful to say about the existence of any “neighborhood effects” on families. In the previous section, however, we showed that MTO moves generate changes in neighborhood disadvantage and social processes that are, during the period initially after random assignment, extremely large.

⁷ These administrative records might understate MTO's effects on safety, because the geographic resolution of the local area crime data we can get from police departments varies greatly across cities and is quite large in some places. Moreover, only about one-half of all violent crimes nationwide are reported to police (Truman and Rand, 2011), and we might worry that reporting rates are even lower in distressed areas where people tend to distrust the police.

These effects are still sizable when averaged over the entire study period, viewed in either absolute terms or as a share of the control group's average neighborhood attributes. Why, then, do many people argue that MTO is a "weak treatment" that is of limited value for answering the social science question of whether and how neighborhood environments affect behavior?

One concern that I think is legitimate is that some potentially important neighborhood attributes were not changed very much by MTO, and in particular neighborhood racial composition. It is worth reiterating that many of the leading theories about why neighborhood environments might affect the well-being of residents focus on neighborhood attributes other than racial composition. For example, the seminal work of Wilson (1987), which helped stimulate the sizable neighborhood-effects research literature that has developed over the past 25 years, focused on the consequences for low-income African Americans from having middle-class African Americans move out to other areas. Wilson's hypothesis is about the importance of neighborhood socioeconomic disadvantage, not racial segregation.

Some people have expressed the view that MTO is a weak treatment even with respect to the sorts of socioeconomic measures that I have argued in the previous section were strongly affected. Why is that? One reason is a frequent tendency to focus exclusively on the ITT effect on neighborhood conditions, even though the TOT effect can also be identified from the experimental data so long as we are willing to assume that assignment to one of the two voucher groups has little effect on those families who do not actually move with a voucher. Both types of estimates are of interest. ITT estimates are relevant for public policy because most housing-mobility programs in the real world would be voluntary, and so compliance will inevitably be less than perfect. The TOT estimates are of interest because they help extrapolate MTO results to other mobility interventions that might have different voucher compliance rates, and they are of scientific interest because relative to ITT estimates, the TOT more directly identifies the effects of changing neighborhood contexts on people's outcomes.

A second reason MTO can look like a weak treatment is if one focuses on how far families change their rank in the national census tract poverty distribution. For example, Quigley and Raphael (2008) note that the low-poverty voucher ITT effect moves families from the 96th percentile to the 88th percentile within the census tract poverty rate distribution for the five MTO cities. As a share of all census tracts in the United States as a whole, however, there are just not all that many census tracts that have very high poverty rates. This means that large absolute changes in tract poverty rates can lead to relatively small changes in rank order at the top end of the distribution.

A different way to think about how MTO changes people's neighborhood "quality" within the larger neighborhood-quality distribution is to measure MTO's impacts in standard deviation (sd) units. This metric essentially compares the size of the MTO impacts on census tract poverty rates with the amount of "spread" in the larger census tract poverty rate distribution. Exhibit 2 shows that, 1 year after random assignment, the experimental group ITT effect is about -1.4 sd within the national tract distribution as measured in the 2000 decennial census data, whereas the TOT effect is equal to fully -2.8 sd.⁸ The experimental group effects on duration-weighted average tract

⁸ Exhibit 2 also shows results that standardize MTO's impacts on tract poverty rates using the standard deviation of the control group's tract poverty distribution, rather than the national tract poverty distribution found in the 2000 census.

poverty rates averaged over the entire study period equal about -0.7 sd (ITT) and -1.5 sd (TOT). It is difficult to think of many social experiments that generate such large changes in important aspects of the living conditions of poor families.

MTO can also look like a weak treatment if analysts divide neighborhoods up into a small number of discrete and essentially arbitrary “types,” which has the effect of throwing away information and making it harder to see how neighborhood conditions differ across randomly assigned groups. For example, Clampet-Lundquist and Massey (2008) create four neighborhood categories by dividing them on two separate dimensions: “poor” versus “nonpoor” (whether the tract’s poverty rate is above or below 20 percent); and “segregated” versus “integrated” (whether the tract’s minority share is above or below 30 percent). Similarly, Turner et al. (2011) use threshold values of tract characteristics to define various categories of “high-opportunity” neighborhoods, such as those with “high work and income” (tract poverty rates below 15 percent and employment rates above 60 percent) or “high education” (20 percent or more of adults have a college degree). They conclude: “Although MTO enabled families to escape from the most severely distressed neighborhoods, very few actually gained and sustained access to high-opportunity neighborhoods” (Turner et al., 2011: 7).

Defining “low-poverty” or “high-opportunity” neighborhoods on the basis of whether tract characteristics are above some threshold value makes sense if and only if we believe that neighborhoods only influence behavior once they reach some “quality” threshold. Put differently, dividing neighborhoods into a small number of categories is sensible only if neighborhood effects on outcomes are nonlinear, so that (say) moving from a tract that has a 50-percent poverty rate to one with a 16-percent poverty rate has no effect on people’s outcomes (both of those neighborhood types would be “poor” under the Turner et al. definition), but moving from a neighborhood with a 16-percent poverty rate to a 15-percent poverty rate would have important impacts on outcomes (this would be a move from a “poor” to “nonpoor” area in a Turner et al.-type definition).

The evidence presented in Kling, Liebman, and Katz (2007), however, seems to suggest that a 1-percentage-point change in tract poverty rates has the same effect on people’s life outcomes regardless of whether we are going from 16 to 15 percent poor, or 26 to 25 percent, or 36 to 35 percent, and so on. If neighborhood effects on people’s outcomes are linear, as the findings by Kling, Liebman, and Katz seem to suggest, then dividing up neighborhoods into a small number of categories winds up masking some of MTO’s impacts on the neighborhood conditions of participating families, by treating all neighborhoods with poverty rates above some threshold value as indistinguishable members of the same type of place (in Turner et al.’s typology, going from 50 percent to 16 percent poor leaves one within the same neighborhood “type”). If neighborhood effects on outcomes are linear, then the most appropriate way to measure MTO impacts on neighborhoods is by reporting the impact on percentage point changes in the tract characteristics themselves—that is, looking at continuous measures.

So is MTO too much of a “weak treatment” to be useful for social science purposes? Is there enough difference in average neighborhood conditions between the two treatment groups and the control group to let us learn something about neighborhood effects? One benchmark we might use is to compare the amount of variation we see in neighborhood conditions in the MTO data with that captured by what is arguably the most important observational (nonexperimental) study of neighborhood effects ever carried out, the PHDCN. Sampson, Sharkey, and Raudenbush (2008)

used the PHDCN to examine effects on verbal ability of African-American children from living in a census tract in the bottom quartile of Chicago's distribution with respect to concentrated tract disadvantage (defined previously), or the "treatment group" in their study, compared with all other African-Americans in their study, the "controls." The treatment group in their study lived in tracts that were 38 percent poor compared with control tracts that were 20 percent poor on average, for a difference of 18 percentage points—almost identical to what we see in MTO.⁹

What Do the MTO Results Mean for Social Science?

Twenty-five years ago, Wilson (1987) argued that a key reason why people living in high-poverty central-city neighborhoods tended to drop out of school or be out of the labor market was because of the harmful effects of the neighborhood environments in which they were living. The MTO data do not seem to support that hypothesis, at least for the sort of low-income, disadvantaged family that signed up for MTO.

This raises the question of whether families as disadvantaged as those enrolling in MTO could have been expected to experience improved schooling and labor market outcomes from moving to less distressed areas. Presumably, the U.S. Congress and HUD thought so, because schooling and earnings were key outcomes mentioned as a focus of the demonstration. Previous observational studies like PHDCN have reported finding neighborhood effects on schooling outcomes for people about as disadvantaged as those in MTO.¹⁰ And the sorts of very disadvantaged families who live in our nation's most distressed public housing projects have, for understandable reasons, commanded a disproportionate share of the media and policy attention. Although the MTO results might not generalize to families with higher levels of socioeconomic status, knowing whether neighborhoods exert causal effects on key outcomes like schooling and work for very disadvantaged families is important in its own right for social science and public policy.

Some people have concluded that MTO could have had bigger impacts on schooling outcomes if only the experimental group moves generated larger changes in the characteristics of the schools that children attended (see also the articles in this symposium by Turner and Oreopoulos). Maybe. Previous studies suggest that attending a higher quality urban school (public or charter) tends to

⁹ If we look instead at Sampson, Sharkey, and Raudenbush's (2008) concentrated disadvantage index, defined without share African American included in the index, the treatment group in their study has an average value of 1.71 and controls have a value of 1.04, for a difference of 0.67. As shown in MTO, the control mean is 1.39 and the average value for those who move with an experimental group voucher is 0.90, for a difference of 0.49.

¹⁰ For example Sampson, Sharkey, and Raudenbush (2008) report statistically significant neighborhood effects on verbal test scores among African-American children in Chicago who were in the PHDCN study. As reported in the previous footnote, the average value of the concentrated disadvantage index for their high-poverty ("treatment") group was 1.71 compared with an average value for the MTO high-poverty group (which we happen to call our "control group," instead) was 1.31—or, put differently, their study sample is living in neighborhoods that are, on average, even more distressed than those of the average MTO family. Supplemental Table 6 for their paper reports on the mean values of their baseline covariates among all African Americans in their study sample. Their study children are living in overwhelmingly (92 percent) female-headed households, just as in MTO. A lower share of their PHDCN study sample is receiving welfare at baseline than in MTO (49 versus about 75 percent), but it is important to note that the baseline covariates they present are averaged across the entire set of African-American families in the PHDCN. If they reported baseline covariate values just for the families living in highly distressed neighborhoods, their baseline covariates would surely be even closer to what we see in MTO (<http://www.pnas.org/content/suppl/2007/12/12/0710189104.DC1/10189Table6.pdf>).

have beneficial impacts on behavioral outcomes like schooling persistence or delinquency. This is not as consistently true with respect to achievement test scores, which have, for better or worse, been an outcome of particular interest in policy discussions and for which previous studies tend to find more mixed impacts (Abdulkadiroglu et al., 2011; Angrist et al., 2010; Angrist, Pathak, and Walters, 2011; Cullen, Jacob, and Levitt, 2006; Deming, forthcoming; Hastings, Kane, and Staiger, 2006). How do we make sense of the fact that gaining access to a better school does not always lead to higher achievement test scores for all students?

One candidate explanation is that not all children experience a given school environment the same way. As my University of Chicago colleague Stephen Raudenbush once said to me: “Dealing with heterogeneity across students in their academic needs is *the* challenge of education.” What a child gets out of attending a given school might all too often depend on where he or she falls within the school’s test-score distribution. Anyone who has ever taught will be familiar with the idea that teachers tend to target instruction towards the middle of a classroom’s achievement distribution. Some previous studies suggest teachers might even devote disproportionate attention to those students at the top of the distribution (B. Bloom, 1984). Children who are already far behind in school might not benefit much from attending a better school if that means that they experience a lot of instruction pitched above their heads. Common components to many successful educational interventions include frequent assessments to gauge what students are learning, targeted instruction through tutoring or small-group settings, and extra time for this sort of instruction—something that regrettably few disadvantaged children seem to receive regardless of where they live and go to school.¹¹

Just as MTO lets us rule out the strong claim that neighborhoods always matter, I spend a lot of my time talking to economists who tell me that, to them, the lesson from MTO is that neighborhood environments are just not that important for poor families. The fact that MTO moves generated changes in some important outcome domains, particularly mental and physical health, means that we can reject that view as well.

What is particularly remarkable about the MTO health impacts is how massive they are. As Sanbonmatsu et al. note in their article, moving with an MTO experimental group voucher reduced rates of extreme obesity (Body Mass Index ≥ 40) and diabetes (HbA1c ≥ 6.5 percent) by around 40 percent expressed as a share of the control group’s prevalence rate. Although clinical trials in medicine rarely enroll study samples quite as economically disadvantaged as that in MTO, it is still quite striking that the MTO impact on diabetes is about as large as what we see from best-practice pharmaceutical treatment and public health lifestyle interventions. Similarly, Kling, Liebman, and Katz (2007) noted that MTO’s impacts on mental health outcomes in the interim (4- to 7-year) followup were about the same size as what we see from best-practice drug treatment.¹²

¹¹ For example, Success for All, a comprehensive reading intervention that involves extra time for reading, ability grouping, frequent assessment, and remediation (including tutoring), has been found to improve reading scores for elementary school children and perhaps middle schoolers as well (Borman et al., 2007; Chamberlain et al., 2007). Angrist, Pathak, and Walters (2011) noted that the more effective urban charter schools they studied in the Boston area tended to be those adopting the “No Excuses” approach of the Knowledge is Power Program (KIPP) schools, which emphasize extra math and reading instruction time. Angrist et al. (2010) showed that those students who benefit the most from attending a KIPP school are those with low baseline test scores, with limited English proficiency, or in special education programs.

¹² Note that, although we might have expected improved mental health among MTO adults to translate into improved children’s schooling and other outcomes, the size of the impact on children that we would expect from improved adult mental health would not be detectable in the MTO data.

A more difficult question to answer is *why* MTO had such pronounced impacts on health. Experiments in general tend not to be so well suited to answering *why* questions. In MTO, the problem is further compounded by the fact that the treatment (MTO moves) wound up changing a very large number of housing and neighborhood characteristics for participating families, as exhibit 2 makes clear, which complicates any attempt to figure out what is responsible for the observed differences in average health outcomes between the two treatment groups and the control group (or the lack of observed differences in other outcome domains). Therefore, trying to figure out why MTO affected health more than other outcomes will necessarily involve some speculation.

With that qualifier in mind, one hypothesis is that the MTO health impacts may be due in part—perhaps even in large part—to changes in neighborhood safety. It is easy to see why MTO's impacts on mental health outcomes like the K6 index of psychological distress might be due to improvements in neighborhood safety. An important role for safety in explaining MTO impacts on mental health would also be consistent with the fact that three-quarters of MTO adults said safety was one of the most important reasons they signed up to move as part of the program.

Safety might also be an important contributing factor to the large impacts of MTO on physical health measures like extreme obesity and diabetes, through the effects of safety on stress that previous research has in turn linked to sleep and metabolism. One reason to suspect this safety-stress-health link in MTO is by process of elimination: we do not see large, consistent MTO impacts on other candidate mechanisms around diet, exercise, and access to medical care, though it should be said that our measures of these mechanisms are not as detailed as one might ideally wish. It is worth keeping in mind, however, that the effects of moving through MTO on diabetes and extreme obesity are extremely large. It would be surprising if diet, exercise, or access to medical care could change by enough to explain entirely such significant MTO effects on diabetes and obesity yet give such little indication of change in the mediating measures included on our surveys.¹³

If safety is an important mechanism behind MTO's health impacts, then why do we not also see MTO impacts on other outcomes like schooling? After all, Sharkey (2010) finds some evidence in the PHDCN data for very large (.50 to .66 standard deviation) short-term effects of neighborhood homicide rates on children's achievement test scores. Perhaps the contrast between the PHDCN

¹³ An alternative approach to understanding more about the mediating pathways through which MTO affects these health outcomes is that of Kling, Liebman, and Katz (2007): to use interactions of indicators for treatment group and demonstration sites as instrumental variables for different neighborhood characteristics as endogenous explanatory variables. This method basically estimates a dose-response relationship and asks whether, in those demonstration sites where randomization to a given treatment group generates a relatively larger change in a candidate mediating measure, randomization to that treatment arm also generates relatively larger changes in the outcome variable of interest. The method assumes that the only reason why different randomized groups in different sites respond differently to treatment group assignment is because they experience a relatively larger change in the mediating measure, not because of some other systematic variation across sites and groups in how people would respond to a given unit change in some neighborhood characteristic or other mediator. The method also assumes that the only pathway through which treatment assignment affects the outcome of interest is through the endogenous explanatory variables (the candidate mediators) included in the model. Given the large number of candidate mediators through which MTO might affect outcomes and the limited number of instrumental variables available with this design, this assumption will be challenging to meet. We can, however, interpret candidate mediators used in this way as markers or proxies for the collection of neighborhood attributes that covary (for example, Kling, Liebman and Katz, 2007, interpreted census tract poverty rates as a marker for a collection of features of neighborhood economic disadvantage that are correlated). Using this approach to explore the mechanisms behind MTO's health impacts should be a priority for future work.

and MTO data could reflect in part the difference between the short-term and long-term effects of exposure to neighborhood crime and violence. Over the longer term, parents may engage in a variety of protective behaviors that try to shield their children from the harmful effects of dangerous neighborhoods, although, in principle, adaptations like this could wind up generating costs in other ways. When we examine the data in ways that extend beyond MTO's pure experimental design, we see some hints that schooling outcomes for female youth could actually be better in more unsafe neighborhoods. One imagines children being kept inside more often in dangerous areas and so having more time to do homework, but that is just speculation. Understanding more about the safety-schooling link should be an important priority for future research.

Implications of MTO for Public Policy

One way to read the MTO demonstration is as an evaluation of a program (voucher-assisted residential mobility) that policymakers might consider carrying out at scale. One thing we have learned from MTO is that this sort of mobility program can have surprisingly large, beneficial impacts on important mental and physical health outcomes. Whether these benefits from MTO are large enough to justify the costs of the mobility program is difficult to determine with the available data. As Olsen notes in his article in this symposium, the costs to government housing agencies of an MTO-like switch from public housing to housing vouchers is likely to be negative—that is, to save money. Some of the most important potential costs of MTO are unlikely to show up on any government budget spreadsheet, however. The whole logic behind MTO—that being surrounded by relatively more affluent neighbors could be good for the life outcomes of low-income families—raises the possibility that MTO moves could have adverse effects on other families outside of the MTO demonstration who are living in destination areas or the origin neighborhoods that MTO families left.

In principle, it could be that mobility programs like MTO are just a zero-sum game, with whatever benefits arise to MTO families from living in a lower poverty area being exactly offset by adverse impacts on other families in destination areas from experiencing an increase in the poverty rate of their neighborhood. If every family responds the same way to living in a neighborhood of a given type, and if the relationship between people's outcomes and neighborhood poverty or other characteristics are linear (so that a 1-percentage-point change in tract poverty or some other neighborhood attribute always has the same effect on people's outcomes, regardless of whether we are moving from 50 to 49 percent poor or from 16 to 15 percent) then mobility programs like MTO will change the geographic distribution of social problems, but not their overall rates in society. MTO is great for studying the effects of MTO moves on the movers, but it is not well suited to learning anything about these larger societywide effects.

Even if the health benefits from MTO were sufficient to justify the program's costs, there is still the question of what else we need to do in order to improve those outcome domains that were not affected in MTO, particularly schooling and labor market outcomes. A common reaction to MTO is to conclude that because MTO-like moves did not generate detectably large gains in schooling and labor market outcomes, then more intensive mobility interventions are needed. It is not obvious, however, that such mobility programs will necessarily have the effects on schooling and labor market outcomes that proponents hope for, or that such policies are even feasible at large scale.

One reason I am not sure that more intensive mobility programs will necessarily generate big schooling or labor market gains comes from previous quasi-experimental analyses that have tried to learn more about mechanisms. These results suggest that MTO participants who experience relatively larger changes in neighborhood poverty or related characteristics have larger improvements in physical or mental health outcomes (Ludwig et al., 2011). In the interim MTO data, however, Kling, Liebman, and Katz (2007) did not see the same “dose-response” relationship for schooling or labor market outcomes, which means that a larger neighborhood “dose” need not lead to larger changes in education or work outcomes. One qualification here is that there is one particular type of move—namely, to affluent, mostly White suburbs—are not very well represented in the MTO data. Although MTO itself does not have much to say about those sorts of moves, followup Gautreaux research using longitudinal administrative records has not found large beneficial effects from moving to the suburbs (DeLuca et al., 2010).

A different sort of question is whether mobility programs that achieve even more socioeconomic or racial integration than did MTO are feasible at large scale. The standard concern has to do with political feasibility, given some of the political opposition that arose to MTO itself (Goering, 2003). I do not claim to have any special insight on this question of political feasibility, although it is perhaps worth noting that the few programs that I know of to have moved poor urban families to affluent suburbs (Gautreaux in Chicago, Thompson in Baltimore) were enacted by judges rather than elected politicians.

There is another important constraint on our ability to achieve even greater levels of economic integration than what we saw in MTO, which is the sheer amount of poverty itself that we have in the United States. A common measure of residential segregation is the “dissimilarity index,” which is defined as the share of people who would need to be moved across census tracts within a given area in order to have the share of poor people in each tract equal the share of the larger area that is poor. The five MTO demonstration cities have poverty rates right now in the ballpark of 20 percent.¹⁴ The average tract poverty rate of MTO experimental group movers (about 21 percent) corresponds basically to the benchmark of perfect poverty integration in these MTO cities. Even if we implemented a residential mobility program that would move inner-city families all over the country, the poverty rate in the United States as a whole right now is 15 percent.¹⁵ There is just not that much room to achieve more economic integration at large scale when the overall poverty rate is on the order of 15 to 20 percent.¹⁶

Another way to read the MTO demonstration is as a way to help inform community-level interventions (not just mobility programs), by trying to shed light on the specific neighborhood attributes that might matter most for people’s life outcomes. If we had all the money in the world, the first,

¹⁴ Data from the Census Bureau’s American Community Survey for 2006 through 2010 show the poverty rates for the five MTO cities are 21.3 percent for Baltimore, 21.2 percent for Boston, 20.9 percent for Chicago, 19.5 percent for Los Angeles, and 19.1 percent for New York. See <http://www.census.gov>.

¹⁵ <http://www.nytimes.com/2011/09/14/us/14census.html?pagewanted=all>.

¹⁶ It is always possible to have some poor families live in tracts with poverty rates below 15 percent. Because 15 percent of the population is poor, however, that would require some other poor families to then live in tracts with poverty rates above 15 percent. The key point is that if 15 percent of all Americans are poor, it is simply not possible to have each and every poor family live in a tract in which less than 15 percent of all tract residents are poor.

best way to learn about community-level interventions is to carry out randomized experiments that test community-level interventions. Implementing most community-level programs in enough communities to provide adequate statistical power to detect effects quickly becomes cost prohibitive, however. A second best approach for learning about community-level interventions might be to study the effects of moving families into different types of communities, as in MTO and in the spirit of “mechanism experiments” suggested by Ludwig, Kling, and Mullainathan (2011).

Although one potential concern is that MTO might have less beneficial impacts on people’s lives than would community-level interventions, given the potentially disruptive effects of moving itself, this concern strikes me as less serious than it initially appears once we recognize the high rates of residential mobility that we see in general in the United States. Typically around 18 to 22 percent of Americans change addresses each year, about twice the rate we see in other developed countries like Japan or Britain (Long, 1992). Mobility rates are higher still among American renters, around 32.5 percent per year (Crowley, 2003). If we implemented a community-level program in a subset of neighborhoods, after a 10- to 15-year followup period, a large share of the original residents would have turned over. A large share of the people who currently lived in the new-and-improved neighborhood would have moved in from somewhere else; that is, the net effect of the community improvement effort would be to enable a subset of low-income people to move into a new, less disadvantaged neighborhood. Over the long term, therefore, MTO and a community-level intervention might wind up looking not all that different.

Given my discussion of the MTO results so far, it is probably not surprising that I think safety seems like a particularly important target for community-level interventions. The MTO families themselves reported on the baseline surveys that safety was far and away the most common reason they signed up to participate in MTO. The beneficial effects of MTO on neighborhood safety may be one of the key drivers for MTO’s impacts on mental health outcomes, and potentially on physical health outcomes like extreme obesity and diabetes. Improving safety would also have important direct effects on public health of low-income populations by reducing the toll of violence. Homicide is the leading cause of death to African Americans ages 15 to 24, by far. Homicides, because they are so heavily concentrated among young people, are responsible for nearly as many years of potential life lost before age 65 among African Americans as is the nation’s leading overall killer, heart disease. Devoting more attention to the crime problem that plagues our inner cities might be one of the most helpful things we could do for the low-income families living there.

I am not sure why researchers, advocates, philanthropists, and policymakers who care deeply about improving the lives of poor people do not take the crime problem more seriously. Perhaps one reason might be lingering concern that the cure might be worse than the disease. America’s imprisonment rate has increased sevenfold since the 1970s, with minorities particularly affected (Western and Pettit, 2010).

A growing body of evidence, however, has shed light on different ways to prevent criminal behavior from occurring in the first place, which can lead to less crime and less imprisonment (Cook and Ludwig, 2011). For example, several studies suggest that stepped-up policing can deter criminal behavior (Evans and Owens, 2007; Owens, 2011; Zimring, 2011) and, because deterrence is a key mechanism, perhaps even reduce overall jail and prison commitments as well as crime, although

doing urban policing in a way that is seen as fair and legitimate remains a challenge. Efforts to address deficits among at-risk young people in academic skills and nonacademic (or “social-cognitive”) skills like self-regulation, conflict resolution, and future orientation can also prevent criminal behavior (and hence also reduce incarceration rates) and improve people’s schooling outcomes at the same time (Heller et al., 2012; Hill et al., 2011; Lochner, 2011).

HUD itself could also try to get in the game and contribute to crime control through community development efforts that try to stimulate and support local “collective efficacy” (Sampson, Raudenbush, and Earls, 1997) or provide young people with supervised, productive activities during high-crime periods.¹⁷ Given that such a large share of HUD’s budget is devoted to housing rather than urban development, efforts to improve safety would require putting the “UD” back in HUD.

In sum, I think there are three important lessons that come out of the last 15 years of MTO research that were not self-evident when the program started. The first is that the William Julius Wilson hypothesis that schooling and employment outcomes are strongly affected by the geographic concentration of poverty does not seem to be borne out, at least for very disadvantaged families of the sort that signed up for MTO. Second, neighborhood environments do seem to have surprisingly large impacts on an outcome domain that was not at all the focus of MTO when the demonstration began—health. Third, neighborhood safety might be even more important for families participating in MTO than anyone might have initially expected.

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¹⁷ The term *supervised activities* is used because some evidence from Jacob and Lefgren (2003) suggests that bringing young people together might prevent them from engaging in property crimes but creates some risk of elevated rates of violent behavior, because young people aggregated together might get into arguments that turn into fights.

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