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Jens Ludwig

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Jens Ludwig

University of Chicago and NBER

jludwig@uchicago.edu

Sciences Po | LIEPP
27 rue Saint-Guillaume
75337 Paris Cedex 07
Tel : 01 45 49 83 61
www.sciencespo.fr/liepp

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October, 2012

Jens Ludwig
University of Chicago and National Bureau of Economic Research

Contact:
Jens Ludwig
Harris School of Public Policy Studies
University of Chicago
1155 East 60th Street
Chicago, IL 60637
(773) 834-0811
jludwig@uchicago.edu

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I. Introduction

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 (Watson, 2009, Kneebone, Nadeau, and Berube, 2011). 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 pro-social 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 over where they live. A large body of research going 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 themselves and their families. What remains unclear is the degree to which these patterns reflect true "neighborhood effects," 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) launched one of the most ambitious social experiments ever carried out by the agency – the Moving to Opportunity (MTO) demonstration. Via random lottery, MTO offered some public housing families but not others the chance to use a housing voucher to move from high- to lower-poverty neighborhoods. Some of the families that were offered the chance to relocate were offered special vouchers that could initially only be used to move into very low-poverty areas, although after one year families could use the vouchers to move again (including to higher-poverty places).

This special issue of *Cityscape* focuses on the long-term follow-up that measured outcomes of MTO families 10-15 years after random assignment carried out by a research team assembled by the National Bureau of Economic Research (NBER), which was supported by a contract with HUD and additional grants from other agencies and private foundations. In this guest editor's introduction I provide some basic background

[1] For excellent reviews of the theoretical and empirical literatures on "neighborhood effects" see Jencks and Mayer (1991), Ellen and Turner (1997), Leventhal and Brooks-Gunn (2000), Sampson, Morenoff and Gannon-Rowley (2002), and Kawachi and Berkman (2003).

about MTO that frames all of the paper's that follow, and offer some thoughts of my own about what lessons we might take from MTO for both social science and public policy.

In Section 2, 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 essay in this symposium by Mark Shroder and Larry Orr. The symposium paper by Jennifer Comey, Susan Popkin and Kaitlin Franks shows that MTO was successful in helping families move into higher-quality housing units. The commentary by Edgar 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. But Olsen notes that there would be great value in exploiting the MTO platform to learn more about these cost-effectiveness issues.

In Section 2 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 Section 3 I review the evidence showing that the very large initial differences in average neighborhood conditions between the MTO treatment and control groups 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've 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 Section 4 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 10-15 year follow-up study period moving with a MTO voucher reduces average census tract poverty rates by about 18 percentage points, equal to nearly 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 datasets like the Project on Human Development in Chicago Neighborhoods (PHDCN). MTO generates less change in racial segregation, although as Mark Shroder and Larry Orr discuss in their essay, much of the discussion leading up 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. Since families were followed up over such a long time (10-15 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 surveys had low response rates. And yet as Nancy Gebler and her co-authors note in their paper in this symposium, the team from the University of Michigan tasked with carrying out the surveys achieved remarkably high response rates, 90% for adults and 89% for youth, very similar across groups, to preserve the key strength of MTO's experimental design. Gebler et al.'s chapter includes some useful lessons for future researchers about how to track similar populations, and 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 chapters in this symposium by Lisa Sanbonmatsu and co-authors and by Lisa 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 Section 5 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," since we did not find detectable effects on schooling or labor market outcomes across the five demonstration sites in either the interim (4-7 year) or long-term (10-15 year) follow-up 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. But these are exactly the sorts of disadvantaged families that have commanded (for good reason) a disproportionate share of media and policy attention, 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 a 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. In Section 5 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 mechanisms of action behind MTO's impacts on health outcomes.

In Section 6 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 comments in this symposium by Philip Oreopolous, Margery Turner, and Kathy Edin and colleagues. But 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.

II. The Moving to Opportunity (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 a black 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 into different parts of Chicago that were poor and segregated, but improving, while others were relocated into low-poverty, racially integrated suburbs (Keels et al., 2005).

A 1988 follow-up survey carried out 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 not be due to other differences in the characteristics of the families themselves. Follow-up research has provided some support for this concern and has tended to find smaller impacts on family outcomes (Mendenhall, Duncan and Deluca, 2006, Votruba and Kling, 2009, Deluca et al., 2010).

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 Moving to Opportunity (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 of the MTO participants from the Baltimore and Chicago sites are African-American, while the other three sites are split about evenly between blacks 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 a total of 4,604 families. Surveys collected at baseline (Table 1) show just how disadvantaged these 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, while three-quarters were on welfare.

Perhaps the most striking result from Table 1 is that over 40 percent of MTO applicants had someone in the household victimized by a crime during the six 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 that volunteered for MTO were then randomly assigned them to one of the following three conditions:

- The *Experimental group* was offered the chance to use a housing rent-subsidy voucher[2] 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 one year. If they moved before the year was up, they would lose their voucher. But after their initial one-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 non-profit organizations.[3]
- The *traditional Section 8 voucher group* was offered a regular 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 (non-experimental) 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 a 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. While many people outside

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[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 Olsen, 2003 and Jacob and Ludwig, 2012 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 and long-term HUD technical reports summarizing the MTO results (Orr et al., 2003, and Sanbonmatsu et al., 2011) describe the three groups as experimental, Section 8, and controls. In some of our research team's other writings (for example, Ludwig et al., 2011) we use instead the more descriptive terms "low-poverty voucher group," "traditional voucher group," and controls.

the housing-policy research community have been surprised by these take-up rates, these figures are generally similar to what has been found in previous studies of other housing voucher programs (Rubinowitz and Rosenbaum, 2000, Olsen, 2003). 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 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 non-profit 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 that have been raised to increase voucher take-up rates 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 don't. 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

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[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. But this creates the risk of reducing the share of vouchers that are being used by low-income families at any given point in time. Alternatively we could spend more money on housing-mobility counseling assistance for voucher recipients, or efforts to encourage landlords to accept housing vouchers. But 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 a MTO voucher by doing something non-experimental such as comparing just the Experimental group movers with the controls, because the families in the Experimental group who move with a voucher are a self-selected subset of families assigned into that group – and so this self-selected subset cannot be compared with all the families assigned to the control group, because this 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-only) group does not have much effect on families who do not use a 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-only) group who use a MTO voucher to relocate (H. Bloom, 1984). Since no control group families can use a 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 a MTO voucher.

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 below) 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. MTO does not tell us anything about the effects of giving housing vouchers to people who are living unsubsidized in the private housing market, which leads to large gains in disposable income from families because they can now spend much less out-of-pocket on rent, but generates relatively little change in neighborhood conditions (see Mills et al., 2006, Jacob and Ludwig, 2012). The comparison of vouchers to 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 not less than a third of income-eligible families are in means-tested housing programs (Olsen, 2003).

III. 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 papers in this *Cityscape* issue do not have to replicate this material over and over, 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 paper in this symposium by Jennifer Comey, Susan Popkin and Kaitlin Franks.

Table 2 shows that one year after the time of random assignment, even the intention-to-treat (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 Table 2 show that one year after baseline, families assigned to the Experimental or Section 8 voucher groups 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 will tend to focus on the Experimental group versus Control group contrast, which winds up providing the strongest test of "neighborhood effects," although the contrast between the Section 8-group and Control group 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 effects on neighborhood poverty rates diminishes. Table 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 five years after baseline, and about 5 percentage points measured 10-12 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-only group families. The average census tract poverty rates for families assigned to the Experimental group *declined* over the period from 1 to 10-12 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).

Regardless of the cause, it is clearly true that the neighborhood conditions of the MTO treatment 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 follow-up study period. Looking at MTO's effects on average neighborhood conditions that families experience over the entire follow-up 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). Table 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 in 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 to 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 is contributing to the convergence in neighborhood conditions between the Experimental and control groups. But as mentioned above, it is also possible to calculate the effects of MTO on the neighborhood conditions of those who actually move through the program, or the effects of treatment on the treated (TOT). Table 2 shows the TOT effect on duration-weighted tract poverty rates was fully 18 percentage points, equal to about 45 percent of the control group's average tract poverty rate over the study period of 40 percent.

Table 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 to just looking at poverty alone. The index is a weighted average of census tract share poor, unemployed, share of households headed by a single parent, 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 low-income because they are comprised of two-parent families that are mostly working but have low earnings, while other neighborhoods are poor because 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. Table 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 -.49 on this index, equal to around 35 percent of the control mean.

While 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, 1997, 2003). Yet as Table 2 makes clear, MTO's impacts on racial segregation for participants were 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 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). Note that Table 2 focuses on the self-reports of MTO adults about their social networks and neighborhood social processes measured 10-12 years after random assignment – that is, *after* the convergence in neighborhood poverty rates between treatment and control groups has occurred.

Table 2 shows that 10-12 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 (what Sampson, Earls and Raudenbush, 1997 call "collective efficacy") was over 15 percentage points, about one-quarter of the control group's value of 59 percent.

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[6] While Sampson, Sharkey and Raudenbush (2008) calculate 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 Table 2 are based on a principal components analysis that Sampson, Sharkey and Raudenbush (2008, Table 1) calculate using tract-level data for Chicago from the 2000 decennial census, and equal .90 for share tract 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.

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. These administrative records might under-state 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 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. Self-reported data about neighborhood safety from MTO participants shows 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 a 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.

IV. What can MTO tell us about "neighborhood effects"?

If it had turned out that there were few differences in average neighborhood conditions between the treatment and control groups in MTO, then the MTO demonstration will not have much useful to say about the existence of any "neighborhood effects" on families. Yet in the previous section we showed that MTO moves generate changes in neighborhood disadvantage and social processes that are massive initially after random assignment. These effects are still sizable when averaged over the entire study period, viewed in absolute terms, or as a share of the control group average neighborhood attributes. So why 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. But 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 William Julius 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 blacks move out to other areas. This is a story about the importance of neighborhood socio-economic disadvantage, not racial segregation.

Some people have expressed the view that MTO is a weak treatment even with respect to the sorts of socio-economic measures that I have argued in the pervious 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 a voucher group 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 also of interest because they help extrapolate MTO results to other mobility interventions that might have different voucher compliance rates, and are of scientific interest because they more directly identify the effects of location 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, p. 22) 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. But this is a little misleading because as a share of all census tracts in the U.S. as a whole, there are just not all that many census tracts that have high tract poverty rates. This means that very 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 to the amount of "spread" in the larger census-tract poverty-rate distribution. Table 2 shows that one 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, while the TOT effect is equal to fully -2.8 sd.[7] The Experimental group effects on duration-weighted average tract 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.

A third reason some observers conclude that MTO is a weak treatment is because they divide neighborhoods up into a small number of discrete and essentially arbitrary "types." For example, Clampet-Lundquist and Massey (2008) create four neighborhood categories by dividing them on two separate dimensions: "poor" versus "non-poor" (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" (p. 7).

[7] Table 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.

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 up into a small number of categories is sensible only if neighborhood effects on outcomes are *non-linear*, so that (say) moving from a tract that has a 50 percent to 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 "non-poor" area in a Turner-type definition).

Yet the evidence presented in Kling, Liebman and Katz (2007) 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, etc. If neighborhood effects on people's outcomes are linear, as the findings by Kling et al. seem to suggest, then dividing up neighborhoods into a small number of categories winds up unhelpfully 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'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 treatment and control groups 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 to what we see in what is arguably the most important observational (non-experimental) study of neighborhood effects ever carried out, the Project on Human Development in Chicago Neighborhoods (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 above), or the "treatment group" in their study, compared to all other African-Americans in their study, the "controls." The treatment group in their study lived in tracts that were 38 percent poor, compared to control tracts that were 20 percent poor on average, for a difference of 18 percentage points – almost identical to what we see in MTO.[8]

V. What do the MTO results mean for social science?

[8] If we look instead at Sampson et al.'s 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 .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 0.9, for a difference of 0.49.

Twenty-five years ago William Julius Wilson argued in the *Truly Disadvantaged* (1987) 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, since 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.[9] And the sorts of very disadvantaged families that live in our nation's most distressed public housing projects have, for understandable reasons, commanded a disproportionate share of the media and policy attention. While the MTO results might not generalize to families with higher levels of socio-economic 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 MTO moves generated larger changes in the characteristics of the schools that children attended (see also the comments in this issue by Margery Turner and Philip Oreopolous). Maybe. Previous studies suggest that attending a higher-quality urban school (public or charter) tends to have beneficial impacts on behavioral outcomes like schooling persistence or delinquency. But this is not as consistently true with respect to achievement test scores, which has for better or worse been an outcome of particular interest in policy discussions, and for which previous studies tend to find more mixed impacts (Cullen, Jacob and Levitt, 2006, Hastings, Kane and Staiger, 2006, Abdulkadiroglu et al., 2011, Angrist et al., 2010, 2011, Deming, 2012). 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?

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[9] 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 to 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 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 slightly 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 almost surely be even closer to what we see in MTO. (http://www.pnas.org/content/suppl/2007/12/12/0710189104.DC1/10189Table6.pdf).

One candidate explanation is that not all children experience a school 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 a focus on extra instructional time for core reading and math skills, frequent assessments to gauge what students are learning, and targeted instruction through tutoring or small-group settings – something that regrettably few disadvantaged children seem to receive regardless of where they live and go to school.[10]

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 make a claim that is equally strong in the other direction – that the real 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 Lisa Sanbonmatsu notes in her chapter, moving with a MTO Experimental-group voucher reduced rates of extreme obesity (BMI≥40) and diabetes (HbA1c≥6.5 percent) by around *40 percent* expressed as a share of the control group's prevalence rate. This 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 (4-7 year) follow-up were about the same size as what we see from best-practice drug treatment.[11]

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 Table 2 makes clear, which complicates any attempt to figure out what is responsible for the observed differences in average health outcomes between

[10] 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 et al. (2011) note that the more effective urban charter schools they study in the Boston area tend to be those adopting the "No Excuses" approach of KIPP, which emphasizes extra math and reading instruction time. Angrist et al. (2010) shows that those students who benefit the most from attending a KIPP school are those with low baseline test scores, or are limited English proficient, or are in special education.

[11] Note that while 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.

the treatment and control groups (or the lack of observed differences in other outcome domains). So 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 or in our measure of clinical depression 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.

But safety might also be an important contributing factor to the massive (40 percent) 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. Another reason is because we can look across MTO demonstration sites and voucher-treatment groups and see whether those groups that experience relatively larger changes in a given neighborhood characteristic also experience relatively larger changes in health outcomes. Using this quasi-experimental dose-response approach, we see that families who have relatively larger changes in safety experience relatively larger changes in psychological distress, and in diabetes.[12]

If safety is an important mechanism behind MTO's health impacts, then why don't we also see MTO impacts on other outcomes like schooling? After all, Sharkey (2010) finds some evidence in the PHDCN data for very large (.5 to .66 standard deviation) short-term effects of neighborhood homicide rates on children's achievement test scores. Perhaps the contrast between the PHDCN 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. For example in the MTO data when we look across sites and groups using the quasi-experimental

[12] This suggestive finding deserves some additional explanation, and some caveats. More technically, we use the approach from Kling, Liebman and Katz (2007) and use interactions of indicators for treatment group and demonstration sites as instrumental variables for different neighborhood characteristics as endogenous explanatory variables. We standardize the neighborhood measures (subtract off the control group mean, and divide by the control group standard deviation) so that we can meaningfully compare the size of the different coefficients. When we run a model that instruments for a single neighborhood measure at a time, the coefficients for the relationship between safety and the K6, or a measure of severe psychological distress, or BMI≥35, or BMI≥40, or diabetes (HbA1c≥6.5%) are usually four to six times as large as what we see when we instrument for tract poverty rate as the endogenous explanatory variable of interest. But when we instrument for both safety and tract poverty rate at the same time in the same model, we typically cannot statistically distinguish between the effects of the two measures.

dose-response model, 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.

VI. 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 Edgar Olsen notes in his essay in this volume, 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. But some of the most important potential costs of MTO are unlikely to show up on any government budget spreadsheet. 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 area 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 is not well suited to learning anything about these larger society-wide 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 since MTO-like moves did not generate detectably large gains in schooling and labor market outcomes, then more intensive mobility interventions are needed. But it is not obvious 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 looking at MTO data across sites and groups using the quasi-experimental dose-response model described above. This approach shows that MTO participants who experience relatively larger changes in neighborhood poverty or related characteristics have larger improvements in physical or mental health outcomes (Kling, Liebman and Katz, 2007, Ludwig et al., 2011). But we do 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. While MTO itself does not have much to say about those sorts of moves, follow-up 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 socio-economic 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.

But 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 U.S. 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.[13] 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 U.S. as a whole right now is 15 percent.[14] 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.[15]

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

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[13] Data from the Census Bureau's American Community Survey for 2006-10 show the poverty rates for the five MTO cities equal 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 Los Angeles. See www.census.gov.

[14] http://www.nytimes.com/2011/09/14/us/14census.html?pagewanted=all

[15] It is always possible to have some poor families live in tracts with poverty rates below 15%. But since 15% of the population is poor, that would require some other poor families to then live in tracts with poverty rates above 15%. The key point is that if 15% 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% of all tract residents are poor.

neighborhood attributes that might matter most for people's life outcomes. If we had all the money in the world, the first-best way to learn about community-level interventions is to carry out randomized experiments that test community-level interventions. But implementing most community-level programs in enough communities to provide adequate statistical power to detect effects quickly becomes cost-prohibitive. 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).

While 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 U.S. Typically around 18-22 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 sub-set of neighborhoods, after a 10-15 year follow-up 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. So over the long term 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 as well. 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 15-24 by far. Homicides, because they are so heavily concentrated among young people, are responsible for nearly as many years of potential life lost before 65 among blacks 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 myself why researchers, advocates, philanthropists and policymakers who care deeply about improving the lives of poor people don't 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 seven-fold since the 1970s, with minorities particularly affected (Western and Pettit, 2010).

But a growing body of evidence 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, 1997, Owens, 2011,

Zimring, 2012), 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 non-academic (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 (Hill et al., 2011, Lochner, 2011, Heller et al., 2012).

HUD itself could also try 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.[16] 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. And third, neighborhood safety might be even more important than anyone might have initially expected, potentially for health outcomes but certainly for the perceived well-being of the MTO families themselves.

[16] I say "supervised" activities because there is some evidence from Jacob and Lefgren (2003) that bringing young people together might prevent them from engaging in property crimes, but creates some risk of elevated rates of violent behavior just because young people aggregated together might get into arguments that turn into fights.

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Table 1
Baseline Characteristics

	Experimental Voucher	Section 8 Voucher	Control
	N=1456	N=678	N=1139
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 ethnicity (any race)	0.314	0.340	0.304
Other Demographic Characteristics			
Never married	0.623	0.624	0.637
Parent before 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
GED	0.159 *	0.183	0.199
Receiving Aid to Families with Dependent Children (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 six months	0.434	0.414	0.416
Streets unsafe at night	0.493	0.517	0.512
Very dissatisfied w/ neighborhood	0.478	0.477	0.467
Lived in neighborhood 5+ years	0.599	0.616	0.606
Very likely to tell neighbor about child getting into trouble	0.556	0.521	0.555

Table 1, continued

	Experimental Voucher	Section 8 Voucher	Control
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

Notes: * = P <.05, ~ = P <.10 on pair wise probability-weighted t-test of the difference between the experimental voucher or Section 8 voucher group and the control group. An omnibus F-test fails to reject the null hypothesis that the set of baseline characteristics reported above are the same across MTO sample random assignment groups (p-value for the experimental housing voucher vs. control comparison is P =.90; p-value for the Section 8 housing voucher vs. control comparison is P =.69). All values represent shares. Shares are calculated using sample weights to account for changes in random assignment ratios across randomization cohorts and for subsample interviewing. Sample is adults with valid self-reported happiness.

Data source and sample: Adult long-term survey. All adults with who were interviewed.

Measures: The baseline head of household reported on the neighborhood characteristics listed here.

Table 2
MTO Effects on Housing and Neighborhood Conditions,
10-15 Years After Random Assignment

	Control Mean	Experimental voucher vs. Control ITT	Experimental voucher vs. Control TOT	Section 8 voucher vs. Control ITT	Section 8 voucher vs. Control TOT	N
Number of moves after random assignment	2.165	0.561 *	1.157 *	0.629 *	1.014 *	3273
		(0.073)	(0.151)	(0.096)	(0.155)	
Tract Poverty at Baseline						
Share poor	0.531	-0.004	-0.009	-0.004	-0.007	3227
		(0.005)	(0.009)	(0.006)	(0.009)	
Share poor, z-score on U.S. tracts	3.172	-0.034	-0.070	-0.034	-0.054	3227
		(0.037)	(0.076)	(0.046)	(0.074)	
Share poor, z-score on MTO controls	0.000	-0.027	-0.055	-0.032	-0.051	3220
		(0.031)	(0.063)	(0.038)	(0.061)	
Tract Poverty 1 Year Post-Random Assignment						
Share poor	0.499	-0.170 *	-0.351 *	-0.140 *	-0.224 *	3224
		(0.008)	(0.016)	(0.009)	(0.014)	
Share poor, z-score on U.S. tracts	2.916	-1.377 *	-2.847 *	-1.133 *	-1.818 *	3224
		(0.062)	(0.128)	(0.072)	(0.116)	
Share poor, z-score on MTO controls	0.000	-1.053 *	-2.170 *	-0.873 *	-1.386 *	3217
		(0.047)	(0.097)	(0.055)	(0.087)	
Tract Poverty 5 Years Post-Random Assignment						
Share poor	0.399	-0.099 *	-0.203 *	-0.070 *	-0.114 *	3208
		(0.007)	(0.015)	(0.009)	(0.015)	
Share poor, z-score on U.S. tracts	2.109	-0.803 *	-1.646 *	-0.571 *	-0.921 *	3208
		(0.060)	(0.124)	(0.076)	(0.123)	
Share poor, z-score on MTO controls	0.000	-0.607 *	-1.241 *	-0.425 *	-0.678 *	3201
		(0.045)	(0.093)	(0.057)	(0.091)	
Tract Poverty 10-12 Years Post-Random Assignment						
Share poor	0.330	-0.045 *	-0.093 *	-0.040 *	-0.065 *	3196
		(0.007)	(0.014)	(0.009)	(0.014)	
Share poor, z-score on U.S. tracts	1.544	-0.365 *	-0.752 *	-0.324 *	-0.525 *	3196
		(0.056)	(0.115)	(0.071)	(0.115)	
Share poor, z-score on MTO controls	0.000	-0.285 *	-0.587 *	-0.248 *	-0.398 *	3189
		(0.043)	(0.088)	(0.055)	(0.088)	
Duration-Weighted Census Tract Characteristics						
Share poor	0.396	-0.089 *	-0.184 *	-0.069 *	-0.111 *	3270
		(0.006)	(0.012)	(0.007)	(0.011)	
Share poor, z-score on U.S. tracts	2.083	-0.722 *	-1.487 *	-0.556 *	-0.897 *	3270
		(0.046)	(0.094)	(0.055)	(0.088)	
Share poor, z-score on MTO controls	0.000	-0.715 *	-1.470 *	-0.548 *	-0.873 *	3263
		(0.045)	(0.093)	(0.054)	(0.086)	
Share minority	0.880	-0.061 *	-0.125 *	-0.018 *	-0.029 *	3270
		(0.007)	(0.014)	(0.009)	(0.015)	
Share minority, z-score on U.S. tracts	1.798	-0.195 *	-0.401 *	-0.057 *	-0.092 *	3270
		(0.022)	(0.045)	(0.029)	(0.047)	
Share minority, z-score on MTO controls	0.000	-0.377 *	-0.774 *	-0.115 *	-0.183 *	3263
		(0.042)	(0.086)	(0.056)	(0.090)	
Concentrated Disadv. index	1.390	-0.238 *	-0.490 *	-0.189 *	-0.304 *	3270
		(0.016)	(0.032)	(0.018)	(0.029)	
Concentrated Disadv. index, z-score on MTO controls	0.000	-0.648 *	-1.333 *	-0.513 *	-0.818 *	3263
		(0.042)	(0.087)	(0.050)	(0.079)	
Share college graduates	0.161	0.042 *	0.087 *	0.018 *	0.029 *	3270
		(0.004)	(0.008)	(0.005)	(0.008)	

Table 2, continued

	Control Mean	Experimental voucher vs. Control ITT	Experimental voucher vs. Control TOT	Section 8 voucher vs. Control ITT	Section 8 voucher vs. Control TOT	N
Local Area Violent Crime Rate (per 100,000 residents)						
Duration-weighted	2419.7	-425.550 *	-876.260 *	-347.520 *	-560.180 *	3269
		(44.4)	(91.3)	(52.8)	(85.1)	
One year after random assignment	3551.1	-999.020 *	####### *	-766.470 *	####### *	3077
		(86.2)	(187.1)	(100.4)	(165.4)	
Five years after random assignment	2468.1	-448.390 *	-960.060 *	-274.030 *	-455.070 *	3094
		(59.6)	(127.7)	(69.6)	(115.6)	
Ten years after random assignment	1619.9	-152.270 *	-325.110 *	-40.660	-66.830	3073
		(37.2)	(79.4)	(56.5)	(92.9)	
At December 31, 2001	2612.62	-532.990 *	####### *	-390.960 *	-649.730 *	3057
		(68.100)	(146.690)	(76.700)	(127.460)	
At May 31, 2008	1447.94	-100.100 *	-215.080 *	-25.970	-42.600	3053
		(34.70)	(74.56)	(50.51)	(82.830)	
At baseline	4047.04	-97.840	-202.400	-31.630	-51.260	3165
		(91.920)	(190.150)	(116.860)	(189.370)	
At final evaluation	1401.40	-85.190 *	-180.920 *	-82.840 ~	-136.340 ~	3037
		(34.350)	(72.960)	(43.840)	(72.160)	
Local Area Property Crime Rate (per 100,000 residents)						
Duration-weighted	4928.8	-164.1	-337.9	-267.9 **	-431.2 **	3268
		(109.2)	(224.8)	(110.5)	(177.9)	
One year after random assignment	6215.1	-470.1 *	-1021.6 *	-368.8 *	-607.7 *	3080
		(250.6)	(544.5)	(204.9)	(337.6)	
Five years after random assignment	5094.7	-187.2	-402.0	-54.4	-90.3	3090
		(125.7)	(269.8)	(160.6)	(266.7)	
Ten years after random assignment	3995.1	-13.6	-29.1	-59.8	-98.3	3083
		(101.6)	(217.1)	(132.2)	(217.3)	
At December 31, 2001	5151.31	-248.840 *	-536.350 *	-294.690 *	-489.650 *	3056
		(118.430)	(255.270)	(143.620)	(238.640)	
At May 31, 2008	3626.20	45.340	97.160	61.140	99.990	3078
		(82.16)	(176.05)	(122.19)	(199.840)	
At baseline	6849.97	193.880	401.070	106.630	172.790	3165
		(239.930)	(496.330)	(224.420)	(363.690)	
At final evaluation	3542.42	1.670	3.550	-112.020	-183.500	3065
		(82.510)	(175.220)	(100.860)	(165.230)	
Housing Self-Reports and Interviewer Observations from Long-Term Survey						
Condition excellent	0.213	0.003	0.006	0.030	0.048	3267
		(0.018)	(0.037)	(0.024)	(0.038)	
Condition excellent or good	0.570	0.053 *	0.109 *	0.031 *	0.050 *	3267
		(0.021)	(0.044)	(0.029)	(0.046)	
Number of housing problems (0-7)	0.836	-0.169 *	-0.347 *	-0.181 *	-0.290 *	3267
		(0.059)	(0.121)	(0.071)	(0.114)	
Number of housing problems, z-score	0.000	-0.118 *	-0.242 *	-0.122 *	-0.195 *	3264
		(0.040)	(0.083)	(0.049)	(0.078)	
Interviewer rated building condition on	0.836	0.021	0.044	0.036	0.057	3199
		(0.016)	(0.032)	(0.020)	(0.033)	
Interviewer rated building condition on	0.348	0.024	0.049	0.007	0.011	3199
		(0.021)	(0.044)	(0.028)	(0.045)	
Interviewer observation of neighborhood	0.316	-0.018	-0.037	-0.004	-0.007	3207
		(0.011)	(0.022)	(0.014)	(0.022)	
Interviewer observation of neighborhood	0.000	-0.075 ~	-0.156 ~	-0.026 ~	-0.041 ~	3200
		(0.043)	(0.089)	(0.056)	(0.088)	

Table 2, continued

	Control Mean	Experimental voucher vs. Control ITT	Experimental voucher vs. Control TOT	Section 8 voucher vs. Control ITT	Section 8 voucher vs. Control TOT	N
Housing Self-Reports and Interviewer Observations from Long-Term Survey (continued)						
Received any housing assistance	0.620	0.026	0.054	0.045	0.072	3273
		(0.021)	(0.043)	(0.027)	(0.044)	
Total monthly housing cost	$678.73	19.500	39.665	-6.261	-10.004	3180
		(23.30)	(47.38)	(30.73)	(49.10)	
Household is rent-burdened	0.676	0.011	0.022	0.020	0.032	3169
		(0.020)	(0.041)	(0.027)	(0.043)	
Household is severely rent-burdened	0.426	-0.004	-0.007	0.017	0.027	3169
		(0.021)	(0.043)	(0.029)	(0.046)	
Collective Efficacy: Very Likely/Likely to Report						
Kids spraying graffiti	0.589	0.076 *	0.156 *	0.042 *	0.067 *	3255
		(0.021)	(0.043)	(0.028)	(0.045)	
Kids skipping school	0.346	0.029	0.059	0.075	0.119	3250
		(0.021)	(0.043)	(0.028)	(0.045)	
Social Networks						
1+ friend with college degree	0.532	0.071 *	0.145 *	0.007 *	0.010 *	3203
		(0.021)	(0.044)	(0.029)	(0.046)	
No close friends	0.145	-0.018	-0.038	0.042	0.066	3265
		(0.015)	(0.030)	(0.022)	(0.034)	
Medical Care						
Place to go for routine care (not ER)	0.935	-0.012	-0.024	0.010	0.015	3264
		(0.011)	(0.022)	(0.012)	(0.020)	
Safety						
Feel unsafe during day, z-score	0.000	-0.089 *	-0.183 *	-0.115 *	-0.184 *	3259
		(0.041)	(0.085)	(0.054)	(0.086)	
Feel unsafe during day	0.196	-0.036 *	-0.074 *	-0.045 *	-0.072 *	3262
		(0.016)	(0.034)	(0.021)	(0.034)	
Feel unsafe during night, z-score	0.000	-0.084 *	-0.174 *	-0.151 *	-0.240 *	3243
		(0.042)	(0.087)	(0.056)	(0.088)	
Feel unsafe during night	0.404	-0.043 *	-0.088 *	-0.073 *	-0.117 *	3246
		(0.021)	(0.043)	(0.027)	(0.043)	
Police don't respond	0.420	-0.067 *	-0.138 *	-0.075 *	-0.118 *	3146
		(0.021)	(0.044)	(0.028)	(0.045)	
Saw drugs used/sold last 30 days	0.310	-0.062 *	-0.128 *	-0.057 *	-0.090 *	3249
		(0.019)	(0.039)	(0.025)	(0.040)	
Household Crime Victimization (Last 6 Months)						
Any crime	0.184	-0.022	-0.046	0.025	0.040	3241
		(0.016)	(0.033)	(0.022)	(0.035)	
Assault	0.074	-0.014	-0.030	-0.005	-0.008	3238
		(0.011)	(0.022)	(0.014)	(0.023)	
Break-in/attempted break-in	0.043	-0.003	-0.007	0.015	0.025	3242
		(0.009)	(0.019)	(0.013)	(0.021)	
Snatched purse/wallet/jewelry	0.077	-0.005	-0.010	0.008	0.012	3234
		(0.011)	(0.023)	(0.014)	(0.023)	
Stabbing/shooting	0.029	-0.006	-0.013	-0.008	-0.013	3241
		(0.007)	(0.015)	(0.008)	(0.013)	
Threatened with knife/gun	0.066	-0.008	-0.016	0.001	0.001	3236
		(0.010)	(0.021)	(0.013)	(0.021)	

Table 2, continued

Notes: * = P <.05, ~ = P <.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intent-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted.

Model: Experimental voucher and Section 8 voucher impacts were estimated jointly using an OLS regression model controlling for baseline covariates and field release, weighted, and clustering on family.

Data source and sample: Adult long-term survey. All adults who were interviewed.

Measures: 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/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% of their monthly household income. A household is "severely rent-burdened" if monthly housing costs are greater than or equal to 50% of their monthly household income. The concentrated disadvantage index is a weighted combination of census tract percent [i] poverty, [ii] on welfare, [iii] unemployed, [iv] female-headed family households, and [v] under age 18, with loading factors developed using 2000 Census tracts in Chicago by Sampson, Sharkey, and Raudenbush (2008), but does not include percent African-American.

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Directeurs de publication:
Etienne Wasmer & Cornelia Woll

Sciences Po - LIEPP
27 rue Saint Guillaume
75007 Paris - France
+33(0)1.45.49.83.61
liepp@sciencespo.fr