Cityscape

A Journal of Policy Development and Research

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Contents

Guest Editor's Introduction

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The contents of this introduction are the views of the author and do not necessarily reflect the views or policies of the U.S. Department of Housing and Urban Development, the Congressional Budget Office, the U.S. government, or any state or local agency that provided data.

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)

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[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 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 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

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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 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 takeup 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.

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[4] For example, one potential way to improve voucher takeup 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)	− 1172.9* (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).

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[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.

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[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

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[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 character-istics 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 charac-teristics 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 neigh-borhoods 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 neighbor-hood 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

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[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]

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[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

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[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,

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[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, Rauden-bush, 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 con-centration 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 participat-ing in MTO than anyone might have initially expected.

Acknowledgments

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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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Moving to Opportunity: Why, How, and What Next?

Mark D. Shroder
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Abstract

We discuss the policy background for the Moving to Opportunity (MTO) for Fair Housing demonstration experiment, the innovations in its design and implementation, and a few of the implications for future policy. We explain why a full-blown randomized experiment was necessary, in what ways MTO was unique, and whether the issues posed by concentrated poverty are the same today as when Congress first authorized the experiment.

Introduction

In 2001, Shroder wrote the following in this journal (italics added):

> Moving to Opportunity (MTO) is a demonstration designed to ensure a *rigorous* evaluation of the impacts of helping very low-income families with children to move from public and assisted housing in high-poverty inner-city neighborhoods to *middle-class neighborhoods throughout a metropolitan area.*

> Poverty in the United States has become *increasingly* concentrated in high-poverty areas. These concentrated high-poverty, usually urban, and frequently segregated neighborhoods are widely thought to deny their residents opportunities by denying them access to good schools, safe streets, successful role models, and good places to work…

> We do not know the extent to which moving the poor out of concentrated poverty neighborhoods, in fact, increases their life chances. Poor people who live in concentrated poverty may differ from other poor people both in ways that can be observed, like race

or age, *and in ways that may not be observed, like aspiration or persistence*. Any differences in people's outcomes that seem to be associated with the neighborhoods in which they reside might be caused by those neighborhoods—or might be caused by unobserved factors that also affect the sorting of people into different neighborhoods. Only an experiment in which *neighborhoods are allocated randomly* can answer this question. (Shroder, 2001: 57)

In this article, we discuss the policy background for the experiment, the innovations in its design and implementation, and a few of the policy implications, providing particular attention to the items italicized in the preceding passage from 2001.

The problem of concentrated poverty was much in the news in the early 1990s, when MTO began. The U.S. Department of Housing and Urban Development (HUD), in principle, could have conducted this experiment 10 years earlier or 10 years later than it did, but Congress would not have had quite the same motivation to authorize it. In the early 1990s, gangs ruled or contested certain neighborhoods in the inner city, obtaining large illicit revenues from crack cocaine. In a background of high poverty, family disintegration, and social isolation, either the illicit revenues or the effects of the drug itself drove a tenfold increase in the rate of homicide among young African-American men in the late 1980s (Cook, 2009). Journalists like Kotlowitz (1991) and Lehman (1991) produced affecting portraits of brutality, isolation, and hopelessness oppressing another generation of young people. The idea of an underclass barely under the control of the larger society grew markedly after the Los Angeles riots of 1992, associated with the beating of Rodney King.

The subsidized housing stock was in the center of the storm. The National Commission on Severely Distressed Public Housing (1992) estimated that 86,000 public housing units (6 percent of the stock) were severely distressed, based on high incidences of serious crime, physical deterioration, and a constellation of management issues—high vacancy rates, high move-out rates, high refusal rates from tenants offered units, and low rates of rent collection. Much of the distressed stock was located in the center-city areas of blighted "ghetto poverty" that urban planners had no idea how to address (Jargowsky and Bane, 1991).

In certain jurisdictions, federal courts had ordered mobility programs to remedy the racial segregation of much of the public housing stock. One of these court orders came in the case of *Dorothy Gautreaux et al.* vs. *Chicago*.[1] The remedy agreed upon involved a special program to locate rental units in White suburbs or generally improving city neighborhoods and to facilitate their leasing to low-income African-American families. Early followup studies with the clients of the Gautreaux initiative (for example, Kaufman and Rosenbaum, 1992; Rosenbaum, 1991; Rosenbaum et al., 1991) appeared to show striking evidence for neighborhood effects. Low-income African-American families had moved with housing assistance to largely White neighborhoods in both the city and the suburbs, but researchers later observed considerable differences in employment and education outcomes between those who had moved to the suburbs and those who had moved to the city.

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[1] The Gautreaux case was first filed in 1966. It resulted in a U.S. Supreme Court decision in the plaintiffs' favor in 1976, and the first of several negotiated settlement agreements occurred in 1981.

The lead plaintiffs' attorney in the Gautreaux case, Alexander Polikoff, approached HUD and its congressional appropriators with this evidence and broached the idea of replicating Gautreaux in one or more other cities. The idea, in general, was acceptable to both Congress and the George Bush Administration when framed in terms of poverty concentration rather than racial segregation.

Indeed, the leading social science thinking of that time (Kain, 1968; Wilson, 1987) emphasized nonracial aspects of the isolation of high-poverty neighborhoods as critical to their apparent negative effects. Kain's spatial mismatch theory pointed to the physical distance between the residents of such neighborhoods and the jobs for which they were qualified. Wilson's theory stressed the departure of the African-American middle class, after the Fair Housing Act of 1968, from ghettos to which open discrimination had previously confined them. As beneficial as that departure might prove to those who had left, Wilson held that the loss of role models and community leaders had severely affected those who remained.

HUD[2] proposed a rigorous experiment testing differences in outcomes between two groups of very low-income families with children, drawn from high-poverty neighborhoods in large metropolitan areas: one group that was offered counseling intended to result in a low-poverty subsidized rental placement (later known as the experimental group, or the low-poverty voucher group), and one group simply received a rent subsidy voucher with no such assistance (later known as the Section 8 group, or the traditional voucher group). Congress approved the study in the Appropriations Acts funding HUD operations in fiscal years 1992 and 1993 and authorized it in Section 152 of the Housing and Community Development Act of 1992, deviating from the HUD language only in narrowing the target population to residents of public and assisted housing in high-poverty census tracts and in mandating both short- and long-run reporting on the results.[3] The change in targeting was significant: it required greater cooperation from public housing authorities (PHAs) and project owners; furthermore, unsubsidized tenants in high-poverty neighborhoods have somewhat different incentives and might have different characteristics from their assisted neighbors.[4] The Appropriations Acts provided $70 million in additional voucher funding to support the demonstration.

HUD implementation of the demonstration required further decisionmaking. First, high- and low-poverty areas had to be defined. The high-poverty criterion—40 percent or more of the census tract population—was readily available from Jargowsky and Bane (1991). HUD chose as a criterion for target tracts that less than 10 percent of the population lived below the poverty line. HUD derived that standard from U.S. Census maps, which showed that very large portions of the landmass in most metropolitan areas were located in tracts meeting that specification, a fact that,

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[2] Shroder wrote most of the original legislative language.

[3] HUD Assistant Secretary John Weicher and Senate Appropriations staff member Bruce Katz negotiated the narrower targeting in a meeting in 1992, but neither remembers who proposed it. Both HUD Secretary Jack Kemp and Senator Barbara Mikulski of Maryland, Katz's principal, were committed to the orderly demolition of distressed public housing without reconcentration of poverty in other places.

[4] Giving a housing voucher to someone living in private-market housing is a fundamentally different "treatment" from giving a voucher to someone who is already living in public housing. The latter is already receiving a substantial housing subsidy, although the person has no choice about where to use it; the former is unsubsidized. For some consequences, see Jacob and Ludwig (2012) and Mills et al. (2006).

presumably, would enhance the possibility for successful placements.[5] In a competitive process requiring a joint application from a PHA and a nonprofit provider of counseling services, HUD selected five metropolitan areas as demonstration sites: Baltimore, Boston, Chicago, Los Angeles, and New York.[6]

HUD also had to determine whether the experimental group would face any special limitations in its use of vouchers. In general, geographic limitations on the use of vouchers would expressly violate the Housing Act of 1937. The authorization for the demonstration, however, could validly be interpreted as permitting deviation from that general rule. In 1993, HUD decided that, for purposes of testing the effect of neighborhood on families, it would limit the experimental group vouchers to low-poverty census tracts.[7] Without that decision, MTO would have been a test of the effect of counseling on achieving mobility to low-poverty neighborhoods. Without the constraints on use, however, many fewer low-poverty placements would have been in the sample.

HUD used an existing contract with Abt Associates Inc. to design and implement the experiment.[8] The first and most important issue for the contractor was how many random-assignment groups to create. Although $70 million for vouchers, in 1992 money, was a substantial support for a social science project, the number of vouchers available would be quite limited. The appropriations supported vouchers not for 1 year, which is current practice, but for 5; given inflation, that funding pattern effectively reduced the number of new families assisted by a factor of more than five. Moreover, families with children, in general, require larger units than the voucher program average unit size, and units in metropolitan areas, especially in better neighborhoods, require more subsidy because area rents are higher than the national average. Finally, the authorizing language required that some of the vouchers be used within the regular Section 8 program rules.

Given that HUD could not furnish large numbers of eligible families with vouchers in any case, Abt[9] argued that it was both ethical and scientifically necessary to have a control group that would receive neither a low-poverty nor a traditional voucher. The creation of a control group in addition to the two voucher treatment groups would allow for strong comparisons of the effect of neighborhood on low-income families with children.

Random assignment began in Baltimore in 1994 and concluded in Los Angeles in 1998. We leave the results of the experiment to other articles in this symposium. The remainder of this article concerns the following issues: (1) Was a full-blown randomized social experiment necessary, or

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[5] During implementation of the demonstration, families in the experimental group with large numbers of children were allowed to use their vouchers in somewhat higher poverty tracts.

[6] Shroder and Bill Murphy were the authors of the Notice of Funding Availability for the site competition. The selection of the 40-percent poverty standard proved not to be innocuous, because some metropolitan areas did not have large numbers of such tracts or large amounts of public and assisted housing in them. This standard worked against the selection of otherwise strong proposals from Seattle and Fort Worth, among others.

[7] The principal policymaker was Assistant Secretary Michael Stegman.

[8] Shroder and John Goering were the authors of the initial Statement of Work.

[9] Orr was Abt's principal investigator on the project.

could "natural experimental" or "quasi-experimental" studies have produced equally valid results? (2) What was unique about MTO? (3) Where do we stand today regarding the concentration of poverty? Are the issues the same or are they different?

Was a Randomized Social Experiment Necessary?

The use of administrative records about families placed in different neighborhoods on a quasi-random basis has led to claims, both in the Gautreaux program and elsewhere, that the experience of those families constitutes a "quasi-experiment" or a "natural experiment."[10] Unless these phrases are stretched well beyond their intended meaning, this claim is mistaken. Housing agencies in every case used by researchers to date have not kept data on the families who refused the placement, and the families who refuse placement in a "good" neighborhood are probably different from the families who refuse placement in a "bad" one. Consequently, even if the offer of the unit was made randomly, the placement was not.

Mendenhall, DeLuca, and Duncan (2006) reported known characteristics of the Gautreaux households that actually leased up at the time the offer of a city or suburban unit occurred. Exhibit 1 displays the results.

We can, with considerable confidence, rule out the possibilities that the Gautreaux placements were effectively randomized and that the two groups in the exhibit differ only randomly in observable measures. Of eight baseline measures, the study detected significant differences between the two groups in five at the 10-percent level and in three at the 5-percent level in the Aid to Families with Dependent Children sample. It also found significant differences in five measures at the 10-percent level and in four measures at the 5-percent level in the employment sample. The difference in

Exhibit 1

Nonrandomness of Assignment to Treatment in Gautreaux

Baseline Measures	AFDC Sample (N = 793)			Employment Sample (N = 1,258)		
	City	Suburban	p-Value	City	Suburban	p-Value
Number of children	1.98	2.06	.319	1.71	1.90	.005***
On AFDC	0.74	0.74	.911	0.68	0.70	.391
Years since move in 1990	5.97	5.26	.000***	6.62	6.32	.093*
Age of youngest child in 1990	10.42	9.76	.017**	12.97	12.33	.109
Moved from public housing	0.41	0.35	.060*	0.41	0.35	.025**
Mean family income in origin tract ($1,000s)	26.50	28.21	.057*	27.78	30.64	.000***
Percent non-Hispanic African American in origin tract	85.61	84.24	.445	84.66	81.56	.039**
Violent crime per 1,000 people in origin tract	23.47	25.99	.021**	21.99	23.32	.110

AFDC = Aid to Families with Dependent Children.

*Significant difference at the .10 level. **Significant difference at the .05 level. ***Significant difference at the .01 level.

Source: Mendenhall, DeLuca, and Duncan (2006)

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[10] In addition to the Gautreaux studies previously cited, other examples are Oreopoulos (2003) and Schwartz (2010).

observables is itself of no great importance from an evaluation perspective, because one can control for observables in multivariate analysis. Necessarily, however, one cannot control for unobserved differences that might also bias the analysis.

Differences that would normally be unobserved (because information about them is generally not collected) turned out to be important in MTO. Shroder (2002) analyzed the probability of a family in the experimental group making use of the offer of a low-poverty voucher. Exhibit 2 shows that uncertainty about liking a new neighborhood, level of comfort with sending a child to a nearly all-White school, dissatisfaction with the current neighborhood, and preferred distance from that current unit that the family head would like to move are all strongly associated, either positively or negatively, with actual lease up. These factors associated with placement might also be associated with other outcomes, such as employment and education, and could bias studies based on observational data. These attitudinal indicators would not normally be collected and, therefore,

Exhibit 2

Probability of Moving From High- to Low-Poverty Census Tract if Assigned to the Experimental Group

Variable	Coefficient	Standard Error
Metropolitan area vacancy rate	0.247***	0.073
Household size	− 0.258***	0.082
Number of school-age children	0.108	0.081
Number of preschool children	0.084	0.095
Hispanic head	− 0.387***	0.143
Uncertainty about finding an apartment	− 0.056	0.058
Uncertainty about liking a new neighborhood	− 0.194***	0.062
Belongs to a church within 15 minutes of origin project	− 0.045	0.117
Has many friends in current neighborhood	− 0.066	0.162
Comfort with children in nearly all-White school	0.243**	0.113
Housing condition at baseline	0.091	0.074
Dissatisfaction with current neighborhood	0.176***	0.066
Feels very good about moving	0.140	0.145
Preferred distance from origin project	0.181***	0.061
Head attended school last week	0.407**	0.180
Previously applied (Boston only)	0.657***	0.224
Hourly wage	0.013	0.026
Weekly hours of work	− 0.008	0.006
SSI/SSDI/SS survivor benefits	− 0.322**	0.132
Car or license	0.161**	0.075
Years in metropolitan area	− 0.011**	0.005
Intensity of counseling services	0.030**	0.012
Baltimore	− 0.504*	0.265
Boston	− 0.162	0.236
Chicago	− 0.818**	0.350
Los Angeles	− 0.429	0.296
Constant	− 1.629**	0.646

MTO = Moving to Opportunity. SS = Social Security. SSDI = Social Security Disability Insurance. SSI = Supplemental Security Income.

*Significant difference at the .10 level. **Significant difference at the .05 level. ***Significant difference at the .01 level.

Notes: N = 1,740. Logistic regression: dependent variable = 1 if family leases up. Reference group: New York MTO sample.

Source: Shroder (2002)

researchers could not control for them. Even with all the variables shown, however, the model in exhibit 2 correctly predicts only 65 percent of lease-up outcomes. We simply do not know all the factors that affect residential decisions for a given family, and asserting that what we don't know won't hurt us when we analyze other outcomes would be presumptuous. The value of a large-sample randomization is to ensure that these unmeasured or unmeasurable factors will balance out among the two treatment groups and the control group.

As long as tenants can refuse a unit that is offered to them, and housing authorities do not track the families that refuse units, it is difficult to say whether differences in the realized outcomes of families who accept random placements reflect the effect of neighborhood or merely the differing unmeasured characteristics of the families themselves.[11]

Readers can find additional evidence on the critical importance of randomization in Kling, Liebman, and Katz (2007) and Ludwig and Kling (2007). Their evidence is entirely different from what we present here but is entirely supportive of this point. For example, Ludwig and Kling wrote that applying nonexperimental techniques to MTO data yields evidence that crime is contagious—that is, bad examples from peers in bad neighborhoods tend to cause young men to commit crimes—but that after making use of the experimental design of MTO, they find "no evidence that contagion is as important as much of the previous research would suggest…" (Ludwig and Kling, 2007: 511).

In short, a randomized social experiment was necessary to estimate the effects of neighborhood on very low-income families with children. Nonexperimental techniques cannot solve the fundamental obstacles to valid inference.

What Makes MTO Unique?

MTO was remarkable in (1) the questions it asked, (2) the depth and scope of the effects it analyzed, (3) the range and quality of data sources it tapped, and (4) the long period over which it tracked participating families. Taken together, these attributes make MTO pathbreaking social policy research. In this section, we explore each way that MTO distinguished itself from other social research.

The Research Question

The question that MTO set out to address—What is the effect of neighborhood on low-income families?—is absolutely fundamental to our understanding of the nature of poverty. At least since the 1960s, various social scientists have hypothesized that concentrated poverty engenders a "culture of poverty" that encourages shortsighted, self-defeating behavior that traps low-income families in the underclass (see Harrington, 1962; Lewis, 1959). Although the culture of poverty is not synonymous with spatial concentration of low-income residents, such concentration would at least facilitate the social isolation that is advanced as a hallmark of that culture. Although the concept of a culture of poverty is controversial among social scientists, it is at least plausible that

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[11] If housing authorities both randomized placement offers and maintained good records on families who rejected them, the baseline situation would be equivalent to that of a randomized experiment.

the lack of community resources and successful role models in areas of concentrated poverty could undermine the motivation and aspirations of low-income families and limit their access to high-quality education, jobs, and other prerequisites of a healthy, prosperous life.

Nonetheless, the personal attributes of low-income people that may serve as barriers to success—for example, low education, lack of skills and job experience, poor health—would be present regardless of their residential location. The relative contributions of personal characteristics and residential environment to the poverty problem have been an unsolved riddle since the inception of the War on Poverty in the 1960s. Unlike most demonstrations, which seek only to test some specific approach to a problem, MTO set out to help us understand the problem in the only way we could hope to—with a rigorous experimental design capable of separating the effects of neighborhood from the effects of personal characteristics.

MTO did more than that, however. It also sought to measure the effectiveness of the two principal approaches that HUD had used throughout most of its history to address the poverty problem: public housing and rent vouchers. This ambitious combination of basic research and policy research objectives makes MTO unique among the major social experiments of the past 40 years.

The key feature of MTO that made it a test of the effects of neighborhood was restricting the use of the experimental group vouchers to low-poverty neighborhoods. As noted previously, without this restriction, MTO would have been simply a test of the addition of counseling to the traditional Section 8 voucher—a relatively unremarkable, if possibly useful, test much like many other experiments. With the locational restriction, MTO engineered a substantial difference in social environment for statistically matched groups of families—a circumstance that would never occur naturally.

The feature that allowed MTO to test both the effect of neighborhood and the relative effects of Section 8 rent vouchers and public housing was the random three-way assignment to the experimental group with locationally restricted vouchers, the Section 8 group with traditional vouchers, and a control group that remained in public housing. This design enabled the researchers to answer three distinct questions: (1) What is the effect of living in private housing in a low-poverty neighborhood relative to living in public housing in an area of concentrated poverty? (2) What is the effect of living in private housing in a low-poverty neighborhood relative to living in private housing in a substantially higher poverty neighborhood? (3) How effective are Section 8 vouchers, relative to public housing, in improving the lives of low-income people?

The Effects Analyzed and the Methods Used To Measure Them

The hypothesis that residential environment shapes the lives of low-income people implies that neighborhood effects may be felt across virtually every domain of life. A project that sets out to measure the effects of residential environment must therefore measure a very large number of potential effects of intervention. MTO measured the effects of neighborhood in six broad domains: mobility, housing, neighborhood, and social networks; physical health; mental health; economic self-sufficiency; risky and criminal behavior; and educational achievement.

To measure outcomes across all these domains, the study tapped a wide range of data sources: personal interviews with family members, interviewer observations, census data, audio recordings and

physical biomarkers (discussed further in the following sections), administrative data on earnings and arrest histories, data from several national databases on the characteristics of schools attended by MTO youth, and study-administered achievement tests in math and reading.

This problem of compliance with treatment is an issue in most random-assignment studies and, from the outset, was treated as a serious problem in this one. Most personal outcomes are measured with considerable noise—variation not systematically related to measurable factors—and small numbers of lease ups in the treatment groups would threaten the chances that analysts could detect any statistical effect of neighborhood. Abt allocated the families recruited into the sample among the two treatment groups and the control group to minimize the variance of the treatment-control comparisons, using initial lease-up assumptions that were periodically updated.

In all cases, the outcome measures used by the study were state of the art. Survey measures were taken from, or designed to be comparable with, those used in large-scale national surveys. For example, the MTO educational assessments were those used in the fifth- and eighth-grade followup waves of the Department of Education's Early Childhood Longitudinal Study, Kindergarten Cohort. MTO took survey-based measures of physical and mental health largely from the National Health Interview Survey, the Behavioral Risk Factor Surveillance System, the National Survey on Drug Use & Health, the World Health Organization's Composite International Diagnostic Interview, and other widely used survey batteries. The demonstration based measures of risky and criminal behavior on those used in the National Longitudinal Survey of Youth. Use of these established measures not only ensures that the MTO outcomes are based on well-tested interview scales, but it also allows for direct comparison of the MTO sample with national populations.

Perhaps the most innovative feature of the data collection was the use of biomarkers to assess physical health. As in the preceding interim analysis, survey interviewers measured respondents' height and weight during the home visit. In addition, for the long-term study, interviewers measured blood pressure and waist circumference (a better measure of obesity-related health risks than previous height- and weight-based measures) and collected blood spots from finger pricks. These blood samples enabled the researchers to detect the presence of uncontrolled diabetes and to measure high-sensitivity C-reactive protein levels, an important predictor of cardiovascular disease.

The MTO Followup Period

Given the large sample and broad scope of investigation, the MTO followup data collection was of unprecedented length.[12]

MTO enrolled families from 1994 to 1998. Abt conducted the interim impacts evaluation (Orr et al., 2003) followup survey in 2001, 4 to 7 years after random assignment.[13] Orr et al.'s (2003) analysis,

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[12] For context, Greenberg and Shroder (2004) reported longer term followups in the following cases: National Supported Work Demonstration, 8 years, sample of 6,600, administrative data only; New York Nurse Home Visitation, 15 years, sample of 400, survey and administrative data; Perry Preschool, 27 years, sample of 123, survey only; Carolina Abecedarian, 18 years, sample of 111, survey only. David Greenberg found these examples. Grinstein-Weiss et al. (2011) reported on an Individual Development Accounts experiment, 10 years, sample of 1,100, survey only.

[13] Other federal agencies and private foundations contributed to the interim data collection and analysis, responding to proposals primarily written by Jeffrey Kling.

based on this survey and a wide array of other data sources, provided what in most demonstrations would have been considered a long-term followup; few experiments are able to follow sample members this long.

HUD recognized, however, that the effects of neighborhood might not only be pervasive but also take some time to develop. Changing the behavior of families from impoverished areas might, for example, take prolonged exposure to a better residential environment. Teenagers who had lived most of their lives in lower poverty environments might also behave very differently than teenagers who had spent their early years in a high-poverty area before moving to a low-poverty neighborhood. For these reasons, one could not confidently conclude that the lack of a statistically significant effect on a given outcome 4 to 7 years after random assignment meant that the effect on that outcome would never be significant. On the other hand, strong interest persisted in determining how long some of the positive effects found in the interim impacts evaluation—for example, the improvements in adult mental health and obesity and the positive effects on girls' risky behavior and criminal activity—would persist. These considerations led HUD, with support from other agencies and foundations, to fund long-term followup data collection and analysis 10 to 15 years after random assignment. The National Bureau of Economic Research (NBER) conducted a final impacts evaluation (Sanbonmatsu et al., 2011) survey in 2009 and 2010, again supplemented by an array of other data sources.

Many low-income households are difficult to interview, either because of high mobility or for other reasons. Both the Abt and the NBER surveys employed a two-stage sampling process to ensure that the people actually reached were representative of the full sample. For example, in the main phase of the final survey, the Michigan Survey Research Center (the survey subcontractor) first obtained interviews with approximately 75 percent of the sample using standard respondent incentives and standard intensity of search by the staff. The researchers then randomly chose 35 percent of the remaining sample and substantially increased the respondent incentives and intensity of staff search for that subsample. Sanbonmatsu et al. (2011) then "weighted up" the respondents from the subsample to obtain the effective response rate; thus, if the first-stage rate was 0.75 and the second-stage rate was 0.6, the effective response rate would be 0.75 + (0.25 x 0.6) = 0.9. The effective response rates obtained in the final impacts evaluation survey were 90 percent for adults and 87 percent for youth (Groves et al., 2004; Sanbonmatsu et al., 2011).

If one considers the length of followup in association with the other elements enumerated and unenumerated previously—the rigor of the research design, the size of the sample (more than 15,000 individuals), the broad array of outcome measures collected, the remarkably high survey response rates, the difficulty and importance of the research and policy questions—MTO emerges as a landmark study.[14]

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[14] The continuity of support for the experiment is the more remarkable because it occurred over six administrations. HUD Assistant Secretaries for Policy Development and Research John Weicher, Michael Stegman, Susan Wachter, Al Trevino, Dennis Shea, Darlene Williams, and Raphael Bostic all provided support for the project.

Has the Problem of Concentrated Poverty Changed?

According to the Census Bureau, in 1990, 13.5 percent of the U.S. population lived below the poverty line. In 2010, the figure was 15.1 percent. It is mathematically possible for the concentration of poverty to fall while the poverty rate rises, but the popular perception is that the nation became more, not less, economically segregated over those two decades. That perception has a strong factual basis. "After declining in the 1990s, the population in extreme-poverty neighborhoods— where at least 40 percent of individuals live below the poverty line—rose by one-third from 2000 to 2005–09" (Kneebone, Nadeau, and Berube, 2011: 1).

Certain changes, however, might affect the way that policymakers view the urgency of the problem and their policy levers for affecting it. The crack cocaine epidemic has passed and, with it, some of the surge in homicides, although the danger of violent death lingers in many low-income neighborhoods. As of September 30, 2011, the HOPE VI Program had effected the demolition of 96,797 public housing units, including nearly all of the most notoriously ungovernable and deteriorating projects.

Perhaps these projects contributed to the assumption that the concentration of poverty was anchored in place. Exhibit 3 maps the percentage of poverty by census tract in five metropolitan areas— Boston (3a), an MTO site, and Denver (3b), Houston (3c), Minneapolis (3d), and Nashville (3e), which were not MTO sites—both in 1990 and over a 5-year period from 2003 through 2007.[15] In this exhibit, a tract with less than 10 percent of the population living in poverty is shown as white, a tract with more than 40 percent of the population living in poverty is black, and tracts in between are in graduated shades of gray. The number of tracts in low poverty shrank in all five metropolitan areas. Many of the high-poverty areas became gray, but the total landmass of high-poverty tracts did not decline. The concentration of poverty has often shifted from one place to another. Boston exhibits a reconcentration of poverty in the near southwest. Concentrated poverty has moved with marked centrifugal force in Houston. Some of Denver's concentrations have leapfrogged other neighborhoods in the shift to the east and northeast of the city center. The deep poverty south of the center of Minneapolis has shifted to other locales. Only in Nashville does the pattern of neighborhood poverty appear stable.[16]

If the concentration of poverty is a moving target, lumbering federal policy based on rapidly obsolescing data will have a hard time dealing with it. Orr et al. (2003) repeatedly noted that the variability of the poverty rate over time tends to diminish the strength of the mobility treatment. For example, "Because many [of the experimental group movers] moved to neighborhoods where the poverty rate was increasing between 1990 and 2000, we estimate that only about half of their destinations had poverty rates below 10 percent at the time of the move…" (Orr et al., 2003: viii), whereas, even among members of the control group who stayed in their origin project, about 21 percent were no longer living in tracts with more than 40 percent poverty.

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[15] Ron Wilson, a social science analyst in HUD PD&R, created these exhibits. With the elimination of the long-form decennial census, reliable poverty statistics at the census tract level depend on 5-year averages of American Community Survey data.

[16] "The population in extreme-poverty neighborhoods rose more than twice as fast in suburbs as in cities from 2000 to 2005–09" (Kneebone, Nadeau, and Berube, 2011:1).

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

a. Boston, Massachusetts

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

a. Boston, Massachusetts

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Shroder and Orr

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

b. Denver, Colorado

1990

Northglenn

Thornton

Westminster

Commerce City

Arvada

Wheat Ridge

Denver

Aurora

Lakewood

Englewood

Littleton

Percent in poverty
0–10
11–20
21–30
31–40
41–100

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

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Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

b. Denver, Colorado

2007

Northglenn

Thornton

Westminster

Arvada

Commerce City

Wheat Ridge

Denver

Aurora

Lakewood

Englewood

Littleton

Percent in poverty

0–10
11–20
21–30
31–40
41–100

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

c. Houston, Texas

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

c. Houston, Texas

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

d. Minneapolis, Minnesota

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

d. Minneapolis, Minnesota

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

e. Nashville, Tennessee

1990

Nashville

Percent in poverty
- 0–10
- 11–20
- 21–30
- 31–40
- 41–100

Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

Exhibit 3

Changes in Concentration of Poverty in Five Metropolitan Areas, 1990–2007

e. Nashville, Tennessee
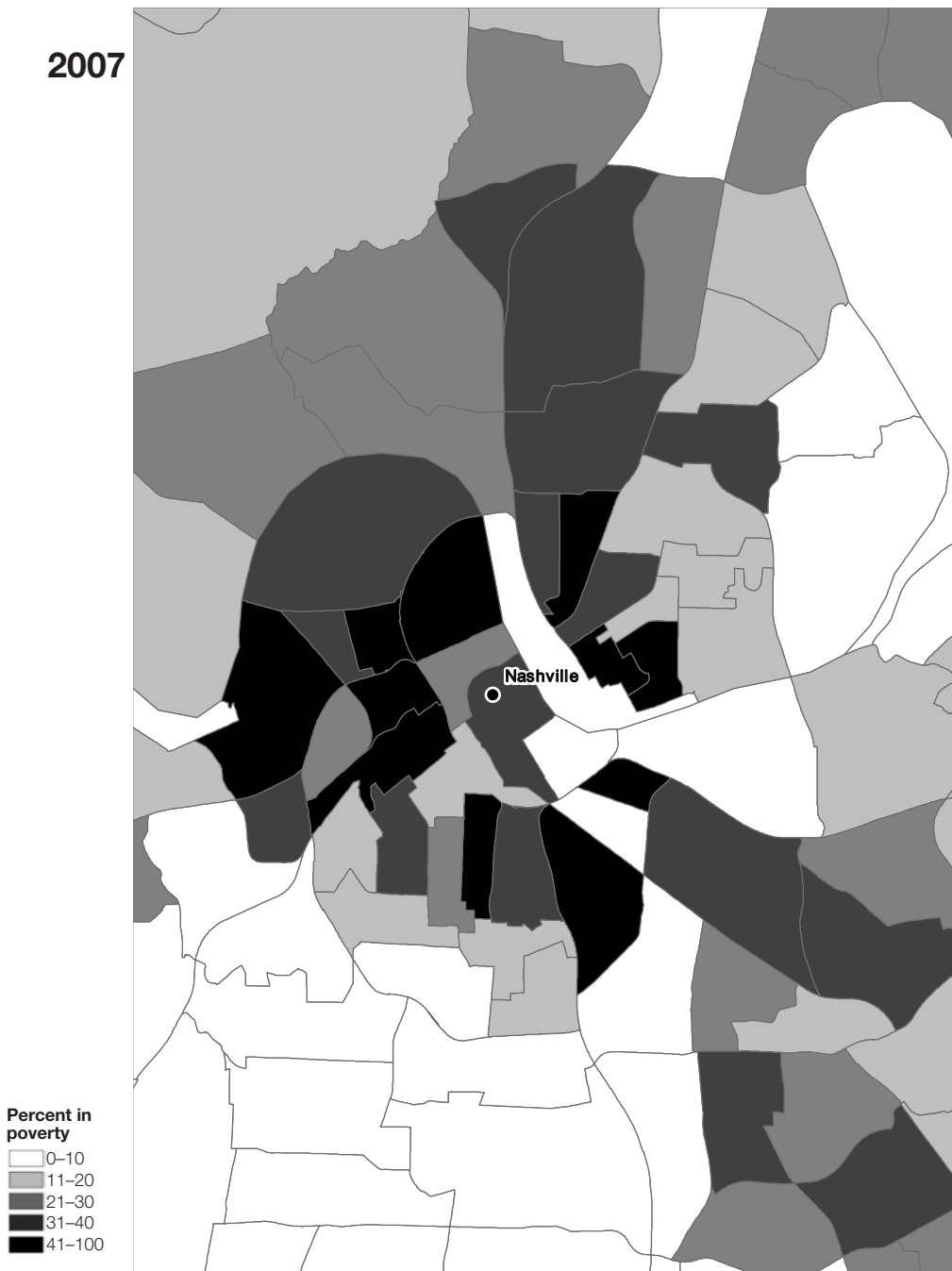
Sources: 1990–1990 Census; 2007–American Community Survey 5-year estimates

If the movement of poverty concentrations can undercut the case for a demonstration, the attenuation would likely be larger in a program that was to scale. Over the course of 5 years, the demonstration moved 813 families in the experimental group to low-poverty neighborhoods. Polikoff (2004) describes what a national program to scale would look like:

> Suppose 50,000 housing choice vouchers were made available annually, were earmarked for use by black families living in urban ghettos, and could be used only in non-ghetto locations—say, census tracts with less than 10 percent poverty and not minority impacted. Suppose that the vouchers were allocated to our 125 largest metropolitan areas. Suppose also that to avoid "threatening" any receiving community, no more than a specified number of families (an arbitrary number—say, ten, or a small fraction of occupied housing units) could move into any city, town or village in a year.

> If an average of 40 municipalities in each metropolitan area served as "receiving communities," the result would be—using ten as the hypothetical annual move-in ceiling—that 50,000 families each year, or 500,000 in a decade, would move "in Gautreaux fashion." Notably, *the 500,000 moves would equal almost half the black families living in metropolitan ghetto tracts* [emphasis in original]. (Polikoff, 2004: 1)

Note that Polikoff specifies that the receiving neighborhood would have to be "not minority impacted" and low poverty, a combined threshold that most experimental group moves would not have met, and that even with the lower standard in the demonstration just 47 percent of families in the experimental group moved to neighborhoods meeting the target criteria for their treatment. If one assumes that reducing the number of eligible destination neighborhoods would reduce the lease-up rate to just 40 percent, the Polikoff proposal would involve offering 125,000 families per year a location-constrained voucher and appropriate housing counseling services, with 50,000 of those families actually making use of the vouchers. This number would be 307 times the average annual number of experimental group placements.

If we can project forward from the MTO results, however, the dynamics of neighborhood change would imply that perhaps only 25,000 of the 50,000 families would live in tracts that actually met the desired criteria. Further, perhaps 5,000 of the 25,000 would have moved from "metropolitan ghetto tracts" in which poverty had subsided to less than the target level, so that just 20,000 of the 125,000 families receiving the offer would feel the full, desired effect of the program.

The program as described here would likely frustrate or infuriate many of the people involved in it. The quantitative and qualitative evidence from MTO shows that concentrations of poverty are harmful in certain measurable ways and consistently damaging in certain subjective ways. The concentrations do not stay in one place, however, posing a huge and unsolved issue in policy design.

Conclusions

We return here to the questions we posed at the beginning of the article.

Was a full-blown randomized social experiment necessary, or could "natural experimental" or "quasi-experimental" studies have produced equally valid results?

Neither "natural experiments," at least those that have been analyzed so far, nor observational data are capable of answering fundamental scientific questions about the effect of neighborhood on individuals and families. Families assign themselves to neighborhoods, and they do not do so randomly. Even if a PHA randomly offers subsidized placement units in higher and lower income neighborhoods, the families receiving the offers will not randomly accept or reject them, and, to date, researchers have not captured the identities, baseline characteristics, or subsequent experiences of the families who refuse the placements. Differences in their neighborhood choices are correlated with a variety of observable individual characteristics and with a variety of not-usually observed characteristics as well. On the basis of both MTO and previous research, we have every reason to believe that these choices are also correlated with characteristics that have never been measured, which may result in biases that we cannot ordinarily observe, predict, or control for.

What was unique about MTO?

The rigor of the research design, the size of the sample (more than 15,000 individuals), the variety of sites, the length of followup, the broad array of outcome measures collected, the high effective survey response rate, and the difficulty and importance of the research questions both to national policy and to social science made MTO unique.

Where do we stand today regarding the concentration of poverty? Are the issues the same or are they different?

Poverty has increased in the past 20 years, and so has the population living in concentrated poverty. It appears, however, that the demolition of the notorious projects has meant that the locations of concentration are driven less by the locations of project-based assistance than they used to be. It turns out that an implicit and unexamined assumption of the demonstration was that low- and high-poverty tracts would largely retain their low- and high-poverty status over time. Contrary to that unstated expectation, it appears that concentrated poverty often moves from one tract to another, while tracts that initially meet the criteria of neighborhoods of opportunity lose those criteria with surprising speed.

These apparent trends pose a perplexing problem in policy design. The mobility of low-income families who were not part of the demonstration weakened the treatment effect on the families in the treatment groups relative to the control group. There can be little question that the same mobility would have similar consequences for any replication of the experiment or any policy attempting to bring the experiment to scale.

MTO has provided invaluable insight into the ways in which neighborhoods do and do not affect individual outcomes. In the ongoing debate between place-based versus person-based mobility initiatives, however, MTO has yielded no final conclusion. The readers of this symposium are likely to come to widely disparate conclusions about what the logical next steps should be, in either policy or research. HUD welcomes those suggestions.[17]

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[17] This sentence is the only one in this article that is a statement of HUD policy.

We have noted some of our own doubts about the value of simple large-scale replication. Neither HUD nor most other government agencies can commit to research demonstrations of this scale and scope on a regular basis. In some ways, MTO is the kind of project that occurs not more than once in a generation. The authors and, we believe, nearly everyone else who has had a hand in MTO feel honored to have had some role in it.

Acknowledgments

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Achieving MTO's High Effective Response Rates: Strategies and Tradeoffs

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Abstract

The Institute for Social Research (ISR) at the University of Michigan successfully led an intensive, long-term, in-person survey for the Moving to Opportunity (MTO) for Fair Housing demonstration final impacts evaluation (Sanbonmatsu et al., 2011), achieving final effective response rates (ERRs) of 89.6 percent among MTO adults and 88.7 percent among youth, well above what response rates of surveys with comparable low-income populations have accomplished. A variety of survey field strategies ISR employed— careful staff selection, strategic use of financial incentives, and close collaboration between ISR and the National Bureau of Economic Research—all contributed to these high ERRs. The high costs associated with achieving high ERRs for in-person surveys like that employed in MTO raises questions about added value. Costs per survey interview nearly quadrupled during the last 4 fielding months. This extra investment increased the MTO adult survey ERR by only about 3.2 percentage points. A reanalysis of intention-to-treat estimates on selected outcomes suggests the merits of such an investment. If survey fielding had stopped at an 81-percent ERR for adults, we would have falsely concluded that MTO had no effect on two of four key health outcomes, that MTO had no effect on female youth mental health, and that MTO increased female youth idleness.

Introduction

As early as the 17th century, scientists observed that individuals who live in economically disadvantaged neighborhoods fare worse on a range of outcomes—from physical and mental health to employment and earnings, schooling, crime, and consumer bankruptcy filings—than individuals who live in economically well-off neighborhoods (Macintyre and Ellaway, 2003). Untangling whether neighborhoods per se, or the variety of characteristics of the individuals residing in particular neighborhoods, drive this observed association has been difficult. The question of whether neighborhoods matter is further complicated by the fact that we cannot always observe or measure the reasons why individuals decide to live in particular types of neighborhoods, and these very same reasons might be highly related to their outcomes. The Moving to Opportunity (MTO) for Fair Housing demonstration is a study uniquely positioned to contribute to our understanding of whether neighborhoods have causal effects on individuals' well-being. The MTO experiment produced changes in housing mobility and subsequent experiences in low-poverty neighborhoods that can help isolate the effects of neighborhood circumstances on outcomes from a host of other individual, household, or local community characteristics.

To maximize MTO's contribution to science and policy, the long-term survey for the final impacts evaluation[1] (Sanbonmatsu et al., 2011) had an ambitious data collection strategy that included a broad set of outcomes measured from administrative records sources and an intensive in-person survey, occurring up to 15 years after study entry, led by the Institute for Social Research (ISR) at the University of Michigan. The National Bureau of Economic Research (NBER) research team set very high response rate goals to ensure that survey data collection adequately represented the eligible MTO sample and captured a breadth of outcomes across the domains of housing, neighborhood safety, physical and mental health, employment, education, financial security, and youth risky behavior. ISR successfully reached a final effective response rate (ERR)[2] of 89.6 percent among MTO adults and 88.7 percent among MTO youth (ages 10 to 20 as of December 2007) in the long-term survey for the final impacts evaluation. These ERRs are much greater than what some studies of low-income populations have accomplished (Weiss and Bailar, 2002) and on par with several long-established and well-resourced survey initiatives such as the Panel Study of Income Dynamics[3] (American Association of Public Opinion Research, 2012; Gouskova, 2008; Groves et al., 2004).

Reaching such high response rates not only required substantial time investment from the NBER research team (to fundraise and design the survey) and financial commitment from the U.S. Department of Housing and Urban Development (HUD) and various other funders, but it also required creativity and flexibility among ISR staff to navigate and strategize in real time while

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[1] Research on MTO originally launched separately for each site, with a series of academic research investigators leading each site. The followup survey for the MTO interim impacts evaluation (Orr et al., 2003) that Abt Associates Inc. conducted was the first effort to administer a comparable data collection for MTO families overall. HUD also funded Abt Associates to canvass MTO families through 2007 to maintain an updated contact list.

[2] Ludwig (2012) describes the calculation of the effective response rate, which reflects the weighted proportion of interviews completed for the eligible adult and youth samples.

[3] The Panel Study of Income Dynamics (PSID) obtains response rates of 93 to 94 percent, and recent studies of PSID youth have response rates of between 87 and 91 percent.

interviewers worked in the field. Following and finding thousands of economically disadvantaged families who lived or currently live near resource-poor, potentially unsafe neighborhoods is a complex task. This complexity was compounded by the amount of time that had passed since the last in-person or phone contact with MTO study members and changes in MTO households—many of the youngest cohort at MTO study entry have since split off to create their own households. The previous in-person interview with an MTO study household member had been a minimum of 5 years before the start of the long-term survey data collection for the final impacts evaluation, and some of the sample (37 percent, or 3,830) had not been interviewed at the followup survey for the interim impacts evaluation (Orr et al., 2003), meaning the most recent contact may have occurred more than 10 years before the start of this data collection. In addition to facing the pure locational challenges of finding the eligible MTO survey sample in light of the high overall ERR aims, MTO researchers wanted to maintain balance in the temporal flow of completed interviews by site and by treatment status. Maintaining sample balance in this way required particular monitoring, nimbleness, and flexibility among ISR's data collection staff to target eligible survey sample members strategically, on a week-by-week basis, for extra attention from interviewers.

Some challenges that the ISR data collection staff faced for the MTO long-term survey effort are common to survey data collection efforts in general, but many were relatively unique or new to the experiences of ISR. The first sections of this article describe the various data collection design strategies ISR employed to maximize the probability of achieving the high ERR and strategies ISR implemented to address unanticipated challenges that protected, as much as possible, the quality of data and research design after survey data collection was in the field. These sections address factors that contributed to (and worked against) achieving a high response rate for both adults and youth.

Overall, the MTO in-person long-term survey for the final impacts evaluation was a reliable, efficient, and essential resource for capturing multiple aspects of life circumstances and individual outcomes that otherwise would have been difficult to capture at scale compared with lower cost alternatives. As one very poignant example, researchers would not have discovered MTO's surprising effects on mental health outcomes if not for a survey instrument with diagnostic questionnaires used to measure mental health disorders. The intensive efforts required to achieve the very high MTO response rates do raise questions, however, about the relative worth of extra resources necessary to complete interviews among those last, difficult-to-find respondents. If NBER researchers and the MTO study funders had spent fewer resources and stopped data collection at a lower response rate, what would the estimated effects on survey-based outcomes have looked like? In an attempt to evaluate the ex post scientific value of expending additional resources on increasing the survey response rate in a study such as MTO, the last section of this article describes the cost of MTO long-term survey data collection and a few back-of-the-envelope calculations of MTO's effects under varying response rate assumptions.

Background

The MTO demonstration began in the mid-1990s at five sites (Baltimore, Boston, Chicago, Los Angeles, and New York City). Low-income families with children living in public housing in highly disadvantaged areas who volunteered for the MTO program were randomly assigned to one of

three groups: an experimental group that was offered housing vouchers that had to be used in a low-poverty area along with mobility counseling from nonprofit agencies, a Section 8 group that was offered a traditional housing voucher with no locational restrictions, and a control group that was not offered a housing voucher but remained eligible for any public assistance to which they were otherwise entitled.

MTO long-term survey fielding launched in June 2008 and continued through April 2010, with a carefully staged release of sample by each of the initial five MTO sites across three waves,[4] with second-stage subsampling of the hardest-to-locate cases triggered at a predetermined initial response rate threshold of 75 percent. The ISR data collection staff conducted interviews in person, using a laptop computer and averaging 108.3 minutes for adult interviews and 116.7 minutes for youth interviews. The staff interviewed one adult and up to three youth ages 10 to 20 in each MTO family. In families with more than three eligible youth, ISR randomly selected three for inclusion in the sample. In addition to employing computer-assisted interviews, the data collection protocol included taking physical measurements, collecting dried blood spot samples from adults, and facilitating achievement assessments for youth.

Conducting an extensive and complex data collection effort with a highly disadvantaged and mobile population posed many challenges. First locating the family; then convincing respondents to participate; and finally completing a survey, physical measurements, and achievement assessments on multiple individuals in a single household combined to make this data collection operation extremely difficult. Some MTO families included foster children or youth who left home several years before the interview and had not kept in contact with other family members. Address information was often outdated or incorrect, and many families were living under the radar, without credit cards, mortgages, driver's licenses, or other identification that could help locate respondents. As a result, interviewers often conducted tracking with a labor-intensive, door-to-door search, checking address information and asking neighbors if they had information about where the respondent or family may have moved.

In addition to tracking challenges, the ISR data collection staff faced considerable challenges working in and around economically disadvantaged neighborhoods. The interviewing protocol required interviewers to carry a laptop computer and a large bag of supplies and equipment.[5] Many areas had no public parking, requiring interviewers to carry the equipment long distances in inclement weather and up multiple flights of stairs in highrise buildings. Interviewers often had to work in unsafe neighborhoods and conducted many interviews in suboptimal locations, including small and crowded living rooms, sometimes with no heat or electricity. Family members and friends coming and going, loud televisions and radios, and other distractions often made it difficult to maintain respondents' focus and confidentiality. Finally, ISR experienced staffing shortages in some areas because of the inability to recruit and retain qualified interviewers willing and able to

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[4] The first release of sample was in June 2008, the second in September 2008, and the third in February 2009, upon securing enough funding to survey a random two-thirds of Section 8 group adults.

[5] The combined weight of the laptop computer, equipment, and supplies needed for completing an interview was approximately 30 pounds.

work successfully under the challenging conditions MTO required. As a result, production progress across the five MTO cities was at times uneven and required adjustments in field protocols to take into account differences in completion rates by site.

MTO Survey Data Collection: Strategies for Optimizing Response Rates

ISR used a multifaceted set of approaches to address the previously listed challenges. This section discusses the ISR field structure and tracking efforts and the tools and strategies it used to maximize respondent participation.

Field Team Structure

In any study, building an effective field team is an essential element of successful data collection. ISR developed its field staffing model for MTO to take advantage of the clustered sample and to address the challenges of working with a sample that was highly mobile and lived in disadvantaged areas. The ISR data collection staff was made up of seven teams: a team of approximately 8 to 20 field interviewers in each of the five MTO cities, a team of Internet trackers supporting the field interviewers, and a travel team of field interviewers. The tracking team was composed of individuals who had Internet access to public records and were skilled at conducting Internet searches, networking, and piecing together information from many different sources. The travel team comprised experienced field interviewers with a demonstrated ability to work effectively and efficiently in the field and who lived in other parts of the country and were available to travel to interview respondents who had moved away from the main MTO cities. The travel team also supplemented data collection in cities that were understaffed.

A team leader, who provided coaching and monitored quality and production and efficiency statistics, supervised each team. A production manager in the central office worked with a production coordinator in the field to supervise the team leaders and provide guidance and direction for the field activities, lead training activities, and act as the main conduit for the flow of information between the central office and the dispersed MTO field team. Exhibit 1 depicts the field staffing structure.

Interviewer Hiring and Training

ISR recruited and hired all data collection staff—field interviewers, travelers, tracking experts, and supervisory staff. When recruiting staff for MTO, making sure the field staff members could work safely in low-income neighborhoods was important. The production manager and human resources specialist worked together with local interviewers already on staff at ISR to identify local advertising resources, such as neighborhood newspapers, community centers, libraries, and churches. The job posting identified some areas of the city in which MTO interviewers would work, clearly outlined the challenges interviewers would face working on this project, and emphasized the importance of the study. This emphasis on hiring local interviewers who were familiar with MTO neighborhoods and comfortable working in disadvantaged areas proved challenging (recruitment goals were not met in all areas), but it also yielded an exceptionally dedicated data

Exhibit 1

ISR Field Team Structure

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                              │      NBER       │
                              │  research team  │
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                    ┌──────────────────┴──────────────────┐
          ┌─────────────────┐                    ┌─────────────────┐
          │   ISR project   │────────────────────│   ISR field     │
          │    managers     │                    │ production managers │
          └─────────────────┘                    └─────────────────┘
                                                          │
                                                ┌─────────────────┐
                                                │ Field production│
                                                │   coordinator   │
                                                └─────────────────┘
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Baltimore team leader	Boston team leader	Chicago team leader	Los Angeles team leader	New York team leader	Tracking team leader	Travel team leader
Baltimore interviewing team	Boston interviewing team	Chicago interviewing team	Los Angeles interviewing team	New York interviewing team	Tracking team	Travel team

ISR = Institute for Social Research (University of Michigan). NBER = National Bureau of Economic Research.

collection staff who remained with MTO until the end of the project. Approximately 75 percent of the ISR interviewers were newly hired (some had previous social science interviewing experience with other organizations but were new to ISR). All of the field supervisory staff (team leaders, production coordinator, and production manager) were experienced staff members who had worked on a variety of other social science data collection studies at ISR.

ISR conducted two separate training sessions for field interviewers in June and September 2008. Although having two training sessions was not the initial plan, it turned out to be beneficial in many ways. The smaller groups enabled team leaders to work more closely with each interviewer, and the slower start to production in June provided a good shakedown period, during which ISR identified and resolved issues and made adjustments ahead of the later training. Having two recruitment and training periods also enabled ISR to capitalize on the enthusiasm for the project of the first group of interviewers to help recruit additional field interviewers. Finally, the ISR data collection staff trained during the first session shared valuable tips and tricks with the newer team members, helping to mentor new interviewers and bring them up to speed more quickly after training.

Retention of the field staff was higher for MTO than for similar ISR studies. Of the 91 interviewers and trackers trained and certified to work on MTO, 85 percent were still on staff 6 months after the start of data collection, and 74 percent remained on staff through the first year of data collection. Starting in June 2009, ISR began intentionally consolidating the interview sample and releasing some interviewers to work on other projects as the second-stage subsampling began and the available number of respondents was reduced. Even with this reduction in the second half of 2009, 50 percent of the field staff stayed with MTO through the end of 2009.

Communication

The central office project managers and production manager, in consultation with the NBER research team, set data collection goals and priorities for the data collection staff in the field. Each week, the ISR project managers met with the NBER research team to discuss progress, issues, requests from the field, strategies for improvement, and priorities. The priorities and action steps that emerged from this meeting provided direction for the following week's field activities. The close collaboration among the ISR project managers, the NBER research team, and the field supervisors on the data collection staff resulted in a common set of goals and priorities, which were relayed to the field teams through standardized weekly meeting agendas and reinforced in a weekly newsletter, continuous training sessions, and coaching for groups and individuals.

In addition, field supervisors on the data collection staff met weekly with the ISR project managers to review progress, learn of project updates and priorities from the NBER research team, and together develop a set of standardized agenda items for the weekly team meetings. Each of the seven data collection teams met weekly and reviewed the project agenda items and discussed team-specific issues and items. The ISR project managers produced a weekly newsletter that reinforced the training and discussions in the weekly meetings. The newsletter started as a tool to help connect the ISR project managers to the field, focusing on project updates and reminders from the ISR central office. It was adapted over time to include notes of thanks and congratulations, progress updates, training reminders, tips for effective interviewing, and more. Communication flowed from the data collection staff in the field to the ISR central office and NBER through weekly written field reports and discussions in weekly management meetings. This communication structure often led to adjustments or changes in procedures and materials based on recommendations from team leaders and interviewers. Communication was also very important from a safety perspective. ISR provided interviewers and team leaders with cell phones and asked them to let their team leader know when they were interviewing alone in dangerous neighborhoods, in unfamiliar areas, or after dark.

Team Interviewing

The use of team interviewing was another feature of the MTO data collection. Because the MTO protocol included interviewing multiple individuals in a family, ISR encouraged team interviewing, whereby two (or occasionally three) interviews were conducted in a household at the same time. This procedure had several benefits: it reduced the time burden on families when multiple interviews were conducted; improved interviewer efficiency; and reduced safety concerns for interviewers traveling into dangerous areas, especially for evening interviews. It also helped ensure confidentiality by keeping the parent occupied with her[6] own interview while the youth was being interviewed. Finally, it enabled experienced interviewers to mentor others, helped create strong local teams that supported each other, and fostered the sharing of strategies for effective data collection. In addition to participating in team interviewing, teams often worked together on tracking blitzes, in which all members of a local team would focus on tracking for a weekend, working closely with the tracking team to try to locate as many individuals as possible and set up interview appointments for the following week. Tracking was a difficult and often frustrating part of the process, and having the

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[6] Most adult respondents were female (although some male adults were in the sample).

entire team track respondents together and share success stories proved to be very motivating and an effective way to complete interviews. Likewise, interviewers who were effective at tracking could pair with those who were not as strong to help teach valuable skills and techniques.

Tracking and Locating Respondents

ISR integrated tracking into its data collection operations from the start of the project and developed sample management systems to facilitate both the interviewing process and tracking lost respondents. To help limit the number of contacts needed to locate each family member, take advantage of family connections for making multiple interview appointments, and locate respondents who had moved, ISR loaded all addresses (including those at study entry, those at the time of the interim MTO study, and updates obtained through the National Change of Address system and from the HUD database) into the sample management system for each family. ISR assigned all members of a family to one interviewer, making that single interviewer responsible for locating the family. The goals were to reduce burden on the family by using one point of contact and to gather information for multiple family members in a single contact. Using this procedure served to contain costs by working first with a family-locating instrument, which enabled the interviewer to contact any family member (or informant who knew where a family or respondent may be located) and update contact information for all selected members of the family. The family-locating instrument also gave the interviewer a view of the entire work scope for that family: the number of interviews to be completed at a single address and the ages of the respondents (which determined, for example, which youth achievements assessments would be administered). After the family instrument was complete, individual sample lines were released to the interviewer with contact information updated from the family instrument, enabling each individual survey respondent to be contacted, tracked if necessary, and interviewed. Often, interviewers were able to set up the appointments for interview sessions (and arrange for team interviewing) when working with the family instrument.

In addition to taking advantage of family connections to help locate respondents who had moved since the last interview, the data collection staff included a tracking team that was integrated into the five local interviewing teams (one at each MTO site) and the travel team. Although all trackers could and did work sample lines from any location, having one primary tracker working with each local interviewing team enabled the tracker to establish strong working relationships with the field interviewers and to become very familiar with the geographic region in which they were working. The tracking team used a combination of pay-for-service Internet search engines that pulled from public records and free tools, such as reverse phone searches and white page telephone directories, coupled with extensive telephone networking with family members and contact people to generate leads for the field interviewing team to follow in person when searching for lost respondents. Although telephone and Internet tracking was effective for many cases, a substantial amount of in-person tracking was required to reach the response rate goals. Often, the tracking team would identify a potential address, and the local interviewer would visit the address to find that the person no longer lived there. The local interviewer would then check with neighbors to gather information about where the respondent had moved.

This process sometimes generated additional leads that were then passed back to the tracking team for further Internet searches and telephone work. For example, one fieldworker shared a

tracking story about working with an interviewer, "Tim," who was traveling in a particular city trying to locate an address where the respondent reportedly owned a house. The first attempt to find the address was unsuccessful because GPS, maps, and the initial in-person visit revealed only the 300-to-500 block of the street name, and the respondent's address was in the 1400 block. Tim e-mailed the tracker to report the finding, and the tracker double-checked the property records and determined that the respondent's residence was in the 1400 block of the street in question. The tracker suggested that Tim check with the post office or a municipal office to see if the road continued elsewhere. Another tracker suggested checking with the fire department, which led the interviewer to a new subdivision where he found the address and the respondent. Teamwork and persistence paid off, and the team completed the interview.

Some of the more difficult respondents to locate included youth who had run away or left home without keeping contact with family, respondents intentionally living off the grid, and institutional-ized respondents (in nursing homes, detention facilities, and so on). Trust was an issue for some MTO respondents; interviewers required a great deal of persistence to convince some family members that the family was not going to be reported to the authorities. The data collection staff was very successful in getting family members or friends to ask the respondent to call the centralized ISR toll-free line or agree to meet in a public place (such as a library or fast food restaurant) to explain the study and set up an appointment for an interview. The toll-free line forwarded calls directly to the interviewer's MTO cell phone, which was very helpful because the difficult-to-reach respondents often did not have a phone or had a very limited number of cell-phone minutes. If a call was missed, the respondent may have run out of minutes or would no longer be near a tele-phone by the time the interviewer was able to return the call. Over the course of the project, the data collection staff tracked respondents who were in jail or detention facilities, noting their release dates and keeping in touch with family members to enlist their assistance in setting up an interview as soon as possible after their release. Some respondents did not speak English or Spanish (the two languages in which the survey instrument was available), requiring translators to help locate and interview family members.

Advantages to Real-Time Tracking

The tracking team used a real-time, web-based sample management system that provided a history of all searches and telephone contacts and enabled trackers to work together to locate particularly difficult-to-find respondents.

At times, the interviewer in the field would visit an address, find it vacant, and telephone the tracker, who would identify another lead or the name of a neighbor while the interviewer was still in the neighborhood. This close collaboration between the tracking and interviewing teams was especially helpful in avoiding the expense of a return trip when travelers were working in non-MTO cities.

Interviewers offered small monetary incentives (finder's fees) of $5 to $10 to contact people who provided the interviewer or tracker with information on the location of a difficult-to-find respondent. ISR added the finder's fees late in the study, with some limited success. Near the end of the project, finder's fees rose to $50 as interviewers worked to find the last of the hardest-to-locate respondents.

Nearly one-half (46 percent) of MTO families required tracking by the tracking team. Even after someone in the family was located, tracking did not stop. A total of 9 percent of sampled individuals (adults and youth) were referred to the tracking team after the family had been located. ISR was able to find and interview 48 percent of respondents referred to the tracking team. The remaining 52 percent were divided among refusals (5 percent), final noninterviews—such as respondents in institutional settings like jails or on military duty abroad—who were not reasonably accessible to the data collection staff (23 percent), and respondents who were randomly excluded from the sample as part of the second-stage subsampling design (24 percent).

Strategies To Maximize Respondent Participation

After locating the respondent, the interviewer's next challenge was to convince each selected participant in the MTO family to agree to complete an interview (and convince parents to give consent for their children to participate). The financial incentive was by far the most effective tool for obtaining respondent participation. Interviewers offered each respondent a $50 cash payment at the time of the interview and offered adult respondents a $25 cash payment for agreeing to provide a dried blood spot sample.

ISR implemented additional monetary incentives throughout the data collection period to help meet specific project goals. Consent to audio record the interviews was especially important for MTO, which used audio recordings for quality control checks and for questionnaire items designed to measure the effect of MTO on language patterns. Early in the project period, audio recording consent was lower than projected. In addition to working with interviewers to improve their persuasion skills and increase the number of respondents selected for audio recording, in December 2008, ISR added a $10 cash incentive for respondents who consented to having their interview recorded. These efforts resulted in an increase in the overall audio consent rate by 10 percentage points, from 74 to 84 percent.

End-Game Strategies

ISR used several strategies at the end of the data collection period in an attempt to obtain the last few interviews needed to raise the response rate to the desired level. We offered additional cash incentives to respondents and interviewers, implemented two-stage subsampling to enable the interviewers to focus limited resources on a smaller number of cases, and extended the study period to allow more time to locate lost respondents and convince reluctant individuals to participate.

As mentioned previously, hiring shortfalls and attrition led to some cities having a smaller data collection staff than planned, with the result being that completion rates (and response rates) varied by site. Rather than implementing end-game strategies at one point in time for the entire sample, ISR added the end-game strategies and incentives on a city-by-city basis.

The survey fielding design employed two-stage subsampling to obtain responses from a representative subsample of hard-to-locate respondents. In stage 1, the ISR data collection staff attempted to contact and interview all the adults and youth in the sample frame. When the response rate at each site reached approximately 75 percent, the team selected a random subset of 35 percent of the remaining cases for more intensive interviewing efforts during stage 2. When calculating the ERR

and analyzing the survey data, respondents interviewed as part of stage 2 received an additional weighting factor so that they represent the other hard-to-reach respondents who were not selected for stage 2 and that ISR did not attempt to interview.

ISR implemented end-game cash incentives in each MTO city based on the completion rate for that city's sample and coupled subsampling with an increase in respondent incentives. ISR increased the interview incentive by $25 once and then a second time, bringing the incentive offer up to $100 per interview (plus $25 for adults for the dried blood spot sample and $10 for audio recording). When ISR first added end-game incentives in each city, the data collection staff had flexibility in offering the incentives. The interviewer and the team leader discussed each case and decided which cases they were most likely to complete if they offered the additional financial incentive. For the final 2 months of data collection, interview incentives increased to $200 as the ISR data collection staff attempted to convince the most reluctant and elusive respondents to participate. The final phase of the end-game effort also included two additional options: completing the interview by telephone or completing a shorter version of the survey for a smaller incentive ($100).

The response rate for respondents to whom interviewers offered an end-game incentive was nearly identical to the response rate for respondents whom interviewers contacted during the end-game period of the study but to whom the interviewers did not offer the end-game incentive. Among respondents who had initially refused to do the interview, 49 percent of those who were offered an end-game incentive and 45 percent of those who were not offered the incentive completed interviews. These findings may reflect the fact that the team leaders used the end-game incentive offers very judiciously, encouraging interviewers to do their best to complete the interview without offering the additional monetary incentive if possible. By the time the end-game incentives were in place, the data collection staff was very experienced and had built up a large toolkit of effective introductions, and their tracking and persuasion skills were well refined.

One aspect of the end-game incentives apparent during the field period was the motivation that they provided to the interviewers. Having something new to say when calling respondents made it easier for interviewers to make additional contact attempts with respondents who had been avoiding them for months. ISR also gave interviewers the flexibility to offer small nonmonetary incentives, such as a small plant or gift, as part of the end-game strategy. These alternative incentives were sometimes effective in helping the interviewer gain access to the respondent's home, thereby enabling the interviewer to pitch the study to a family member or the respondent in an attempt to elicit participation from a reluctant respondent.

Strategies To Retain and Motivate Data Collection Staff

In addition to making efforts to select interviewers who were well suited to the type of interviewing MTO required, ISR focused considerable effort on maintaining staff morale and motivation over the course of a nearly 2-year data collection period. Successful strategies included the continuous efforts of the field supervisory staff, frequent communication among teams and between the ISR central office and the field staff, and a variety of monetary and nonmonetary incentives offered to interviewers and team leaders throughout the data collection period. The direct involvement of the ISR project managers and the NBER research team, along with the importance of the study topic, provided a great deal of motivation to the field staff and helped reduce attrition despite the many

challenges the study presented. The field supervisors of the data collection staff and ISR project managers incorporated continuous interviewer training into all field activities, including standardized training topics in the weekly team meetings, one-on-one coaching with team leaders, working in pairs, and sharing tips and tricks with each other.

One especially motivating technique used during the MTO long-term survey was having the NBER research team meet with the data collection staff. The interviewing and tracking teams met in person in each of the five MTO cities, when possible, and also used telephone conference calls to help contain costs. During the meetings, teams discussed the purpose of the study and progress to date, and, in later months, the NBER research team shared some preliminary demographic information or other simple results with the interviewers. The meetings also included time for discussion: the interviewers provided observations and stories from the field, asked the research team questions, and gathered information that they used in their introductions to help convince reluctant respondents of the importance of the MTO project. The field teams greatly appreciated these meetings, which often resulted in changes to materials or procedures based on interviewers' comments and suggestions. The fact that the research team took the time to meet with—and listen to—the interviewing team was a very positive factor in maintaining high morale and commitment in the field.

ISR offered incentive programs for interviewers at several points in the study. At the end of each calendar year, ISR distributed production bonuses based on productivity and efficiency. In March 2009, ISR held a March Madness competition. The interviewers did not receive any additional compensation, but ISR project managers made a donation to a local food bank in each city based on the number of interviews completed during the month of March. ISR included fun, sports-inspired progress updates in weekly newsletters. This challenge led to a big spike in production and engendered a great deal of team spirit, with the teams adopting names (for example, the New York "Hard Knocks" and the Chicago "Terminators") and team leaders even engaging in some good-natured competitive trash talk on weekly conference calls. Later in the study period, ISR offered interviewers bonuses for completing high-priority interviews ($15 for each high-priority interview completed and $20 if the interview had previously been coded as reluctant). In 2010, as the study was winding down and a very small number of cases remained to work, ISR offered remaining staff a retention bonus for staying on the project and a weekly incentive if they met goals for number of hours worked, followed the work plan for tracking, and made a specified number of contact attempts during the week.

In addition to offering monetary and nonmonetary incentives, ISR provided training opportunities for interviewers and team leaders to help the data collection staff remain productive and effective as the study progressed and as completing interviews became increasingly difficult. Team meetings and newsletter entries provided techniques for convincing reluctant respondents to participate, tracking strategies, and tips for successful interviewing. Newsletters also highlighted success stories, including naming staff to the "Century Club" when they completed 100 interviews and a weekly "kudos" segment recognizing individuals and teams for their efforts. All incentive programs included team leaders. The positive attitude of the team leaders was contagious, and their leadership was a big factor in the success of the field effort. In addition to including team leaders in the incentive programs, ISR developed a professional development series for team leaders, offering seminars on topics such as communication, motivating and getting the best from a team, and work/life integration (recognizing that working from a home office can be very challenging).

Coordination Between NBER and ISR

Throughout the preproduction and data collection periods, the ISR project managers and the field supervisors of the data collection staff worked very closely with the NBER research team, which helped ensure that all parties clearly understood goals, priorities, and the realities of fieldwork. When issues arose, the ISR project managers, field supervisors of the data collection staff, and NBER research team members discussed options and made joint decisions that best met the project needs within the limits and constraints of time, available resources, cost, and quality.

In addition to participating in weekly meetings and countless telephone calls and exchanging e-mails, ISR and NBER developed a wide variety of reports to facilitate the close monitoring and coordination of the field data collection effort. Statistical reports displayed, in tabular and graphical form, information about project cost, data quality, consent rates for components (for example, audio recording or blood spot collection), and completion and response rates. ISR modified the reports over time to better meet the needs of the ISR project managers, field supervisors of the data collection staff, and NBER research team members. This information exchange enabled the data collection staff to identify areas needing additional training or encouragement in the field and to develop programs to meet those needs. The field supervisory staff wrote weekly qualitative reports, providing context and stories from the field that helped to complete the picture of how data collection was progressing.

The ISR project managers, field supervisors of the data collection staff, and NBER research team members carefully monitored completion and response rates across cities, treatment and control groups, and respondent type (adults and youth) throughout the project. Such rates differed for a variety of reasons. Differences in staffing levels led to differential completion rates across sites (for example, the Section 8 group adult sample was released midway through the data collection period [February 2009], when additional funding was obtained) and completion rates naturally differed across the three groups. When differences in completion rates began to appear, ISR added a priority flag to the data collection protocol. This flag was a designation given to selected respondents, with the goal of improving sample balance in completion and response rates. ISR project managers instructed the field team to arrange its work so that team members called and tracked high-priority cases more aggressively, with the goal of completing as many high-priority interviews as possible. As the study progressed and all interviews became more difficult to complete, ISR added a monetary incentive for each high-priority case that was completed as an interview.

The weekly newsletter shared production statistics for the overall project with the data collection staff, and the weekly team calls reviewed team production and efficiency statistics. The NBER research team was fully engaged in monitoring production and worked closely with the ISR project managers and field production managers to review progress, set priorities, identify issues, and develop and implement solutions to problems as they arose.

Survey Data Collection Costs

The ISR project managers and NBER research team spent a great deal of time discussing response rate goals and the options for end-game strategies to reach the NBER research team's ambitious goal of 89-to-90-percent ERRs while staying within budget. These debates often took the form of assessing tradeoffs between the high per-interview cost of achieving the last few response rate

percentage points and the benefit to the MTO long-term survey of decreasing nonresponse bias in MTO's estimated effects. When looking at costs, ISR project managers focused on two components: (1) the variable costs associated with obtaining each interview (interviewer hours and nonsalary charges such as mileage, respondent incentive payments, and so on) and (2) fixed costs (for example, for the ISR central office staff to support data collection, project management, creating and checking statistical reports, writing progress reports and weekly memos) that were less dependent on the number of interviews being completed each week. Although the central office staff size decreased along with the field staff size as the study neared completion, the fixed cost per interview increased at the end of the study because those costs were amortized across fewer and fewer completed interviews.

Exhibits 2 and 3 show relatively distinct fluctuation points when survey data collection costs escalated. Exhibit 2 presents average costs per interview throughout MTO survey fielding. Note that these costs are not necessarily real-time costs, because of the delay between the actual interviews and when those costs were entered from an accounting perspective. Nonetheless, average costs per interview were quite steady at about $470 from July 2008 through July 2009, at which point most of the fresh sample from all three releases had been worked relatively thoroughly. Average interview costs increased substantially in the fall of 2009, to $802 per interview from August through October 2009 and to $1,076 per interview for November and December 2009. These costs are not surprising in light of the anticipated work it would take to complete interviews with

Exhibit 2

MTO Long-Term Survey Interview Costs by Month
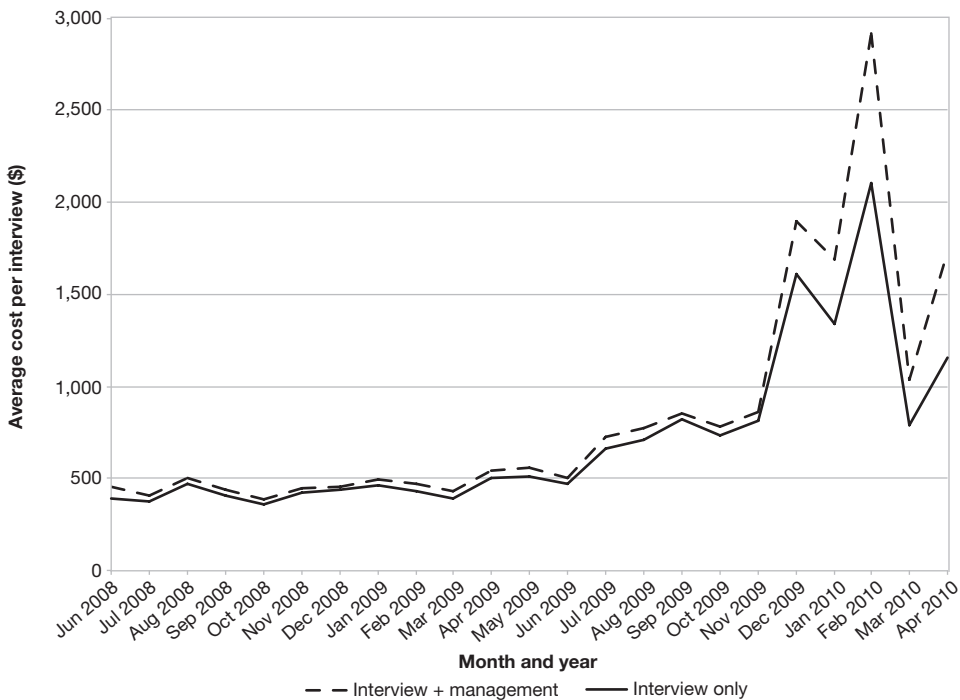
MTO = Moving to Opportunity.

Exhibit 3

MTO Long-Term Survey Interview Hours by Month
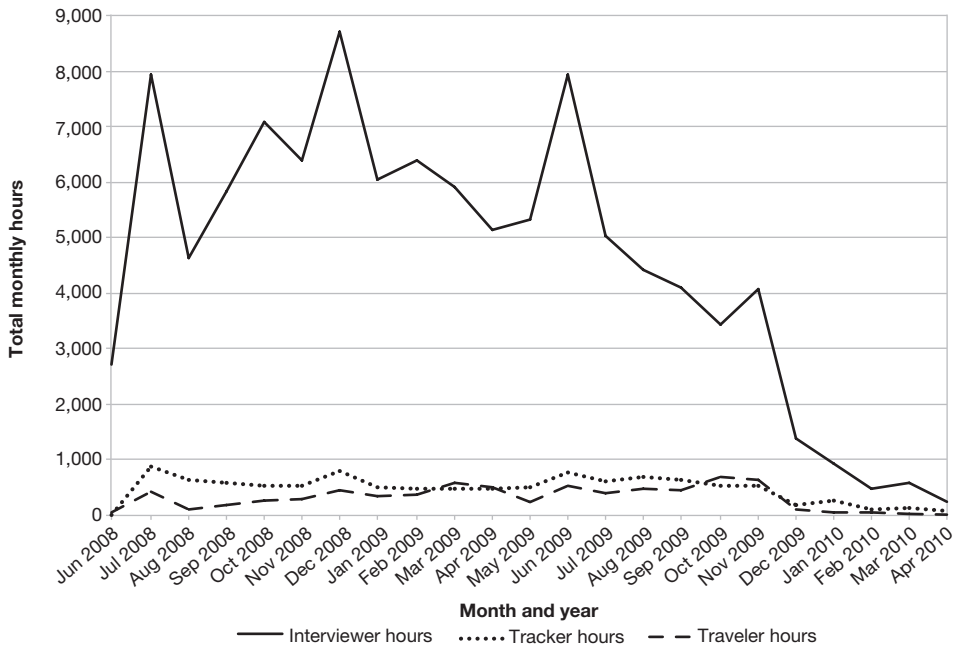
MTO = Moving to Opportunity.

the hardest-to-locate cases. The two-stage subsampling strategy was in place for each site at this point. Average monthly costs per interview escalated starting in December 2009 to about $1,600. Exhibit 3 presents an alternative but key metric of costs: total interviewer hours and hours spent by travel interviewers (that is, interviewers who were shared across sites or called on duty to travel to zones outside of the immediate area of the five initial MTO sites) and trackers (that is, those ISR staff who located sample using a variety of web, in-person, and alternative techniques, as described previously). Notably, December 2009 represented a key point at which ISR project managers, field supervisors of the data collection staff, and the NBER research team very seriously evaluated the costs and benefits of continuing survey data collection. October 2009 represents another cost flex point, at which time ISR had implemented the two-stage subsampling strategy at many of the sites.

As previously mentioned, the team had to balance meeting the high effective overall response rate with creating a completed survey sample that was balanced by site and by treatment status. Exhibit 4 shows how costs and survey completion rates varied by site. Baltimore had an accelerated survey interview completion rate in terms of reaching ERR targets, whereas Boston and Los Angeles had slower completion rates, in part because of field staff constraints. This exhibit exemplifies the site-based balancing act that ISR project managers and field supervisors of the data collection staff considered in attempting to meet the overall ERR target. This tension in approaches to meet an overall high ERR target, whether via triaging by working the hardest sample for each site or via focusing efforts on one site at a time, was a key balancing act that, as we will discuss in the following section, influenced the study's main findings.

Exhibit 4

MTO Long-Term Survey Interview Hours by Month: Baltimore, Boston, and Los Angeles
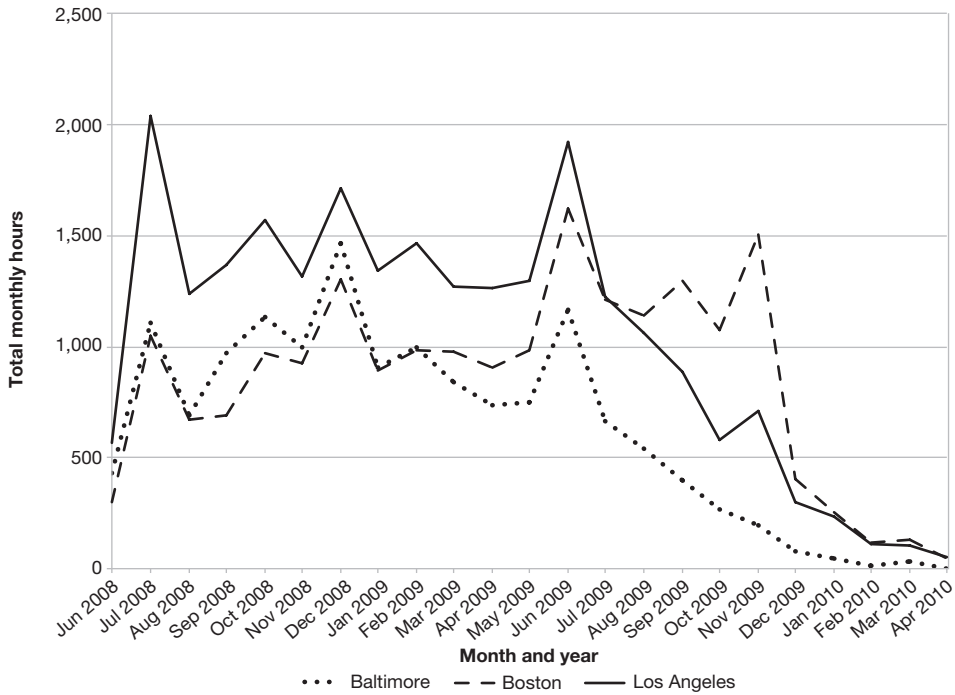
MTO = Moving to Opportunity.

MTO's Effects Under Varying ERRs

The average cost per interview from the beginning of survey fielding (June 2008) through October 2009 was about $500. As mentioned previously, average interview costs jumped substantially thereafter: to almost $1,100 in November and December 2009 and to more than $1,600 from December 2009 through April 2010, when survey fielding ended. The adult survey ERR increased by 5.3 percentage points (285 interviews) between October 2009 and December 2009 and by 3.2 percentage points (97 interviews) between December 2009 and April 2010. This increase roughly translates to $58,000 per 1-percentage-point gain in ERR (calculated as the cost per interview plus the number of interviews completed) between October and December 2009 and $49,000 per 1-percentage-point gain in ERR between December 2009 and April 2010. Did the additional dollars spent toward gaining 1 extra percentage point in the ERR measurably or qualitatively alter the main conclusions in the final impacts evaluation (Sanbonmatsu et al., 2011)? We do not conduct a formal cost-benefit analysis but rather employ a simple back-of-the-envelope comparison of survey data collection costs at various time points during survey fielding and with a small number of metrics to attempt to capture the benefit via the MTO demonstration's contribution to research and policy.

For our back-of-the-envelope analysis, we focused on two representative thresholds that align with observable fluctuations in survey data collection costs: October 2009, when the survey achieved an 81-percent ERR for adults (80 percent for youth), and December 2009, when the survey achieved an 86-percent ERR for adults (85 percent for youth). These ERRs, and the associated dates when they were achieved, also have general appeal. First, we did not want to confound our analyses with the cost efficiencies gained through the two-stage subsampling strategy to allocate more resources per a randomly selected, hard-to-locate case that was triggered at roughly a 75 percent ERR. Second, the ERRs at these cut points generally represent the range of ERRs normally achieved in a wide variety of survey data collection efforts (80 to 90 percent). We use these cut points as simulated dates at which survey fielding ended to construct new samples to reestimate MTO's effects and to examine whether a qualitative difference emerged in three factors: the size of the intention-to-treat (ITT) estimate, the precision of that estimate, and the depiction of the control group. These proposed metrics are of scientific and policy interest; that is, they help inform the following questions: Would our description of the status of the sample have changed had we ended the survey fielding period early? Would our confidence of MTO's effect have changed? Would our interpretation of the program or policy influence on the outcome of interest have changed?

We reanalyzed MTO's effects (for more explanation about the ITT and treatment-on-the-treated [TOT] estimates, see Gennetian et al., 2012; Ludwig, 2012; and Sanbonmatsu et al., 2012) under varying ERR scenarios—those mapped with the December 2009 and October 2009 cut points—in the following manner. First, we replicated the MTO ITT and TOT results for the outcome of interest for the completed MTO long-term survey. Recall that the final ERRs were 90 percent for adults and 89 percent for youth. We then compared the full-sample ITT and TOT estimates with ITT and TOT estimates reanalyzed using the following strategies: (1) using data from the completed pooled sample as of December 31, 2009, reflecting an overall 86-percent ERR for adults (85 percent for youth), and (2) using data from the completed overall sample as of either December 31, 2009 (when some sites, such as Baltimore, achieved something greater than 86-percent ERR), or the date at which the site achieved the equivalent ERR of 86 percent for adults (85 percent for youth). Strategy 2 recognizes the heterogeneity in ERR completion rates by site, whereas strategy 1 is relatively agnostic about site and instead focuses on the pooled ERR. The ERR target for the MTO long-term study was set for the entire MTO survey sample, with an important but secondary target to have a relatively representative sample from each site. In reality, ISR's site-based field staff strategy, coupled with other factors—such as difficulty recruiting or retaining interviewers and the relative ease of finding sample for geographic or comparable reasons—meant that some sites achieved ERR targets faster than others. The variation in site-based survey data completion rates also implies that the date of the last completed interview will vary by site.[7] We replicated strategies 1 and 2 under a

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[7] Additional analyses that created a sample based on these ERR targets within treatment or control group did not uncover qualitative differences from the final sample results. This result was expected, in part, because by construction, the NBER research team and ISR project managers carefully monitored temporal balance by treatment or control group; that is, that roughly equivalent interviews were being completed for experimental, Section 8, and control group members in any one week or month and, if that was not the case, adjustments were made in real time to achieve this balance by flagging and prioritizing work on selected respondents.

slightly altered assumption of stopping survey fielding as of October 31, 2009, reflecting an overall 81-percent ERR for adults (80 percent for youth). Note in this latter case that Boston, New York, and Los Angeles were just shy of achieving the 81-percent ERR by October 2009.

Exhibits 5, 6, and 7 visually present results for a few of the outcomes and are the focus of our discussion. (Exhibits 8, 9, and 10 at the end of the article provide more detail.) The results shown in the exhibits suggest that the final MTO findings would have differed qualitatively for some of the important outcomes if survey fielding had stopped earlier. For example, if survey fielding had stopped at 81 percent for adults—either pooled or by site—the NBER research team would have falsely concluded that MTO had no effect on two of the four health outcomes. Of the four survey outcome measures for female youth, we would have falsely concluded that MTO had no effect on female youth mental health and that MTO increased female youth idleness (neither employed nor in school).

When examining MTO's effects on neighborhood poverty, exhibit 5 (and the first outcome in the more detailed exhibit 8) suggests little qualitative difference in MTO's effects across the various ERR assumptions, either through the size of the effect, the precision of the effect, or the description of the control group. Turning to MTO effects on other outcomes, exhibits 6 and 7 illustrate a slightly different pattern of results. MTO effects on adult psychological distress are very slightly larger (that is, larger reductions in psychological distress) at an 86-percent ERR. The differences are magnified when comparing MTO's effects for the final sample (90-percent ERR) with an 81-percent ERR. The difference between the analyses at the 86-percent ERR and at the 81-percent ERR is especially pronounced for the within-site ERR adjustments; if survey fielding had stopped when each site reached an 81-percent ERR, MTO's effect on adult psychological distress, at -0.128, would have been 21 percent larger than the effect for the final sample, at -0.107. On the other hand, if fielding had stopped at an overall ERR of 81 percent, with variation in ERR by site, MTO effects would have been qualitatively very similar to those estimated at the final 90-percent ERR. Exhibit 7 (and the fifth outcome in the more detailed exhibit 9) suggests a similar, yet even more striking, pattern for female youth: MTO's effects on female youth psychological distress, at -0.116 for the full sample (89-percent ERR), is qualitatively larger and more precisely measured than estimates measured at overall ERRs of 85 percent (-0.084 ITT) or 80 percent (-0.050 ITT).

Thus, taking the female youth psychological distress outcome as a starting place, the roughly $460,000 expended to achieve the last 8.7 percentage points in ERR (between November 2009 and April 2010) translated to a 43-percent difference in the effect estimate.

Discussion and Conclusion

Several strategies contributed to achieving the high response rate goals set for the MTO long-term survey, including selecting and training a data collection team that was well equipped to work in a challenging environment and having staff who understood (and were motivated by) the importance of the MTO demonstration. Starting with a small team and bringing on additional staff after the demonstration started, although not in the original plan, turned out to be very beneficial to a demonstration as complex and difficult as MTO. The close collaboration between the ISR and NBER teams, effective communication between and across the ISR data collection staff, and a solid management structure were also keys to the success of the field effort.

Exhibit 5

ITT Effects on Duration-Weighted Neighborhood Poverty Under Varying Response Rate Assumptions
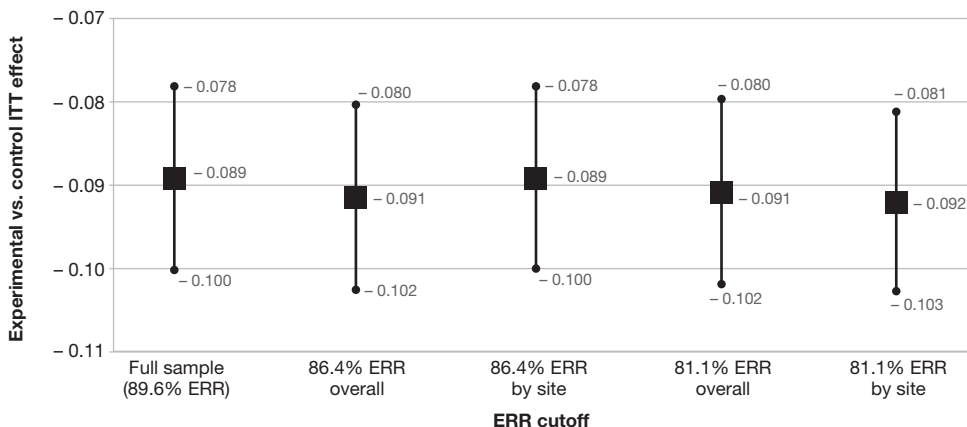
ERR = effective response rate. ITT = intention to treat.
Note: All ITT effects are statistically significant (p < .05).

Exhibit 6

ITT Effects on Adult Psychological Distress and Obesity Under Varying Response Rate Assumptions
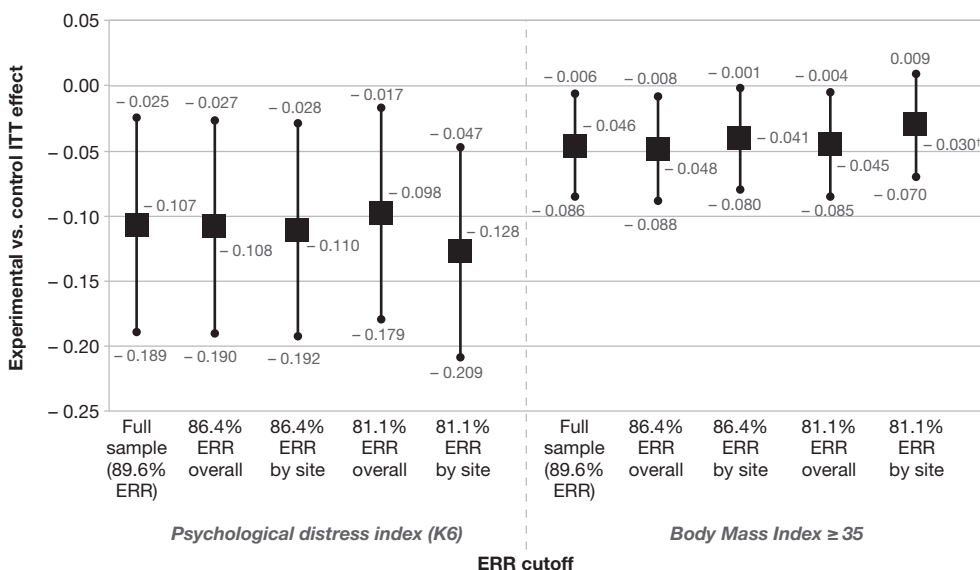
ERR = effective response rate. ITT = intention to treat.
[†] ITT effect is not statistically significant (p < .05).

Exhibit 7

ITT Effects on Female Youth Psychological Distress and Idleness Under Varying Response Rate Assumptions
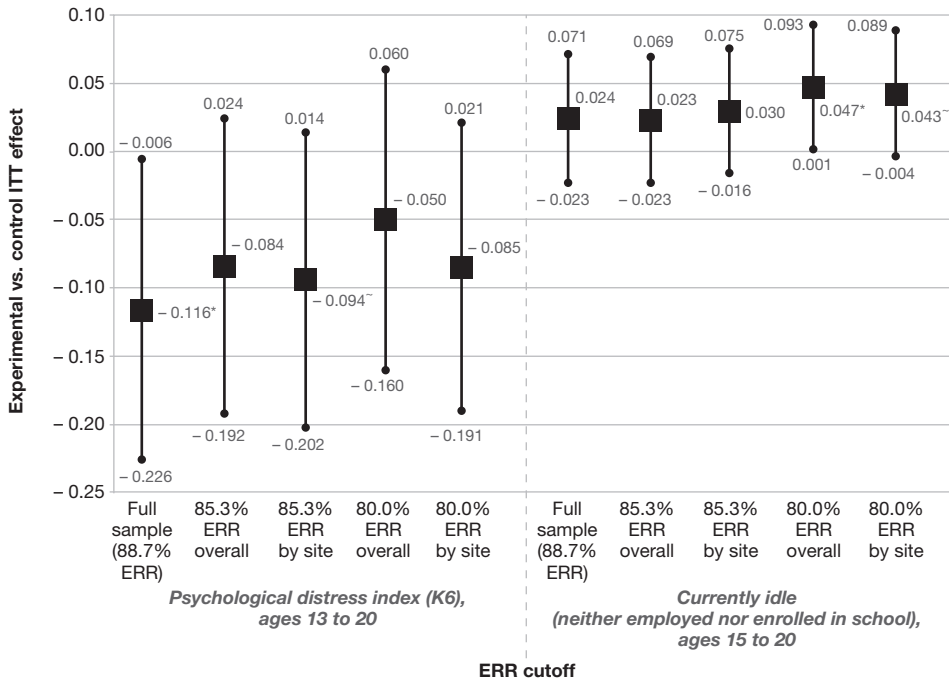
ERR = effective response rate. ITT = intention to treat.

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

These strategies all complemented the purer financial incentives that were offered to respondents and interviewers as motivations for behavior. The offer of the end-game incentive did not garner the expected response; the share of respondents who completed an interview was the same between those offered the original incentives and those offered the additional end-game incentives. Keeping the study in the field and refreshing interviewers with new types of incentives to offer to respondents did increase the ERR, because they motivated the interviewers to keep trying to locate and interview elusive and reluctant respondents.

The uneven number and slight variation in the quality of staffing across MTO sites led to differential completion rates and necessitated adjustments to the field procedures (using travelers to supplement local staff), and they also led to incremental implementation of the end-game activities. The strategy of pushing for as-high-as-possible within-site ERRs (as opposed to focusing only on a high overall ERR) certainly put strain on study resources (imagine the cost savings of shutting down one or two sites early, for example) but also added value. The analysis of ITT estimates at different response rates is one marker of potential value, wherein MTO effects on psychological distress of adults and female youth are larger and more precise with the final ERR compared with results that would have been reported had the study stopped when the field reached lower ERRs.

Exhibit 8

MTO Effects on Selected Adult Outcomes Under Varying Response Rate Assumptions (1 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Share poor: all addresses since random assignment (duration-weighted) [CEN]						
89.6% ERR (no restrictions)	0.396	– 0.089* (0.006)	– 0.184* (0.012)	– 0.069* (0.007)	– 0.111* (0.011)	3,270
86.4% ERR						
Overall (December 31, 2009)	0.398	– 0.091* (0.006)	– 0.185* (0.011)	– 0.070* (0.007)	– 0.110* (0.011)	3,219
By site	0.395	– 0.089* (0.006)	– 0.182* (0.011)	– 0.068* (0.007)	– 0.109* (0.010)	3,213
81.1% ERR						
Overall (October 31, 2009)	0.398	– 0.091* (0.006)	– 0.184* (0.012)	– 0.069* (0.007)	– 0.109* (0.011)	3,102
By site	0.397	– 0.092* (0.006)	– 0.186* (0.011)	– 0.073* (0.007)	– 0.116* (0.010)	3,118
Felt safe or very safe during the day [SR]						
89.6% ERR (no restrictions)	0.804	0.036* (0.016)	0.074* (0.034)	0.045* (0.021)	0.072* (0.034)	3,262
86.4% ERR						
Overall (December 31, 2009)	0.806	0.036* (0.016)	0.072* (0.033)	0.042* (0.021)	0.067* (0.034)	3,216
By site	0.806	0.036* (0.016)	0.073* (0.033)	0.047* (0.021)	0.075* (0.033)	3,207
81.1% ERR						
Overall (October 31, 2009)	0.812	0.029~ (0.016)	0.060~ (0.032)	0.037~ (0.022)	0.057~ (0.034)	3,099
By site	0.802	0.042* (0.016)	0.085* (0.033)	0.056* (0.021)	0.088* (0.034)	3,115
Rates current housing as excellent or good [SR]						
89.6% ERR (no restrictions)	0.570	0.053* (0.021)	0.109* (0.044)	0.031 (0.029)	0.050 (0.046)	3,267
86.4% ERR						
Overall (December 31, 2009)	0.569	0.044* (0.021)	0.090* (0.043)	0.028 (0.029)	0.045 (0.046)	3,221
By site	0.575	0.046* (0.021)	0.094* (0.043)	0.028 (0.028)	0.044 (0.045)	3,212
81.1% ERR						
Overall (October 31, 2009)	0.577	0.042* (0.021)	0.086* (0.043)	0.028 (0.029)	0.044 (0.045)	3,105
By site	0.577	0.042* (0.021)	0.084* (0.042)	0.032 (0.029)	0.051 (0.045)	3,120

Exhibit 8

MTO Effects on Selected Adult Outcomes Under Varying Response Rate Assumptions (2 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Annual individual earnings (previous calendar year, 2009 dollars) [SR]						
89.6% ERR (no restrictions)	12,288.52	326.93	677.92	– 613.60	– 982.43	3,141
		(583.44)	(1,209.79)	(807.20)	(1,292.40)	
86.4% ERR						
Overall (December 31, 2009)	12,226.32	116.85	238.58	– 655.16	– 1,030.37	3,092
		(573.27)	(1,170.50)	(797.54)	(1,254.29)	
By site	12,443.13	284.46	586.33	– 1,225.73	– 1,939.20	3,089
		(578.11)	(1,191.63)	(794.67)	(1,257.24)	
81.1% ERR						
Overall (October 31, 2009)	12,231.15	147.26	300.22	– 438.14	– 680.85	2,983
		(572.09)	(1,166.32)	(797.54)	(1,239.34)	
By site	12,258.56	157.97	320.39	– 826.60	– 1,294.17	3,002
		(571.88)	(1,159.90)	(774.43)	(1,212.49)	
Major depressive disorder with hierarchy (lifetime) [SR]						
89.6% ERR (no restrictions)	0.203	– 0.032~	– 0.066~	– 0.048*	– 0.077*	3,269
		(0.017)	(0.035)	(0.021)	(0.034)	
86.4% ERR						
Overall (December 31, 2009)	0.198	– 0.027	– 0.055	– 0.039~	– 0.061~	3,222
		(0.016)	(0.033)	(0.021)	(0.034)	
By site	0.202	– 0.034*	– 0.070*	– 0.041~	– 0.065~	3,214
		(0.017)	(0.034)	(0.021)	(0.034)	
81.1% ERR						
Overall (October 31, 2009)	0.199	– 0.023	– 0.046	– 0.046*	– 0.072*	3,105
		(0.017)	(0.034)	(0.021)	(0.033)	
By site	0.205	– 0.036*	– 0.073*	– 0.038~	– 0.060~	3,121
		(0.017)	(0.034)	(0.022)	(0.034)	
Psychological distress index (K6) (z-score) [SR]						
89.6% ERR (no restrictions)	0.000	– 0.107*	– 0.221*	– 0.097~	– 0.156~	3,273
		(0.042)	(0.087)	(0.056)	(0.091)	
86.4% ERR						
Overall (December 31, 2009)	0.000	– 0.108*	– 0.220*	– 0.089	– 0.141	3,222
		(0.042)	(0.084)	(0.056)	(0.089)	
By site	0.000	– 0.110*	– 0.226*	– 0.109*	– 0.173*	3,216
		(0.042)	(0.085)	(0.055)	(0.088)	
81.1% ERR						
Overall (October 31, 2009)	0.000	– 0.098*	– 0.199*	– 0.096~	– 0.151~	3,105
		(0.041)	(0.084)	(0.057)	(0.089)	
By site	0.000	– 0.128*	– 0.258*	– 0.097~	– 0.152~	3,121
		(0.041)	(0.084)	(0.055)	(0.087)	

Exhibit 8

MTO Effects on Selected Adult Outcomes Under Varying Response Rate Assumptions (3 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Body Mass Index ≥ 35 [M, SR]						
89.6% ERR (no restrictions)	0.351	– 0.046*	– 0.095*	– 0.053*	– 0.086*	3,221
		(0.020)	(0.042)	(0.027)	(0.043)	
86.4% ERR						
Overall (December 31, 2009)	0.359	– 0.048*	– 0.097*	– 0.054*	– 0.085*	3,172
		(0.020)	(0.041)	(0.027)	(0.043)	
By site	0.347	– 0.041*	– 0.083*	– 0.053*	– 0.085*	3,166
		(0.020)	(0.041)	(0.026)	(0.042)	
81.1% ERR						
Overall (October 31, 2009)	0.359	– 0.045*	– 0.091*	– 0.043	– 0.068	3,055
		(0.021)	(0.042)	(0.027)	(0.043)	
By site	0.342	– 0.030	– 0.061	– 0.039	– 0.062	3,073
		(0.020)	(0.041)	(0.027)	(0.042)	
HbA1c test detected diabetes (HbA1c ≥ 6.5) [M]						
89.6% ERR (no restrictions)	0.204	– 0.052*	– 0.108*	– 0.011	– 0.017	2,737
		(0.018)	(0.038)	(0.024)	(0.038)	
86.4% ERR						
Overall (December 31, 2009)	0.203	– 0.053*	– 0.107*	– 0.013	– 0.020	2,700
		(0.018)	(0.037)	(0.024)	(0.037)	
By site	0.200	– 0.046*	– 0.094*	– 0.010	– 0.016	2,695
		(0.018)	(0.037)	(0.023)	(0.037)	
81.1% ERR						
Overall (October 31, 2009)	0.199	– 0.043*	– 0.087*	0.010	0.016	2,599
		(0.018)	(0.037)	(0.024)	(0.037)	
By site	0.192	– 0.038*	– 0.078*	0.009	0.014	2,613
		(0.018)	(0.036)	(0.023)	(0.037)	

CEN = 1990 and 2000 census data. ERR = effective response rate. ITT = intention to treat. M = measured. MTO = Moving to Opportunity. SR = self-reported. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Census tract characteristics are linearly interpolated from the 1990 and 2000 Decennial Census. Major depressive disorder with hierarchy takes into account the comorbidity of mania and hypomania. The psychological distress index consists of six items (sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, worthlessness) scaled on a score from 0 (no distress) to 24 (highest distress) and then converted to z-scores using the mean and standard deviation of control group adults. For obesity inputs (height and weight), only a very small percentage of the sample self-reported their height or weight. Body Mass Index is measured as weight in kilograms divided by height in meters squared.

Source: Adult long-term survey

Exhibit 9

MTO Effects on Selected Female Youth Outcomes Under Varying Response Rate Assumptions (1 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Reading assessment score, ages 13–20 [ECLS-K]						
88.7% ERR (no restrictions)	0.000	– 0.020	– 0.040	0.055	0.083	2,286
		(0.056)	(0.113)	(0.063)	(0.095)	
85.3% ERR						
Overall (December 31, 2009)	0.000	– 0.023	– 0.046	0.062	0.093	2,258
		(0.055)	(0.113)	(0.063)	(0.095)	
By site	0.000	– 0.033	– 0.068	0.056	0.086	2,252
		(0.056)	(0.114)	(0.063)	(0.096)	
80.0% ERR						
Overall (October 31, 2009)	0.000	– 0.022	– 0.047	0.071	0.106	2,188
		(0.055)	(0.115)	(0.063)	(0.093)	
By site	0.000	– 0.016	– 0.033	0.052	0.077	2,196
		(0.056)	(0.116)	(0.064)	(0.094)	
Math assessment score, ages 13–20 [ECLS-K]						
88.7% ERR (no restrictions)	0.000	– 0.036	– 0.073	– 0.038	– 0.057	2,280
		(0.060)	(0.121)	(0.067)	(0.101)	
85.3% ERR						
Overall (December 31, 2009)	0.000	– 0.039	– 0.079	– 0.031	– 0.047	2,251
		(0.060)	(0.121)	(0.067)	(0.102)	
By site	0.000	– 0.051	– 0.104	– 0.026	– 0.040	2,245
		(0.060)	(0.123)	(0.067)	(0.102)	
80.0% ERR						
Overall (October 31, 2009)	0.000	– 0.046	– 0.097	– 0.006	– 0.009	2,182
		(0.057)	(0.119)	(0.067)	(0.100)	
By site	0.000	– 0.043	– 0.088	– 0.031	– 0.046	2,190
		(0.061)	(0.126)	(0.068)	(0.100)	
Currently idle (neither employed nor enrolled in school), ages 15–20 [SR]						
88.7% ERR (no restrictions)	0.194	0.024	0.049	0.031	0.048	1,838
		(0.024)	(0.048)	(0.027)	(0.043)	
85.3% ERR						
Overall (December 31, 2009)	0.192	0.023	0.047	0.038	0.059	1,807
		(0.023)	(0.047)	(0.027)	(0.042)	
By site	0.190	0.030	0.060	0.038	0.060	1,806
		(0.023)	(0.048)	(0.027)	(0.042)	

Exhibit 9

MTO Effects on Selected Female Youth Outcomes Under Varying Response Rate Assumptions (2 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
80.0% ERR						
Overall (October 31, 2009)	0.182	0.047* (0.023)	0.098* (0.049)	0.043 (0.026)	0.065 (0.041)	1,747
By site	0.190	0.043~ (0.024)	0.087~ (0.049)	0.036 (0.026)	0.053 (0.039)	1,753
Risky behavior index, ages 13–20 [SR]						
88.7% ERR (no restrictions)	0.442	– 0.027 (0.019)	– 0.054 (0.037)	– 0.017 (0.020)	– 0.026 (0.031)	2,358
85.3% ERR						
Overall (December 31, 2009)	0.443	– 0.029 (0.019)	– 0.059 (0.038)	– 0.017 (0.020)	– 0.027 (0.031)	2,326
By site	0.441	– 0.028 (0.019)	– 0.057 (0.038)	– 0.014 (0.020)	– 0.023 (0.031)	2,321
80.0% ERR						
Overall (October 31, 2009)	0.440	– 0.021 (0.019)	– 0.043 (0.039)	– 0.005 (0.020)	– 0.008 (0.031)	2,256
By site	0.435	– 0.020 (0.019)	– 0.041 (0.038)	– 0.004 (0.020)	– 0.006 (0.030)	2,262
Psychological distress index (K6) (z-score), ages 13–20 [SR]						
88.7% ERR (no restrictions)	0.000	– 0.116* (0.056)	– 0.234* (0.113)	– 0.013 (0.065)	– 0.020 (0.101)	2,371
85.3% ERR						
Overall (December 31, 2009)	0.000	– 0.084 (0.055)	– 0.171 (0.112)	0.026 (0.066)	0.040 (0.102)	2,332
By site	0.000	– 0.094~ (0.055)	– 0.193~ (0.113)	0.021 (0.065)	0.033 (0.103)	2,334
80.0% ERR						
Overall (October 31, 2009)	0.000	– 0.050 (0.056)	– 0.105 (0.117)	0.055 (0.065)	0.084 (0.099)	2,262
By site	0.000	– 0.085 (0.054)	– 0.175 (0.111)	0.035 (0.064)	0.052 (0.096)	2,271
Currently good or better health, ages 10–20 [SR]						
88.7% ERR (no restrictions)	0.862	0.003 (0.019)	0.007 (0.038)	0.006 (0.021)	0.010 (0.034)	2,600
85.3% ERR						
Overall (December 31, 2009)	0.866	– 0.005 (0.019)	– 0.010 (0.038)	0.002 (0.021)	0.003 (0.033)	2,560
By site	0.866	– 0.003 (0.019)	– 0.006 (0.038)	0.000 (0.021)	0.000 (0.034)	2,561

Exhibit 9

MTO Effects on Selected Female Youth Outcomes Under Varying Response Rate Assumptions (3 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
80.0% ERR						
Overall (October 31, 2009)	0.868	− 0.011	− 0.023	0.005	0.008	2,479
		(0.019)	(0.039)	(0.021)	(0.032)	
By site	0.865	− 0.004	− 0.007	0.001	0.001	2,495
		(0.019)	(0.039)	(0.021)	(0.032)	

ECLS-K = Early Childhood Longitudinal Study-Kindergarten cohort study. ERR = effective response rate. ITT = intention to treat. MTO = Moving to Opportunity. SR = self-reported. TOT = treatment on the treated.

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

Note: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. Age ranges as of December 2007 are specified for each measure. The reading and math achievement assessment scores are theta scores transformed into z-scores via standardization on the mean and standard deviation for control group youth ages 13 to 20. The risky behavior index is the fraction of four risky behaviors (smoking, alcohol use, marijuana use, and sex) that the youth reports ever having exhibited. The psychological distress index consists of six items (sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, worthlessness) scaled on a score from 0 (no distress) to 24 (highest distress) and then converted to z-scores using the mean and standard deviation of control group youth. Results reported for the achievement assessment and K6 measures differ slightly from those in Sanbonmatsu et al. (2011) because here, standardization was separate by gender, whereas Sanbonmatsu et al. standardized only on the overall control group mean and standard deviation. The overall (male and female combined) z-score values combine the z-scores by gender and thus are not themselves standardized (the control mean is 0 but the standard deviation is not exactly 1).

Source: Youth long-term survey

Exhibit 10

MTO Effects on Selected Male Youth Outcomes Under Varying Response Rate Assumptions (1 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Reading assessment score, ages 13–20 [ECLS-K]						
88.7% ERR (no restrictions)	0.000	0.027	0.057	0.025	0.035	2,146
		(0.054)	(0.115)	(0.057)	(0.079)	
85.3% ERR						
Overall (December 31, 2009)	0.000	0.031	0.063	0.033	0.046	2,111
		(0.055)	(0.113)	(0.058)	(0.081)	
By site	0.000	0.024	0.049	0.036	0.050	2,099
		(0.055)	(0.114)	(0.058)	(0.081)	
80.0% ERR						
Overall (October 31, 2009)	0.000	0.030	0.063	0.042	0.059	2,027
		(0.056)	(0.115)	(0.059)	(0.082)	
By site	0.000	0.022	0.045	0.053	0.074	2,036
		(0.056)	(0.114)	(0.060)	(0.083)	

Exhibit 10

MTO Effects on Selected Male Youth Outcomes Under Varying Response Rate Assumptions (2 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Math assessment score, ages 13–20 [ECLS-K]						
88.7% ERR (no restrictions)	0.000	– 0.014	– 0.030	0.034	0.046	2,140
		(0.056)	(0.119)	(0.063)	(0.087)	
85.3% ERR						
Overall (December 31, 2009)	0.000	– 0.012	– 0.024	0.053	0.074	2,105
		(0.057)	(0.119)	(0.063)	(0.087)	
By site	0.000	– 0.033	– 0.069	0.032	0.044	2,092
		(0.057)	(0.118)	(0.061)	(0.084)	
80.0% ERR						
Overall (October 31, 2009)	0.000	– 0.020	– 0.040	0.037	0.051	2,021
		(0.058)	(0.121)	(0.062)	(0.086)	
By site	0.000	– 0.015	– 0.030	0.063	0.087	2,030
		(0.058)	(0.118)	(0.062)	(0.085)	
Currently idle (neither employed nor enrolled in school), ages 15–20 [SR]						
88.7% ERR (no restrictions)	0.235	– 0.011	– 0.023	0.022	0.032	1,766
		(0.027)	(0.058)	(0.031)	(0.045)	
85.3% ERR						
Overall (December 31, 2009)	0.235	– 0.017	– 0.036	0.006	0.009	1,724
		(0.027)	(0.056)	(0.030)	(0.043)	
By site	0.237	– 0.010	– 0.020	– 0.005	– 0.007	1,721
		(0.027)	(0.056)	(0.029)	(0.042)	
80.0% ERR						
Overall (October 31, 2009)	0.234	– 0.016	– 0.034	0.005	0.007	1,658
		(0.027)	(0.056)	(0.030)	(0.043)	
By site	0.245	– 0.021	– 0.043	– 0.006	– 0.009	1,664
		(0.027)	(0.055)	(0.030)	(0.042)	
Risky behavior index, ages 13–20 [SR]						
88.7% ERR (no restrictions)	0.491	0.025	0.053	0.029	0.042	2,265
		(0.018)	(0.039)	(0.020)	(0.028)	
85.3% ERR						
Overall (December 31, 2009)	0.487	0.028	0.058	0.030	0.042	2,220
		(0.018)	(0.038)	(0.020)	(0.028)	
By site	0.484	0.020	0.041	0.032	0.046	2,211
		(0.018)	(0.038)	(0.020)	(0.028)	
80.0% ERR						
Overall (October 31, 2009)	0.482	0.022	0.046	0.030	0.043	2,132
		(0.019)	(0.039)	(0.020)	(0.028)	
By site	0.485	0.021	0.043	0.030	0.042	2,143
		(0.018)	(0.038)	(0.020)	(0.028)	

Exhibit 10

MTO Effects on Selected Male Youth Outcomes Under Varying Response Rate Assumptions (3 of 3)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Psychological distress index (K6) (z-score), ages 13–20 [SR]						
88.7% ERR (no restrictions)	0.000	0.041 (0.056)	0.088 (0.120)	0.087 (0.063)	0.124 (0.089)	2,273
85.3% ERR						
Overall (December 31, 2009)	0.000	0.047 (0.056)	0.098 (0.117)	0.096 (0.062)	0.135 (0.087)	2,226
By site	0.000	0.056 (0.055)	0.117 (0.116)	0.120~ (0.062)	0.171~ (0.088)	2,218
80.0% ERR						
Overall (October 31, 2009)	0.000	0.075 (0.056)	0.157 (0.117)	0.146* (0.063)	0.206* (0.088)	2,138
By site	0.000	0.048 (0.055)	0.098 (0.114)	0.101 (0.061)	0.143 (0.087)	2,149
Currently good or better health, ages 10–20 [SR]						
88.7% ERR (no restrictions)	0.903	0.006 (0.016)	0.012 (0.035)	– 0.007 (0.019)	– 0.010 (0.027)	2,500
85.3% ERR						
Overall (December 31, 2009)	0.903	0.009 (0.016)	0.018 (0.034)	– 0.006 (0.019)	– 0.008 (0.026)	2,453
By site	0.905	0.010 (0.015)	0.021 (0.032)	– 0.011 (0.019)	– 0.015 (0.027)	2,442
80.0% ERR						
Overall (October 31, 2009)	0.902	0.011 (0.016)	0.023 (0.033)	– 0.007 (0.019)	– 0.010 (0.027)	2,358
By site	0.906	0.009 (0.016)	0.018 (0.032)	– 0.008 (0.019)	– 0.012 (0.026)	2,373

ECLS-K = Early Childhood Longitudinal Study-Kindergarten cohort study. ERR = effective response rate. ITT = intention to treat. MTO = Moving to Opportunity. SR = self-reported. TOT = treatment on the treated.

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

Note: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. Age ranges as of December 2007 are specified for each measure. The reading and math achievement assessment scores are theta scores transformed into z-scores via standardization on the mean and standard deviation for control group youth ages 13 to 20. The risky behavior index is the fraction of four risky behaviors (smoking, alcohol use, marijuana use, and sex) that the youth reports ever having exhibited. The psychological distress index consists of six items (sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, worthlessness) scaled on a score from 0 (no distress) to 24 (highest distress) and then converted to z-scores using the mean and standard deviation of control group youth. Results reported for the achievement assessment and K6 measures differ slightly from those in Sanbonmatsu et al. (2011) because here, standardization was separate by gender, whereas Sanbonmatsu et al. standardized only on the overall control group mean and standard deviation. The overall (male and female combined) z-score values combine the z-scores by gender and thus are not themselves standardized (the control mean is 0 but the standard deviation is not exactly 1).

Source: Youth long-term survey

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MTO: A Successful Housing Intervention

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Abstract

At its core, Moving to Opportunity (MTO) was a housing intervention offering public housing families tenant-based vouchers to move to the private market. Giving families vouchers resulted in better quality housing for them 10 to 15 years later, potentially contributing to the physical and mental health improvements of those who participated in MTO. Using a triangulated, multisource strategy, we find that two-thirds of all MTO households still receive housing assistance. The Section 8 group experienced higher rates of doubling up, although the MTO intervention had no effect on housing cost burdens. The experimental group experienced material hardship, making tradeoffs between paying their rent on time and paying utilities.

Introduction

The research objective of the Moving to Opportunity (MTO) for Fair Housing demonstration was to test the long-term effect of moving families with children from public or project-based housing developments located in very low-income neighborhoods to subsidized private-market rental units in neighborhoods with low poverty levels. The hypothesis tested was that exposing families to low-poverty environments would result in improvements in their employment, income, education, health, and social well-being. Although it was also a larger social intervention, MTO was, at its core, a housing intervention offering families living in some of the worst public housing developments in the nation the opportunity to receive a tenant-based voucher and move to the private market. Most cities have extremely long waiting lists for housing assistance, particularly tenant-based vouchers, because need is greater than supply, which makes it difficult for those already in public housing to switch to using vouchers. The MTO lottery offered a rare opportunity for these residents to jump to the head of the line (Finkel and Buron, 2001; Turner and Kingsley, 2008).

The MTO demonstration's experimental design (described in other articles in this symposium and in Sanbotmatsu, 2011) also makes comparing different forms of housing assistance possible. Currently, the federal government provides rental housing subsidies for very low-income households in two basic forms: project-based subsidies are attached to specific apartments or homes managed by public housing agencies (PHAs) or private owners, whereas tenant-based housing vouchers help pay the rent for homes and apartments on the private market (for more detail, see Turner and Kingsley, 2008). Over the years, vouchers have accounted for a growing share of all federal assistance to very low-income renters. Housing advocates and policymakers, however, continue to debate the relative strengths and weaknesses of these two approaches. MTO provides an opportunity to compare rigorously, all else being equal, the benefits for low-income families of living in subsidized projects as opposed to receiving vouchers or vouchers plus an incentive to locate in a low-poverty neighborhood. Note, however, that the subsidized projects targeted for participation in the MTO demonstration are not typical of all federally subsidized rental housing. By design, the targeted projects were located in high-poverty neighborhoods and suffered from physical deterioration and social distress.

Context for the MTO Demonstration

In 1994, when MTO began, the public housing program had become a national symbol of the failures of social welfare programs. The grim highrise towers and sprawling barracks-style developments that dominated urban landscapes were a highly visible reminder of the crime, poverty, and other social ills afflicting many central city communities (Popkin et al., 2000). Many public housing properties were poorly constructed, badly managed, and inadequately funded, leading to extensive repair backlogs and putting residents at risk of injury or disease (Landrigan, Todd, and Wedeen, 1995; Manjarrez, Popkin, and Guernsey, 2007; Rosenstreich et al., 1997). Furthermore, these developments were often on undesirable urban renewal sites close to other types of subsidized housing, resulting in communities with high concentrations of racially and economically segregated, very low-income households (HUD, 1994; Turner, Popkin, and Rawlings, 2009).

MTO was a key element of the policy changes that began in the 1990s with the intention to transform public housing and use housing assistance to promote access to neighborhoods that offered greater social and economic opportunities for assisted tenants (Turner, Popkin, and Rawlings, 2009). The largest component of this effort was the $6 billion federal HOPE VI Program, which provided large grants to housing authorities across the nation to demolish their most distressed developments and replace them with new, mixed-income housing (Popkin, Levy, and Buron, 2009).[1]

In all five MTO sites—Baltimore, Boston, Chicago, Los Angeles, and New York City—the public housing developments were physically distressed and would have met the formal definition of substandard housing (NCSDPH, 1992; Scharfstein and Sandel, 1998). At baseline, participants reported extreme dissatisfaction with their housing and complained of problems such as vermin (rats, mice, and cockroaches), mold, and broken plumbing, all of which presented dangers to

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[1] HOPE VI stands for Housing Opportunities for People Everywhere. The program began providing grants to housing authorities in 1993 (Popkin et al., 2000).

health and well-being. Nearly one-half of participants at baseline identified wanting a bigger or better apartment as a motivation for moving (Orr et al., 2003). Therefore, a key question for MTO is how this demonstration affected basic housing outcomes—housing quality, reliance on housing assistance, affordability, and homelessness.

How the MTO demonstration would affect housing outcomes overall was not clear at baseline. Because of the nature of the voucher program and limits on Fair Market Rent (FMR), voucher holders tend to be concentrated in low-income communities where rents are affordable, vacancy rates are high, and quality is relatively low. MTO offered the experimental group participants (those who were offered a voucher that could be used only in census tracts with 1990 poverty rates of less than 10 percent and who received additional mobility counseling) the assistance to move to low-poverty neighborhoods that might theoretically offer access to better quality housing stock and more responsive landlords. Even with assistance, however, participants might have trouble navigating the private market, have difficulty leasing up or securing a unit, and encounter discrimination. In the long term, even those who initially were successful in finding a unit and leasing up might experience housing instability if they had to move because of changes in the rental market or problems with their landlords. Conversely, if MTO really helped participants improve their social and economic circumstances, they might earn their way off housing assistance, leaving them vulnerable to economic reversals.

As it turned out, in addition to the basic challenges of navigating the rental market with a voucher, the demonstration took place in the context of a rapidly changing housing market, during a period when the incomes of Americans with the lowest incomes declined (after adjusting for inflation) and housing markets became even more segregated by income and race (Briggs, Popkin, and Goering, 2010).

We begin this article briefly summarizing the key findings. Then we review the data sources and methodology used for analysis, describing in detail the new multisource triangulation method used to identify MTO participants' housing assistance at the time of the final impacts evaluation (Sanbonmatsu et al., 2011). We review our findings regarding housing quality, homelessness and doubling up, housing costs and burdens, and housing payment challenges. We finish by considering the implications of the MTO demonstration and receiving vouchers in general.

Summary of Findings

Originally, giving low-income families vouchers at the start of the demonstration resulted in better quality housing compared with that of the control group (participants who were not offered vouchers or counseling). This finding is consistent with research on the effect of HOPE VI redevelopment on outcomes for relocated residents (Popkin, Levy, and Buron, 2009). The public housing developments in the MTO demonstration were in such poor shape that participants both experienced immediate improvements (evidenced by the results from the interim survey [Orr et al., 2003]) and sustained them over the long term (evidenced by the final evaluation survey results). These improvements in housing quality could have very likely contributed to the improvements in the physical and mental health of MTO participants.

Approximately two-thirds of MTO participants continued to rely on housing assistance at the time of the final impacts evaluation. Because voucher holders did not experience any improvements in their employment or earnings over the course of the demonstration, the fact that they are still as likely to rely on housing assistance as the control group is not surprising.

We found limited evidence that providing vouchers results in more housing instability compared with not offering vouchers. Receiving a voucher did not have any differential effect on experiencing homelessness compared with that of the control group; the Section 8 group (those who were offered a voucher without any geographic restrictions and no additional counseling), however, experienced more instances of doubling up with friends and family than the control group did. Vouchers also had no disproportionate effect on housing costs, which were very high for all MTO participants, even those still receiving housing subsidies. The recent housing boom may have played a factor in explaining these high housing costs, causing all MTO voucher holders who stayed in place to pay more out of pocket for rent.

Voucher holders were more likely to ensure they were not late paying their rent, reflecting the requirements of living in the private market. It appears, however, that they were also more likely to make a tradeoff by paying their utilities late or not at all, which resulted in the utilities being turned off. Again, this finding is consistent with other research on families moving from public housing to the private market and suggests a need for greater attention to helping voucher holders meet the costs of utilities in private-market units.

Data and Methods

For most of our analysis in this article, we rely on the survey data collected by the University of Michigan's Institute for Social Research between June 2008 and April 2010 under its contract with the National Bureau of Economic Research for the final impacts evaluation. The survey used for the MTO final impacts evaluation (Sanbonmatsu et al., 2011) collected information from 3,273 adults and 5,101 targeted youth, covering a wide variety of outcomes and mediators in six domains, with response rates of 89.6 percent for adults and 88.7 percent for youth. In this article, we focus on the following housing outcomes and mediators self-reported by the MTO participants: housing quality, housing assistance, homelessness and doubling up, housing costs (rents or mortgages plus utilities), housing cost burdens, difficulty paying rents or mortgages plus utilities on time, eviction because of late rent or mortgage payments, and utilities being turned off because of late payments.

Most of our analysis in this article focuses on the intention-to-treat (ITT) effects, comparing the average housing outcomes of the experimental group with those of the control group. We also compare the average outcomes of the Section 8 group with those of the control group. All ITT effects are listed as such in the exhibits. The exhibits also include the treatment-on-the-treated (TOT) effects, which capture the effect of moving with either an MTO low-poverty or a traditional Section 8 voucher.[2]

[2] We used the same sampling weights and regression models as described in Sanbonmatsu et al. (2011) to report the same ITT and TOT results as published in the final impacts evaluation.

Because we are interested in assessing whether offering vouchers improves low-income families' outcomes, we look at how both the experimental and Section 8 groups fared compared with the control group.

Identifying Instances of Homelessness and Doubling Up

Analysts tracked MTO participants' residences throughout the duration of the demonstration, including addresses identified between surveying periods. The final impacts evaluation survey asked MTO participants to confirm each past address and report the month and year that they first moved to and left that address. In addition, the survey asked participants if, at any time between these addresses, they did not have a place of their own to stay. It also asked those responding in the affirmative with whom or where they stayed—such as, with friends or relatives, on the street, in a shelter, in an abandoned building, in a car, or in a hotel or motel, among other options—how long they were without a place of their own, and if their child(ren) was (were) living with them at the time. Participants were identified as having an instance of doubling up if they reported that they did not have a place of their own to stay and lived with their friends or family. People who reported being doubled up are, by definition, unstably housed. Heads of household were identified as having been *literally homeless*[3] if they reported that they did not have a place of their own to stay and lived on the street or in shelters, abandoned buildings, cars or vans, movie theaters, or laundromats—essentially anywhere that is not deemed fit as a typical residence. Neither definition included participants staying at a hotel or motel, even when the respondent did not have a place of his or her own.

Identifying Housing Assistance Status Using Multiple Data Sources

We employed a new multistep, multisource process to identify more accurately whether each MTO head of household was receiving *any* federal rental assistance[4] and to determine the *specific type* of assistance received among those who were assisted at the time of the final impacts evaluation.[5] Although housing assistance status is a key outcome of the MTO demonstration, determining whether a household is still receiving a subsidy and, if so, what type of subsidy it is receiving has been surprisingly difficult to determine. Other research has documented that recipients often misidentify the type of housing assistance they receive or erroneously report not receiving any assistance at all (see the appendix of Shroder, 2002). For instance, those using housing vouchers often misreport that the PHA is their landlord or simply say that they pay their own rent. Residents in all types of assisted housing often just respond that they live in "housing" without being able to specify which type. Relying on administrative housing assistance data can also be unreliable, because resident

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[3] HUD uses the term *literally homeless* to differentiate between families living in places not fit for everyday residence (such as on the street, in abandoned buildings, in cars or vans) and families who are precariously housed, such as those who are doubled up. The full definition is available in HUD (2006).

[4] Federal rental assistance, also referred to as *deep subsidy*, is defined as participating in a program that cuts housing costs to 30 percent of income (or some specified flat cost) for all participants in that program.

[5] The specific types of assistance include public housing, tenant-based federal rental assistance, project-based nonpublic housing federal rental assistance, and no federal rental assistance (including owners, unassisted renters, the homeless, and those with other statuses).

annual recertification records are not always entered into the appropriate databases (Olsen, Davis, and Carrillo, 2005). To solve this problem, other researchers determined housing assistance status only where survey and administrative data match (Verma and Riccio, with Azurdia, 2003). This methodology, however, can exclude a significant proportion of those receiving housing assistance. At the time of the interim survey, Orr et al. (2003) reported two housing assistance statuses of MTO participants: one based on survey responses and the second from administrative data. Only a 78-percent agreement existed between the two data sources.

To reduce misreporting by MTO participants, the survey for the MTO final impacts evaluation included a new series of questions to assess MTO participants' housing assistance status.[6] We then compared the survey responses with two annually collected administrative sources—Multifamily Tenant Characteristics System (MTCS)/Public Indian Housing and Information Center (PIC) and Tenant Rental Assistance Certification System (TRACS)/Multifamily data—to identify each MTO participant's type of housing assistance. MTCS/PIC data contain longitudinal information on families living in public housing or receiving tenant-based housing vouchers (Form 50058), whereas TRACS/Multifamily data contain longitudinal information on families living in project-based Section 8 housing (Form 50059). The U.S. Department of Housing and Urban Development (HUD) Office of Policy Development and Research successfully matched approximately 90 percent of MTO heads of household to one or both longitudinal administrative data sources using a combination of first and last names, date of birth, and Social Security number.

In the first step of this new process to identify participants' housing assistance status, we analyzed the series of housing assistance survey responses (step 1 in exhibit 1). The researchers coded respondents' answers to each survey question as either eliminating or not eliminating each of eight possible housing assistance statuses tracked in this first step.[7] As a result, MTO participants could have more than one possible assistance status at this point. Researchers chose this elimination method, as opposed to identifying affirmative answers to questions, to remove the nonresponse bias, particularly from inconsistently applied skip patterns. It also enabled the analysts to confirm participants' multiple possible responses against the two administrative data sets. For instance, if we had used an affirmative method, a head of household who answered that the PHA is his or her landlord, even if that was not the case, would eliminate all housing assistance statuses except public housing, which often mistakenly occurs. Another example is that nine heads of household in the MTO final survey affirmatively answered that they received housing vouchers but denied that their landlords required proof of income for housing. By keeping the possibility that the person was using a voucher, we later were able to use the administrative sources to further hone down the possible housing assistance types.

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[6] Researchers based the new questions on the MTO interim survey (Orr et al., 2003) and the HOPE VI Panel Study, a five-site study that tracked outcomes for 887 residents of public housing developments targeted for redevelopment. See Popkin et al. (2002) for a full description of the study.

[7] The eight possible housing categories are renter with tenant-based assistance, renter in public housing, renter with project-based assistance, renter without housing assistance, homeowner, homeless individual, individual who lives with family or friends and does not pay rent, and individual with another housing arrangement. The researchers could not determine assistance status for owners, because most owners were not asked any questions about housing assistance. For this reason, the final categories include information only on rental assistance, not on homeownership assistance.

Exhibit 1

Multistep Triangulation Process To Identify Housing Assistance Status
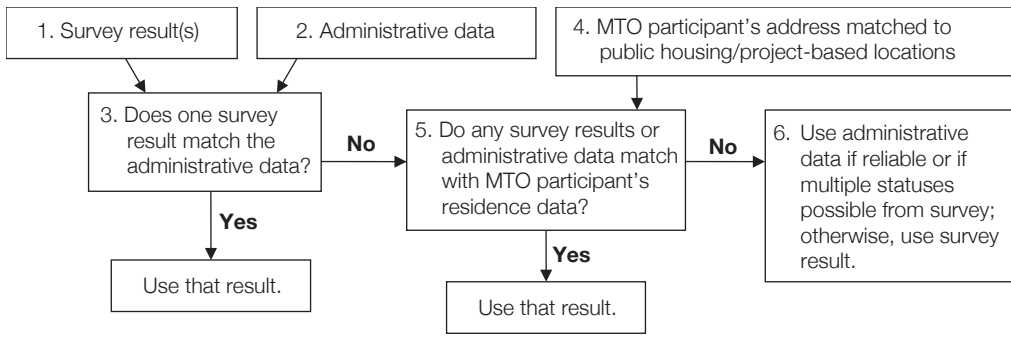
Researchers then separately analyzed MTO heads of household who were successfully linked to the MTCS/PIC and TRACS/Multifamily data to determine the housing assistance status for any head of household on the administrative files (step 2). Researchers then compared the identified type of housing assistance from the survey responses and the administrative sources (step 3). If one status from the survey analysis matched a status from the administrative data, analysts assigned the respondent that housing assistance status.

Survey responses and the administrative sources did not match for 14 percent of MTO participants. Analysts compared those participants' residences at the time of the final evaluation survey with the known addresses of the PHA's housing developments and project-based assistance buildings (step 4). Also, they compared MTO participants' addresses (ZIP+4) at the time of the final survey with both the survey responses and administrative data (step 5). For the 7 percent of MTO participants who still had conflicting housing assistance statuses after step 5, analysts selected the housing assistance status from the administrative data if the participant's administrative records matched residents' characteristics from the survey file and they found no duplicate records (step 6). Otherwise, analysts assigned participants a status based on the survey result. Exhibit 1 summarizes the process.

The following sections describe the housing-related findings from the survey for the MTO final impacts evaluation.

Findings

The experimental and Section 8 group households experienced improvements in housing quality, but findings on housing stability were mixed. Changes in the housing market affected all house-holds in the MTO demonstration. Neither the experimental nor Section 8 group experienced any differences in housing affordability or housing assistance.

Sustained Housing Quality Improvements

The MTO demonstration improved the housing quality of households that formerly lived in rundown public housing projects, consistent with findings from the HOPE VI Panel Study (Comey, 2004; Popkin, Levy, and Buron, 2009). MTO participants started off at baseline living in very dire housing conditions. For instance, 25 percent reported their housing to be in poor condition, 58 percent reported problems with plumbing, and 61 percent reported problems with rats or mice. At baseline, MTO participants identified wanting better housing quality or a bigger sized unit (or wanting to leave their unsafe neighborhoods) as one of the main reasons for wanting to move (Orr et al., 2003).

At the time of the interim survey, evidence suggested that the demonstration positively affected both the experimental and Section 8 groups' housing quality: 52 percent of the control group rated their housing as excellent or good compared with 62 percent of the experimental group and 59 percent of the Section 8 group (Orr et al., 2003). The experimental and Section 8 groups also reported fewer problems with vermin and peeling paint.

Exhibit 2 shows that, at the time of the final impacts evaluation, the positive effects on housing quality were sustained, particularly for the experimental group. Participants in the experimental group were more likely than those in the control group to rate their housing as excellent or good. The Section 8 and control groups no longer exhibited any statistical difference, however. Both the experimental and Section 8 groups were significantly less likely than the control group to report a variety of housing problems, including peeling paint, broken plumbing, vermin, and broken windows. Households in the Section 8 group experienced fewer problems than the control group with broken heat, and the households in the experimental group experienced fewer problems with broken locks.

The consequences of living in substandard housing have been well documented. Poor housing quality has been linked with poor physical and mental health (Krieger and Higgins, 2002; Matte and Jacobs, 2000). Children and adults living in damp, moldy housing, often the result of plumbing problems, tend to have more medical problems such as respiratory problems, headaches, nausea, and vomiting compared with residents living in drier houses (Hunt, 1993; Platt et al., 1989). Increased incidents of asthma have been linked to exposure to vermin such as cockroaches, mice, and rats (Rosenstreich et al., 1997). In addition, in a review of literature connecting mental health and housing quality, Evans, Wells, and Moch (2003) found a positive correlation between housing quality and psychological well-being. This same result has also been seen in children; children living in poor-quality housing are more likely to display behavioral problems and have difficulties concentrating (Evans, Saltzman, and Cooperman, 2001).

The MTO final impacts evaluation found important positive mental and physical health effects for adults and female youth (Sanbonmatsu et al., 2011). Most previous research studies relied primarily on correlations that omitted individual or family characteristics that could have affected the types and quality of housing those families selected. The experimental and longitudinal design of the MTO demonstration removed the selection bias that may have challenged previous studies. The sustained housing improvements documented for the families in the experimental group might have contributed to these important physical and mental health gains.

Exhibit 2

ITT/TOT Estimated Effect on Housing Quality

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Overall rating of current housing						
Rated housing as excellent or good	0.570	0.053* (0.210)	0.109* (0.044)	0.031 (0.029)	0.050 (0.046)	3,267
Current housing problems (big or small)						
Heat broken	0.179	– 0.013 (0.016)	– 0.027 (0.033)	– 0.037~ (0.020)	– 0.059~ (0.033)	3,252
Lock broken	0.144	– 0.029~ (0.015)	– 0.059~ (0.030)	– 0.019 (0.019)	– 0.031 (0.030)	3,267
Paint peeling	0.466	– 0.091* (0.021)	– 0.187* (0.044)	– 0.120* (0.028)	– 0.192* (0.044)	3,265
Plumbing broken	0.327	– 0.058* (0.020)	– 0.119* (0.040)	– 0.053* (0.026)	– 0.085* (0.041)	3,265
Rats or mice present	0.347	– 0.038~ (0.020)	– 0.079~ (0.041)	– 0.045~ (0.027)	– 0.072~ (0.043)	3,265
Cockroaches present	0.356	– 0.053* (0.019)	– 0.110* (0.040)	– 0.089* (0.025)	– 0.142* (0.039)	3,267
Window broken	0.233	– 0.065* (0.017)	– 0.134* (0.036)	– 0.078* (0.023)	– 0.124* (0.036)	3,267

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release.

Source: Adult long-term survey

No Effect on Housing Assistance Receipt

Part of the rationale for the MTO demonstration was that moving to low-poverty, resource-rich neighborhoods could potentially improve families' economic well-being. It follows that if families' economic status improved, they might earn their way off housing assistance. The specific form of housing assistance a family receives (that is, public housing or tenant-based housing voucher) may also be an important mediator for other key MTO demonstration outcomes. Families who receive vouchers may have more and better options about where to live than those living in public housing. Alternatively, voucher holders may experience more residential instability and have trouble making payments and operating in the private market, which could undermine other outcomes.

Ultimately, MTO had no effect on housing assistance receipt at the time of the final impacts evaluation. No statistical significance emerged between the experimental and control groups in the proportion of households still on assistance, and the Section 8 group was only slightly more likely to be receiving any sort of assistance than the control group (5 percentage points more, where $p < .10$). This finding is consistent with other findings from the final impacts evaluation, showing

no effect on families' employment levels and earnings. Therefore, our finding that families in the experimental group were no more likely than the families in the control group to "income out" of housing subsidies is unsurprising.

Although no ITT effect existed, exhibit 3 shows that almost two-thirds (62 percent) of the households in the control group were still receiving housing assistance 10 to 15 years after the start of the demonstration. The proportion still on assistance was slightly lower than at the time of the interim survey, which was 71 percent (determined exclusively from the interim survey responses) or 66 percent (determined exclusively from the administrative data) of the control group (Orr et al., 2003). Analysts did not use the previously described new triangulation method for the interim survey results.

The fact that so many MTO participants were still receiving housing subsidies after 10 to 15 years is unusual. The median length of time that households use housing assistance is 4.7 years for those living in public housing and 3.1 years for voucher holders, and families with children receive assistance for even less time—3.2 years for those living in public housing and 2.6 years for those participating in the voucher program (Turner and Kingsley, 2008). MTO families started in severely distressed public housing developments, however; by the 1990s, when the MTO demonstration began, families with more resources or better options largely had fled the original public housing and project-based housing because of crime and disorder, leaving behind a population dominated by the most vulnerable households. The HOPE VI Panel Study, which tracked families relocated as the result of HOPE VI redevelopment initiatives, found a similar pattern; at baseline, those respondents reported having lived in public housing for 10 or more years, on average (Popkin, Levy, and Buron, 2009). These long-term public housing residents, in general, have tenuous connections to the labor market and are unlikely to earn their way off assistance (Briggs, Popkin, and Goering, 2010; Theodos et al., 2012).

Finally, compared with households in the control group, households in both the experimental and Section 8 groups were less likely to be living in public housing and more likely to be using a

Exhibit 3

ITT/TOT Estimated Effect on Receipt of Housing Assistance

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Any housing assistance	0.620	0.026 (0.021)	0.054 (0.043)	0.045~ (0.027)	0.072~ (0.044)	3,273
Public housing	0.296	– 0.107* (0.017)	– 0.220* (0.035)	– 0.110* (0.022)	– 0.177* (0.036)	3,273
Voucher	0.252	0.159* (0.019)	0.328* (0.040)	0.194* (0.026)	0.312* (0.042)	3,273

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release.

Source: Adult long-term survey

voucher (16 and 19 percentage points more, respectively). This finding is not surprising, given that both groups were originally offered vouchers. More surprising is that 25 percent of the households in the control group were receiving vouchers at the time of the final impacts evaluation. This finding likely reflects the fact that 42 percent of MTO families started off in public housing developments that were later "HOPE VI'ed" and could have been offered a voucher during the relocation process (Sanbonmatsu et al., 2011).

Mixed Effect on Housing Instability

The ITT analysis found mixed results regarding whether being offered a voucher resulted in more instances of homelessness or doubling up. Being offered a voucher had no discernible additional effect on the amount of time spent homeless for the experimental or Section 8 groups (exhibit 4), a finding that did not support the hypothesis that the treatment would improve families' social and economic circumstances, enabling families to earn their way off housing assistance and possibly leaving them vulnerable to economic reversals. The Section 8 group, however, was 5 percentage points more likely than the control group to experience doubling up with friends and family. Looking at the groups who actually leased up with the voucher (TOT), as opposed to just being offered the voucher (ITT), the instances of doubling up for the Section 8 group increased to 7 percentage points more than the control group.

Exhibit 4

ITT/TOT Estimated Effect on Homelessness

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Homeless at least once after random assignment						
Ever homeless	0.214	0.020 (0.017)	0.040 (0.017)	0.056* (0.024)	0.090* (0.039)	3,273
Ever doubled up	0.191	0.008 (0.017)	0.017 (0.034)	0.045* (0.023)	0.073* (0.037)	3,273
Ever literally homeless	0.049	0.009 (0.009)	0.018 (0.019)	0.018 (0.014)	0.030 (0.022)	3,273
Number of homeless spells						
Doubled up	0.257	0.014 (0.026)	– 0.028 (0.033)	0.074* (0.035)	0.120* (0.056)	3,273
Literally homeless	0.061	0.003 (0.012)	0.006 (0.025)	0.024 (0.020)	0.038 (0.032)	3,273

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. "Doubled up" is defined as staying with friends or family when a respondent did not have a home of his or her own. "Literally homeless" is defined as staying in a shelter, on the street, in an abandoned building, in a car or van, in a movie theater or laundromat, and so on, when a respondent did not have a home of his or her own. Neither definition includes those staying at a hotel or motel, even when he or she did not have a place of his or her own.

Source: Adult long-term survey

Although results were mixed regarding whether being offered a voucher influenced homelessness or doubling up, 21 percent of control group participants reported experiencing being either doubled up or homeless at some point during the demonstration. Nearly one in five (19 percent) of the control group experienced at least one instance of doubling up and 5 percent experienced a spell of homelessness at some point during the demonstration.

Assessing whether these rates of homelessness and being doubled up are higher than usual for a population that has received housing assistance is difficult. The best benchmarks are the HOPE VI Panel Study, which found similar rates of homelessness among original residents of HOPE VI developments, and MTO participants at the time of the interim evaluation. The HOPE VI Panel Study found that at the time of the survey or during the previous 12 months, 1.7 percent of HOPE VI participants lived in a homeless shelter or on the streets and 3.9 percent lived in a doubled-up situation (McInnis, Buron, and Popkin, 2007). These shares were similar to those found in an analysis of all three groups of MTO participants at the time of the interim survey who reported that they did not live in the same housing unit for the past 12 months.

Another often-referenced random assignment research study, the Welfare-to-Work voucher study, found that housing vouchers dramatically reduced the risk of homelessness (Khadduri, 2008; Mills et al., 2006; Patterson et al., 2004). Participants in the Welfare-to-Work voucher study, however, were likely to start off in the unsubsidized private market, not in public housing like the MTO families. Only 13 percent of Welfare-to-Work participants reported receiving some type of housing assistance at baseline, before any treatment was applied (Mills et al., 2006). In addition, the effects were tracked for a much shorter period of time. Note that at baseline, 25.8 percent of Welfare-to-Work participants reported living with family or friends (comparable to the MTO doubled-up category) and 1.9 percent reported living in a homeless shelter or transitional housing; the percentage of MTO participants who reported experiencing homelessness at the time of the final impacts evaluation was twice as great. Because the populations differ so markedly, however, a clear comparison is difficult to make.

No Effect on Housing Affordability

Exhibit 5 shows that average housing costs for households in the control group at the time of the final impacts evaluation were relatively low, at $493 for monthly rent or mortgage and $679 total, even when compared with just the average rents in the five metropolitan areas. Because almost two-thirds of MTO heads of household were receiving federal housing subsidies, we would expect their housing costs to be relatively low. Furthermore, no statistically significant differences emerged between either the experimental and control groups or the Section 8 and control groups, except that low-poverty voucher group households paid approximately $10 per month more for electricity than control group households.

Whether the low-poverty voucher treatment would result in higher cost burdens for families in the experimental group was another question.[8] Economic improvements for households in the

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[8] Housing cost burdens are the ratio of the amount that families pay for their rent or mortgage plus utilities over their household income. HUD recommends that families pay no more than 30 percent of their income on housing costs; otherwise, they are considered to have high housing cost burdens.

experimental group could have resulted in families' "incoming out" of their housing assistance, removing an important safety net. As exhibit 6 shows, however, neither low-poverty nor traditional voucher offers had an effect, either positive or negative, on housing cost burden.

Exhibit 6 also shows that many MTO families had high housing cost burdens. More than two-thirds of the households in the control group paid more than 30 percent of their incomes on housing and 43 percent were severely burdened with housing costs, paying more than 50 percent of their

Exhibit 5

ITT/TOT Estimated Effect on Housing Costs

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Total housing costs	$678.73	19.50 (23.30)	39.67 (47.39)	– 6.26 (30.73)	– 10.00 (49.10)	3,180
Monthly rent or mortgage	$493.04	3.16 (20.16)	6.43 (41.01)	– 24.44 (26.13)	– 39.05 (41.76)	3,180
Electricity costs	$114.07	10.44* (5.30)	21.49* (10.91)	11.56 (7.36)	18.50 (11.78)	3,255
Gas costs	$73.81	4.49 (5.36)	9.23 (11.03)	5.31 (7.60)	8.49 (12.16)	3,255

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

* = p < .05.

Notes: Robust standard errors shown in parentheses. Costs are in 2009 dollars. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release.

Source: Adult long-term survey

Exhibit 6

ITT/TOT Estimated Housing Cost Burdens

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Household is housing cost burdened (monthly housing costs/income > 30%)	0.676	0.011 (0.020)	0.022 (0.041)	0.020 (0.027)	0.032 (0.043)	3,169
Household is severely housing cost burdened (monthly housing costs/ income > 50%)	0.426	– 0.004 (0.021)	– 0.007 (0.043)	0.017 (0.029)	0.027 (0.046)	3,169

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

Notes: Robust standard errors shown in parentheses. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release.

Source: Adult long-term survey

incomes on housing. This finding is surprising considering that two-thirds of MTO participants received housing subsidies that should have buffered households from spending so much of their incomes on housing.

A number of factors could contribute to these very high housing cost burdens. First, the information to calculate housing cost burdens comes primarily from self-reports provided during the survey and not from HUD administrative records that verify rents paid and family incomes. The survey asked families how much they pay for their portion of rent or mortgage, whether their utilities (that is, electricity and gas) are included in their rent, and, if not, how much utilities cost the previous month.[9] As in the case with the American Housing Survey, families did not have to provide documentation to prove amounts. Determining whether respondents provided information that represented the full private-market rent (as opposed to just their portion of rent) or whether they took the PHA's utility allowances into account when reporting their utility costs is also not possible.[10] The denominator of the housing cost burden—families' incomes—also comes from survey responses and, in some cases, administrative data sources. Researchers did not apply HUD's countable income rules and family income adjustments, which are used for Housing Choice Voucher Program (HCVP) programmatic purposes to the information collected from the survey, because much of the information would not be available. Therefore, any comparison of MTO families' housing cost burdens to other cost burdens that took HUD's income and programmatic calculations into account would be misleading.[11]

The fact that, under certain circumstances, subsidized families may pay more than originally expected also may explain the very high housing cost burdens. The HCVP is administered locally by PHAs under contract with HUD. Under voucher program rules, participants are responsible for finding and leasing a private-market unit that meets certain criteria (for example, HUD's Housing Quality Standards and rents must be "reasonable" or similar to comparable units). HUD subsidizes the voucher based on a payment standard set by the PHA between 90 and 110 percent of FMR. If the approved rent for the unit is equal to or less than the payment standard, participants pay 30 percent of their adjusted income toward rent and utilities and the PHA pays the difference. Should the rent and utility costs exceed the PHA's payment standard, the PHA pays the difference between the

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[9] The rent question included in the survey was, "Altogether in the month just passed, what did you pay in rent? We are interested only in knowing your part of the payment." The first question regarding utilities was, "Now I have some questions about your utilities. Do you pay for your own [specific utility] or is that included in the rent?" If the answer to the first question was that they pay for the utility, then the second question was, "How much was the [specific utility] bill last month?" (Sanbonmatsu et al., 2011)

[10] PHAs are free to decide their method of subsidizing utilities; some PHAs reimburse residents after payments are made and others pay directly to the utility company (HUD, 2003). In addition, housing authorities vary in terms of utility allowances. Information on utility allowance maximums for the different housing authorities is available on the PHA websites: Baltimore (http://static.baltimorehousing.org/pdf/2010util.pdf), Boston (http://www.bostonhousing.org/pdfs/LHS2011UtilityChart.pdf), Chicago (http://www.lakecountyhousingauthority.org/HousingChoiceVoucherProgram/Owners/ProgramRentsUtilityAllowance.aspx), Los Angeles (http://www.hacla.org/attachments/wysiwyg/149/Util-12-1-11MFR1.pdf), and New York (http://www.nyc.gov/html/nycha/html/section8/voucher_payment.shtml).

[11] Additional policy reasons could explain the high housing cost burdens. HUD applies prorated rents to households with one or more people without eligible citizenship status. By definition, all those households pay more than 30 percent of their adjusted income for rent. This policy could be a factor in Los Angeles, in particular.

payment standard and 30 percent of the participant's adjusted income, and the participant pays 30 percent of his or her adjusted income plus the additional rent and utility costs. If the total payment for a unit exceeds 40 percent of the recipient's income, however, the unit does not meet program requirements and cannot be rented with a voucher (Finkel and Buron, 2001). This hard cap at 40 percent of families' incomes applies only to those renting new units or to new participants using assistance in place, however. MTO families who have not moved in more than 1 year could be paying more than 40 percent of their incomes for housing if landlords increased rent.[12] The fact that the MTO demonstration occurred during a national housing boom (and bust) provides some evidence that rent increases could be another factor contributing to the surprisingly large housing cost burdens.

Housing Boom Contributed to High Housing Cost Burdens

The national housing boom affected all five MTO sites, creating serious challenges for voucher holders attempting to navigate the private market (Briggs, Popkin, and Goering, 2010). When the MTO demonstration began, the rental market was relatively soft (moderate vacancy rates and prices), particularly in Los Angeles, where participants were able to lease single-family homes in the San Fernando Valley. Starting in the early- to mid-2000s, prices soared for both owner-occupied and rental units. For example, controlling for inflation, home values in Boston's metropolitan area increased from an average of $343,533 in 2000 to $451,153 during 2005 through 2009. This increase was modest in comparison with that in the Los Angeles metropolitan area, where average home values increased from $384,905 in 2000 to $604,337 during 2005 through 2009.[13]

Meanwhile, the affordable housing stock plummeted, especially for low-income renters. From 2003 through 2009, the number of very low-income renters across the nation (with incomes of less than 50 percent of the area median) swelled from 16.3 to 18.0 million, while the number of rental units affordable at those income levels, not rented by higher income households and of adequate quality, dropped from 12.0 to 11.6 million. In 2009, extremely low-income renters (with incomes of less than 30 percent of area median) outnumbered affordable, available, and adequate units almost three to one (Steffen et al., 2011).

Exhibit 7 shows that in all five MTO sites, families faced housing markets in which average rents had increased substantially between the beginning and end of the decade.[14] Even after controlling for inflation, average monthly rents in Baltimore, Los Angeles, and New York increased more than $100 during this period. To a lesser extent, rents at the lower end of the spectrum also increased, particularly in Los Angeles, where the 25th percentile of monthly rent increased nearly $100.

With the exception of Baltimore, rental vacancy rates also started relatively high in 1990, giving unsubsidized and subsidized renters more opportunities to rent. Rental vacancy rates had tightened

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[12] For more information on the HCVP, see http://portal.hud.gov/hudportal/HUD?src=/program_offices/public_indian_housing/programs/hcv/about/fact_sheet.

[13] Home values are from the 2000 Census and the 2005/2009 American Community Survey. Values are CPI-adjusted to 2009 U.S. dollars.

[14] Exhibit 7 shows the increases in rent at the city level. The trends for the metropolitan statistical areas are similar.

considerably by 2000, meaning that low-income renters faced a much more challenging housing market.[15] These tight rental markets also could have encouraged MTO families to stay put and attempt to pay increased rents out of pocket. Exhibit 8 shows that vacancy rates rose again after 2005, presumably reflecting the national recession.

Exhibit 7

Rent for MTO Cities

	Average Rent ($)			25th Percentile Rent ($)		
	2000	**2005/09**	**Difference**	**2000**	**2005/09**	**Difference**
Baltimore	644	809	166	369	434	65
Boston	1,045	1,145	100	544	560	16
Chicago	833	911	78	511	559	48
Los Angeles	934	1,116	182	596	693	97
New York	977	1,110	133	589	636	47

MTO = Moving to Opportunity.
Notes: Data are at the city level. Reported in 2009 dollars.
Sources: 2000 Census; 2005/2009 American Community Survey

Exhibit 8

Average Rental Vacancy Rates

	Rental Vacancy Rates (%)		
	1990	**2000**	**2009**
Baltimore	7.7	7.7	11.8
Boston	7.9	3.2	6.1
Chicago	9.7	6.4	8.3
Los Angeles	6.8	3.8	5.6
New York	4.2	3.5	3.8

Sources: 1990 and 2000 Census; 2009 American Community Survey

Challenges of Managing the Private Market

One question raised at the beginning of the MTO demonstration was whether families from distressed public housing who received vouchers would be able to meet the private-market standards of paying rent and utilities on time. For many experimental and Section 8 group families, these standards were a first-time experience, and our analysis indicates that participants, particularly the experimental group families, appeared to be making tradeoffs between keeping up with rent payments and paying utilities. This pattern is consistent with findings from MTO families collected after the interim impacts evaluation (Briggs, Comey, and Weisman, 2010) and research on HOPE VI relocatees who move from distressed public housing to the private market (Levy and Woodley, 2007; Popkin et al., 2002; Popkin, Levy, and Buron, 2009).

Exhibit 9 shows that participants in both the experimental and Section 8 groups were less likely to be more than 15 days late in paying their rent or mortgage than were participants in the control

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[15] Exhibit 8 shows the rental vacancy rates at the city level. The trends for the metropolitan statistical areas are similar.

Exhibit 9

ITT/TOT Estimated Effect on Housing Stability and Late Utility Payments

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Housing stability						
More than 15 days late in paying rent or mortgage at least once during past 12 months	0.258	– 0.055* (0.018)	– 0.112* (0.037)	– 0.067* (0.024)	– 0.106* (0.038)	3,242
Received eviction or foreclosure threat because of non-payment at least once during past 12 months	0.124	– 0.017 (0.014)	– 0.035 (0.028)	– 0.033~ (0.018)	– 0.053~ (0.028)	3,261
Late utility payments (renters only)						
More than 15 days late in paying utilities at least once during past 12 months	0.333	0.052* (0.020)	0.108* (0.041)	0.033 (0.027)	.0530 (0.043)	3,236
Received shutoff notice because of nonpayment at least once in past 12 months	0.235	0.054* (0.018)	0.112* (0.038)	0.050* (0.025)	0.081* (0.040)	3,236
Utilities shut off for nonpayment at least once in past 12 months	0.052	0.021* (0.010)	0.043* (0.022)	0.016 (0.014)	0.026 0.022	3,236

ITT = intention to treat. OLS = ordinary least squares. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. Experimental and Section 8 effects were estimated jointly using an OLS regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release.

Source: Adult long-term survey

group (6 and 7 percentage points, respectively). No differences emerged in eviction rates between the experimental and control groups, however. The Section 8 group was only slightly less likely than the control group to be evicted (significant at the p < .10 threshold). Experimental group participants, however, were significantly more likely to report both making late utility payments and having their utilities shut off. For instance, experimental group participants were 5 percentage points more likely to be 15 days late paying their utilities, 5 percentage points more likely to have received shutoff notices for their utilities, and 2 percentage points more likely to have had their utilities shut off for nonpayment compared with control group participants.

Conclusion

At its core, MTO was a housing intervention offering options to families living in some of the worst public housing developments in the nation. MTO demonstrated that giving low-income families vouchers results in higher quality housing compared with lower quality public housing or project-based assisted housing in both the short term (as evidenced by the TOT effect at the time of the survey for the interim impacts evaluation) and the long term (the effects were sustained by the time of the survey for the final impacts evaluation, particularly for the experimental group). This result is supported by studies of other similar populations (Popkin, Levy, and Buron, 2009).

These housing quality improvements could have acted as mediators contributing to the significant gains for MTO participants in mental and physical health outcomes (Sanbonmatsu et al., 2011). The health improvements could either be in response to physical improvements, such as a lack of vermin and mold, or through stress reduction and a general improvement in quality of life. Either way, the importance of these gains for families' well-being cannot be overstated.

The MTO demonstration proved not to have much effect on many of the other housing outcomes tracked. A surprisingly large share of MTO participants continued to rely on housing assistance 10 to 15 years after the start of the demonstration. The Section 8 group experienced more instances of doubling up with friends and family than the control group did, which may indicate that being offered vouchers somehow contributes to housing instability, although this higher instance of doubling up was not found for the experimental group. The MTO demonstration did not affect homelessness (for example, living in a shelter or on the street), and whether the overall MTO participants' share of homelessness at the time of the final impacts evaluation is more or less than expected is unknown. This area could be explored further.

In addition, even with such high shares of families continuing to use housing assistance, housing costs continue to be very high for all MTO participants, even those still receiving subsidies. One possible explanation is that the nation went through a housing boom that could have resulted in more low-poverty and traditional voucher holders staying in place and paying more out of pocket for rent.

Finally, some evidence suggests that MTO did result in more challenges for the experimental group in navigating the private market. Although families in the experimental group were more likely than families in the control group to pay rent on time, they were also more likely to make a tradeoff in paying their utilities late or not at all, which resulted in having the utilities turned off. Again, this finding is consistent with other research on families moving from public housing to the private market and suggests a need for greater attention to helping voucher holders meet the costs of utilities in private-market units.

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The Long-Term Effects of Moving to Opportunity on Adult Health and Economic Self-Sufficiency

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Abstract

Adults living in high-poverty neighborhoods often fare worse than adults in more advantaged neighborhoods on their physical health, mental health, and economic well-being. Although social scientists have observed this association for hundreds of years, they have found it difficult to determine the extent to which the neighborhoods themselves affect

Abstract (continued)

well-being versus the extent to which people at greater risk for adverse outcomes live in impoverished neighborhoods. In this article, we examine neighborhood effects using data from the 10- to 15-year evaluation of the Moving to Opportunity (MTO) for Fair Housing demonstration, which offered randomly selected families a housing voucher. The experimental design of MTO allows us to isolate the effects of neighborhoods from selection bias. We find that, 10 to 15 years after enrolling participants, the program had very few detectable effects on economic well-being but had some substantial effects on the physical and mental health of adults. For adults whose families received the offer of a housing voucher that could be used to move only to a low-poverty neighborhood, we find health benefits in terms of lower prevalence of diabetes, extreme obesity, physical limitations, and psychological distress. For adults offered a Section 8 voucher, we find benefits in terms of less extreme obesity and lower prevalence of lifetime depression.

Introduction

Adults living in disadvantaged neighborhoods have worse health and economic outcomes than people living in less distressed areas. Previous research has shown that living in a high-poverty or high-crime neighborhood is associated with an increased risk of poor overall health status, premature mortality, heart disease, obesity, serious mental illness, suicide, unemployment, and dropping out of school (Bagley, Jacobson, and Palmer, 1973; Diez-Roux and Mair, 2010; Kawachi and Berkman, 2003; Macintyre and Ellaway, 2003; Pickett and Pearl, 2001; Rezaeian et al., 2005; Weinberg, Reagan, and Yankow, 2004; Whitley et al., 1999).

What remains less clear is the extent to which such variation in people's well-being across neighborhoods reflects the causal influence of the neighborhood environments themselves, beyond the variation explained by the nonrandom sorting of individuals among residential neighborhoods. People who are at elevated risk for adverse health or labor-market outcomes may face limited housing options and may be more likely to live in distressed neighborhoods. Observational (or nonexperimental) studies try to isolate the independent effects of neighborhoods on people's well-being by statistically controlling for observable risk and protective factors of individuals and their families. Studies that control for a larger number of individual factors tend to find smaller neighborhood effects than do other studies (Pickett and Pearl, 2001). In addition, substantial concerns remain that some of the key variables that directly affect both neighborhood selection and health or economic outcomes are missing from or are imperfectly measured in standard data sets. Observational studies may confound the influence of neighborhood environments with the effects of unmeasured individual or family attributes that directly affect outcomes and neighborhood selection, a problem that social scientists call selection bias.

The existence, magnitude, and nature of any causal neighborhood effects on economic and health outcomes are of great relevance to housing policies that affect the geographic concentration of poverty in America. An understanding of neighborhood effects can inform policy choices related to: (1) whether to help low-income families meet their housing needs through project-based housing or, instead, through housing vouchers; (2) the types of relocation assistance and support to provide voucher holders; (3) where to locate new public housing projects; and (4) the potential value of mixed-income housing projects that subsidize nonpoor families to live near poor families. The nature of neighborhood effects is also relevant to the design of other social policies, such as suburban zoning rules and education policies that sort children across different schools and influence how low-income families are distributed across social settings (Duncan and Murnane, 2011). These choices also have important implications for health policy decisions, such as the degree to which governments allocate public health investments for low-income areas to traditional safety-net health care instead of changing "the context to make individuals' default decisions healthy" (Frieden, 2010: 590). For example, policymakers could allocate investments to creating safe green space for walking and sports, which might lead people to choose more physical activities over sedentary activities.

To more convincingly identify the effects of neighborhood environments on low-income families and to overcome selection bias concerns that may have compromised previous studies, the U.S. Department of Housing and Urban Development (HUD) launched the Moving to Opportunity (MTO) for Fair Housing demonstration, a residential mobility experiment, in the early 1990s. The MTO program targeted families living in public or project-based housing in highly impoverished neighborhoods. MTO was open to families living in Baltimore, Boston, Chicago, Los Angeles, and New York City. Between 1994 and 1998, more than 4,600 families enrolled in the MTO program (Goering, Feins, and Richardson, 2003; Goering et al., 1999). To be eligible, a family had to have at least one child younger than age 18 and live in public housing or project-based assisted housing in a high-poverty area, defined as a census tract in which the income of more than 40 percent of residents falls below the federal poverty threshold.

MTO randomly assigned eligible families to one of three groups:

- Families in the **experimental group** received Section 8 rental assistance certificates or vouchers that, initially, they could use only in low-poverty census tracts—those in which less than 10 percent of residents had incomes below the federal poverty threshold in 1990. Nonprofit organizations in each city provided mobility counseling to help families find and lease a unit in a low-poverty area. After 1 year in the low-poverty tract, families could use the voucher to live in any neighborhood. In addition to abiding by the geographic requirement, families had to abide by all of the regular rules of the Section 8 certificate and voucher programs.

- Families in the **Section 8 group** also received Section 8 certificates or vouchers to move into private-market housing but without any mobility counseling or additional locational constraints under the MTO program design.

- Families in the **control group** did not receive any housing vouchers or certificates through MTO but continued to be eligible for all programs and services to which they would be otherwise entitled.

Random assignment enables us to isolate the effects of different neighborhood and housing conditions on people's well-being by comparing the average outcomes of groups of families who are similar on average in every way at baseline except that some were subsequently offered the opportunity to use a housing voucher to move to a new neighborhood with different characteristics.

In this article, we compare adults in the two treatment groups with the adults in the control group on their health and economic self-sufficiency 10 to 15 years after they enrolled in the program. Before turning to the results of our analyses, we first discuss the hypothesized pathways through which moving to a lower poverty neighborhood might affect adult health and economic self-sufficiency. We then summarize the results of previous waves of MTO research and other studies. We describe our sample and analytic strategy and then present estimates of the long-term effects of MTO on selected mental health, physical health, and economic outcomes. (Ludwig et al., 2011, and Sanbonmatsu et al., 2011, present additional results for other health and economic outcomes.)

We find that, 10 to 15 years after random assignment, MTO moves had sizable effects on a number of important physical and mental health outcomes but no detectable effects on almost any measure of economic self-sufficiency. More specifically, in the area of mental health, we find that MTO moves led to lower levels of psychological distress and lower prevalence of depression. In the area of physical health, we find beneficial effects on severe obesity, diabetes, and physical limitations, although we do not detect effects on hypertension or overall self-rated health. Earnings and employment rates have risen for all random assignment groups since they initially joined the program, and very few statistically significant differences emerged across the randomized groups on economic outcomes. We conclude this article with a discussion of some of the limitations of our findings and their implications for future research and policy design.

Hypothesized Pathways

The MTO program offered families in the two treatment groups a housing voucher that they could use to relocate from a public housing project to an apartment (or house) in another neighborhood. Families in both the experimental and Section 8 treatment groups could use the voucher to move to a low-poverty neighborhood, but for the experimental group, moving to a low-poverty area was an initial requirement of using the voucher at all. Exhibit 1 illustrates our hypothesis that moving to a lower poverty area might affect adults' outcomes by changing their social environment, the resources of their community, and their physical environment.

Changes in the Social Environment

One set of potential pathways involves the *social environment*—the environment created by the people living in the neighborhood. Moving to a more affluent area may reduce exposure to violence because of a safer social environment, expose movers to higher socioeconomic status (SES) peers, and offer new social networks. At the same time, moving may disrupt social ties such as relationships with families, friends, service providers, and faith communities.

A safer environment could improve physical health directly by reducing the likelihood of injuries from assaults and indirectly by providing a safer environment for outdoor activity such as exercise.

Exhibit 1

Hypothesized Pathways Through Which MTO May Affect Adult Well-Being
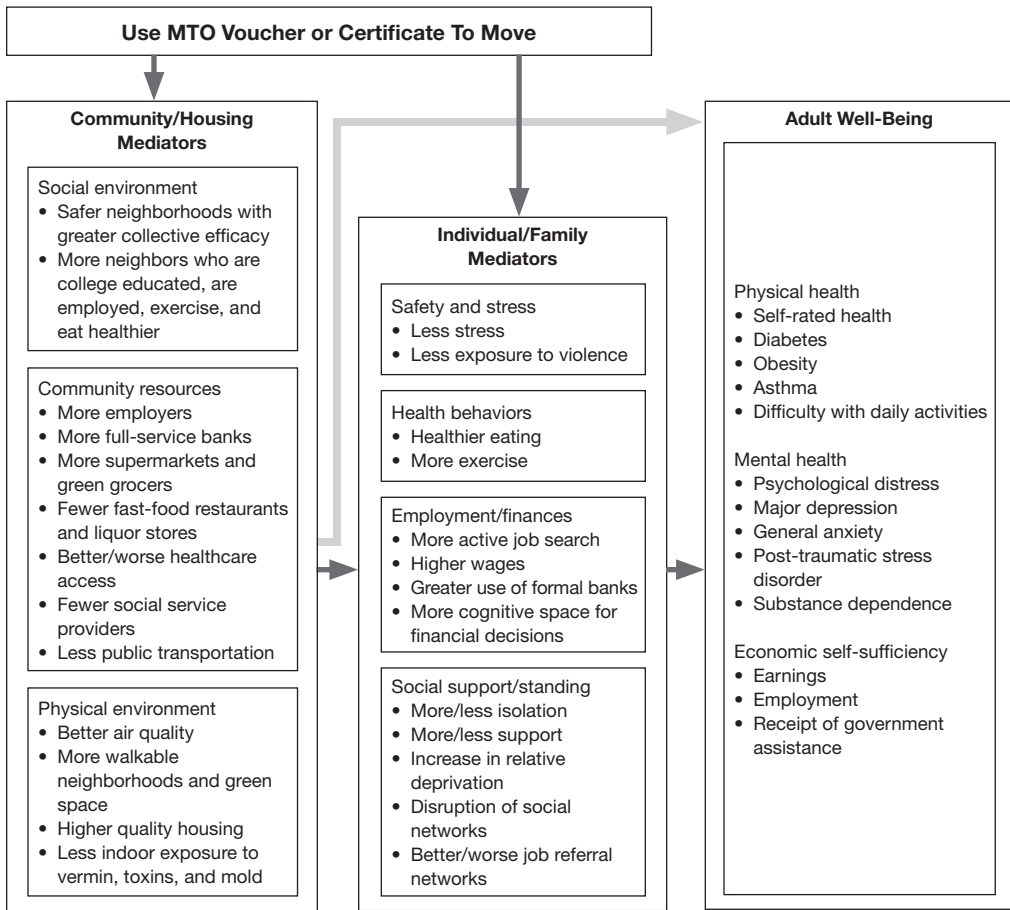
MTO = Moving to Opportunity.

Note: This figure builds on the pathway diagrams shown in various chapters of the MTO interim impacts evaluation (see Orr et al., 2003).

Unsafe neighborhoods may discourage outdoor physical activity and exercise (Bennett et al., 2007; Harrison, Gemmell, and Heller, 2007). Lower likelihoods of being the victim of violence and of witnessing violence could also improve mental health, because these types of exposures are associated with higher levels of psychological distress, depression, generalized anxiety disorder (GAD), and post-traumatic stress disorder (PTSD) (Ross and Mirowsky, 2001; Silver, Mulvey, and Swanson, 2002; Zapata et al., 1992). Neighborhoods with lower crime may also generate higher levels of trust and greater *collective efficacy* (the willingness of neighbors to work together and support shared values), which may help serve as a buffer against other types of stressors (Berkman and Glass, 2000; Cohen, 2004; Sampson, Raudenbush, and Earls, 1997).

Any MTO effects on mental health may amplify effects on physical health, given the link between chronic stress and the regulation of physiological systems and behavior (McEwen and Stellar, 1993; Merkin et al., 2009). Previous research has associated higher stress levels with hypertension (Kornitzer, Dramaix, and De Backer, 1999; NIH, 1997) and asthma attacks (Bloomberg and Chen, 2005; Wright, Rodriguez, and Cohen, 1998). Stress may lead people to increase their intake of dense-calorie foods (Torres and Nowson, 2007) and to turn to alcohol, tobacco, or other substances in an effort to cope. The new social environment may also expose movers to neighbors with higher education levels who, at least on average, may have healthier lifestyles (Darmon and Drewnowski, 2008; Pampel, Krueger, and Denney, 2010).

Higher SES neighbors and less stress may improve economic prospects. Living in a neighborhood with a greater share of employed adults may provide more social support for work (for example, Maurin and Moschion, 2009) or more referrals for job openings (Bayer, Ross, and Topa, 2008). Moving out of a stressful environment may free up cognitive space for deliberative cognitive processes, such as financial planning (Porcelli and Delgado, 2009).

The degree to which having new neighbors translates into actual behavioral changes may depend on how much exposure adults have to their new neighbors and how well they connect with them (Christakis and Fowler, 2007). Similarly, new referral networks may not be helpful if new neighbors know mostly about jobs in occupations and industries that are not relevant for adults in the MTO program. Thus, moving to a more affluent area need not have any beneficial effects on behavior. Moving could even have negative effects if the move itself initially disrupted relationships and left adults feeling socially isolated, particularly if they do not have much in common with their new neighbors. MTO participants who move to lower poverty areas could also experience a decline in their socioeconomic standing relative to their new peers if their incomes do not rise with the move. Feelings of relative deprivation could negatively affect their physical and mental health (Stafford, 2003). In addition, although higher income people tend to lead healthier lifestyles on some dimensions, other health-risk behaviors, such as drinking, may be more prevalent among more affluent people, at least over part of the SES distribution (Pollack et al., 2005; SAMHSA, 2010). For immigrant families in MTO, moving to a more affluent area could mean greater acculturation and a less healthy lifestyle (Lara et al., 2005).

Changes in Community Resources

Part of the hope of mobility programs such as MTO is that higher SES communities can offer low-income families better access to community resources such as jobs, high-quality schools, high-speed broadband infrastructure, and supermarkets. Research studies generate mixed findings, however, as to whether more advantaged neighborhoods always offer low-income families greater access to resources.

Scholars hypothesize that geographic proximity to employers may be important for economic self-sufficiency. The spatial mismatch theory of John Kain (1968) suggests that a mismatch between the location of manufacturing and other jobs and the concentration of low-income families in urban areas might account for some of the lower employment levels observed. More recently, a series of studies in the late 1980s and early 1990s linked neighborhood poverty and job accessibility to labor force participation and wages for African-American and Hispanic workers (see Ihlanfeldt and

Sjoquist, 1998, for a review). If MTO moves enable access to a broader range of employment opportunities, they may also lead to increased wages and fringe benefits. Lower poverty areas, however, are not always closer than higher poverty areas to the sort of job opportunities that may be relevant for lower skilled MTO families (Anil, Sjoquist, and Wallace, 2010). Higher SES neighborhoods may offer increased access to formal banks as opposed to high-cost alternatives such as check cashers and payday lenders, who seem to target high-poverty areas (see Graves, 2003, for details), and thus neighborhoods may improve economic welfare through better personal finance management.

The types of retail stores, restaurants, and advertisements in a new neighborhood might affect the health of low-income people who move there. The presence of more grocery stores could improve diet by reducing the purchase price or transportation cost of healthy food (Morland et al., 2002). The presence of fewer liquor stores and fast-food restaurants could also affect diet and potentially reduce the consumption of high-caloric foods and alcohol (Dubowitz et al., 2008; Franco et al., 2008; Inagami et al., 2006; Zenk et al., 2005).

Living in a community with greater resources does not mean families will actually be able to take advantage of those resources or that those resources will be well suited to their needs. One reason low-income people may concentrate in urban cores is the availability of public transportation, which, in general, is more affordable than maintaining a private car (Glaeser, Kahn, and Rappaport, 2008). If new neighborhoods offer more limited public transportation options, MTO moves could reduce access to jobs, grocery stores, and other services. Furthermore, even if overall access to services remains the same or improves, access to certain types of social services could be worse if service providers for the poor are less common in higher income areas (Allard, 2004; Small and Stark, 2005).

For similar reasons, the effects of moves on healthcare access are hard to predict. The MTO demonstration took place in cities with some of the top-ranked hospitals in the country, many of them academic medical centers located, for historical reasons, near economically disadvantaged inner-city neighborhoods.[1] These academic medical centers may be more experienced in meeting the linguistic and cultural needs and daily realities of low-income patients than are the healthcare facilities located in more affluent areas. Moreover, free or very low-cost primary healthcare services may be concentrated in high-poverty areas, and so access to low-cost healthcare services could decrease with moves to better neighborhoods.

Changes in the Physical Environment

Finally, moving to a less impoverished neighborhood may influence well-being through changes in the physical environment of the neighborhood and the housing unit. Higher SES neighborhoods may have lower pollution, trash, and vandalism levels; higher quality buildings and outdoor spaces; and built environments more conducive to exercise. The built environment of a neighborhood—its buildings, parks, and streets—may also affect health behaviors such as exercise. Studies have associated greater walkability of a neighborhood with greater physical activity and lower

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[1] Four of the five MTO cities are home to at least 1 of the top 14 honor-roll hospitals, as ranked by *U.S. News & World Report* on the basis of specialty rankings (Comarow, 2010).

rates of overweight and obesity (Lovasi et al., 2009; Mujahid et al., 2008; Sallis et al., 2009). Other studies have found green space buffers stress, especially in lower income socioeconomic groups (Maas et al., 2006), and have linked compromised outdoor air quality, which may be present in low-income neighborhoods, to coronary heart disease (Kan et al., 2008).

Poor housing quality can increase exposure to household dangers such as vermin, toxins, mold, and poor ventilation. These hazards may lead to a greater incidence of asthma (Bryant-Stephens, 2009). Poor-quality housing may also affect mental health: a study by Galea et al. (2005) links living in a neighborhood with dilapidated buildings to a greater incidence of depression after controlling for individual demographics and neighborhood median income. Poor-quality housing can also increase injuries from falls and fires and exposure to noise that can impair sleep. We expect to observe improvements in the health of participants who are able to move into better apartments using MTO vouchers.

Summary of Hypotheses

We hypothesized that, compared with adults in the control group, adults offered the opportunity to move to lower poverty neighborhoods would have better mental health, physical health, and economic outcomes through improved neighborhood safety, less stressful environments, greater community resources, positive peer influences, and fewer environmental hazards. We expected to see lower incidences of mental illness such as depression, anxiety, and stress-related disorders as compared with the control group. We also expected to see physical health improvements in terms of lower rates of asthma, obesity, diabetes, and hypertension. We also hypothesized that families who moved to neighborhoods with improved conditions would have higher levels of employment and earnings and would receive less public assistance.

Previous Findings From MTO and Other Studies

MTO was inspired by findings from the Gautreaux residential mobility program in Chicago, which was part of the legal settlement of a racial discrimination case. Gautreaux gave African-American families living in the inner city an opportunity to move to new neighborhoods. The initial Gautreaux findings suggested better employment outcomes for low-income African-American families living in public housing who moved to predominantly White suburbs compared with those of their counterparts who moved to predominantly African-American and urban neighborhoods (Rosenbaum, 1995). Gautreaux, however, was not a randomized study and the neighborhood preferences of Gautreaux families may, at least in part, have influenced where they ended up living (Votruba and Kling, 2009). Furthermore, over the long term, the program shows little in the way of gains in self-sufficiency for Gautreaux mothers, suggesting that one might not expect to see economic gains for MTO women (DeLuca et al., 2009).

HUD launched MTO to more systematically evaluate the types of neighborhood effects found in the Gautreaux study. An interim evaluation of MTO was conducted in 2002 by Abt Associates Inc. in partnership with researchers at the National Bureau of Economic Research (Orr et al., 2003). The evaluation examined effects an average of 4 to 7 years after the families joined MTO. These interim findings suggested that adults in the experimental group had lower levels of psychological

distress and felt calmer than adults in the control group. The results also hinted at lower levels of depression, although these results were sensitive to the construction of the depression measure. Based on self-reported height and weight, the treatment appeared to reduce obesity levels (Body Mass Index [BMI] of 30 or greater) for those in the experimental group (42.0 percent) in comparison with those in the control group (47.1 percent) and perhaps to increase rates of exercise and improve diet (Orr et al., 2003). The results, however, did not show significant effects on other health measures, such as self-rated health, hypertension, physical limitations, asthma, and a summary health index (Kling, Liebman, and Katz, 2007; Orr et al., 2003). Nor did the results show any statistically significant effects on economic outcomes, such as employment or earnings of the adults or youth.

Other studies of residential mobility programs have also yielded mixed findings on economic outcomes. Oreopoulos (2003) took advantage of the fact that public housing units in the city of Toronto were located in different types of neighborhoods to compare the outcomes of families living in different types of neighborhoods.[2] He found no evidence of improved economic outcomes for youth who grew up in higher SES neighborhoods. Studies of HUD's HOPE VI public housing demolition programs also yielded mixed results. Levy's (2010) study of families who relocated through HOPE VI suggested that families who relocated because of the demolition of their public housing projects experienced improved neighborhood conditions, but their economic self-sufficiency changed little. In contrast, Anil, Sjoquist, and Wallace (2010), studying the HOPE VI demolitions in the Atlanta area, found evidence of employment gains.

The 10- to 15-Year Evaluation

Our 10- to 15-year followup with MTO families (Sanbonmatsu et al., 2011) extends beyond previous MTO research by studying the long-term effects of MTO; expanding the outcomes examined; and using objective measures of health in conjunction with self-reports on health. We expanded the MTO long-term data collection to include new outcomes, such as diabetes and PTSD. In addition, we replaced the brief questions on problems such as anxiety with more detailed and widely used structured diagnostic interview instruments, and we replaced self-reported height and weight measures with anthropometric measurements taken by the interviewers. For the first time, we gathered finger-stick dried blood spot samples from MTO respondents, enabling us to measure biological risk factors and undiagnosed disease.

The long-term survey for the final impacts evaluation enables us to examine how effects have changed over time. MTO's effects might have followed three very different trajectories. Program effects might have faded over time as the average neighborhood environments of the two treatment groups and the control group converged, which could occur if families in the two treatment groups moved back to their old neighborhoods, if families in the control group moved out of public housing on their own (and into similar low-poverty areas as the treatment groups), or if

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[2] The Toronto housing authority offered a housing unit with the necessary number of bedrooms to high-need families who reached the top of the waiting list on a first-available basis; families could not specify the housing project or type of housing project that they wanted to live in (Oreopoulos, 2003).

the neighborhoods themselves changed over time. Alternatively, MTO effects on families might have strengthened over time as the initial disruptions of moving diminished and families became increasingly connected to their new communities. Greater connections to neighbors in low-poverty areas might have produced continued changes in diet and exercise patterns or new job referrals. Over time, we might have seen even larger MTO effects on health and the emergence of MTO effects on economic self-sufficiency. Or, effects might have followed a more intermediate path, whereby families maintained the initial benefits from moving that reached a plateau in the early years of the program. This latter scenario might have occurred if the moves led to some initial change that persisted over time, even as families made additional moves, but that was a one-time change that did not continue to increase. For example, moving from public housing to private-market rentals might reduce the noise level in the home. This improvement might persist with subsequent moves and lead to greater mental calm or improved sleep, but it may be a one-time shift with no additional gains over time.

Sample and Analytic Approach

Our sample draws on the adults from the original MTO households. To measure MTO's impacts over the long term, we selected up to one adult for interview in each MTO household.[3] The Institute for Social Research (ISR) at the University of Michigan interviewed adults using a computer-assisted survey between June 2008 and April 2010 (10 to 15 years after families were randomized in the program). Interviewers asked questions about the adult's health and economic circumstances, took physical measurements, and collected blood samples with a simple finger stick (McDade, Williams, and Snodgrass, 2007). ISR used a two-stage field design. In the first stage, ISR tried to interview as many adults in the survey sample as possible. After the response rate reached 75 percent for a site and sample release, ISR randomly selected 35 percent of the remaining, hard-to-reach respondents for the second stage of more intensive survey recruitment efforts. In all, ISR interviewed 3,273 adults and achieved an overall effective response rate of 90 percent (excluding deceased adults). In addition to collecting data from the survey, we gathered data from administrative records.

Exhibit 2 shows the baseline characteristics of the adults interviewed for the final impacts evaluation. At baseline, the vast majority (92 percent) of households were female headed and three-fourths of household heads were on welfare. The median household income of interviewed participants was $10,614 (in 2009 dollars) in the year preceding entry into MTO. Only about one-fourth of adults were working. Slightly more than one-third of adults in MTO families had graduated from high school. Nearly two-thirds were African American and most of the rest were Hispanic. The average age of our interviewed sample adults at the time they joined the program was 32.9, and the average family size was about 3.7 members.

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[3] In selecting the adult survey sample, we prioritized female adults and household heads from the core family, adopting the same approach used for the interim impacts evaluation (Orr et al., 2003). We selected for interview one adult from each family in the experimental group and the control group. Because of funding constraints, we were unable to interview adults from all families in the Section 8 group and instead randomly selected a 68-percent subsample of these families for adult interviews.

Exhibit 2

Baseline Characteristics of the Adult Analytic Sample, by Random Assignment Group

	Control Group	Experimental Group	Section 8 Group	All Groups
Household characteristics				
Number of family members	3.7	3.7	3.7	3.7
Female head of household	92.1%	91.9%	93.6%	92.4%
Employment status				
Full-time	14.2%	14.1%	15.2%	14.5%
Part-time	9.4%	12.1%	11.0%	10.9%
Received welfare benefits				
Social Security or disability benefits	8.4%	9.0%	7.3%	8.4%
AFDC or TANF	76.3%	76.3%	73.6%	75.6%
Takes more than 30 minutes to get to…				
Grocery store	21.4%	21.5%	22.2%	21.7%
Doctor	45.0%	42.7%	43.6%	43.7%
Household income (2009 dollars)				
Average income	$12,439	$12,866	$12,788	$12,709
Median income	$10,353	$10,629	$10,892	$10,614
Adults interviewed at 10 to 15 years				
Age at baseline	32.8	32.9	33.1	32.9
Number interviewed	1,139	1,456	678	3,273

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

Notes: Percentages are percent distributions and may not sum to 100 because of rounding and missing information. Data are weighted using the survey weights.

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

HUD asked families applying to the program about their primary and secondary reasons for wanting to move. By far, the most common reason for signing up for MTO was to get away from drugs and gangs (77 percent of adults listed this reason first or second), followed by finding better schools (49 percent) and finding a better apartment (44 percent; Sanbonmatsu et al., 2011, Exhibit 1.2). The importance of safety in motivating families to participate in MTO is perhaps not surprising, given that more than two of every five baseline respondents said that someone in their household had been the victim of a crime during the 6 months preceding the baseline survey.

We present two types of estimates for how MTO affected the life outcomes of participating adults. The first estimate, the intention-to-treat (ITT) effect, represents the effect of being *offered* a housing voucher or certificate, which we generate by comparing the outcomes of all adults randomly assigned to the experimental or Section 8 group (regardless of whether those adults moved with a program voucher) with the outcomes of all adults assigned to the control group. We calculate the ITT estimate using an ordinary least squares regression in which the outcome of interest is the dependent variable being predicted on treatment group assignment and a series of baseline covariates. The basic regression equation is

$$Y_i = \alpha_0 + \alpha_1 Exp_i + \alpha_2 S8_i + \alpha_3 X_i + e_i, \tag{1}$$

where Y_i is an outcome for MTO program participant i; Exp_i and $S8_i$ are binary indicator variables equal to 1 if participant i was randomly assigned to the experimental or Section 8 group (the control

group is the omitted reference group); and X_i represents a series of baseline covariates that Sanbonmatsu et al. (2011) described and that is similar to the covariates Orr et al. (2003) used. The coefficients on Exp_i and $S8_i$ capture the ITT estimates for the experimental and Section 8 groups, respectively. The equation weights the data to account for adjustments to the randomization ratios during the study enrollment period and the probability sampling.

If we are willing to assume that assignment to one of the treatment groups does not have much effect on those families who were offered a voucher but did not use it, we can also estimate the effect of actually moving using an MTO program voucher, known as the effect of treatment on the treated (TOT).[4] We can calculate the TOT effect by dividing the ITT effect by the share of the experimental or Section 8 group that relocated with an MTO voucher (Angrist, Imbens, and Rubin, 1996; Bloom, 1984). Because approximately one-half of the families in the experimental group used the MTO program voucher, the estimated TOT effect will be about twice as large as the ITT effect (that is, TOT ≈ ITT/0.5 ≈ ITT × 2). The statistical significance of both the ITT and TOT estimates are identical under this calculation, because we scale up the standard error and the impact estimate by the same factor (1/voucher use rate).

Measures of Health and Economic Self-Sufficiency

Mental Health Measures

To assess adult mental health outcomes, we used responses on the survey and the structured diagnostic interview within the survey. We measured psychological distress with the Kessler 6 scale (K6), which consists of questions about sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, and feelings of worthlessness (Kessler et al., 2003). The raw scores from the K6 can range from 0 (no distress) to 24 (highest level of distress). We assessed major depressive disorder, GAD, and PTSD using the World Health Organization's Composite International Diagnostic Interview (CIDI; Kessler and Ustün, 2004), which is designed to be consistent with the *Diagnostic and Statistical Manual of Mental Disorders*, 4th edition Task Force on DSM-IV (2000). To meet the criteria for major depression, a respondent's depressed mood or loss of interest had to last for a period of at least 2 weeks and be accompanied by at least five of the following symptoms: depressed mood, diminished interest or pleasure, unintentional weight loss or gain, insomnia, restlessness or slowing down, fatigue, feelings of worthlessness or excessive guilt, diminished ability to concentrate, and recurrent thoughts of death. Furthermore, these symptoms had to cause significant distress or impair the respondent's functioning at work or in social situations.

GAD required that the adult experience a period of at least 6 months in which they had excessive anxiety about multiple things and at least three of the following symptoms: restlessness, easy fatigue, difficulty concentrating, irritability, muscle tension, and sleep disturbance. In addition, the anxiety

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[4] Our TOT estimates assume that families who are offered a voucher but do not take up the offer (that is, do not use a program voucher to move) are not affected by the offer itself. This assumption may not be strictly true, because families may have changed some of their thinking or behaviors (such as looking at apartments) as a result of the offer. We think, however, that the effects of actually using the voucher are likely to be much larger than any effects of being offered the voucher and not using it, and that it is reasonable to assume that the ITT effects are driven strictly by effects on compliers.

had to cause significant distress or impair respondents' functioning. Our measure of PTSD used a subset of the CIDI questions and then imputed the probability of PTSD from those responses using data from a national study.[5] To be categorized as having PTSD, the respondent had to have experienced, witnessed, or been confronted by a traumatic event that involved actual or threatened death or serious injury to themselves or others,[6] and the trauma had to invoke at least three of the following symptoms: avoiding activities, places, or people that arouse recollections of the trauma; reduced interest in activities; feelings of detachment; restricted range of feelings; and a foreshortened sense of the future. PTSD also involves difficulty falling or staying asleep, an exaggerated startle response, and impairment of functioning.

We measured alcohol or drug dependence using the Severity of Dependence Scale (SDS). The SDS consists of five questions about out-of-control use, anxiety or worry about missing a fix or a drink, worry about use, frequency of desire to end use, and difficulty of going without use (Gossop et al., 1995). The SDS ranges from 0 to 15, and we consider a score of 3 or greater to indicate substance dependence.

Physical Health Measures

We studied physical health outcomes using a combination of survey questions, physical measurements, and assays from dried blood spot samples. The survey asked adults if their health was excellent, very good, good, fair, or poor; if they had suffered an asthma or a wheezing attack in the past year; and if their health limited them in climbing several flights of stairs or lifting or carrying groceries (Wiener et al., 1990). To assess obesity, interviewers measured each respondent's height and weight.[7] We calculated BMI by dividing respondents' weight in kilograms by the square of their height in meters. We looked at effects stratified by three levels of obesity (BMI of 30 or greater, 35 or greater, and 40 or greater)[8] because of evidence that very high BMI values may be strongly associated with subsequent adverse health outcomes (NHLBI Obesity Education Initiative, 1998). A woman who is of average height in the United States (5 feet, 4 inches) would need to weigh 204 pounds to have a BMI of 35 and would need to weigh 233 pounds to have a BMI of 40. In addition to height and weight, interviewers took respondents' seated blood pressure using an automated cuff.[9] We used the average of two readings to assess hypertension[10] and considered individuals to be hypertensive if their average systolic pressure was 140 millimeters of mercury or higher or their average diastolic pressure was 90 millimeters of mercury or higher (Chobanian et al., 2003).

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[5] We used data from the National Comorbidity Survey Replication (Kessler and Merikangas, 2004) to calculate the likelihood of PTSD based on the questions asked in the MTO survey and taking into account age, gender, race, and ethnicity.

[6] The MTO survey asked about the following events: beaten up as a child by a primary caregiver, a spouse or romantic partner, or someone else; raped or sexually assaulted; mugged or threatened with a weapon; unexpected death of a loved one; traumatic event experienced by a loved one; witnessed physical fights at home; witnessed death or saw a dead body or someone seriously hurt; or some other traumatic event.

[7] We measured height and weight in accordance with the protocols developed for the Health and Retirement Survey (ISR, 2008).

[8] Ludwig et al. (2011) reported findings on MTO's effects on obesity and diabetes.

[9] The automated sphygmomanometer used was Omron model HEM-711DLX.

[10] We considered a reading valid if diastolic blood pressure (pressure when the heart is at rest between contractions) was more than 40 millimeters of mercury, the systolic blood pressure (pressure when the heart is contracting) was more than 60 millimeters of mercury, and the systolic was at least 10 points higher than the diastolic.

We measured diabetes using both survey questions and blood samples. Interviewers asked respondents whether they had diabetes (or high blood sugar) or had received treatment for it during the past year. Because many people with diabetes are unaware of their condition, we collected blood spot samples from respondents and measured glycosylated hemoglobin level as an indicator of diabetes.[11]

Economic Measures

Our last set of measures focused on the economic self-sufficiency of adults. We measured employment, earnings, household income, and use of government assistance programs through both survey questions and administrative records. We drew on employment questions from the Current Population Survey and considered MTO respondents to be employed currently if they reported working for pay during the week prior to the interview (or reported being temporarily absent from their job because of illness or vacation). Interviewers asked respondents how much money they earned in the previous year, whether they were currently receiving food stamps or Temporary Assistance for Needy Families (TANF), and how much income their household (all members combined) received in the previous year. Information on household income enabled us to determine whether their household was above or below the U.S. Census Bureau's poverty threshold. For example, a family consisting of one mother and two children would be below the poverty threshold if they had an income of less than $17,285 in 2009. We also matched the MTO sample to administrative data on quarterly earnings from state unemployment insurance (UI) agencies and to TANF and food stamps records from state (or county) agencies. We used the matched data to look at employment and earnings in 2007 and at receipt of TANF or food-stamp benefits over the 2-year period from July 2007 through June 2009.[12]

MTO Effects on Adult Outcomes

In this section we present our estimates of MTO's effects on the mental health, physical health, and economic self-sufficiency of adults in the program. Our impact estimates are based on the regression model from equation 1.

Mental Health Effects

Exhibit 3 shows evidence of beneficial MTO effects on the mental health of adults in terms of lower depression and levels of psychological distress, but the experimental group also shows an increase in substance dependence. All remaining exhibits are structured the same way as Exhibit 3. Each row presents the findings for the outcome listed in the left-hand column; the first outcome is depression. The second column, Control Mean, shows that approximately 20.3 percent of adults in the control group suffered from major depression during their lifetime. The third column displays the ITT effect of being offered an experimental voucher, estimated by comparing the entire experimental group with the entire control group. Adults in the experimental group met the criteria for major depression at a rate that was 3.2 percentage points less than the rate for adults in the

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[11] Glycosylated hemoglobin (HbA1c) captures the average glucose level in the blood during the past several months. The American Diabetes Association (2010) recommends using HbA1c levels of 6.5 percent or higher to diagnose diabetes.

[12] Data availability limits our analyses of TANF and food stamps to participants from Boston, Chicago, and Los Angeles.

Exhibit 3

MTO Effects on Adult Mental Health, 10 to 15 Years After Random Assignment

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Major depression with hierarchy, lifetime	0.203	– 0.032~ (0.017)	– 0.066~ (0.035)	– 0.048* (0.021)	– 0.077* (0.034)	3,269
GAD with hierarchy, lifetime	0.065	– 0.003 (0.010)	– 0.005 (0.021)	– 0.020~ (0.011)	– 0.033~ (0.017)	3,273
PTSD, lifetime	0.219	– 0.012 (0.018)	– 0.024 (0.037)	0.004 (0.024)	0.006 (0.038)	3,269
Psychological distress index (K6) z-score, past month (higher score indicates greater distress)	0.000	– 0.107* (0.042)	– 0.221* (0.087)	– 0.097~ (0.056)	– 0.156~ (0.091)	3,273
Dependence on drugs or alcohol, past month	0.055	0.029* (0.011)	0.060* (0.022)	0.015 (0.015)	0.024 (0.023)	3,269

GAD = generalized anxiety disorder. ITT = intention to treat. MTO = Moving to Opportunity. PTSD = post-traumatic stress disorder. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates (and field release) and applying weights. Psychological distress consists of six items (sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, worthlessness) scaled on a score from 0 (no distress) to 24 (highest distress) and then converted to z-scores using the mean and standard deviation of control group adults. Disorders with hierarchy take into account the comorbidity of other disorders: Major depression with hierarchy takes into account mania and hypomania; GAD with hierarchy takes into account depression and mania. Depression, GAD, and PTSD are measured using the World Health Organization's Composite International Diagnostic Interview (Kessler and Üstün, 2004), modified to include those sections used to diagnose. Substance dependence is developed from the Substance Abuse and Mental Health Services Administration's National Household Survey on Drug Use and Health and consists of five items about drug or alcohol use (use out of control, anxiety or worry about missing fix or drink, worry about use, frequency of desire to end use, difficulty of going without use) scaled on a score from 0 (no dependence) to 15 (highest level of dependence). A score of 3 or higher indicates dependence.

Source: Adult long-term survey

control group (p < .10, as indicated by the tilde). The standard error (shown in parentheses) is 1.7 percentage points.[13] In the fourth column, our estimates suggest that moving using an experimental voucher, or the TOT effect, reduces the prevalence of lifetime depression by 6.6 percentage points (about twice the size of the ITT effect). In the fifth and sixth columns, we turn to the effects (ITT and TOT) for the Section 8 group. Being offered a traditional Section 8 voucher reduces the prevalence of lifetime depression by 4.8 percentage points, and actually using the voucher reduces depression by an estimated 7.7 percentage points (p < .05). The final column of the table shows the number of observations used in the analysis.

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[13] The standard error indicates the estimate's precision. The effect plus or minus the standard error, multiplied by 1.96, captures the 95-percent confidence interval around the effect, in this case implying a confidence interval ranging from an increase of 0.1 percentage points to a decrease of 6.5 percentage points in depression.

Similar percentages (about 6.5 percent in each group) of adults in both the control group and the experimental group met the criteria for generalized anxiety disorder (lifetime). Adults in the Section 8 group were marginally less likely than adults in the control group to have had GAD (p = .057). Of adults in the control group, 22 percent met the criteria for PTSD, and the prevalence for the voucher groups was not significantly different from this.

Turning to a more global measure of mental health—psychological distress—we find beneficial program effects. The average psychological distress levels of adults in the experimental and Section 8 groups are about one-tenth of a standard deviation less than adults in the control group. The impact estimate achieves statistical significance for the experimental group (p = .011) but not the Section 8 group (p = .084). (To make the K6 results easier to interpret, we standardized the units by subtracting off the control group mean and dividing by the control group standard deviation to create what is known as a z-score.)

We observe an adverse effect on substance dependence. About 5.5 percent of adults in the control group met the criteria for substance dependence during the past month and assignment to the experimental group was associated with a prevalence that was 2.9 percentage points higher than that of the control group. For the Section 8 group, we do not detect a statistically significant effect on dependence.

Physical Health Effects

As shown in Exhibit 4, MTO appears to reduce the share of adults with diabetes and the likelihood of severe obesity, but we do not detect any treatment effects on several other health measures. About 56 percent of adults in the control group indicated that their current health was good or better; reports by adults in the experimental and Section 8 groups were similar. About 29 percent of adults in the control group reported having had an asthma attack in the past year. The rates were slightly lower for the two treatment groups, but the differences were not statistically significant. The average weight of adults in the control group was about 190 pounds, and about 58 percent of controls met the criteria for obesity (BMI of 30 or greater). Although we detect no statistically significant effects on the likelihood of having a BMI of 30 or more, we do detect beneficial program effects at more extreme obesity levels. The experimental and Section 8 groups are 4.6 and 5.3 percentage points, respectively, less likely to have had a BMI of 35 or greater compared with adults in the control group who had a prevalence of 35 percent (p < .05). These estimates imply that actually moving using a voucher reduces the prevalence of a BMI of 35 or greater by about 9.5 percentage points for the experimental group and 8.6 percentage points for the Section 8 group. For a BMI of 40 or greater, the ITT effect was a 3.4-percentage-point reduction for adults in the experimental group (p < .05) and a 2.9-percentage-point reduction for adults in the Section 8 group (not significant).

Both types of diabetes measures (self-reports and blood samples) point in the direction of lower diabetes rates among adults in the treatment groups compared with adults in the control group, although the exact magnitudes and levels of statistical significance vary. When we rely on respondent self-reports to measure diabetes, we find that the estimated decline in diabetes prevalence for adults in the experimental group relative to adults in the control group is not quite statistically significant, whereas adults in the Section 8 group have a significantly lower rate (ITT of 6.1 percentage points) compared with adults in the control group. Compared with self-reports, the blood test results

Exhibit 4

MTO Effects on Adult Physical Health, 10 to 15 Years After Random Assignment

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Self-rated health						
Currently good or better health [SR]	0.564	0.002 (0.020)	0.004 (0.042)	– 0.005 (0.027)	– 0.009 (0.044)	3,269
Asthma						
Asthma or wheezing attack during the past year [SR]	0.293	– 0.018 (0.020)	– 0.038 (0.040)	– 0.042 (0.026)	– 0.066 (0.041)	3,267
Obesity and height/weight measurements						
Height, in feet [M, SR]	5.315	0.009 (0.010)	0.019 (0.020)	– 0.001 (0.013)	– 0.002 (0.021)	3,242
Weight, in pounds [M, SR]	189.985	– 3.163 (2.077)	– 6.521 (4.281)	– 2.852 (2.915)	– 4.615 (4.717)	3,222
Currently obese: BMI ≥ 30 [M, SR]	0.584	– 0.012 (0.022)	– 0.025 (0.044)	– 0.011 (0.029)	– 0.018 (0.046)	3,221
BMI ≥ 35 [M, SR]	0.351	– 0.046* (0.020)	– 0.095* (0.042)	– 0.053* (0.027)	– 0.086* (0.043)	3,221
BMI ≥ 40 [M, SR]	0.175	– 0.034* (0.016)	– 0.071* (0.032)	– 0.029 (0.021)	– 0.048 (0.034)	3,221
Diabetes						
HbA1c test detected diabetes (HbA1c ≥ 6.5%) [DBS]	0.204	– 0.052* (0.018)	– 0.108* (0.038)	– 0.011 (0.024)	– 0.017 (0.038)	2,737
Had diabetes or treated for it in the past year [SR]	0.160	– 0.024 (0.015)	– 0.049 (0.030)	– 0.061* (0.018)	– 0.098* (0.029)	3,251
Physical limitations						
Health limits respondent in climbing several flights of stairs or lifting and carrying groceries [SR]	0.510	– 0.048* (0.021)	– 0.100* (0.043)	– 0.023 (0.028)	– 0.038 (0.045)	3,270
Hypertension						
Currently has high blood pressure (systolic ≥ 140 mmHg or diastolic ≥ 90 mmHg) [M]	0.315	0.007 (0.020)	0.015 (0.042)	– 0.026 (0.027)	– 0.041 (0.043)	3,102

BMI = body mass index. DBS = dried blood spot assays. ITT = intention to treat. M = direct measurement. MTO = Moving to Opportunity. SR = self-reported. TOT = treatment on the treated. * = $p < .05$. ~ = $p < .10$.

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Square brackets indicate the source of the outcome information. Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates (and field release) and applying weights. Height and weight were directly measured except for a very small percent of the sample for whom self-reported values are used. BMI is measured as weight in kilograms divided by height in meters squared. For diastolic and systolic blood pressure, data are the average of two readings if available; otherwise, data are from one reading.

Source: Adult long-term survey

show that a greater share of adults in the control group had diabetes. Using the blood test results, we find a statistically significant 5.2-percentage-point experimental ITT effect (and corresponding 10.8-percentage-point TOT effect), but we do not detect an effect for the Section 8 group. (We previously reported diabetes results for MTO adult women in Ludwig et al., 2011.)

The last two health measures we present are physical limitations and hypertension, on which we only detect an effect on health limitations among adults in the experimental group. Of adults in the control group, 51 percent reported that their health limited them in everyday activities, and adults in the experimental group were 4.8 percentage points less likely to report this type of limitation. The difference between the control group and the Section 8 group was not statistically significant. Nearly 32 percent of adults in the control group had hypertension, and the incidence of hypertension for adults in the experimental and Section 8 groups was similar to that of adults in the control group, with no significant differences detected.

Economic Effects

In contrast to finding effects on health, we generally do not detect any beneficial effects on employment and earnings, household poverty, or reliance on government assistance programs. Exhibit 5 shows that slightly more than one-half of adults in the control group were employed at the time they were interviewed. Compared with controls, adults in the experimental group were not significantly more likely to be employed. Adults in the Section 8 group were 7.7 percentage points less likely than adults in the control group to report that they were working ($p < .05$), but we interpret this result with caution because adults in the Section 8 group were interviewed slightly later in calendar time than adults in the control group, during a period of rising unemployment. Differences across groups may in part reflect different economic conditions at the time we surveyed the adults rather than effects on labor-market outcomes. (We think our mental and physical health measures are less susceptible to changes over short periods of time and that timing differences are less of a problem for those outcomes. One reason we think the Section 8 versus the control group difference in self-reported economic outcomes might be the result of the slight differences in when adults were surveyed comes from our analysis of administrative UI system data. The UI data enable us to compare employment for the Section 8 and control groups at the exact same point in time. When examining employment in 2007 for all three groups, we find no statistically significant differences across them.

We also do not detect differences in household income or receipt of food stamps and TANF, except for a slight increase in the amount of food stamps received by the experimental group. The average household income of adults in the control group was $12,289 (in 2009 dollars),[14] and the averages for adults in the experimental and Section 8 groups were similar. The proportion of households at or below the poverty threshold was roughly equal across the three randomized groups, with 59 percent of adults in the control group living in poor households. Of adults in the control group, 47 percent reported currently receiving food stamps and 16 percent reported receiving TANF benefits. The corresponding rates for adults in the experimental and Section 8 groups were not significantly different. Using administrative records, we were also able to estimate the amount of food-stamp and

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[14] We adjusted the responses to 2009 dollars using the Consumer Price Index for All Urban Consumers provided by the U.S. Bureau of Labor Statistics.

Exhibit 5

MTO Effects on Adult Economic Self-Sufficiency, 10 to 15 Years After Random Assignment

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Employment						
Currently employed [SR]	0.525	– 0.007 (0.021)	– 0.014 (0.042)	– 0.077* (0.028)	– 0.124* (0.045)	3,264
Employed in calendar year 2007 [UI]	0.465	– 0.004 (0.017)	– 0.009 (0.036)	0.000 (0.019)	0.000 (0.030)	4,194
Earnings and household income						
Earnings in previous calendar year (2009 dollars) [SR]	$12,288.51	326.94 (583.44)	677.92 (1209.79)	– 613.60 (807.20)	– 982.43 (1292.40)	3,141
Earnings in calendar year 2007 (2009 dollars) [UI]	$11,325.14	– 347.83 (523.80)	– 731.73 (1101.92)	112.93 (580.69)	180.50 (928.11)	4,194
Household income is at or below 100% of poverty line [SR]	0.590	– 0.032 (0.021)	– 0.067 (0.043)	0.036 (0.028)	0.059 (0.045)	3,258
Food stamps						
Currently receiving food stamps [SR]	0.470	0.016 (0.021)	0.032 (0.043)	0.029 (0.028)	0.046 (0.045)	3,253
Food-stamps benefits received during 2-year period from July 2007 to June 2009 (2009 dollars) [FS]	$3,074.08	309.94* (156.50)	664.54* (335.54)	171.07 (184.98)	261.80 (283.09)	2,708
Temporary Assistance for Needy Families (TANF)						
Currently receiving TANF [SR]	0.158	0.010 (0.015)	0.021 (0.031)	0.026 (0.021)	0.041 (0.033)	3,262
TANF benefits received during 2-year period from July 2007 to June 2009 (2009 dollars) [TANF]	$1,402.33	56.10 (114.48)	120.29 (245.44)	– 94.47 (123.82)	– 144.57 (189.49)	2,708

FS = food stamps records. ITT = intention to treat. MTO = Moving to Opportunity. SR = self-reported. TANF = Temporary Assistance for Needy Families records. TOT = treatment on the treated. UI = unemployment insurance administrative records.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Square brackets indicate the source of the outcome information. Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates and applying weights. Analyses of survey measurse also control for field release. Annual earnings are adjusted to 2009 dollars using the Consumer Price Index for All Urban Consumers. Poverty is the U.S. census 2009 poverty threshold and adjusts for head of household's age, household size, and number of children. TANF outcomes are at the family level and represent the total amount of benefits received during those 12 months for all family members' cases. This family-level measure does not restrict to active months in order to capture all benefits received by the family and dependents. For example, a payment could have been made to a child even though the adult on the case was sanctioned.

Source: Adult long-term survey

TANF benefits received during the 2-year period from July 2007 through June 2009. We observed no significant difference across the three randomized groups in the amount of TANF benefits received but did observe a higher level of food-stamp receipt (by $310 over 2 years, or an average of $13 per month) for adults in the experimental group compared with adults in the control group.

Discussion

As Ludwig (2012) discussed, MTO generated large differences in neighborhood disadvantage and other conditions between the two treatment groups and the control group in the period immediately following random assignment. Over time, these differences narrowed, in large part because of improvements in the neighborhood conditions of the control group, but even with this narrowing the differences are sizable between groups over the entire 10- to 15-year period.

Our 10- to 15-year followup evaluation shows that MTO-induced changes in neighborhood conditions were associated with beneficial impacts on a number of key mental and physical health outcomes but with few, if any, effects on different economic self-sufficiency measures. Specifically, we found that adults in the experimental group had lower levels of psychological distress than did adults in the control group and that adults in the Section 8 group had lower levels of lifetime depression. The results also suggest, however, an increase in substance dependence for the experimental group relative to the control group. On physical health, we detect beneficial effects on diabetes, severe obesity, and health limitations, but we do not detect effects on self-rated health, asthma, or hypertension. The voucher offer reduced the prevalence of diabetes by 5.2 percentage points for the experimental group.

Effects on some key outcomes are of a clinically relevant magnitude. For example, MTO generated very large reductions in diabetes that might be comparable to those found in studies or programs designed to prevent diabetes. In comparing MTO to medical interventions, keep in mind that most clinical trials in medicine usually enroll study samples that are more socioeconomically advantaged than the low-income families who enrolled in MTO, often enroll individuals at high risk for a particular health problem, and may measure the outcome differently. In addition, reproductive-age women are disproportionately underrepresented in these studies. With those qualifications in mind, MTO's effects on diabetes appear to be similar in magnitude to those found in the Diabetes Prevention Program (DPP) randomized trial of lifestyle interventions designed to prevent diabetes that took place in clinical centers across the United States (Knowler et al., 2009). In DPP, a case manager met with participants for 16 initial sessions (and monthly afterwards) to help them modify their diet and exercise habits, with the goal of reducing body weight by 7 percent and engaging in 2.5 hours of moderate physical activity per week. Over a 10-year period, the lifestyle intervention reduced the incidence of new cases of diabetes by about 34 percent relative to the incidence in the placebo group, an effect of similar magnitude to the experimental group treatment.[15] MTO diabetes

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[15] Under the assumption that about 5 percent of MTO adults may have had diabetes at the start of the program, we estimate that the incidence of new cases among adults in the control group was about 1.22 per 100 person years (where 1.22 = [0.204 final prevalence − 0.05 assumed baseline prevalence] x [1/12.67 years] x 100 years) and was about 0.81 per 100 person years (where 0.81 = [0.204 final control prevalence − 0.052 effect − 0.05 assumed baseline prevalence] x [1/12.67 years] x 100 years) for the MTO treatment group. Thus, we estimate about a 34-percent reduction in the incidence of new cases (0.34 = [0.81 − 1.22]/1.22).

effects are also noteworthy because of the costs associated with the disease; Trogdon and Hylands (2008) estimate that the annual medical expenditures of people with diabetes are 239 percent greater than those of people without diabetes (after adjusting for demographic differences).

The results we report here from our long-term (10- to 15-year) survey of MTO adults for the final impacts evaluation suggest that the lack of MTO effect on economic outcomes found in the followup (4- to 7-year) survey for the interim impacts evaluation was not simply because of the disruptive effects of moving itself or of the fact that families may not have been in their new neighborhoods long enough to fully adapt and take advantage of any new opportunities in those areas. Given the previous nonexperimental research literature suggesting that neighborhood environments affect labor market outcomes, what might explain why we do not observe beneficial effects on adult self-sufficiency? That MTO may have had only modest effects on the mechanisms that affect employment and earnings is one explanation. For example, the areas to which families moved through MTO may not have offered more plentiful job opportunities. At 4 to 7 years after baseline, the interim impacts evaluation showed little effect of moves on local job availability as measured by employment growth by residential ZIP Code (Kling, Liebman, and Katz, 2007). In addition, although MTO moves appear to have increased the likelihood that adults in the experimental group had a college-educated friend, qualitative work with MTO families suggests that new neighbors may not have known about the types of job openings that MTO adults were seeking (Turney et al., 2006).

Our findings suggest that housing mobility programs similar to MTO are unlikely, by themselves, to be panaceas for the labor-market difficulties of disadvantaged families living in public housing projects in inner-city neighborhoods. Investing in high-quality training and employment services may be a more promising way to improve labor-market outcomes for very disadvantaged adult samples of the sort that enrolled in MTO. For example, the Jobs-Plus demonstration produced sustained (7-year) earnings gains for adult public housing residents without disabilities through employment and training services, changes in rent rules to increase work incentives, and neighbor-to-neighbor outreach centering on work (Riccio, 2010). Several training programs that prepare underskilled workers for skilled positions in a specific industry and connect them to employers with job openings have also produced substantial earnings gains for disadvantaged adults in large U.S. cities, and these gains applied to women and African-American adults in the study (Maguire et al., 2010).

For very disadvantaged adults like those who participated in MTO, policies that directly increase skills, help with the acquisition of work experience, assist with job searches, and provide workplace supports may be necessary to improve economic self-sufficiency. The MTO findings suggest that housing mobility programs could help improve the physical health and mental well-being of disadvantaged adults. Our hope is that future work helps illuminate the mechanisms through which community environments influence health outcomes to help guide the design of community-level interventions that can improve health without having to rely on relocating families to new areas.

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The Long-Term Effects of Moving to Opportunity on Youth Outcomes

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Abstract

Evidence about the effects of neighborhood environments on children and youth is central to the design of a wide range of public policies. Armed with long-term survey data from the Moving to Opportunity (MTO) for Fair Housing demonstration final impacts evaluation (Sanbonmatsu et al., 2011), we have the opportunity to understand whether neighborhood poverty and related characteristics exert an independent causal effect on the life chances of young people. Findings from analyses of youth in the long-term survey for the final impacts evaluation show that MTO had few detectable effects on a range of schooling outcomes, even for those children who were of preschool age at study entry. MTO also had few detectable effects on physical health outcomes. In other youth outcome domains, patterns of effects on youth were similar to, but more muted than, those in the interim impacts evaluation (Orr et al., 2003), with favorable patterns among female youth—particularly on mental health outcomes—and less favorable patterns among male youth.

Introduction

The life chances of children vary dramatically across neighborhoods. Youth who grow up in areas of concentrated poverty tend to have elevated rates of a wide range of adverse outcomes—such as school dropout, low test scores, and delinquency—even after statistically controlling for observable characteristics of the youth and their families (Chalk and Phillips, 1996; Duncan and Murnane, 2011; Ellen and Turner, 1997; Ginther, Haveman, and Wolfe, 2000; Leventhal and Brooks-Gunn, 2008, 2000; Shonkoff and Phillips, 2000). These patterns have led to a longstanding concern that neighborhood environments may exert an independent causal effect on the life chances of young people. Because low-income individuals comprise nearly one-half of the 8.7 million people living in census tracts with poverty levels of 40 percent or higher (Kneebone, Nadeau, and Berube, 2011), poor children growing up in neighborhoods of concentrated poverty may be "doubly disadvantaged"—they face potential risks from growing up in a low-income household and in an economically poor neighborhood.

Evidence about the effects of neighborhood environments on children and youth is central to the design of a wide range of public policies, from means-tested housing programs to place-based strategies such as those of the U.S. Department of Education's (ED's) Promise Neighborhoods and Harlem Children's Zone, Inc. Empirically isolating the causal effects of neighborhood environments on youth outcomes from the range of other youth and family characteristics with which they are correlated is complicated, however. Most families have at least some degree of choice about where they live. As a result, hard-to-measure individual- or family-level attributes associated with neighborhood selection and directly affecting youth outcomes can confound the estimated effects of neighborhood environment.

The U.S. Department of Housing and Urban Development (HUD) launched the Moving to Opportunity (MTO) for Fair Housing demonstration randomized mobility experiment to try to overcome this empirical challenge of selection bias (that is, of nonrandom associations between neighborhood characteristics and the preexisting characteristics of residents that influenced their decisions to live in the neighborhood). Between 1994 and 1998, MTO recruited more than 4,600 families with children living in severely distressed public housing projects in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York City). HUD offered some MTO families the opportunity to use a housing voucher to move into private-market housing in lower poverty neighborhoods and did not make the same offer to others. This random assignment to different groups—experimental, Section 8, and control—in the MTO study broke the link between family preferences and neighborhood environments, and it thus provides us with the opportunity to overcome the standard self-selection concern and identify the causal effects of neighborhoods on child and youth outcomes. This article summarizes key findings regarding the effects of neighborhood characteristics found in the long-term (10- to 15-year) survey of MTO youth, who were approximately ages 10 to 20 in December 2007 (age 11 or younger at baseline), conducted for the final impacts evaluation (Sanbonmatsu et al., 2011).[1]

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[1] For more detail about MTO's long-term effects on youth outcomes, see Sanbonmatsu et al. (2011), chapters 2 through 7. A history of MTO research is available at http://mtoresearch.org.

Previous MTO research, based on data collected 4 to 7 years after random assignment, showed a more mixed and complicated pattern of findings than that predicted by the existing neighborhood effects literature. At the time of the followup survey for the interim impacts evaluation (Orr et al., 2003), MTO had produced few detectable effects on the achievement test scores or health of children, most of whom were already of school age when their families signed up for MTO (Fortson and Sanbonmatsu, 2010; Sanbonmatsu et al., 2006). Violent-crime arrests were fewer among male and female youth who moved via the experimental group vouchers compared with those assigned to the control group that received no vouchers. MTO effects on most other behavioral outcomes varied by gender, however, with beneficial effects on female youth and adverse effects for males (Kling, Liebman, and Katz, 2007; Kling, Ludwig, and Katz, 2005).

This article addresses three key questions for the final impacts evaluation. (1) Because disruptive effects from the act of moving are likely to fade and the beneficial influences of better neighborhoods likely to grow with time, do MTO's effects on children become more beneficial over time? (2) Are MTO's beneficial effects on children concentrated on the subset who had not entered school when their families enrolled in the program? Early childhood is a particularly malleable stage of early brain development and, therefore, a time when children are perhaps most susceptible to the benefits of social interventions (Becker and Murphy, 2000; Carneiro and Heckman, 2003; Knudsen et al., 2006; Shonkoff and Phillips, 2000). (3) Do the gender differences in MTO effects that the followup survey for the interim impacts evaluation found emerge in the final impacts evaluation? We draw our outcome measures from survey self-reports[2] of behavior, schooling, mental and physical health, and peer relationships; math and reading achievement assessments; physical measurements of height and weight; and administrative records on other outcomes such as quarterly earnings from state unemployment insurance (UI) data and arrest records.

The "Guest Editor's Introduction" to this issue of *Cityscape* describes how MTO succeeded in generating persistent differences in neighborhood environments across treatment and control groups (Ludwig, 2012). Youth in the experimental group, like adults in the experimental group, report feeling more safe in their neighborhoods, but the characteristics of the schools that children in the experimental group attended in their neighborhoods differed only modestly from the schools that children in the control group attended. For example, the schools that youth in the experimental and Section 8 groups attended had student bodies that were more mixed by income and by racial or ethnic groups than those of youth in the control group but that still included mostly poor and overwhelmingly minority students. Test scores in the schools that youth in the experimental and Section 8 groups attended were also slightly better than in the schools that the control group attended but were still usually in the bottom one-fourth of the statewide performance distribution. These mixed MTO effects on school environments do not preclude the possibility of MTO affecting schooling outcomes, because socioeconomic composition or social processes in neighborhoods might differ across schools and matter for achievement independent of school quality. Indeed, additional analyses of the followup (4- to 7-year) survey data for the interim impacts evaluation found signs of MTO effects on achievement test scores in only those demonstration sites with the highest levels of concentrated neighborhood disadvantage that also had few detectable effects on

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[2] The long-term youth survey is available at http://mtoresearch.org.

schools (Burdick-Will et al., 2011). Analyses of the long-term (10- to 15-year) survey data for the final impacts evaluation, however, show that MTO had no detectable effect on math or reading achievement.

Overall, MTO had few detectable effects on a range of schooling outcomes, even among those children who were of preschool age at study entry, and few detectable effects on physical health outcomes. In other outcome domains, the long-term survey found that MTO had patterns of effects that were similar to, but more muted than, those the interim followup survey found, with favorable patterns among female youth—particularly on mental health outcomes—and less favorable patterns among male youth.

The next section of the article reviews the candidate mechanisms through which neighborhood environments might influence children's outcomes. A section reviewing the data that we collected during the long-term survey for the final impacts evaluation and a section presenting the results follow. The final section discusses the implications of these findings for policy and future research on neighborhood effects.

Background and Conceptual Framework

A large empirical literature, as discussed in the Introduction, generally points in the direction of neighborhood effects on children's schooling outcomes, youth crime, parent joblessness and earnings, and even mortality. A framework Jencks and Mayer (1990) posited nicely describes the pathways through which neighborhoods can affect youth achievement and behavior. *Epidemic models* emphasize the power of peers to spread behaviors. Such contagion effects can arise from learning from peers, pure preference externalities (individuals enjoy imitating their peers), stigma effects (negative signals from delinquent behaviors declines when more people do them), and physical externalities (for example, higher crime rates reduce the chances of getting arrested because of congestion effects in law enforcement; see Brock and Durlauf, 2001; Cook and Goss, 1996; Glaeser and Scheinkman, 1999; Manski, 2000; Moffitt, 2001). *Collective socialization models* concentrate on the way adults in a neighborhood influence young people who are not their children, through human capital externalities (Borjas, 1995) or by acting as role models or enforcers of public order (Sampson, Raudenbush, and Earls, 1997; Wilson, 1987). *Institutional models* focus on the influence of adults who mainly reside outside the community but who work in the schools, police force, and other neighborhood institutions. *Competition models* emphasize the competition between neighbors for scarce resources like grades or jobs. *Relative deprivation models* focus on the psychological effect on individuals or self-evaluation based on relative standing in the community (Luttmer, 2005). The failure to compete successfully for prosocial rewards, as competition models hypothesize, could in fact lead some people to reverse course and try competing for resources or social standing by engaging in antisocial behaviors. Furthermore, relative deprivation models might predict that comparisons with the status and accomplishments of new neighbors in more affluent areas could have negative psychological effects.

The remaining subsections review the previous empirical research based around the key youth outcome domains of interest and the extent to which this literature sheds any light on the pathways by which neighborhoods have these effects.

Education and Employment

Perhaps the most obvious way in which neighborhood context may affect educational outcomes is through the quality of local public schools. Moving to better neighborhoods for better schools arose as one key motivation for MTO study families to subsequently move, although realizing their aspirations for improved educational opportunities was difficult and often influenced by informal networks (Ferryman et al., 2008). Indeed, the composition of neighborhood residents might also matter, because adults convey shared prosocial (or antisocial) values or serve as positive or negative role models; that is, the Jencks-Mayer collective socialization model (Connell et al., 1995; Crane, 1991; Sampson, 1993; Sampson and Groves, 1989; Wilson, 1987). Such neighborhoods also may provide youth with a safe physical environment, which may be conducive to academic success (Connell et al., 1995). Epidemic models raise the possibility of a variety of spillover effects from exposure to higher achieving peers; for example, through opportunities to participate in more productive study groups, exposure to more rigorous instruction, and increased time on task from reductions in student disruptions (Lazear, 2001). On the other hand, the competition for grades may be more intense in more affluent areas. Increased competition could have a detrimental effect on some MTO children, although these effects might dissipate over time if their academic competencies improve with exposure to new schools.

Previous nonexperimental research generally has found positive correlations between affluent neighbors and a variety of academic outcomes, such as IQ, reading and math achievement scores, school completion, and self-reported grades for children and adolescents (for example, Brooks-Gunn et al., 1993; Chase-Lansdale et al., 1997; Connell and Halpern-Felsher, 1997; Crane, 1991; Dornbusch, Ritter, and Steinberg, 1991; Duncan, Brooks-Gunn, and Klebanov, 1994; Entwistle, Alexander, and Olson, 1994). A study of the Gautreaux mobility program found that young adults in households that had moved from public housing in the city of Chicago to suburban locations were less than one-fourth as likely to drop out of school and more than twice as likely to attend college compared with the outcomes of young adults initially living in the same public housing units whose families moved to other parts of the city (Rubinowitz and Rosenbaum, 2000). Preliminary analyses using longer run data on a larger group of Gautreaux children suggested smaller and more specialized effects (Keels et al., 2005).

Schools also can be gateways to other types of educational or work programs. School-to-work programs administered by local public schools may help youth secure internships while they are still enrolled in high school and help non-college-bound youth secure employment after high school. Factors such as the stigma surrounding entry-level jobs or local criminal activity, the level of violence associated with the local illegal economy and the quality of local policing, and the level of difficulty in competing for jobs and related positive rewards for behavior that supports schooling or employment can also influence youth decisions about whether to participate in the formal labor market or to pursue underground or informal work. Although having more affluent neighbors appears to correspond with having improved labor market outcomes (see, for example, Corcoran et al., 1992; Page and Solon, 2003; Sharkey, 2008), several more recent studies suggest a mixed pattern of neighborhood environment influence. Child neighborhood environments do not appear related to adult labor market outcomes among children assigned to public housing projects in substantially different neighborhoods of Toronto (Oreopoulos, 2003). Further analyses of Gautreaux

(Rosenbaum, 1995) found strong positive gains in educational and economic outcomes for the children of suburban movers relative to those of city movers, but longer term followups found less striking contrasts between suburban and city movers (DeLuca et al., 2009). On the other hand, Gould, Lavy, and Paserman (2009) found positive effects on long-term adult economic outcomes for Yemenite refugees to Israel who, as children, were placed initially in more prosperous neighborhoods with better infrastructure.

Delinquency and Risky or Problem Behavior

In addition to potentially affecting educational and employment outcomes, MTO may have important effects on problem behaviors. Social stigma associated with criminal behavior may be lower in areas where such behavior is relatively more common. Similarly, if police resources assigned or available to a community are relatively fixed, an increase in criminal activity by one's peers will reduce the probability that a given criminal offense results in arrest (Cook and Goss, 1996). The literature is more mixed regarding other risky behaviors, such as drug or substance abuse and sexual activity (Brooks-Gunn et al., 1993; Crane, 1991; Esbensen and Huizinga, 1990; Hogan, Astone, and Kitagawa, 1985; Hogan and Kitagawa, 1985).

Health

Although Jencks and Mayer (1990) did not consider neighborhood processes to be related to health, we have reason to believe that moves to lower poverty neighborhoods may improve both physical and mental health. Physical health may improve with safer and less stressful environments, greater community resources, or residents who practice healthy behaviors such as exercise. Low-income neighborhoods may also have compromised air quality, which has been linked to coronary heart disease (Kan et al., 2008) and poor health for infants (Currie and Walker, 2011). Poor children living in disadvantaged urban areas may be at higher risk of exposure to lead and secondhand smoke, both of which can impair brain development (Bombard et al., 2010; Filippelli and Laidlaw, 2010). The prevalence of accidents and injuries—the most common causes of death among children ages 1 to 14 in the United States—may be higher among children living in distressed urban communities, owing to unsafe playgrounds and other features of the environment (Quinlan, 1996; Scharfstein and Sandel, 1998).

Adults and children who live in high-poverty, high-crime urban settings are also at risk for poorer mental health outcomes (for example, Bagley, Jacobson, and Palmer, 1973; Rezaeian et al., 2005; Whitley et al., 1999). To the extent that MTO reduces exposure to crime and violence, we would expect it to improve overall well-being and reduce psychological distress, depression, and anxiety (Aneshensel and Sucoff, 1996; Ross and Mirowsky, 2001; Silver, Mulvey, and Swanson, 2002). Moving to lower poverty neighborhoods could influence a variety of externalizing behavior disorders (for example, oppositional defiant disorder), because these disorders are strongly related to contagion processes in peer environments and norms regarding the appropriateness of violence and antisocial behaviors (Deater-Deckard, 2001; Gifford-Smith et al., 2005). Male and female youth may also have different coping styles and capacities as they navigate different neighborhood environments. Adolescent males tend to be subject to less parental supervision than females, and they also tend to be greater risk takers (Block, 1983; Bottcher, 2001; LaGrange and Silverman, 1999).

Psychosocial stress sometimes can have more pronounced effects on males than females, in part, because males are more likely to use confrontational techniques (that is, the Jencks-Mayer collective socialization model) to deal with stress, particularly stress involving interpersonal problems, whereas females are more likely to turn to supportive adults (Coleman and Hendry, 1999; Zaslow and Hayes, 1986).

The Jencks and Mayer (1990) typology and empirical literature, in turn, generally implies that the effect of MTO moves may become more beneficial (or less detrimental) as youth spend more time in lower poverty areas. For example, over time, we may expect MTO youth to become more socially integrated into their new communities and more attuned to local social norms, and thus more responsive to the peer and adult social influences that are central to the epidemic and collective socialization models. Parents may also learn over time how to better navigate the potential opportunities and pitfalls in low-poverty schools. More generally, the effects of exposure to new social environments and institutions may accumulate over time and lead to more pronounced positive effects on youth behavior. A different time path in MTO effects may arise from the effects of neighborhood safety and crime, which, as mentioned previously, may be relevant for outcomes in the schooling, employment, and delinquency domains. On the other hand, some of the theories described in the Jencks and Mayer typology—the competition and relative deprivation models in particular—predict potentially adverse effects as youth spend time in lower poverty neighborhoods. Ultimately, whether youth benefit based on these theories is an empirical question.

The MTO Study Design, Sample, and Data

From 1994 to 1998, HUD launched MTO in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. HUD limited eligibility to families with children living in public or other government housing in designated high-poverty census tracts. The study then randomly assigned the 4,604 families who signed up to one of three groups. HUD offered families in the experimental group the opportunity to use a rent-subsidy voucher to move into private-market housing but, under the MTO design, families in this group could redeem their vouchers only in census tracts with a 1990 poverty rate of less than 10 percent. Families in the experimental group also received housing search assistance and relocation counseling from local nonprofit organizations. HUD offered the randomly assigned families in the Section 8 group a traditional housing voucher that had no location requirements and did not come with any search assistance beyond what Section 8 voucher recipients normally receive. Families in the control group did not receive a voucher through MTO, but they did not lose access to any housing or other social services to which they would otherwise have been entitled.

The final impacts evaluation youth survey sample frame included up to three youth per original MTO family who were between ages 10 and 20 as of December 2007. Older adolescents (ages 13 to 20 as of December 2007) answered the full-length survey that we developed, whereas younger children (ages 10 to 12 as of December 2007) answered a shorter subset of items. Although MTO participants who were younger than 18 at baseline and older than 20 by December 2007 were not in our survey sample frame, we did try to track their outcomes through proxy reports of parents on the adult MTO surveys and through administrative data on employment, postsecondary schooling, and arrests.

Response rates for the youth survey were very high and were balanced across the control and treatment groups. The overall effective response rate was 89 percent, and the effective response rates by randomization group were as follows: experimental group, 90 percent; Section 8 group, 87 percent; control group, 89 percent. The analysis sample for the survey-based measures presented in this article includes a total of 5,101 youth ages 10 to 20, comprising 457 younger children and 4,644 older adolescents. All youth ages 10 to 20 (N = 6,645) in the 4,604 families in the program (as opposed to only those youth who were interviewed) were eligible for submission to administrative data agencies.[3] Roughly 56 percent (2,969 of 5,345) of the youth who were interviewed as part of the followup survey for the interim impacts evaluation were interviewed again as part of the long-term survey for the final impacts evaluation.

The youth in the long-term survey sample, who on average were age 5 at baseline (ranging from newborn to age 11), were not particularly disadvantaged regarding learning and behavioral problems. They attended schools, however, characterized by high poverty, high minority composition, and low achievement; their parents had low educational achievement; and they were living in dangerous neighborhoods. The baseline heads of household reported that 12 percent of youth ages 6 to 11 had a learning problem and 6 percent had behavioral or emotional problems. About 13 percent had been enrolled in a program for gifted and talented students or had done advanced coursework. These numbers are consistent with national averages; about 13 percent of the school-age population receives special education services (Kaufman, Alt, and Chapman, 2001) and 10 percent are enrolled in gifted classes (Fields et al., 2001). On the other hand, about 85 percent of students at the baseline schools of MTO youth were eligible for free or reduced-price lunches, more than 90 percent of students were minorities, and most of the schools were in the bottom 15 percent of the statewide performance distribution. Furthermore, parents had relatively low educational attainment: only 35 percent held a high school diploma, and another 18 percent had earned a certificate of General Educational Development (GED). Finally, as Ludwig (2012) described, when families listed their reasons for wanting to move, about three-fourths reported wanting to get away from gangs and drugs (that is, safety) as their first or second most important reason, about one-half listed better schools for the children, and about 45 percent listed a bigger or better apartment.

We estimate both the effects of being offered an MTO low-poverty voucher or a traditional Section 8 voucher, known as the intention-to-treat (ITT) effect in the program evaluation literature, and the effects of actually moving with a low-poverty or traditional voucher, known as the treatment-on-the-treated (TOT) effect. We calculate ITT using an ordinary least squares regression in which the outcome of interest is the dependent variable being predicted on group assignment and a series of baseline covariates. The basic equation is

$$Y_i = \alpha_0 + \alpha_1 Exp_i + \alpha_2 S8_i + \alpha_3 X_i + e_i, \tag{1}$$

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[3] In practice, however, the outcomes in this article's exhibits generally are limited to smaller samples, because they include age-based subsets of all youth (for example, we submitted only youth ages 15 to 20 for the postsecondary schooling data match). We also have proxy reports on 3,217 grown children (from 3,273 adult survey interviews), and we submitted all 4,643 grown children from the 4,604 families to administrative data agencies.

where Y_i is some outcome for MTO program participant i; Exp_i and $S8_i$ are binary indicator variables equal to 1 if participant i was in the experimental or Section 8 group (and the control group is the omitted reference group); and X_i represents a series of individual- and family-level baseline covariates that Sanbonmatsu et al. (2011) described and similar to the covariates Orr et al. (2003) described. The coefficients on Exp_i and $S8_i$ capture the ITT estimates for the experimental and Section 8 groups, respectively. The ITT effect represents the estimated effect of MTO on the assigned group as a whole, including both families who leased up and families who never used an MTO voucher. The ITT estimate eliminates the problem of self-selection bias, because it compares the average outcomes of the entire treatment group (regardless of whether the family moved through MTO) with the average outcomes of the control group. Because of random assignment, the treatment and control groups should, on average, be identical regarding their baseline characteristics, so we can confidently attribute any subsequent differences in outcomes to the fact that the treatment groups were offered the opportunity to relocate through the MTO demonstration.

The TOT estimate represents the effect of MTO on the program movers; that is, the experimental and Section 8 group members who actually moved with the program vouchers. Under certain assumptions (for example, that the program did not affect families who did not use their MTO voucher), we can estimate TOT by dividing the ITT effect by the share of the experimental or Section 8 group that relocated with an MTO voucher (Angrist, Imbens, and Rubin, 1996; Bloom, 1984). The TOT estimate does not remove the self-selection bias, because it compares the members of the treatment group who leased up, a self-selected group, with would-be movers in the control group.[4] Because 47 percent of the experimental group and 62 percent of the Section 8 group relocated with an MTO voucher (Ludwig, 2012), TOT estimates are substantially larger than ITT estimates. For example, if the ITT for an outcome was 8 percentage points for the experimental group, the TOT estimate would be [.08/.47] = .17, or 17 percentage points.

Measures

The MTO long-term survey for the final impacts evaluation included an innovative combination of survey and administrative data collection. Within the survey interview setting, we administered math and reading achievement assessments; measured height and weight; constructed a full history of schools attended over the followup period; and used audio-enhanced, computer-assisted self-interviewing (audio-CASI) to ask about sensitive items related to mental health and risky behavior. We also collected a variety of administrative data, including postsecondary schooling data, criminal justice records, UI data, and government assistance data (food stamps and Temporary Assistance for Needy Families records).

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[4] The TOT approach assumes that those who did not use an MTO voucher experienced no average effect of being offered a voucher, which we believe is reasonable. Although the TOT estimates do not remove self-selection bias, the estimates are policy relevant because they focus on the effects that a new neighborhood environment would have on the individuals who would be most likely to participate in a housing voucher program.

School Characteristics

We used two types of information to describe school characteristics: (1) a variety of socioeconomic and demographic characteristics available from three national databases, and (2) students' self-reports of school climate. We constructed a full history of schools attended for each youth by combining parent reports on the youth's schooling through the time of the followup survey for the interim impacts evaluation (or kindergarten for youth who were not of school age when the family volunteered for the MTO program) with youth self-reports through the time of the long-term survey for the final impacts evaluation (or the highest grade attended for youth who were no longer in a primary or secondary school). We then matched the school histories to school characteristics from two National Center for Education Statistics databases (the Common Core of Data for public schools and the Private School Universe Survey for private schools) and a school-level test score database.

We also constructed a school climate index based on whether youth strongly agreed, agreed, disagreed, or strongly disagreed with five statements about their most recent school's climate. We asked youth if teachers were interested in students, if they felt "put down" by their teachers, if discipline was fair, if students who studied hard were teased, and if they felt safe in school. We constructed the index as the fraction of positive responses on the five items; that is, strongly agree or agree responses on teacher interest in students, fair discipline, and feeling safe, and disagree or strongly disagree responses on feeling put down by teachers and teasing of students who study hard.

Math and Reading Achievement

At the end of the survey interview, we administered a 45-minute achievement assessment, an adapted version of the assessment used for the ED's Early Childhood Longitudinal Study-Kindergarten Cohort (ECLS-K). Youth ages 13 to 20 as of December 2007 took a slightly modified version of the eighth grade ECLS-K assessment, administered in two stages: a first-stage routing test that the survey interviewers scored in real time, the score of which then determined which form of the second-stage test to administer.[5] We contracted with the Educational Testing Service (ETS) to score the assessments via estimates (known in the testing literature as theta scores) of each youth's underlying academic ability from a statistical model based on item response theory (IRT). IRT scoring allows for the reliable prediction of a student's ability on a full set of testing items based on only a subset of those items, which was important for the MTO study, given the limited time available in the survey interview setting.[6] We converted the ETS achievement theta scores into z-scores by subtracting the control group's average test score from each youth's individual test score, then dividing by the standard deviation of the control group's test score distribution. By construction, the control group's average test score in this z-score metric will equal 0.

We selected the ECLS-K assessments for several reasons. They are designed to measure what children learn in school (as opposed to measuring aptitude only) and are sensitive to capturing whether MTO moved children into improved schooling and learning environments. They also

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[5] Youth ages 10 to 12 also took an assessment, based on the ECLS-K fifth grade test, but the results for that age group do not qualitatively differ from those for youth ages 13 to 20, and we focus here on the eighth grade test.

[6] Further details are available upon request. Also, see Reardon (2008).

include appropriate coverage of material that is relevant for the wide dispersion of ages of youth in the long-term survey for the final impacts evaluation, have good discriminating power across a wide range of ability levels, and have been extensively pretested and piloted (for example, to ensure that the test items work equally well for racial and ethnic subgroups).

The possibility that older youth in our survey sample would find the items on the tests too easy (known in the testing literature as a ceiling effect) was one concern about the ECLS-K assessments. To address this concern, we supplemented the ECLS-K eighth grade test with a small set of math and reading items from ED's National Educational Longitudinal Survey-1988 (NELS) assessment for high school students. Only about 8 percent of MTO youth ages 13 to 20 performed well enough to take these additional NELS items in math or reading. The possibility of a floor effect, in which the assessment is too difficult for some children and so loses its ability to distinguish the achievement of students at the bottom of the distribution, was another concern. About 14 percent of youth ages 13 to 20 performed at less than the level of chance on the reading test—that is, more poorly than we would have expected if they had simply guessed at every test question—and about 7 percent did so on the math test.

Educational Completion and Idleness

During the survey interview, we asked older MTO youth (ages 15 to 20 as of December 2007) about their schooling, completed education, and participation in employment or training. From these measures, we constructed a measure of youth or young adult idleness defined as "not currently employed" and "not currently in school" at the time of the survey interview. We also obtained UI records and postsecondary enrollment data from the National Student Clearinghouse (NSC).[7]

Physical Health

Self-reported physical health measures included overall health, asthma, and accidents and injuries. To measure obesity, interviewers measured youth height and weight using the same protocols they used for adults then converted the results to the standard Body Mass Index (BMI) formula of weight in kilograms divided by height in meters squared. Because BMI tends to increase naturally through adolescence, instead of using the standard definition of obesity used for adults (BMI ≥ 30), we defined obesity using criteria developed by the International Obesity Task Force (Cole et al., 2000). Those criteria use growth curves based on age and gender that align with the standard adult BMI standards. The criteria further break down curves by gender because, whereas BMI tends to follow a linear trend for males, it tends to follow a more concave trend line for females, and because puberty generally affects female bodies at different ages than it does male bodies.

Mental Health

We administered two short questionnaires to measure psychological distress and behavioral and emotional problems. The first was the Kessler 6 (K6), a six-item questionnaire used to determine

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[7] NSC data were available back through 2001, but it took until the end of 2006 for NSC to be near complete (96 percent of schools had joined NSC by then), so we have limited our analysis to the 3-year period from January 2007 to January 2010.

general psychological distress. Youth reported how often in the past 30 days they felt so sad that nothing could cheer them up, nervous, restless or fidgety, hopeless, that everything is an effort, and worthless. The K6 raw score can range from 0 (no distress) to 24 (highest distress), and our K6 measure is a z-score based on the control group mean and standard deviation, with standardization separated by gender and flipped such that a lower score indicates less psychological distress. The second questionnaire was a brief version of the Strengths and Difficulties Questionnaire (SDQ), which is used to identify behavioral and emotional problems. Interviewers read five statements to youth, who reported how true (very, somewhat, or not) each statement was about their general behavior. The five statements concerned general obedience, worry and anxiety, feeling unhappy or depressed, getting along better with adults than with peers, and task completion and attention span. Raw SDQ scores can range from 0 (no behavioral or emotional problems) to 10 (severe behavioral or emotional problems). A score of 6 or higher is a commonly used indicator of serious behavioral or emotional problems.

Risky and Criminal Behavior

Similar to the approach used in the followup survey for the interim impacts evaluation, our approach measured risky and criminal behavior through both youth reports and data matches with criminal justice records. To reduce the likelihood of youth underreporting sensitive or illegal behaviors, we administered many of the sensitive items in the survey about risky behaviors via audio-CASI. We constructed three indices: risky, problem, and delinquent behavior. The risky behavior index is the fraction of 4 risky behaviors in which the youth reported ever having engaged: smoking, alcohol use, marijuana use, and sex. The delinquency index is, similarly, the fraction of 8 delinquent behaviors: drug selling, gang involvement, gun possession, attack on another person, property destruction, theft of an item worth less than $50, theft of an item worth more than $50, and other property crime. Finally, the behavior problems index is the fraction of 11 behaviors that the youth reported were true or sometimes true (as opposed to not true) of their behavior in the 6 months before the survey interview: trouble paying attention, lying or cheating, teasing others, disobeying parents, trouble sitting still, hot temper, would rather be alone, hanging out with kids who get in trouble, disobeying at school, not getting along with other kids, and trouble getting along with teachers.

Results

MTO had few detectable long-term effects on achievement and educational outcomes, physical health, and several aspects of risky behavior. Children assigned to the experimental and Section 8 groups had similar scores on reading and math achievement tests compared with those in the control group. This finding held true for children who had not yet enrolled in school at baseline and who would have experienced particularly large MTO-induced changes in neighborhood environments very early in their development of cognitive, social-emotional, and behavioral skills. A pattern of generally beneficial effects on female youth and some detrimental effects on male youth echoes, but is more muted than, the pattern the followup survey for the interim impacts evaluation found (Kling, Liebman, and Katz, 2007; Kling, Ludwig, and Katz, 2005). Male youth who moved through MTO engaged in relatively more of some risky behaviors (smoking) than male youth in

the control group, and female youth who moved through MTO experienced declines in some risky behaviors (drinking) and improvements in mental health outcomes compared with female youth in the control group.

Neighborhood and School Environments

Ludwig (2012) showed MTO effects on neighborhood poverty and a range of neighborhood characteristics with improved reports of safety, crime rates, and household victimization in the experimental group compared with effects on households in the control group. Whether youth from these households experience the same types of neighborhood differences depends on where they spend their time and, in some cases, whether they continue to live with their parents. Like the adults whose reports about neighborhood safety Ludwig (2012) described, MTO youth reported feeling safer and witnessing less drug use and selling in their neighborhoods, as exhibit 1 shows.

Exhibit 1

Youth Neighborhood Safety and Social Networks

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Neighborhood safety [SR]						
Feels safe or very safe during the day	0.801	0.018 (0.016)	0.037 (0.033)	– 0.012 (0.018)	– 0.018 (0.026)	4,863
Feels safe or very safe at night	0.540	0.035~ (0.021)	0.074~ (0.043)	0.019 (0.022)	0.028 (0.033)	4,862
Saw drugs being sold or used in the neighborhood in the past 30 days	0.388	– 0.056* (0.020)	– 0.116* (0.040)	– 0.041* (0.021)	– 0.062* (0.031)	4,879
Any household member was a crime victim in the past 6 months (ages 13 to 20)	0.246	– 0.027 (0.018)	– 0.056 (0.038)	– 0.020 (0.020)	– 0.030 (0.029)	4,618
Social networks [SR]						
Visits with baseline friends at least a few times a week	0.280	– 0.035* (0.017)	– 0.073* (0.036)	– 0.038* (0.018)	– 0.057* (0.027)	5,001

ITT = intention to treat. SR = self-reported. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details.

Source: MTO youth long-term survey

One might expect such effects on neighborhood poverty and safety to translate to improvements in schools. Exhibit 2 shows, however, that MTO had relatively modest and somewhat mixed effects on school environments. We present youths' average school by combining characteristics for all schools and weighting them by the number of grades attended at each school. Both the experimental and Section 8 treatments had statistically significant effects on the racial and socioeconomic composition and schoolwide student academic performance of the new schools attended by youth in those groups. Youth in the control group, however, attended schools that, on average, had overwhelmingly (about 90 percent) minority enrollments, and most (70 percent) students eligible for

Exhibit 2

Characteristics of Schools Attended by Youth and Youth Perceptions of School Climate

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Characteristics of the average school attended by youth ages 10 to 20						
Share minority [CCD, PSS]	0.904	– 0.037* (0.008)	– 0.077* (0.016)	– 0.016~ (0.008)	– 0.023~ (0.012)	5,077
Share eligible for free lunch [CCD]	0.701	– 0.048* (0.007)	– 0.101* (0.015)	– 0.026* (0.008)	– 0.039* (0.012)	5,043
Number of students [CCD, PSS]	927.1	25.6~ (14.9)	53.4~ (31.1)	41.8* (17.5)	62.5* (26.1)	5,077
Pupil-teacher ratio [CCD, PSS]	17.834	– 0.103 (0.103)	– 0.214 (0.215)	– 0.061 (0.113)	– 0.091 (0.170)	5,076
School-level percentile ranking on state exam [SLAD]	18.684	3.070* (0.651)	6.430* (1.364)	1.218~ (0.661)	1.810~ (0.983)	4,884
School climate index among youth ages 10 to 17 [SR]						
All	0.797	0.020~ (0.011)	0.043~ (0.023)	– 0.002 (0.012)	– 0.003 (0.017)	3,328
Female	0.786	0.025~ (0.015)	0.052~ (0.032)	0.006 (0.016)	0.010 (0.025)	1,694
Male	0.807	0.016 (0.014)	0.034 (0.031)	– 0.011 (0.016)	– 0.015 (0.022)	1,634

CCD = common core of data. ITT = intention to treat. PSS = Private School Universe Survey. SLAD = school-level assessment data from the National Longitudinal School-Level State Assessment Score Database. SR = self-reported. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details. The number of students is based on enrollment as of October of each year. The percentile ranking measure includes schools through eighth grade only for New York and Maryland. The school climate index is the fraction of positive responses on five items (students get teased if they study hard, discipline is fair, often feels put down by teachers, feels safe in school, and teachers interested in students). If the youth agreed or strongly agreed with a positive school climate item (for example, fair discipline), or if the youth disagreed or strongly disagreed with a negative school climate item (for example, students teased if they study hard), then the response was counted as positive. Average school characteristics are weighted by the amount of time youth spent at each school.

Source: MTO youth long-term survey

free or reduced-priced lunches, so despite these MTO effects, the new schools differed only very slightly on these characteristics. For example, with a TOT estimate of 7.7 percentage points on share minority and a TOT estimate of 10.1 percentage points on share eligible for free lunch, youth who moved with the low-poverty voucher were still in schools that had 82.3 percent minority enrollments and where more than one-half of the student population was eligible for free or reduced-price lunches. Youth in both treatment groups were also more likely to attend larger schools than were youth in the control group, but an increase in the pupil-to-teacher ratio did not accompany the increase in student body size. Although MTO effects on school characteristics are mixed, youth in the experimental group were more likely to report a favorable climate in their most recent school, and the effects appear to be driven by interactions with teachers; youth in the experimental group were more likely to report that teachers were interested in students and less likely to report that they felt put down by their teachers.[8]

Schooling and Labor Market Outcomes

MTO, in general, had very few detectable effects on achievement, education, or employment outcomes for youth. Exhibit 3 shows that MTO had no detectable effects on math or reading achievement test scores among youth ages 13 to 20, even for the subset of children who were younger than 6 years old at the time of study enrollment and who we had hypothesized would benefit the most from moves to lower poverty neighborhoods. The standard errors on these estimates are quite large; sometimes nearly double the size of the point estimates. In general, with the caveat that we adjusted the ECLS-K tests slightly for this study, MTO youth ages 13 to 20 as of December 2007 appeared to perform similarly to national estimates among low-income eighth graders. The MTO

Exhibit 3

Achievement Assessment Results for Youth Ages 13 to 20 as of December 2007 (1 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Overall						
Reading assessment score [ECLS-K]						
All	0.000	0.003	0.006	0.041	0.060	4,432
		(0.041)	(0.085)	(0.044)	(0.064)	
Female	0.000	– 0.020	– 0.040	0.055	0.083	2,286
		(0.056)	(0.113)	(0.063)	(0.095)	
Male	0.000	0.027	0.057	0.025	0.035	2,146
		(0.054)	(0.115)	(0.057)	(0.079)	
Math assessment score [ECLS-K]						
All	0.000	– 0.025	– 0.052	– 0.003	– 0.004	4,420
		(0.044)	(0.091)	(0.048)	(0.070)	
Female	0.000	– 0.036	– 0.073	– 0.038	– 0.057	2,280
		(0.060)	(0.121)	(0.067)	(0.101)	
Male	0.000	– 0.014	– 0.030	0.034	0.046	2,140
		(0.056)	(0.119)	(0.063)	(0.087)	

[8] The analysis is limited to youth younger than age 18 as of December 2007, because older youth had to recall secondary school experiences from multiple years before the interview.

Exhibit 3

Achievement Assessment Results for Youth Ages 13 to 20 as of December 2007 (2 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Baseline ages 0 to 5						
Reading assessment score [ECLS-K]						
All	0.000	0.026	0.055	0.078	0.105	2,542
		(0.052)	(0.112)	(0.055)	(0.074)	
Female	0.000	0.021	0.044	0.085	0.119	1,309
		(0.072)	(0.152)	(0.079)	(0.110)	
Male	0.000	0.030	0.067	0.069	0.090	1,233
		(0.069)	(0.152)	(0.070)	(0.091)	
Math assessment score [ECLS-K]						
All	0.000	– 0.057	– 0.123	– 0.060	– 0.081	2,535
		(0.057)	(0.121)	(0.059)	(0.079)	
Female	0.000	– 0.072	– 0.150	– 0.072	– 0.100	1,306
		(0.080)	(0.169)	(0.083)	(0.115)	
Male	0.000	– 0.042	– 0.093	– 0.048	– 0.063	1,229
		(0.072)	(0.157)	(0.079)	(0.103)	
Baseline ages 6 to 11						
Reading assessment score [ECLS-K]						
All	0.000	– 0.028	– 0.055	– 0.006	– 0.009	1,890
		(0.061)	(0.120)	(0.070)	(0.112)	
Female	0.000	– 0.066	– 0.127	0.021	0.035	977
		(0.085)	(0.163)	(0.102)	(0.173)	
Male	0.000	0.013	0.027	– 0.035	– 0.053	913
		(0.084)	(0.169)	(0.092)	(0.140)	
Math assessment score [ECLS-K]						
All	0.000	0.020	0.039	0.080	0.129	1,885
		(0.063)	(0.124)	(0.073)	(0.118)	
Female	0.000	0.020	0.039	0.028	0.047	974
		(0.083)	(0.160)	(0.103)	(0.175)	
Male	0.000	0.017	0.035	0.135	0.203	911
		(0.086)	(0.174)	(0.099)	(0.149)	

ECLS-K = achievement assessment from the Early Childhood Longitudinal Study-Kindergarten cohort study. ITT = intention to treat. TOT = treatment on the treated.

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details. The reading and math achievement assessment scores are theta scores transformed into z-scores via standardization on the mean and standard deviation for control group youth ages 13 to 20. Results reported differ slightly from those in Sanbonmatsu et al. (2011) because here standardization was separate by gender in the overall results and by baseline age group and gender in the baseline age results, whereas Sanbonmatsu et al. standardized only on the overall control group mean and standard deviation. The overall (male and female combined) z-score values combine the z-scores by gender and thus are not themselves standardized (the control mean is 0 but the standard deviation is not exactly 1).

Source: MTO youth long-term survey

youth mean for the 10-point routing score for reading is about 0.6 points higher than the national mean for eighth graders in the lowest quintile of socioeconomic status; for math, the MTO youth mean is about 0.4 points lower than the comparable national average (Najarian et al., 2009).

Exhibit 4 shows that, in general, MTO also had few detectable effects on measures of educational completion and employment. As of the survey interview (late 2008 to early 2010), about one-fifth of the older youth (ages 15 to 20 as of December 2007) sample was currently idle; that is, neither working nor in school. Another way of characterizing this finding is that about 80 percent of the older youth sample was educationally on track; that is, was currently in school or had received a high school diploma or GED. About one-fourth had attended any college, most at 2-year and public colleges (control group means of 14 percent and 15 percent, respectively). About 40 percent reported being currently employed. Self-reported employment rates appear lower for experimental group members, a finding the UI data corroborated (although variation by site merits further investigation).[9]

MTO had few detectable effects on grown children's schooling, completed education, and participation in employment or training, based on proxy reports from adults, UI records, and postsecondary enrollment data from NSC. Parents reported that nearly 40 percent of grown children were idle and that about 70 percent had a high school diploma or GED, and NSC data indicate that about 20 percent had attended college since 2007 (as with the younger youth sample, public and 2-year colleges comprised most of the older youth sample's enrollment). Both parent reports and UI records showed that about one-half of the grown children sampled were employed.[10] On all of these measures, female grown children look better than their male counterparts, with mean differences of 10 to 15 percentage points.

Exhibit 4

Education and Employment Outcomes for Youth Ages 15 to 20 as of December 2007 (1 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Currently idle (neither employed nor enrolled in school) [SR]						
All	0.215	0.007 (0.018)	0.014 (0.038)	0.026 (0.020)	0.039 (0.031)	3,604
Female	0.194	0.024 (0.024)	0.049 (0.048)	0.031 (0.027)	0.048 (0.043)	1,838
Male	0.235	– 0.011 (0.027)	– 0.023 (0.058)	0.022 (0.031)	0.032 (0.045)	1,766
Educationally on track [SR]						
All	0.814	– 0.014 (0.018)	– 0.028 (0.037)	– 0.029 (0.021)	– 0.044 (0.031)	3,614
Female	0.827	– 0.007 (0.023)	– 0.015 (0.046)	0.008 (0.026)	0.012 (0.040)	1,842
Male	0.801	– 0.019 (0.026)	– 0.041 (0.055)	– 0.066* (0.029)	– 0.096* (0.042)	1,772

[9] The UI data for this finding came from Maryland, Massachusetts, Illinois, and California; data from New York were not available.

[10] Again, the UI data for this finding came from Maryland, Massachusetts, Illinois, and California; data from New York were not available.

Exhibit 4

Education and Employment Outcomes for Youth Ages 15 to 20 as of December 2007 (2 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Currently enrolled in school [SR]						
All	0.615	0.018	0.037	– 0.019	– 0.029	3,621
		(0.020)	(0.041)	(0.022)	(0.034)	
Female	0.637	0.001	0.002	0.001	0.001	1,845
		(0.027)	(0.054)	(0.030)	(0.047)	
Male	0.594	0.036	0.077	– 0.039	– 0.057	1,776
		(0.028)	(0.061)	(0.030)	(0.044)	
Currently employed [SR]						
All	0.395	– 0.046*	– 0.095*	– 0.033	– 0.050	3,604
		(0.021)	(0.043)	(0.022)	(0.034)	
Female	0.410	– 0.051~	– 0.104~	– 0.042	– 0.066	1,838
		(0.029)	(0.059)	(0.031)	(0.049)	
Male	0.381	– 0.041	– 0.087	– 0.025	– 0.036	1,766
		(0.030)	(0.064)	(0.033)	(0.049)	
Employed [UI]						
All	0.256	– 0.036*	– 0.076*	0.004	0.006	3,490
		(0.015)	(0.032)	(0.018)	(0.025)	
Female	0.272	– 0.030	– 0.063	0.012	0.017	1,691
		(0.024)	(0.051)	(0.026)	(0.037)	
Male	0.241	– 0.045*	– 0.093*	– 0.002	– 0.003	1,799
		(0.021)	(0.044)	(0.023)	(0.032)	
Attended any college since 2007 [NSC]						
All	0.262	– 0.014	– 0.029	– 0.021	– 0.033	4,717
		(0.016)	(0.033)	(0.017)	(0.025)	
Female	0.305	– 0.011	– 0.023	0.000	0.001	2,300
		(0.023)	(0.048)	(0.025)	(0.039)	
Male	0.222	– 0.016	– 0.033	– 0.042*	– 0.064*	2,417
		(0.021)	(0.044)	(0.021)	(0.032)	

ITT = intention to treat. NSC = National Student Clearinghouse. SR = self-reported. TOT = treatment on the treated. UI = unemployment insurance administrative records.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details. On-track youth are those who were currently in school or received a high school diploma or general equivalency diploma. Currently enrolled in school also includes youth who were on summer vacation. UI data look only at the last four common calendar quarters (fourth quarter of 2007 through third quarter of 2008).

Source: MTO youth long-term survey

Physical and Mental Health

Exhibit 5 shows that MTO's effects on the physical health measures were similar overall and by gender across treatment groups but that the low-poverty vouchers had some encouraging effects on mental health, primarily for female youth. Nearly 90 percent of youth reported good, very good, or excellent (as opposed to poor or fair) health. Almost 20 percent of youth reported having had an asthma or wheezing attack in the past year, and a similar share had had an accident or injury requiring medical attention in the past year.[11] Obesity rates were much higher for females

Exhibit 5

Youth Physical and Mental Health (1 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Physical health (ages 10 to 20)						
Currently good or better health [SR]						
All	0.883	0.005	0.009	0.000	– 0.001	5,100
		(0.013)	(0.027)	(0.015)	(0.022)	
Female	0.862	0.003	0.007	0.006	0.010	2,600
		(0.019)	(0.038)	(0.021)	(0.034)	
Male	0.903	0.006	0.012	– 0.007	– 0.010	2,500
		(0.016)	(0.035)	(0.019)	(0.027)	
Asthma or wheezing attack during the past year [SR]						
All	0.190	0.006	0.013	– 0.013	– 0.019	5,092
		(0.015)	(0.032)	(0.016)	(0.024)	
Female	0.206	– 0.009	– 0.017	– 0.021	– 0.032	2,595
		(0.021)	(0.043)	(0.023)	(0.037)	
Male	0.174	0.021	0.045	– 0.006	– 0.008	2,497
		(0.020)	(0.043)	(0.022)	(0.031)	
Had accidents or injuries requiring medical attention in the past year [SR]						
All	0.178	0.006	0.013	0.008	0.012	5,097
		(0.014)	(0.030)	(0.016)	(0.024)	
Female	0.164	– 0.023	– 0.046	– 0.024	– 0.037	2,597
		(0.018)	(0.037)	(0.020)	(0.031)	
Male	0.192	0.035	0.076	0.039	0.056	2,500
		(0.022)	(0.047)	(0.024)	(0.034)	
Currently obese [M, SR]						
All	0.229	– 0.010	– 0.022	– 0.010	– 0.014	5,034
		(0.017)	(0.035)	(0.018)	(0.028)	
Female	0.274	– 0.028	– 0.057	– 0.020	– 0.031	2,560
		(0.025)	(0.051)	(0.026)	(0.041)	
Male	0.187	0.007	0.016	0.000	0.000	2,474
		(0.021)	(0.045)	(0.023)	(0.033)	

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[11] In results not shown, however, we do see that males were somewhat more likely than females to report more serious accidents or injuries.

Exhibit 5

Youth Physical and Mental Health (2 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Mental health (ages 13 to 20)						
Psychological distress index (K6) z-score, past month (higher score indicates greater distress) [SR]						
All	0.000	– 0.038	– 0.079	0.038	0.056	4,644
		(0.041)	(0.086)	(0.047)	(0.070)	
Female	0.000	– 0.116*	– 0.234*	– 0.013	– 0.020	2,371
		(0.056)	(0.113)	(0.065)	(0.101)	
Male	0.000	0.041	0.088	0.087	0.124	2,273
		(0.056)	(0.120)	(0.063)	(0.089)	
Serious behavioral or emotional problems [SR]						
All	0.103	– 0.022~	– 0.046~	0.019	0.029	4,644
		(0.011)	(0.024)	(0.014)	(0.021)	
Female	0.127	– 0.033*	– 0.068*	0.030	0.047	2,371
		(0.017)	(0.034)	(0.021)	(0.032)	
Male	0.081	– 0.010	– 0.021	0.007	0.010	2,273
		(0.015)	(0.032)	(0.017)	(0.024)	

ITT = intention to treat. M = measured. SR = self-reported. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details. Obesity is defined according to the International Obesity Task Force. The psychological distress index consists of six items (sadness, nervousness, restlessness, hopelessness, feeling that everything is an effort, worthlessness) scaled on a score from 0 (no distress) to 24 (highest distress) and then converted to z-scores using the mean and standard deviation of control group youth. Results reported differ slightly from those in Sanbonmatsu et al. (2011) because here standardization was separate by gender, whereas Sanbonmatsu et al. standardized only on the overall control group mean and standard deviation. The overall (male and female combined) z-score values combine the z-scores by gender and thus are not themselves standardized (the control mean is 0 but the standard deviation is not exactly 1). The serious behavioral or emotional problems measure is based on the strengths and difficulties questionnaire, which consisted of five behavioral and emotional items (obedience, worry or anxiety, unhappiness, getting along better with adults than peers, attention span) scaled on score from 0 (no behavioral or emotional problems) to 12 (severe behavioral or emotional problems). A score of 6 or higher indicates serious behavioral or emotional problems.

Source: MTO youth long-term survey

(27 percent) than for males (19 percent).[12] Self-reports of overall health, obesity, asthma, and injury rates were similar among youth in the experimental, Section 8, and control groups. For mental health, however, females in the experimental group were 0.12 standard deviations lower than their counterparts in the control group on the K6. They also were 3.3 percentage points less likely to have serious behavioral or emotional problems compared with a mean of 12.7 percent for

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[12] These MTO obesity rates appear to be higher than even the low socioeconomic status (SES) group in one national study (Sherwood et al., 2008). That study showed that about 34 percent of females and 32 percent of males in the low SES group were overweight, a less stringent standard than obese. For comparison, MTO youth overweight rates were 48 and 42 percent for females and males, respectively.

the control group, a more than 25-percent reduction in prevalence. These results are consistent with results from the interim impacts evaluation (Kling, Liebman, and Katz, 2007; Orr et al., 2003) and with MTO qualitative investigations in suggesting that female youth responded more strongly to the new and safer social environments in the neighborhoods to which the families in the experimental group moved (Popkin, Leventhal, and Weismann, 2008).

Via proxy reports from the adults, MTO had few detectable effects on the physical and mental health of grown children. Parents reported less than 5 percent of grown children as having a physical health problem that kept them from normal activities and less than 10 percent as having had a chronic health issue, such as cancer or a heart problem. About 10 percent had depression or another serious mental health problem, and about 5 percent had an alcohol or drug problem. Physical health problems did not vary by gender, but parents were somewhat more likely to report males as having a mental health or substance abuse problem.

Risky and Delinquent Behavior

Exhibit 6 shows few detectable effects on the three risky and problem behavior indices used in the long-term survey to measure a composite of behaviors related to drinking, drug abuse, sexual activity, and gun possession. Although MTO had no detectable effects on the overall risky behavior index, males in both treatment groups reported higher rates of smoking than males in the control

Exhibit 6

Risky and Criminal Behavior Outcomes for Older Youth (1 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Risky and delinquent behavior for youth ages 13 to 20						
Risky behavior index [SR]						
All	0.467	– 0.001 (0.014)	– 0.002 (0.029)	0.007 (0.015)	0.010 (0.022)	4,623
Female	0.442	– 0.027 (0.019)	– 0.054 (0.037)	– 0.017 (0.020)	– 0.026 (0.031)	2,358
Male	0.491	0.025 (0.018)	0.053 (0.039)	0.029 (0.020)	0.042 (0.028)	2,265
Behavior problems index [SR]						
All	0.379	0.004 (0.010)	0.008 (0.022)	0.009 (0.011)	0.013 (0.017)	4,629
Female	0.371	– 0.007 (0.014)	– 0.014 (0.028)	– 0.010 (0.015)	– 0.015 (0.024)	2,361
Male	0.387	0.015 (0.015)	0.032 (0.032)	0.027~ (0.016)	0.038~ (0.023)	2,268
Delinquency index [SR]						
All	0.146	– 0.002 (0.008)	– 0.004 (0.017)	0.008 (0.009)	0.012 (0.014)	4,625
Female	0.110	– 0.006 (0.009)	– 0.011 (0.019)	– 0.005 (0.010)	– 0.008 (0.016)	2,360
Male	0.181	0.002 (0.013)	0.004 (0.029)	0.021 (0.016)	0.030 (0.022)	2,265

Exhibit 6

Risky and Criminal Behavior Outcomes for Older Youth (2 of 2)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control		Respondents (N)
		ITT	TOT	ITT	TOT	
Ever smoked [SR]						
All	0.312	0.042*	0.088*	0.043*	0.064*	4,618
		(0.019)	(0.039)	(0.020)	(0.030)	
Female	0.297	0.022	0.044	0.016	0.026	2,355
		(0.025)	(0.051)	(0.028)	(0.043)	
Male	0.327	0.062*	0.134*	0.069*	0.098*	2,263
		(0.025)	(0.054)	(0.028)	(0.040)	
Ever had alcoholic drink [SR]						
All	0.534	– 0.032	– 0.067	– 0.017	– 0.026	4,618
		(0.020)	(0.041)	(0.021)	(0.032)	
Female	0.541	– 0.061*	– 0.124*	– 0.032	– 0.050	2,355
		(0.026)	(0.053)	(0.029)	(0.045)	
Male	0.528	– 0.003	– 0.006	– 0.004	– 0.005	2,263
		(0.027)	(0.058)	(0.029)	(0.041)	
Number of arrests by crime type for youth ages 15 to 20						
Violent crime arrests [CJR]						
All	0.325	0.043	0.091	– 0.062	– 0.094	4,717
		(0.037)	(0.078)	(0.039)	(0.059)	
Female	0.155	0.027	0.055	– 0.048	– 0.074	2,300
		(0.033)	(0.069)	(0.033)	(0.050)	
Male	0.481	0.060	0.128	– 0.076	– 0.115	2,417
		(0.064)	(0.138)	(0.068)	(0.102)	
Property crime arrests [CJR]						
All	0.239	0.065*	0.136*	– 0.013	– 0.019	4,717
		(0.031)	(0.064)	(0.034)	(0.051)	
Female	0.091	0.044~	0.090~	– 0.010	– 0.015	2,300
		(0.026)	(0.053)	(0.023)	(0.035)	
Male	0.375	0.086	0.183	– 0.016	– 0.025	2,417
		(0.054)	(0.117)	(0.060)	(0.090)	

CJR = criminal justice records. ITT = intention to treat. SR = self-reported. TOT = treatment on the treated.

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

Notes: Robust standard errors shown in parentheses. The control mean is unadjusted. Unless otherwise indicated, the control mean and effects are expressed as shares of the sample in the category (for example, a control mean of 0.250 for working would indicate that 25 percent of the control group was working). Experimental and Section 8 effects were estimated jointly using an ordinary least squares regression model controlling for baseline covariates, weighted, and clustering on family. Youth and grown children effects by gender were estimated as an interaction with treatment status. See the forthcoming technical appendixes to Sanbonmatsu et al. (2011) for details. The risky behavior index is the fraction of four risky behaviors (smoking, alcohol use, marijuana use, and sex) that the youth reports ever having exhibited. The behavior problems index is the fraction of 11 problem behaviors (for example, difficulty concentrating and having a strong temper) that the youth reported as true or sometimes true at present or in the past 6 months. The delinquency index is the fraction of eight delinquent behaviors (for example, carrying a gun and destroying property) that the youth reported ever having exhibited. Violent crime arrests involve charges of force or threat of force including homicide, rape, robbery, assault, kidnapping, and weapons charges. Property crime arrests involve taking money or property and include burglary, motor vehicle theft, larceny, trespassing, and receiving stolen property.

Source: MTO youth long-term survey

group, and females in the experimental group reported lower rates of drinking than females in the control group.[13] In general, the number of violent and property crime arrests for male youth in the control group is about 0.86, more than 3.5 times the number for female youth. MTO increased property crime arrests among female and male youth in the experimental group relative to their peers in the control group, although the point estimates are imprecisely estimated and not quite statistically significant ($p = .12$). Unlike patterns observed with the followup survey data for the interim impacts evaluation, the number of violent crime arrests, at about 0.48 for males and 0.16 for females, is similar across groups. In results not shown, however, males in the experimental group appear to have a lower number of arrests for drug selling or distribution (roughly 11 percent were ever arrested for drug possession and 8.7 percent were ever arrested for drug distribution). We have a bit more confidence in this preliminary finding, which calls for further investigation, because the pattern is apparent among both older male youth and grown male children. MTO showed no detectable effects on the property or violent crime arrests of grown children.

Discussion and Conclusions

Youth in economically disadvantaged neighborhoods fare less well across a host of educational, health, and behavioral outcomes than do youth who grow up in more economically advantaged neighborhoods. Isolating the extent to which neighborhoods per se contribute to this variation is important for the design of means-tested housing programs and other place-based programs, but isolating neighborhood effects has been challenging in practice because of the difficulty in uncoupling neighborhood effects from so many other aspects of the youth and their families that are associated with moving to and staying in particular places. The MTO study helped overcome these issues of selection bias.

In conducting the long-term survey for the final impacts evaluation, we discovered that MTO had a number of unanticipated effects on youth health (as found with the adults), particularly benefiting female mental health outcomes, but few detectable effects on achievement, education, employment, and a range of health and risky behavior outcomes among either youth or grown children. These patterns of findings are quite similar to what the followup survey for the interim impacts evaluation found. In particular, gender differences in MTO's effects appear for this cohort of youth in the long-term survey for the final impacts evaluation just as they did for a slightly older cohort of youth in the followup survey for the interim impacts evaluation.

MTO's effects on achievement and related schooling outcomes were disappointing, particularly among the youngest cohort of children at MTO enrollment, whom we hypothesized would benefit the most from MTO moves into lower poverty neighborhoods. Although many MTO parents seem to believe that the quality of local public schools is an important mechanism through which neighborhood location may influence children's academic achievement and attainment (Briggs, Popkin, and Goering, 2010), MTO had more modest effects on school quality than on other neighborhood

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[13] An alternative version of the risky behavior index, not presented here, includes a measure of ever having been pregnant (female) or having impregnated someone (male) instead of the current item on sexual activity. MTO's effects on the index including the pregnancy item do not statistically differ across groups. Roughly 20 percent of male youth and 27 percent of female youth reported ever having impregnated someone or ever having been pregnant.

social conditions. Children assigned to the two treatment groups attended schools that served students who were slightly less likely to have very low test scores, be poor, or be members of racial and ethnic minority groups compared with the student served in the schools that children in the control group attended, but they were still in generally low-performing schools that served overwhelmingly poor and majority-minority student populations. These findings raise questions about whether investing directly in schools might be more effective for improving schooling outcomes among economically disadvantaged youth (see a recent review of literature in Duncan and Murnane, 2011). For example, studies have found Success for All, a comprehensive reading intervention that involves extra time for reading, ability grouping, frequent assessment, and remediation (including tutoring), to improve reading scores for elementary and perhaps middle schoolchildren (Borman et al., 2007; Chamberlain et al., 2007). High schools organized as career academies that integrate academic and technical curricula and work-based learning opportunities with local employers produced sizable long-term (11-year) earnings improvements for youth in low-income urban settings (Kemple, 2008).

The MTO design is not well suited to answer how or why MTO produced the effects it did, particularly the differences between females and males. The survey data measured several of the proposed mechanisms, however, and future research will investigate them. Qualitative interviews can further deepen our understanding of how MTO altered the lives of families. Qualitative interviews with families after the interim followup survey suggest that the nature of how boys and girls interact socially with peers may mean that girls are more likely to successfully adapt to life in low-poverty areas. Parents reported that girls were more likely to visit with friends on their porches or inside their homes, in part because they placed their girls on a shorter leash than they did their boys. Boys, on the other hand, often hang out in public spaces, elevating the risk for conflict with neighbors and police and increasing their exposure to delinquent peer groups and opportunities to engage in delinquent activities themselves (Clampet-Lundquist et al., 2006; Popkin, Leventhal, and Weismann, 2008).

MTO generated large reductions in the neighborhood poverty and improvements in the neighborhood safety experienced by families in the two treatment groups relative to the families in the control group during the 10 to 15 years after random assignment. The low-poverty and traditional vouchers led to much smaller changes in MTO children's school quality than in their residential neighborhood conditions, however. Because of its random-assignment design, MTO provides crucial data and compelling evidence on the likely effects on families of such moves from extremely poor to less poor neighborhoods. One difference between the MTO experiment results and those from the broader literature on neighborhood effects is that MTO did not result in many families moving to truly affluent neighborhoods. Another is, of course, that most of that broader literature is nonexperimental, which raises concerns about the nonrandom sorting of families into neighborhoods, even conditional on rich controls for the variables contained in standard data sets. Our overall conclusions about those effects on youth outcomes after 10 to 15 years are similar to those we reached after 4 to 7 years. For educational outcomes in particular, we conclude over both time horizons that if achievement effects exist, they are small and not detectable in our analysis. Most surprisingly, that result holds for children who were very young at the time of their initial moves out of poor neighborhoods.

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Making MTO Health Results More Relevant to Current Housing Policy: Next Steps

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Abstract

This article examines the Moving to Opportunity (MTO) for Fair Housing demonstration and concludes that it has limited relevance for understanding the effects of the federal Section 8 Housing Choice Voucher Program (Section 8 Program) for four reasons. First, MTO focused on a group of people who lived in public housing at the outset of the study, and this subpopulation represents a small fraction of the recipients of the Section 8 Program. Second, MTO improves neighborhood quality more, on average, than the Section 8 Program does. Third, MTO fails to activate a mechanism that often improves health and is central to the Section 8 Program. Fourth, the U.S. Department of Housing and Urban Development could probably not bring MTO's major treatment condition to scale because of the relative shortage of affordable rental units in affluent neighborhoods. Because MTO had its clearest effects in the health domain, this article briefly outlines a study of the health effects of the Section 8 Program.

Introduction

The Moving to Opportunity (MTO) for Fair Housing demonstration was designed about 20 years ago. It involved randomly assigning a sample of families living in very poor public housing neighborhoods to one of three groups: (1) a control group that initially remained in public housing, (2) a Section 8 group that initially received traditional housing vouchers to help pay rent on any private-market dwelling unit that met U.S. Department of Housing and Urban Development (HUD) qualifications, and (3) an experimental group that initially received vouchers that the families could use only if they moved to a qualified unit in a neighborhood with a poverty rate of less than 10 percent. Comparing outcomes across the three different groups 10 to 15 years after random assignment

informs us about the long-term effects of public housing residents leaving a high-poverty neighborhood in a research project that did not involve the usual selection process for housing vouchers, which is heavily oversubscribed nationally. In MTO, new vouchers were set aside for participants; they were not obtained via a local lottery from existing voucher stocks.

MTO was designed in response to social science theories suggesting that people living in concentrated poverty are cut off from legitimate work opportunities and middle-class behavioral norms and that this social isolation is responsible for generating a host of negative social outcomes (Wilson, 1987). Wilson's theory is about the consequences of living among many poor families in an underresourced setting; by contrast, MTO is about the effects of moving from such a setting into a more affluent one. MTO is relevant to public policy, however, because, over the past 20 years, housing vouchers have increasingly replaced public housing as the main source of government housing support for low-income families, and such vouchers are supposed to promote mobility into better housing units in better neighborhoods. This national goal makes it important to learn whether voucher-induced moves to neighborhoods with fewer poor families can, within a single generation, overcome the individual and familial damage caused by the high-poverty neighborhoods in which public housing families formerly lived.

MTO is not the first study to take advantage of a lottery that randomly assigns some families to a housing voucher treatment group and others to a control group. Whereas other voucher experiments have compared voucher-based subsidies with no subsidies, however, MTO is unique in contrasting voucher receipt with living in subsidized public housing. All past voucher lottery studies have emphasized outcomes in the domains of labor force participation, welfare use, criminal behavior, and child and adult education. None of these studies observed consistent effects in these domains, and MTO is no exception to this disappointing picture. As MTO progressed, however, it became more health-focused than its predecessor studies thanks to its interim survey findings (Orr et al., 2003), which suggested that MTO reduced depression and anxiety among female heads of family and female youth (Kling, Liebman, and Katz, 2007). These interim findings led MTO researchers to increase the number of health assessments they made in the long-term followup survey, when the results (Ludwig et al., 2011; Sanbonmatsu et al., 2011) showed that, among adult women, upgrading neighborhood quality (1) maintained the superior mental health status previously noted, (2) reduced extreme obesity and diabetes, and (3) improved glycosylated hemoglobin levels (HbA1c). This last is a biomarker of likely future cardiovascular complications also associated with diabetes and obesity. These conceptually consistent MTO health results suggest that all past voucher studies may have looked for effects under the light of the wrong lamppost. It is now clear that, because of how MTO evolved and what it discovered, health outcomes deserve a higher profile in research on housing in general and on housing vouchers in particular.

Of course, no single study can do everything. MTO has several features that make it look like an evaluation of the Section 8 Housing Choice Voucher Program (Section 8 Program), the current budget of which is about $16 billion per year. These features include the use of housing vouchers in both treatment groups, one of which was called the Section 8 group because group members could use their voucher just like any family exiting public housing with a voucher. Nonetheless, we argue that MTO has limited relevance as an evaluation of the Section 8 Program writ large because of its restriction to families who were living in public housing at baseline. Of course, MTO did not

set out to be an evaluation of the Section 8 Program. Rather, it sought to describe how an enriched neighborhood alternative affects many different adult and youth outcomes, including health, relative to living in public housing. It is not, therefore, the fault of the MTO research design that so few new voucher holders come from public rather than private housing or that some important program requirements that might affect outcomes differ between these two groups. We use the MTO demonstration not to cavil about how the study was framed, designed, or analyzed, but rather to describe some possible next research steps in the study of housing voucher effects on health.

The argument we make is along four main lines. First, MTO's population does not represent the Section 8 Program population. MTO sought to maximize differences in neighborhood poverty concentration by studying public housing residents whose neighborhoods had some of the highest poverty rates in the United States and by requiring the principal treatment group of residents to use vouchers to move to neighborhoods with very low poverty rates. This dual strategy created the theoretically desired large neighborhood poverty contrast but, in so doing, led to a side effect that reduced MTO's relevance to the Section 8 Program writ large. In the Section 8 Program, most families applying for a voucher are already in the private housing market and so are not receiving a public housing subsidy. They also tend to be better off, less frequently members of racial or ethnic minorities, less female-headed, and almost certainly healthier—given the usual gradient linking health to socioeconomic status (Adler and Stewart, 2010)—than public housing families. These differences mean that the Section 8 Program involves a less vulnerable population than the public housing families in MTO, leading us to ask: Would MTO's health effects on public housing residents be replicated for the larger, more heterogeneous, and less vulnerable population of Section 8 Program voucher holders?

Second, the treatment contrasts achieved in MTO are greater than the mobility changes most Section 8 Program voucher holders spontaneously experience. Before their move, the average family in the Section 8 Program tends to live in less densely poor settings than public housing residents; when the family moves, it is probably to neighborhoods less affluent than those into which the MTO low-poverty housing voucher families were constrained to move, thus entailing a larger mobility difference in MTO than in the Section 8 Program. As we describe in the following sections, the MTO experimental group families moved from neighborhoods with about 50-percent poverty rates to those with about 10-percent poverty rates. Few families in the Section 8 Program make such dramatic neighborhood mobility changes. Of course, MTO also included a Section 8 group with no constraints on the poverty levels of the new neighborhoods. As we again describe in the following sections, however, the Section 8 group's initial 50-percent neighborhood poverty rate exceeds that of the average participants in the Section 8 Program, who are already in the more affluent private housing market when they get a voucher. Therefore, the mobility treatment contrast is probably even greater in the MTO Section 8 group than in the Section 8 Program, in which former public housing residents are rare.

Third, MTO probably involved causal mechanisms different from those found in the Section 8 Program. Public housing residents can use their housing vouchers only to change neighborhood and residence. By contrast, most Section 8 Program families already in private housing can use some of their voucher's monetary value to increase disposable income. More specifically, families already spending more than 30 percent of their adjusted income on rent in the private market—the vast

majority of voucher holders—can reduce out-of-pocket spending on rent by using their new voucher to pay a portion of the rent they used to pay and pocketing the difference. Section 8 Program rules place limits on how much substitution is possible, with the total amount depending on a family's income, their new and old rent payments, and local Fair Market Rent (FMR) values. The more a family wants to pay in rent after receiving a voucher compared with their prevoucher spending, the greater the gain in housing quality. By contrast, when families opt for a lesser difference between premove and postmove rent, the implicit income supplement is greater. The difference between MTO and the Section 8 Program is that MTO families could use their vouchers only to move to better housing, and most new voucher holders in the Section 8 Program are free to choose how they trade off between increasing their housing quality and supplementing their disposable income. Section 8 Program rules—and Chicago data we present in the following section—indicate that neighborhood upgrades are therefore greater but income supplements are therefore less for MTO families. MTO activates one mechanism to a greater degree than the Section 8 Program, but the Section 8 Program can activate two mechanisms—better housing and more disposable income.

Fourth, how MTO's health results would scale up to the national level is unclear because the Section 8 Program disproportionately comprises families leaving private-sector housing. These much greater numbers give rise to concerns about the limited supply of affordable rental units in neighborhoods with a poverty rate of 10 percent or less. Also, many poorer families are doubtlessly reluctant to relocate spontaneously in neighborhoods that are socially very different from those they know. MTO results we present in the following sections suggest this reluctance exists. By contrast, families already in private housing do not have to make such dramatic changes to move into neighborhoods that are 10 percent poor, and they are less likely to be racial or ethnic minority families reluctant to move into predominantly White settings. Therefore, scaling up MTO's findings would probably be problematic in the larger Section 8 Program, in which public housing families are quite rare in housing lotteries.

For all four reasons, we argue that the MTO demonstration's exciting health consequences cannot yet be responsibly extrapolated to the Section 8 Program. We call for a new voucher lottery study: a study in which (1) the population is all new Section 8 Program-eligible households, not just those currently living in subsidized public housing; (2) the variation in neighborhood poverty rate is one that spontaneously occurs rather than one that is experimentally imposed; (3) study families are free to use their vouchers not just for better housing but also to increase their disposable income; and (4) the major outcomes are a wide array of health and biological statuses assessed, not just on adult females and youth as in MTO, but on young children as well.

The MTO Population Is Different From the Current Voucher Population

The MTO participants were families living in public housing units in census tracts where at least 40 percent of the household incomes fell below the federal poverty line. In fact, the average tract poverty of the initial sample was 53 percent, emphasizing that concentrated poverty is especially prevalent in the public housing population. These facts make it plausible to assume that the MTO study population lived in worse housing and neighborhood conditions than current eligible

voucher holders in the Section 8 Program. Indeed, Jacob and Ludwig (2012) examined an expansion of the Section 8 Program in Chicago in the late 1990s. More than 80,000 people applied for a new voucher. About 90 percent were living in unsubsidized private housing when they applied, suggesting that any voucher evaluation results limited to public housing residents will not necessarily apply to the average voucher holder nationally.

In the same Chicago study, the average voucher applicant lived in a neighborhood with a poverty rate of about 29 percent. By contrast, the Chicago MTO sample's average baseline neighborhood poverty rate was about 50 percent across MTO's three groups. It seems likely, therefore, that the MTO study population is poorer than the overall voucher-eligible population and lives in poorer quality housing and worse neighborhoods. If so, these poverty rate differences are also likely to be associated with worse initial health status (Adler and Stewart, 2010), including the extreme obesity, diabetes, and HbA1c obtained in MTO. Were the families receiving vouchers through MTO initially less healthy in the aggregate than Section 8 Program voucher holders? If so, would MTO's health findings be replicated with the relatively more healthy (and more economically advantaged) national population of Section 8 Program voucher holders?

The Average Size of MTO's Treatment Contrast in Neighborhood Poverty Exceeds What We Would Expect in the Section 8 Program

HUD designed MTO to maximize differences in neighborhood poverty concentration, so it chose a public housing population whose pretreatment poverty rate averaged 53 percent. Some families were then assigned to the MTO experimental group. After 1 year, those so assigned who actually moved were living in neighborhood tracts averaging 11 percent poor. This 42-percent difference in neighborhood poverty is very large and totally commendable from MTO's theory-testing perspective. The size of this contrast decreased over time. Ten to 15 years later, the control group had moved, on average, from 53 to 31 percent poor tracts, whereas the experimental group movers had gone from 53 to 21 percent poor tracts. Movers among the Section 8 group had gone from 54 to 24 percent poor tracts. Thus, by the end of the study, a contrast of about 10 percentage points characterized how the control group differed from both the experimental and Section 8 groups. In the following sections, we examine some reasons for this temporal decrease in contrast size. For now, however, we point out that the health differences between the control and experimental groups was always statistically significant and large enough over time to obtain health effects in intention-to-treat analyses with only modest compliance rates (Ludwig et al., 2011)—a considerable achievement.

From the perspective of evaluating the Section 8 Program, however, the pertinent question is, "How big of a neighborhood poverty contrast would we expect when members of the broader Section 8 Program population move?" It is impossible to know exactly, but consider the following. Members of MTO's "traditional voucher group" were randomly assigned a Section 8 voucher and were free to move wherever they wanted. They initially moved to tracts with 29 percent poor, on average, appreciably better than the neighborhoods they left but not as affluent as the tracts, with an average 11 percent poor, to which the experimental group moved. The traditional housing voucher group, however, moved into neighborhoods with as many poor families (29 percent) as characterized the

premove Chicago voucher applicants in Jacob and Ludwig (2012), a stark contrast to the neighborhoods with 53 percent poor when MTO began. It is therefore impossible for the Chicago group, beginning at 29 percent, to experience the MTO low-poverty voucher group's poverty reduction of 42 percentage points. It would also be next to impossible to achieve the 24-percent reduction obtained in MTO's Section 8 group.

Over 10 to 15 years, MTO's treatment contrast shrank to 10 percent. We have no responsible way of knowing whether voucher holders in the Section 8 Program would achieve such a reduction across the same period. In Chicago, for instance, to achieve MTO's long-term, 10-percent absolute contrast in poverty rates would entail families starting in neighborhoods that are about 29 percent poor and eventually living in neighborhoods that are 19 percent poor. This 10-percent decrease is possible in the national program but does not reflect that families in the Section 8 Program start off in neighborhoods less disadvantaged than MTO's initial tract with 53 percent poor. It is almost impossible, therefore, for Section 8 Program families to experience a temporal pattern of neighborhood improvement as great as that in MTO. Because the treatment contrast in housing and neighborhood quality will be less in the Section 8 Program than in MTO, we must ask, "Would MTO's health effects be replicated in the Section 8 Program, in which the neighborhood quality contrast is almost certainly smaller than in the MTO low-poverty treatment group and likely to be even smaller than in the MTO traditional voucher treatment group?

MTO Varied Housing and Neighborhood Quality, Whereas the Section 8 Program Also Varies Disposable Income

For families living in public housing, obtaining a voucher replaces their public housing subsidy. They can use their new voucher to purchase better housing and a better neighborhood in the private housing market, but that is all. On the other hand, families who are already in the private market can also use a new voucher to increase disposable income and pay for things such as clothes, car repairs, food, and phone service. The voucher works this way for them because families already in the private housing market can use their voucher to substitute for the rent they used to pay before getting the voucher. The size of this substitution depends on their income, rent, and local FMR values. In practice, most Section 8 Program families probably apportion their voucher's monetary value between upgrading their housing and increasing their disposable income. Thus, Jacob and Ludwig (2012) estimated that a voucher enabled the average Chicago Section 8 Program household to spend about $3,840 more per year for housing and add $4,425 to its disposable income. Because public housing residents pay their new rent with a voucher and get nothing else, even if their rent is less than the voucher's full value, it is highly likely that voucher holders coming from private-market housing experience smaller neighborhood (and housing unit) upgrades but larger cash transfers than those MTO produced.

How will the Section 8 Program affect health if most of its participants come from private housing, and so its neighborhood contrast is smaller than MTO's but its disposable income supplement is larger? The additional income a family receives could reduce its members' psychosocial stress, or it could purchase more health services. Either or both of these mechanisms could then improve disease-related biological processes and physical and mental health in both adults and children.

Numerous correlational studies imply a link among income, biology, and health, as do some laboratory analog studies described in Adler and Stewart (2010), as do well-identified causal analyses of the health effects of both food stamps (Almond, Hoynes, and Schanzenbach, 2011) and the Earned Income Tax Credit (Hoynes, Miller, and Simon, 2011). Still unknown, however, is how the total effect of combining the larger income supplement and the smaller neighborhood upgrade in the Section 8 Program writ large compares with MTO's total health effect. Future research to examine this issue should also probe causal mechanisms. Is the average income supplement from vouchers substantial enough by itself to affect health to a degree that is meaningful for policy? Is the reduced neighborhood contrast relative to MTO nonetheless large enough to affect health to a meaningful extent? Perhaps especially important are questions about how income supplements and neighborhood improvements combine and interact to jointly influence health.

Housing Supply and Demand Would Probably Be Different in the Section 8 Program Than in MTO

Imagine a policymaker who wants to use the MTO health results to justify redesigning the Section 8 Program so that its recipients can use vouchers only to move to neighborhoods with less than 10 percent poor households. Such a policy supposes two things that are very likely wrong. The first is that the supply of affordable rental units in these affluent settings can meet the increased demand from new voucher holders. Affluent communities tend to be characterized by a greater fraction of individually owned homes as opposed to rental units, and many rental units in these communities are more expensive than voucher-eligible families can afford, even with a voucher. Also, a national program restricting voucher use to affluent neighborhoods would surely bid up rents in those places. Offering incentives to construct more rental units would, of course, offset such an increase. In affluent neighborhoods, however, we anticipate considerable reluctance to authorize the construction of more rental units at prices affordable for voucher-eligible families. Such resistance would probably be weaker for subsidized construction for elderly people and would probably be especially strong if the construction were for families with children, especially teenagers. In many affluent locations, it would be very difficult to achieve the number of affordable units needed to meet the increased demand that would follow from a Section 8 Program mandate to use vouchers only in affluent neighborhoods.

It is important to realize, however, that many voucher-eligible families might not want to live in affluent settings. Of families in the MTO experimental group, 53 percent did not use their voucher at all, one (of many) possible reason being that they did not want to live in places so different from the neighborhoods they were used to. Moreover, some of the families who moved initially did so again over the ensuing study years, after the requirement to live for 1 year in a neighborhood that was 10 percent poor lapsed. Most subsequent moves were to less affluent neighborhoods; that is, to settings more like those they initially left than like those into which they originally moved. Many reasons might explain this systematic mobility pattern, but one is surely that families from public housing preferred settings more sociologically like those they already knew. This predicament is most acute for racial or ethnic minority families who are fearful that affluent neighborhoods will tend to be majority White and replete with overt or covert racial prejudice. The MTO data suggest this

possibility, because almost all of MTO's voucher holders from racial or ethnic minority groups moved into nearby affluent minority neighborhoods and not into nearby affluent White neighborhoods.

MTO families randomly offered a traditional Section 8 voucher, despite coming from public housing, probably provide the closest approximation to the behavior of the typical private-market housing family in the Section 8 Program. Families in the MTO Section 8 group were more likely to lease up (62 percent) than families in the experimental group (47 percent). Also, their spontaneous moves were to neighborhoods with fewer nonpoor families (71 percent nonpoor) than those of the low-poverty voucher group (89 percent nonpoor). Again, we can invoke many reasons for such data, but one possibility is that the Section 8 group families voted with their feet in ways that reveal a preference for neighborhoods less affluent than those that MTO's low-poverty voucher required.

What about families in the Section 8 Program who are relatively more affluent and more likely to be White? Will they be as inclined to avoid neighborhoods with poverty rates as little as 10 percent? We do not know, but consider that they started in 2007 in Chicago from a base rate of 29 percent, not 53 percent, poor. Many of those families would therefore live in areas close to 10 percent poor, and for the others, the transition from 29 to 10 percent is less than from 53 to 10 percent. Our speculation is that fewer families in the Section 8 Program than in MTO would want to avoid neighborhoods that are 10 percent poor. This speculation means that scaling up the main MTO finding in the experimental group may be more difficult for most Section 8 Program voucher recipients, who are not as poor or as likely to be racial or ethnic minorities as are those in the MTO population. They might be more likely to want to move into affluent neighborhoods, thus swelling the demand for units in areas where the supply is already limited. Scale-up would be less problematic, of course, if federal authorities issued many fewer new vouchers, or if they somehow managed to impose real constraints on local private housing markets to implement a policy with teeth that encouraged moves into affluent neighborhoods. Currently, neither policy seems likely. It is hard to see, therefore, how MTO's main treatment arm could be scaled up within the Section 8 Program to capture MTO's health results.

Beyond MTO's Biological and Health Measures

HUD did not originally design MTO with a central health focus. That focus emerged as primary halfway through MTO, when it became clear that the anticipated socioeconomic and educational effects were not occurring but that positive mental health effects were. Height and weight measures therefore gained new salience, and researchers added some health and biological measures to the final data collection wave. They obtained positive results for extreme obesity, diabetes, and HbA1c, suggesting a causal pathway between improved glucose regulation and reduced cardiovascular disease. The theoretical link among the three health outcomes, and from there to cardiovascular disease, makes the MTO health findings so credible, as does the fact that each is assessed in a quite different way—by the physical measurement of height and weight, self-report, and dried blood, respectively. Also adding credibility to MTO's health findings is the consistency of the positive mental health findings obtained at both the study's middle and end points, and for females in both their adult and youth years.

Many senior members of the medical research and policy establishments tend not to take social science findings seriously, especially if only self-reported or simple anthropometric assessments are available. They prefer biological measures that are part of well-established medical theories that manifestly predict subsequent serious diseases and are collected from, say, blood, sputum, or urine. They also prefer clinical assessments and cutoff values that are normative among health researchers and policy analysts. Like other scientists, they also seem to take more seriously findings that have a broad rather than a narrow reach. Justifying any housing policy because of its health consequences requires housing researchers to provide knowledge that the medical research and medical policy communities can freely embrace because the knowledge fits within their professional frames of reference. Thus, the concern in housing research on health is to use general clinical diagnoses, demonstrate biological mechanisms, assess clinical disease end points, and be applicable to large populations of individuals.

MTO went a considerable distance along this path, but probably not as far as it would have had it been initially framed as a study of housing and health. Its findings are from a smaller (but on average needier) population than the national population of Section 8 Program voucher holders. Positive findings emerged for a category labeled "extremely obese" but did not statistically replicate for the larger and more commonly used "clinically obese" group, with its lower cutoff value. MTO examined asthma by self-report, but these reports did not vary by treatment group. Although MTO obtained a positive result for health outcomes and the HbA1c biomarker, indicative of improved glucose regulation, much past interest in how physical and social settings affect health has concentrated on immunological pathways that lessen resistance to pathogens and thus promote many kinds of disease, including cardiovascular disease (Adler and Stewart, 2010). Other biomarkers, such as Interleukin 6, C-reactive protein (CRP), and Epstein-Barr 18 Virus, therefore, also require careful examination. CRP was assessed in MTO, and it was marginally related to the low-poverty treatment, raising at least some hope that housing will affect pathways to disease based on regulating immunological and glucose functioning. We need a study of housing mobility that is initially and explicitly focused around causal links from housing to health.

What About Child Health?

The final measurement wave of MTO included not only adults but also youth. These youth were children when MTO began, but few health measures were taken from them as children, so MTO reports only youth results. There was a positive mental health effect for female (but not male) youth and no positive results for either gender for any physical health measure, so MTO's preadult health story is a mixed bag.

There are good reasons, however, for expecting positive health consequences before adulthood. Regular Section 8 Program voucher holders can move to better homes or neighborhoods, increase their discretionary income, or combine both. Each of these options should separately reduce psychosocial stress in the family, and reduced stress is a well-established mediator of improved biology and health in adults and even small children (Adler and Stewart, 2010). It is not, however, the only relevant causal mediator. Also relevant is that the discretionary income a voucher provides in the Section 8 Program can be used to access more and better health services. In addition, after moving, a family might also increase its members' exposure to information and social models relevant to

leading a healthier lifestyle. All three mechanisms—reduced stress, more access to health services, and exposure to healthier lifestyles—should complement each other and promote better biology and health in general, including in children.

Examining child outcomes is especially important because young children are particularly susceptible to many biological changes and illnesses associated with environmental exposures of all kinds. Moreover, if prevented or detected early, some such illnesses can be cured or managed in ways that are beneficial to the child, the family, and the national health dollar. The key here is that young children's biology and health are susceptible to the socioeconomic circumstances of their families (Adler and Stewart, 2010). Using a housing voucher to upgrade housing and increase family disposable income are two forms of socioeconomic upgrading, each of which occurs for most families in the Section 8 Program because they live in private housing when they get their voucher.

Conclusions

MTO is a very important study and was well designed and analyzed for its own primary purpose— to test the consequences of a dramatic shift in the density of neighborhood poverty. The designers of the study never intended it to be an evaluation of the health consequences of the Section 8 Program, the conceptual framework we adopted in this article. Our remarks are not, therefore, critical of the MTO research. Rather, they are intended for all those who might be tempted to take MTO's positive health results and extrapolate them to the Section 8 Program to declare it an empirical success. The MTO team never attempted such an extrapolation, and this article merely cautions those who might want to do so by outlining how the MTO demonstration differs from the Section 8 Program in (1) study population, (2) the size of the neighborhood affluence contrast, (3) the role of supplemental household income as a possible causal mediating mechanism, and (4) the limited supply of affordable rental housing in neighborhoods as affluent as those to which families in the main MTO treatment group moved.

The article also briefly outlines a different study for testing voucher effects in the Section 8 Program writ large. It particularly emphasizes the need for (1) including samples from the national population of Section 8 Program-eligible families; (2) including treatment contrasts that reflect the range of neighborhood and housing unit quality changes the Section 8 Program typically achieves; (3) measuring and analyzing how much discretionary income flows to voucher holders who are already paying private-market rent before entering the Section 8 Program; (4) assessing the supply of, and demand for, affordable housing in the Section 8 Program; (5) measuring more biological and health outcomes than MTO did; and (6) examining biological and health changes in people of all ages, especially children.

Ironically, the evaluation emphasis in this article is somewhat at odds with our own view of social experiments (Shadish, Cook, and Leviton, 1991). MTO sought to be a bold enterprise that transcended the policy concerns of the era when it began, about 20 years ago. That is, it would create a treatment so bold that it could not exist in the world as it is currently socially conceived. Campbell (1969) has championed this conception of bold social experiments and has cautioned against using scarce and expensive experimental resources to test options that are already considered to be policy relevant. After all, good science need not have immediate payoffs, and what is deemed

unreasonable at any one moment in time may be considered feasible later. As we have shown here, a study to assess the effects of the Section 8 Program would involve a neighborhood improvement contrast smaller than what MTO achieved and, in some interpretations at least, this contrast would confound neighborhood change with an increase in disposable income. MTO was bolder and tested a theoretical policy alternative that reached beyond what was then considered a feasible alternative to public housing: locating families in settings considerably more affluent than the somewhat more safe and somewhat less poor, but otherwise not very different, neighborhoods into which they would otherwise have spontaneously moved. There has to be a place for such bold studies in our armamentarium of social experiments, and it is important to us that the present argument not be construed as an advocacy of doing only those social experiments that evaluate current policies like the Section 8 Program. At the national level, we need a both/and strategy: social experiments to examine both bold innovations and current policies. MTO is a great start and has successfully shone the light on health as an outcome of social mobility programs. Now is the time to enrich our understanding of the causal links between housing and health by conducting an evaluation of the less adventurous, but more immediately relevant, Section 8 Program.

Acknowledgments

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Constrained Compliance: Solving the Puzzle of MTO's Lease-Up Rates and Why Mobility Matters

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Abstract

The Moving to Opportunity (MTO) for Fair Housing demonstration provided an opportunity for low-income renters to move to low-poverty neighborhoods. Many of these renters, however, did not move with their vouchers, and many of those who moved did not stay in low-poverty neighborhoods. In this article, we explore the mechanisms behind these residential outcomes and what they mean for housing policy. First, we review evidence suggesting that MTO families wanted to live in low-poverty "opportunity areas." We then describe how some aspects of the Housing Choice Voucher Program, the structural features of the housing market, and the beliefs and coping mechanisms of low-income renters—shaped by years of living in extreme poverty—prevented these families from achieving their goals of residential mobility. Finally, we consider the negative consequences on the life chances of the poor if housing policy does not address constraints to mobility and identify potential policy solutions that might lead to opportunities for low-income renters to live in low-poverty neighborhoods.

Introduction

Across the nation, the Moving to Opportunity (MTO) for Fair Housing demonstration has raised a chorus of "why didn't" responses (for example, see Briggs, Popkin, and Goering, 2010; Sampson, 2012). The loudest question (Clampet-Lundquist et al., 2011; Clampet-Lundquist and Massey, 2008; Ludwig et al., 2008; Sampson, 2008; Turney et al., 2006) has commanded most of the attention: Why didn't MTO have *more* of an effect? Some researchers (Goetz and Chapple, 2010; Imbroscio, 2012) have even claimed that MTO shows that assisted housing mobility programs do not improve life chances. Two more specific questions point more directly to how well the program really worked, however: Why did more people not move with their vouchers, and why did more people not stay longer in their low-poverty neighborhoods?

To these questions, one might reasonably add another: why do we need a program like MTO, which circumscribes where people can lease up, when renters can exercise choice and live where they want via the traditional Housing Choice Voucher Program (HCVP)? The answer to this question, in a nutshell, is the rationale for MTO: without restricting the voucher so initial lease ups occurred in low-poverty neighborhoods, MTO's architects feared that the public housing residents the program sought to serve—who were largely African American or from other disadvantaged minority groups—would not make a move to such neighborhoods on their own. They were right about this concern; perhaps one of the most startling outcomes of MTO is that families in the Section 8 group, who were offered a traditional HCVP voucher rather than the restricted low-poverty voucher offered to families in the experimental group, spent a median of only 9 months in lower poverty neighborhoods (less than 20 percent poor) over the 10- to 12-year window of the study. In contrast, MTO succeeded in helping families in the experimental group spend a median of 36 months in such neighborhoods (the figures for *compliers* in the Section 8 and experimental groups were 24 and 87 months, respectively)—perhaps not as much time as many had hoped, but not an insignificant amount.

Because the U.S. Department of Housing and Urban Development (HUD) was generous enough to allow several qualitative research teams into the field along with the survey researchers, we know a lot about the reasons why families in the two treatment groups might not have made or maintained larger gains in neighborhood quality. These reasons teach us a lot about the very problem MTO was trying to solve: how to get voucher holders into lower poverty neighborhoods with more resources. As we show in this article, this body of research defies the conventional wisdom, which assumes that families did not lease up or stay in opportunity neighborhoods because they did not want to—that they instead preferred a set of neighbors who were more like them. The qualitative research conducted on MTO, other mobility programs, and the voucher population more generally, disputes this simplistic assumption. As we show in the following sections, the opposite was often the case: families who moved with a voucher were often overjoyed by their new neighborhood environments and experienced considerable relief upon leaving high-poverty neighborhoods behind. Over time, however, profound structural and cultural forces shaped families' residential trajectories to diminish the contrast between families in the experimental group and those in the control group.

In 2003 and 2004, just after the MTO followup survey for the interim impacts evaluation (Orr et al., 2003), our team entered the field in Baltimore and Chicago to study MTO adults and youth, using indepth qualitative interviews and neighborhood and classroom observations. We spent time in nearly all of the origin neighborhoods from which the MTO families hailed and many of the neighborhoods to which they moved with the voucher and in which they had ended up by the time we contacted them, 7 to 9 years after random assignment. We conducted another round of intensive qualitative work in 2010 and 2011, after the long-term survey for the final impacts evaluation (Sanbonmatsu et al., 2011). In both rounds, we spoke at length with hundreds of MTO participants, both adults and teens, about their experiences in their origin, placement, and subsequent neighborhoods and the process through which they ended up where they were. Other MTO qualitative teams undertook similar studies in the other three MTO cities: Boston, Los Angeles, and New York City (Briggs, Popkin, and Goering, 2010). These data, along with recent qualitative studies of the residential trajectories of families in other mobility programs and in the traditional Section 8 voucher program, offer powerful lessons about three factors that shape the residential trajectories of poor families: (1) the difficulty of using the voucher to navigate the private housing market in cities highly segregated by income and race, (2) problems with the voucher program itself, and (3) the beliefs and coping strategies—factors that economists might subsume under the label *preferences*—of the families MTO sought to serve. We argue that these forces, taken together, were likely a main cause of depressed lease-up rates (especially for low-poverty voucher holders, but also for the Section 8 group) and of returns to poorer neighborhoods after initial opportunity moves.

Where MTO Families Wanted To Live

The conventional wisdom about why experimental group participants did not lease up or remain in what the MTO program called *opportunity areas* is that they wanted to live with others "like them" or in areas more like the neighborhoods they had moved from. Although some experimental group families might certainly have felt that way, the accounts from our interviews (and from the followup survey for the interim impacts evaluation) do not support this assertion. In fact, families often told us exactly the opposite. For most experimental group participants we interviewed, the contrast between public housing and their MTO placement neighborhoods was hardly lost on them (Turney, Kissane, and Edin, 2011).

Jacqueline said of her origin neighborhood,

> I was living [in] what they call the danger zone. . . . You don't know how bad I wanted to get out of that place. . . . There was shooting and all that. By me living on the corner, all the junkies and all hung right on that corner.

Keisha, a participant in the experimental group, characterized her public housing neighborhood in this way: "It's like they got you in a cage. . . . You are in this hole, where all these people cramped in."

Tammy, a participant in the control group, recounted her time in public housing as follows:

> That was the worst experience that I ever experienced, living in an environment which made you feel trapped, caged, and worthless—just stuck into the atmosphere of absolutely no progress. It was a whole little community of pure dissatisfaction. . . . No one encouraged no one.

These examples are in striking contrast to the way many movers in the experimental group described their new environments to Turney, Kissane, and Edin (2011).

Niecy said she could see grass, birds, and squirrels out the window of her new apartment; she felt she "had moved from night to day."

Amy said the change in the neighborhood's physical environment had dramatically improved her outlook on life:

> You living in a high-rise, you got a lot [of] cement. And there's something to that effect in the psychology…, the hardness you get from all that concrete. The greenery [here], it softens you. It's just so beautiful and peaceful, the space, the open space.

Peaches' account about her new place demonstrates how transformative a new unit in a better neighborhood can be:

> Oh God, when I first moved in…everything was just so neat, clean, and well kept and quiet and peaceful, I was like 'Thank you God. This is what I have been waiting for,' you know? And when I first moved in the house, I just cried. I just really cried. I was like 'Oh my God… Now I can raise my family in the way I want to raise them,' you know?

A Baltimore mover in the experimental group told us that her daughter's asthma disappeared after the MTO move away from the projects in West Baltimore. Some parents who made it to Howard County, Maryland—which has some of the highest performing schools in the area—were impressed with how much attention their children received in school and how their children's behaviors changed as they were exposed to new peers (DeLuca and Rosenblatt, 2010). Drawing from the followup survey for the interim evaluation and the various qualitative studies of MTO, we know of no evidence to suggest that experimental group families did not lease up or stay in opportunity areas because they wanted to live among fewer middle-class or White neighbors (Briggs, Popkin and Goering, 2010; Rosenblatt and DeLuca, forthcoming).

Problems With the Voucher Program and Private Markets in Segregated Cities

MTO's design was based on the best research available at the time. Concentrated poverty is associated with any number of deleterious outcomes for families and children, and the idea of giving families in some of the most highly distressed public housing in the country a chance to move to a low-poverty neighborhood made perfect sense. MTO did not occur in a vacuum, however; rather, a modified version of the Section 8 voucher program, a program that has limitations of its own, launched families on housing searches in metropolitan areas with significant structural barriers to residential mobility. Although MTO offered experimental group members modest counseling to help navigate housing markets (Feins, McInnis, and Popkin, 1997), MTO's architects may have underestimated how weaknesses in the Section 8 voucher program and the power of highly segregated city housing markets would impede the movement of low-income families out of high-poverty neighborhoods. Over time, these same forces also served to bounce opportunity movers out of low-poverty neighborhoods and into neighborhoods that were similar to those of their counterparts in the control group.

Baltimore and Chicago, two of the MTO sites, serve as good examples. In Baltimore, a crisis of available affordable housing arose in the metropolitan area just at the time when low-poverty and traditional Section 8 voucher holders were trying to lease up. The rental market was very tight, and few units with more than three bedrooms were available. Families were subject to all the choice-limiting aspects of the traditional Section 8 voucher program. For example, MTO did not provide relief for the burdensome portability procedures that would have more easily enabled movers in the experimental group to use their vouchers outside the Housing Authority of Baltimore City's (HABC's) jurisdiction. In Baltimore, landlords may have been hesitant to participate because the administrator of the larger voucher program, HABC, had become notorious for late payments and inspection delays. In addition, Baltimore and most of its adjacent counties did not have source-of-income protection that could have prevented landlords from refusing to lease to voucher holders.[1] As in other cities, Fair Market Rent (FMR) levels were tagged to the 40th percentile of the metropolitan area median rent, not to the median rent of a smaller geographic unit, and thus were much less than the median rents in many mostly White and affluent areas in the city and suburbs, further restricting the scope of possible units to which families could move.

Chicago's Cook County, which encompasses Chicago and some of its inner suburbs, does have source-of-income protection, but that did not eliminate all the barriers to lease up there (and the protection does not apply outside of Cook County). Navigating the private housing market was still enormously difficult for voucher holders. In 2002, a 3-year longitudinal study of roughly 100 participants in a new wave of Chicago's Gautreaux program (Gautreaux Two) recruited families at the first orientation session so researchers could observe the process of lease up. Virtually every client wanted to leave public housing behind, but many struggled mightily to secure a unit within program guidelines. Families often visited dozens of units over several months that subsequently failed to qualify, either because they were not in census tracts that met program rules or because they did not pass Section 8 inspection. In the tight housing market of that time, larger families had a particularly hard time leasing up within the prescribed time limit, as did families headed by an adult who was working full time or was both working and going to school. For these busy families, the time and energy involved in mounting a housing search in opportunity neighborhoods, with which they may have had limited familiarity, were simply too much, even with the modest counseling the program provided. Similarly, families with physical or mental health problems also often found the process too onerous (Pashup et al., 2005).

In both MTO and Gautreaux Two, families often moved on from their placement neighborhoods after 1 year, when the voucher became portable, often moving to higher poverty neighborhoods. Retrospective interviews of adults in Baltimore (Rosenblatt and DeLuca, forthcoming) and in Los Angeles, Boston, and New York (Briggs, Comey, and Weisman, 2010; Briggs, Popkin, and Goering, 2010) suggest that unit and landlord problems—everything from units failing their annual inspection to landlords raising the rent to greater than the FMR or failing to respond to a major maintenance problem—played leading roles in prompting these subsequent moves. This finding

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[1] The Public Justice Center in Baltimore contacted 42 apartment complexes located in areas of adjacent Baltimore County that have few voucher holders. In 34 of the 42 complexes, amounting to 12,000 total units, property managers reported that they did not accept vouchers. Some of the same owners, however, do accept vouchers in complexes located in predominantly African-American or lower income neighborhoods (Samuels, 2012).

is consistent with the prospective research on the experiences of families who moved through Gautreaux Two; one-half moved at the 1-year point, with unit and landlord problems cited as the leading reasons prompting the moves. Fieldworkers often witnessed these unit problems first hand—leaking roofs, ill-fitting windows that let in moisture and cold, broken plumbing or heating systems, and rodent infestation— as well as conflicts with landlords (Boyd et al., 2010).

In particular, the unit problems were often quite severe, suggesting that, when used in a neighborhood that qualified as an opportunity area, the voucher often afforded a family units and landlords at the very low end of the quality scale. Wood's (2011) indepth interviews with African-American participants in Baltimore's Section 8 voucher program showed that voucher holders were very sensitive to the fact that poorer neighborhoods with lower rents often offer more "bang" in terms of unit quality and size for the voucher "buck" than less poor, less African-American neighborhoods. DeLuca, Wood, and Rosenblatt (2011), who studied African-American voucher holders in Baltimore and Mobile, Alabama, found that the program limitations on time to lease up (in general, 60 days) conditioned families' choices when moving. When units failed physical inspection, or when landlords failed to respond to maintenance requests or raised the rent, families under pressure often took the first or second unit they looked at, afraid they would lose their voucher or even end up homeless. These dynamics led them to move to mostly poor and racially segregated neighborhoods, where qualifying units are more plentiful and landlords are typically more eager to take a voucher.

World Views and Preferences

The MTO program left families to choose their own units and neighborhoods as long as they met the criteria of being located in a census tract that was less than 10 percent poor. After 1 year, the low-poverty voucher became fully portable; it could be used in any neighborhood. MTO's design, however, was predicated on the idea that the housing authority could not leave mobility decisions up to choice alone—at least not at the outset—because participants would be unlikely to move to an opportunity area on their own. To this end, each site engaged in some level of housing counseling to aid clients as they tried to lease up in low-poverty neighborhoods, but no ongoing counseling was available to help families stay in these neighborhoods when a unit failed annual inspection, the landlord raised the rent or was unnecessarily intrusive, or broken pipes spilled sewage into the basement (a condition we observed more than once).

Over and above the considerable structural barriers outlined previously, a surprising set of preferences governed these subsequent moves—preferences honed over years of living in distressed public housing located in some of America's poorest and most dangerous neighborhoods. Again, these preferences were not what readers might think: both our interviews, mentioned previously, and the followup survey for the interim impacts evaluation showed that families seldom left low-poverty neighborhoods because they found such communities uncomfortable or disliked the economic mix or racial diversity there (Briggs, Popkin, and Goering, 2010; Rosenblatt and DeLuca, forthcoming). To the contrary, in other mobility programs such as Gautreaux Two, movers often spontaneously cited race and class diversity as something they enjoyed and were reluctant to leave behind (Boyd et al., 2010). Nor was the desire to live near kin always, or even often, a major factor; in fact, sometimes people wanted to get away from the "needy" ties of their families (Boyd et al., 2010;

Briggs, Popkin, and Goering, 2010; Rosenblatt and DeLuca, forthcoming). Wood's (2011) sample of Baltimore low-income renters, including voucher holders, held strong notions of what constitutes a suitable place to raise children: a private entrance to increase safety, a multilevel unit to manage noise and ensure a quiet place for children to sleep and do schoolwork, a basement for extra room and to house older (especially male) children who may be a deleterious influence on younger children, and a fenced-in back yard so children could play safely outdoors without extensive monitoring. These unit considerations usually trumped neighborhood and school considerations. Retrospective interviews from MTO movers reflected some of the same themes (Rosenblatt and DeLuca, forthcoming).

DeLuca, Wood, and Rosenblatt (2011) and Rosenblatt and DeLuca (forthcoming) detailed the psychological and practical coping mechanisms that shape such residential decisions among voucher holders. On the psychological side, respondents who moved on to high-poverty neighborhoods often engaged in *telescoping*—defining the neighborhood as the block face and seeking visual evidence that the unit was at least on a good block, regardless of the surrounding neighborhood's quality—and typically adopted the belief that the quality of the unit was more important for family well-being and child development than neighborhood surrounds ("we live in here, we don't live out there"). On the practical side, they "kept to themselves"—avoiding contact with neighbors who might bring trouble and restricting the children to playing indoors, strategies that proved less effective for teens than for younger children. In short, their expectations about the quality of their neighborhoods were often quite low, and they believed that by keeping to themselves, they could ensure their children's well-being.

Similarly limited expectations for their children's schools were also pervasive in the interviews with parents in the experimental group who moved on; many mothers told us they believed that good schools were those that required uniforms and had security guards (qualities that made perfect sense in the chaotic schools we visited in the course of our research). Other parents told us that the school environment was irrelevant and that their children's efforts were a more important determinant of their academic success. After their own experiences coming up in low-performing urban schools, these parents believed that their children could make their own way, just like they could survive unsafe neighborhoods (DeLuca and Rosenblatt, 2010).

When combined with the structural difficulties of obtaining affordable housing in opportunity areas, these strong beliefs about ideal home environments for raising children, along with a powerful set of adaptive coping skills that reduced expectations about what constituted suitable neighborhoods and schools, were probably strong enough forces to draw families in the experimental group back into higher poverty neighborhoods over time. These forces also likely kept those families in the Section 8 group and those in the control group who left public housing via a voucher from using the vouchers to move to very low-poverty neighborhoods.

Previously, we used the word *preferences* to describe these beliefs and coping strategies. This terminology, however, oversimplifies the process and implies freedom of choice. Instead, the preferences this body of qualitative work has revealed have been honed by years of enduring discrimination and significant neighborhood adversity. To make our point, we turn to the way courts perceived "freedom-of-choice" plans after *Brown* v. *Board of Education*. In a subsequent case in 1968, *Green* v. *County School Board of New Kent County*, the court struck down freedom-of-choice plans because

they placed too much of the burden of integration on the families, many of whom were too intimidated to choose the White schools in the county. Other legal scholarship refers to the idea of *tainted choice*—choices that are restricted because of previous discrimination (Gewirtz, 1986). In the case of MTO, many families—at least in Baltimore and Chicago, where participants were almost entirely African American—had been living for generations in neighborhoods that were heavily segregated (often by design). In the face of more information and a more diverse range of previous experiences, their preferences might have been different. Indeed, the Gautreaux Two study showed that families who had recent experience living outside of public housing were more likely to persist in opportunity areas (Boyd et al., 2010), and studies of Chicago's original Gautreaux program found that families reported more positive assessments of low-poverty White neighborhoods after having lived there (Rosenbaum, DeLuca, and Tuck, 2005). One experimental group mover told us—

> If I had not had that opportunity to go into the MTO program, I would not have known what it would have been like to live in a house in a positive environment—to see how middle class people live…. It just made me want that. (DeLuca and Rosenblatt, 2010: 1468)

Sticking to the Status Quo?

What happens if families do not have an opportunity to experience lower poverty, less segregated, safer communities? Children's educational outcomes provide a good example. Recent observational studies suggest that neighborhood context can have profound effects on the cognitive development of young children. Burdick-Will et al. (2011) noted that, despite a lack of educational effects for the five-city study, experimental group families' neighborhood changes in Baltimore and Chicago did improve children's reading scores. In a nonexperimental study, Sampson, Sharkey, and Raudenbush (2008) compared the verbal cognitive ability of African-American children living in Chicago neighborhoods of concentrated disadvantage to that of African-American children with nearly identical individual characteristics and family backgrounds who live in Chicago neighborhoods with higher socioeconomic status. They found that living in a neighborhood of concentrated disadvantage reduces the verbal cognitive score of African-American children by 4 points, the equivalent of missing 1 year of schooling. They also found that this effect develops over time, emphasizing the durable role of neighborhoods in children's early development. Wodtke, Harding, and Elwert (2011) also showed that negative effects of living in neighborhoods of concentrated poverty accrue over time and that sustained exposure can have devastating effects on the odds of graduating from high school.

Burdick-Will et al. (2011) identified two explanations for why some recent observational studies found more consistent evidence that neighborhoods matter for educational outcomes, whereas MTO found only limited evidence from two sites. First, it seems that changing a child's neighborhood context matters more for children in the most disadvantaged neighborhoods. Second, and most striking, exposure to violence helps explain achievement differences across neighborhoods (see also Sharkey, 2010). Families in the experimental group in the Chicago and Baltimore sites left neighborhoods that were both more disadvantaged and more violent at baseline than at other sites, and as a result, their children benefited in school after these moves.

Neighborhoods of concentrated poverty negatively shape children's outcomes, such as education, and the fact that these dynamics stand the test of time, across generations, is even more remarkable. Sharkey (2008) found that children who grow up in very poor neighborhoods live in similar neighborhoods as adults, and the cumulative neighborhood contexts of multiple generations can affect children's outcomes. Sharkey and Elwert (2011) found that the neighborhood contexts in which parents grew up have a nearly equal effect on children's cognitive abilities as that of the children's own neighborhood contexts. These findings suggest that the stakes of changing a family's neighborhood are high; neighborhood contexts matter not only for parents and children, but for the children's children as well. Research shows that these dynamics are especially acute for African-American children, whose parents are much less likely to move to a low-poverty neighborhood via a voucher than their White counterparts, as we describe in the following section.

The Cost of Doing Nothing

In sum, despite MTO's substantial early success—MTO managed to move one-half of the families in the experimental group to very low-poverty neighborhoods—a wide array of problematic program features and structural forces of highly segregated housing markets, together with underlying beliefs and coping strategies predicated on years of living in highly distressed neighborhoods, may have spelled disaster for the longer term neighborhood attainment of participants in the two treatment groups. These dynamics limited choice so as to funnel families in the experimental group into higher poverty neighborhoods over time. The behavior of families in the Section 8 group— who at median spent *not a single month* in a very low-poverty neighborhood (less than 10 percent poor) and only 9 months in a neighborhood that was less than 20 percent poor over the duration of the study—is further testament to this constrained choice. Evidence from the larger HCVP suggests that the dynamics affecting low-poverty and traditional voucher holders, who are largely African American or another minority, may be felt among African-American voucher holders nationally—they seldom lease up in neighborhoods with lower poverty rates, despite the fact that, given the voucher, they can theoretically afford to do so (Devine et al., 2003). These trends are part of what motivated MTO in the first place. In the absence of mobility programs, minority voucher holders rarely manage to leverage the subsidy to access neighborhoods of opportunity, leaving them uniquely disadvantaged by the program (DeLuca, Garboden, and Rosenblatt, forthcoming).

Exhibit 1 shows this striking pattern for the Chicago voucher program. In Cook County in 2000, 36 percent of African-American voucher holders lived in neighborhoods with poverty rates of more than 30 percent, and more than 60 percent lived in tracts where at least 20 percent of the residents were poor. Within Chicago city limits, these figures are even higher: nearly 50 percent of African-American voucher holders lived in neighborhoods with poverty rates of more than 30 percent and more than 75 percent lived in neighborhoods with poverty rates of more than 20 percent. Residential racial segregation among voucher holders is also extremely high. Nearly two-thirds of African-American voucher holders lived in neighborhoods composed of 90 percent or more African-American residents; conversely, nearly two-thirds of White voucher holders lived in areas with less than 10 percent African-American residents. None of these figures have improved over time: a slightly higher proportion of African-American voucher holders were concentrated in poor, minority-dominated neighborhoods by 2008 (authors' own calculations based on HUD, 2008, 2000, and on 2000 census and 2005–2009 American Community Survey data).

Exhibit 1

Distribution of African-American Voucher Holders by Neighborhood Poverty Rates, Chicago and Cook County, Illinois, 2000
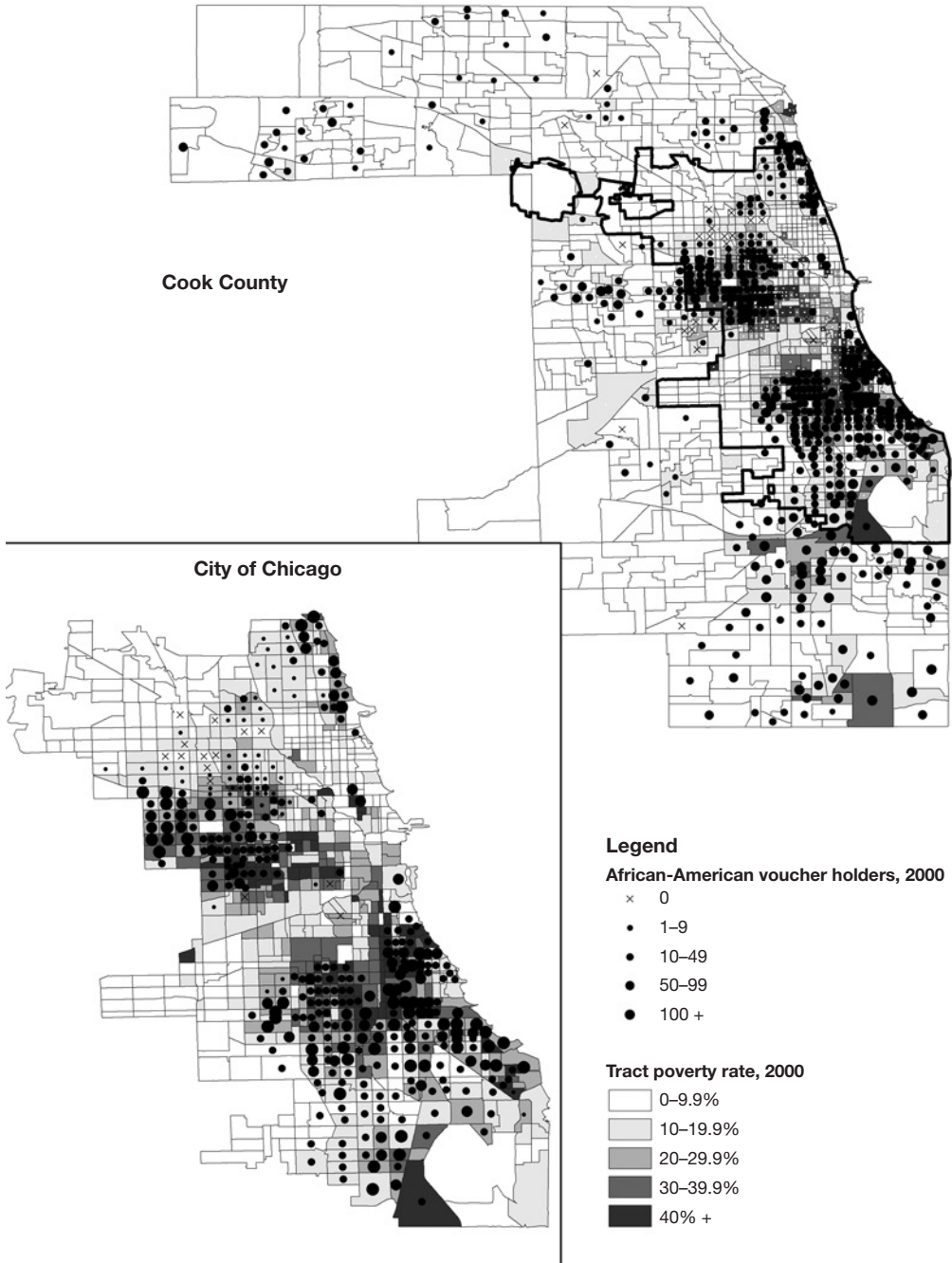
Note: Tracts with no symbol have fewer than 11 total vouchers; thus, data on the racial composition of voucher holders are not available.

Sources: HUD (2000); Neighborhood Change Database

HUD is currently at a crossroads; given the results of MTO, should it fund or reward public housing authorities for engaging in mobility programs? Although most of the HUD budget goes toward tenant-based rental assistance in the form of housing vouchers, the HCVP alone does not facilitate mobility to low-poverty neighborhoods. Currently, a number of HUD's new initiatives (for example, Choice Neighborhoods) are devoted to the opposite philosophy—to infuse poor communities with more resources through redevelopment, services, and subsidies; virtually no new efforts explicitly encourage the kind of assisted mobility that poor families need to access less poor, more integrated communities with better schools. We know of no hard evidence, however, that these in-place strategies actually work to improve family and child outcomes. Meanwhile, significant circumstantial evidence from quasi-experimental research (reported previously) and from the reanalysis of data on educational attainment among children in the Chicago and Baltimore MTO sites (Burdick-Will et al., 2011) suggests that continuing to subsidize units in high-poverty, low-resource neighborhoods with federal housing dollars could do children long-term, serious harm. Because African-American voucher holders are especially subject to these forces, African-American children may be especially harmed. The previously described research, and both quantitative and qualitative analyses of MTO, point to mental and physical health impairments that both youth and adults suffer when trapped in violent, high-poverty neighborhoods (see Sanbonmatsu et al., 2011; Turney, Kissane, and Edin, 2011), findings that speak volumes about the level of the damage such neighborhoods can wield. This situation introduces a puzzle: why are policymakers now more willing to back community development, which has virtually no empirical track record of improving family life, than mobility programs, which have moved the needle on several important outcomes? Should we not at least include mobility approaches in the package of resources poor renters can access?

The first rule of policy ought to be to do no harm. Paying the rent on a unit in a neighborhood with toxic rates of violence, deplorable schools, and weak institutional resources is simply a poor use of federal dollars in our view, when results from MTO and other programs, such as the original Gautreaux, show us that it is possible to get voucher holders to move to, and even stay in, better neighborhoods (DeLuca and Rosenbaum, 2003; Keels et al., 2005). HUD has any number of tools at hand to accomplish this goal: mobility counseling that eases access to lower poverty neighborhoods; smaller area FMRs that can eliminate the bang for the buck problem Wood (2011) identified among low-income renters, including voucher holders; streamlining portability procedures; and pushing for source-of-income protection in more metropolitan areas. The government may not be able to save every family from the potential harms of high-poverty neighborhoods, but it can at least make sure that government programs are not putting people in harm's way. While we wait for proven, in-place strategies, HUD should support local housing authorities in ways that ensure that mobility is a central goal.

Acknowledgments

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Increasing the Value of MTO Research for Housing Policy Development

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Abstract

The Moving to Opportunity (MTO) for Fair Housing demonstration has estimated the effects of two concrete reforms of low-income housing policy on one important group those reforms affected. Reforms of this type have received, and will continue to receive, serious consideration in housing policy debates. At this stage, estimating the effects of the MTO reforms on all the people they affected significantly is not feasible. Estimating the effects of two similar reforms that would have almost the same effect on the families studied in MTO, however, is feasible and desirable. These reforms would have no effect on the number of families who receive housing assistance. Instead, they would affect the nature of the housing assistance offered and the taxpayer cost of providing the assistance. One alternative reform would almost surely have generated cost savings and additional revenue exceeding the cost of the vouchers, thereby providing greater benefits than the current system at a lower taxpayer cost. The MTO results supplemented with estimates of the taxpayer cost of either reform would provide a reasonably comprehensive analysis of its effects. This article suggests how to estimate the taxpayer costs of the alternative reforms. Doing so would significantly increase the value of MTO research for housing policy development.

Introduction

The primary purpose of the Moving to Opportunity (MTO) for Fair Housing demonstration was to study the effects of better neighborhoods on a wide range of individual outcomes. However, it also produced results that are relevant for assessing the likely effects of major reforms of low-income housing policy. Literally, the final impacts evaluation (Sanbonmatsu et al., 2011) reported the effects of offering housing vouchers to families with children living in subsidized housing projects in census tracts with high poverty rates as opposed to the effects of alternatives that would have been available to these families in the absence of this offer.

Political leaders have proposed similar reforms, and some have been implemented. The Clinton Administration proposed comprehensive legislation for phasing out project-based assistance (HUD, 1995) and, in his campaign against President Clinton, Robert Dole also proposed vouchering out public housing. Although the Clinton proposals were not adopted, the *Quality Housing and Work Responsibility Act of 1998* (Title V, Public Law 105-276) mandated demolishing public housing projects and providing housing vouchers to their residents under certain circumstances and allowed these actions under other circumstances. Furthermore, HOPE VI grants and other sources have funded the redevelopment of many public housing projects in census tracts with high poverty rates, and the families whose projects have been demolished have almost always been offered housing vouchers.

Although MTO has produced evidence on important effects of its reforms on members of the families offered vouchers, this evidence is inadequate to judge the desirability of the reforms because MTO also affected taxpayers and the families who replaced MTO voucher recipients in subsidized housing. To maximize the benefit from MTO's enormous investment in data collection, it is important to supplement the results in the final impacts evaluation with evidence on the effects on other individuals.

This article argues that, at this stage, producing the information needed to estimate the effects on all groups of the exact reforms MTO implemented is not feasible, but that the MTO results are applicable for assessing several similar reforms whose primary effects on other people could be estimated with reasonable effort. MTO provided substantial benefits to the families who moved into the public housing units vacated by those who accepted an MTO voucher, but it did not collect information relevant for assessing the effects of the reform on this group, and it is not feasible to assemble it now. It is feasible, however, to estimate the effects of two similar reforms that would have almost the same effect on the families studied in MTO.

The alternative reforms would have made the same voucher offer to the same families as MTO. They would have differed primarily regarding what was done with the units vacated by the public housing tenants who accepted the offered vouchers. Under one option, the units vacated by families with MTO vouchers would have been rented at market rates. Under the other option, buildings with a number of units equal to the number vacated by MTO voucher recipients, plus a fraction of the previously vacant units in the projects involved, would have been demolished and the land sold to the highest bidder. Under both alternatives, when the initial recipients give up their vouchers, those vouchers would be offered to occupants of subsidized housing projects in high-poverty census tracts. Therefore, unlike MTO, these reforms would have had no effect on the number of families who received housing assistance. They would instead have affected the nature of the housing assistance offered and the taxpayer cost of providing it. Evidence on the cost-effectiveness of different types of housing assistance strongly suggests that the second reform would have substantially reduced taxpayer cost and hence would have benefited both voucher recipients and taxpayers.

MTO research focuses heavily on the effects of the experimental group treatment that required voucher recipients to live for the first year in a census tract with a 1990 poverty rate of less than 10 percent. Because no Congress is at all likely to change the voucher program to limit participation to families willing and able to live in a low-poverty neighborhood, the MTO results on the effects of offering traditional Section 8 housing vouchers are more important for an analysis of the reforms under consideration. The points made in this article, however, are equally applicable to the low-poverty voucher option.

MTO Housing Policy Reforms

For purposes of this article, certain features of the MTO reforms are especially important. Under MTO, tenants from the public housing waiting list filled the public housing units that MTO voucher recipients vacated. Public housing authorities (PHAs) received the same operating and capital subsidies and HOPE VI grants to redevelop some of their projects that they would have received in the absence of MTO. Therefore, MTO did not affect the number of families in public housing units or their taxpayer cost. Indeed, it did not affect the budget of any other low-income housing program. It provided additional subsidies to serve the families who accepted the MTO voucher offer to leave their public housing units.

Feasibility of Comprehensive Analysis of MTO Housing Policy Reforms

The MTO final impacts evaluation indicated many beneficial effects and a few unintended negative consequences of the two reforms for the individuals in families who were offered and used MTO vouchers. Because it was not designed to provide a comprehensive analysis of the reforms, the report is silent on the reforms' effects on two other groups—namely, taxpayers and the additional families who received the housing assistance that would have gone to MTO voucher recipients in the absence of the demonstration.

Because MTO provided additional subsidies to serve the families who accepted the voucher offer, the reforms had a cost to taxpayers. Using MTO administrative data to determine this cost in every year would surely be possible. The additional taxpayer cost of assisting the Section 8 group was the cost of providing its members with MTO vouchers. For the experimental group, the cost of mobility counseling and search assistance would be added to the cost of their vouchers. After initial lease up, some voucher recipients relinquished their vouchers to move to a publicly or privately subsidized project. Because MTO did not affect the budgets of existing low-income housing programs, however, the assistance that treatment families received from these programs is not an additional taxpayer cost.[1]

It is very important to recognize that the additional taxpayer cost provided benefits not only to families who accepted MTO vouchers but also to the additional families who replaced those with MTO vouchers in existing programs. MTO did not collect information relevant for assessing the effects of the reform on the latter group, and this omission is important from the viewpoint of assessing the consequences of the reforms. Immediately after the initial lease up, these additional families were about as numerous as the families who used the offered vouchers. They replaced the public housing tenants who used MTO vouchers. Furthermore, the benefits to these additional families could easily have been larger than the benefits to those who used MTO vouchers, because almost all of them came from unsubsidized housing. Initially, the net benefit to the families who

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[1] Taxpayers who care about the voucher recipients also benefit from the reforms. Because of our inability to estimate these altruistic benefits, comprehensive empirical analyses of the benefits and costs of the reforms are not possible. These benefits should not be forgotten, however, in assessing the desirability of the reforms.

replaced MTO voucher recipients in public housing may have been modest, because they moved to public housing projects in bad neighborhoods and often in poor condition. The immediate benefit may have resulted primarily from spending less on housing and hence consuming more of other goods. A substantial fraction of these families ultimately received larger benefits, however. About 42 percent of units in MTO housing projects were demolished under HOPE VI or some other redevelopment initiative before data collection for the final impacts evaluation. Their occupants were offered the choice between a housing voucher and a vacant unit in another (likely better) public housing project. By the time of final data collection, about one-fourth of the members of the control group (to whom the MTO demonstration did not offer vouchers) had housing vouchers. By this time, about the same fraction of the families who replaced the MTO voucher recipients at the outset of the experiment almost surely had them. Because MTO did not collect the relevant data, it is not possible at this stage to estimate the effects of the MTO reforms on the many additional families who received housing assistance in existing programs as a result of the MTO reforms.

Alternative Reforms

Although it is not possible to conduct a comprehensive analysis of the MTO reforms implemented, it is feasible to produce a broader analysis of the effects of two similar reforms that would have yielded almost the same outcomes for members of the experimental and Section 8 groups. The MTO results would be part of a comprehensive analysis of these reforms.

Unlike the MTO reforms, both alternative reforms are designed to serve the same number of families as the current system. They would have made the same offer to the same families living in the same housing projects. Other subsidized households, however, would not have filled the units vacated by families who used the offered voucher. Under the first alternative reform, households paying market rents would have occupied those units. The second reform would have demolished buildings in subsidized projects with a number of units equal to the number vacated by MTO voucher recipients plus a fraction of the vacant units in the projects involved, sold the land to the highest bidder, and reduced the public housing budget by the amount that would have been spent on these units with a continuation of the current system.

The public housing parcels sold under the second alternative would have been a subset of those that were redeveloped with HOPE VI grants and funding from other sources during the course of MTO. In essence, selling these parcels would be an alternative use of some, but not all, of the properties that were redeveloped during MTO. As mentioned previously, about 42 percent of public housing units involved in MTO were demolished and replaced with new housing before data collection for the final impacts evaluation. The MTO results suggest that if all public housing tenants living in projects in high-poverty neighborhoods had been offered traditional Section 8 vouchers, only 15 percent of the families would have used them—about 25 percent of eligible families enrolled in MTO and 61 percent of enrollees in the Section 8 group used the offered vouchers. Therefore, the number of units that would have been sold under the second alternative is much less than the number redeveloped during MTO.

Under the second alternative, the public housing properties would have been sold at the earliest reasonable time after the departure of families with vouchers. In almost all cases, this would have happened earlier than redevelopment occurred in the MTO projects. This alternative would have avoided expenditures on operating these units under the status quo until their redevelopment and generated the revenue from selling them sooner. It would also have avoided leaving vacant for long periods many additional units in public housing projects that would eventually be redeveloped. The accelerated demolition would almost surely have benefited some and harmed other residents of these housing projects. Because it affects only the timing of the demolition of their units, however, the magnitudes of these benefits and costs arguably would be modest.

Because so many units in the housing projects redeveloped under HOPE VI were not available for occupancy due to their failure to meet the program's minimum housing standards, it would have been possible to demolish buildings with many more units than the number of families that accepted MTO vouchers without reducing the number of assisted households. Popkin et al. (2004) reported that about one-third of the units in projects redeveloped under HOPE VI were vacant at the time of the grant award. Demolishing buildings with more units than the number occupied by voucher recipients and selling the land would have generated additional revenue to reduce the taxpayer cost of assisting the same number of families.

To continue to assist the same number of households over time under either alternative reform, something must be done to replace the initial voucher recipients who give up their vouchers. By the time of data collection for the final impacts evaluation, only 45 percent of households in the Section 8 group continued to receive vouchers. To offset attrition among the initial voucher recipients, the alternative reforms would have offered vouchers to households that moved into the same housing projects after the initial voucher recipients departed or who lived in housing projects in census tracts with only slightly lower poverty rates. Those households would have benefited and taxpayers would have incurred additional costs on their behalf. We could use the MTO data to estimate these magnitudes because the recipient benefits and taxpayer costs should be about the same as for initial voucher recipients with the same observed characteristics.

Both alternative reforms would have generated additional revenue to offset the cost of the vouchers. For the first alternative, the additional revenue would have come from the new public housing tenants who pay market rents. For the second, it would have come from selling public housing land. The second alternative also would have led to considerable cost savings. It would have avoided the cost of operating some public housing projects before their redevelopment and the considerable construction cost of HOPE VI redevelopment.[2] Indeed, as mentioned in the final impacts evaluation and discussed in more detail later in this article, the second alternative would almost certainly have generated cost savings and additional revenue exceeding the cost of the vouchers. In this case, the cost of providing the benefits to families who used vouchers would have been negative; that is, the reform would have provided greater benefits than the current system at a lower taxpayer cost.

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[2] Turner et al. (2007) reported a construction cost of $160,400 per dwelling unit for the 192 HOPE VI projects initiated before 2004.

Applicability of the MTO Results to the Alternative Reforms

The alternative reforms might have had somewhat different effects on the studied outcomes than the MTO reforms. Because they would have affected the desirability of remaining in public housing to some extent, the alternative reforms would have affected treatment group members' decisions about whether to remain in public housing and, hence, the MTO results would be less applicable. This section argues that these differences likely would have been very small.

Under the MTO reform, families from the top of the public housing waiting list initially occupied units vacated by families with MTO vouchers. Under the first alternative reform, they would have been rented at market rents, that is, to the households willing to pay the most for them. Therefore, the alternative reform would have led to a somewhat different set of neighbors for each subsidized family in public housing than the MTO reform. However, because the vacated units are in bad neighborhoods and usually in bad condition, the families willing to pay the most to live in them would surely have had very low incomes, like the families on public housing waiting lists. Furthermore, only 15 percent of the units would be rented at market rents. The differences in the characteristics of the families who would occupy public housing units under MTO and under the first alternative reform are likely to be modest. They would have little effect on families' decisions about whether to use the offered voucher.

In deciding whether to use the MTO voucher, a farsighted eligible person would consider the future housing assistance that would be available later if he or she remained in public housing. This consideration, however, would not lead to different decisions under the alternative reforms compared with under MTO because the future options are about the same under all reforms. For example, many families offered vouchers under MTO lived in projects that were subsequently demolished, and their occupants were offered the option of a housing voucher or a vacant unit in another public housing project at that time. The same options would have been available under the first alternative reform. This reform assumes the same pattern of public housing redevelopment. Its only difference from the MTO reform is that enough occupants of public housing units pay market rents so that the voucher offer does not affect the total number of assisted households.

The MTO evidence is unambiguously applicable to a version of the second alternative reform that retains the same timing of the demolition of public housing projects as occurred under MTO. If the same projects would have been demolished at the same time under either reform, the effect on the occupants of these projects would have been essentially the same. Public housing tenants would have had the same options at each point in time. In this case, the MTO results should provide excellent estimates of the effects of the second reform on the families offered vouchers.

If the projects had been sold at the earliest feasible time after voucher lease up, occupants of the projects sold would have been offered the option of a housing voucher or a unit in another public housing project earlier than under MTO. MTO evidence would be somewhat less applicable to this reform because the accelerated demolition of public housing projects would make staying in public housing more attractive to some families in the two treatment groups and less attractive to others. For the reasons mentioned previously, however, these changes in the desirability of remaining in public housing are likely to be small, and the bias in the MTO estimates of effects on the individuals in families initially offered vouchers would be modest.

In short, the two alternative reforms should have about the same effects as the MTO reform on families living in public housing projects in high-poverty census tracts who are offered vouchers, so the MTO evidence applies to the effects of the alternative reforms on these families.

Estimating the Costs to Taxpayers of the Alternative Reforms

As explained previously, the MTO data could be used to estimate the taxpayer cost of the vouchers offered to households under the alternative reforms. This section addresses the estimation of the additional revenue and cost savings from the alternative reforms that would be available to offset the cost of these vouchers. Existing evidence on the relative cost-effectiveness of public housing and housing vouchers gives us good reason to expect that the former exceeds the latter for the second alternative reform. That is, the reform would have achieved benefits to voucher recipients at a lower taxpayer cost than the current system.

The first alternative reform would have charged market rents for the public housing units vacated by the families who accepted the initial voucher offer. Therefore, it would have generated additional revenue that could have been used to defray a part of the cost of the vouchers. The additional revenue is the difference between the market rents of the units vacated and the rents that the voucher recipients would have paid for them. MTO administrative data on voucher recipients could be used to estimate the latter. Market rents of the public housing units might be predicted in one of two ways. Since the early 2000s, PHAs have been required to estimate the market rents of their units to give their tenants a choice between a flat rent and an income-based rent. To the best of my knowledge, no one has ever studied the accuracy of these estimates. Nevertheless, they could be used for the years available, and the earliest available PHA estimate could be adjusted for real depreciation and inflation to produce estimates for earlier years. Alternatively, the data on MTO voucher units could be used to estimate a hedonic relationship between the rents paid to landlords and the characteristics of the dwelling units and their neighborhoods, and this estimated equation could be used to predict the market rents of public housing units. Previous research has indicated that the rents paid to landlords of voucher units are very close to the rents of unsubsidized units with the same characteristics (Leger and Kennedy, 1990; ORC Macro, 2001; Wallace et al., 1981; Weinberg, 1982).

The second alternative reform would have generated substantial cost savings and additional revenue compared with the MTO reform. The cost savings is the money that was spent to operate and redevelop the public housing projects that would have been sold under the second reform. Data on these magnitudes are or were in PHA records. However, some considerable assembly may be required, and it may be necessary to impute values for the early years because records have been discarded. Because the new public housing built under HOPE VI and other public housing redevelopment initiatives during the years of the MTO study will provide benefits beyond the time of data collection, the entire cost of this redevelopment should not be subtracted from the cost of the vouchers during this period. Applying an interest rate to the redevelopment cost yields a simple approximation of the cost savings in each year.[3]

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[3] Olsen (2009) suggests a more refined approach.

Selling the projects would have generated the additional revenue. Although many of the structures have little or no market value, the land often has great value. Many projects are located near the center of large metropolitan areas. Because of their proximity to employment, their land would often command a high price if only for its value in future development. Large parcels are particularly valuable. Many public housing sites are in or near gentrifying areas (Holin et al., 2003; Popkin et al., 2004). In gentrifying areas, the highest bidder for vacant land might be a developer of an upscale condominium complex to house people who work downtown and want to avoid lengthy commutes to their jobs. Or the highest bidder might be the developer of a retail store that would serve these people or the developer of an office building. Over time, business uses of land in central locations of metropolitan areas have squeezed out residential uses. In areas that are not currently experiencing gentrification, the highest bidder for vacant land might be an entity that will hold it for future use. In either type of area, the highest bidder might be the developer of a low-income housing tax credit project. The tax credit program provides a substantially higher subsidy to developers of projects in census tracts with poverty rates in excess of 25 percent. Estimation of the market value of the land could be based on the sales prices of nearby properties whose structures were demolished in order to convert the land to another use, and on the cost of demolishing the public housing project (Dye and McMillen, 2007; Weber et al., 2006).

Evidence on the performance of low-income housing programs suggests that the cost savings and additional revenue resulting from this reform would have been substantial. The best study that compares the cost-effectiveness of public housing with that of housing vouchers pertains to public housing projects built before 1975. More than 75 percent of public housing units had been completed by that time. The results imply that the excess total costs of public housing compared with those of housing vouchers for equally desirable housing in the two cities studied (Phoenix and Pittsburgh) were 64 and 91 percent, respectively, and the excess taxpayer costs were 97 and 150 percent, respectively (Mayo et al., 1980).

As with all the best cost-effectiveness analyses of housing programs, this study compared the total cost of providing the housing under each program, with estimates of their market rents based on detailed information about the characteristics of the units and their neighborhoods. For tenant-based vouchers and certificates, the approach is straightforward because all the costs associated with providing the housing during a period occur in that period and they are all in the records of the administering agency. Dealing with project-based assistance is more difficult because the time path of costs bears no particular relationship to the time path of the market rents of the units and all project-based assistance involves indirect costs that are not in the records of the administering agency. The ideal measure of cost-effectiveness for project-based housing assistance is the ratio of the present value of the rents paid by tenants and all direct and indirect costs incurred by federal, state, and local governments to the present value of the market rents of the units over the period that the units house subsidized families. If a government owns the project at the time that it stops housing subsidized families, the present value of the project's market value at that time should be subtracted from the present value of the costs.

A U.S. General Accounting Office study based on a less complete accounting of the cost and much less information about the housing provided found that the HOPE VI public housing redevelopment program was the least cost effective of the active construction programs, with a 27-percent

excess total cost compared with the cost of housing vouchers (GAO, 2001). This estimate clearly understated the total cost of providing housing in the redeveloped projects because it omitted the opportunity cost of the land and the cost of preparing the site from the cost of HOPE VI projects.

General Equilibrium Effects

Because of the possibility of general equilibrium effects, Sanbonmatsu et al. (2011) were reluctant to claim that the results would apply to a national policy of offering vouchers to all families with children living in public housing projects in census tracts with poverty rates exceeding 40 percent. Some simple statistics suggest that these effects are likely to be minimal.

First, the results would surely have been very similar in the metropolitan areas involved had the experiment been expanded to all public housing in the metropolitan area rather than limited to public housing in the central cities of these areas. According to the U.S. Department of Housing and Urban Development's (HUD's) *Picture of Subsidized Households* (HUD, 2008), the central-city PHAs in these areas accounted for 88 percent of all public housing in their metropolitan areas. They surely accounted for an even higher fraction of all public housing units in census tracts with poverty rates exceeding 40 percent. Therefore, the MTO results already reflect any general equilibrium effects of the policy initiative to a considerable extent.

Second, the reforms affected a part of the market that was too small to have any significant general equilibrium effects. According to HUD (2008), about 1.07 million households are living in public housing units and 37 percent are female headed with children. Data on MTO families indicate that 97.8 percent of the demonstration's families with children are female headed. Under the assumption that this rate holds true for all families in public housing, about 37.8 percent of all families in public housing have children. Newman and Schnare (1997) estimated that about 36.5 percent of households in public housing live in census tracts with poverty rates in excess of 40 percent. These numbers suggest that about 155,000 families with children live in public housing projects in these high-poverty neighborhoods. The MTO results suggest that 15 percent of these families would accept a traditional voucher to leave their public housing unit. Therefore, if the policy reform were limited to these families, about 23,000 of the 155,000 eligible households would use the offered vouchers.

This 23,000 figure, however, is an upper limit on the increase in net total demand for housing units in the private market. Under the MTO reform, households moved from the private market to public housing to replace the families who used the MTO vouchers. Thus, the net increase in total demand for units in the private market was about zero, albeit positive in the middle of the quality spectrum and negative for lower quality units. Voucher recipients tend to occupy rental units of about average quality, whereas families who enter public housing typically come from the worst units in the private market. The first alternative reform would have added as many units to the supply in the private market as to the demand (albeit not at each quality level) because the units MTO voucher recipients vacated would have been rented at market rents. They would have become a part of the unsubsidized supply. Only for the second alternative reform would the increase in net demand in the private market have been about 23,000 households. Under this reform, about 23,000 inhabitable public housing units would have been demolished.

This number of voucher recipients would easily be absorbed into the rental housing market with minimal effects on market rents or vacancy rates in any segment of the housing market. According to the American Community Survey, there were more than 44 million rental units and about 3.6 million vacant units available for rent in the U.S. in 2010. More than 220,000 of the vacant units had monthly contract rents in each $50 interval between $400 and $800, more than 350,000 units had asking rents between $800 and $899, more than 250,000 units had asking rents between $900 and $999, and more than 400,000 units had asking rents between $1,000 and $1,250 a month.

The evidence from the Experimental Housing Allowance Program leads to a similar conclusion. Its entitlement Housing Assistance Supply Experiment, which offered tenant-based housing assistance to the 15 to 20 percent of households with the lowest incomes in two metropolitan areas, had minimal effects on the market rents of units of any type (Lowry, 1983).

Conclusion

MTO provides evidence on the effects of two important reforms of low-income housing policy on a subset of the people affected by those reforms. At this stage, it is not feasible to estimate the effects of these reforms on many others, namely, the additional people who received housing assistance on account of MTO. This article argues that the MTO evidence is applicable to two similar reforms that, unlike MTO, would have no effect on the number of families who receive housing assistance but would instead affect the nature of the housing assistance offered and the taxpayer cost of providing it. Supplementing the MTO results with estimates of the taxpayer cost of the reforms would provide a reasonably comprehensive analysis of their effects. This article suggests how to estimate the taxpayer costs of the alternative reforms. Doing so would significantly increase the value of MTO research for housing policy development. Existing evidence gives good reason to expect that one of the reforms would achieve the benefits to public housing tenants who accept the offered vouchers at a lower taxpayer cost than the current system.

Acknowledgments

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Moving Neighborhoods Versus Reforming Schools: A Canadian's Perspective

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The Moving to Opportunity (MTO) for Fair Housing demonstration provided a definitive opportunity to consider the often-wondered question of what would result from helping relocate low-income families away from some of the worst living conditions in the United States. I commend all those involved for the study's extremely clever design, well-executed operation, and thorough data analysis. I hope the work helps further inspire government and academic collaboration to create demonstration projects to answer equally important policy questions.

The public housing projects in which MTO families initially resided were often characterized by poorly maintained facilities, high levels of crime, extreme ethnic and racial segregation, and isolation from other neighborhoods. Ethnographic studies documenting life in these projects offer persuasive examples of how these conditions adversely affected every aspect of tenants' day-to-day existence (for example, Venkatesh, 2000). Given this setting, it is perhaps not surprising that, 5 years after being offered housing vouchers and assistance to move, participants felt substantially safer at night and more satisfied with their environment. As the final impacts evaluation (Sanbonmatsu et al., 2011) suggests, these improvements in living conditions may very well relate to the observed subsequent improvements in self-reported mental health (feeling calm and less depressed).

The MTO experiment, however, appears to have had no effect, or possibly negative effects, on children's educational outcomes. This article focuses on these provoking results. Sanbonmatsu et al. (2011) showed that MTO had no detectable long-term effects on math or reading achievement test scores, whether for youth aged 10 to 12, who spent little of their lives in housing projects, or for the combined youth sample aged 13 to 20. High school graduation and postsecondary attendance rates for older youth were actually lower for the experimental group than for the control group, and the difference was statistically significant in some cases at the 5- and 10-percent levels. Student attitudes and expectations about school were about the same, although male youth in the experimental group were more likely to have been suspended or expelled.

These findings are consistent with those from Jacob (2004) and some of my earlier research (Oreopoulos, 2003). Jacob (2004) examined families who were offered housing vouchers that enabled them to move from buildings in Chicago housing projects set for demolition because of irreparable conditions. Average census-tract poverty rates for families who received vouchers fell significantly

after 5 years, but children's math test scores, attendance, retention, and dropout rates were no different compared with those of families from the same projects whose units were not scheduled for demolition. In a previous paper, I used administrative data to track children who grew up in Toronto public housing projects—some in high-density areas, others in smaller buildings in more residential and middle-income areas. Applicants had virtually no control over which project was offered to them at the time they were at the top of the waiting list, so project assignment was effectively random. Although living conditions and exposure to crime varied substantially, I found no differences in eventual earnings, unemployment likelihood, and welfare receipt between residents of the largest and smallest projects. I concluded that, although social interactions may affect social and economic well-being, neighbor interactions may not be important enough to significantly influence behavior, at least on average. Individuals have greater choice over whom to interact with in a neighborhood setting than in a classroom or college dormitory setting. Perhaps for this reason, evidence of group effects is more convincing using within-school variation of classmates or roommates than across-school or across-neighborhood variation (for example, Carrell, Fullerton, and West, 2009; Friesen and Krauth, 2011; Hoxby, 2000; Lavy and Schlosser, 2011).

At the time I completed my study, I wondered whether the final impacts evaluation would make my study obsolete, given its more compelling experimental design and its more interesting sample population of individuals from some of the most impoverished neighborhoods in the United States. Arguably, if MTO showed no detectable effects from moving these individuals away from extremely poor housing conditions, we would not expect to detect effects from anywhere else. On the other hand, if MTO showed significant effects, sample and neighborhood differences would plausibly reconcile those findings with my results. Although large public housing projects in Toronto are unattractive, they do not exhibit nearly the same degree of crime and racial segregation that occur in high-poverty neighborhoods in the United States (for example, Oreopoulos, 2008).

It turns out, however, that MTO led to neighborhood change, but it did not lead to much school-quality change. Parents who were offered assistance to move went from living in census tracts where 53 percent of residents were classified as low income to tracts where 37 percent were classified as low income (on average, and including those who did not move). Their children, however, were about as likely to end up at a charter or magnet school as those from the control group. The percentile rankings of children's schools based on state exams were also similar between the control and experimental groups (about 25th, on average). DeLuca and Rosenblatt (2010) and Ferryman et al. (2008) discussed multiple reasons why little changed. Most children stayed in the same school district, where all schools within a district were of similar quality. Some stayed in the same school because parents believed that taking children away from friends and familiar places would be disruptive. In other cases, although children moved into census tracts with fewer low-income people, nearby public schools did not fare much better than the ones from which they came, because more affluent residents chose to send their children elsewhere. Parents also seemed to lack information that would have helped them better consider charter and magnet schools in the area. Jacob (2004) observed similar responses from households given housing vouchers to move from their units in a large project in Chicago. Many chose simply to transfer to other units in the same project. Others relocated close to their original residence, and very few students changed schools.

Overall, the MTO results suggest that policies to relocate low-income households from areas of highly concentrated poverty to areas of less concentrated poverty are likely to be ineffective, especially relative to costs, in improving education outcomes. Perhaps assistance relocating to much more affluent areas or much less segregated areas would be more successful, but such movement on a large scale is not possible without unrealistic financial cost, and it is not clear how many low-income households would want to go. Parents from poor neighborhoods seem to focus on a desire to reduce exposure to crime and drug activity, but many also seem reluctant to move far from their current residence. Even if movement to more affluent or less segregated areas were possible, policymakers would want to consider the potential negative effects on households already living in these neighborhoods. I am not aware of any MTO research that has examined this possibility. The tendency for voucher holders to move into neighborhoods where crime rates are increasing or school conditions are deteriorating complicates the investigation of potential negative externalities from providing vouchers (for example, Ellen, Lens, and O'Regan, 2011).

Another reason for caution about the potential effectiveness of mobility programs on improving educational outcomes is that low-income households move often. Even households from MTO's control group moved, on average, more than twice over about 12 years, with less than 30 percent of the control group in public housing by the end of this period. As a consequence, differences in neighborhood quality between the experimental and control groups dissipated over time. This result may explain why initial effects on test score performance in two MTO sites (Chicago and Baltimore) died out over time (Burdick-Will et al., 2011). In general, more than one-half of low-income households entering high-poverty neighborhoods leave within 3 years (Quillian, 2003). Thus, the period during which households are exposed to high-poverty neighborhoods may not be long enough to produce significant influence, or individuals who receive assistance to move into more attractive neighborhoods may not stay for long.

As the final report notes, the MTO results do not imply that school quality never matters because the experiment had little effect on school quality. In fact, a growing body of evidence suggests impressive gains from attending charter schools, especially among minority students living in high-poverty areas (for example, Angrist, Pathak, and Walters, 2011). Winning a lottery to attend a charter school in Boston increased student achievement by about 0.2 standard deviations per year in English language arts and by about 0.4 standard deviations per year in math compared with achievement in traditional public schools. Lottery studies in New York City and Washington, D.C., showed similar gains. Angrist, Pathak, and Walters (2011) and Dobbie and Fryer (2011) concluded that these schools were effective because of an adherence to a "no excuses" approach to urban education, emphasizing instruction time, intensive tutoring, high expectations, and traditional math and reading skills.

Two other studies used exogenous refugee placement into different neighborhoods and different schools. Gould, Lavy, and Paserman (2004) examined Ethiopian children airlifted to Israel over 3 days and placed in absorption centers throughout the country in essentially random order. Children who attended schools in wealthy and urban settings were substantially less likely to drop out of high school and more likely to pass a matriculation exam than were children assigned to less developed and smaller towns. The effects remained even after accounting for community charac-teristic differences, leading the authors to conclude that school-quality differences explained the

results. Gould, Lavy, and Paserman (2011) also concluded, from a study of Yemenite refugees, that children airlifted to Israel and placed in areas with more modern infrastructure were more likely to obtain post-secondary education.

Thus, rather than expend resources moving children away from high-poverty areas, facilitating greater access to better schools, preferably through public-school reform so that all children can take advantage of these potential gains, may be a more effective approach to improving long-run gains. Applying an evidence-based policy approach to improve schools also has the advantage of potentially benefiting all children. Moving at-risk youth to better neighborhoods, in contrast, could lead to negative outcomes for youth already living in these areas. To realize mental health gains found from MTO without mobility, perhaps continued revitalization of high-poverty neighborhoods into more mixed-income developments may help reduce crime and drug activity and, in turn, lower stress.

Even within high-poverty neighborhoods, the variance in long-run outcomes is high. In my study of children from Toronto public housing projects, some children ended up doing very well in terms of earnings by age 30, whereas others ended up earning almost nothing. Family differences, as measured by sibling outcome correlations, mattered a great deal, accounting for more than 30 percent of the earnings differences. Were we better able to understand what specific family factors lead some disadvantaged youth to rise above their circumstances, perhaps we could develop more effective tools to combat poverty. Until then, focusing on school reform seems to me the best bet.

Acknowledgments

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Commentary:
MTO's Contribution to a Virtuous Cycle of Policy Experimentation and Learning

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When the Moving to Opportunity (MTO) for Fair Housing demonstration[1] began in the mid-1990s, policymakers at the U.S. Department of Housing and Urban Development (HUD) were newly aware of the terrible damage inflicted on families and children living in severely distressed neighborhoods and the role federal housing policy played in concentrating and isolating poor (mostly minority) families in these neighborhoods. Findings from the Chicago Gautreaux experiment suggest that helping families escape from deeply poor neighborhoods and move to neighborhoods of opportunity might dramatically improve their well-being and life chances. At the time, however, few people (whether policymakers, practitioners, or scholars) saw HUD as a source of policy innovation or rigorous experimentation, and federal housing policy was an afterthought in most discussions about antipoverty strategies and welfare reform (Briggs, Popkin, and Goering, 2010).

Because reliable answers about what works in public policy are hard to find, labeling experiments like MTO as either "successes" or "failures" is tempting. Did the demonstration prove that using housing vouchers to relocate poor minority families works? If not, did it fail? In fact, MTO succeeded in ways no one anticipated when it was launched, generating valuable lessons and raising new questions about the effects of neighborhood distress and the potential role of assisted housing mobility. Findings to date have spurred successive rounds of policy innovation and research that test new hypotheses about how, where, and for whom neighborhoods matter and how both housing

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[1] HUD randomly assigned residents of public and assisted housing projects who volunteered for the MTO demonstration to one of three groups. The experimental group received housing vouchers that, for the first year, families could use only in low-poverty neighborhoods, along with mobility counseling and search assistance. The Section 8 group received traditional housing vouchers that families could immediately use in any neighborhood. The control group continued to receive housing subsidies in the original development. Researchers have tracked MTO participants systematically over the intervening years to support analysis of long-term economic, educational, and health outcomes.

mobility and neighborhood revitalization can improve outcomes for families and kids. In addition, MTO has dramatically raised the profile of HUD (and federally subsidized housing) as an important contributor to both innovation and learning in the world of antipoverty policy.

Unexpected Improvements in Health and Mental Health

The initial hypotheses about the potential benefits of assisted housing mobility did not anticipate health improvements. The earliest exploratory studies of MTO families suggested, however, that moving out of dangerous and chaotic environments (and into better quality housing in safer neighborhoods) might yield important health and mental health benefits (see, for example, Goering and Feins, 2003). MTO researchers responded by focusing more quantitative and qualitative attention on these outcomes and the processes driving them, thereby enriching a growing body of evidence from other fields about the damaging effects of trauma and stress on children's physical, emotional, and intellectual development. MTO findings have also triggered related investigations of the health effects of neighborhood crime and violence, in particular the possibility that girls suffer from "sexually corrosive" neighborhood environments (Popkin, Leventhal, and Weissman, 2010).

The significance of health outcomes in MTO research (see Sanbonmatsu et al., 2011)—combined with other research on the costly spillover effects of chronic illnesses like obesity, diabetes, asthma, and depression—has already influenced policy and practice. This research has heightened awareness among housing policymakers and practitioners about health risks facing the families they serve. Both HUD and public housing agencies have begun giving much greater attention to the physical and mental health of public and assisted housing residents, partnering with health-service providers to improve healthcare access and targeting conditions in housing units, properties, and neighborhoods that may undermine residents' health.[2]

Disappointing Results for Education and Employment

MTO teaches that, although many high-poverty neighborhoods lack both good schools and proximity to good jobs, moving to a low-poverty neighborhood does not guarantee that children will attend high-performing schools or that their parents will gain access to secure, well-paying jobs. The absence of measurable gains in education or employment for MTO participants has led researchers to dig deeper into both issues, including qualitative investigation of the factors underlying families' school choices and spatial analysis of the distribution of low-skilled job opportunities in the five MTO metropolitan areas (Baltimore, Boston, Chicago, New York, and Los Angeles). These findings have prompted mobility programs across the country to supplement their counseling and support services to more explicitly help families connect to good schools and stable employment.

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[2] For example, HUD's Choice Neighborhoods grant program, which provides funding to redevelop distressed public and assisted housing developments and the neighborhoods surrounding them, explicitly targets improved physical and mental health as priority outcomes. See http://portal.hud.gov/hudportal/HUD?src=/press/press_releases_media_advisories/2012/HUDNo.12-006.

A core assumption underlying the MTO demonstration was that children in families in the experimental group would attend dramatically better schools than those serving their original neighborhoods. As discussed further in the next section, however, most of the families in the experimental group stayed in the same school district, so their children did not experience dramatic improvements in school quality. Roughly 7 of 10 MTO families sent their children to their assigned schools, whether in the immediate neighborhood or in some larger, administratively defined attendance zone. For others, informed choices proved difficult. Some parents were unaware of the options available in their new neighborhoods, in part because most relied on limited information resources, such as word-of-mouth referrals from relatives and friends. In addition, many parents emphasized order, discipline, and convenience as indicators of a "good" school more than evidence of academic supports and achievement. Not surprisingly, because they were fleeing some of the nation's most unsafe neighborhoods and schools, these parents placed the highest priority on en-suring that their children would be safe at school—even if this meant staying at the school in the original neighborhood (Briggs et al., 2008).

Just as MTO did not automatically yield better public school assignments, most new locations did not offer better access to jobs, despite their lower poverty rates and dramatically improved safety. In fact, geographic analysis suggests that MTO families in Los Angeles and Chicago moved to neighborhoods that were no closer to low-skilled job opportunities than were the neighborhoods they left behind (Cove, Turner, and Briggs, 2008). Ethnographic research highlights the tremendous struggles MTO families faced in trying to line up a secure, three-way spatial match: access to affordable child care (often provided by relatives or friends), a house or apartment that stayed affordable, and a reasonably secure job. This struggle often led to instability, difficult commutes, and frequent moves and job changes, and these challenges were particularly daunting for parents facing multiple barriers not directly affected by location, such as disabling physical or emotional illness or limited skills.

MTO Intervention Fell Short of Its Vision

One possible reason that MTO gains were limited to health outcomes is that the special mobility assistance provided by the demonstration did not enable families to gain and sustain access to high-opportunity neighborhoods. Families in the experimental group moved to better quality housing and safer neighborhoods than their counterparts in the control group, but few of these families spent more than a year or two in low-poverty or high-opportunity neighborhoods. By the end of the demonstration period, differences in exposure to high-opportunity neighborhoods were quite modest across the three randomized groups. For example, families in the experimental group spent, on average, only 22 percent of the time between random assignment and the final impacts evaluation living in neighborhoods of very low poverty and unemployment compared with 9 percent for their counterparts in the control group. Families in the experimental group lived in predominantly White neighborhoods only 9 percent of that time compared with 5 percent for the control group (Turner et al., 2011). In other words, MTO did not produce the dramatic improvements in neighborhood environment (or school quality) its designers envisioned and had almost no desegregative effect.

What happened to the experimental group families after their initial moves to low-poverty neighborhoods? Why did the mobility assistance they received not result in longer exposure to high-opportunity neighborhoods? Analyzing mobility trajectories shows both that MTO participants were quite mobile and that some of the neighborhoods to which they moved changed over the course of the decade, mostly becoming poorer and more predominantly minority. Families who moved to high-opportunity neighborhoods at the outset followed one of four distinct patterns over the subsequent decade: (1) roughly one in four sustained their residence in high-opportunity neighborhoods throughout the period, (2) about one-third immediately lost access and never regained it, (3) about 20 percent immediately lost access but subsequently regained and sustained it, and (4) about 20 percent maintained access to high-opportunity neighborhoods for more than 1 year but lost access later (Turner et al., 2011).

Other research found that families left high-opportunity neighborhoods because they experienced problems with their lease or their landlord, were dissatisfied with how their housing was maintained, or wanted a bigger or better quality apartment. During the housing boom that occurred during the early to middle years of the 2000s, families faced additional stressors, such as units being sold and rehabbed, rented for more than the voucher program rent ceiling, or removed from the voucher program altogether (Briggs, Comey, and Weissman, 2010). Many families who made initial moves to safe, opportunity-rich neighborhoods were unprepared for the competition in the new, "hot" housing market: rents skyrocketed, landlords would not accept the vouchers, and extensive savings were required for security deposits, first and last months' rent, and other credit requirements.

What's Next for Policy, Practice, and Research?

Neighborhood change alone cannot overcome the problems of poverty, but it would be a mistake to conclude from MTO's results that neighborhoods are irrelevant to employment or school success, that helping families relocate to opportunity-rich communities—and stay there—has no effect on these domains, or that implementing a program that yields more dramatic changes in neighborhood outcomes is infeasible. MTO's findings strongly establish the importance and benefit to families of escaping from severely distressed and dangerous neighborhoods.

Moving forward, policymakers can draw on MTO findings to develop *place-conscious* strategies that both improve the neighborhoods in which poor people currently live and simultaneously open up wider opportunities for them to move to neighborhoods offering greater opportunities (Pastor and Turner, 2010). In my view, MTO argues for next steps that include—

- Investing more (and more effectively) to restore the safety, stability, and vitality of inner-city neighborhoods so the families who choose to remain there can thrive.

- Encouraging rental property owners in safe, well-resourced communities to participate in the voucher program and informing voucher recipients about the full range of locational options available.

- Expanding the supply of moderate-cost rental housing in healthy, well-functioning neighborhoods (particularly neighborhoods that offer both safety and good schools).

- Helping families *stay* in new, opportunity-rich communities after they make initial moves.

Housing and neighborhood policymakers still have much to learn from MTO. Although the demonstration may not have produced all its expected results, it offers a gold mine of information about the challenges facing low-income families, their patterns of residential mobility, and the possible effects of changing neighborhood conditions on their immediate well-being and longer term life chances. These data can be a resource for innumerable investigations, cutting across policy domains to explore current issues in housing, neighborhoods, health, employment, education, crime, and social networks. In particular, researchers will be able to use these data to dig deeper into the question of whether the MTO families who spent more time in better neighborhoods enjoyed better outcomes and to explore what neighborhood characteristics are associated with what outcome gains and for what kinds of people.

Today—in part because of its investment in the MTO demonstration—HUD has gained respect as a source of policy innovation and rigorous hypothesis testing. Distinguished scholars from many disciplines are investigating interactions between neighborhood environment and individual well-being, and local practitioners are drawing on research evidence to refine and strengthen programs that help poor families make good choices about where to live—implementing a next generation of innovative models and hypotheses for researchers to test. I consider that success.

Acknowledgments

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Point of Contention: Defining Neighborhoods

Guest Editor: Ron Wilson

U.S. Department of Housing and Urban Development

Neighborhoods are a natural construct widely used for analytical purposes in research, policymaking, and practice, but defining a neighborhood for these purposes has always been difficult. This Point of Contention offers four articles about precisely bounding this often fuzzy concept. The authors provide a range of perspectives, from practitioner to researcher, about the construction of neighborhoods and the complexity of what neighborhood really means.

The Tyranny of Census Geography: Small-Area Data and Neighborhood Statistics

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The opinions expressed in this article are those of the author and do not necessarily reflect those of the U.S. Department of Housing and Urban Development.

Census-defined small-area geographies and statistics in the United States are highly accessible, relatively easy to use, and available across time and space. The singular and strict use of block groups, census tracts, or ZIP Codes as proxies for neighborhood, however, are often inappropriate and can result in flawed findings, poor public policy decisions, and even situations in which families or businesses are disqualified from place-based government programs. Perceptions of neighborhoods are social constructs and context dependent. Yet social science literature is replete with an unquestioning use of these geographies to measure neighborhood effects, despite evidence that the use of alternative spatial scales and techniques can deliver very different results.[1]

Census small-area statistics are artifacts of the geographic boundaries created by the Census Bureau, often in collaboration with local stakeholders. Census small-area data were first used for policy and research purposes based on a request by New York City in the early 1900s for permanent census tract areas (sanitary districts), not subject to the political manipulation of wards

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[1] Although this article focuses uniquely on census small-area geographies, any geographic template will have limitations and resulting analyses should not be considered definitive.

and assembly districts, to measure and monitor public health conditions (Salvo, 2012). Given ongoing changes in socioeconomic, demographic, land use, and other characteristics, maintaining permanent statistical areas over time has proved to be neither ideal nor practical.

Geographers and statisticians learn early in their careers that, when you change the geography, you change the statistics. For example, those involved in legislative redistricting understand how alternative boundaries can affect political outcomes, and families with school-age children understand how redistricting school boundaries can affect their children's social relationships and educational outcomes. Similarly, neighborhood statistics take on new meanings by sometimes small changes in their boundaries (Wong 2009).

Before every decennial census, local statistical areas committees must evaluate tradeoffs between different user needs (for example, research and planning) in creating, maintaining, or revising small-area statistical boundaries. Collectively, they must resolve the competing objectives and inherent contradiction between maintaining spatial continuity for data comparability and the need to create meaningful statistics based on areas with relatively similar socioeconomic and demographic characteristics as they attempt to capture the spatial growth and reorganization of the built environment. And, with the advent of the American Community Survey (ACS), these tradeoffs have become even more challenging as increased emphasis has been placed on the population size of census tracts and block groups, in an effort to promote more reliable estimates.

Despite the importance of defining and/or revising census statistical boundaries in the current 10-year cycle, local input varies from place to place. Broad participation from stakeholder groups may not exist and participants may not be uniformly cognizant of the full effect of their work. In some instances where local input is minimal or nonexistent, Census Bureau staff will create or redesignate census tracts and block groups at their discretion.

Constructing Neighborhoods From Census Geographies

Census tracts are often the de facto neighborhood unit of analysis in social science research (for further discussion, see Coulton et al., 2001; Nicotera, 2007; White, 1987). Therefore, defining and delineating these and other statistical area boundaries are critical in determining place-based characteristics. The most relevant data aspects of using tracts, block groups, or ZIP Codes as neighborhood geographies can be categorized along three dimensions—definitional, spatial, and temporal.

Using census geography to define neighborhood borders requires an understanding of the basic building block of all census geography and statistics—the census block. The Census Bureau first published census block data in 1940 for major urban areas as part of a newly created Census of Housing. These blocks provided "a detailed inventory of housing conditions within major cities for purposes such as efficiently upgrading the level of urban services, modifying building codes, establishing and implementing zoning ordinances, and preparing plans for capital improvements" (U.S. Census Bureau, 2012: 11-3).

The use of census blocks aligned well with the early technical and geocoding advances that made a mail-out/mail-back census feasible and later as a means to support state redistricting needs. With the advent of nationwide block numbering and the development of the 1990 TIGER database,

census blocks became the geographic and statistical unit of analysis for all areas of the country. Since 2000, the entire United States has been divided into the common small-area geographies of block group, census tract, and ZIP Code Tabulation Area.

Use of the census block model, however, has involved a number of challenges and limitations. Changes in local government boundaries from secession and annexation rarely conform to census blocks and create ongoing data quality challenges. Just as census tracts do not conform to the popular conception of a neighborhood, census blocks do not always align with the popular notion of a block, particularly in urban areas where both sides of a street are considered part of the same block. As one consequence, people and businesses are sometimes denied program eligibility for place-based programs because they are located on the wrong side of a street, which is in a different census tract.

Census tracts are the most widely used small-area geography for neighborhood and community research. Eligibility for many federal and state place-based funding programs often require that the household or business be located in neighborhoods that meet specified criteria. In many cases, neighborhoods are equated with census tract boundaries and the criteria to determine eligibility are based on sample data from the census tract, often without regard to standard errors.

Tracts, however, may contain pockets of demographic, social, and economic characteristics and patterns that may not be reflected in their summary statistics. Indeed, as stated earlier, maintaining tract homogeneity is no longer really possible given changing patterns of settlement and density in large urban areas. For example, Housing Choice Voucher (Section 8) recipients may be moving to "neighborhoods" of less poverty, but, in practice, they may be moving to high or similar poverty areas within a different census tract. Tracts that contain both high-income and low-income areas may appear as middle-income tracts. Furthermore, spatial proximity or inclusion in a wider statistical area, in itself, does not necessarily lead to increased opportunity or integration.

Block groups—hierarchical subdivisions of census tracts and aggregations of census blocks—were rarely used in any systematic way until the 1990 Census. They offer a better approximation of neighborhood areas, albeit a smaller and less reliable sample size that limits their broader use. While more likely to change their boundaries from census to census, block groups were seen as a way to create more socioeconomically homogeneous areas because census tracts were becoming more and more disparate from their original conceptions.

The sample size of ACS block groups, however, is considerably less than previous decennial censuses so that most researchers are returning to the census tract as the default small-area geography. In fact, for many purposes census tracts need to be aggregated into larger geographic areas because the ACS sample is too small. For example, in New York City, demographers have aggregated more than 2100 census tracts into Neighborhood Tabulation Areas as a means of achieving adequate sample size, using a template that has at least some on-the-ground connection to neighborhoods.

ZIP Codes are also well-known, popular geographies for representing neighborhoods. ZIP Codes, however, are designed solely to meet the day-to-day operational needs of the U.S. Postal Service and are subject to change at any time, with no systematic method for delineating them. ZIP Codes have no minimum population or housing unit thresholds and often contain widely disparate demographic and socioeconomic characteristics, making them more susceptible to capturing

extreme data points within the same boundary. Although ZIP Code data may be good enough for marketing or other gross estimates, they are rarely appropriate for rigorous social science research. These entities present an additional data quality challenge in that the Census Bureau uses its block-based model to represent ZIP Codes as tabulation areas, which leads to a built-in misallocation of housing units along the boundaries.

Until the advent of the ACS, small-area census estimates were based on data collected at a specific point in time. Fundamentally different than these decennial long-form surveys, ACS block group, tract, and ZIP Code estimates are based on rolling samples averaged across 5 years. Although small-area estimates are now more current and non-sampling errors have been reduced, sampling issues are considerably more problematic than in previous decennial censuses.

Conclusion

Census small-area geographies are often used as proxies for neighborhoods in social science research as well as for policy and decisionmaking. Yet, neighborhoods are context dependent. Residents may define their neighborhood by their block, school bus route, or other formal or informal associations, and that definition may change as the context and characteristics of a place change. No unit of statistical or administrative geography will ever be able to capture all of these nuances, and neighborhood boundaries will always be the subject of countless debates.

What is a social science researcher to do? Consideration needs to be given to a process that uses property address or parcel boundaries as the nuclear unit for defining neighborhoods. These units are the cornerstones of constructing any larger areal unit that would represent a neighborhood. Then, a discussion needs to take place on how meaningful these spatial units are, given historical and common on-the-ground definitions of neighborhoods, always with an understanding that these will be approximate in their designation. Once designated, these areas then need to be examined relative to the statistical geography that is available for the tabulation of data and a compromise achieved.

Do pockets of significantly different populations exist within a census tract that may be hidden by overall averages? Do analyses of similar phenomena using different geographies and alternative representations create different stories? Have appropriate statistical and spatial methods been used to account for sample estimates and standard errors and the "rolling" or "continuous measurement" nature of the ACS when combining and comparing small-area census data across geography and time? These are some questions that can be asked about the census boundaries chosen to represent neighborhoods and the resulting analyses of these areas.

This article focused on some of the challenges and appropriateness of the singular use of small-area census data and geography to represent neighborhoods. Although their use can lead to misinter-pretation, the absence of alternative data-rich spatial configurations forces us not to abandon them altogether. Awareness of their limitations and the uncertainty of our findings from their generalized use are critical. Implied from this discussion is the need for the concomitant use of methodologies and spatial analysis techniques that both aid the defining of neighborhood boundaries and offer complementary and more nuanced perspectives and meaning to available data from the census and

ancillary sources. For the purposes of research and public policy, both the selection of geographic areas and the use of spatial and statistical techniques must be adaptive and based on the particular questions being posed as well as the type of data available. Otherwise, social science researchers may continue to use neighborhood definitions that may not reflect or represent the true nature of the areas being studied.

Acknowledgments

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Defining Neighborhoods in Space and Time

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Defining Neighborhood and Community

The terms *community* and *neighborhood* reference some of the most notoriously slippery social science concepts. One publication (Hillery, 1955) appearing more than five decades ago listed more than 90 definitions of community, tapping 16 different themes. The concept of neighborhood is similarly diffuse, precluding scholarly consensus (Keller, 1968). "There are many ways of defining neighborhood" and "different definitions serve different interests" (Brower, 1996: 17). Each of these two concepts has received scholarly attention for a century or more (Burgess, 1925; McKenzie, 1923), has waxed and waned in that period as a topic of interest to both scholars and policymakers, and has been defined in numerous ways.

The core idea of community is "social interaction, common ties, and coresidency" (Brower, 2011: 3). The core idea of neighborhood is that it is "a social/spatial unit of social organization, and that it is larger than a household and smaller than a city. The problem with presenting a further list of definitive characteristics is that they often become *normative* rather than *descriptive*" (Hunter, 1979: 270; emphasis added). Although a neighborhood is geographically delimited, all residents may not strongly agree about exactly where it begins and ends, depending on a range of housing, land use, personal, and political factors (Firey, 1945; Taylor, 2001). Recognizing the important differences between the terms *community* and *neighborhood* (Brower, 1996; Brower and Taylor, 1997; Hunter, 1974; Hunter, 1975), this work uses the term *neighborhood,* given the generally stronger spatial and geographical emphasis linked to this term versus *community*. Quoted materials with the term *community* retain that language.

Key Structural Attributes

In urban and suburban settings, at least in countries such as the United States and Canada and those in Western Europe, neighborhood has several key structural attributes. First, neighborhoods have layers of concentric geography within them, starting with the streetblock or segments thereof (Suttles, 1972). Streetblocks have their own microecological principles (Taylor, 1997). Although the character of and dynamics in a streetblock link to the broader neighborhood setting, residents may feel more socially integrated and safer on their own streetblock compared with locations even a bit farther away (Taylor and Brower, 1985). Additional neighborhood layers extend out from the streetblock and may even include large regions of a city.

No single layer of neighborhood is correct for research or policy purposes. Rather, the spatial scale chosen to represent a neighborhood layer should match the spatial scale of the dynamics considered from a policy or research perspective (Hipp, 2007). For example, if contested drug corners are the focus of policymakers and research analysts, then Thiessen polygons[1] around intersections are conceptually compatible for research, policy, and practice purposes (Taniguchi, Ratcliffe, and Taylor, 2011). From a policy perspective, like that of crime analysts, the key definitional issue may be how the operationalization of the concept links to police resource needs and deployment and to quality-of-life concerns of residents (Buslik, 2009).

Second, neighborhoods are imbricated, or overlapping, for organizational and political economy reasons (Logan and Molotch, 1987). "Overlapping of community boundaries not only produces confusion and ambiguity but means that residents do not necessarily see the city as divided into mutually exclusive local areas" (Hunter, 1974: 87). The ambiguous nature of these boundaries has positive integrative aspects and may reinforce rather than undercut the symbolic functions of neighborhood (Hunter, 1974). Operationalizing these overlapping ideas into workable, bounded, and nonoverlapping neighborhoods presents challenges. Consequently, it is not surprising that police struggle "when defining neighborhoods, applying crime-prevention services, and deploying resources to an area" (Buslik, 2009: 5).

Third, neighborhoods change over time, often because of changes in local organizations and connections between those neighborhoods and outside interests. So it is no surprise that neighborhood boundaries and names change over time as well (Hunter, 1974). A study of empirically derived Baltimore neighborhoods and how their boundaries and names shifted between 1979 and 1995 identified eight different types of boundary and name changes (Taylor, 2001: 303–363). The sizes and types of changes arose from how local power differentials and ecological disparities intersected with one another. The disturbing policy implication was that the places most in need of stable coproduction arrangements with agencies such as police at the same time presented the greatest challenges to such partnerships because of just these changes.

Fourth, neighborhoods are personally variable in contour, character, and salience. Sizable literatures in environmental psychology, community psychology, political science, and geography examine what individual and contextual factors link to how people cognitively organize their neighborhood. Neighborhood cognitions span views about the neighborhood's character, placement of its boundaries, locations of landmarks or pathways (Devlin, 1976; Devlin and Bernstein, 1995; Downs, 1981; Downs and Stea, 1977; Evans, 1980; Evans and Pezdek, 1980; Lynch, 1960), and application of which neighborhood names (Crenson, 1983; Hunter, 1974; Taylor, Gottfredson, and Brower, 1984). Individuals personally construct how and what their neighborhood is and how it changes (Aitken, 1990, 1992). In addition, they have different ways of transacting with their locale, that is, different modes of environmental knowing (Aitken and Bjorklund, 1988; Anooshian, 1996).[2] Those modes may shift over time for individuals (Anooshian and Young, 1981). Further deepening the

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[1] Thiessen polygons around points are such that the area within the polygon is closer to that point than any other point. In this work, the polygons were centered on corners.

[2] Aitken and Bjorklund (1988: 59) identify four modes of "transaction and transformation": "habitual" vs. "purposive behavior" applied to either "ordinary events" or "extraordinary events."

variability, in addition to individual differences and different modes of environmental knowing, is a range of different techniques with different properties. These are used for extracting information about mental maps or for organizing features of such maps (Golledge, 1987; Golledge and Stimson, 1997). The toolkit for examining all these matters has expanded dramatically with the arrival of Geographic Information Systems (GIS) (Golledge, 2003). Given these three sources of personal, mode, and toolkit variation, it is no surprise that people often disagree a lot about neighborhood names and boundaries (Coulton, Chan, and Mikelbank, 2010; Coulton et al., 2001; Lee, 1970).

Once thinking starts about *where* an individual is located relative to a census or community boundary (Rengert and Lockwood, 2009), additional person-place variation is introduced. Finally, overlaid on top of all this psychological and methodological variation are the previously mentioned political (Crenson, 1983; Logan and Molotch, 1987), structural (Hunter, 1974), and small-group (Suttles, 1968) dynamics that are also in play when thinking about labeling or bounding a neighborhood.

To some extent, variation in definitions can be incorporated in a number of meaningful ways for analytic purposes. Census data at the tract, block group, or block levels can be reallocated to correspond to defined areas around an individual or a location. Census-enhanced GIS spatial units can be created (Rengert and Lockwood, 2009; Wilson, 2007). Of course, with different types of units configured different ways, different varieties of the modifiable area unit problem (MAUP) can surface (Openshaw and Taylor, 1979). The MAUP is conceptually related to, but at the same time distinct from, aggregation issues (Oliver, 2001).

Closing Comment

When struggling through graduate school more than three decades ago, I heard one faculty member quip: "this is not so hard." You have to remember only two laws of psychology. First law: "everyone is the same." Second law: "everyone is different." Of course, what he did not add was the hard part: how to know when someone's behavior is evidence of the first law, and when it is evidence of the second law. When researchers think about neighborhoods, they have the same dilemma: all neighborhoods are the same; at the same time, all neighborhoods are different. Key structural commonalities do exist in neighborhoods. Which commonalities will be revealed or hidden depends on numerous factors, not the least of which is the researcher's or policymaker's disciplinary lens. Nested within those very commonalities are important variations in spatial scale, overlap, and temporal durability. These variations are not problematic per se, and may even be functional. The key issue for researchers and policymakers is finding a definition that aligns with theory and policy. Is the definition theoretically congruent with the dynamics of interest? Does it serve the needs of the policies or agencies in question? After those questions are answered, geographic boundaries representing "the" neighborhood can be delineated.

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Defining Neighborhoods for Research and Policy

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Introduction

The neighborhood is a social and geographic concept that plays an increasingly important role in research, policymaking decisions, and practice that address disparities in the well-being of urban populations. Research on neighborhood effects is burgeoning, with an increasing number of policies being directed at reducing disparities through place-based initiatives. Most studies of neighborhoods and community initiatives geared toward neighborhood improvement, however, make simplifying assumptions about boundaries. Most studies rely on census geography or political jurisdictions to operationalize the neighborhood units. Conversely, theories about the interactions between residents and their neighborhoods are seldom simple. Among the many pathways of influence, it is often assumed that social and psychological processes are at work within a place. The effect these processes have on one another occurs when residents interact with their surrounding context or environment to give the place meaning (Sampson, Morenoff, and Gannon-Rowley, 2002; Shinn and Toohey, 2003). To the degree that neighborhood influence is predicated on residents' experience in, exposure to, or perceptions of the place in which they live, critical examination of the appropriate delineation of the space designated as the neighborhood unit is important. If neighborhood units depart markedly from real-world experience, the result can be measurement error, misspecification of models, and the solving of practical problems by looking for results or effect in the wrong places.

Standard methods used to define and measure neighborhoods may falter when the methods assume that neighbors share similar perceptions of their neighborhood space or that neighborhood units are fixed or constant in their boundaries. Researchers need a set of spatially calibrated and resident-informed methods that allow variations in perception to be investigated and enable the neighborhood unit to be crafted so that it is optimally bounded regarding the assumptions and purposes of the study. They can use Geographic Information System (GIS) tools, illustrated in this article, to craft neighborhood units that are more useful and authentic for research, policy, and practice than the commonly used administrative boundaries.

Background

The problem of defining a neighborhood and the practical struggle of defining its boundaries has received critical attention in recent years (Downey, 2006; Galster, 2001; Nicotera, 2007). Conceptually, neighborhoods are not merely territory, but "social constructions named and bounded differently by numerous and diverse individuals" (Lee, Oropesa, and Kanan, 1994: 252). Individuals have agency regarding neighborhoods (Entwisle, 2007) and, when they move through their surroundings, they carve their own activity space that does not necessarily map onto arbitrary geographic boundaries (Sherman et al., 2005). Neighborhood boundaries are not static but often dynamic and contested, and social interaction shapes the meaning of places for individuals and groups (Gotham, 2003). Residents can embrace some of the surrounding space and disavow other parts of it, making it more or less relevant to their everyday lives (Gotham and Brumley, 2002).

Although residents may live in geographic proximity, it cannot be assumed that their perception of a neighborhood identity is the same (Coulton et al., 2001). In particular, relative position in the social structure, such as that dictated by age, race, class, or gender, may affect how someone evaluates a neighborhood (Burton, Price-Spratlen, and Spencer, 1997; Campbell et al., 2009; Charles, 2000; Krysan, 2002; Sampson and Raudenbush, 2004). Moreover, neighborhoods themselves may differ in the degree to which they are identifiable, such as whether they have naturally occurring boundaries, demarcations, or commonly recognized neighborhood names (Taylor, 1988).

Although most researchers and practitioners acknowledge the importance of residents' experience of neighborhood, the fact is that most substantive work relies on fixed units from administrative agencies such as the Census Bureau, city governments, or planning groups. Nevertheless, studies that examine resident perceptions confirm that considerable variation exists in how individuals view the size of their neighborhood and where they locate the boundaries (Campbell et al., 2009; Coulton et al., 2001; Lee and Campbell, 1997; Lohmann and McMurran, 2009; Pebley and Sastry, 2009). Given this definitional ambiguity, it is important to further investigate residents' perceptions and other factors affecting neighborhood identity and craft neighborhood units that are informed from their input.

Using GIS Tools in Specification of Neighborhoods for Research

Neighborhoods are rooted in geography—the land, buildings, people, and organizations that compose the place—but research has often treated neighborhoods as units that are untethered to their spatial location. Increasingly, however, researchers are using GIS tools to investigate alternative neighborhood definitions and boundaries that can be informed by residents' perceptions, spatial parameters, or features of the social and physical landscape.

Community Mapping Exercises

A community mapping exercise is one technique that researchers have used to identify neighborhood boundaries. An illustration comes from the Annie E. Casey Foundation's Making Connections program, a community change initiative in low-income sections of 10 cities that focused on

strengthening families and improving neighborhood conditions. Representative samples of adults in each community were asked to draw the boundaries of their neighborhoods as they viewed them onto GIS-generated cartographic maps. One use of these digitized maps was to uncover the core area of collectively defined neighborhoods (Coulton, Chan, and Mikelbank, 2011). This collective definition was made clear by grouping maps from residents who provided the same neighborhood name and then overlaying their maps to find areas of consensus. The blocks that were included in a plurality of resident maps were considered to be core parts of the neighborhood for the purposes of community identity. Local stakeholders reviewed the resulting neighborhood units and provided evidence of face validity of the resident-defined neighborhoods based on their understanding of the local context. These collectively defined neighborhoods were then used as the basis for aggregation of other survey data and block-level census data that yielded social and economic measures for the neighborhood.

Researchers can also use data from the community mapping exercise to create unique person-defined neighborhoods for each resident based on his or her own boundary definitions. For each person-defined neighborhood, the other survey respondents who lived inside the individual's map were grouped. This made it possible to calculate aggregate scores on social and economic measures for each unique person neighborhood using the data from other residents. In addition, researchers used GIS tools to apportion census block level data into each unique person-defined neighborhood map. In this application of the community maps, it did not matter whether agreement was reached among the residents about neighborhood boundaries, because each person's neighborhood was uniquely defined.

Person-Centric Buffers

Researchers can also use GIS to define neighborhood units by drawing buffers of varying sizes around individuals' residential locations. They then calculate variables of interest for these overlapping spaces, which enable them to test hypotheses that can further clarify the scale at which neighborhood influences operate. Evidence of the magnitude of contextual effects on some health outcomes is greater when researchers use these sliding, rather than census-defined, neighborhoods in statistical models (Chaix et al., 2005). Moreover, the optimal size of the buffer may depend on the particular neighborhood characteristic being modeled. Neighborhoods based on varying buffers avoid some of the criticisms of fixed neighborhood units, such as the concern that households at the edge of a fixed unit may be more influenced by the contiguous neighborhood than by households in the center. Rather than directly asking residents to define their neighborhood, these methods infer an optimal neighborhood scale from the magnitude of neighborhood effects.

Pedestrian Street Networks

To define neighborhood boundaries, researchers can also use aspects of the built environment that structure social processes and everyday life. T-communities are theorized networks of pedestrian streets that structure localized social interaction, which are consistent with the concept of neighborhood (Grannis, 2005). Researchers can use GIS tools to identify pedestrian streets and tertiary streets, drawing neighborhood boundaries along those main streets that bound the intersecting pedestrian areas. They can also combine the resulting areal units with local knowledge to further refine this definition of neighborhood units (Foster and Hipp, 2011).

Automated Zone Design

For some purposes, it may be desirable to craft neighborhood units that are demographically homogeneous, are of a designated size, or do not cross selected barriers or landmarks. Researchers can use automated zone-design programs to aggregate areas together while optimizing such criteria (Cockings and Martin, 2005). This method of crafting neighborhood units was investigated after an interactive process that imposed various population and housing characteristic constraints, area size, and geographic considerations (Haynes et al., 2007). The resulting neighborhood units compared favorably with community areas that were designated by local government officers.

Conclusions

Currently, the capacity exists to calibrate neighborhood definitions to be more reflective of residents' experiences and spatial attributes than the commonly used administrative units. On a practical level, these methods require more fine-grained geographic data than are often available from surveys or administrative agencies. Given the nuances of residents' experience and spatial dynamics, justification is strong for making investments in the data and technology that could validate neighborhood definitions and measures. Such units should provide more explanatory power on which to base neighborhood research, policy formulation, and practical solutions.

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Dynamic Geography: The Changing Definition of Neighborhood

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The concept of place matters to the police. This article describes how the police use geographic semantics to define neighborhoods and form practical solutions to resource deployment. It also addresses the nebulous concept of neighborhood and how that term connotes different meanings to citizens, elected officials, and the police, all of which form the basis for deploying police resources that meet the needs of all constituents and community stakeholders.

Perspective From the Ground

As a captain in the Chicago Police Department, I appreciate that the city plays an important role in creating the history of place as a criminological concept. Although Guerry (1833) and Quetelet (1831) presented the first sociological perspective on place, it was the Chicago school of sociology that provided an empirical analysis of how place and crime bore a meaningful correlation (Shaw and McKay, 1942). Other research confirms Chicago's role in the study of place and crime (Liberman, 2007; Maltz, 1995; Maltz, Gordon, and Friedman, 1991; Sampson, Raudenbush, and Earls, 1995; Thrasher, 1927), including the numerous publications on neighborhood effects from the Project on Human Development in Chicago Neighborhoods (PHDCN).[1] Without presuming scholarly equivalence to these works, I offer a police officer's perspective of the influence that differing definitions of neighborhood have on place and crime.[2]

My experiences and views are not markedly different from what research and policing have provided during the past five decades. I recognize how poverty, joblessness, racism, gangs, drugs, political corruption, police misconduct, and societal neglect correlate into crime rates. This article examines the term neighborhood, however, and explores how neighborhoods affect crime and disorder.

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[1] An extensive listing of research articles about PHDCN is available on the Inter-university Consortium on Political and Social Research (ICPSR) website at http://www.icpsr.umich.edu/icpsrweb/PHDCN/biblio/series/00206/resources.

[2] This article is the author's opinion and does not represent the viewpoints of the Chicago Police Department or the University of Illinois. In addition, the circumstances described using the Chicago police as an example are far from unique. Rather, Chicago is used here as a generalization for the difficulties police across the country face and work toward resolving.

Neighborhood? What Does That Mean on the Ground?

Researchers define the term *neighborhood* based on the objectives of their research. For researchers, a neighborhood can be defined by many variables, such as the area lying within a census tract, ZIP Code, or physical boundary, or such as the grouping of citizens who fall into a demographic category (for example, those who have a certain income level, educational attainment, or other socioeconomic status). Regardless of neighborhood characteristics, the associated geography of the neighborhood remains constant in analyses.

Police, however, interpret a neighborhood's geography more dynamically when considering it a unit of analysis to translate the factors that more fully define a neighborhood as a physical location that demands patrol crime prevention or response resources. Police are less concerned with defining neighborhood characteristics than with the results of public safety efforts in a neighborhood: reduced crime, fear of crime, and calls for service. Results are the translation outcome from research to practice—for example, research suggesting where to send the officers, how to respond to community concerns, or how to service an elected official's requests.[3] Essentially, although criminologists are interested in the factors that create criminogenic neighborhoods and lead to crime and disorder, police think about a neighborhood as a "place" to deploy resources. For this reason, a police perspective of place is less prone to subjective interpretations and decisions.

This perspective does not mean that defining place is not important for police. Criminology theory helps to define what police do and how and where to do it, and the term *place* now has a role in everyday policing as a unit of geographic analysis. Weisburd (2008) is responsible for illuminating how police need to interpret the concept of place; he shows that, for police, a criminologist's view of a neighborhood is simply not an accurate description of geography. A scholarly perspective defines a neighborhood as a piece of geography that is usually too big or, in some cases, too small for police purposes.

Police view a neighborhood through the rubric of routine activity theory, in which the victim and offender converge in time and place. Whether through problemsolving in a community-policing environment, a more direct problem-oriented approach, hotspot policing, or intelligence-led policing, law enforcement officers use place or geography to focus limited resources on what needs their attention. The police look not at what constitutes a neighborhood but instead look at neighborhoods as the output of numerous factors and how these factors interact to produce an effect that requires police resources.

Understanding the Neighborhood From a Police Perspective

The various types of boundaries represented in Chicago illustrate the police perspective. Neighborhoods change over time both by boundary and by demographic. Neighborhoods are organic—they grow, divide, merge, decline, and regenerate.

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[3] In particular, a police department operating under a community-policing philosophy will account for a wider range of inputs to the deployment process. Decisionmaking will incorporate feedback from the community, rather than being based solely on a police perspective.

Chicago is often called a city of neighborhoods. These neighborhoods have been formalized into 77 community areas used to identify local history and characteristics. The Chicago Police Department does not formally use these community area boundaries, nor do the boundaries represent formal city-service boundaries. Although these pieces of geography can be as small as informal neighborhoods, they remain too broad to serve police patrol needs.

The police department uses districts[4] and their included beats to deploy patrol resources. With approximately 275 beats, the Chicago Police Department can respond to local community concerns well. Beats can encompass various sized areas and are designed to help average police workloads, but beats often divide multiple census tracts, ethnic groupings, and other demographic variations.

In addition, Chicago is divided into 50 political subdivisions, called wards. An elected city council member, called an alderman, represents each ward. Wards are drawn based on federal decennial census data that are distributed to provide equitable political representation. Police districts and wards criss-cross each other so that one district commander may work with multiple aldermen, or an alderman may work with multiple district commanders, to serve the needs of citizens. As with districts, wards provide no better sense of serviceable neighborhoods. The final account of these overlapping boundaries is a map that shows the difficulty the police have when defining neighborhoods to apply crime prevention services and deploy patrol resources.

Understanding these conflicting boundaries helps to shape effective partnerships with citizens and other community stakeholders. Police strive to deploy resources to resolve the effects of crime on neighborhoods, not to resolve the effects of neighborhoods on crime.

Bringing Definitions Together

Defining neighborhoods involves integrating the perspectives of police administrators, local government personnel, and community members to create working neighborhood boundaries for deploying police resources that capture the essence of the place, yet effectively serve all constituencies involved. This task is by no means easy, nor is it finished once achieved. The research community, in particular, needs to continue to help the police understand what neighborhood characteristics can be used to inform the police planning process within boundaries that best represent a serviceable neighborhood.

Police, in turn, need to apply information technologies[5] to capture local ground truth that leads to the subtle characteristics of place that researchers need to differentiate associated factors to crime and disorder, as well as—potentially—cause and effect.

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[4] The Chicago Police Department divides the city into 23 patrol districts for deployment and administrative purposes. Detective and specialized units operate citywide or by grouped (that is, multiple districts) geographical distributions. Each district has between 9 and 15 beats, each staffed by one or two police officers 24 hours a day. The beat is both a unit of analysis and a response unit. Workload variations based on the time of day require additional patrol units within a district. Chicago's district is equivalent to the New York Police Department's precinct and the Los Angeles Police Department's division.

[5] That is, they need to use Geographic Information Systems.

Static geography used for responses and reporting can now be made more dynamic because of near-realtime information about police workloads and community needs. The beat may be passé. Incorporating geographic data when making resource deployment decisions enables the police to become more responsive to each neighborhood's particular needs. This approach is a useful component of intelligence-led policing (Ratcliffe, 2008), a broad, strategic approach to making deployment decisions for the provision of public safety.

Still, police continue to struggle with decisions of resource deployment based on need. I would suggest that the police listen to both researchers and citizens when trying to understand what elements define a neighborhood. The differences in definitions of neighborhood are not in conflict, but are rather the same landscape viewed through different lenses.

Acknowledgments

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Geographic Patterns of Serious Mortgage Delinquency: Cross-MSA Comparisons

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Abstract

This article examines the distribution of impaired mortgages across neighborhoods, defined at the ZIP Code level, in 91 metropolitan areas as of the fourth quarter of 2008, well into the recent U.S. mortgage crisis. We catalogue serious mortgage delinquency patterns by metropolitan area based on features of the geographic distribution, including measures of dispersion across neighborhoods and of spatial autocorrelation. The findings are potentially informative for assessing local and neighborhood consequences of the mortgage crisis and for selecting and implementing strategies to ameliorate the effects of foreclosure.

Introduction

The tremendous volume of mortgage delinquencies and foreclosures since 2007 is an ongoing national crisis, but fashioning an appropriate policy or private-sector response requires assessing the local manifestations of the crisis. That the appropriate response depends on the neighborhood distribution of seriously delinquent mortgages in a metropolitan area—the extent to which such mortgages are concentrated in high-foreclosure neighborhoods and whether the latter are sparse or numerous, and are clustered together, dispersed, or isolated—has become increasingly clear.

For example, Goldstein (2010) introduced a data-based tool labeled "Market Value Analysis" that can be used to target public-sector and nonprofit neighborhood stabilization funds.[1] The author emphasized that "targeting places where the problem is manageable and the surrounding markets have strength is critical to success" (Goldstein, 2010: 73). An illustrative application to the city of Philadelphia identified neighborhoods where vacancy and foreclosure were geographically confined so that interventions are likely to succeed.

This article surveys and classifies the variety of spatial patterns of serious delinquency observed across U.S. metropolitan areas. The article's primary objectives are to highlight important differences in the spatial distribution of mortgage delinquency across metropolitan areas and to promote discussion of what public- and private-sector strategies are most suitable in each context. In particular, our typology may facilitate information sharing among cities with similar circumstances.

Secondarily, the article examines some housing market and economic conditions associated with the different spatial patterns. Although overall delinquency rates are highest in cities with large house price declines or high unemployment rates, this examination highlights how most other cities have high-delinquency pockets, mostly because of subprime lending concentrations.

Specifically, this article examines the mortgage delinquency distribution across neighborhoods, defined at the ZIP Code level, within U.S. metropolitan statistical areas (MSAs) as of the fourth quarter of 2008, well into the mortgage crisis. The results classify metropolitan areas into six groups:

1. Low-to-moderate mean and high spatial autocorrelation: a modest number of high- or moderately high-delinquency neighborhoods that are clustered together.

2. High mean and standard deviation: wide variation across neighborhoods, with most delinquencies occurring in distressed neighborhoods.

3. High positive skewness: mostly multiple high-delinquency neighborhoods, some with extremely high delinquency rates.

4. Low-to-moderate mean, high positive skewness, and steep gradient around the peak delinquency neighborhood: a modest number of neighborhoods distinguished by high delinquency rates, including at least one spatial outlier.

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[1] The analytical approach constructs a set of neighborhood indicators, such as foreclosure and vacancy rates, assessed at the census block-group level, and uses them to cluster neighborhoods into categories reflecting dimensionality and degree of distress.

5. Steep gradient around the peak delinquency neighborhood, indicating at least one spatial outlier: in general, isolated problem neighborhoods.

6. All other cities: somewhat more varied, but generally exhibiting moderate mean and low-to-moderate standard deviation of spatial delinquency.

This article contributes to a developing literature analyzing foreclosures and REO (Real Estate Owned) properties from a geographic perspective and deriving implications for neighborhood stabilization strategy.[2] Immergluck (2009) classified metropolitan areas based on the level and change in density of REO properties from 2006 through 2008 and compared REO accumulation across central city and suburban locations. The analysis highlighted three types of metropolitan areas: (1) areas with low-to-moderate initial REO densities and stable prices, (2) those with initially high REO density and either stable prices or declines in value and increases in REO density from 2006 through 2008, and (3) "boom and bust" areas characterized by steep declines in home values accompanied by rising REO density over this period. The latter category tended to have higher REO concentrations in suburban areas.[3] The author emphasized that "understanding the accumulation of REO inventories across and within metropolitan areas is important for formulating policies and informing community development practice regarding how to stabilize communities and neighborhoods that have been affected by surging foreclosures and vacant properties" (Immergluck, 2009: 28).[4]

Immergluck (2010a) revisited the subject, drilling down to the neighborhood (ZIP Code) level to investigate factors affecting REO accumulation from 2006 through 2008. The analysis indicated that the locations of high-risk lending activity and rapid housing development explain most of the urban-versus-suburban distribution of REO accumulation across metropolitan areas.[5] Edmiston (2009) examined factors associated with foreclosure rate differences across census tracts within the 10th Federal Reserve District as of year-end 2008.[6] The analysis found that concentrations of foreclosures in lower income areas are explained by concentrations of subprime mortgages.

The analysis in this article proceeds as follows. We first calculate distributional moments of the ZIP Code-level delinquency rates, and several measures of their spatial distribution across ZIP Codes. We next conduct a cluster analysis (using the principal component measures) to determine metropolitan area groupings based on common geographical patterns. Finally, we conduct a principal components regression analysis, exploring the relationship of these distributional moments and spatial measures (reduced to their principal components) to subprime lending patterns and economic factors.

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[2] REO properties are those that have been acquired by lenders via foreclosure.

[3] The analysis also indicates that among suburban ZIP Codes, those with long commute times experienced larger REO increases over the November 2006-to-2008 period than those with shorter commute times.

[4] For instance, the paper suggests, as an implication of disproportionate REO shares in ZIP Codes with long commute times, that "it may be unwise to spend scarce resources attempting to redevelop residential patterns that may not be highly sustainable in the context of more conservative mortgage markets or higher long-term energy and transportation costs" (Immergluck, 2009: 28).

[5] Immergluck (2010b) examined both levels of and changes in REO activity from August 2006 through August 2008 across metropolitan areas, particularly in relation to changes in home values and the legal environment affecting foreclosures.

[6] The 10th Federal Reserve District consists of Colorado, Kansas, Nebraska, Oklahoma, Wyoming, and parts of western Missouri and northern New Mexico.

The article is organized correspondingly. The next section describes our data sources. The section on geospatial characterization follows with the calculation of the distributional moments and spatial measures and their principal components. The section following that presents the cluster analysis, emphasizing the implications of the results for developing appropriate policy responses. The principal component regression analysis precedes the concluding section.

Data Sources

We draw data for the study from several sources. We obtain estimates of prime and subprime mortgage delinquency rates as of October 2008 by ZIP Code, using the CoreLogic TrueStandings Servicing® online data analytics tool.[7] This online business intelligence platform accesses the prime and subprime mortgage databases of CoreLogic. These databases provide current information on the payment status of active mortgages serviced by the top mortgage-servicing institutions or sold to Fannie Mae or Freddie Mac.[8] Historical information for both paid-off and active loans is also available, by origination month, as are the state, county, and ZIP Code location of the financed property. We restrict our attention to first-lien, conventional mortgages. For this article, we define delinquency as 60 or more days past due.[9]

The CoreLogic databases do not provide a full count of all active mortgage loans in all ZIP Codes, because not all institutions that service mortgages contribute to these databases. Therefore, we adjust the active loan counts from the CoreLogic servicing data based on an estimate of the under-count in each ZIP Code.[10] Specifically, we measure the undercount by comparing the number of 2005 and 2006 mortgage originations in the CoreLogic data against the number reported to federal regulatory authorities in Home Mortgage Disclosure Act (HMDA) data. The procedure is discussed in greater detail in the following section.

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[7] Information about TrueStandings Servicing® is available at http://www.corelogic.com.

[8] The loans CoreLogic assigns to its subprime database are either serviced by institutions that specialize in servicing sub-prime loans or identified as subprime by the servicing institution. Despite the recent demise of most subprime-specializing institutions, the subprime database continues to track active subprime loan performance because the servicing of these loans has largely transferred to other institutions that contribute to the database. In contrast to CoreLogic's more commonly used, loan-level subprime securities database, the subprime-servicing database provides information on loans retained in bank portfolios as well as those in securities.

[9] Although adverse neighborhood effects generally are associated with properties in later stages of foreclosure and REO, we favor including all loans 60 or more days past due in our analysis of delinquency patterns, for several reasons. First, foreclosure moratoria and loan modification programs have artificially slowed the transition through foreclosure into REO, so that our measure may be a better indicator of actual "facts on the ground." Second, our measure is somewhat forward looking, because most loans in early stages of delinquency as of the analysis date will move into later stages of foreclosure and REO, given the relatively low cure rates associated with the mortgage crisis. Third, early stages of delinquency are relevant when considering effective policy responses. Moreover, because the 60-plus-days-past-due measure is dominated by longer term delinquent loans that are in foreclosure or REO, and because neighborhoods with lower delinquency rates in general will also have higher cure rates, we would not expect classifications based on longer term delinquency to be much different from those arising from our cluster analysis.

[10] Although CoreLogic takes steps to eliminate duplication, some duplicate reporting of loans may occur in the data obtained from Fannie Mae and Freddie Mac by the servicers of these loans. In some ZIP Codes, we observe excess counts and adjust these counts, as well.

Thus, we develop estimates of active loan counts, prime and subprime, as of October 2008, by ZIP Code. We aggregate the estimated prime and subprime delinquency rates and active loan counts to obtain estimates of overall mortgage delinquency rates by ZIP Code.

We use additional data sources to obtain explanatory variables for the regression analysis of metropolitan-area delinquency characteristics. We use 2005, 2006, and 2007 HMDA data to construct variables descriptive of the mortgage market in a metropolitan area, such as share of home purchase loans by occupancy type (owner *vs.* nonowner occupied). We rely on Economy.com for data describing local economic and housing market conditions from 2005 through 2008, including annual house price appreciation rates, annual changes in housing starts, affordability index, and unemployment rates by MSA.

Estimating Active Loan Counts by ZIP Code

As discussed previously, we adjust the active loan counts from the CoreLogic servicing data by comparing 2005 and 2006 origination counts in the CoreLogic data with origination counts from HMDA data. Because the CoreLogic data provide the state, county, and ZIP Code associated with a mortgage, whereas HMDA data indicate the state, county, and census tract, not the ZIP Code, we first map state, county, and census tract into ZIP Code(s).[11] We apply separate adjustments to prime and subprime loan counts, associating high-cost mortgages in HMDA data (those with a reported above-prime rate spread) with subprime.

Let n_j denote the number of originations reported in the CoreLogic subprime servicing data, and let N_j denote the number of subprime (high-cost) originations in HMDA data, for ZIP Code j in 2005 through 2006. Our adjustment factor is then the ratio $\alpha_j = n_j/N_j$. We multiply the 2008 active loan count in the CoreLogic subprime servicing data by α_j to obtain the estimated active subprime loan count for ZIP Code j. We apply the analogous procedure to estimate active prime loan counts. ZIP Codes with fewer than 50 estimated total (prime plus subprime) active loans are excluded from the study.[12]

Note that this procedure assumes that the within-ZIP delinquency rates observed for subprime loans included in the CoreLogic subprime servicing data are representative of the aggregate (observed and unobserved) within-ZIP delinquency rate; we make the same assumption regarding the prime data. Likewise, this procedure assumes that the servicing databases are representative with respect to within-ZIP proportions of 2005-to-2006 originations that remain active in 2008. Although assessing the accuracy of these assumptions is not possible, the fact that we are holding constant both geographic (ZIP Code) location and risk category (prime versus subprime) provides some assurance that the observed quantities will be reasonable approximations. At the least, correcting for the undercounts is preferable to not doing so.

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[11] Where a census tract traversed more than one ZIP Code, we allocated the mortgages across the ZIP Codes in proportion to the loan counts observed in Freddie Mac internal data.

[12] We also exclude ZIP Codes where α_j is implausibly large or small. In addition, we apply consistency checks for the prime active counts using Freddie Mac internal data. For instance, if the estimated active prime loan count for a ZIP Code is less than the number of active loans in Freddie Mac data, we use the active loan count and delinquency rate from Freddie Mac data instead.

MSA Selection

As defined by the Office of Management and Budget, 371 MSAs were in the United States, as of December 2006.[13] To limit the scope of this study to major cities and to ensure the statistical relevance of the measures calculated at the ZIP Code level, we select the 88 MSAs with at least 50 ZIP Codes or 100,000 active mortgages in our data. We include an additional 3, marginally smaller MSAs (Knoxville, Tennessee, Boise, Idaho, and Sioux Falls, South Dakota) to achieve better geographic representation. In Appendix A, we provide the complete list of selected MSAs and the number of ZIP Codes and active mortgages in each.

Large MSAs usually contain several cities along with the suburban areas around the cities. For simplicity, we abbreviate the full name of an individual MSA in the following text by referring to the major city in the MSA. For example, we refer to the New York-Northern New Jersey-Long Island MSA as "New York." Note that we include as part of an MSA any ZIP Codes that extend beyond the MSA boundary into adjacent non-MSA areas.

Geospatial Characterization

In this article, we address how delinquent loans, as of October 2008, in individual MSAs were distributed in relation to neighborhood delinquency rate, and whether any generalized patterns emerge across MSAs. Using the ZIP Code-level data described previously, we calculate eight MSA distributional statistics to quantify the patterns in a standardized way. These distributional statistics become the basis of cross-MSA comparisons and analysis.

Note that the focus is the distribution of delinquent loans in relation to neighborhood delinquency rate, not the distribution of the overall population of mortgage borrowers, homeowners, or households in relation to neighborhood delinquency rate. Although these distributions will tend to be similar, we view the former as more relevant for policy analysis addressing the mortgage crisis. For example, the share of a city's delinquent mortgages contained in high-delinquency neighborhoods is a more important consideration for judging the relevance of the neighborhood dimension than the share of the city's population located in these neighborhoods.

From a policy perspective, characterizing the shape of the distribution is of interest; for example, knowing whether neighborhoods with extremely high delinquency rates comprise a long tail may be important. Initially, we attempted to fit metropolitan-area delinquency distributions to two-parameter lognormal or beta functional forms. In many cases, however, the data do not conform to these distributions and require greater flexibility in fitting the mean, standard deviation, and shape characteristics (skewness and kurtosis) of the distributions. Therefore, we calculate four descriptive statistics characterizing how the delinquent mortgages in an MSA are distributed in relation to the neighborhood delinquency rate: mean, standard deviation, skewness, and kurtosis.

These moments characterize the delinquent loan distributions across individual ZIP Codes but have no spatial component. The extent to which high-delinquency neighborhoods are spatially isolated, dispersed, or clustered is also of interest from a policy perspective. For example,

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[13] See OMB (2006) for more detail.

clustering may imply that delinquency problems are contained (or containable) within a limited geographical area and likely require neighborhood-specific responses. Therefore, we also calculate four gradient and spatial autocorrelation measures, which indicate spatial aspects of the neighborhood delinquency distribution.

We calculate the *mean* as the mean neighborhood delinquency rate for all the delinquent loans in the MSA. Because our data are at the ZIP Code level, we represent neighborhood by ZIP Code and calculate the mean as the weighted average ZIP Code delinquency rate, weighting by number of delinquent loans in the ZIP Code. Note that this is *not* equivalent to the overall measured delinquency rate for the MSA, which we would obtain by weighting by number of active loans.

We use the same weighting concept to calculate standard deviation, skewness, and kurtosis. Note that the standard deviation from this calculation is small because each loan in the same ZIP Code is assigned the same delinquency rate. Therefore, the deviation among delinquent loans in the same ZIP Code is 0; the measure captures only the deviation among the ZIP Codes.

We compute weighted skewness (tendency of the deviations to be larger in one direction) as[14]

$$Skewness = \frac{n}{(n-1)(n-2)} \sum w_i^{3/2}((x_i - \bar{x})/\hat{\sigma})^3, \tag{1}$$

where *n* is the number of ZIP Codes, *w* is the number of delinquent loans in a ZIP Code, *x* is the ZIP Code delinquency rate, and σ is the standard deviation of the delinquency rate distribution. We compute weighted kurtosis (heaviness of the tail of the distribution) as

$$Kurtosis = \frac{n(n+1)}{(n-1)(n-2)(n-3)} \sum w_i^2((x_i - \bar{x})/\hat{\sigma})^4 - \frac{3(n-1)^2}{(n-2)(n-3)}. \tag{2}$$

MSAs with high kurtosis usually have high skewness as well; details are provided in the following paragraphs.

Gradient. We calculate two measures of gradient—greatest rate of change in delinquency rate between the ZIP Code with the highest (peak) delinquency rate and neighboring ZIP Codes.[15] When we restrict attention to ZIP Codes directly adjacent to the peak-delinquency ZIP Code, we obtain what we call the "first-layer gradient." We obtain the "second-layer gradient" by focusing on those ZIP Codes adjacent to the directly adjacent ZIP Codes (those that touch the boundaries of the first layer). Specifically,

$$FirstLayerGradient = \underset{i=1...n}{Max}(D_i - D_{Max})/D_{Max}, \tag{3}$$

$$SecondLayerGradient = \underset{j=1...k}{Max}(D_j - D_{Max})/D_{Max}, \tag{4}$$

where D_{Max} is the highest ZIP Code delinquency rate in the MSA, D_i is the delinquency rate of the *n* ZIP Codes adjacent to the ZIP Code with the highest delinquency rate, and D_j is the delinquency rate of the *k* ZIP Codes adjacent to the *n* first-layer ZIP Codes.

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[14] See "The Univariate Procedure—Descriptive Statistics" from SAS 9.1.3 Online Documentation (The SAS Institute, 2003) at http://support.sas.com/onlinedoc/913/docMainpage.jsp.

[15] In calculus, the gradient of a vector field is the vectors that point in the direction of the greatest rate of increase, with magnitude equal to the greatest rate of change.

A steep gradient suggests that high-delinquency neighborhoods are more isolated or extreme. An MSA with flat first- and second-layer gradients is likely to have a broad region of high-delinquency neighborhoods. An MSA without any high-delinquency-rate areas will have low gradient measures.[16]

Spatial Autocorrelation

Spatial autocorrelation refers to the degree to which observations from nearby locations (in our context, nearby ZIP Codes) are more likely to have similar magnitude (similar delinquency rate) than by chance alone (Fortin, Dale, and ver Hoef, 2002). We calculate two spatial autocorrelation measures: Moran's I and Geary's C.[17]

Moran's I measures autocorrelation with respect to deviations between individual values of the spatial variable and the mean value:

$$I = \frac{\sum\sum W_{ij}(X_i - \bar{X})(X_j - \bar{X})/W}{\sum(X_i - \bar{X})^2/n}, \tag{5}$$

where X stands for the ZIP Code delinquency rate, i and j represent any pair of ZIP Codes in the MSA, W_{ij} is a weight that declines with the distance between the centroids of ZIP Codes i and j; W is the sum of W_{ij}, and n is the total number of ZIP Codes in the MSA. Moran's I takes a value between -1 and 1, where 1 means perfectly positive correlation and 0 represents random distribution.

Geary's C measures autocorrelation directly with respect to differences between individual values of the spatial variables. Geary's C is computed as

$$C = \frac{\sum\sum W_{ij}(X_i - X_j)^2/2W}{\sum(X_i - \bar{X})^2/(n-1)}. \tag{6}$$

It varies from 0 for perfect positive autocorrelation to about 2 for a strong negative autocorrelation. If correlation is absent, the expected value equals 1.

A low value of Geary's C corresponds to a high value of Moran's I, both indicating a high degree of spatial autocorrelation. Moran's I is a global indicator, whereas Geary's C is more sensitive to local differences across neighborhood pairs. In general, Moran's I and Geary's C will agree on the existence of spatial autocorrelation, but not necessarily on the magnitude.

Exhibit 1 shows the summary statistics for the eight analysis variables. The mean value across the 91 MSAs of the MSA mean variable is about 0.08, and the mean skewness is about 1.6, consistent with substantial positive skewness for most MSAs.

[16] The gradient measures apply only to the neighborhoods surrounding the ZIP Code with the highest delinquency rate. If a large MSA has multiple pockets of high-delinquency areas, the gradient measures will describe only one of them. Also, the ZIP Code size may affect the gradient measure, as does the delinquency rate differential across neighborhoods; for instance, larger ZIP Codes may mask substantial within-ZIP variation. Nevertheless, the results of our cluster analysis that follows suggest that the gradient measure is an effective tool for identifying metropolitan areas where high-delinquency neighborhoods tend to be more isolated.

[17] Much of our discussion of these spatial autocorrelation measures is drawn from Fortin, Dale, and ver Hoef (2002) and Lembo (2008).

Exhibit 1

Summary Statistics of Analysis Variables

ZIP Code Variable	Mean	Standard Deviation
Mean	0.079	0.034
Standard deviation	0.005	0.002
Skewness	1.566	0.921
Kurtosis	5.711	7.170
First-layer gradient	0.261	0.181
Second-layer gradient	0.329	0.198
Moran's I	0.142	0.113
Geary's C	0.946	0.185

The mean values of the spatial autocorrelation measures (0.14 for Moran's I and 0.95 for Geary's C) suggest that spatial autocorrelation in each city, in general, is not high. These values may be somewhat misleading, however, because we define neighborhoods rather broadly, at the ZIP Code level. Thus, the measure does not reflect the degree of homogeneity of smaller neighborhood units sharing the same ZIP Code. A higher degree of spatial autocorrelation likely would be observed with a narrower definition of neighborhood, such as the census tract level.[18]

Appendix B reports the mean and skewness statistics and the Moran's I spatial autocorrelation measure for each of the 91 MSAs. Although we do not use it in classifying metropolitan-area delinquency patterns, appendix B also includes the Gini coefficient, a measure of inequality in the spatial distribution of delinquent loans.[19] It is calculated using the following formula, where X is the cumulated portion of active loans and Y is the cumulated portion of delinquent loans across ZIP Codes, ordered by number of delinquent loans:

$$G = 1 - \sum_{k=1}^{n} (X_k - X_{k-1})(Y_k + Y_{k-1})$$ (7)

A Gini coefficient equal to 0 indicates that delinquent loans in the MSA are distributed exactly in proportion to active loans. The greater the Gini coefficient, the more likely some ZIP Codes contain a disproportionate share of delinquent loans relative to active loans.[20]

Principal Component Analysis

The multidimensionality associated with the full set of descriptive statistics introduced in the previous sections would confound an effort to analyze delinquency patterns or to draw intuitively meaningful comparisons across metropolitan areas. Moreover, a high correlation exists among these measures, especially among those that quantify related, but not identical, aspects of the

[18] Moreover, the spatial autocorrelation measures indicate the overall degree of spatial autocorrelation, not specifically the degree to which neighborhoods in the high-delinquency tail of a distribution are clustered. Nonetheless, they may be useful as relative measures for comparing spatial patterns across metropolitan areas.

[19] The Gini coefficient is commonly used along with the Lorenz Curve to measure income distribution inequality (Litchfield, 1999).

[20] Among the 91 selected MSAs, the Gini coefficient has a mean value of 0.20 and a standard deviation of 0.09. San Francisco has the highest Gini coefficient (0.47).

distribution (exhibit 2). These correlations complicate the description of delinquency patterns and impart redundancy to an analysis conducted using the full set of descriptive measures.

For example, the mean and standard deviation have a 0.72 correlation, whereas skewness and kurtosis are 92-percent correlated. The variables associated with the spatial aspects of the distribution also are highly correlated with each other and with the standard distribution moments. At the 5-percent significance level, 11 pairings of the 8 focus variables have significant Pearson correlation measures.

Therefore, in anticipation of conducting classification (cluster) and regression analyses of geographic delinquency patterns, we reduce dimensionality by applying principal component analysis (PCA). PCA is often applied in the economic geography literature to reduce the number of variables used to describe and group cities or places along a number of socioeconomic dimensions without losing all the information contained in the numerous variables of interest (Vicino, Hanlon, and Short, 2007).[21] In this application, we use PCA to reduce the number of variables used to describe a metropolitan area from eight measures to four principal components. The principal components essentially are indices that enable us to describe delinquency rate patterns that vary across metropolitan areas, reducing the dimensions of analysis without significant loss of information contained in the original set of analysis variables.

PCA identifies components that are linear combinations of the original variables (factors), orthogonal, and ordered with respect to proportion of variance in the data that is explained. Using PCA with eight variables results in eight components. We reduce the number of components used in the analysis to four by examining the eigenvalues of each component and the proportion of variance explained by each component. We can also provide an interpretation of each component by examining the loadings (weights) on each factor. We choose components that account for a greater variance than any single variable contributes, as captured by an eigenvalue greater than 1. Otherwise, the component accounts for less variance than what is attributed to individual variables.

Exhibit 2

Correlation Coefficients of Analysis Variables

	Mean	Standard Deviation	Skewness	Kurtosis	First-Layer Gradient	Second-Layer Gradient	Moran's I	Geary's C
Mean	1.00							
Standard deviation	0.72*	1.00						
Skewness	(0.16)	(0.10)	1.00					
Kurtosis	(0.11)	(0.13)	0.92*	1.00				
First-layer gradient	(0.39)*	(0.19)	0.07	0.08	1.00			
Second-layer gradient	(0.28)*	(0.13)	0.27	0.24*	0.55*	1.00		
Moran's I	0.08	0.26*	(0.08)	(0.12)	(0.15)	(0.17)	1.00	
Geary's C	0.06	0.01	0.36*	0.31*	(0.15)	0.05	(0.30)*	1.00

* Significant at the 5-percent level.

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[21] Researchers have also used PCA, for example, to develop neighborhood quality indices as a function of neighborhood characteristics (Can, 1992) and to include a composite measure of neighborhood quality in house price index construction (Can and Megbolugbe, 1997).

Exhibit 3 reports the eigenvalues associated with each component. An inspection of the eigenvalues shows that the first four components have eigenvalues greater than 1.00 and the fifth component's eigenvalue is only 0.59.

Exhibit 3 also shows the proportion of the variance in the data that each component captures. The first two components account for more than one-half of the variation, and the third and fourth components account for nearly one-third of the variation. Each remaining component accounts for less than 10 percent of the total variation in the data, and omitting them is consistent with the analysis of the eigenvalues.

Examining the factor coefficients (loadings) can yield a high-level interpretation for each of the first four components. The fact that each component has at least two significant loading variables, whereby the variables with the largest coefficients are conceptually related, facilitates interpretation. In the first component, the skewness and kurtosis—both measures related to the tails of the delinquency distribution—have coefficients near 0.50. Thus, this component is viewed as a skewness/kurtosis component. With the second component, the mean and standard deviation are the most relevant coefficients; they contribute 0.52 and 0.59, respectively. The third component is most related to the spatial gradient measure, with our two variations of the gradient having the largest coefficients, about 0.55 each. The fourth component is the measure of autocorrelation captured by Moran's I and Geary's C. The coefficients of C and I have opposite signs, consistent with the negative and statistically significant correlation between the two measures.

Exhibit 3

Principal Component Analysis Results

Component	Eigenvalue	Percent of Variance	Cumulative Percent
1	2.59	32	32
2	1.85	23	55
3	1.18	15	70
4	1.08	14	84
5	0.59	7	91
6	0.41	5	96
7	0.22	3	99
8	0.07	1	100

Cluster Analysis

Each of the four principal components from the PCA has a specific value, or "component score," that equals the weighted sum of the original eight distributional measures, whereby the weights are the factor loadings. To classify MSAs based on delinquency patterns, we conduct a k-means cluster analysis of these component scores (Derudder et al., 2003).

Examining the clusters obtained under alternative specifications of number of groupings, we find that six clusters are most satisfactory. Appendix C lists the metropolitan areas by cluster.

Broadly speaking, the groupings suggested by the cluster analysis reflect the degree to which an MSA's delinquent mortgages are concentrated in high-foreclosure neighborhoods and the spatial pattern of those neighborhoods: congregated, dispersed throughout the MSA, or relatively few and isolated.

To help characterize the clusters and enable us to visualize how delinquency patterns vary across metropolitan areas in relation to component scores, we create density plots selected as examples for each cluster. The density plots presented in exhibit 4 show how delinquent mortgages in each MSA are distributed in relation to the ZIP Code delinquency rate. They provide a visual reference for components 1 and 2, which are closely associated with this distribution.

The bars in each chart represent the proportion of delinquent loans associated with each neighborhood delinquency rate band, whereby we apply a 1-percentage-point bandwidth. For example, about 5 percent of the delinquent loans in Miami are located in ZIP Codes with a delinquency rate of between 9 and 10 percent, whereas about 10 percent are associated with a neighborhood delinquency rate between 17 and 18 percent.

We also created and examined density maps highlighting the range of ZIP Code delinquency rates through color coding. These maps provide a visual reference for spatial autocorrelation and gradient (closely associated with components 3 and 4). We are unable to reproduce them here, but note some of our observations in the following discussion.[22]

Exhibit 4

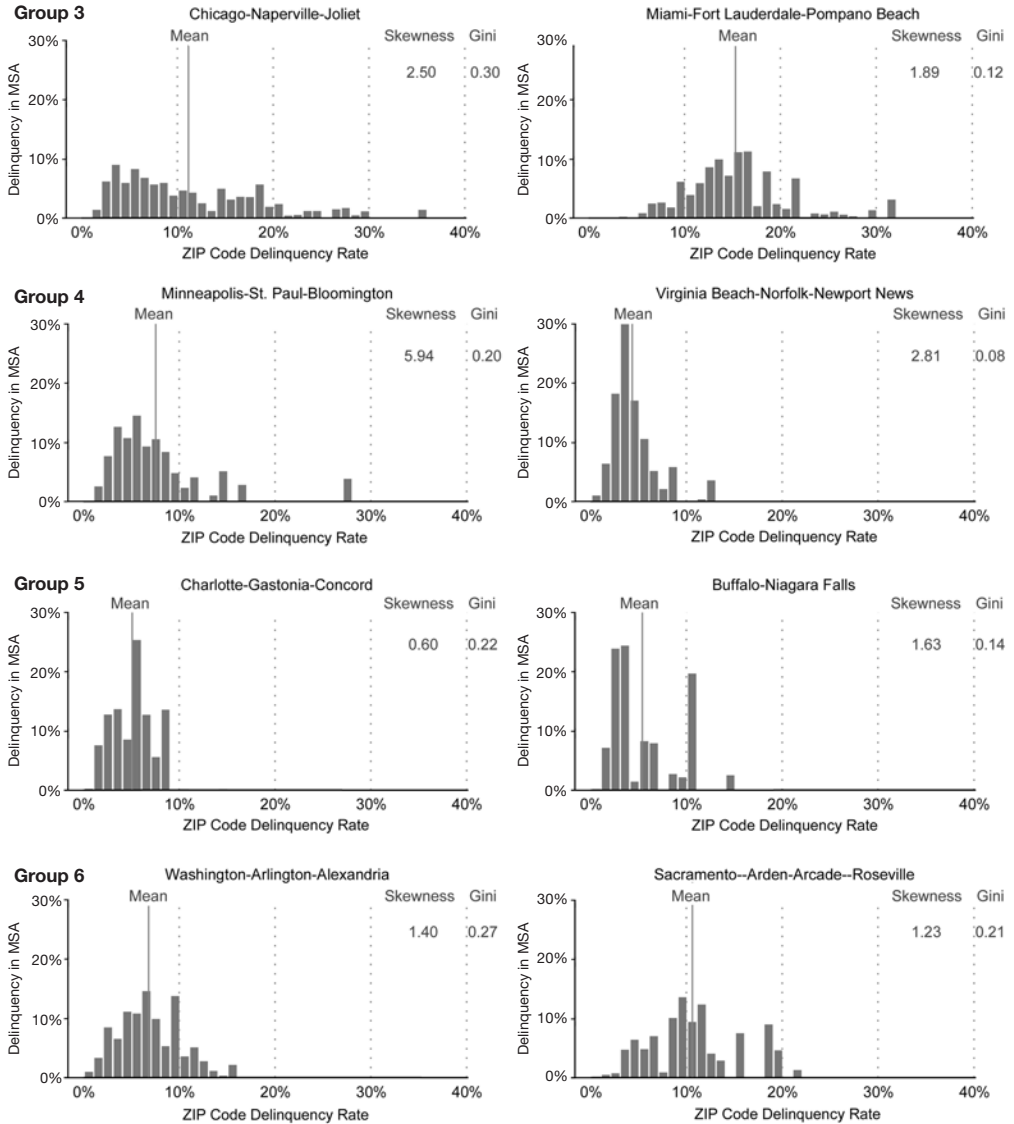Loan-Level Delinquency Density by ZIP Code Delinquency Rate (1 of 2)

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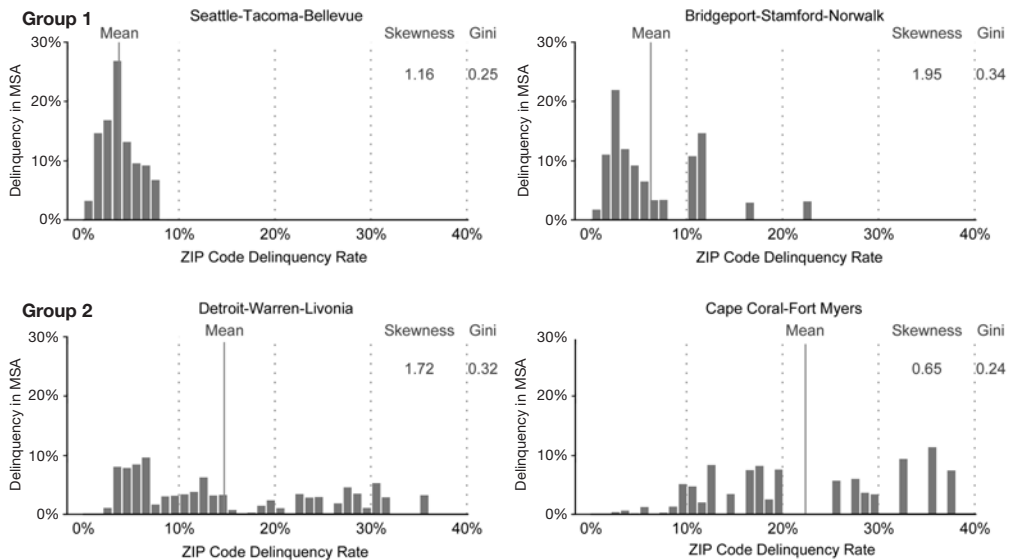
[22] The authors will provide the maps on request.

Exhibit 4

Loan-Level Delinquency Density by ZIP Code Delinquency Rate (2 of 2)

MSA = metropolitan statistical area.

Group 1

The first cluster analysis grouping consists of MSAs with high spatial autocorrelations and low or moderate delinquency rate means. These MSAs contain a modest number of high- or moderately high-delinquency neighborhoods that are clustered together or comprise a distinct pocket of neighborhoods within the MSA. Examples include Austin, Raleigh, and (as of the third quarter of 2008 analysis date) Seattle.[23]

The density plots associated with this group, as illustrated by those of Bridgeport and Seattle, are relatively compact, with most of the mass in low-delinquency neighborhoods. The distinguishing characteristic of this group, spatial clustering of the higher delinquency neighborhoods, is not evident from the density plots but is observable in density maps. For example, for the Bridgeport metropolitan area, we observe a distinct, concentrated pocket of high delinquency in the urban core. Throughout the remainder of the MSA, we observe lower delinquency rates.

In general, these metropolitan areas have relatively stable housing market and economic environments overall; foreclosure rates in the higher delinquency neighborhoods may or may not rise to a level of concern. Neighborhood effects of delinquency and foreclosure, to the extent they are a concern, would be limited to the higher delinquency pockets, which should then receive particular attention.

We would advise first assessing the potential for effects on house values in adjacent neighborhoods that could cause the foreclosure problem to expand, and taking countermeasures as needed.[24] Targeted use of Neighborhood Stabilization Program (NSP) funds to acquire and rehabilitate properties close to the boundaries of the high-foreclosure area is a possible containment strategy.

In many, if not most, cases, the high-foreclosure pocket will consist of neighborhoods where subprime lending was concentrated (the regression analysis in the following section provides some empirical support for this statement). Thus, strategies to prevent foreclosure, such as loan modification to reduce the payment burden on households with high-cost subprime loans, could help stem neighborhood decline. The high-delinquency pocket may also need to be the focus of efforts to mitigate adverse neighborhood effects of REO and vacant properties, applying the kinds of strategies discussed at length in Fleischman (2010), Ryan (2010), and others in the same volume.[25]

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[23] Housing values in Seattle declined substantially and unemployment rose after the third quarter of 2008. As a result, the current delinquency distribution for Seattle likely is different from that in our data, with mean delinquency higher.

[24] Negative externalities associated with foreclosures include lower prices for nearby properties, reduced local property tax base, and high crime rates. Kingsley, Smith, and Price (2009) include a survey of the literature regarding the effect of foreclosures on families and communities.

[25] These strategies include (1) use of public- and nonprofit-sector resources to acquire and rehabilitate foreclosed properties, including bulk acquisitions; (2) partnerships of public-sector and nonprofit agencies with mortgage lenders and servicers to facilitate the sale of REO properties to owner occupants, particularly first-time homebuyers, or to existing occupants (tenants or former owners); (3) partnerships of public-sector and nonprofit agencies with mortgage lenders, servicers, and investors to develop viable REO rental or rent-to-own options for former owners or for existing or new tenants; (4) property code enforcement to mitigate the adverse neighborhood effects of vacancy and abandonment, and legal strategies to facilitate lien transfers to parties willing to perform maintenance or rehabilitation; and (5) demolition of vacant properties and planning for long-term reuse and redevelopment of vacant lots.

In some cases, particularly if the high-foreclosure pocket is an area where overdevelopment led to severe home value declines, market-driven recovery may be the best option. Home value declines may suffice to bring homebuyers back into the community as owner occupants, or to attract private investors who see an opportunity to rehabilitate properties for rental or resale.[26]

Group 2

The second grouping from the cluster analysis exhibits a high mean and standard deviation for delinquency rates. These cities have wide variation across neighborhoods, with most delinquencies occurring in distressed neighborhoods.

Metropolitan areas in this group include Cape Coral, Detroit, Memphis, Palm Beach, and Stockton. Although they may have some spatial concentrations, high means and very high-delinquency-rate areas in the right tail of the distribution are their most prominent features, as illustrated by the density plots for Cape Coral and Detroit. Widespread occurrence of moderate-to-high delinquency rates characterizes the density maps for these metropolitan areas.

The large number and broad swath of neighborhoods affected by high and very high delinquency necessitate a citywide or regional planning perspective, in contrast with the neighborhood focus associated with Group 1. Strategies to address foreclosure and REO, such as developing viable REO rental or rent-to-own options for former owners or for existing or new tenants, will have to be scalable. Using public- and nonprofit-sector resources directly to acquire and rehabilitate fore-closed properties is unlikely to be an effective strategy, given the scale and scope of the problem. Redevelopment plans may need to incorporate demolition of vacant and abandoned properties and planning for long-term reuse and redevelopment of vacant lots, a strategy that is being used effectively in Cleveland, for example.

The cities in Group 2 contain neighborhoods with extremely high delinquency rates, where as many as one out of every three or four borrowers is delinquent. Restoring stability to these neigh-borhoods will be a special challenge, requiring particularly intensive or imaginative strategies.

These metropolitan areas also are characterized by steep declines in house prices or by high unemployment. The extent of market-driven recovery will be tied to long-term population and employment prospects for the city or region.

Group 3

A third grouping is distinguished by a highly positively skewed, long, or fat-tailed distribution. Most metropolitan areas in this group have mean delinquency rates in the moderate range and multiple high-delinquency neighborhoods, which may be clustered together. Delinquency rate dispersion is more one-sided than for Group 2, as reflected in the measures of skewness and kurtosis. This group includes Atlanta, Baltimore, Chicago, Miami, and New York.

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[26] Local economic trends, property age and condition, and long-run neighborhood conditions such as high vacancy rates before the mortgage crisis will influence the extent or pace of market-driven recovery, as emphasized by Newburger (2010).

Miami is an example of a metropolitan area with both widespread high delinquency and substantial positive skewness. It has by far the highest mean delinquency rate among cities in Group 3 and is closer to Group 2 in this respect. Chicago is more typical of Group 3. A delinquency map of Chicago shows many high-delinquency neighborhoods, mostly clustered on the city's south side and into neighboring areas southeast of the city, including Gary, Indiana. Chicago has a kurtosis value of 12 and Miami's measured kurtosis is 7.9, both well above the 5.7 sample average or the 3 associated with a normal distribution.

In the case of Miami, where high delinquency rates are widespread throughout the city and its environs, a regional perspective is required, as with the cities in Group 2. In a city more typical of Group 3, such as Chicago, the focus can be on the neighborhoods constituting the high-delinquency tail of the distribution.

Most metropolitan areas in this group have numerous high-delinquency-rate neighborhoods, requiring a planning perspective that encompasses sizable sections of the city or region. In these cases, strategies to address foreclosure and REO will have to be scalable, as discussed for Group 2.[27] The cities in Group 3, like those in Group 2, contain neighborhoods with extremely high delinquency rates, presenting a special challenge.

Often, the higher delinquency neighborhoods will be those where subprime lending was concentrated. Thus, strategies to prevent foreclosure, such as loan modification to reduce the payment burden on households with high-cost subprime loans, could help stem neighborhood decline.

As with Group 1, assessing the potential for spillover effects that could cause the foreclosure problem to expand into adjacent neighborhoods, and taking countermeasures as needed, would be advisable. Again, targeted use of NSP funds to acquire and rehabilitate properties is a possible containment strategy.

Group 4

Group 4 consists of metropolitan areas with low-to-moderate mean delinquency rates, high positive skewness, and steep gradient around the peak-delinquency neighborhood. Low-to-moderate delinquency neighborhoods predominate in these MSAs. As reflected in the skewness measure, however, some neighborhoods will have distinctly higher delinquency, and at least one spatial outlier neighborhood is characterized by a high gradient value.

In general, metropolitan areas in Group 4 have fewer and less extreme high-delinquency neighborhoods than those in Group 3. They tend to have more high-delinquency pockets, or more spatial separation of high-delinquency neighborhoods, in comparison with Group 1. They also are distinguished by the outlier neighborhood having a much higher delinquency rate than neighboring ZIP

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[27] A few cities in Group 3 (Hartford, Oklahoma City, Rochester, and Syracuse) have a relatively low mean delinquency rate. Thus, although the neighborhood delinquency rate distribution is positively skewed, relatively few neighborhoods have high or very high delinquency. From a policy perspective, these cities more closely resemble those in Group 4, although they lack the gradient or spatial outlier aspect.

Codes, which suggests that the high-delinquency pockets are relatively self-contained (spillover is limited). In many, if not most, cases, the high-delinquency neighborhoods reflect concentrations of subprime lending.

Areas in this group include Albany, Barnstable, Minneapolis-St. Paul, and Virginia Beach. The delinquency map for Minneapolis, for example, shows two distinct high-delinquency pockets, one on the east side of St. Paul and another in northwest Minneapolis, extending north over the city boundary into the lower suburbs. They are relatively self-contained, largely surrounded by areas with much lower delinquency rates.

As with Group 1, neighborhood effects of delinquency and foreclosure would be limited to the higher delinquency pockets, which should then receive particular attention. The policy considerations noted for Group 1 apply to Group 4, with two nuances. First, the spatially separated high-delinquency pockets that are more characteristic of Group 4 may not be amenable to the same responses. Second, the high gradient measure suggests that containing the foreclosure problem may be of less concern.

Group 5

Group 5 is dominated by the gradient measure. Unlike the cities in Group 4, the cities in Group 5 have more or less symmetrical delinquency rate distributions, but, like those cities, they have a high gradient measure. All metropolitan areas slotted to this group have low-to-moderate delinquency means except for Riverside-San Bernardino, which may more appropriately belong to Group 2 (high-mean-delinquency cities), and fell into this group only because of an extreme outlier ZIP Code.

The large gradient suggests that the ZIP Code with the highest delinquency rate is isolated from other problematic ZIP Codes. It is possible that this ZIP Code is one of several problematic neighborhoods that are not near each other or that the MSA does not have many neighborhoods with very high delinquency rates.

The density plots for Buffalo and Charlotte, selected for exhibit 4, suggest that policy implications for this group vary, depending on the nature of the outlier ZIP Code and on potential effects of foreclosure in other, higher delinquency neighborhoods. For example, the Buffalo MSA has a single outlier ZIP Code that is associated with the large gradient but, reflecting the distribution's symmetry, also has substantial mass in neighborhoods with delinquency rates above 10 percent. A delinquency map of Buffalo indicates that the outlier ZIP Code is in the Niagara Falls area, where the delinquency rate is higher than in the other portions of the MSA beyond central Buffalo, whereas a large portion of urban Buffalo has moderately high delinquency rates. Thus, if the neighborhood foreclosure rates are considered problematic, Buffalo may require scalable strategies analogous to those discussed for Group 3. Charlotte, on the other hand, has no ZIP Code with a delinquency rate of 9 percent or more, which suggests that effects on neighborhood stability may not be a concern.

Group 6

The sixth group is the largest cluster. Group 6 consists of metropolitan areas that have low-to-moderate scores for all components; examples include Philadelphia, Pittsburgh, Sacramento, and Washington, D.C. Most cities in this group have moderate mean and skewness. A few, such as

Sacramento and Tampa, have high means but are distinguished from cities in Groups 2 and 3 by lower standard deviation and skewness; that is, less heterogeneity of neighborhood default rates, without the extremes associated with Groups 2 and 3.[28]

The density plots for Sacramento and Washington, D.C., selected for exhibit 4, illustrate the relatively compact, mildly skewed delinquency distributions that characterize most cities belonging to this group. A delinquency map of Washington, D.C., shows scattered high-delinquency neighborhoods, mostly adjacent to and east of the city or in outlying suburbs to the southwest of the city. A delinquency map of Sacramento illustrates the different case of widespread high delinquency rates through much of the metropolitan area.

Policy implications for these cities vary with the share of delinquencies in high-delinquency neighborhoods. The more typical metropolitan areas in this group, such as Washington, D.C., have a moderate delinquency mean and some scattered high-delinquency neighborhoods, largely tied to subprime concentrations. As with Group 1, neighborhood effects of delinquency and foreclosure would be limited to the higher delinquency pockets, which should then receive particular attention. Cities in this group, such as Sacramento, with high delinquency means and widespread high delinquency rates, require a citywide or regional perspective, comparable with that of Group 2.

Regression Analysis of Spatial Characteristics

Although this article's primary objective is to classify cities according to spatial characteristics of mortgage delinquency, a secondary goal is to examine the housing market and economic conditions that influence these characteristics. As we emphasized in the preceding section, understanding these factors is important for developing appropriate policy responses. For example, a high foreclosure rate in a lower income neighborhood that is a consequence of concentrations of high-risk lending to vulnerable borrowers might require a different response than would a spike in foreclosures in a far suburb resulting from overbuilding during the housing boom.

In this section, we develop an exploratory, multivariate regression analysis relating the spatial characteristics to economic and housing market conditions across metropolitan areas. The analysis highlights the contribution of subprime lending patterns and identifies the aspects of a metropolitan area's delinquency patterns that are most closely tied to the housing market cycle and to economic conditions. This analysis is a preliminary attempt to identify some basic relationships; it is not intended to be comprehensive.

First, we introduce a set of potential explanatory variables that we classify into three groups: (1) subprime lending spatial distribution measures, (2) housing market factors, and (3) other economic factors. Next, we estimate regression equations for each of the four principal components characterizing the spatial distributions.[29]

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[28] The cluster classifications for these cities are robust to using predicted values for their components from the regression analysis in place of actual values.

[29] To select efficiently among the large number of potential explanatory variables, we initially use a stepwise regression procedure for each of the four principal components. Because stepwise regression may generate some arbitrary selections, we subsequently evaluate and test the robustness of the resulting variable selections to the inclusion of omitted variables and adjust the specifications as appropriate. We dropped a few variables where the selection was questionable because of marginal statistical significance and colinearity or redundancy with other included variables.

Subprime Spatial Distribution

Because subprime loans are disproportionately represented among delinquent mortgages, we expect that distributional moments and spatial patterns of previous subprime lending activity in a metropolitan area influence mortgage delinquency patterns. We describe the characteristics of subprime lending across ZIP Codes using measures analogous to those used for mortgage delinquency: mean, standard deviation, kurtosis, and skewness for percent of active loans that are subprime (weighting by active subprime count), as well as spatial autocorrelation and gradient measures. Exhibit 5 provides summary statistics for the eight analysis variables.

Applying a PCA again reduces the set of descriptive measures to four principal components, again closely associated with (1) skewness and kurtosis, (2) gradient measures, (3) mean and standard deviation, and (4) autocorrelation measures, respectively. Exhibit 6 shows the results of the PCA of subprime spatial characteristics. We include these four principal components as explanatory variables for the regression analysis.

Exhibit 5

Summary Statistics of Analysis Variables (Subprime)

ZIP Code Variable	Mean	Standard Deviation
Mean	0.208	0.058
Standard deviation	0.011	0.005
Skewness	1.238	0.709
Kurtosis	3.661	4.841
First-layer gradient	0.199	0.165
Second-layer gradient	0.274	0.154
Moran's I	0.148	0.077
Geary's C	0.930	0.177

Exhibit 6

Principal Component Analysis Results (Subprime)

Component	Eigenvalue	Percent of Variance	Cumulative Percent
1	2.43	30	30
2	2.17	27	58
3	1.23	15	73
4	1.13	14	87
5	0.41	5	92
6	0.29	4	96
7	0.26	3	99
8	0.07	1	100

Housing Market Variables

We expect distributional moments and spatial patterns of mortgage delinquency to be tied to housing market activity. For example, delinquency rates will be higher in cities with more rapidly depreciating home values during 2007 and 2008.

Variables associated with the housing market boom and bust considered in the regression analysis include (1) annual home price appreciation rate from the third quarter of 2005 through the third

quarter of 2006 and from the third quarter of 2006 through the third quarter of 2008 in each MSA, (2) annual change in MSA housing starts over these periods, (3) the percentage of MSA home purchase loans in 2005 and 2006 that were for nonprimary residence (investment property or second home), and (4) the National Association of REALTORS® housing affordability index for the third quarter of 2005 and the third quarter of 2006.

We also construct measures of the spatial distribution of housing market activity for inclusion in our regression equations. Specifically, we calculate the distributional moments of percent change in home purchase loan originations from the third quarter of 2005 through the third quarter of 2006 and from the third quarter of 2006 through the third quarter of 2007 in each MSA: mean, standard deviation, kurtosis, and skewness (weighting by *ex ante* number of originations), along with the spatial autocorrelation measures. Spatial patterns of home purchase lending activity during the housing boom or at the beginning of the downturn may help differentiate neighborhoods where the market "overheated," as reflected in subsequent mortgage delinquency patterns.

We include, as a potential control variable for each regression equation, the size of the MSA housing market, measured as the log of the total number of active loans as of October 2008. In the equation for the gradient component, we include the ratio of maximum to median ZIP Code delinquency rate in the MSA to control for the potential effect of an outlier neighborhood.[30]

Economic Factors

Deteriorating economic conditions also affect delinquency patterns. For instance, we expect higher average delinquency rates, on average, in cities with more rapidly increasing unemployment during 2008, or in cities with higher unemployment levels.

Spatial patterns of mortgage delinquency in an MSA may reflect the spatial distribution of borrowers' incomes. We describe the spatial characteristics of borrowers' median incomes within an MSA using the distributional moments—mean, standard deviation, skewness, and kurtosis (weighting by active loan count)—along with the two spatial autocorrelation measures.[31]

Regression Results

Appendix D lists the economic and housing market variables that we ultimately selected for inclusion in one or more of the regression equations based on consideration of statistical significance and robustness.[32] The mean value and standard deviation of each variable across the 91 metropolitan areas included in the study also appear in appendix D.

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[30] The neighborhood delinquency gradient factor may reflect idiosyncratic factors that determine the maximum neighborhood delinquency rate, rather than economic or housing market conditions affecting the broader metropolitan area.

[31] We calculate the borrowers' median income for each ZIP Code relative to MSA median income from pooled 2005, 2006, and 2007 HMDA data.

[32] We employed stepwise regression as a first pass to develop baseline specifications, which we then evaluated for robustness by testing each variable excluded by the stepwise procedure.

Exhibit 7 summarizes the regression results. Results for the skewness, mean, gradient, and spatial autocorrelation components appear in columns 1 through 4, respectively.

Exhibit 7

Stepwise Regression Results for Delinquency Spatial Characteristics (Coefficient Estimates, With t-Values in Parentheses)

Explanatory Variable	Dependent Variable			
	(1) Skewness Component	(2) Mean Component	(3) Gradient Component	(4) Spatial Auto-Correlation Component
Intercept	2.7935*** (2.8)	– 0.0158 (1.2)	4.4175*** (3.0)	– 1.2038*** (9.6)
Subprime skewness component	0.7753*** (10.7)			– 0.2168*** (4.5)
Subprime gradient component			0.1847** (2.1)	0.1153** (2.3)
Subprime mean component	– 0.1886*** (2.7)	0.5491*** (9.3)		– 0.1497*** (3.1)
Subprime autocorrelation component			– 0.1661* (1.9)	0.6060*** (12.4)
Home price percent change, third quarter 2006 to third quarter 2007		– 0.1507*** (11.9)		
Home price percent change, third quarter 2005 to third quarter 2006			0.0416** (2.5)	
Affordability index, third quarter 2006			– 0.0048** (2.2)	
Unemployment percent change, third quarter 2007 to third quarter 2008	– 0.0128*** (2.7)	0.0081** (2.2)		
2008 unemployment rate			0.1146* (1.9)	
Home purchase percent nonprimary residence 2006	– 2.8750*** (2.6)			
Ratio, maximum-to-median ZIP Code delinquency rate			0.7109*** (5.7)	
Log of MSA active loan count			– 0.5333*** (4.7)	
Spatial autocorrelation of home purchase lending percent change, third quarter 2006 to third quarter 2007				1.2024*** (10.4)
Skewness of 2006 ZIP Code median borrower relative to MSA median family income			– 0.1861* (1.9)	
R-squared	0.597	0.711	0.442	0.798
Adjusted R-squared	0.578	0.701	0.387	0.786

MSA = metropolitan statistical area.

* Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

One broad conclusion that emerges from the analysis is that the shape (skewness and kurtosis) of the neighborhood delinquency rate distribution and the spatial autocorrelation of neighborhood delinquency rates are closely tied to spatial patterns of subprime lending activity during 2005 and 2006.[33] In the regression equations for the skewness/kurtosis and autocorrelation components, the estimated coefficient of the subprime lending counterpart of the dependent variable is the strongest explanatory variable. Thus, the regression analysis supports our previous contention that high-delinquency pockets in metropolitan areas characterized by high positive skewness or high spatial autocorrelation will often be neighborhoods with high subprime concentrations.

A second, broad, and not particularly surprising, conclusion is that economic conditions are at least as important as the subprime share mean/standard deviation component in influencing the delinquency rate mean/standard deviation component. This conclusion is consistent with our previous observation that metropolitan areas in Group 2 experienced harsher housing market or economic declines.

Third, spatial autocorrelation of neighborhood delinquency rates is strongly influenced by spatial autocorrelation of market decline during the "bust" period. Specifically, neighborhood delinquency spatial autocorrelation is positively related to the spatial autocorrelation of percent change in home purchase loan originations from the third quarter of 2006 through the third quarter of 2007.

The gradient component exhibits a somewhat eclectic set of associations. It is positively related to the subprime gradient component and inversely related to subprime spatial autocorrelation. In addition, the neighborhood delinquency gradient component exhibits a positive association with MSA house price appreciation from the third quarter of 2005 through the third quarter of 2006 and an inverse association with housing affordability as of the third quarter of 2006. The latter relationships are consistent with rapidly rising house prices triggering overdevelopment that subsequently generated high-foreclosure pockets.[34]

Conclusion

We first classified metropolitan areas into six groupings distinguished by their geographic patterns of serious mortgage delinquency. Understanding these patterns and their contributing factors may be informative for assessing local and neighborhood effects of the mortgage crisis and for developing appropriate strategies to mitigate the effects on communities.

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[33] We also estimated a regression equation for the Gini coefficient and found that it is very closely tied to the Gini coefficient of subprime lending (relative to total lending) activity.

[34] We observe various additional results specifically for the gradient component. It is inversely related to the log of MSA active loan count, indicating that steeper gradients occur in smaller cities. It is positively related to the ratio of maximum-to-median ZIP Code delinquency rate, confirming the importance of controlling for idiosyncratic neighborhood effects. The stepwise regression for the delinquency gradient component also yields three variables that are statistically significant at the 10-percent level in the equation: the subprime autocorrelation component, the third quarter 2008 unemployment rate, and the skewness of 2006 borrowers' median income relative to MSA median family income across ZIP Codes. F-tests indicate joint and pairwise significance at the 5-percent level for these three variables.

Second, we examined some housing market and economic conditions associated with the different spatial patterns. Although overall delinquency rates are highest in cities with large home price declines or high unemployment, the examination in this article highlights how most other cities have high-delinquency pockets, mostly because of subprime lending concentrations.

The first cluster consists of MSAs with high spatial autocorrelation and low- or moderate-delinquency rate means. These MSAs contain a modest number of high- or moderately high-delinquency neighborhoods that are clustered together or comprise a distinct pocket of neighborhoods within the MSA. The second grouping exhibits a high mean and standard deviation for delinquency rates. These MSAs have wide variation across neighborhoods, with most delinquencies occurring in distressed neighborhoods.

A third grouping is distinguished by a highly positively skewed, long, or fat-tailed distribution. Metropolitan areas in the fourth cluster are characterized by low-to-moderate mean delinquency rates, high positive skewness, and a steep gradient around the peak delinquency neighborhood, whereas those in the fifth cluster are distinguished specifically by their steep gradient. The sixth group consists of metropolitan areas that have low-to-moderate scores for all components.

These classifications are potentially useful for understanding the effects of the mortgage crisis on the dynamics of housing market decline and recovery. For instance, home prices appear to be stabilizing in some metropolitan areas despite little reduction in the inventory of foreclosed properties. Most likely, the foreclosures are concentrated in specific neighborhoods that are lagging behind the overall market recovery, as negative spillover effects tend to diminish with distance.[35]

We believe the analysis has practical applications for selecting or adapting appropriate strategies and policy responses to stabilize neighborhoods and contain foreclosure spillover effects. For example, NSP funds might be most effective for reversing or containing problems associated with foreclosure when spatially targeted to neighborhoods detached from or on the perimeters of broader areas of elevated delinquency and foreclosure. Metropolitan areas with low-to-moderate delinquency means and highly skewed delinquency distributions (Groups 3 and 4, and some cities in Group 6) are those where strategic deployment of NSP funds could be particularly effective at containing neighborhood decline.

Finally, we recognize that this study relies on data from 2008 and that housing markets have further deteriorated in many cities since then, so some cities may need to be reclassified. Although looking back has value, we wish to emphasize the role of this study as an example or template for ongoing analysis.

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[35] See Frame (2010) and Lee (2008) for reviews of the literature on price-related spillover effects.

Appendix A

Exhibit A-1

Metropolitan Areas Included in the Study (1 of 2)

Metropolitan Area	CBSA Code	Number of ZIP Codes	Number of Active Loans
Akron, OH	10420	45	106,942
Albany-Schenectady-Troy, NY	10580	89	95,775
Albuquerque, NM	10740	30	110,559
Allentown-Bethlehem-Easton, PA-NJ	10900	65	124,184
Atlanta-Sandy Springs-Marietta, GA	12060	186	952,203
Austin-Round Rock, TX	12420	74	183,871
Bakersfield, CA	12540	37	105,919
Baltimore-Towson, MD	12580	140	424,934
Barnstable Town, MA	12700	50	62,961
Baton Rouge, LA	12940	51	83,673
Birmingham-Hoover, AL	13820	97	144,998
Boise City-Nampa, ID	14260	31	95,176
Boston-Cambridge-Quincy, MA-NH	14460	227	553,793
Bridgeport-Stamford-Norwalk, CT	14860	46	154,159
Buffalo-Niagara Falls, NY	15380	68	101,719
Cape Coral-Fort Myers, FL	15980	28	143,676
Charleston-North Charleston, SC	16700	37	109,212
Charlotte-Gastonia-Concord, NC-SC	16740	69	314,654
Chicago-Naperville-Joliet, IL-IN-WI	16980	353	1,275,160
Cincinnati-Middletown, OH-KY-IN	17140	137	310,996
Cleveland-Elyria-Mentor, OH	17460	93	308,361
Columbia, SC	17900	37	103,838
Columbus, OH	18140	90	247,042
Dallas-Fort Worth-Arlington, TX	19100	238	638,709
Dayton, OH	19380	62	113,818
Deltona-Daytona Beach-Ormond Beach, FL	19660	26	104,561
Denver-Aurora, CO	19740	117	432,145
Des Moines-West Des Moines, IA	19780	53	82,061
Detroit-Warren-Livonia, MI	19820	212	748,855
Duluth, MN-WI	20260	50	33,292
Fresno, CA	23420	42	123,920
Grand Rapids-Wyoming, MI	24340	53	110,170
Hartford-West Hartford-East Hartford, CT	25540	84	161,788
Honolulu, HI	26180	28	115,861
Houston-Sugar Land-Baytown, TX	26420	210	718,052
Indianapolis-Carmel, IN	26900	95	263,862
Jacksonville, FL	27260	53	216,240
Kansas City, MO-KS	28140	165	254,570
Knoxville, TN	28940	44	99,447
Las Vegas-Paradise, NV	29820	59	326,813
Los Angeles-Long Beach-Santa Ana, CA	31100	341	1,544,348
Louisville/Jefferson County, KY-IN	31140	88	176,558
Madison, WI	31540	51	77,252

Exhibit A-1

Metropolitan Areas Included in the Study (2 of 2)

Metropolitan Area	CBSA Code	Number of ZIP Codes	Number of Active Loans
Memphis, TN-MS-AR	32820	65	176,957
Nashville-Davidson-Murfreesboro-Franklin, TN	34980	80	223,277
New Haven-Milford, CT	35300	40	123,831
New Orleans-Metairie-Kenner, LA	35380	64	147,791
New York-Northern New Jersey-Long Island, NY-NJ-PA	35620	820	1,956,999
Oklahoma City, OK	36420	74	125,597
Omaha-Council Bluffs, NE-IA	36540	76	90,279
Orlando-Kissimmee, FL	36740	86	391,956
Oxnard-Thousand Oaks-Ventura, CA	37100	24	127,552
Palm Bay-Melbourne-Titusville, FL	37340	24	114,259
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	37980	317	798,019
Phoenix-Mesa-Scottsdale, AZ	38060	129	604,605
Pittsburgh, PA	38300	200	255,626
Portland-South Portland-Biddeford, ME	38860	72	85,125
Portland-Vancouver-Beaverton, OR-WA	38900	114	343,125
Poughkeepsie-Newburgh-Middletown, NY	39100	67	88,915
Providence-New Bedford-Fall River, RI-MA	39300	99	233,860
Raleigh-Cary, NC	39580	44	158,985
Richmond, VA	40060	90	210,529
Riverside-San Bernardino-Ontario, CA	40140	146	713,532
Rochester, NY	40380	84	106,879
Sacramento--Arden-Arcade--Roseville, CA	40900	99	399,296
Salt Lake City, UT	41620	37	127,176
San Antonio, TX	41700	90	194,622
San Diego-Carlsbad-San Marcos, CA	41740	94	520,128
San Francisco-Oakland-Fremont, CA	41860	153	778,902
San Jose-Sunnyvale-Santa Clara, CA	41940	55	233,907
Santa Rosa-Petaluma, CA	42220	30	100,052
Sarasota-Bradenton-Venice, FL	42260	37	139,775
Scranton--Wilkes-Barre, PA	42540	56	63,693
Seattle-Tacoma-Bellevue, WA	42660	149	576,654
Sioux Falls, SD	43620	30	27,949
Springfield, MA	44140	77	87,622
St. Louis, MO-IL	41180	183	380,268
Stockton, CA	44700	29	117,827
Syracuse, NY	45060	61	68,873
Tampa-St. Petersburg-Clearwater, FL	45300	122	454,621
Toledo, OH	45780	58	107,862
Tucson, AZ	46060	33	150,494
Tulsa, OK	46140	62	99,479
Virginia Beach-Norfolk-Newport News, VA-NC	47260	87	246,509
Washington-Arlington-Alexandria, DC-VA-MD-WV	47900	270	1,125,879
Wichita, KS	48620	55	75,696
Worcester, MA	49340	77	120,910
Youngstown-Warren-Boardman, OH-PA	49660	59	76,062

CBSA = Core Based Statistical Area.

Appendix B

Exhibit B-1

Selected Distributional Measures (1 of 2)

Metropolitan Area	CBSA Code	Mean	Skewness	Gini Coefficient	Moran's I
Akron, OH	10420	0.1011	0.7611	0.2485	0.32132
Albany-Schenectady-Troy, NY	10580	0.0609	2.8483	0.1480	0.02833
Albuquerque, NM	10740	0.0471	2.2612	0.2120	0.09045
Allentown-Bethlehem-Easton, PA-NJ	10900	0.0523	2.4191	0.1419	0.12101
Atlanta-Sandy Springs-Marietta, GA	12060	0.0919	3.2221	0.2523	0.14049
Austin-Round Rock, TX	12420	0.0407	2.3014	0.1672	0.11915
Bakersfield, CA	12540	0.1284	1.1880	0.1189	0.15969
Baltimore-Towson, MD	12580	0.0619	2.5160	0.2544	0.20582
Barnstable Town, MA	12700	0.0525	4.1814	0.2769	0.12282
Baton Rouge, LA	12940	0.0657	0.6837	0.0359	0.03857
Birmingham-Hoover, AL	13820	0.0981	0.5870	0.2278	0.17512
Boise City-Nampa, ID	14260	0.0512	0.6044	0.1518	0.29785
Boston-Cambridge-Quincy, MA-NH	14460	0.0700	2.1865	0.3684	0.11547
Bridgeport-Stamford-Norwalk, CT	14860	0.0633	1.9451	0.3447	0.34573
Buffalo-Niagara Falls, NY	15380	0.0546	1.6284	0.1355	0.06291
Cape Coral-Fort Myers, FL	15980	0.2259	0.6535	0.2364	0.07114
Charleston-North Charleston, SC	16700	0.0504	1.0662	0.0213	0.06203
Charlotte-Gastonia-Concord, NC-SC	16740	0.0516	0.6037	0.2160	0.08647
Chicago-Naperville-Joliet, IL-IN-WI	16980	0.1126	2.4996	0.3025	0.15895
Cincinnati-Middletown, OH-KY-IN	17140	0.0659	0.9297	0.1037	0.08096
Cleveland-Elyria-Mentor, OH	17460	0.1129	1.7977	0.2763	0.22396
Columbia, SC	17900	0.0559	0.2479	0.0691	0.09935
Columbus, OH	18140	0.0861	1.5809	0.2257	0.22189
Dallas-Fort Worth-Arlington, TX	19100	0.0656	1.7768	0.2015	0.13064
Dayton, OH	19380	0.1044	1.2386	0.2032	0.25994
Deltona-Daytona Beach-Ormond Beach, FL	19660	0.1018	0.3803	0.1537	0.02248
Denver-Aurora, CO	19740	0.0628	1.4959	0.2304	0.06629
Des Moines-West Des Moines, IA	19780	0.0683	1.5758	0.2299	0.07425
Detroit-Warren-Livonia, MI	19820	0.1487	1.7237	0.3200	0.22105
Duluth, MN-WI	20260	0.0491	1.0778	0.0354	0.01245
Fresno, CA	23420	0.0951	(0.7155)	0.0659	0.13991
Grand Rapids-Wyoming, MI	24340	0.0739	3.2392	0.1880	0.02531
Hartford-West Hartford-East Hartford, CT	25540	0.0481	2.2628	0.2192	0.08115
Honolulu, HI	26180	0.0357	1.6452	0.2797	0.20223
Houston-Sugar Land-Baytown, TX	26420	0.0658	1.1848	0.1085	0.08990
Indianapolis-Carmel, IN	26900	0.0920	1.4298	0.2475	0.12805
Jacksonville, FL	27260	0.1071	2.0112	0.1780	0.22384
Kansas City, MO-KS	28140	0.0729	1.6895	0.2186	0.11854
Knoxville, TN	28940	0.0460	0.8428	0.1424	0.07278
Las Vegas-Paradise, NV	29820	0.1247	0.7089	0.0890	0.19760
Los Angeles-Long Beach-Santa Ana, CA	31100	0.0848	2.6415	0.2944	0.08394
Louisville-Jefferson County, KY-IN	31140	0.0915	2.6032	0.2267	0.18207
Madison, WI	31540	0.0324	2.4176	0.0517	0.15087
Memphis, TN-MS-AR	32820	0.1421	0.2423	0.2622	0.05278
Miami-Fort Lauderdale-Pompano Beach, FL	33100	0.1546	1.8939	0.1163	0.10401
Milwaukee-Waukesha-West Allis, WI	33340	0.1119	1.6206	0.4404	0.26654

Exhibit B-1

Selected Distributional Measures (2 of 2)

Metropolitan Area	CBSA Code	Mean	Skewness	Gini Coefficient	Moran's I
Minneapolis-St. Paul-Bloomington, MN-WI	33460	0.0762	5.9378	0.2029	0.06764
Nashville-Davidson-Murfreesboro-Franklin, TN	34980	0.0557	1.6972	0.1803	0.05318
New Haven-Milford, CT	35300	0.0683	0.8762	0.2614	0.18388
New Orleans-Metairie-Kenner, LA	35380	0.0643	0.8645	0.1011	0.15283
New York-Northern New Jersey-Long Island, NY-NJ-PA	35620	0.0857	2.8248	0.3672	0.08619
Oklahoma City, OK	36420	0.0562	2.2695	0.0327	0.11450
Omaha-Council Bluffs, NE-IA	36540	0.0654	2.1485	0.2275	0.07015
Orlando-Kissimmee, FL	36740	0.1409	0.8574	0.1644	0.10881
Oxnard-Thousand Oaks-Ventura, CA	37100	0.0754	1.0449	0.2381	0.89622
Palm Bay-Melbourne-Titusville, FL	37340	0.1071	1.3009	0.1575	0.10468
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	37980	0.0659	1.6754	0.2785	0.11554
Phoenix-Mesa-Scottsdale, AZ	38060	0.1289	1.0739	0.3001	0.17823
Pittsburgh, PA	38300	0.0675	1.6749	0.1077	0.07618
Portland-South Portland-Biddeford, ME	38860	0.0495	1.2519	0.0650	0.21971
Portland-Vancouver-Beaverton, OR-WA	38900	0.0358	0.7176	0.1386	0.09663
Poughkeepsie-Newburgh-Middletown, NY	39100	0.0562	0.4385	0.0987	0.05572
Providence-New Bedford-Fall River, RI-MA	39300	0.0834	2.4916	0.2760	0.14911
Raleigh-Cary, NC	39580	0.0410	1.5304	0.2310	0.40647
Richmond, VA	40060	0.0526	1.6577	0.2453	0.05181
Riverside-San Bernardino-Ontario, CA	40140	0.1487	1.0673	0.1543	0.11091
Rochester, NY	40380	0.0516	2.1316	0.1556	0.14953
Sacramento--Arden-Arcade--Roseville, CA	40900	0.1072	1.2348	0.2130	0.20979
Salt Lake City, UT	41620	0.0329	0.4505	0.1421	0.04594
San Antonio, TX	41700	0.0564	0.9467	0.1606	0.19861
San Diego-Carlsbad-San Marcos, CA	41740	0.0750	0.3835	0.2473	0.16840
San Francisco-Oakland-Fremont, CA	41860	0.1029	1.2504	0.4745	0.14809
San Jose-Sunnyvale-Santa Clara, CA	41940	0.0790	0.9437	0.4204	0.18254
Santa Rosa-Petaluma, CA	42220	0.0702	2.1641	0.2843	0.00757
Sarasota-Bradenton-Venice, FL	42260	0.1168	1.8652	0.1700	0.08905
Scranton--Wilkes-Barre, PA	42540	0.0639	1.0407	0.1653	0.08902
Seattle-Tacoma-Bellevue, WA	42660	0.0380	1.1601	0.2524	0.26297
Sioux Falls, SD	43620	0.0432	1.6651	0.1184	0.05387
Springfield, MA	44140	0.0922	1.6064	0.2997	0.15298
St. Louis, MO-IL	41180	0.0864	1.3642	0.2687	0.19471
Stockton, CA	44700	0.1568	0.6151	0.1601	0.15657
Syracuse, NY	45060	0.0517	2.0942	0.1833	0.14539
Tampa-St. Petersburg-Clearwater, FL	45300	0.1155	1.0958	0.1374	0.07677
Toledo, OH	45780	0.0960	2.2450	0.2553	0.36648
Tucson, AZ	46060	0.0600	0.8474	0.2653	0.11890
Tulsa, OK	46140	0.0669	2.2258	0.0745	0.09841
Virginia Beach-Norfolk-Newport News, VA-NC	47260	0.0439	2.8062	0.0805	0.04837
Washington-Arlington-Alexandria, DC-VA-MD-WV	47900	0.0687	1.4021	0.2728	0.15820
Wichita, KS	48620	0.0399	0.9506	0.1035	0.01248
Worcester, MA	49340	0.0774	1.2448	0.2081	0.09926
Youngstown-Warren-Boardman, OH-PA	49660	0.0931	1.7036	0.0273	0.11455

CBSA = Core Based Statistical Area.

Appendix C

Exhibit C-1

Groupings From the Cluster Analysis

Group 1
Albuquerque, NM
Austin-Round Rock, TX
Boise City-Nampa, ID
Bridgeport-Stamford-Norwalk, CT
Honolulu, HI
Madison, WI
Oxnard-Thousand Oaks-Ventura, CA
Portland-South Portland-Biddeford, ME
Raleigh-Cary, NC
San Antonio, TX
Seattle-Tacoma-Bellevue, WA

Group 2
Akron, OH
Bakersfield, CA
Cape Coral-Fort Myers, FL
Cleveland-Elyria-Mentor, OH
Dayton, OH
Deltona-Daytona Beach-Ormond Beach, FL
Detroit-Warren-Livonia, MI
Las Vegas-Paradise, NV
Memphis, TN-MS-AR
Milwaukee-Waukesha-West Allis, WI
Orlando-Kissimmee, FL
Palm Bay-Melbourne-Titusville, FL
Phoenix-Mesa-Scottsdale, AZ
San Francisco-Oakland-Fremont, CA
Sarasota-Bradenton-Venice, FL
Stockton, CA

Group 3
Atlanta-Sandy Springs-Marietta, GA
Baltimore-Towson, MD
Boston-Cambridge-Quincy, MA-NH
Chicago-Naperville-Joliet, IL-IN-WI
Columbus, OH
Hartford-West Hartford-East Hartford, CT
Jacksonville, FL
Los Angeles-Long Beach-Santa Ana, CA
Louisville/Jefferson County, KY-IN
Miami-Fort Lauderdale-Pompano Beach, FL
New York-Northern New Jersey-Long Island,
    NY-NJ-PA
Oklahoma City, OK
Rochester, NY
Springfield, MA
Syracuse, NY
Toledo, OH
Tulsa, OK
Youngstown-Warren-Boardman, OH-PA

Group 4
Albany-Schenectady-Troy, NY
Allentown-Bethlehem-Easton, PA-NJ
Barnstable Town, MA
Grand Rapids-Wyoming, MI
Minneapolis-St. Paul-Bloomington, MN-WI
Providence-New Bedford-Fall River, RI-MA
Santa Rosa-Petaluma, CA
Virginia Beach-Norfolk-Newport News, VA-NC

Group 5
Baton Rouge, LA
Buffalo-Niagara Falls, NY
Charleston-North Charleston, SC
Charlotte-Gastonia-Concord, NC-SC
Des Moines-West Des Moines, IA
Nashville-Davidson-Murfreesboro-Franklin, TN
New Haven-Milford, CT
Omaha-Council Bluffs, NE-IA
Richmond, VA
Riverside-San Bernardino-Ontario, CA
Scranton--Wilkes-Barre, PA
Sioux Falls, SD
Wichita, KS

Group 6
Birmingham-Hoover, AL
Cincinnati-Middletown, OH-KY-IN
Columbia, SC
Dallas-Fort Worth-Arlington, TX
Denver-Aurora, CO
Duluth, MN-WI
Fresno, CA
Houston-Sugar Land-Baytown, TX
Indianapolis-Carmel, IN
Kansas City, MO-KS
Knoxville, TN
New Orleans-Metairie-Kenner, LA
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD
Pittsburgh, PA
Portland-Vancouver-Beaverton, OR-WA
Poughkeepsie-Newburgh-Middletown, NY
Sacramento--Arden-Arcade--Roseville, CA
Salt Lake City, UT
San Diego-Carlsbad-San Marcos, CA
San Jose-Sunnyvale-Santa Clara, CA
St. Louis, MO-IL
Tampa-St. Petersburg-Clearwater, FL
Tucson, AZ
Washington-Arlington-Alexandria, DC-VA-MD-WV
Worcester, MA

Appendix D

Exhibit D-1

Economic and Housing Market Variables Selected by the Stepwise Regression Procedure

Variable	Mean	Standard Deviation
Subprime Gini coefficient	0.1711	0.0938
Percent owner occupied among 2006 conventional home purchase loans	0.8320	0.0737
Log of MSA active loan count	12.1988	0.8728
Percent change in HPI third quarter 2005 to third quarter 2006	6.6987	5.9766
Percent change in HPI third quarter 2006 to third quarter 2007	0.6482	4.8186
Percent change in unemployment rate third quarter 2006 to third quarter 2007	1.7955	13.0455
Percent change in unemployment rate third quarter 2007 to third quarter 2008	31.4344	16.1052
Third quarter 2008 unemployment rate	5.9697	1.5235
Third quarter 2006 affordability index	125.1476	48.4117
Home purchase lending percent change third quarter 2007 to third quarter 2008	– 0.2364	0.1168
The mean, avg_income_to_mfi_05	0.0014	0.0003
Skewness, avg_income_to_mfi_05	1.6476	1.0766
The mean, avg_income_to_mfi_06	0.0015	0.0004
Skewness of 2006 ZIP Code median borrower income relative to MSA median family income	1.6877	0.8648
Geary's C for home purchase lending percent change third quarter 2006 to third quarter 2007	1.0012	0.4399
Ratio of maximum-to-median ZIP Code delinquency rate	2.8754	0.8092

HPI = house price index. MSA = metropolitan statistical area.

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Additional Reading

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The Housing Needs of Rental Assistance Applicants

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Abstract

Federal rental assistance programs are not funded adequately to serve all, or even most, eligible households. As a result, millions of households are on Public Housing Authority (PHA) waiting lists to receive a Housing Choice Voucher or a unit in a public housing development. Applicants typically wait years before being offered assistance, and many PHAs have closed their waiting lists to new applicants. Although this problem is long-standing and widely acknowledged, very little is known about the characteristics and experiences of households on waiting lists for rental assistance. A 2009 survey of nearly 1,000 nonelderly, nondisabled rental assistance applicants, selected from a nationwide sample of 25 PHAs, provides new information on these households. The survey shows that households that apply for and receive housing assistance differ significantly from households that the U.S. Department of Housing and Urban Development considers as having worst case housing needs (WCN). Specifically, most rental assistance applicants did not spend more than one-half of their income on housing, primarily because they reduced their housing costs by living with family or friends or by receiving some form of government subsidy. Applicants frequently reported other housing-related problems not included in the WCN measure, such as homelessness, overcrowding, and certain housing quality problems. In addition, many applicants appear to apply for rental assistance to form their own households rather than continue living with family or friends. These findings have implications for our understanding of housing needs and the function of rental assistance programs in addressing those needs.

Introduction

This article focuses on applicants to the public housing program and the Housing Choice Voucher Program (HCVP)—the two largest federal rental assistance programs, which serve roughly 1 and 2 million households, respectively. Public housing households live in units that the local housing

authority owns and operates, whereas HCVP (also called Section 8) households receive vouchers that they use to lease rental units in the private market. With some exceptions, households in both programs pay 30 percent of their monthly income—after taking certain deductions for childcare and medical expenses—toward rent, and the housing authority pays the difference between the tenants' rent contribution and the total cost of the unit.[1]

To be eligible for public housing or the HCVP, a household's income must be less than 80 percent of the Area Median Income (AMI) within the Public Housing Authority's (PHA's) metropolitan area. Unlike the Supplemental Nutrition Assistance Program (food stamps), Medicaid, or other means-tested programs, however, housing assistance is not an entitlement, and only one in four eligible renter households currently receives any form of federal rental assistance (Steffen et al., 2011). Rental assistance applicants are placed on waiting lists and offered assistance as public housing units or vouchers become available.

Although no one knows exactly how many households are currently on public housing or HCVP waiting lists, the number is surely in the millions. The National Low Income Housing Coalition (NLIHC) surveyed the administrative plans of 134 PHAs for information about their waiting lists. More than 1.5 million people were on waiting lists just for those PHAs, and more would have been if many PHAs had not closed their waiting lists to new applicants (NLIHC, 2004). A 2009 survey of a nationally representative sample of PHAs with at least 500 units found that 15 percent of PHAs were not accepting new applicants for public housing and 58 percent of PHAs were not accepting new HCVP applicants (Buron et al., 2010). The same survey found that the wait for a public housing unit in most PHAs was 1 year or longer and the wait for a voucher was more than 2 years.

Federal and local policies regarding how to allocate rental assistance resources affect the amount of time applicants spend on waiting lists. In 1979, Congress established federal priorities for admission for households with severe rent burdens, households in severely substandard housing, and households that were displaced by government actions. The Quality Housing and Work Responsibility Act (QHWRA), enacted in 1998, removed these federal preferences. Today, housing agencies must ensure that 75 percent of new admits into the HCVP and 40 percent of new admits into public housing have extremely low incomes—meaning incomes of 30 percent or less of AMI. Other than meeting these quotas, PHAs have discretion to develop their own admissions preferences for selecting households from their waiting lists.

No national statistics are available on how PHAs set their admissions preferences. NLIHC's review of administrative plans found that 75 percent of the 134 PHAs in its sample used some sort of local preference system to order their waiting lists, whereas the other 25 percent selected applicants based on a lottery or a first-come, first-served system. The PHAs' admissions preferences rarely reflected the former federal preference for households that were rent burdened or living in

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[1] Most housing authorities require households with zero reported income to pay a minimum monthly rent, which, at most, is $50. In addition, some households in public housing units opt to pay a flat rent, which housing authorities set based on the market value of the unit. Voucher recipients also have the option of paying up to 40 percent of their income to rent units with rents that are greater than the PHA's payment standard at the time of the initial lease up, and many recipients pay more thereafter.

substandard housing. The most common PHA admissions preferences were for applicants who were employed, were involuntarily displaced as a result of natural disasters or government actions, were domestic violence victims, or lived or worked within the PHA's jurisdiction (NLIHC, 2004).

The characteristics of households on waiting lists for rental assistances are also not well under-stood. Studies that involve waitlisted households typically include them as a control group to study the effects of rental assistance. The high number of unassisted applicants and the lottery-based selection process that many PHAs use has allowed for several experimental evaluations of rental assistance programs. Jacob and Ludwig (2008) found that households that received a voucher through the Chicago Housing Authority (CHA) had lower quarterly earnings but higher Tempo-rary Assistance for Needy Families takeup rates compared with households still on CHA's waiting list. An evaluation of the Welfare-to-Work program, which randomly assigned Section 8 applicants to the treatment (voucher) and control (remain on waiting list) groups, found that the treatment group had significantly lower rates of homelessness and overcrowding than the control group. These effects narrowed, however, as more people from the control group received assistance (Wood, Turnham, and Mills, 2009). Sharfstein et al. (2001) surveyed 74 families who had recently received a voucher through the Boston Housing Authority and found that applicants' housing units before receiving assistance were significantly more likely to have housing hazards, such as rats, lack of heat, and absence of running water, than their units after receiving assistance. No known studies have focused on why eligible households apply for rental assistance and how they would benefit from receiving it.

Although the literature on the specific characteristics and housing needs of rental assistance ap-plicants is limited, the literature on the housing needs of very low-income unassisted renters (those with incomes of less than 50 percent of the AMI) is extensive. The most influential report on this subject is the U.S. Department of Housing and Urban Development's (HUD's) worst case housing needs (WCN) report to Congress. HUD submits this report, based on data from the American Housing Survey (AHS), to Congress every other year to "inform public policy decisions, including decisions on targeting existing resources, determining the need for additional resources, and the form housing assistance should take" (Steffen et al., 2011: 61). Only very low-income households that are living in a rental unit and not receiving government housing assistance can be considered WCN households. Two types of housing problems are considered WCN: severe rent burden and severely inadequate housing. Households have a severe rent burden if they spend 50 percent or more of their monthly income on housing (rent plus utilities). Severely inadequate housing units have one or more serious physical problems related to heating, plumbing, and electrical systems and maintenance (Steffen et al., 2011).

The most recent WCN study, based on AHS data from 2009, found that 7.1 million households, or 55 percent of all unassisted renters with very low incomes, had WCN. Of these households, 94 percent had a severe rent burden but were living in adequate housing, 3 percent were in severely inadequate housing but not severely rent burdened, and 3 percent were both severely rent bur-dened and living in severely inadequate housing.

The WCN reports have consistently identified severe rent burden as the dominant cause of WCN among very low-income renters (Bostic, 2011). Based on this evidence, a common assumption is that most households that apply for and receive rental assistance are also severely rent burdened.

The WCN report states that, "most assisted households would otherwise experience worst case needs" (Steffen et al., 2011: 10). Other studies that analyzed the relationship between the number of households receiving rental assistance in an area and the number of households with WCN have estimated that between 68 and 76 percent of households that receive housing assistance are selected from the WCN population (McClure, 2011; Shroder, 2002).

These findings have shaped an assumption among some housing policy experts that rental assistance, particularly Section 8, "generally does not materially improve the physical housing conditions experienced by its target population" (Grigsby and Bourassa, 2004: 815). Rather, for most recipients, rental assistance essentially functions as an income support. Assisted households use rental assistance to reduce their housing costs, enabling them to consume more of other goods such as food, clothing, education, and health care. Grigsby and Bourassa (2004: 816) argue in their call for fundamental reform of the Section 8 Program that "[t]he purpose of Section 8 has become not improvement in the housing inventory at affordable rents, but for all practical purposes, affordability alone that is, to reduce rent/income ratios to 30 or 40 percent." Therefore, the authors argued that Section 8 should be converted into an income-transfer program, giving money directly to eligible households that would presumably spend the money on housing, because it is their greatest expense (Grigsby and Bourassa, 2004).

It is not clear, however, that the WCN measure is a reliable proxy for understanding who applies for assistance and how they benefit from receiving it. Besides very low-income renter households, a variety of other groups might apply for rental assistance. For example, although reducing homelessness is one of the primary functions of rental assistance (Khadduri, 2008), homelessness is not included in the WCN measure because the AHS does not survey households not living in a housing unit. The authors of the WCN report acknowledge this omission as a limitation of the measure.

The WCN measure also excludes renters currently receiving government housing assistance. It does not place restrictions on assisted households applying for other forms of rental assistance, however. For example, nothing prevents a household in a public housing unit from applying for a Section 8 voucher. Using AHS data, Koebel and Renneckar (2003) found that roughly 1.5 million households that claimed to receive rental assistance were either severely rent burdened or living in severely substandard housing. Thus, reported receipt of some form of rental assistance is not necessarily an indication that a household is not motivated to apply for other forms of rental assistance.

When the WCN measure was originally developed, it reflected the federal priorities for rental assistance, as established by Congress. The authors of the WCN report acknowledge that many other housing-related needs are not included in this measure. Applicants may seek rental assistance because they are living in housing that is overcrowded, of poor quality (although not severely substandard), or in a poor-quality neighborhood (Koebel and Renneckar, 2003). They may also apply for assistance so they can afford to live closer to where they work or go to school (Belsky, Goodman, and Drew, 2005). Finally, applicants may use rental assistance as a means to establish their own household rather than live with family or friends (Shroder, 2002). PHAs may offer assistance to applicants with these needs rather than to applicants with worst case needs.

Survey of Waitlisted Households

This article is based on a survey of rental assistance applicants conducted as part of the *Study of Rents and Rent Flexibility*, a study of possible changes to the rent structure of HUD's public housing and the HCVP (Buron et al., 2010). The study team interviewed 1,204 nonelderly, nondisabled families from 25 PHAs who were either still waiting for housing assistance or had started receiving assistance within the past 12 months.

In selecting the sample, Buron et al. (2010) purposively chose PHAs in Cambridge, Massachusetts; Keene, New Hampshire; and Tulare, California, because they had used their enhanced flexibility as Moving to Work (MTW) sites to implement major reforms to their rent structures.[2] These 3 PHAs account for a small percentage of all assisted households but 28 percent of all survey respondents. Buron et al. (2010) selected the other 22 PHAs included in the survey through a stratified, random sampling process based on location, size, and the cost of rental housing within the PHA.

One-half of all sampled households were still waiting to be offered assistance; the other half had been offered assistance within the past year and was living in public housing or in a rental unit leased with a Housing Choice Voucher. The survey asked households currently receiving assistance to report on their housing status both before and after receiving assistance. The analysis focuses on the housing status of waitlisted households at the time they were interviewed and of new admits immediately before being offered assistance. For purposes of this article, both waitlisted and new-admit households are referred to as rental assistance applicants. Because this was a cross-sectional survey, it is not possible to determine if the housing issues experienced by applicants were the same problems that motivated them to apply for assistance or if their housing situation adapted over time.

Exhibit 1 shows the number of households within each PHA that completed a survey. Overall, 1,875 households were sampled, and 1,204 completed a survey. Although the sampling process was intended to screen out applicants with disabilities or who were elderly, 211 respondents were removed from the analysis because they reported having a disability or being elderly. Survey results are based on the responses of the 993 remaining applicants.

The survey had a response rate of 64 percent. The response rate for new admits (71 percent) was higher than that for waitlisted applicants (58 percent). The nonresponses were primarily the result of an inability to locate applicants. The research team was unable to locate 15 percent of new admits and 23 percent of applicants still on a waiting list. Rental assistance applicants are likely to be living in tenuous or transient living arrangements and, not surprisingly, many were no longer living at the address listed on the PHA's waiting list. An additional 16 percent of the sample refused to be interviewed, were unable to schedule an interview during the data collection period, or had a language barrier or other issue that prevented them from completing the survey.

The low response rate to the survey raises the problem of nonresponse bias. The results may un-derrepresent the worst off applicants, because homeless or tenuously housed applicants were the

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[2] The MTW program was implemented as part of the QHWRA. MTW PHAs have the flexibility to implement reforms not generally allowed under HUD regulations. Some MTW sites used this flexibility to implement reforms such as charging all households a flat rent, establishing work requirements, and implementing time limits for housing assistance.

Exhibit 1

Household Survey Respondents by PHA and Sample Type

Housing Authority Name	Waiting List		New Admits		Total
	Vouchers	Public Housing	Vouchers	Public Housing	
Austin Housing Authority	19	5	15	23	62
Bessemer Housing Authority	5	15	6	12	38
Birmingham Housing Authority	8	9	28	15	60
Boise City Housing Authority	15	2	20	0	37
Cambridge Housing Authority	18	10	29	18	75
Charleston/Kanawha Housing Authority	20	4	16	7	47
Charlotte Housing Authority	22	11	4	35	72
Chicago Housing Authority	35	0	26	10	71
Dubuque Housing Authority	12	0	21	0	33
Eastern Iowa Housing Authority	12	3	15	3	33
Framingham Housing Authority	12	1	16	4	33
Gastonia Housing Authority	10	5	18	6	39
Idaho Housing Finance Association—Section 8	19	0	19	0	38
Keene Housing Authority	65	27	11	4	107
Lake County Housing Authority	9	4	12	5	30
McKeesport Housing Authority	17	1	4	14	36
Muncie Housing Authority	13	1	6	7	27
Pittsburgh Housing Authority	30	14	12	37	93
Santa Barbara City Housing Authority	9	0	20	2	31
Santa Barbara County Housing Authority	8	1	10	5	24
Somerville Housing Authority	7	4	14	1	26
Travis County Housing Authority	23	2	6	2	33
Tulare Housing Authority	34	10	40	14	98
Vancouver Housing Authority	12	3	12	7	34
Waterbury Housing Authority	6	4	12	5	27
Total	**440**	**136**	**392**	**236**	**1,204**

PHA = Public Housing Authority.
Source: Buron et al. (2010)

hardest to locate. Possibly, however, applicants who were not located had applied for assistance less recently but were not necessarily worse off, or applicants who declined to be interviewed were better off and no longer needed or were interested in receiving rental assistance.

Because the survey relied solely on self-reporting, there is also some risk of response bias. Respondents were not required to provide documentation to verify their reported incomes or expenses. In addition, new admits were asked to report on their housing expenses and housing quality both before and after receiving housing assistance. In some cases, new admits may have had a difficult time remembering the condition of their former housing or exactly how much they were paying for rent and utilities. Also, respondents may have underreported their income under the mistaken assumption that the information they reported would be shared with HUD and used to set their monthly rent contribution.

The survey of rental assistance applicants and the population of renters with possible WCN differ in several important ways. First, because the survey of rental assistance applicants was originally

intended to study the effects of rental assistance on earned income, it excluded applicants with disabilities or who were elderly. By contrast, one-third of WCN households include people who either have disabilities or are elderly (Steffen et al., 2011). Second, unlike the WCN measure, the survey of rental assistance applicants included households that were homeless, were already receiving some form of housing assistance, were living in owner-occupied housing, or had incomes exceeding 50 percent of AMI. Finally, the WCN measure is based on a survey of households, whereas the survey of rental assistance applicants asks questions about the applicant and the family the applicant heads. For cases in which an applicant is living with people with whom he or she will no longer live after receiving assistance, the survey does not capture the household's total housing cost or income.

Characteristics of Rental Assistance Applicants

Because the number of surveyed households is fairly small and not nationally representative, some question exists regarding how comparable they are with the national profile of rental assistance applicants. Unfortunately, no survey collects national data on the demographic characteristics of rental assistance applicants. HUD, however, requires PHAs to report on the characteristics of assisted households. Exhibit 2 compares the demographic characteristics of rental assistance appli-cants with the characteristics of all nonelderly, nondisabled households in public housing and the HCVP as of 2008. Other than the slightly higher proportion of American Indians among surveyed

Exhibit 2

Characteristics of Surveyed Households Compared With All Nonelderly, Nondisabled Assisted Households

Characteristics	Nonelderly, Nondisabled Assisted Households (N = 1,895,256)	Surveyed Rental Assistance Applicants (N = 993)
Gender of adults		
Female	89%	82%
Ethnicity		
Hispanic	23%	21%
Race		
White	43%	42%
Black	53%	52%
American Indian or Alaska Native	1%	4%
Asian	2%	1%
Native Hawaiian or other Pacific Islander	1%	1%
Age		
Average age	36	34
18–24	13%	22%
25–34	36%	39%
35–44	27%	21%
45–62	23%	18%
Income		
Median monthly income	$862	$1,000

Sources: Buron et al. (2010); 2008 U.S. Department of Housing and Urban Development, Office of Policy Development and Research, Public and Indian Housing Information Center data set

households, the racial and ethnic demographics of the two groups were nearly identical. Adults in surveyed households were slightly younger, which could be a result of the long wait times typical for rental assistance. Survey respondents were also slightly more likely to be men.

Survey respondents reported slightly higher median monthly incomes than assisted households. This difference could be a result of the high proportion of MTW sites, which were purposively selected because their rent structures were designed to be more attractive to working households (Buron et al., 2010). The income disparity could also emerge, however, because applicants with lower incomes were more likely to be offered assistance because of federal quotas for admitting extremely low-income applicants. In addition, studies have found that household income sometimes decreases after the receipt of rental assistance (Jacob and Ludwig, 2008).

At the time they were interviewed, 48 percent of surveyed households were still waiting for rental assistance and 52 percent had begun receiving assistance within the past 12 months. New admits reported that they spent an average of 2.6 years on the waiting list before being offered assistance, and the average reported wait time for households currently on the waiting list was 2.5 years. A few households with very long waits skew these averages. The median wait time for new admits was 1 year, whereas the median wait time for waitlisted households was 2 years.

New admits may have different characteristics and housing needs than applicants still on the waiting list because of federal income quotas and PHA admissions preferences. Only 4 of the 25 PHAs included in the sample reported that they selected households from their waiting lists on a lottery or first-come, first-served basis. The other 21 PHAs used admissions preferences to prioritize assistance for certain households. The two most common preferences, each cited by 48 percent of PHAs, were for applicants who were displaced by either natural disasters or government action and for applicants who were employed or enrolled in some kind of training program. The next most common preferences were for applicants living within the PHA service area (40 percent) and homeless applicants (24 percent). Only 8 percent of PHAs reported a preference for applicants who were rent burdened or living in substandard housing.

It is unclear what effect these admissions preferences had on whether eligible households applied for or received assistance. Some PHAs reported that the QHWRA income quotas limited their ability to prioritize assisting working households, because these households often do not have extremely low incomes. In addition, local admissions preferences might not have been well understood by potential applicants or well-advertised by PHAs. For instance, only 14 percent of applicants in PHAs with a preference for working households reported being told by their local housing authority that they would receive assistance sooner if someone in their household were working for pay.

Exhibit 3 shows that waitlisted households had higher average monthly incomes than new admits. Whether new admits had a lower income at the time they were offered assistance or their incomes decreased after receiving assistance, however, is unclear. New admits may have had a greater incentive to underreport their income if they mistakenly believed that their responses would be shared with the local housing authority and used to determine their rent contribution. On the other hand, new admits may have had a greater incentive to report their income accurately if they believed the housing authority data would be validated against the Enterprise Income Verification system. In

any case, although the difference in incomes was statistically significant, waitlisted households also had low incomes; 75 percent had extremely low incomes and 92 percent had very low incomes.

Exhibit 4 shows the living situations of waitlisted households at the time they were interviewed and of new admits immediately before receiving assistance. Among all applicants, 40 percent were living with family or friends, 38 percent were living independently without government assistance, 16 percent were living independently but receiving some form of housing subsidy, and 7 percent were homeless or living in an institutional setting. Among applicants living independently, 4 percent were owners and the other 96 percent were renters. The survey did not ask doubled-up applicants if they were living in owner-occupied or rental housing.

Households currently on the waiting list were more likely than new admits to report living independently without a subsidy (44 versus 31 percent) and were less likely to report being homeless or living in an institutional setting (4 versus 10 percent).

As shown in exhibit 4, the largest share of rental assistance applicants were living in doubled-up situations with family or friends. Exhibit 5 provides more detail about the living situation of these applicants. Of doubled-up applicants, 80 percent were living with family and slightly more than one-half of these applicants were paying some portion of the household's housing costs. The other 20 percent of doubled-up applicants were living with friends; one-half of these applicants helped pay rent and one-half did not.

Exhibit 3

Reported Monthly Income of Waitlisted and New-Admit Respondents

	Median Monthly Income ($)	Percent With Extremely Low Incomes ($\leq$ 30% of AMI)	Percent With Very Low Incomes ($\leq$ 50% of AMI)
Waiting list (N = 518)	1,190[a]	75	93
New admits (N = 473)	807	82	98

AMI = Area Median Income.

[a] The difference in means was statistically significant at the .01 level.

Note: Information on income was missing for 2 newly admitted applicants, and 131 waitlisted and newly admitted applicants did not know or refused to provide their monthly income.

Source: Buron et al. (2010)

Exhibit 4

Living Situation of Applicants Before Receiving Assistance

	Waiting List		New Admits		All Sampled Households	
	(N)	(%)	(N)	(%)	(N)	(%)
Living independently—no subsidy	227	44	146	31	373	38
Living independently—with subsidy	81	16	77	16	158	16
Living with family or friends	190	37	203	43	393	40
Other living arrangement[a]	19	4	46	10	65	7
Total	**517**	**100**	**472**	**100**	**989**	**100**

[a] This group includes people who were incarcerated or living in dorms, barracks, hospitals, nursing homes, specialty schools, hotels, or motels.

Note: Information on income was missing for four respondents.

Source: Buron et al. (2010)

Exhibit 5

Details About Rental Assistance Applicants Living With Family or Friends

	Applicants (N)	Doubled-Up Applicants (%)	Paying Some Rent (%)	Paying No Rent (%)
Living with family	314	80	55	45
Living with friends	79	20	50	50

Source: Buron et al. (2010)

Exhibit 6 shows the characteristics of rental assistance applicants based on their living situation before receiving assistance. On average, applicants living with family or friends were more than 4 years younger than applicants living on their own without a subsidy (31.1 versus 35.2 years old). Doubled-up applicants were also more likely to report extremely low incomes than were applicants living on their own without a subsidy (82 versus 72 percent). This factor might explain why a higher proportion of doubled-up applicants were offered assistance compared with applicants living independently—because housing agencies must ensure that a certain percentage of new admits have extremely low incomes. Both groups of applicants had similarly sized households and were almost equally likely to have young children.

On average, rental assistance applicants paid $481 each month for housing, but housing costs varied greatly based on living situation (exhibit 7). Applicants who were living independently spent an average of $771 on housing each month, whereas applicants receiving a housing subsidy spent an average of $434. Applicants who were living with family or friends and helping with the rent spent an average of $401 each month; these applicants paid almost the same in rent as subsidized applicants but paid slightly less for utilities. Also, a large group of applicants who lived with family or friends did not have any housing costs. This analysis does not include the housing costs of applicants who were homeless or living in an institutional setting, because the survey did not collect their housing costs. The survey asked applicants only about the housing costs for the family that they headed and not the total costs of their housing unit, so the total rent of the housing unit is not known.

Exhibit 6

Characteristics of Rental Assistance Applicants by Living Situation

	Applicants (N)	Average Age	Applicants With Extremely Low Incomes (%)	Average Household Size	Applicants With Young Children[a] (%)
Living independently—no subsidy	373	35.2	72	3.4	55
Living independently—with subsidy	158	34.9	78	3.2	63
Living with family or friends	393	31.1	82	3.7	57
Other living arrangement	65	34.5	89	3.3	52
Total	**989**	**33.5**	**78**	**3.5**	**57**

[a] *Children less than 7 years old.*
Note: This information was missing for four respondents.
Source: Buron et al. (2010)

Exhibit 7

Average Monthly Housing Costs of Rental Assistance Applicants

	Applicants (N)	Total Housing Costs ($)	Rent ($)	Utilities ($)
Living independently—no subsidy	373	771	619	152
Living independently—with subsidy	158	434	329	106
Living with family or friends—paying some rent	219	401	330	72
Living with family or friends—paying no rent	174	0	0	0
Total	**924**	**481**	**384**	**97**

Notes: Excludes applicants who were homeless or living in institutional settings. Rent + utilities may not equal total housing costs because of rounding.

Source: Buron et al. (2010)

Housing Needs

This section discusses the incidence of housing needs identified by the household survey, specifically rent burden, homelessness, housing quality issues, and overcrowding. The analysis looks at how housing needs differ based on households' housing status—living independently, living with family or friends, or other—and, whenever possible, compares waitlisted households with very low-income renters in the same metropolitan areas.

Housing Affordability

Exhibit 8 shows the percentage of rental assistance applicants who were rent burdened. The survey did not ask new admits to report their monthly income before receiving rental assistance. Therefore, the analysis of rent burden applies only to applicants who were still waiting to receive assistance. Roughly one-third of all waitlisted applicants were severely rent burdened, an additional 22 percent were moderately rent burdened, and nearly one-half (46 percent) were not rent burdened. Not surprisingly, the incidence of severe rent burden varied significantly depending on housing applicants' living situation. Most (55 percent) applicants living independently without a housing subsidy were

Exhibit 8

Incidences of Rent Burden Among Rental Assistance Applicants by Living Situation

	Applicants (N)	Applicants Not Rent Burdened (0–30% of Income) (%)	Applicants Moderately Rent Burdened (31–50% of Income) (%)	Applicants Severely Rent Burdened (>50% of Income) (%)
Living independently—no subsidy	204	14	31	55
Living independently—with subsidy	72	61	25	14
Living with family or friends— paying some rent or mortgage	84	58	19	23
Living with family or friends— paying no rent or mortgage	85	100	0	0
Total	**445**	**46**	**22**	**32**

Notes: Excludes applicants who were homeless or living in institutional settings. Information on monthly housing costs and/or monthly income was missing for 40 waitlisted applicants.

Source: Buron et al. (2010)

severely rent burdened. Some applicants who were doubled up and paying some rent or receiving a housing subsidy were also severely rent burdened. A substantial portion of applicants did not have any housing costs, however, because they lived with family or friends and did not pay rent.

Rental assistance applicants were far less likely to be severely rent burdened than were very low-income renters identified as having WCN. This finding is not surprising, given that severe rent burden is one of two measures used to identify WCN and that the other measure—severely substandard housing—has become increasingly rare. It is more instructive to compare incidence of severe rent burden among rental assistance applicants with that of all very low-income renters within the same metropolitan area.[3]

This comparison of rental assistance applicants with other very low-income households does not include applicants who were homeless or living in an institutional setting, because these groups are not captured in the AHS. This comparison does, however, include very low-income renters currently receiving rental assistance, because this group is included in the AHS and, as shown in exhibit 4, many assisted households apply for and receive assistance from other housing programs. The AHS comparison group also includes households with people who have disabilities or who are elderly. Filtering these households out of the analysis might have made for a more direct comparison, but it would have restricted the sample size such that the analysis would not have been feasible. The comparison is based on first calculating the proportion of severely rent-burdened respondents within each PHA (for the survey of applicants) and metropolitan area (for the AHS comparison group), then calculating a weighted average based on the number of respondents within each site.[4] Because the waitlisted respondents are not a representative sample, this analysis compares only the incidences of severe rent burden among the *surveyed* waitlisted applicants with that among very low-income AHS *respondents* within the same metropolitan areas. Thus, the results are only suggestive of differences between the total populations of rental assistance applicants and all very low-income renters.

On average, 36 percent of rental assistance applicants were severely rent burdened compared with 56 percent of very low-income renters surveyed by the AHS in the same metropolitan areas. The z-test showed that the difference in means was significant at the .01 level (exhibit 9). Thus, rental assistance applicants were significantly less likely to be severely rent burdened than were very low-income renters in the same metropolitan areas.

The survey asked new admits to report their monthly housing costs both before and after receiving rental assistance. The comparison shows that, on average, new admits experienced a $112 reduction in their monthly housing costs. New admits who had been living independently without a subsidy reduced their housing costs by $390 per month after receiving assistance. Households already receiving a housing subsidy reduced their housing costs by an average of $125. The average monthly housing costs of applicants living with family or friends increased by $93 after receiving assistance (exhibit 10).

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[3] The AHS public data set includes only a household-level metropolitan statistical area code for metropolitan areas with populations of 100,000 or more. Thus, this analysis applies only to the 14 sampled PHAs in 9 metropolitan areas of at least 100,000 people.

[4] Appendix A presents the formulas for the analysis.

Exhibit 9

Comparison of Rent Burden Between Rental Assistance Applicants and All Very Low-Income Renters

	Rental Assistance Applicants (N = 190)	Very Low-Income Renters (N = 381)
Percent of applicants rent burdened by PHA (n = 375)	35.8	55.9
Variance	.0012	.0006
z-test		− 3.3
p-value		< 0.01

AHS = American Housing Survey. PHA = Public Housing Authority.

Notes: Includes only applicants in metropolitan areas with populations of 100,000 or more. Data on very low-income renters are from households within the same metropolitan areas surveyed by the American Housing Survey.

Sources: AHS; Buron et al. (2010)

Exhibit 10

Average Monthly Housing Costs of Newly Admitted Applicants Before and After Receiving Assistance

	All Applicants (N = 426)	Applicants Living Independently— No Subsidy (N = 146)	Applicants Living Independently— With Subsidy (N = 77)	Applicants Living With Family or Friends (N = 203)
Housing costs before assistance	$476	$802	$548	$215
Housing costs after assistance	$364	$412	$423	$308
Change in housing costs after receiving assistance	− $112	− $390	− $125	+ $93

Note: Excludes newly admitted applicants who were homeless or living in institutional settings before receiving assistance.

Source: Buron et al. (2010)

Homelessness and Housing Instability

To assess housing instability, the survey asked rental assistance applicants if, at any time in the past 12 months, they did not have a place of their own to stay.[5] In addition to the 7 percent of applicants who were literally homeless at the time they were interviewed or immediately before receiving assistance, 23 percent of housing applicants reported that they had been without a place of their own to live at some point during the past 12 months (exhibit 11). Among applicants without a place of their own to live, 64 percent reported that this problem persisted for more than 2 months. The survey asked applicants who were without a place of their own to live if they spent time living either in a shelter or on the streets, the HUD definition of literally homeless. Of these applicants, 15 percent reported living in a shelter at some point when they did not have a place of their own and 17 percent reported living on the streets.

Applicants living with friends appeared to be at greater risk of homelessness than applicants living independently or with family. Applicants living with friends were the most likely (54 percent) to

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[5] Waitlisted applicants were asked if, at any time in the past 12 months, they did not have a place of their own to stay, and new admits were asked about the 12-month period immediately before they began receiving assistance.

report being without a place of their own to live in the past 12 months, the most likely (78 percent) to report that this condition lasted for more than 2 months, and the most likely (26 percent) to report living on the streets during this period.

As another measure of housing stability, applicants were asked how long they had been living at their current address.[6] On average, applicants had been living at their current address for more than 3 years (exhibit 12). Applicants living with friends reported the shortest average tenure at their current address (1.8 years) and applicants living with family reported the longest tenure (5 years).

Exhibit 11

Rental Assistance Applicants With No Place of Their Own To Live During the Past 12 Months

	All Applicants (N = 924)	Applicants Living Independently (N = 531)	Applicants Living With Family (N = 314)	Applicants Living With Friends (N = 79)
Percent of applicants with no place of their own to live during the past 12 months	23	15	34	54
Among applicants with no place of their own to live, the percent who…				
… were without a place of their own for more than 2 months	64	54	66	78
… stayed in a shelter	15	16	14	15
… stayed on the streets, in their cars, or in abandoned buildings	17	14	16	26

Notes: Excludes applicants who were homeless or living in institutional settings. Newly admitted applicants were asked about the 12-month period before they were offered assistance.
Source: Buron et al. (2010)

Exhibit 12

Average Tenure at Current Address Among Rental Assistance Applicants

	All Applicants	Applicants Living Independently	Applicants Living With Family	Applicants Living With Friends
Years at current address	3.2	2.3	5.0	1.8

Notes: N = 924. Excludes applicants who were homeless or living in institutional settings. Includes only waitlisted respondents.
Source: Buron et al. (2010)

Substandard Housing

The survey of housing applicants asked waitlisted households about housing quality problems in their current residence and new admits about housing quality problems in their last residence before receiving housing assistance. The housing quality questions asked of applicants were taken from a study of housing quality problems in the Gulf States after Hurricane Katrina.[7] Therefore, the results cannot be compared directly with the AHS questions used to identify renter households in severely substandard housing.

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[6] New admits were asked about their tenure at their last address before receiving assistance.

[7] Appendix B compares each housing quality question asked of applicants with the most similar housing quality question included in the AHS.

More than one-half (51 percent) of rental assistance applicants reported at least one specific problem with the quality of their housing before receiving assistance, and one-third reported two or more problems (exhibit 13). Applicants living independently were more likely to report housing quality problems than were applicants living with family or friends.

Exhibit 14 shows the specific housing quality problems of rental assistance applicants. The most commonly reported housing quality problem, reported by 24 percent of applicants, was mildew, mold, or water damage. Applicants also reported problems with their heating (18 percent), electricity (15 percent), and plumbing (14 percent).

Exhibit 13

Frequency of Housing Quality Problems Among Rental Assistance Applicants

Number of Housing Quality Problems	All Applicants	Applicants Living Independently—No Subsidy	Applicants Living Independently—With Subsidy	Applicants Living With Family or Friends
Percent of households with no housing quality problems	49	45	48	54
Percent of households with one housing quality problem	19	21	20	16
Percent of households with two or more housing quality problems	32	3	32	30

Notes: N = 924. Excludes applicants who were homeless or living in institutional settings. Includes only waitlisted respondents.
Source: Buron et al. (2010)

Exhibit 14

Housing Quality Problems Among Rental Assistance Applicants

Type of Housing Quality Problems	Percent Reporting Problem			
	All Applicants	Applicants Living Independently—No Subsidy	Applicants Living Independently—With Subsidy	Applicants Living With Family or Friends
Mildew, mold, or water damage	24	29	30	18
Floor problems, such as having boards, tiles, carpeting, or linoleum that are missing, curled, or loose	20	21	21	19
Use of oven to heat home in cold weather	18	18	20	16
In the past 3 months, toilet has not worked for 6 hours or more	16	18	12	16
In the past 3 months, electricity has not worked for 2 hours or more	15	13	16	17
In the past 3 months, bathroom floor has been covered by water because of a plumbing problem	14	16	8	14
Holes or large cracks where outdoor air or rain can come in	13	16	16	10
Bad odors such as sewage or natural gas	10	10	9	10

Notes: N = 924. Excludes applicants who were homeless or living in institutional settings. Includes only waitlisted respondents.
Source: Buron et al. (2010)

Overcrowding

The survey of housing applicants asked about the number of residents living in the applicant's housing unit and the number of rooms in the unit, excluding bathrooms and hallways. Exhibit 15 examines the incidence of overcrowding among rental assistance applicants. This analysis considers a housing unit overcrowded if the number of people in the housing unit exceeds the number of rooms. The AHS uses the same measure to identify overcrowded units. The WCN report classifies overcrowding as a "moderate" housing problem rather than a WCN.

Overall, 18 percent of rental assistance applicants lived in overcrowded housing units. Surprisingly, applicants living independently were more likely than applicants living with family or friends to live in overcrowded units. Applicants in subsidized units were the least likely to live in overcrowded housing. Applicants with young children, defined as children less than 6 years old, were three times as likely to be in overcrowded households as other applicants (25 versus 8 percent). Because young children can share a bedroom with their parents or siblings, these units may not feel as crowded as similarly sized units with only adults and older children. Immigrant cultures are often perceived to have more permissive attitudes towards overcrowding and doubling up (Koebel and Renneckar, 2003). This is consistent with our results, which found that Hispanic and Asian applicants were more likely to live in overcrowded conditions.

Exhibit 16 compares the incidence of overcrowding among rental assistance applicants with that of very low-income renters in the same metropolitan area. As in exhibit 9, this comparison is based

Exhibit 15

Percentage of Rental Assistance Applicants Who Live in Overcrowded Housing

	Percent
All applicants (N = 922)	18.0
Applicants by housing status before receiving assistance	
Living independently—no subsidy (N = 373)	19.8
Living independently—with subsidy (N = 158)	14.5
Living with family or friends (N = 391)	17.6
Applicants by presence of children less than age 6	
Households with children less than age 6 (N = 526)	8.0
Households without children less than age 6 (N = 396)	25.0
Applicants by ethnicity	
Hispanic (N = 189)	21.0
Non-Hispanic (N = 727)	14.0
Applicants by race	
White (N = 368)	23.0
African American (N = 472)	12.0
American Indian or native Alaskan (N = 30)	20.0
Asian (N = 11)	55.0
Native Hawaiian or other Pacific Islander (N = 3)	0.0

Notes: Excludes applicants who were homeless or living in institutional settings. Includes only waitlisted respondents. Two applicants were missing information on the number of rooms in their housing unit, 8 applicants were missing information on ethnicity, and 40 applicants were missing information on race.

Source: Buron et al. (2010)

Exhibit 16

Comparison of Overcrowding Among Rental Assistance Applicants and All Very Low-Income Renters in the Same Metropolitan Areas

	Rental Assistance Applicants (N = 403)	Very Low-Income Renters (N = 404)
Percent of households with young children	51	19
Percent of households in overcrowded housing	13.4	6.2
Variance	0.0003	0.0001
z-test	3.46	
p-value	<0.01	
Excluding households with young children		
Number of households	196	328
Percent of households in overcrowded housing	6.1	1.5
Variance	0.0003	0.0001
z-test	2.5	
p-value	0.01	

AHS = American Housing Survey

Note: Includes only applicants in metropolitan areas with populations of 100,000 or more.

Sources: AHS; Buron et al. (2010)

on first calculating the proportion of overcrowded respondents within each PHA (for the survey of applicants) and metropolitan area (for the AHS comparison group), then calculating a weighted average based on the number of respondents within each site.[8]

Rental assistance applicants were more than twice as likely as very low-income renters to live in overcrowded housing (13 versus 6 percent). These differences were statistically significant at the .01 level. Rental assistance applicants were also more likely than very low-income renters to have young children (51 versus 19 percent). Excluding households with young children from both population groups, applicants were still significantly more likely than very low-income renters to live in overcrowded housing (p-value = 0.01).

Household Formation

Low-income families may choose to live with people with whom they would rather not live if they could afford to live independently. The inability to form a household of one's own is not considered a WCN, but it could be a major reason why households choose to apply for rental assistance (Shroder, 2002). Of applicants on waiting lists for rental assistance, 63 percent reported that they currently lived with one or more other adults. More than two-thirds of these applicants (68 percent) said that they did not plan on living with all of the other adults in their household after receiving assistance (exhibit 17). The preponderance of single female-headed households in assisted housing is sometimes viewed as a negative effect of the rent subsidy structure, because

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[8] Appendix A presents the formulas for the analysis. As noted, regarding exhibit 9, because the waitlisted respondents are not a representative sample, this analysis compares only the incidence of overcrowding among the surveyed waitlist applicants with that among very low-income AHS respondents within the same metropolitan areas. Thus, the results are only suggestive of differences between the populations of rental assistance applicants and all very low-income renters.

Exhibit 17

Influence of Rental Assistance on Number of Adults in Household

	Number	Percent
Waitlisted applicants living with other adults	328	63
Will all adults remain with you after receiving assistance?[a]		
Yes, all adults will remain in household after receiving assistance	100	32
No, not all adults will remain in household after receiving assistance	216	68

[a] *Information is missing for 12 respondents living with other adults.*
Source: Buron et al. (2010)

households with multiple wage earners pay higher rents. Most applicants, however, reported that the reason other adults would not live with them after receiving assistance was because they preferred to live independently.

Discussion

Although these results are from a relatively small and not nationally representative sample, they have important implications for our understanding of housing needs and the function of rental assistance programs. These results suggest that households that apply for and receive rental assistance differ in important ways from WCN renters or from very low-income unassisted renters in general. Specifically, more than one-half of rental assistance applicants are not rent burdened, because they are either doubled up with family or friends or receiving some form of housing assistance. These applicants may experience a variety of other housing-related hardships, however, including homelessness, substandard housing, and a lack of independence.

The results do not suggest that the WCN report is in error. The WCN measure is an assessment of housing needs among very low-income unassisted renters and does not claim to represent the housing needs of all rental assistance applicants. In addition, nothing requires that PHAs prioritize WCN applicants over applicants with other housing needs. The WCN report, however, is meant to inform our understanding of the need for housing assistance and the form that assistance should take. Housing policy experts sometimes use WCN households as a proxy for households that apply for and receive rental assistance. The survey results suggest that relying on the WCN measure to understand the effects of rental assistance overestimates the direct financial benefits of assistance in reducing housing costs and underestimates its benefits for increasing housing consumption. This reliance may also lead policy experts to underestimate the value of the nonfinancial components of rental assistance programs, such as housing quality standards.

The WCN measure would be more representative of rental assistance applicants if it included homeless people and households already receiving some form of government housing assistance. Nearly one-fourth of rental assistance applicants fall into one of these two categories.

The WCN measure does not include homelessness because the AHS captures only people living in housing units. At the time the WCN measure was initially developed, no regular efforts were made to count the homeless, and national estimates of homelessness varied widely (Koebel and Rennecker, 2003). HUD now produces an Annual Homeless Assessment Report to Congress and

its estimates of homelessness have become increasingly precise. In 2010, almost 650,000 people were homeless on a single night and more than 1.6 million people used homeless shelters during a 12-month period (HUD, 2011). Despite the danger of double counting households that experience both homelessness and other WCN, including homeless households in the WCN report would lead to a more accurate picture of WCN and the need for rental assistance.

The WCN measure excludes households currently receiving some form of housing assistance, because these households are assumed to be living in adequate and affordable housing, but 16 percent of rental assistance applicants reported already receiving some form of housing subsidy. Of those applicants, 25 percent were severely rent burdened and 55 percent reported at least one housing quality problem. Thus, some subsidized applicants appear to have serious housing problems. In addition, other studies of AHS data have shown that households often mistakenly believe that they are receiving housing assistance (Koebel and Renneckar, 2003). Excluding these households may cause the WCN report to underestimate the number of WCN households.

Although many rental assistance applicants were not rent burdened, they did experience a variety of hardships related to the quality of their housing. More than one-half of applicants reported one or more housing quality problems. This finding is surprising, because incidences of severely substandard housing, as measured by the WCN report, have become rare, which is often taken as an indication that even low-income renters live in housing that is physically adequate (Grigsby and Bourassa, 2004).

The housing quality questions asked in the survey were originally developed to test damages to the housing stock after Hurricanes Katrina and Rita and are not specific enough to judge the severity of applicants' housing problems. Nonetheless, some of the problems reported by applicants are potentially very serious. Almost one-fourth of applicants lived in housing with mold or water damage, which is positively associated with asthma and other respiratory problems (Bush et al., 2006). More than 10 percent of applicants reported problems with their electric, plumbing, or heating systems. Some of these problems may be only inconveniences, whereas others are potentially unsanitary and unsafe. In addition, 18 percent of applicants lived in overcrowded housing, which has been associated with higher risk of meningitis, tuberculosis, and other respiratory problems (Office of the Deputy Prime Minister, 2004) and food insecurity (Cutts et al., 2011). The housing quality problems reported by applicants are especially troubling because most of these households include young children.

The high incidence of housing quality problems suggests that the lack of adequate, affordable housing is a public health issue as well as an economic and social problem. Further research is needed to determine the severity of these housing quality problems and whether they are representative of problems among very low-income households in general. These results suggest, however, that the quality of the housing stock for very low-income households remains a problem even if incidences of severely substandard housing have become rare.

Finally, the survey of applicants raises some questions about whether rental assistance is being targeted to households with the greatest housing needs. Of new admits, 16 percent were already receiving some form of housing assistance. Although subsidized applicants were not immune from having severe rent burdens or other housing hardships, they were less likely to experience these

problems than were applicants not receiving a subsidy. Assisted applicants, however, were as likely to be selected from the waiting list as were unassisted applicants. PHAs may want to consider prioritizing waitlisted households that are not already receiving housing assistance.

The treatment of doubled-up applicants is a larger issue for rental assistance policy. Among all rental assistance applicants, 40 percent were living with family or friends. Because these applicants were typically extremely low income, they appear to be more likely to be selected from the waiting list than applicants living independently without a subsidy. Doubled-up applicants, however, were less likely than applicants living independently to be severely rent burdened or to live in housing with quality problems and, counterintuitively, were also less likely to live in overcrowded housing.

These findings raise the question of whether doubling up is a solution to a housing affordability problem or is a serious housing need itself. The answer depends in part on the stability of the doubled-up arrangement. The survey results suggest that living with friends is often not a viable long-term living arrangement. Applicants living with friends had the shortest average tenure at their current address and were the most likely to report being literally homeless at some point during the past 12 months. By contrast, applicants living with family had been in their current address for an average of 5 years, suggesting that, for many applicants, living with other family members was a stable, long-term living arrangement.

Of course, anyone who has ever moved back in with their parents recognizes that this is not an ideal long-term living arrangement and many doubled-up applicants have a strong desire to form their own households. A number of studies have established that rental assistance is a means by which families can create their own household (Shroder, 2002). Household formation has psychological benefits; assisted households have reported decreases in stress and depression as a result of having their own home rather than having to "mooch" off of family or friends (Wood, Turnham, and Mills, 2009). Household formation may also have positive effects for the development of human capital and for the overall economy (Painter, 2010; Shroder, 2002). Some policymakers may not see household formation as one of the primary goals of rental assistance, however, and may prefer to see scarce resources allocated to applicants who are homeless, severely rent burdened, or living in substandard housing.

Appendix A. Formulas for Comparing Rental Assistance Applicants With Very Low-Income Households in the Same Metropolitan Area

The comparison of incidences of severe rent burden and overcrowding between the two groups was done using a weighted average approach. The mean was calculated for each site, and the weighted mean was the average across all sites weighted by the number of respondents in each site such that

$$m_s = \frac{1}{N_s} \sum_{j=1}^{\bar{}} \left(m_{sj}\right)\left(n_j\right)$$

where

$$m_{sj} = \frac{1}{n_j} \sum_{i=1}^{n_j} x_{ij}$$

so that

$$m_s = \frac{1}{N_s} \sum_{j=1}^{N_s} \sum_{i=1}^{n_j} \left(\frac{x_{ij}}{n_j}\right)\left(n_j\right)$$

where

M_s = the mean outcome for the survey sample;

N_s = the total number of survey respondents;

M_{sj} = the mean outcome for the survey sample in the site;

N_j = the number of survey respondents in the jth site; and

X_{ij} = the outcome for the ith respondent in the jth site.

The calculation used to determine the variance of each population was

$$\sigma^2 = \frac{p(1-p)}{n}$$

where
P = the weighted mean outcome; and
N = the number of respondents.

Appendix B. Comparison of Housing Quality Questions Asked in the Survey of Rental Assistance Applicants and the American Housing Survey

Survey of Rental Assistance Applicants	Most Comparable Question in the American Housing Survey
Does your housing have any mildew, mold, or water damage on any wall, floor, or ceiling?	Did water leak in from the outside within the past 12 months?
Does your housing have any floor problems such as boards, tiles, carpeting, or linoleum that are missing, curled, or loose?	Are any holes in the floors big enough for someone to catch their foot on?
Does your housing have any holes or large cracks where outdoor air or rain can come in?	In the inside walls or ceilings of this housing unit are there any open holes or cracks wider than the edge of a dime? Does the roof have any holes?
Does your housing have any bad odors such as sewer, natural gas, etc.?	Has the sewage system broken down in your home since the last interview?
	[If yes] How many of these breakdowns lasted 6 hours or more?
In the last 3 months has any bathroom floor been covered by water because of a plumbing problem?	Did water leak in from the outside within the past 12 months?
In the last 3 months has your toilet not worked for 6 hours or more?	Was there any time in the last 3 months when the toilet broke, or stopped up, or otherwise was not working, so you couldn't use it?
	[If yes] And how many of those times was the toilet not working for 6 hours or more?
In the last 3 months has your electricity not worked for 2 hours or more?	Does every room have an electric outlet or wall plug that works?
	Have any fuses blown or circuit breakers tripped in your home?
	[If yes] How many times have fuses blown or breakers tripped in last 3 months?
In cold weather, do you ever need to use your over to heat your home?	Last winter, for any reason, was your housing unit so cold for 24 hours or more that it was uncomfortable?
	[If yes] Was that because the main heating equipment broke down?
	[If yes] How many times did main heating equipment break down for 6 hours or more?

Acknowledgments

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Geographic Patterns of Regional Unemployment Versus Unemployment Compensation in the United States—2009

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The opinions expressed in this article are those of the author and do not necessarily reflect those of the U.S. Department of Housing and Urban Development.

In 2009, the unemployment rate was the highest it has been in the United States since 1982 (BLS, 2012a). Cresting at 10 percent, the unemployment rate coincided with one of the most serious economic downturns in U.S. history. State governments respond to unemployment by providing compensation through insurance. Unemployment insurance comes from state-managed funding that provides monetary compensation to workers who have suffered job loss.[1] Unemployment compensation acts as a stabilizer for both family incomes and local economies. Individual state policies affect unemployment compensation amounts and eligibility. Unemployment compensation, then, may have geographic patterns that differ from unemployment rates and reveal the extent to which states are attempting to buffer the fallout from unemployment.

Location Quotients (LQs) used in this analysis highlight relative differences in the geographic patterns of unemployment rates (BLS, 2012b) and compensation levels (BEA, 2012) across the

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[1] For a general description of unemployment benefits, see http://en.wikipedia.org/wiki/Unemployment_benefits.

nation. The LQ is simply the ratio of the county unemployment rate, or the share of unemployment benefits in the county's personal income, to its national counterpart. If a county's LQ is 1, it has the same unemployment rate (dependence on unemployment benefits) as the nation. A divergent color scheme for both unemployment rates and compensation levels shows whether counties have a similar (white), lesser (light gray), or greater (dark gray) LQ than the nation.

Exhibit 1 shows regional unemployment patterns by county in 2009, with clear regional distinctions. Approximately 51 percent of counties had rates of unemployment similar to the national rate (LQs between 0.76 and 1.24). An extensive and cohesive pattern of lower unemployment rates dominates the Great Plains states of Montana, North Dakota, South Dakota, Nebraska, Iowa, and Oklahoma. Nebraska, North Dakota, and South Dakota are made up almost entirely of counties with unemployment rates that were less than one-half the national rate.

The Northeastern states from Maine to Virginia show a regional pattern with similar to lower unemployment rates compared with the national rate. Michigan, California, and Oregon had a much higher than normal unemployment level, with most counties in these states having an unemployment rate of 1.25 to nearly 3 times greater than the national rate. Several localized clusters in the Southern states have unemployment rates higher than the national rate.

Exhibit 1

County Shares of the Unemployment Rate in 2009 for the Contiguous 48 States—(manual classification of location quotient breaks)

Unemployment Rate (2009)
Location Quotient

- 0.00–0.50 (lowest)
- 0.51–0.75
- 0.76–1.24 (similar to the national)
- 1.25–1.49
- 1.50–2.75 (highest)
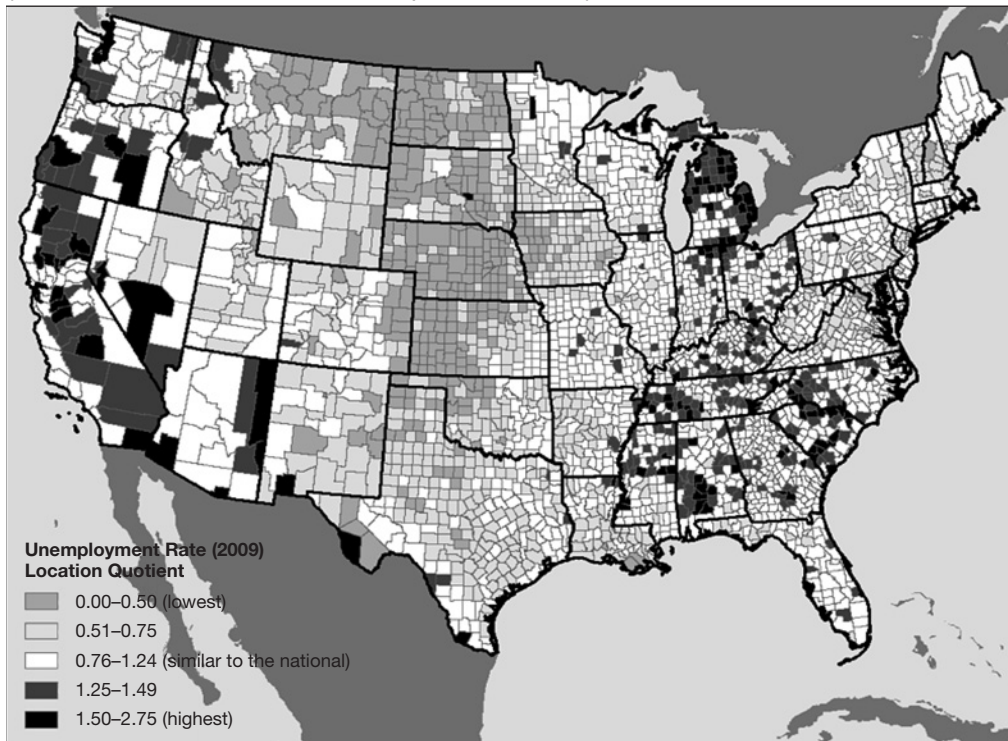
Exhibit 2 also shows clear regional patterns of unemployment insurance benefits by county in 2009.[2]

Unemployment insurance patterns in exhibit 2 are far more geographically divergent than the unemployment rates shown in exhibit 1. Only 34 percent of counties had similar levels (LQ values between 0.76 and 1.24) of unemployment compensation compared with the national level. Rust Belt and West Coast states had an extensive, cohesive pattern of counties with 1.5 to 3 times greater levels of unemployment compensation than the national level. Local clusters of unemployment insurance compensation are also present in the Southern states but are somewhat more geographically extensive than the unemployment rate pattern in exhibit 1. Clusters of extreme values in Indiana, Illinois, Wisconsin, Minnesota, and central Pennsylvania are visible in exhibit 2 that have no counterparts in exhibit 1.

In both exhibits, regional patterns of low unemployment rates and insurance compensation levels clearly overlap in the Great Plains states, but unemployment compensation extends farther down

Exhibit 2

County Shares of Unemployment Insurance Benefits as a Percentage of Personal Income in 2009 for the Contiguous 48 States—(manual classification of location quotient breaks)

Unemployment
Insurance (2009)
Location Quotient
- 0.00–0.50 (lowest)
- 0.51–0.75
- 0.76–1.24 (similar to the national)
- 1.25–1.49
- 1.50–2.75
- 2.76-7.37 (highest)
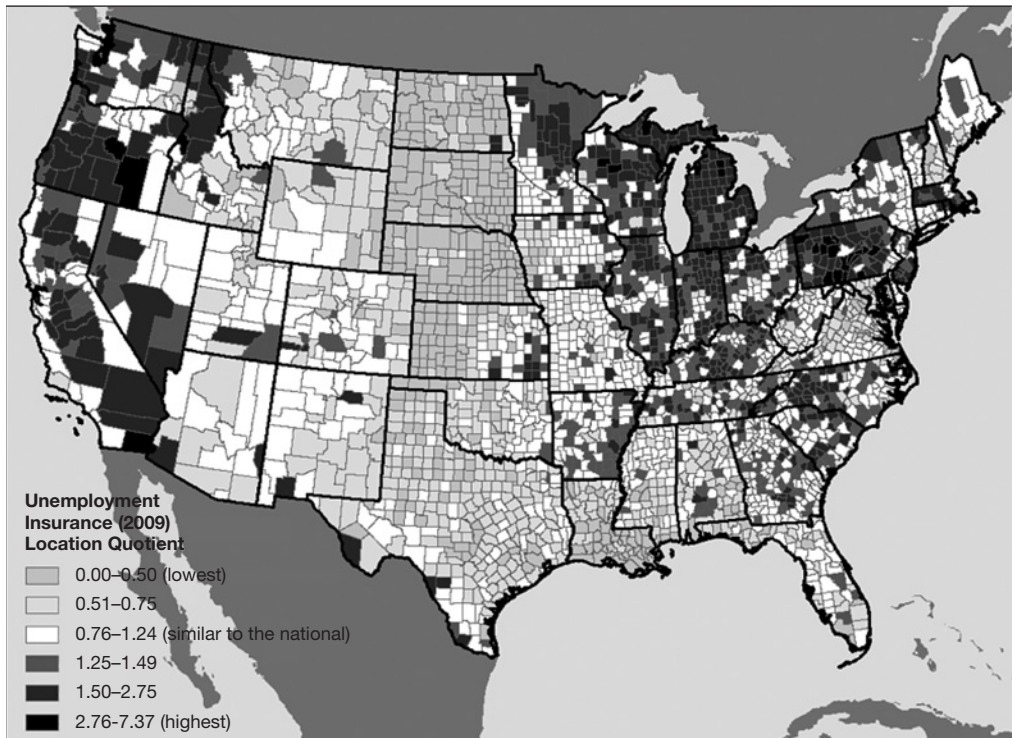
[2] Because of the fat upper tail of the distribution, a class break was added to the map in exhibit 2 to better describe it. Note that the highest value in exhibit 1 is 2.75, but the highest value in exhibit 2 is 7.37.

to Texas and Louisiana. Exhibit 2 reveals that many states in the Mississippi River Valley have high unemployment compensation levels around city centers but have lower levels in rural areas. Exhibit 1 indicates unemployment rates are lower or similar in rural areas. Unlike the patterns in exhibit 1, the Northeastern states in exhibit 2 show a number of counties in New York, New Jersey, Massachusetts, and, in particular, Pennsylvania with higher levels of unemployment compensation than the national level.

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Introducing the Ohio New Establishment Dynamics Data

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Abstract

The Ohio New Establishment Dynamics (O-NED) data set tracks the number of establishments that first started employing people between the second quarter of 1997 and the first quarter of 2008 and measures the employment and payroll data for these new establishments. O-NED enables researchers to measure the growth trends of cohorts of new establishments for up to 5 years after the cohort's birth. These data are the first publicly available data that document the growth rates of new establishments at the substate level. The finest unit of geography O-NED measures is a county. This article describes how O-NED is constructed and defines the variables included in the data. It closes with two examples of how researchers can use the data.

Introduction

Publicly available data are insufficient to answer a number of questions about the growth patterns of new establishments. The Ohio New Establishment Dynamics (O-NED) data set fills some of those gaps for Ohio. An establishment is a single physical location of a firm; firms can have one or many establishments. O-NED focuses on establishments during their first 5 years, a crucial shakeout period for new establishments. New establishments are grouped based on their year of birth and these groupings are called *birth cohorts*. O-NED provides annual tabulations of the number of establishments, employment, and wages for each birth cohort. The tabulations cover the period from April 1, 1997, through March 31, 2008. The data include 11 cohorts—7 for 5 full years and 4 for less than 5 years. O-NED provides separate tabulations for births unaffiliated with preexisting firms (entrepreneurial births), births affiliated with preexisting firms (other births), and fast-growing births (gazelles) of each type. O-NED is an outgrowth of the work of Knaup (2005), Knaup and Piazza (2007), Talan and Hiles (2007), and the U.S. Bureau of Labor Statistics (BLS) Entrepreneurship Team. This article describes the construction and structure of the data and provides two examples of research that the data enable.

Few data measuring business dynamics are publicly available. In 2004, BLS introduced the Business Employment Dynamics (BDM) data series. The quarterly BDM data series enables data users to measure the job creation and destruction and the establishment birth and death numbers that underlie the employment totals published in the Quarterly Census of Employment and Wages (QCEW). The BDM quarterly update is released 7 months after the quarter it covers. BLS continues to improve the BDM data by adding new features. One limitation of the BDM data is that the finest level of geographic detail is the state and the finest level of industry detail is the major sector. Furthermore, it contains only measures of establishment births and deaths and does not shed light on the growth patterns and survival rates of new establishments over time.

In December 2008, the U.S. Census Bureau began releasing Business Dynamics Statistics (BDS), which tabulates annual job creation and destruction statistics by firm age and either firm size or initial firm size. The tabulations are available for the United States as a whole or by sector or state. BDS and O-NED are similar because both measure the employment and number of establishments in businesses aged 1 to 5 years and enable analysts to track how employment and the number of establishments change over time. Several important differences exist, however. O-NED is designed for tracking the growth of new establishments in Ohio for their first 5 years, whereas BDS provides a more comprehensive set of job creation and destruction statistics for the nation as a whole. O-NED uses establishment age and measures firm age only for entrepreneurial establishments, for which firm age equals establishment age. BDS uses firm age, not establishment age, and includes more firm age categories. O-NED provides more geographic detail than BDS, but BDS includes tabulations by firm size and decomposes changes in employment and the number of establishments into the portions due to new entrants, continuing establishments, and exiting establishments. BDS covers the United States as a whole from 1977 through 2009, whereas O-NED covers only Ohio from 1997 through 2007.

Construction of the O-NED Data

The microdata we used to create O-NED is a combination of the longitudinally linked QCEW microdata from BLS and the edited ES202 data housed at the Maxine Goodman Levin College of Urban Affairs (Levin College) at Cleveland State University.[1] Both data sets cover only Ohio and are provided through a special partnership between Cleveland State University and the Bureau of Labor Market Information of the Ohio Department of Jobs and Family Services (ODJFS). BLS provided the longitudinally linked QCEW microdata to ODJFS for this project.

We combined the microdata sources to take advantage of edits that researchers at Levin College made over a period of years. In particular, O-NED took industry and geography codes from the edited ES202 data. One challenge in creating tabulations for cohorts of establishments is that the industry and geography codes of establishments can change over time. New establishments are especially likely to have code changes because some enter the data set with incomplete information, and BLS and ODJFS assign those establishments codes after they have gathered more information. To minimize the effect of these code changes on our tabulations, we applied the last valid codes we had for establishments to the data for all quarters.[2]

The O-NED data cover establishments born from April 1, 1997, through March 31, 2008. The sample is restricted to private establishments that did not experience any identifiable splits or consolidations during their first 5 years. This restriction greatly reduced the volatility in the data, because most splits are not truly new establishments but are continuing establishments that changed how they report their data.

Most splits are identified using relevant comment codes, but some are identified based on substantial changes in the number of establishments affiliated with a single employer identification number (EIN). EIN is used to determine which establishments belong to the same firm. Based on careful exploration of the data, we developed a set of rules to identify these splits by finding EINs that simultaneously have increases in their number of establishments and unusually large decreases in the average size of their establishments. Most cases treated as uncoded splits are those for which the EIN had employment of more than 50 people in the birth quarter, the number of establishments grew from the quarter before the birth to the birth quarter, and the average employment of establishments affiliated with the EIN fell by 80 percent from the quarter before the birth to the birth quarter. It is harder to identify splits for EINs with few units or little employment, and, based on exploring the data, we developed a conservative formula to identify these small splits.[3]

To further reduce the problem of false births, we examined the data for about 250 large EINs in which it was unclear whether the EINs experienced splits or had an unusually large number of

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[1] The ES202 is Ohio's version of the QCEW microdata and is based on establishment data collected as part of the unemployment insurance system.

[2] If an establishment has an invalid code, such as a county code of 999, we use a previous, valid code when possible. We assign cases that have only invalid codes that change a single invalid code for all quarters. Based on our work verifying a subset of the code changes, we believe that no more than 25 percent of the code changes lost by pushing back codes were valid changes.

[3] See Elvery and Cyran (2010) for more details on this and other topics.

births. We examined the data to see if the new establishments affiliated with the EINs had pre-decessors or if ownership changed, which would suggest they are false births. For most cases, we did not find conclusive evidence that they were false births and treated them as births. Even with the careful use of the data and hand checking of large births suspected to be false, it is likely that some false births remain in the data. Uncoded splits and ownership changes, which can appear to be births, are more prevalent for new establishments affiliated with preexisting EINs than for other new establishments. Therefore, we believe that entrepreneurial births are less likely to be false births than are nonentrepreneurial births (Elvery and Cyran, 2010).

One goal of O-NED is to demonstrate what can be created with existing BLS microdata. As such, we use definitions that are consistent with those BLS uses for BDM and those proposed by the Organisation for Economic Co-operation and Development (Ahmad, 2006). Exhibits 1 and 2 provide the precise definitions. A birth is any new establishment in the state, regardless of its ownership. An entrepreneurial birth is the birth of a new firm, not an additional establishment of an existing

Exhibit 1

Definitions of Terms

Term	Definition
Establishment	A work site in the QCEW/ES202 data.
Year *t*	The last three quarters of calendar year *t* combined with the first quarter of the following calendar year.
Quarter 1 of year *t*	First quarter of the calendar year following calendar year *t*.
Birth	The addition of an establishment with positive employment to the QCEW/ES202 data. Establishments that are combinations of previous establishments will not be considered births. Establishments that split off from existing establishments will be births.
Entrepreneurial birth	An establishment birth wherein the establishment has a UI account number and EIN that is unique in Ohio. The establishment must also not be a split off from an existing establishment.
Birth cohort *j*	Establishments born in year *j*.
Entrepreneurial birth cohort *j*	Establishments that had an entrepreneurial birth in year *j*.
Establishment counts	The number of establishments in the first quarter of year *t*.
Employment	Average reported monthly employment in quarter 1 of year t. Employment at birth is the reported employment for the quarter the establishment is born. Employment is restricted to employment covered by the Ohio UI system.
Wages	Reported wages for quarter 1 of year *t* or, in the case of wages at birth, during the quarter the establishment is born.
Total wages	Reported wages for all of year *t*.
Gazelle in year *t*	An establishment with an average annual employment growth rate of at least 20%, averaging across years *t*, *t*–1, and *t*–2, and that has employment of at least 10 in year *t*–2.

EIN = employer identification number. ES202 = establishment data from the Ohio unemployment insurance sysytem.
QCEW = Quality Census of Employment and Wages. UI = unemployment insurance.

Exhibit 2

Definitions of Data Elements Included in the O-NED Data

Data Element	Value	Definition
EMP	502	Sum of average monthly employment during quarter 1 of 2002 for the 1997 cohort.
QTRWAGE	3382199	Sum of quarterly wages during quarter 1 of 2002 for the 1997 cohort.
ANNLWAGE	14215225	Sum of quarterly wages from quarter 2 of 2001 through quarter 1 of 2002 for the 1997 cohort.
UNITS	49	Number of establishments from the 1997 cohort active in quarter 1 of 2001.
EMPVSYR1	125	EMP as a percentage of the cohort's employment at the end of year 1.
UNITSVSYR1	73	UNITS as a percentage of the number of establishments in the cohort at the end of year 1.
QWVSYR1	144	QTRWAGE as a percentage of the cohort's employment at the end of year 1.
AWVSYR1	323	ANNLWAGE as a percentage of the cohort's employment at the end of year 1.
EMPVSPY	81	EMP as a percentage of the cohort's employment in quarter 1 of 2001.
UNITSVSPY	88	UNITS as a percentage of the number of establishments in the cohort in quarter 1 of 2001.
QWVSPY	83	QTRWAGE as a percentage of the cohort's employment in quarter 1 of 2001.
AWVSPY	91	ANNLWAGE as a percentage of the cohort's employment in quarter 1 of 2001.

O-NED = Ohio New Establishment Dynamics.

Notes: O-NED tabulates the variables for each cell. A cell is a combination of a unit of geography, a unit of industry, a type of birth or gazelle, a cohort, and an age. The values come from the cell of Akron CBSA, manufacturing sector, entrepreneurial births, 1997 cohort age 4.

firm.[4] A nonentrepreneurial birth is a new establishment affiliated with an existing firm. Year of birth is the year the establishment first entered the data. Survival to age *t* is defined as having positive payroll and employment for at least one quarter of the year that is *t* years after birth year. Employment and wages are those covered by the unemployment insurance system of Ohio.

Although the QCEW data are updated quarterly, we annualize the data. We define a year as a set of four quarters, starting with the second quarter of a calendar year and ending with the first quarter of the following year. For example, establishments that first enter the data from the second quarter of 1998 through the first quarter of 1999 would be counted as part of the cohort born in 1998. This unit of time is chosen because the QCEW microdata register a disproportionate share of establishment births in the first quarter of the year. A portion of these establishments were likely actually born earlier, so keeping them with those born in the previous three quarters groups establishments by cohort more effectively than using calendar years would. Using four quarters enables more geographic and industry detail by increasing the number of establishments per data cell. Focusing on annual data also keeps the data tractable for a broad group of potential users.

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[4] An EIN is treated like a firm. An EIN of all zeros is sometimes given to new establishments until they report their permanent EIN. Therefore, we treat a birth with an EIN of all zeros as an entrepreneurial birth.

Data users can conduct many different and rich analyses with O-NED, but the utility of the data depends on how much industry and geographic detail is feasible, given the simultaneous need for confidentiality and reliability. We experimented with a variety of ways to balance the tradeoffs of detail, usability, and sample size. In the end, O-NED tabulations are made for the following units of geography: Ohio; Metropolitan Status (central county of metropolitan area, other metropolitan, and nonmetropolitan); Economic Development Region;[5] Metropolitan/Nonmetropolitan status by Economic Development Region; Core Based Statistical Area (CBSA); Central/Noncentral county status by CBSA; and County. The industry detail varies by geography. A cross-sector total is available for all units of geography. Major sector-level tabulations are available for all units of geography except County. Three-digit North American Industry Classification System-level tabulations are available only for Ohio as a whole.

Description of Data Files

This section describes the file structure and variables included in the publicly released O-NED tabulations.[6] The data are released in two formats, a SAS data file and a set of Microsoft Excel® spreadsheets.

Each observation in the data represents a unique combination of a unit of geography, a unit of industry, a type of birth or gazelle, a cohort, and an age. For each observation, the data include the number of establishments, employment, first quarter wages, and annual wages. Survival rates of these variables, both year-to-year and compared with the end of the birth year (age = 1), are also included. Each observation also includes variables to identify the observation and navigate the data, including indicators of geographic and industry detail, geography codes, sector codes, cohort, age, year of data, an indicator for type of birth and whether it is a set of births or a set of gazelles, and a suppression flag.

Exhibit 2 shows the core data elements for one observation of data. These data cover the year 2001 for entrepreneurial births from the 1997 cohort that are located in the Akron, OH CBSA and are in the manufacturing sector. The exhibit includes the values and a description of what the numbers mean for this observation. The 49 establishments remaining in this cohort after 4 years had 502 employees and paid them a total of $14.2 million from the second quarter of 2001 through the first quarter of 2002. The cohort had 25 percent more employment and 27 percent fewer establishments than in its birth year, and both employment and the number of remaining establishments declined between years 3 and 4.

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[5] Ohio's Economic Development Regions are contiguous combinations of counties that have similar or intimately linked economic functions. They are defined by the Ohio Department of Development.

[6] The data and documentation are available online at http://urban.csuohio.edu/economicdevelopment/ONED.html.

Although the structure of the data is space efficient and easy to navigate, data users will find that they need to combine data across observations to calculate some statistics. For example, calculating the percentage of all entrepreneurial births from the 2003 cohort that were gazelles at age 5 requires combining the number of establishments in the cohort at age 0 and the number of gazelles in the cohort at age 5.

Example of Use

The key feature of O-NED is that it measures the growth trends of cohorts of new establishments for the crucial first 5 years of existence.[7] Using survival rates—the cohort's percentage of the initial number of establishments surviving or the percentage of year-1 employment retained—is a convenient way to measure these trends. Exhibit 3 graphs establishment and employment survival rates for groups of cohorts with 5 years of complete data. Exhibit 3a shows establishment survival rates for grouped cohorts. Establishment survival rates among cohorts were similar for all years and were unaffected by whether the business was started before or during the 2001-to-2002 recession. About 85 percent of all establishments survived to the second year. At the end of year 5, slightly more than one-half (53 percent) of all establishments were still in existence for all cohorts.

Business establishments that started toward the end of the expansionary years (1997 and 1998 cohorts) saw steep declines in employment after their third year (exhibit 3b). These establishments initially grew very rapidly but experienced a big hit during the recessionary years. For establishments that started just as the recession began (1999 and 2000 cohorts), employment also decreased with the onset of the recession. The decline in employment was at a lesser rate than the previous cohorts, however, because these establishments did not have the chance to grow as much as companies that were started in previous years. Establishments that started during the recession are grouped in the 2001 to 2003 cohorts. As seen on the graph, these establishments were not severely affected; they declined in employment at a much slower rate.

Exhibit 4 provides the size and survival trends of the average birth cohort for each of the eight largest CBSAs in Ohio. This exhibit covers only entrepreneurial births, not those affiliated with an existing firm. Entrepreneurial businesses in the Columbus, OH CBSA had more employment growth than those in other CBSAs, with an employment survival rate of 111.0 percent.[8] Entrepreneurial establishments represented a higher-than-typical share of total employment in the Columbus, OH CBSA, where the average cohort had 1.53 percent of total employment in its fifth year. The Cleveland, OH CBSA had the largest number of new births and the largest amount of employment, however. In five of the CBSAs, the average cohort of entrepreneurial births had less employment at the end of the fifth year than at the end of the first year.

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[7] We draw these examples from Yamoah, Austrian, and Elvery (2009).

[8] We create the survival rates by averaging across each cohort's survival rate and, therefore, the rates do not match what one would calculate based on the cohort sizes in years 1 and 5.

Exhibit 3

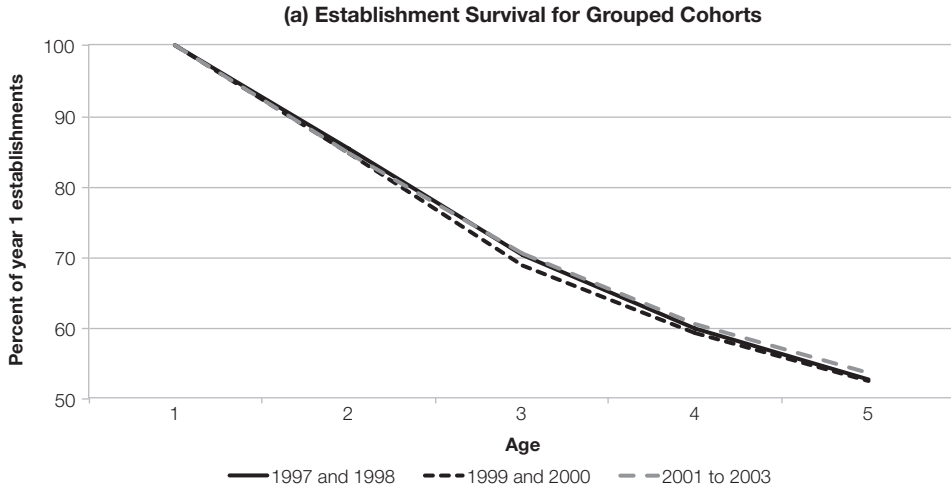Establishment (a) and Employment (b) Survival Rates for Grouped Cohorts

(a) Establishment Survival for Grouped Cohorts

(b) Employment Survival for Grouped Cohorts

Exhibit 4

Size and Survival of Average Entrepreneurial Birth Cohort, by CBSA

CBSA	Year 1		Year 5		Survival Rate		Employment Share	
	Employment	Units	Employment	Units	Employment	Units	Year 1	Year 5
Cleveland	11,433	3,224	12,243	1,888	101.2%	56.7%	1.26%	1.38%
Columbus	9,795	2,670	11,233	1,441	111.0%	53.5%	1.35%	1.53%
Cincinnati	8,859	2,349	9,728	1,290	104.7%	53.4%	1.28%	1.41%
Dayton	3,760	963	3,949	547	99.3%	54.3%	1.11%	1.20%
Akron	3,625	987	3,790	588	98.6%	57.3%	1.35%	1.40%
Toledo	3,293	832	3,330	478	96.3%	54.9%	1.22%	1.25%
Youngstown	2,331	557	2,131	325	84.8%	56.1%	1.42%	1.33%
Canton	1,733	490	1,765	285	97.3%	56.4%	1.15%	1.20%

CBSA = Core Based Statistical Area.

Conclusion

O-NED data described in this article are now available to anyone who wants to use them through the website of the Center for Economic Development at the Levin College at Cleveland State University. These data provide the first set of publicly available tabulations of establishment and employment survival for multiple cohorts from longitudinally linked QCEW microdata. O-NED includes tabulations down to the county level, providing more geographic detail than any comparable data. The value of these data will be determined by how analysts and researchers use it. We encourage people to dive in and use it and hope that similar data can be made available for the nation as a whole.

Acknowledgments

Authors

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Additional Reading

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Impact

A regulatory impact analysis must accompany every economically significant federal rule or regulation. The Office of Policy Development and Research performs this analysis for all U.S. Department of Housing and Urban Development rules. An impact analysis is a forecast of the annual benefits and costs accruing to all parties, including the taxpayers, from a given regulation. Modeling these benefits and costs involves use of past research findings, application of economic principles, empirical investigation, and professional judgment.

Impact Analysis of the Proposed Rule on Streamlining the Portability Process in the Housing Choice Voucher Program

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The opinions expressed in this article are those of the author and do not necessarily reflect those of the U.S. Department of Housing and Urban Development.

Abstract

Proposed regulatory changes would streamline the portability process in the Housing Choice Voucher Program (HCVP) and enable public housing authorities (PHAs) to better serve families and expand housing opportunities. The proposed rule would yield intangible benefits to program participants and, if successful, increase financial transfers between PHAs. The regulatory action would not be economically significant under Executive Order 12866[1] and Office of Management and Budget Circular A-4,[2] however, because the aggregate financial impact is far less than the $100 million annual threshold.

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[1] See OMB (1996).

[2] See OMB (2003).

Background

The Housing Choice Voucher Program (HCVP) is the largest subsidized housing program in the United States that helps very low-income families, elderly people, and disabled people afford decent, safe, and sanitary housing in the private market.[3] In 2010, the HCVP subsidized rents for more than 2.5 million low-income households for an estimated $17.3 billion.[4]

Program Description

Public housing authorities (PHAs) locally administer housing choice vouchers using federal funds from the U.S. Department of Housing and Urban Development (HUD). A family that is issued a housing choice voucher is responsible for finding a suitable housing unit of the family's choice where the owner agrees to rent under the program. This rental unit (which may include the family's current residence) must meet minimum standards of health and safety, as determined by the PHA, which pays a housing subsidy directly to the landlord on behalf of the participating family. The family then pays the difference between the actual rent charged by the landlord and the amount that the HCVP subsidizes.[5] Under certain circumstances, if authorized by the PHA, a family may use its voucher to purchase a modest home.

Eligibility

The PHA determines eligibility for a housing voucher based on the total annual gross income and family size; eligibility is limited to U.S. citizens and specified categories of noncitizens who have eligible immigration status. In general, the family's income may not exceed 80 percent of the median income for the county or metropolitan area in which the family chooses to live. By law, a PHA must provide 75 percent of its vouchers to applicants whose incomes do not exceed 30 percent of the Area Median Income.

Portability

A key feature of the HCVP is the mobility, or portability, of the voucher assistance. The term *portability* refers to the process of leasing a dwelling unit with tenant-based housing voucher assistance outside the jurisdiction of the PHA that initially issued the family its voucher (the initial PHA). Portability allows an eligible family with a housing choice voucher to use that voucher to lease a unit anywhere in the United States where a public housing agency is operating an HCVP. Currently, program regulations detail where a family may move and the responsibilities of the initial PHA and the receiving PHA (the PHA with jurisdiction over the area to which the family desires to move).

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[3] The HCVP is authorized by section 8(o) of the United States Housing Act of 1937 (42 U.S.C. 1473f(o)1437f(o)), and the HCVP regulations are in 24 CFR part 982.

[4] See HUD (2011).

[5] PHA rent policies may specify that the PHA will use a percentage of a family's income or some other reasonable system to determine income-based rents. In general, the total tenant payment is the highest of the following, rounded to the nearest dollar: 30 percent of the family's monthly adjusted income, 10 percent of the family's monthly income, the welfare rent, or the minimum rent.

Administrative Fee

The receiving PHA may either accept an incoming family to its own program (absorption) or bill the initial PHA. Under current regulation, when a voucher is in a portability billing arrangement between the initial PHA and receiving PHA, the initial PHA must pay the receiving PHA 80 percent of its administrative fee for each month the family receives assistance at the receiving PHA.

Basic Facts About the Housing Choice Voucher Program

Exhibit 1 shows the total budget authority for the HCVP was about $17 billion, as of March 2011. The HCVP had about 2.2 million units under lease at that time, and about 50,000 portable vouchers were under lease. The total housing assistance payment—which is the payment to landlords— for portable vouchers under lease was about $463 million.

Exhibit 1

Housing Assistance Payments

	Month of March 2011	Annualized
Total budget	$1,413,075,669	$16,956,908,028
Total number of units under lease	2,162,516	2,162,516
Total HAP for all units under lease	$1,389,606,707	$16,675,280,484
Total number of portable vouchers under lease	49,736	49,736
Total HAP for all portable vouchers under lease	$38,619,715	$463,436,580

HAP = housing assistance payments.

Source: High-performance goal data maintained by the U.S. Department of Housing and Urban Development Office of Public and Indian Housing

Key Provisions of the Proposed Rule

The following provisions of the proposed rule are likely to have programmatic, economic, or financial effects on PHAs or other program participants.

Administrative Fee

Under current regulation, when a voucher is in a portability billing arrangement between the initial PHA and receiving PHA, the initial PHA must pay the receiving PHA 80 percent of its administrative fee for each month the family receives assistance at the receiving PHA.

The proposed rule would set the maximum amount the initial PHA is required to pay at 100 percent of the receiving PHA's administrative fee rate. This change prevents a receiving PHA with a lower administrative fee from profiting from an initial PHA with a higher administrative fee.

Mandatory Absorption of Portability Vouchers

To ensure that a PHA uses its available budget authority to the maximum extent possible, and to reduce the number of portability billing arrangements between agencies, this proposed rule would

require a PHA that (1) is using 95 percent or less of its available budget authority and (2) has a leasing rate of less than 95 percent to absorb incoming portability families until the percentage of available budget authority or the leasing rate is at least 95 percent.

Briefing Families on Housing Choice

Currently, many PHAs supply new tenants with a briefing packet that includes a map of the PHA jurisdiction, area schools, and relevant community organizations, as well as landlords or owners who have expressed interest in participating in the HCVP. This proposed rule would require that the briefing packet include materials identifying housing opportunities in areas where the eligible low-income (ELI) rate is less than 15 percent, both within the jurisdiction of the PHA and in neighboring jurisdictions.

The proposed rule also requires the PHA to identify owners known to the PHA who are interested in participating in the HCVP or available, eligible units known to the PHA that are located in areas outside areas of ELI concentration. To comply with the proposed rule, PHAs in metropolitan Fair Market Rent areas would also be required to include a current list of other community organizations or programs that help families find units outside areas of ELI concentration.

Cost-Benefit Analysis

The implementation of the proposed rule would generate certain benefits to HCVP participants by increasing family choice in locating and securing suitable housing. The proposed rule would also generate some administrative and compliance costs to PHAs. It is estimated, however, that the benefits would largely outweigh the costs. The financial flows that the proposed rule would cause are addressed in the financial transfer section of this article.

Benefits of the Proposed Rule

The HCVP portability policy helps ensure that families have the opportunity to relocate to pursue increased or new employment opportunities or to gain access to higher performing schools for their children. An efficient portability process also helps ensure that victims of domestic violence and stalking have access to the resources necessary to relocate to a safe, stable home away from an abuser.

It is difficult to quantify the effects of mobility on the welfare of a program participant, and the jury is still out on the valuation of existing programs with some mobility component, such as the Moving to Opportunity (MTO) for Fair Housing demonstration[6] and the Gautreaux desegregation program in Chicago. Although, in principle, improved ability to port should result in better employment outcomes from greater access to available jobs, the MTO results do not allow us to quantify such benefits. Improved ability to port will presumably result in better matches between consumer needs and the locational amenities. Again, we do not know how to quantify.

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[6] See http://www.nber.org/mtopublic/ for a comprehensive database on MTO research.

Costs of the Proposed Rule

The rule would not have any effect on the program budget at the national level per se. The portability billing arrangements proposed by this rule, however, may place some additional administrative burden on PHAs. Organizational costs may be associated with agreements and consolidating PHA operations, databases, and documents. For example, the proposed rule would require that the briefing packet include materials identifying housing opportunities outside areas of concentrated poverty both within the jurisdiction of the PHA and in neighboring jurisdictions. PHAs would also be required to identify owners interested in participating in the HCVP or available, eligible units located outside areas of concentrated poverty. To comply with the proposed rule, the PHA would also be required to include a current list of other community organizations or programs that help families find units outside areas of concentrated poverty.

Low-income people make choices based on information they know, including the choices informing their budget constraint. Because the budget constraint of low-income people is tightly binding, they may economize on information gathering as well; that is, they may decide not to gather information on the benefits of options beyond their budget constraint. The selection of a household into the HCVP loosens the household's budget constraint along the housing dimension. The intent of the information disclosure rule discussed in this report is to provide new voucher households with an expanded information set commensurate with their new budget constraint. It in no way cancels out the information on cost and other disadvantages of location in high-income neighborhoods that led the household to select a house in a low-income neighborhood before entering the HCVP. Households with vouchers will make rational decisions based on the combination of their own information and the information that the PHA provides. As for households without vouchers replacing voucher tenants who have moved out of units in low-income neighborhoods, in keeping with Glaeser, Kahn, and Rappaport (2000), it presumes that such moves are at least not welfare-reducing for nonsubsidized tenants operating within their budget constraints.

Financial Transfers

Although the financial effect of the proposed rule is marginal, it does have the potential to create substantial financial transfers among PHAs.

Mandatory Absorptions

In this proposed rule, HUD is proposing mandatory absorptions of portability vouchers when a PHA is using 95 percent or less of its available budget authority and has a leasing rate of less than 95 percent. It is HUD's position that this approach would ensure that PHAs are using their available budget authority to the maximum extent possible while also reducing the number of portability billing arrangements.

Administrative Fee

Under the current regulation, when a voucher is in a portability billing arrangement between the initial PHA and receiving PHA, the initial PHA must pay the receiving PHA 80 percent of its administrative fee for each month the family receives assistance at the receiving PHA. With the removal of potential barriers to mobility, an increase in the number of portability vouchers is

expected and, thus, an increase in the amount of administrative fee transfers between PHAs. Given that PHAs are not required to report on interagency administrative fee transfers, data on such activity are not available.

The proposed rule would set the maximum amount the initial PHA is required to pay at 100 percent of the receiving PHA's administrative fee rate. In other words, the initial PHA would reimburse the receiving PHA for the lesser of (1) 80 percent of the initial PHA's ongoing fee or (2) the full amount of the receiving PHA's administrative fee. This change eliminates the incentive for a receiving PHA with a lower administrative fee to bill an initial PHA with a higher administrative fee to receive a higher administrative fee than what they would normally earn from HUD. This action should reduce portability billings for those PHAs for which 80 percent of the receiving PHA's fee is more than 100 percent of their own administrative fee. For example, assume a receiving PHA's administrative fee is $60. Under current rules, if a family moves to the receiving PHA's jurisdiction from an initial PHA that receives $100 in administrative fees for a housing voucher, the receiving PHA may bill the initial PHA for $80, which is $20 more than the PHA would earn if it simply absorbed the voucher. Under the proposed rule, the receiving PHA will receive $60 regardless of whether the receiving PHA bills the initial PHA or absorbs the family into its own program.

Conclusion

As presented in this analysis, the proposed changes to the portability process provided by the current HCVP regulations would not have a significant incidence on the national program budget (although there would be some amount less billed to HUD without the excess charges) nor have an economically significant effect on the economy, as defined by Executive Order 12866 (Regulatory Planning and Review). Although the proposed rule would not result in an economically significant effect, the proposed changes would yield certain intangible benefits to program participants and, if successful, increase financial transfers between PHAs. The primary purpose of this rule is to streamline the portability process and, in doing so, alleviate some of the administrative complications that families and PHAs both face with the current portability process.

Acknowledgments

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Comparing Public Housing and Housing Voucher Tenants With Bayesian Propensity Scores

Brent D. Mast

Correction

The volume 14, number 1 issue of *Cityscape* contained an error on page 64 in the article titled, "Comparing Public Housing and Housing Voucher Tenants With Bayesian Propensity Scores," by Brent D. Mast. The article stated that, "Of [public housing] tenants, 58.2 percent have rent burdens between 28 and 31 percent, as do 72.7 percent of [Housing Choice Voucher Program] tenants." The sentence should have read, "Of [public housing] tenants, 72.7 percent have rent burdens between 28 and 31 percent, as do 58.2 percent of [Housing Choice Voucher Program] tenants." The author thanks Bill Jacobs for bringing the error to his attention.

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