Moving to Opportunity for Fair Housing Demonstration Program

Final Impacts Evaluation

Visit PD&R's website

www.hud.gov/policy or www.huduser.org

to find this report and others sponsored by HUD's Office of Policy Development and Research (PD&R). Other services of HUD USER, PD&R's Research and Information Service include listservs, special interest and bimonthly publications (best practices, significant studies from other sources), access to public use databases, and a hotline (1-800-245-2691) for help with accessing the information you need.

Moving to Opportunity for Fair Housing Demonstration Program

Final Impacts Evaluation

Prepared by:
Lisa Sanbonmatsu
Jens Ludwig, Project Director
Lawrence F. Katz, Principal Investigator
Lisa A. Gennetian
Greg J. Duncan
Ronald C. Kessler
Emma Adam
Thomas W. McDade
Stacy Tessler Lindau

with:
Matthew Sciandra
Fanghua Yang
Ijun Lai
William Congdon
Joe Amick
Ryan Gillette
Michael A. Zabek
Jordan Marvakov
Sabrina Yusuf
Nicholas A. Potter

National Bureau of Economic Research

November 2011

Acknowledgments

Foreword

Begun in 1994, the Moving to Opportunity for Fair Housing (MTO) Demonstration Program is the most ambitious randomized social experiment ever conducted by HUD. The demonstration was designed to determine the benefits of offering mobility opportunities, with the goal of informing future voucher program policies. Over 4600 low-income families with children living in high-poverty central city public housing participated in this important demonstration.

I am pleased to present the final report of the demonstration, which describes the impacts of the program ten to fifteen years after families enrolled in the program. This study confirms that mobility opportunities and, by extension, neighborhood characteristics matter for family outcomes.

Three important themes emerge from the MTO demonstration:

1. Lower poverty and safer neighborhoods.

    Mobility programs, which provide resources for families to move to a housing unit in a different location, result in families living in lower poverty neighborhoods. Both at the time of the move and at the final follow-up, families in the group that received rental housing vouchers without any restrictions (the "Section 8 group") and in the group that received vouchers and mobility counseling but could only use those vouchers in neighborhoods with poverty rates below ten percent (the "experimental group") lived in lower poverty neighborhoods than those in the group of families that enrolled in the demonstration but did not receive vouchers (the "control group"). The opportunity to move achieved an important goal of the participants: greater safety. Adults and female youths in both the Section 8 group and the experimental group felt safer in their neighborhoods than those in the control group.

2. Better health outcomes.

    The study found that the opportunity to live in lower poverty neighborhoods was associated with better health outcomes. Women in the experimental group were less likely to have extreme obesity and diabetes compared to women in the control group. The women and their female children in the experimental group also experienced less psychological distress and major depression.

3. No better educational, employment, and income outcomes.

    Families in the experimental group did not experience better employment or income outcomes than the other families. The children in the Section 8 and experimental groups did not have better educational achievements than those in the control group and were not significantly less likely to engage in most forms of risky or criminal behavior. This finding leads to two important lessons.

    • Mobility programs designed to give families access to greater opportunity may need to define opportunity more broadly than poverty rates or racial composition if improvements along these other dimensions are valued. Access to entry-level jobs, availability of high performing schools, and other neighborhood characteristics are additional factors that might need to be considered.

- A more comprehensive approach is needed to reverse the negative consequences of living in neighborhoods with heavily concentrated poverty. Housing is a platform for positive outcomes, but it is not sufficient alone for achieving these additional benefits.

HUD's current programs support many of the MTO findings. For example, redevelopment programs like Choice Neighborhoods take a broader, more holistic approach to reforming a distressed, high poverty, high crime neighborhood into a safer one with lower poverty concentrations by linking housing improvements with appropriate services, schools, public assets, transportation, and access to jobs.

The MTO demonstration has contributed significantly to our knowledge of how neighborhoods affect families. It continues to provide an exceptionally rich dataset for understanding the role environment plays in individual outcomes. Moreover, in addition to the work done under HUD's guidance, other complementary quantitative and qualitative studies have been undertaken, funded by a large group of federal agencies and philanthropic organizations. Thus, the MTO demonstration has served as a foundation for the pursuit of a much larger research agenda. Despite the considerable work already done with these data, much remains to be explored as HUD continues to support using data to drive policy.

Raphael W. Bostic, Ph.D.
Assistant Secretary for Policy
Development and Research

TABLE OF CONTENTS

List of Exhibits
EXECUTIVE SUMMARY

CHAPTER 1

CHAPTER 2

CHAPTER 3

CHAPTER 4

CHAPTER 5

CHAPTER 6

CHAPTER 7

EXECUTIVE SUMMARY

THE MOVING TO OPPORTUNITY FOR FAIR HOUSING DEMONSTRATION PROGRAM FINAL IMPACTS EVALUATION

This report presents the long-term impacts of a unique housing mobility demonstration, Moving to Opportunity (MTO), on housing and neighborhood conditions, physical and mental health, economic self-sufficiency, risky and criminal behavior, and educational outcomes. The MTO demonstration was authorized by the U.S. Congress in section 152 of the Housing and Community Development Act of 1992. In 1994, the U.S. Department of Housing and Urban Development (HUD) launched MTO to test whether offering housing vouchers to families living in public housing projects in high-poverty neighborhoods of large inner cities could improve their lives and the lives of their children by allowing them to move to lower-poverty neighborhoods. The original authorizing legislation for MTO charged HUD with describing "the long-term housing, employment, and educational achievements of the families assisted under the demonstration program." This report discharges that responsibility.

Thanks to the generous support of other federal government agencies and private foundations, the scope of our long-term study of MTO families was expanded to include a number of outcome domains beyond those under HUD's charge, most notably physical and mental health. The additional funders that enabled this important expansion to the study's scope included the National Science Foundation, the National Institute for Child Health and Human Development, the Centers for Disease Control, the National Institute of Mental Health, the National Institute for Aging, the National Opinion Research Center's Population Research Center (supported by the National Institute of Child Health and Human Development), University of Chicago's Center for Health Administration Studies, the U.S. Department of Education's Institute of Education Sciences, the John D. and Catherine T. MacArthur Foundation, the Smith Richardson Foundation, the Spencer Foundation, the Bill & Melinda Gates Foundation, the Russell Sage Foundation, and the Annie E. Casey Foundation.

The results show that MTO moves led to sustained improvements in housing quality and in many aspects of the neighborhood's environment, including neighborhood socioeconomic composition and safety. These MTO-induced changes translate into a number of important improvements in mental and physical health for adults, including lower rates of extreme obesity, diabetes, psychological distress, and major depression. MTO had no detectable impacts on work, earnings, or other economic outcomes for adults. For youth, we see some signs of the same gender difference in responses to MTO as were found in the interim study, which reported on outcomes measured four to seven years after random assignment. One outcome for which we see some hints of beneficial MTO impacts on male youth is a reduction in illegal drug selling.

THE CONTRIBUTIONS OF THE MTO DEMONSTRATION

There is a long-standing belief that living in a less economically and socially distressed neighborhood can improve people's well-being and long-term life chances, owing to some combination of better housing conditions, lower crime, less stress, greater job opportunities, more supportive schools, and role models who are more affirming of pro-social behaviors. However, empirically isolating the effect of neighborhoods on people's outcomes from other possible influences has been difficult.

MTO overcomes some of the empirical challenges of identifying neighborhood effects on people's life outcomes because it was implemented as an experiment. Akin to drug trials in medicine, families at the beginning of the experiment were randomly assigned to a control group or one of two treatment groups. An MTO-type experiment enables

us to determine whether moving to a lower-poverty neighborhood itself, rather than some other characteristic of the individuals or families that might be related to both their propensity to move and their behavioral outcomes, directly *caused* improvements in health, economic security, or some other outcome of interest. Because of random assignment, the control group's experience shows, on average, what would have happened to the families in the treatment groups had they not been offered a voucher through MTO.

From 1994 to 1998, the MTO demonstration enrolled 4,604 low-income households in Baltimore, Boston, Chicago, Los Angeles, and New York. Eligibility for MTO was limited to households with children in public or other government-subsidized, project-based housing in selected high-poverty areas. Enrolled families were assigned at random to one of three groups:

1. The ***experimental group*** received Section 8 rental assistance certificates or vouchers that they could use only in census tracts with 1990 poverty rates below 10 percent. The families received mobility counseling and help in leasing a new unit. Forty-eight percent of families assigned to this group moved to a lower-poverty neighborhood with an MTO voucher. One year after relocating, families could use their voucher to move again if they wished, without any special constraints on location.

2. The ***Section 8 only group*** received regular Section 8 certificates or vouchers that they could use anywhere; these families received no special mobility counseling. Sixty-three percent moved using a voucher obtained through MTO.

3. The ***control group*** received no certificates or vouchers through MTO, but continued to be eligible for project-based housing assistance and whatever other social programs and services to which they would otherwise be entitled.

The MTO program population was very economically disadvantaged at baseline. Almost all the households that signed up for MTO were headed by women. Nearly two-thirds were African-American, and most of the remaining one-third were Hispanic. Three-quarters of household heads were on welfare, and fewer than 40 percent had graduated from high school. Most families had two or three children. In more than 40 percent of enrolled households, a household member had been victimized by a crime during the previous six months.

Not all families offered a housing voucher through MTO actually used it to move to a different neighborhood. Those who did move with an MTO housing voucher (that is, those who "complied" with their MTO treatment assignment) were on average relatively younger, more dissatisfied with their original neighborhoods, and had fewer children than those who did not use the voucher.

Accordingly, a useful starting point for our analysis is to compare the average outcomes of all families assigned to the experimental group, regardless of whether they relocated with an MTO voucher, with the average outcomes of those assigned to the control group. The same comparison was calculated for the Section 8 and control groups. These differences are called "intention to treat" (ITT) effects. They capture the effect of offering a family the chance to move with an MTO voucher as part of the experimental or Section 8 group. Our design also enables us to estimate treatment/control group differences for the subset of families who actually moved with an MTO voucher. These are known as the effects of "treatment on the treated" (or TOT).

MTO is thus the first random-assignment social science experiment designed to identify the causal effects of moving from high-poverty to lower-poverty neighborhoods on the social, economic and educational prospects, risky and criminal behavior, health, and well-being of low-income families. Other unique characteristics of the demonstration project include:

- A large study sample.
- Multiple program sites drawn from different regions across the United States.
- Long-term survey and administrative data follow-up of program participants.
- Exhaustive tracking of program participants over time and several sophisticated survey follow-ups of participants that produced high response rates.

- Direct measurement of key physical health outcomes such as height, weight, blood pressure, and biomarkers based on blood samples.
- Multiple administrative data sources for independent measurements that complement information from the surveys.
- Examination of a large set of outcome domains.

INTERIM FINDINGS: FOUR TO SEVEN YEARS AFTER BASELINE

An interim multisite evaluation of MTO's effects was conducted four to seven years after families entered the program. At that point, families who had moved to lower-poverty neighborhoods were doing significantly better than those in the control group in some important respects. On many other dimensions, however, the different groups had virtually identical outcomes, on average. Since previous theoretical work and observational studies had led to expectations of large neighborhood effects, this overall pattern of interim results was disappointing for many in the research and policy communities.

Adults assigned to either the experimental or Section 8 groups were safer and more satisfied with their housing and neighborhoods compared with adults in the control group, and were also less likely to report a household member being a victim of a crime or seeing illicit drugs sold. Compared with the control group, adults in the experimental group had better mental health and some better physical health outcomes and behaviors, such as lower rates of obesity together with some signs of improved diet and exercise. However, the MTO "treatment" led neither to better labor market outcomes nor reductions in social program participation.

MTO's impacts on children in participating families differed by gender, with generally beneficial effects for female youth and, on balance, some deleterious effects for male youth. Female youth in the experimental group had better mental health outcomes than their control group counterparts, whereas male youth in the experimental group were more likely to engage in risky behavior than control group males. Moving also reduced violent behavior for both male and female youth, as

measured by arrests for violent crime. MTO had few detectable effects on child physical health or risky behaviors aside from an increase in nonsports injuries for male youth. MTO also had no detectable impacts on educational achievement as measured by standardized test scores.[1]

LONG-TERM FINDINGS: 10 TO 15 YEARS AFTER BASELINE

The final evaluation, which is the topic of the present report, is an opportunity to answer questions about the longer-term effects of housing mobility on poor families, measured 10 to 15 years after families enrolled in the program. The study's populations of interest were all members of the 4,604 households at the beginning of the program. Subsets include:

- 4,604 adults who were heads of those households at the beginning of the program
- 6,308 youth who were ages 10–20 as of December 31, 2007[2]
- 4,643 grown children (ages 20–30 at the end of 2007).

Executive Summary (ES) Exhibit 1 shows how these groups of adults and youth are distributed across demonstration sites and groups. It also shows the fraction of adults and youth assigned to the experimental and Section 8 groups who moved with an MTO housing voucher, that is, the "compliance rate."

To learn more about the outcomes of this study, we draw on survey interviews with 3,273 of the adult household heads as well as 5,105 youth who were age 10–20 at the end of 2007. The effective response rate for the long-term follow-up survey was 90 percent for the adult household heads and 89 percent for youth selected for the survey. These response rates were generally similar across randomly assigned MTO mobility groups, with the exception of a slightly lower response rate for adults in the Section 8 group (around 87 percent). We also

---

1 These are the overall results covering all five MTO sites. There were some detected improvements in individual sites. Youth saw some gains in test scores in Baltimore and Chicago, for example.

2 This group only partially overlaps with the set of youth who were ages 8–20 at the end of 2001 and interviewed as part of the interim survey.

relied on a variety of government administrative data sources. This report examines the impact of moving to lower-poverty neighborhoods on residential mobility, housing conditions, neighborhood conditions, and social networks of participating families. It also reports on physical health, mental health, economic self-sufficiency, risky and criminal behavior, and educational outcomes. These findings are outlined by topic below.

MOBILITY, HOUSING, NEIGHBORHOOD CONDITIONS, AND SOCIAL NETWORKS

MTO had sizable positive impacts on housing and neighborhood conditions and the social networks with which participating families interacted. As Exhibit ES-2 and 3 show, the experimental and Section 8 groups were more likely than the control group to:

- Live in lower-poverty neighborhoods
- Live in higher-quality homes
- Reside in slightly less racially segregated neighborhoods (although most families even in the experimental and Section 8 groups were still in majority-minority neighborhoods)
- Have more social ties with relatively more affluent people
- Feel safer in their neighborhoods.

MTO enabled many families to move to lower-poverty neighborhoods. In the census tracts in which MTO participants lived at baseline, some 53 percent of tract residents had family incomes below the poverty line. Experimental and Section 8 families who moved with an MTO voucher (the "compliers") lived in neighborhoods with mean tract poverty rates of 11 and 29 percent, respectively, immediately after their initial program moves (see Executive Summary Exhibit 2). Over time, though, the differences between the treatment and control groups narrowed, in large part because some control group families moved on their own to better neighborhoods and treatment group families moved on to neighborhoods with somewhat higher poverty rates. When averaged over the entire 10 to 15-year study period, the control group's neighborhood poverty rates were roughly 40 percent (see Executive Summary Exhibit 2). The average neighborhood poverty rates for all families assigned to the experimental and Section

8 groups were 9 and 7 percentage points lower than this, respectively. (This intention to treat effect is labeled "ITT" in the exhibits). For those families in the experimental and Section 8 groups that moved through MTO, average neighborhood poverty rates were about 18 and 11 percentage points lower, respectively.[3] (This "treatment on the treated" effect is labeled "TOT" in the exhibits.) Median household income was almost $19,000 higher in the census tracts where experimental group movers lived. MTO moves also made participants feel safer in their new neighborhoods, although this was less clearly evident for male youth in the program.

MTO moves only modestly reduced neighborhood racial segregation. For control group members, 88 percent of their neighbors were members of racial and ethnic minority groups. Families in the experimental group saw a decline of nearly 6 percentage points in the share minority, while the decline was smaller for those in the Section 8 group. Families in both treatment groups, even those who moved with an MTO voucher, were still living in census tracts that were three-quarters minority.

MTO also changed the social environments of the neighborhoods in which families were living. MTO helped families move into neighborhoods where neighbors were more willing to work together to support shared norms, a measure of informal social control that previous research suggests may be particularly important in improving the lives of neighborhood residents. MTO increased social connections of adults to other people who were employed full-time or had completed college. MTO moves did not appear to increase social isolation.

A majority of families in all three groups were still receiving some form of housing assistance, with slightly higher rates of assistance among the Section 8 group. However, fewer in the experimental and Section 8 groups than in the control group were living in public housing, and relatively more were instead using vouchers. Given that most families were still receiving some form of housing assistance, it is perhaps not surprising that MTO had only modest effects on out-of-pocket expenditures

---

3 This is the duration-weighted poverty rate; that is, the poverty rate for each of a family's addresses weighted by the amount of time the family lived in a particular census tract.

on housing. Compared with the control group adults, those in the experimental group were less likely to report being late with their rent or mortgage payments, but were more likely to have had trouble paying their utility bills on time.

Finally, MTO improved the quality of housing in which families were living. Adults in the experimental group were about 5 percentage points more likely than control group adults to report that their current housing conditions were excellent or good.

ADULT AND YOUTH PHYSICAL HEALTH

Living in disadvantaged neighborhoods has been hypothesized to worsen physical health. Previous research has found strong correlations between living in poor neighborhoods and prevalence rates of mortality, heart disease, obesity, depression, and substance abuse, even after controlling for observable individual-level health risk factors. These previous findings suggest that moving to a less disadvantaged neighborhood could conceivably improve health, and indeed there is some indication that this is the case for MTO participants on several important health outcomes.

As Exhibit ES-4 shows, a little more than one-half of the adults across all groups, including the control group, rated their health as good or excellent. Evidence from self-reports, physical biomarkers, and blood samples indicate that at the time of the long-term follow-up, compared with the control group, adults in the experimental and Section 8 group have:

- a lower prevalence of extreme obesity
- a lower prevalence of diabetes
- fewer self-reported physical limitations
- similar self-reported health status, as well as similar rates of hypertension and health-related risk behaviors.

Although there were no significant differences across groups in obesity (as measured by body mass index (BMI) of greater than or equal 30, where BMI is defined as weight in kilograms divided by height in meters squared (kg/m^2)), the share of adults with BMI of 35 kg/m^2 or greater was approximately 5 percentage points

lower among those in the experimental group. The share of experimental group adults whose BMI was 40 kg/m^2 or greater, a commonly used definition of extreme obesity, was about 3 percentage points lower than those in the control group. There were no significant differences between the Section 8 and control groups in the prevalence of extreme obesity. Diabetes was 3 to 6 percentage points less prevalent for those in the two MTO treatment groups than in the control group as measured from blood samples. The effect is somewhat smaller for the experimental group if we use respondent self-reports about whether their doctors had told them they had diabetes. An advantage of the blood-based biomarkers over self-reports of diabetes is that they are free from the underreporting caused by respondents being unaware they have the condition.

Adult health outcomes, such as self-rated overall health, asthma, hypertension, chronic pain, and substance use, were largely unaffected by moving to lower-poverty neighborhoods. Among youth, moving to a lower-poverty neighborhood had little to no measured effect on the health outcomes measured (see Exhibit ES-5).

ADULT AND YOUTH MENTAL HEALTH

As with physical health, living in disadvantaged neighborhoods has long been thought to increase the risk for mental health problems. Research has documented large correlations between rates of neighborhood poverty or crime and depression, anxiety, and other mental health problems. Exposure to violence, for example, is associated with long-term behavioral and psychological harm for both youth and adults. Males, who are more often exposed to violence than females, may be particularly vulnerable.

Data from a battery of psychological measures show that, compared with the control group, adults in the experimental or Section 8 group have:

- lower levels of psychological distress
- lower prevalence of depression
- lower prevalence of anxiety
- similar rates of most other mental health problems.

As Exhibit ES-6 shows, MTO lowered the prevalence of depression among adults in both the experimental and Section 8 group by 3 and 5 percentage points, respectively (although the experimental group impact is just marginally significant). MTO did not have statistically significant effects on other mental health measures, such as levels of mental calm, normal sleep patterns, having any mood disorder, panic attacks, post-traumatic stress, or intermittent explosive disorder. However, MTO's impacts on almost all of these measures were in the direction of improved mental health for movers relative to controls. One exception is that moves to lower-poverty neighborhoods seemed to slightly increase drug or alcohol abuse or dependence among adults in the experimental group compared with the control group.

Exhibit ES-7 shows selected mental health outcomes for male and female youth. For female youth ages 10–20, moving to lower-poverty neighborhoods had an overall positive effect on their mental health. More specifically,

as shown in the table and in Chapter 4, relative to female youth in the control group, females in the experimental group have:

- a lower prevalence of any lifetime mood disorder
- fewer serious emotional or behavioral difficulties
- fewer panic attacks in the past year
- less psychological distress
- lower prevalence of oppositional defiant disorder in the past year
- similar rates of other mental health problems.

For male youth ages 10–20, however, moving to lower-poverty neighborhoods may have increased lifetime post-traumatic stress disorder; prevalence of this disorder among male youth in the Section 8 group were about 3 percentage points higher than in the control group. Although not reaching statistical significance, many of the other mental health indicators had worsened for male youth after moving.

EXHIBIT ES–1. ALLOCATION AND COMPLIANCE OF THE LONG-TERM EVALUATION SAMPLES BY SITE AND TREATMENT GROUP						
	ALL GROUPS	CONTROL GROUP	EXPERIMENTAL GROUP		SECTION 8 GROUP	
	N	N	N	COMPLIANCE RATE (%)	N	COMPLIANCE RATE (%)
ADULTS						
Baltimore	572	197	252	53.5	123	79.8
Boston	868	326	366	43.6	176	51.1
Chicago	825	232	460	33.4	133	67.4
Los Angeles	929	389	340	60.5	200	71.6
New York City	948	295	401	46.4	252	45.2
All sites	4,142	1,439	1,819	47.4	884	61.6
YOUTH, AGES 10–20						
Baltimore	762	240	268	59.1	254	79.2
Boston	1,267	440	475	38.2	352	54.6
Chicago	1,363	328	701	31.7	334	72.0
Los Angeles	1,539	592	502	62.2	445	78.2
New York City	1,377	418	471	49.6	488	49.2
All sites	6,308	2,018	2,417	47.6	1,873	66.4

Notes: Percentages are weighted to reflect the randomization ratios and sampling of Section 8 adults and up to three youth per family. "Compliance" is defined as leasing a unit using a housing voucher provided by the Moving To Opportunity (MTO) program.
Data source and sample: MTO data system. The samples are N = 4,142 adults and N = 6,308 youth ages 10 to 20 as of December 2007 selected for the long-term survey. Excluded from the samples are the N = 462 Section 8 group adults and youth from households with greater than three youth ages 10–20 who were not randomly selected for the long-term survey.

EXHIBIT ES–2. NEIGHBORHOOD POVERTY RATES OF ADULTS AT DIFFERENT POINTS IN TIME						
	CONTROL GROUP	EXPERIMENTAL GROUP			SECTION 8 GROUP		
	ALL	ALL	COMPLIERS	NON-COMPLIERS	ALL	COMPLIERS	NON-COMPLIERS
SHARE POOR IN CENSUS TRACT OF RESIDENCE FOR MTO FAMILIES BY TIME SINCE RANDOM ASSIGNMENT							
Baseline address	0.531	0.527	0.530	0.524	0.526	0.540	0.505
Initial MTO-assisted move address			0.107			0.287	
Address at time of MTO interim evaluation (4–7 years after baseline)	0.395	0.299	0.193	0.402	0.326	0.286	0.391
Address at time of MTO long-term evaluation (10–15 years after baseline)	0.313	0.274	0.210	0.334	0.283	0.244	0.345
Average (duration-weighted) of all addresses since Random Assignment	0.396	0.306	0.200	0.407	0.329	0.285	0.400

Notes: Characteristics for the census tracts of addresses at which Moving To Opportunity (MTO) families were living at different points in time were linearly interpolated from the 1990 and 2000 decennial Censuses as well as the 2005–09 American Community Survey. Addresses at the time of the interim and long-term evaluations are as of the beginning of the fielding period for each study (December 31, 2001, for the interim evaluation and May 31, 2008, for the long-term evaluation).
Sample: Adults interviewed as part of the long-term evaluation (N = 3,273).

EXHIBIT ES–3. KEY HOUSING, NEIGHBORHOOD, AND SOCIAL NETWORK OUTCOMES, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ADULT						
HOUSING						
Rates current housing as excellent or good [SR]	0.570	0.053* (0.021)	0.109* (0.044)	0.031 (0.029)	0.050 (0.046)	3,267
Currently receiving any housing assistance [SR, HA]	0.620	0.026 (0.021)	0.054 (0.043)	0.045~ (0.027)	0.072~ (0.044)	3,273
Total housing cost (2009 dollars), monthly [SR]	$678.73	19.50 (23.30)	39.67 (47.39)	– 6.26 (30.73)	– 10.00 (49.10)	3,180
NEIGHBORHOOD						
Median household income (2009 dollars) [CEN]	$27,808.85	9,148.91* (544.97)	18,848.48* (1,122.75)	5,600.18* (660.90)	9,027.10* (1,065.32)	3,270
Average (duration-weighted) census tract share persons who are poor [CEN]	0.396	– 0.089* (0.006)	– 0.184* (0.012)	– 0.069* (0.007)	– 0.111* (0.011)	3,270
Average (duration-weighted) census tract share minority [CEN]	0.880	– 0.061* (0.007)	– 0.125* (0.014)	– 0.018* (0.009)	– 0.029* (0.015)	3,270
Feels safe during the day [SR]	0.804	0.036* (0.016)	0.074* (0.034)	0.045* (0.021)	0.072* (0.034)	3,262
Feels safe at night [SR]	0.596	0.043* (0.021)	0.088* (0.043)	0.073* (0.027)	0.117* (0.043)	3,246
SOCIAL NETWORK						
Has at least one close friend who graduated from college [SR]	0.532	0.071* (0.021)	0.145* (0.044)	0.007 (0.029)	0.010 (0.046)	3,203
Has three or more close friends [SR]	0.432	0.006 (0.021)	0.013 (0.044)	– 0.026 (0.028)	– 0.042 (0.045)	3,265

EXHIBIT ES–3. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
YOUTH AGES 10–20						
NEIGHBORHOOD						
FEELS SAFE DURING THE DAY [SR]						
All	0.801	0.018	0.037	– 0.012	– 0.018	4,863
		(0.016)	(0.033)	(0.018)	(0.026)	
Female	0.784	0.045*	0.090*	0.019	0.030	2,478
		(0.022)	(0.045)	(0.025)	(0.039)	
Male	0.817	– 0.009	– 0.019	– 0.043~	– 0.062~	2,385
		(0.022)	(0.047)	(0.025)	(0.035)	
FEELS SAFE AT NIGHT [SR]						
All	0.540	0.035~	0.074~	0.019	0.028	4,862
		(0.021)	(0.043)	(0.022)	(0.033)	
Female	0.486	0.052~	0.104~	0.067*	0.105*	2,478
		(0.028)	(0.056)	(0.030)	(0.048)	
Male	0.591	0.020	0.043	– 0.030	– 0.043	2,384
		(0.028)	(0.060)	(0.031)	(0.043)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, CEN = 1990 and 2000 decennial census data as well as the 2005–09 ACS.

Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Adult long-term survey. All adults interviewed; Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

Measures: Baseline census tract is based on participant's address at baseline. Interpolated census tract characteristics are linearly interpolated from the 1990 and 2000 decennial census. American Community Survey (ACS) refers to the 2005–09 5-year average ACS data.

EXHIBIT ES–4. KEY ADULT PHYSICAL HEALTH OUTCOMES, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
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						
Currently obese: Body Mass Index (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						
Had diabetes or treated for it during the past year [SR]	0.160	– 0.024 (0.015)	– 0.049 (0.030)	– 0.061* (0.018)	– 0.098* (0.029)	3,251
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
HbA1c test detected diabetes or had or treated for diabetes during the past year [DBS, SR]	0.234	– 0.034~ (0.019)	– 0.071~ (0.040)	– 0.008 (0.025)	– 0.013 (0.039)	2,732
LIMITATIONS AND CHRONIC PAIN						
Health limits respondent's ability to climb several flights of stairs/lifting or 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 mm Hg or diastolic ≥ 90 mm Hg) [M]	0.315	0.007 (0.020)	0.015 (0.042)	– 0.026 (0.027)	– 0.041 (0.043)	3,102

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement, DBS = dried blood spot assays.
Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: For obesity inputs (height and weight), only a very small percent of the sample self-reported their height or weight. Body Mass Index (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. HbA1c = glycosylated hemoglobin.

EXHIBIT ES–5. KEY PHYSICAL HEALTH OUTCOMES FOR YOUTH AGES 10–20, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
GENERAL HEALTH						
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						
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)	
OBESITY						
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)	
ACCIDENTS AND INJURIES						
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)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement.
Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: Obesity is defined according to the International Obesity Task Force.

EXHIBIT ES–6. KEY ADULT MENTAL HEALTH OUTCOMES, LONG-TERM EVALUATION

OUTCOME	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		N
		ITT	TOT	ITT	TOT	
Psychological Distress Index (K6) Z-score, past month (higher score indicates greater distress) [SR]	0.000	– 0.107* (0.042)	– 0.221* (0.087)	– 0.097~ (0.056)	– 0.156~ (0.091)	3,273
Major Depression with Hierarchy, Lifetime [SR]	0.203	– 0.032~ (0.017)	– 0.066~ (0.035)	– 0.048* (0.021)	– 0.077* (0.034)	3,269
Any Mood Disorder, Lifetime [SR]	0.255	– 0.028 (0.018)	– 0.058 (0.038)	– 0.036 (0.024)	– 0.058 (0.039)	3,270
Generalized Anxiety Disorder with Hierarchy, Lifetime [SR]	0.065	– 0.003 (0.010)	– 0.005 (0.021)	– 0.020~ (0.011)	– 0.033~ (0.017)	3,273
Any Anxiety Disorder, Lifetime [SR]	0.308	– 0.020 (0.020)	– 0.042 (0.041)	– 0.005 (0.026)	– 0.008 (0.042)	3,269
Panic Attacks, Lifetime [SR]	0.407	0.004 (0.021)	0.009 (0.044)	– 0.022 (0.028)	– 0.035 (0.045)	3,269
Post-Traumatic Stress Disorder, Lifetime [SR]	0.219	– 0.012 (0.018)	– 0.024 (0.037)	0.004 (0.024)	0.006 (0.038)	3,269
Calm and peaceful most of the time, past month [SR]	0.487	0.018 (0.022)	0.037 (0.045)	– 0.025 (0.029)	– 0.040 (0.047)	3,272
Dependence on drugs or alcohol, past month [SR]	0.055	0.029* (0.011)	0.060* (0.022)	0.015 (0.015)	0.024 (0.023)	3,269

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.

Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

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

Measures: Psychological distress consists of 6 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: Depression with hierarchy takes into account Mania, and Hypomania; Generalized Anxiety Disorder (GAD) takes into account Depression and Mania; Intermittent Explosive Disorder (IED) takes into account Depression, Mania and Hypomania. Any Anxiety Disorder includes Panic Disorder, Generalized Anxiety Disorder (GAD) (without hierarchy), and Post-Traumatic Stress Disorder (PTSD). Any Mood Disorder includes Depression (without hierarchy), Bipolar I/II/Subthreshold, and Mania/Hypomania/Hypomania Subthreshold. Any Disorder includes all disorders from Any Anxiety Disorder and Any Mood Disorder as well as Intermittent Explosive Disorder (IED) (without hierarchy). Index of mental health problems is the average of depression (past year), anxiety (past year), mental calm, distress (K6) and normal sleep (7 to 8 hours last night) after standardizing by the control mean and standard deviation. Substance dependence consists of 5 items about drug and/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. See Section 4.3 for additional details.

EXHIBIT ES–7. KEY MENTAL HEALTH OUTCOMES FOR YOUTH AGES 13–20, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
PSYCHOLOGICAL DISTRESS INDEX (K6) Z-SCORE, PAST MONTH (HIGHER SCORE INDICATES GREATER DISTRESS) [SR]						
All	0.000	– 0.040	– 0.084	0.036	0.054	4,644
		(0.041)	(0.085)	(0.047)	(0.070)	
Female	0.115	– 0.119*	– 0.241*	– 0.013	– 0.021	2,371
		(0.058)	(0.116)	(0.066)	(0.104)	
Male	– 0.110	0.040	0.085	0.084	0.120	2,273
		(0.054)	(0.116)	(0.060)	(0.086)	
MAJOR DEPRESSION WITH HIERARCHY, LIFETIME [SR]						
All	0.105	– 0.015	– 0.031	– 0.014	– 0.021	4,639
		(0.012)	(0.025)	(0.013)	(0.019)	
Female	0.128	– 0.032~	– 0.065~	– 0.029	– 0.045	2,367
		(0.017)	(0.035)	(0.019)	(0.029)	
Male	0.084	0.003	0.006	0.000	0.000	2,272
		(0.016)	(0.034)	(0.017)	(0.025)	
ANY MOOD DISORDER, LIFETIME [SR]						
All	0.178	– 0.013	– 0.027	0.003	0.004	4,644
		(0.015)	(0.031)	(0.016)	(0.024)	
Female	0.218	– 0.048*	– 0.096*	– 0.032	– 0.050	2,371
		(0.022)	(0.044)	(0.024)	(0.038)	
Male	0.140	0.022	0.047	0.038~	0.054~	2,273
		(0.020)	(0.043)	(0.022)	(0.032)	
GENERALIZED ANXIETY DISORDER WITH HIERARCHY, LIFETIME [SR]						
All	0.019	– 0.002	– 0.004	0.010~	0.015~	4,644
		(0.005)	(0.010)	(0.006)	(0.009)	
Female	0.022	– 0.002	– 0.003	0.010	0.016	2,371
		(0.007)	(0.014)	(0.008)	(0.013)	
Male	0.016	– 0.002	– 0.005	0.010	0.015	2,273
		(0.006)	(0.013)	(0.009)	(0.013)	
ANY ANXIETY DISORDER, LIFETIME [SR]						
All	0.121	0.001	0.002	0.005	0.007	4,639
		(0.013)	(0.027)	(0.014)	(0.020)	
Female	0.149	– 0.011	– 0.023	– 0.014	– 0.021	2,367
		(0.019)	(0.038)	(0.020)	(0.031)	
Male	0.095	0.013	0.028	0.023	0.033	2,272
		(0.017)	(0.037)	(0.019)	(0.027)	

EXHIBIT ES–7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
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)	
PANIC ATTACKS, LIFETIME [SR]						
All	0.387	– 0.017	– 0.035	0.014	0.021	4,639
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.423	– 0.039	– 0.079	– 0.005	– 0.008	2,367
		(0.027)	(0.054)	(0.029)	(0.045)	
Male	0.354	0.006	0.012	0.033	0.047	2,272
		(0.027)	(0.057)	(0.030)	(0.043)	
POST-TRAUMATIC STRESS DISORDER, LIFETIME [SR]						
All	0.066	0.006	0.012	0.005	0.007	4,639
		(0.010)	(0.020)	(0.011)	(0.016)	
Female	0.092	0.002	0.003	– 0.019	– 0.030	2,367
		(0.016)	(0.032)	(0.016)	(0.026)	
Male	0.041	0.010	0.021	0.030*	0.043*	2,272
		(0.012)	(0.026)	(0.015)	(0.021)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.

Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.

Measures: Psychological distress consists of 6 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 for of control group youth. Serious mental illness is defined as a raw score of 13 or higher on the K6. Strengths and Difficulties consists of 5 behavioral and emotional items (obedience, worry/anxiety, unhappiness, getting along better with adults than peers, attention span) scaled on score from 0 (no behavioral/emotional problems) to 12 (severe behavioral or emotional problems). A score of 6 or higher indicates serious behavioral/emotional problems. See Section 4.3 for additional details.

ECONOMIC SELF-SUFFICIENCY

A primary motivation of MTO was to determine whether moving to lower-poverty neighborhoods could boost earnings and labor force participation and ease reliance on welfare among vulnerable families. The study began shortly before the implementation of welfare reform, and only one-fourth of adults reported being employed. Three-fourths were receiving Aid to Families with Dependent Children (AFDC), and eight in ten were receiving food stamps. The average annual household income at baseline was $12,827 (in 2009 dollars), well below the official poverty line for a family of three.

Ten to fifteen years later, many more families were employed, incomes were higher, and welfare receipt had plummeted, but these changes were roughly equal for treatment and control-group families. Overall, MTO generated few systematic, detectable long-term effects on economic self-sufficiency for adults, youth, and grown children.

As Exhibit ES-8 shows, compared with control group members, experimental and Section 8 group adults have:

- similar employment levels and earnings
- similar incomes
- less food insufficiency
- somewhat higher use of food stamps (experimental group only).

Earnings and employment trends fluctuated with macroeconomic and policy shifts, with rapidly rising employment rates during economic booms and a recent fall-off during the severe recession. One distinction is a short-term decline in employment during the first two years after moving for those in the experimental and Section 8 groups compared to the control group, perhaps indicating difficulties finding work in a new location or in securing child care. Their employment rates recovered shortly thereafter and matched the ebbs and flows evident in the control group. Ten years after moving, employment outcomes were not statistically different across groups. The types of jobs the participants held also differed little across groups.

Those in the experimental group were slightly less likely to have incomes below the poverty line than control group members, but the differences were not statistically significant. In contrast, the Section 8 group saw a modest, but again not statistically significant, rise in poverty relative to the control group. The study finds higher rates of food stamp use among the experimental group—and less food insufficiency—but few differences in use of Temporary Assistance for Needy Families (TANF, the program that replaced AFDC after welfare reform). TANF receipt was on the decline nationally throughout this time period owing to welfare reform in 1996 and a robust economy in the latter half of the 1990s. There are no significant differences in welfare receipt among the Section 8 and control groups.

For adults, it appears that training, education, and employment services that directly enhance marketable skills and changes in work incentives more directly affect labor market and economic outcomes of low-income adults than do the indirect effects of changes in neighborhood environments, at least in the range observed in the MTO demonstration.

Likewise, unlike early childhood education programs or early school quality improvements, which have been shown to improve economic self-sufficiency later in life, moving to lower-poverty neighborhoods had little systematic detectable effects on the economic outcomes—including being idle, defined as neither working nor in school—for youth and grown children compared with those in the control group (see Exhibit ES-9).

EXHIBIT ES–8. KEY ADULT ECONOMIC SELF-SUFFICIENCY OUTCOMES, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
EMPLOYMENT						
Currently employed [SR]	0.525	– 0.007	– 0.014	– 0.077*	– 0.124*	3,264
		(0.021)	(0.042)	(0.028)	(0.045)	
Calendar year 2007 [UI]	0.465	– 0.004	– 0.009	0.000	0.000	4,194
		(0.017)	(0.036)	(0.019)	(0.030)	
EARNINGS AND INCOME						
Annual individual earnings (previous calendar year, 2009 dollars) [SR]	$12,288.51	326.94	677.92	– 613.60	– 982.43	3,141
		(583.44)	(1,209.79)	(807.20)	(1,292.40)	
Calendar year 2007 (2009 dollars) [UI]	$11,325.14	– 347.83	– 731.73	112.93	180.50	4,194
		(523.80)	(1,101.92)	(580.69)	(928.11)	
Total Household Income (2009 dollars) [SR]	$20,025.90	607.58	1,255.56	– 41.67	– 67.33	3,258
		(727.58)	(1,503.54)	(1,009.18)	(1,630.66)	
Household income is at or below 100% of poverty line [SR]	0.590	– 0.032	– 0.067	0.036	0.059	3,258
		(0.021)	(0.043)	(0.028)	(0.045)	
Adult reported that their household sometimes/often did not have enough to eat in the past 12 months [SR]	0.336	– 0.035~	– 0.072~	– 0.067*	– 0.106*	3,266
		(0.020)	(0.042)	(0.026)	(0.042)	
FOOD STAMPS						
Currently receiving food stamps [SR]	0.470	0.016	0.032	0.029	0.046	3,253
		(0.021)	(0.043)	(0.028)	(0.045)	
Total food stamps benefits received, July 2007–June 2009 (2009 dollars) [FS]	$3,074.08	309.94*	664.54*	171.07	261.80	2,708
		(156.50)	(335.54)	(184.98)	(283.09)	
TEMPORARY ASSISTANCE FOR NEEDY FAMILIES						
Currently receiving TANF [SR]	0.158	0.010	0.021	0.026	0.041	3,262
		(0.015)	(0.031)	(0.021)	(0.033)	
Total TANF benefits received, July 2007–June 2009 (2009 dollars) [TANF]	$1,402.33	56.10	120.29	– 94.47	– 144.57	2,708
		(114.48)	(245.44)	(123.82)	(189.49)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, UI = Unemployment Insurance administrative records, TANF = Temporary Assistance for Needy Families records, FS = Food Stamps records.
Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For self-reports, data source is the adult long-term survey and the sample is all adults interviewed. Unemployment Insurance data uses individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from New York and Massachusetts (representing individuals whose random assignment sites are New York City and Boston). FS and TANF analyses use individual data from Massachusetts, Illinois, Los Angeles County, and South Carolina and represent individuals whose random assignment sites are: Boston, Chicago and Los Angeles. We received TANF and food stamps data from Maryland, but the data quality is suspect. Sample for UI, TANF, and FS is all sample adults with baseline consent.

EXHIBIT ES–9. KEY ECONOMIC SELF-SUFFICIENCY OUTCOMES FOR YOUTH AGES 15–20, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
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)	
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [SR]						
All	0.215	0.007	0.014	0.026	0.039	3,604
		(0.018)	(0.038)	(0.020)	(0.031)	
Female	0.194	0.024	0.049	0.031	0.048	1,838
		(0.024)	(0.048)	(0.027)	(0.043)	
Male	0.235	– 0.011	– 0.023	0.022	0.032	1,766
		(0.027)	(0.058)	(0.031)	(0.045)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 15 to 20 as of December 31, 2007.

RISKY AND CRIMINAL BEHAVIOR

Previous nonexperimental studies suggest that risky and criminal behavior is the outcome domain that has among the strongest associations with neighborhood conditions (Sampson, Morenoff, and Gannon-Rowley, 2002). Neighborhoods may affect risky and criminal behavior through peer influences, the ability of local adults to monitor and support pro-social behavior in the community, or other factors that make pro-social alternatives to crime (such as school and work) more or less attractive. The quality of policing services may also vary across areas. If less economically distressed areas have higher-quality policing, that could help deter criminal behavior but might also lead our analysis of administrative arrest records to understate any beneficial effects of MTO that reduce crime, and overstate any adverse effects towards increased criminality.

Ten to fifteen years after random assignment we found few statistically significant impacts of MTO on risky and criminal behavior, although we did find some signs of a similar sort of gender difference in how youth respond to MTO moves as was found at interim, and some indication of potentially beneficial impacts on arrests for drug selling.

More specifically, as shown in exhibits ES-10 and ES-11, we find:

- Male youth in the Section 8 group engage in more problem behavior, and those in both the experimental and Section 8 groups are more likely to smoke compared with those in the control group.
- Female youth in the experimental group are less likely to have tried alcohol than those in the control group.

- Few statistically significant differences in the number of arrests for violent crimes, property crimes, or other (non-violent, non-property, non-drug) crimes across treatment groups.
- Some indication that the number of arrests for drug-selling declined for male youth in the experimental group and for male grown children in the Section 8 group, and that the likelihood of ever having been arrested for drug-selling may have declined for adults in the experimental group compared with those in the control group.

The estimated effects of MTO on risky and criminal behavior of those who were teens at the long term follow up were more muted compared to MTO's effects on those who were teens at the time of the interim study. The long term data do provide a few indications of the same type of gender difference in youth responses to MTO moves as were found in the interim data, with male youth who moved through MTO engaging in relatively more of some risky behaviors (smoking, behavior problems) compared with controls, and female youth who moved through MTO experiencing declines in some risky behaviors (drinking) compared to controls.

The fact that the effects of MTO on risky and criminal behavior are generally more muted in the long-term data compared to the interim data suggests that contemporaneous neighborhood environments may matter more for these outcomes than does accumulated exposure to less distressed areas. The fact that we see signs of a gender difference in responses to residential mobility in a sample of teens in the long term data that is mostly non-overlapping with the youth who were studied in the interim follow up suggests that the gender difference in MTO impacts documented at interim may not be just a statistical artifact unique to that particular sample.

The one outcome for which we do see at least some hints of more pronounced impacts in the long-term data than in the interim data is with declining arrest rates for drug distribution among the MTO treatment groups compared to controls. These results are more pronounced for teens in the long-term follow-up than for grown children or adults and are consistent with the widely-

documented "age-crime curve" showing that aggregate arrest rates for most crimes peak during late adolescence or early adulthood.

EDUCATIONAL OUTCOMES

We had anticipated that MTO might generate more pronounced positive impacts on educational outcomes in the long-term follow up than in the interim study, in part because more families may have been willing to send their children to schools in lower-poverty areas rather than remain in their original baseline schools. In addition, many social scientists believe that early childhood is a time in which cognitive, socio-emotional, and behavioral skills may be particularly "plastic" and susceptible to intervention, which led us to expect that MTO might have relatively larger effects on the long-term schooling outcomes of participants who were preschool age at baseline.

Ten to fifteen years after random assignment, we found few statistically significant impacts on educational outcomes, including for children who were very young at baseline, and mixed impacts of MTO on different measures of school quality.

More specifically, in comparing children assigned to the experimental and Section 8 groups with control group children, we found:

- Similar average scores on reading and math achievement tests across groups.
- Similar grades in school and rates of grade retention across groups.
- Indications of slightly worse outcomes in some respects for Section 8 males, who were less likely to be on track educationally and less likely to have attended college than control group males.
- No evidence that impacts on educational outcomes varied systematically with the child's age at the time of random assignment.
- A mixed pattern of differences across randomly assigned groups with respect to school measures typically associated with "quality." Youth in the experimental and Section 8 groups attended schools with lower shares of students who are low-income or members of racial and ethnic minority

groups and for the experimental group schools that ranked slightly higher on state exams; but these youth also attended schools that were larger, in terms of the total size of the student body.

These findings do not mean that neighborhood environments never matter for educational outcomes. The MTO mobility intervention generated more pronounced impacts on neighborhood conditions than on school conditions. As with the interim follow up, the long term data show that MTO had modest and mixed impacts on school quality. A majority of MTO children were still attending majority-minority, overwhelmingly low-income public schools located in the districts serving the five original MTO cities.

THE CAUSES OF POVERTY ARE COMPLEX AND EXTEND BEYOND NEIGHBORHOOD EFFECTS

The findings of the long-term study of MTO suggest that housing mobility programs can improve the quality of the immediate environments that families experience -in particular living conditions related to housing quality,

neighborhood poverty and other aspects of disadvantage, and safety. MTO-induced improvements in housing and neighborhood conditions appear to improve some aspects of physical and mental health. Most noteworthy here are reductions in extreme obesity and diabetes and better mental health for adults. MTO moves may also reduce involvement in some forms of risky or criminal behavior of youth, most notably drug selling. However, moving to lower-poverty neighborhoods as part of an MTO-type residential mobility program does not appear to improve educational outcomes, employment, or earnings. The cause of high rates of dropout and unemployment and low earnings found in many disadvantaged urban neighborhoods may rest with barriers that cannot be captured by neighborhood demographics, socio-economic composition, and social processes, at least within the range of neighborhood variation that mobility programs like MTO are able to modify. Put differently, MTO was more successful in improving mental and physical health in poor families that signed up to participate in the program than in bringing about the improvements in education and labor market outcomes needed to boost family incomes.

EXHIBIT ES–10. RISKY AND CRIMINAL BEHAVIOR OUTCOMES FOR YOUTH, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
RISKY AND DELINQUENT BEHAVIOR FOR YOUTH AGES 13–20						
RISKY BEHAVIOR INDEX [SR]						
All	0.467	– 0.001	– 0.002	0.007	0.010	4,623
		(0.014)	(0.029)	(0.015)	(0.022)	
Female	0.442	– 0.027	– 0.054	– 0.017	– 0.026	2,358
		(0.019)	(0.037)	(0.020)	(0.031)	
Male	0.491	0.025	0.053	0.029	0.042	2,265
		(0.018)	(0.039)	(0.020)	(0.028)	
BEHAVIOR PROBLEMS INDEX [SR]						
All	0.379	0.004	0.008	0.009	0.013	4,629
		(0.010)	(0.022)	(0.011)	(0.017)	
Female	0.371	– 0.007	– 0.014	– 0.010	– 0.015	2,361
		(0.014)	(0.028)	(0.015)	(0.024)	
Male	0.387	0.015	0.032	0.027~	0.038~	2,268
		(0.015)	(0.032)	(0.016)	(0.023)	

EXHIBIT ES–10. (CONTINUED)	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
RISKY AND DELINQUENT BEHAVIOR FOR YOUTH AGES 13–20 (CONTINUED)						
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
EVER SMOKED [SR]						
All	0.312	0.042* (0.019)	0.088* (0.039)	0.043* (0.020)	0.064* (0.030)	4,618
Female	0.297	0.022 (0.025)	0.044 (0.051)	0.016 (0.028)	0.026 (0.043)	2,355
Male	0.327	0.062* (0.025)	0.134* (0.054)	0.069* (0.028)	0.098* (0.040)	2,263
EVER HAD ALCOHOLIC DRINK [SR]						
All	0.534	– 0.032 (0.020)	– 0.067 (0.041)	– 0.017 (0.021)	– 0.026 (0.032)	4,618
Female	0.541	– 0.061* (0.026)	– 0.124* (0.053)	– 0.032 (0.029)	– 0.050 (0.045)	2,355
Male	0.528	– 0.003 (0.027)	– 0.006 (0.058)	– 0.004 (0.029)	– 0.005 (0.041)	2,263
NUMBER OF ARRESTS BY CRIME TYPE FOR YOUTH AGES 15–20						
VIOLENT CRIME ARRESTS [CJR]						
All	0.325	0.043 (0.037)	0.091 (0.078)	– 0.062 (0.039)	– 0.094 (0.059)	4,717
Female	0.155	0.027 (0.033)	0.055 (0.069)	– 0.048 (0.033)	– 0.074 (0.050)	2,300
Male	0.481	0.060 (0.064)	0.128 (0.138)	– 0.076 (0.068)	– 0.115 (0.102)	2,417
PROPERTY CRIME ARRESTS [CJR]						
All	0.239	0.065* (0.031)	0.136* (0.064)	– 0.013 (0.034)	– 0.019 (0.051)	4,717
Female	0.091	0.044~ (0.026)	0.090~ (0.053)	– 0.010 (0.023)	– 0.015 (0.035)	2,300
Male	0.375	0.086 (0.054)	0.183 (0.117)	– 0.016 (0.060)	– 0.025 (0.090)	2,417

EXHIBIT ES–10. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
NUMBER OF ARRESTS BY CRIME TYPE FOR YOUTH AGES 15–20 (CONTINUED)						
DRUG POSSESSION ARRESTS [CJR]						
All	0.117	0.010	0.020	– 0.034~	– 0.052~	4,717
		(0.019)	(0.039)	(0.019)	(0.028)	
Female	0.013	0.005	0.009	– 0.011	– 0.017	2,300
		(0.010)	(0.021)	(0.010)	(0.016)	
Male	0.213	0.015	0.032	– 0.057	– 0.085	2,417
		(0.035)	(0.075)	(0.035)	(0.052)	
DRUG DISTRIBUTION ARRESTS [CJR]						
All	0.091	– 0.025	– 0.052	– 0.014	– 0.021	4,717
		(0.017)	(0.036)	(0.022)	(0.033)	
Female	0.006	0.011	0.023	– 0.006	– 0.009	2,300
		(0.009)	(0.020)	(0.010)	(0.015)	
Male	0.169	– 0.059~	– 0.127~	– 0.020	– 0.031	2,417
		(0.032)	(0.069)	(0.042)	(0.063)	
OTHER CRIME ARRESTS [CJR]						
All	0.306	0.000	– 0.001	– 0.048	– 0.072	4,717
		(0.034)	(0.071)	(0.037)	(0.055)	
Female	0.090	0.015	0.030	0.021	0.032	2,300
		(0.023)	(0.047)	(0.026)	(0.040)	
Male	0.503	– 0.014	– 0.029	– 0.113~	– 0.170~	2,417
		(0.064)	(0.136)	(0.067)	(0.100)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report. CJR = criminal justice records.

Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: For the self-reported measures, the data source is the youth long-term survey, and the sample is interviewed youth ages 13 to 20 as of December 31, 2007. For the arrest measures, the data source is individual criminal justice system arrest data (adult and juvenile data from California, Illinois, and Maryland; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived), and the sample is all core household members ages 15 to 20 as of December 31, 2007.

Measures: The Risky Behavior Index is the fraction of 4 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 8 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

EXHIBIT ES–11. NUMBER OF POST-RANDOM ASSIGNMENT ARRESTS FOR GROWN CHILDREN AGES 21–30 AND ADULTS, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
GROWN CHILDREN						
NUMBER OF…						
ANY CRIME ARRESTS [CJR]						
All	2.862	– 0.090	– 0.212	– 0.116	– 0.202	4,641
		(0.171)	(0.404)	(0.192)	(0.332)	
Female	0.967	– 0.054	– 0.126	– 0.037	– 0.063	2,277
		(0.131)	(0.306)	(0.157)	(0.269)	
Male	4.673	– 0.124	– 0.296	– 0.192	– 0.335	2,364
		(0.303)	(0.726)	(0.347)	(0.607)	
VIOLENT CRIME ARRESTS [CJR]						
All	0.626	– 0.055	– 0.129	– 0.042	– 0.072	4,641
		(0.047)	(0.112)	(0.054)	(0.093)	
Female	0.279	– 0.038	– 0.089	– 0.047	– 0.080	2,277
		(0.045)	(0.105)	(0.051)	(0.087)	
Male	0.958	– 0.071	– 0.169	– 0.038	– 0.066	2,364
		(0.082)	(0.196)	(0.092)	(0.161)	
PROPERTY CRIME ARRESTS [CJR]						
All	0.633	– 0.006	– 0.014	– 0.042	– 0.072	4,641
		(0.054)	(0.128)	(0.061)	(0.105)	
Female	0.297	– 0.057	– 0.132	– 0.016	– 0.027	2,277
		(0.046)	(0.107)	(0.065)	(0.112)	
Male	0.953	0.044	0.105	– 0.064	– 0.112	2,364
		(0.093)	(0.222)	(0.100)	(0.175)	
DRUG POSSESSION ARRESTS [CJR]						
All	0.461	– 0.042	– 0.100	– 0.011	– 0.020	4,641
		(0.044)	(0.104)	(0.053)	(0.093)	
Female	0.069	0.001	0.002	0.015	0.026	2,277
		(0.030)	(0.070)	(0.035)	(0.060)	
Male	0.835	– 0.084	– 0.202	– 0.037	– 0.065	2,364
		(0.082)	(0.196)	(0.103)	(0.179)	
DRUG DISTRIBUTION ARRESTS [CJR]						
All	0.338	– 0.047	– 0.111	– 0.063~	– 0.110~	4,641
		(0.035)	(0.082)	(0.037)	(0.064)	
Female	0.058	– 0.023	– 0.055	0.018	0.032	2,277
		(0.024)	(0.056)	(0.028)	(0.049)	
Male	0.605	– 0.069	– 0.165	– 0.140*	– 0.245*	2,364
		(0.063)	(0.151)	(0.067)	(0.116)	

EXHIBIT ES–11. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
GROWN CHILDREN (CONTINUED)						
OTHER CRIME ARRESTS [CJR]						
All	0.804	0.060	0.141	0.042	0.072	4,641
		(0.066)	(0.155)	(0.074)	(0.128)	
Female	0.263	0.063	0.147	− 0.008	− 0.013	2,277
		(0.052)	(0.121)	(0.054)	(0.092)	
Male	1.321	0.056	0.134	0.087	0.153	2,364
		(0.116)	(0.278)	(0.137)	(0.239)	
ADULTS						
NUMBER OF…						
Any crime arrests [CJR]	0.567	0.052	0.110	− 0.016	− 0.026	4,376
		(0.064)	(0.135)	(0.060)	(0.096)	
Violent crime arrests [CJR]	0.149	0.001	0.002	− 0.009	− 0.014	4,376
		(0.020)	(0.043)	(0.022)	(0.035)	
Property crime arrests [CJR]	0.133	0.043~	0.091~	− 0.002	− 0.002	4,376
		(0.025)	(0.052)	(0.022)	(0.036)	
Drug possession arrests [CJR]	0.080	0.006	0.013	− 0.004	− 0.006	4,376
		(0.018)	(0.038)	(0.019)	(0.030)	
Drug distribution arrests [CJR]	0.035	− 0.007	− 0.016	0.001	0.001	4,376
		(0.008)	(0.017)	(0.009)	(0.015)	
Other crime arrests [CJR]	0.171	0.009	0.020	− 0.003	− 0.005	4,376
		(0.027)	(0.056)	(0.025)	(0.040)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.
Model: Experimental and Section 8 impacts were estimated jointly using an OLS (Ordinary Least Squares) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual criminal justice system arrest data. Adult and juvenile data from California, Illinois, Massachusetts, and Maryland; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. The grown children sample is all core household members who are now grown children (under age 18 at baseline and ages 21 to 30 as of December 31, 2007). The adult sample is all long-term survey sampling frame adults with a baseline consent form.
Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

EXHIBIT ES–12. ACHIEVEMENT AND EDUCATIONAL OUTCOMES FOR YOUTH, LONG-TERM EVALUATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
ACHIEVEMENT ASSESSMENT RESULTS FOR YOUTH AGES 13–20						
READING ASSESSMENT SCORE [ECLS-K]						
All	0.000	0.003	0.006	0.040	0.058	4,432
		(0.041)	(0.085)	(0.044)	(0.064)	
Female	0.051	– 0.020	– 0.040	0.054	0.081	2,286
		(0.055)	(0.110)	(0.061)	(0.093)	
Male	– 0.050	0.027	0.058	0.025	0.035	2,146
		(0.056)	(0.118)	(0.058)	(0.081)	
MATH ASSESSMENT SCORE [ECLS-K]						
All	0.000	– 0.025	– 0.052	0.000	0.000	4,420
		(0.044)	(0.090)	(0.048)	(0.069)	
Female	– 0.004	– 0.034	– 0.069	– 0.033	– 0.050	2,280
		(0.055)	(0.112)	(0.061)	(0.093)	
Male	0.004	– 0.016	– 0.034	0.034	0.047	2,140
		(0.060)	(0.128)	(0.067)	(0.093)	
CHARACTERISTICS OF THE AVERAGE SCHOOL ATTENDED BY YOUTH AGES 10–20						
Share minority [CCD, PSS]	0.904	– 0.037*	– 0.077*	– 0.016~	– 0.023~	5,077
		(0.008)	(0.016)	(0.008)	(0.012)	
Share eligible for free lunch [CCD]	0.701	– 0.048*	– 0.101*	– 0.026*	– 0.039*	5,043
		(0.007)	(0.015)	(0.008)	(0.012)	
Number of students [CCD, PSS]	927.1	25.4~	53.0~	41.4*	61.9*	5,077
		(14.9)	(31.1)	(17.5)	(26.1)	
Pupil-teacher ratio [CCD, PSS]	17.834	– 0.103	– 0.214	– 0.061	– 0.091	5,076
		(0.103)	(0.215)	(0.113)	(0.170)	
School-level percentile ranking on state exam [SLAD]	18.684	3.070*	6.430*	1.218~	1.810~	4,884
		(0.651)	(1.364)	(0.661)	(0.983)	
SCHOOLING OUTCOMES FOR OLDER YOUTH						
EDUCATIONALLY ON-TRACK [SR]						
All (ages 15–20)	0.814	– 0.014	– 0.028	– 0.029	– 0.044	3,614
		(0.018)	(0.037)	(0.021)	(0.031)	

EXHIBIT ES–12. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EDUCATIONALLY ON-TRACK [SR] (CONTINUED)						
Female	0.827	− 0.007	− 0.015	0.008	0.012	1,842
		(0.023)	(0.046)	(0.026)	(0.040)	
Male	0.801	− 0.019	− 0.041	− 0.066*	− 0.096*	1,772
		(0.026)	(0.055)	(0.029)	(0.042)	
HIGH SCHOOL DIPLOMA [SR]						
All (ages 19–20)	0.622	− 0.073~	− 0.141~	− 0.056	− 0.092	1,125
		(0.038)	(0.073)	(0.042)	(0.068)	
Female	0.708	− 0.087~	− 0.169~	− 0.054	− 0.089	576
		(0.048)	(0.094)	(0.054)	(0.088)	
Male	0.536	− 0.058	− 0.110	− 0.058	− 0.094	549
		(0.058)	(0.109)	(0.062)	(0.102)	
POST-SECONDARY OUTCOMES FOR OLDER YOUTH						
SINCE 2007, ATTENDED ANY COLLEGE [NSC]						
All (ages 15–20)	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)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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). Average school characteristics are weighted by the amount of time youth spent at each school.

Square brackets indicate the source of the outcome information: ECLS-K = achievement assessment from the Early Childhood Longitudinal Study-Kindergarten cohort study, CCD = Common Core of Data, PSS = Private School Universe Survey, SLAD = School-level assessment data from the National Longitudinal School-Level State Assessment Score Database, SR = self-report, NSC =

Model: Experimental and Section 8 impacts were estimated jointly using an Ordinary Least Squares (OLS) regression model controlling for baseline covariates, weighted, and clustering on family. Outcomes from the adult survey also control for field release. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Executive Summary Exhibit 12. (continued)

Data source and sample: For ECLS-K scores and self-reported measures, the data source is the youth long-term survey. In some cases, the youth school histories on which the school characteristics measures are based combine self-reports from the long-term survey with parent reports from the interim survey. The sample is comprised of long-term survey interviewed youth of varying age ranges (as of December 31, 2007), with details included above. The sample for the NSC measure is all core household members ages 15 to 20 as of December 31, 2007.

Measures: 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 school-level percentile ranking on state exam measure includes schools through 8th grade only for New York and Massachusetts. On-track youth are those who were currently in school or received a high school diploma or GED (certificate of General Educational Development).

CHAPTER 1

THE MTO LONG-TERM STUDY

The neighborhoods of almost every American city provide dramatic contrasts in the well-being of their residents. Children and adults in high-poverty neighborhoods fare worse on a wide range of socioeconomic and health outcomes than do residents of more affluent areas. These disparities have prompted concern about the geographic concentration of poverty in the United States and about the possibility that neighborhood environments themselves may directly affect people's life chances (Jargowsky, 1997; Wilson, 1987).

Why might social problems be much more prevalent in some neighborhoods than in others? Some social scientists emphasize differences in the quality of the local housing stock and in the formal institutions that serve neighborhood residents, such as schools, police, or health care providers. Others point to differences in the capacity of residents to develop the informal social institutions that provide local public goods, as well as differences in the presence of positive peer influences and adult role models (Durlauf, 2004; Jencks and Mayer, 1990; Wilson, 1987). For example, a core group of middle-income residents with high school diplomas and stable employment may be crucial for serving as role models who reinforce (and signal) the value of education and work, and to support a neighborhood's capacity to develop well-functioning informal local networks that may help monitor and support local children. Neighborhoods may also vary in the degree to which peer norms support prosocial rather than antisocial behaviors. Gauging the role that neighborhood environments play in determining the well-being of poor families is vital for housing and other public policies that affect the degree of geographic concentration of poverty in America.

Yet it is difficult in practice to determine the *causal* effects of neighborhood contexts on the long-term life chances of poor families. The social and medical sciences have used data from population surveys to link individuals' earnings, test scores, criminal involvement, health, and other outcomes to information about the characteristics of their residential neighborhoods, which are usually measured at the census tract or ZIP code level. Studies typically find that neighborhood attributes help predict the outcomes of adults and children, even after adjusting for a large number of family background and individual characteristics. (For excellent reviews, see Dietz, 2002; Ellen and Turner, 1997; Kawachi and Berkman, 2003a; Leventhal and Brooks-Gunn, 2000; and Sampson, Morenoff, and Gannon-Rowley, 2002.) A primary concern with these observational or "nonexperimental" studies is that families often have at least some degree of choice over where they live. If, as seems likely, the same attributes directly determine both family well-being and selection of residential location, then what looks like a "neighborhood effect" might instead be caused by hard-to-observe individual attributes associated with both outcomes and residential decisions—what social scientists call "selection bias."

To better understand the causal effects of neighborhood environments on the lives of low-income families, in the early 1990s the U.S. Department of Housing and Urban Development (HUD) launched a randomized housing mobility experiment known as Moving to Opportunity (MTO). From 1994 to 1998, the MTO demonstration program randomly assigned families the opportunity to receive housing vouchers and assistance in moving to low-poverty neighborhoods (census tracts with 1990 poverty rates below 10 percent). Eligibility for MTO was limited to families with children who were living in public housing or in project-based Section 8 assisted housing located in high-poverty areas of five cities (Baltimore, Boston, Chicago, Los Angeles, and New York). It is the first randomized social-science experiment designed to identify the effects of moving from high-poverty to lower-poverty neighborhoods on the social and economic prospects, schooling, health, and well-being of low-income families.

This report summarizes the results of the long-term follow-up study of MTO families, which used in-person surveys, physical health measures, and government administrative records to examine outcomes 10 to 15 years after families entered the study. This introductory chapter provides background on the motivation and design of MTO and discusses how the randomized experimental design helps overcome concerns with selection bias. It also discusses the baseline characteristics and basic mobility outcomes of MTO families, the key results from previous waves of MTO research, and the design of the long-term MTO study. The remaining chapters present the main findings.

1.1 HOW HOUSING POLICIES MIGHT AFFECT LOW-INCOME FAMILIES

The MTO study contributes to debates on housing policy strategies as well as to the large body of scientific and policy research on neighborhood effects. The effects of moving families from one type of neighborhood environment to another need not be identical to what would happen if we changed the neighborhood environments in which people were living without their having to move because, for example, the act of moving itself could create some temporary disruption in a family's life. But evidence from MTO about the aspects of residential mobility that affect people's well-being and life chances is still relevant to the research on neighborhood effects, and might help inform the design of "place-based" community-level interventions as well as residential mobility programs.

The federal government's intervention in the housing market began in earnest during the New Deal in the 1930s, motivated by concerns about the failure of the private housing market to provide decent shelter for low-income families (Hunt, 2009). Inadequate housing quality—for example, units without windows or adequate plumbing or ventilation—was thought to contribute to infant mortality and other adverse health outcomes, as well as other social problems such as delinquency (Hunt, 2009: 17; see also Newman, 1999; Patterson et al., 2004; Harkness and Newman, 2005). These concerns resulted in the U.S. Housing Act of 1937, which led to a system of public housing that peaked in size in the mid-1990s at around 1.4 million units (Olsen, 2003).

Although public support for government housing assistance for the poor remains strong, policymakers have become increasingly interested in alternatives to public housing that may help deconcentrate poverty in America.[1] Of particular concern have been conditions inside the nation's most distressed public housing projects, such as the Henry Horner Homes on Chicago's West Side that Alex Kotlowitz chronicled in his book, *There Are No Children Here* (Kotlowitz, 1992; see also Jones and Newman, 1998). Over the last several decades, housing vouchers and related types of "tenant-based" subsidies have accounted for a growing share of all new federal commitments for low-income housing (Quigley, 2000).[2] More recently, the 1998 Housing Act requires local public housing authorities (PHAs) to provide families in "distressed" public housing with housing vouchers, which provide subsidies to rent units in the private housing market. The 1998 Housing Act also makes it easier for PHAs to convert other projects to vouchers.

Because tenant-based subsidies provide families with more choice over where they live, recipients tend to reside in lower-poverty census tracts than do families with project-based subsidies (Newman and Schnare, 1997; Khadduri, Shroder, and Steffen, 1998; Devine et al., 2003; Olsen, 2003).[3] Other housing policy decisions also influence the geographic concentration of poverty, such as decisions about where to locate project-based units, how much funding to dedicate to developing mixed-income housing projects, and how to fund and operate tenant-based subsidy programs. If neighborhood context exerts an independent effect on family behavior, then housing programs could have cascading influences on different aspects of family life, such as earnings, schooling, health, and child development. This is important because the goal of federal housing policy

---

1 A 2001 survey by National Public Radio, the Kaiser Family Foundation, and Harvard University found that 75 percent of respondents support more spending "for housing for poor people" by government (see www.npr.org/programs/specials/poll/poverty/staticresults3.html, accessed May 5, 2010).

2 A third possibility is government subsidies to private providers of specific housing units. Such policies impose locational constraints on recipients as do other project-based subsidies.

3 A number of studies provide suggestive evidence that per-unit costs may be lower for tenant-based than for project-based subsidy programs (GAO, 2001; HUD, 2000; Olsen, 2000; Shroder and Reiger, 2000).

since at least the Housing Act of 1949 has been to improve family well-being, broadly defined.

A large theoretical literature in the social and medical sciences suggests several mechanisms through which neighborhood characteristics, above and beyond a family's own economic and housing conditions, may affect family outcomes. Our own theoretical orientation leads us to believe that a model recognizing that people respond to incentives can be useful for understanding and predicting behavior in many circumstances. But unlike some "rational choice" approaches, we recognize that aspects of the social environment can also shape the way people respond to incentives, or even directly shape the incentives and opportunities that people face for engaging in different activities.

Exhibit 1.1 presents a general model depicting the MTO intervention's hypothesized pathways that lead from community-level mediators, to individual- and family-level mediators, to outcomes (as originally proposed in Exhibit 1.1 of Orr et al., 2003). The influential typology of Jencks and Mayer (1990) defines four different types of models or theories about why neighborhood environments might affect people's behavioral outcomes and well being—epidemic models, collective socialization, institutional models, and relative deprivation or competition models. Constructing strong non-experimental empirical tests of these models has been challenging for the research community, and new theoretical work has elaborated on many of the types of models mentioned by Jencks and Mayer (see for example, Sampson, Morenoff and Gannon-Rowley, 2002). But this typology nevertheless remains useful for characterizing alternative hypotheses for how residential mobility through MTO, which alters neighborhood context, may affect the outcomes for MTO adults and children.

EXHIBIT 1.1. HYPOTHESIZED PATHWAYS OF MTO IMPACTS

MTO INTERVENTION → RELOCATION TO LOW-POVERTY NEIGHBORHOODS

COMMUNITY-LEVEL MEDIATORS ⟷ PERSON- AND FAMILY-LEVEL MEDIATORS → OUTCOMES

COMMUNITY-LEVEL MEDIATORS	PERSON- AND FAMILY-LEVEL MEDIATORS	OUTCOMES
Housing Market Conditions	Adult Social Networks	Housing Mobility and Assistance
Economic Opportunities	Child/Youth Peer Networks, Peer Behavior	Adult Education, Employment, and Earnings
Social and Physical Environment	Home Environment (including Parenting Behavior)	Household Income and Public Assistance
Social Norms and Values	Parent Attitudes Towards Own and Children's Education, Achievement	Adult/Youth/Child Mental and Physical Health
Schools	Child/Youth Attitudes Toward Own Education, Achievement	Youth/Child Social Well-Being (including Delinquency and Risky Behavior)
		Child/Youth Educational Performance

MTO = Moving To Opportunity
Exhibit 1.1 is a reformatted version of Exhibit 1.1 in the MTO interim impacts evaluation (see Orr et al., 2003)

Epidemic models emphasize the roles played by peer processes, and predict that MTO moves into less disadvantaged and safer neighborhoods will improve outcomes by exposing people to different peer groups. Epidemic models suggest that peers can spread behaviors through learning, imitating their peers, stigma effects (that is, the negative stigma from antisocial behaviors declines when more people do them), and physical circumstances by which, for example, higher rates of crime reduce the chances of getting arrested (Cook and Goss, 1996; Glaeser and Scheinkman, 1999; Manski, 2000; Moffitt, 2001). For example, living near other employed people can increase one's chances of gaining employment through increased social support of work, or by receiving job information or references from neighbors (Kain, 1968; Wilson, 1987, 1996). MTO could affect health outcomes by changing exposure to different types of what Fitzpatrick and LaGory (2000) call "health-related subcultures," which may influence the availability of information about healthy behaviors or social support for such behaviors (Cubbin and Winkleby, 2005), or affect the likelihood of feelings of anomie (Sampson and Bartusch, 1998; Browning and Cagney, 2003).

Some epidemic models predict that peer influences on behavior will vary with the prevalence of the behavior within a community, which can lead to "tipping points" in which the prevalence of the behavior can surge dramatically once the share of people engaging in that behavior crosses some threshold (Crane, 1991; Schelling, 1971, 1978). If there are no tipping points, and if all people respond to a given type of neighborhood or peer environment in the same way, then many epidemic models—those known as linear-in-the-means models where individuals respond to the average behavior of their neighborhood peers—predict that policies to reduce the concentration of disadvantaged families within highly distressed neighborhoods may simply redistribute the volume of anti-social behavior across different neighborhoods (Manski, 2000). But if there are tipping points, so that for example the risk that any given teen engages in delinquency is only influenced by their social environment if more than (say) a third of neighborhood youth engage in delinquency, then policies that help reduce the concentration of disadvantage can help reduce the overall volume of anti-social behavior in a city.

Moves to lower-poverty areas may also affect the extent to which neighborhood adults influence young people who are not their children, what Jencks and Mayer (1990) call *collective socialization* models. For example, children's language development and other learning outcomes may benefit if children live in areas with more highly educated adults (Borjas, 1995). Neighborhood adults who have completed high school and work in the formal labor market might also help serve as role models who signal the value of prosocial activities (Wilson, 1987). Neighborhoods may also vary in the degree to which local adults are willing or able to work together to enforce shared values, what Sampson, Raudenbush, and Earls (1997) call "collective efficacy" (see also Coleman, 1988).

Moves might also enable families to access more productive *institutional resources*. Families may gain access to better schools or a higher quality (or quantity) of policing services (Sherman, 2003; Rivkin, Hanushek, and Kain, 2005). Adults and teenagers moving out of the central city may be closer to low-skilled job opportunities, as in the "spatial mismatch" hypothesis (Kain, 1968). MTO might also help families move into places with higher quality health clinics, safe places for exercise, or grocery stores that sell fresh fruits and vegetables (Blair Lewis et al., 2005; Browning and Cagney, 2003; Huston, Evenson, and Bors, 2003;Kirby and Kaneda, 2005;Morland et al., 2002; Saelens et al., 2003; Subramanian et al., 2006; Zenk et al., 2003). The move may also reduce exposure to pollution, noise, tobacco and alcohol ads, density of fast food restaurants or bars, and crime (Browning and Cagney, 2003; Cubbin and Winkleby, 2005; Dowd, Sisson, and Kern, 1981; Krause, 1993; Morland et al., 2002; Ponce et al., 2005; Skogan, 1990; Thompson and Krause, 2000).

In addition, models from *behavioral economics* suggest that MTO could affect behavior by reducing people's exposure to daily stressors that sap mental energy and willpower, and make it harder for people to resist temptations (Lowenstein and O'Donoghue, 2004). The local context of neighborhoods can also affect self-control and the salience of temptations to buy or use drugs or engage in crime.

Mobility could also improve children's life chances through changes in their **household environments** by, for example, reducing parental stress in ways that lead to less harsh and more supportive or cognitively stimulating parenting. Neighborhoods could also vary in the availability of "marriageable men," which could increase marriage rates and family income for the mostly female heads of household and reduce the likelihood that their children grow up in a single-parent household (Neal, 2004; Wilson, 1987).

Not every social science theory predicts that moves will reduce involvement with risky or antisocial behaviors. **Competition models** emphasize the potential for increased competition for scarce resources like jobs or grades (if teachers grade on a curve) that MTO movers may face in more affluent neighborhoods. The failure to successfully compete for prosocial rewards could lead some people to instead reverse course and try competing for resources or social standing by engaging in antisocial behaviors. Comparisons with the status and accomplishments of new neighbors in more affluent areas could also have a negative psychological impact, as predicted in **relative deprivation** models (Luttmer, 2005). Finally, moving per se could be disruptive even if MTO adults and children choose social "micro-climates" similar to those in their original neighborhoods (Jencks and Mayer, 1990).[4] Because these competing theories make different predictions about how moves could affect the behavior and well-being of parents and their children, MTO's net effect on the well-being and life chances of families is ultimately an empirical question.

All of these theories of how neighborhood environments might affect the behavior and well-being of low-income families are plausible. Yet determining the degree to which these theoretical mechanisms operate in real life is complicated by the fact that most people have at least some degree of choice over where they live. Those residential choices are influenced by an individual's own characteristics, background, and other factors, not all of which can be adequately measured in any social

science study. This makes it difficult to disentangle the effects of neighborhoods on outcomes from those of hard-to-measure individual- or family-level attributes that are associated with residential choices. Suppose a low-income child growing up in the racially and economically integrated Chicago neighborhood of Hyde Park (home to the University of Chicago and President Obama's private residence) does better in school than a low-income child growing up in Washington Park (a very economically distressed, almost entirely African-American neighborhood directly adjacent to Hyde Park). In this case, it is difficult to know whether to attribute the Hyde Park child's better performance in school to the community per se or to whatever it was about her family that motivated or enabled them to live in Hyde Park.

This difficulty, called *selection bias*, is a challenge to observational (nonexperimental) studies that interview families at their current residence. MTO solves this concern by randomly assigning some families but not others the chance to move from high- to low-poverty areas. By virtue of random assignment, the two groups of families should start out identical on average in every way (up to sampling error) except that only one group receives assistance to move to lower-poverty communities. Thus, we can attribute any differences in average outcomes across the groups to the causal effects of moving to low-poverty areas.

Although the design allows us to disentangle causal effects of neighborhoods from selection biases, the fact that MTO-induced moves change so many environmental characteristics at once makes it difficult to pinpoint the specific aspects of neighborhood environments that influence family outcomes. But as we discuss below, experimental estimates can help us rule out the importance of certain mediating mechanisms as explanations for the observed impacts. A review of the overall pattern of results can provide some support for our hypotheses to validate (or invalidate), in part, the theoretical relationship between mechanisms and outcomes. Analyses of the impact of MTO-induced moves on potential mediating variables can help us narrow the possible neighborhood aspects that are most important in accounting for MTO's impacts on key socioeconomic and health outcomes. For example, if

---

4 For example, it is possible that any benefits from moving to less distressed areas through MTO could be lost if youth simply sort themselves back into peer groups that engage in and support antisocial behaviors, even if these types of antisocial peer groups are somewhat less common in their new neighborhood than in their old neighborhood.

we observe an impact on obesity, and we also observe that MTO substantially improves the safety of the neighborhoods in which families reside but has few detectable effects on diet and exercise, this pattern would suggest that something about safety (such as psychological stress) might be more important in explaining the obesity results than explanations that revolve around changing diet and exercise, such as availability of exercise opportunities or food stores.

1.2 MOTIVATION FOR THE MOVING TO OPPORTUNITY EXPERIMENT

HUD sponsored MTO to identify the causal effects of neighborhood environments on the well-being and life outcomes of low-income families. It was motivated in part by an influential series of studies of the Chicago-based Gautreaux residential mobility program (Rubinowitz and Rosenbaum, 2000), which was named after the plaintiff (Dorothy Gautreaux) in a 1966 racial discrimination lawsuit filed against the Chicago Housing Authority (CHA) and HUD. The lawsuit charged discrimination on the basis of the heavy concentration of African-American families in public housing projects located in high-poverty areas. The U.S. Supreme Court agreed in 1976 and ordered CHA to provide housing vouchers to African-American public housing residents that they could use only in neighborhoods in the city or suburbs that were less than 30 percent African-American. Units were assigned to families on a wait list who were either living in or eligible for public housing (Rubinowitz and Rosenbaum, 2000). Some of those apartments were in areas of Chicago that were poor and segregated, but improving, whereas others were in low-poverty, predominantly white or integrated suburban areas (Keels et al., 2005). Some 7,100 African-American families were offered portable Section 8 vouchers. The nonprofit in charge of implementing the Gautreaux program helped identify rental units for families on the program wait list, and most families are believed to have taken the first or second unit offered to them.

A 1988 follow-up survey of 342 families who used Gautreaux vouchers found that moving to the Chicago suburbs instead of other parts of the city was associated with significant improvements in children's eventual educational attainment. Compared with the surveyed students who remained in the city of Chicago, suburban movers were one-quarter as likely to have dropped out of school (5 percent versus 20 percent); more likely to be in a college track in high school (40 percent versus 24 percent); more than twice as likely to attend any college (54 percent versus 21 percent); and almost seven times as likely to attend a four-year college (27 percent versus 4 percent). The only educational attainment measure for which the suburban students did not appear to be doing significantly better than the city students was grade point average, which could reflect higher grading standards in suburban schools (Rubinowitz and Rosenbaum, 2000: 134–136). However more recent studies of Gautreaux show fewer differences between city and suburban movers (Deluca et al., 2010).

Although the Gautreaux program has been extremely influential in housing policy circles, the study was nevertheless not a true randomized experiment. Families may have had some degree of choice about whether to accept the first apartment offered to them, and indeed there is some evidence that the baseline characteristics of families that ended up in the suburbs are systematically different from those who stayed in the city (Mendenhall, Duncan, and Deluca, 2006; Votruba and Kling, 2009). This has made researchers nervous that the Gautreaux city and suburban movers may have differed in preexisting unobserved characteristics as well, which could lead analysts to confound the causal effects of suburban moves with the influence of these unmeasured attributes that may affect both the likelihood of moving to the suburbs and the resulting outcomes.

THE MTO DEMONSTRATION

MTO was authorized by the U.S. Congress in the Housing and Community Development Act of 1992 (for more background on MTO, see Goering et al., 1999; Goering, Feins, and Richardson, 2003). Families were enrolled in MTO between 1994 and 1998 in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. To be eligible, families had to have children under age 18 and live in public housing developments or project-based assisted housing in high-poverty areas (census tracts in which more than 40 percent of the population was living in poverty in 1990). The PHAs in each city conducted

outreach to all eligible households through fliers, tenant associations, and other means, and all those interested received the opportunity to apply for this special program. Interested households joined waiting lists of the local PHAs. Families on the waiting list attended group orientation sessions where they learned about the demonstration program, its experimental design, and specifically the fact they would be randomly assigned to one of three groups (see below). Heads of households that remained interested after the briefing completed the baseline survey and signed an enrollment agreement. Before formal acceptance into the program, families were screened for Section 8 eligibility.

A total of 4,604 households, or around one-quarter of the eligible families living within the designated eligible

housing projects, enrolled in MTO from 1994 to 1998 (Goering, Feins, and Richardson, 2003; Goering et al., 1999, Table 5). Almost all of these households were female-headed. Nearly two-thirds were African-American, and most of the rest were Hispanic (see Exhibit 1.2 for adult characteristics and Exhibit 1.3 for youth characteristics).[5] Three-quarters of household heads were on welfare and the average annual household income was $12,827 (in 2009 dollars), well below the poverty line. Fewer than 40 percent had graduated from high school. These households had an average of 2.5 children under age 18. Although we do not know their immigration status, most spoke English.

_____

5 Supplemental Exhibit 1.1 shows characteristics of the MTO participants who were under 18 when enrolled but adults (aged 21 or older) at the time prior to the initiation of the long-term MTO study in December 2007, a group that we call "grown children" in this report.

EXHIBIT 1.2. BASELINE CHARACTERISTICS OF THE ADULT SAMPLE BY RANDOM ASSIGNMENT GROUP				
	EXPERIMENTAL GROUP	SECTION 8 GROUP	CONTROL GROUP	ALL GROUPS
SAMPLE ADULT CHARACTERISTICS (SR)				
Female	98.2%	97.5%	97.4%	97.8%
AGE				
As of December 2007	45.2	45.3	45.0	45.2
At baseline	33.6	33.7	33.3	33.5
RACE/ETHNICITY				
African-American non-Hispanic	61.1%	60.7%	62.5%	61.4%
Hispanic	30.9%	31.9%	30.2%	31.0%
White non-Hispanic	2.9%	2.5%	2.4%	2.6%
American Indian non-Hispanic	0.5%	0.3%	0.2%	0.4%
Asian/Pacific Islander non-Hispanic	1.6%	2.1%	1.1%	1.6%
Other non-Hispanic	1.8%	1.9%	2.2%	1.9%
MARITAL STATUS				
Never married	61.3%	62.0%	63.3%	62.1%
Married	11.5%	11.7%	10.4%	11.2%
Divorced	9.3%	8.8%	8.7%	9.0%
Widowed or separated	16.8%	16.5%	16.5%	16.6%
EDUCATION				
High school diploma	38.9%	37.1%	36.1%	37.5%
GED	16.5%	17.8%	18.9%	17.6%
Currently in School	15.9%	16.7%	16.4%	16.3%

EXHIBIT 1.2. (CONTINUED)				
	EXPERIMENTAL GROUP	SECTION 8 GROUP	CONTROL GROUP	ALL GROUPS
SAMPLE ADULT CHARACTERISTICS (SR)				
EMPLOYMENT STATUS AND INCOME				
Full-time	14.5%	14.0%	14.1%	14.2%
Part-time	11.5%	10.5%	9.8%	10.7%
Not working	70.4%	72.6%	72.5%	71.7%
Household Income	$12,916.87	$12,703.70	$12,827.91	$12,826.81
GOVERNMENT BENEFITS				
AFDC/TANF	75.0%	75.7%	74.6%	75.1%
Supplemental Security Income	17.7%	17.0%	17.0%	17.2%
Food Stamps	80.7%	81.1%	79.9%	80.6%
Women, Infants, and Children	35.4%	36.0%	35.1%	35.5%
TAKES OVER 30 MINUTES TO GET TO…				
Grocery store	21.0%	22.5%	21.2%	21.5%
Doctor	43.2%	44.1%	44.7%	43.9%
HOUSEHOLD CHARACTERISTICS [SR]				
Number of family members	3.7	3.8	3.7	3.7
Teens in household	39.8%	39.3%	37.4%	38.9%
Disabled household member	15.7%	14.4%	15.5%	15.2%
Female head of household	90.9%	90.8%	91.6%	91.1%
CENSUS TRACT CHARACTERISTICS [CEN]				
Poor	52.3%	51.8%	52.7%	52.3%
Minority	91.0%	91.1%	91.1%	91.1%
Female-headed households	62.7%	62.2%	63.1%	62.7%
Adults with high school degree	90.9%	90.8%	91.6%	91.1%
Adults with college degree	10.3%	10.4%	10.3%	10.3%
Adults employed	74.4%	74.8%	74.4%	74.5%
Adults in professional job	30.1%	29.8%	29.9%	30.0%
MTO SITE DISTRIBUTION [SR]				
Baltimore	13.8%	13.8%	13.8%	13.8%
Boston	20.8%	20.8%	20.8%	20.8%
Chicago	19.4%	19.4%	19.4%	19.4%
Los Angeles	22.5%	22.5%	22.5%	22.5%
New York City	23.5%	23.5%	23.5%	23.5%

EXHIBIT 1.2. (CONTINUED)				
	EXPERIMENTAL GROUP	SECTION 8 GROUP	CONTROL GROUP	ALL GROUPS
MOTIVATIONS FOR SIGNING UP TO MOVE THROUGH MTO [SR]				
Primary or secondary reason for moving was...				
To get away from drugs and gangs	76.9%	76.3%	78.2%	77.1%
Better schools for the children	48.7%	52.4%	47.3%	49.4%
To get a bigger or better apartment	44.9%	45.2%	46.3%	45.4%
To get a job	6.4%	5.2%	6.2%	6.0%
NEIGHBORHOOD CHARACTERISTICS AND MOBILITY EXPERIENCES [SR]				
Household member was crime victim in past 6 months	43.2%	42.6%	41.5%	42.5%
Streets very unsafe at night	49.4%	50.8%	50.4%	50.2%
Very dissatisfied with neighborhood	46.8%	47.0%	46.3%	46.7%
Very confident about finding new apartment	46.7%	49.5%	45.2%	47.1%
Lived in neighborhood 5 or more years	59.5%	62.0%	61.3%	60.8%
Moved more than three times in 5 years prior to baseline	9.0%	9.1%	10.5%	9.5%
Applied for Section 8 previously	40.7%	38.8%	43.0%	40.8%
No family in neighborhood	64.7%	63.2%	64.0%	64.1%
No friends in neighborhood	41.4%	38.8%	40.9%	40.5%
Chatted with neighbors at least once per week	51.7%	49.9%	54.0%	51.9%
Very likely to report neighbor's child getting into trouble	55.0%	53.6%	56.6%	55.1%
SAMPLE SIZE	1,819	1,346	1,439	4,604

Notes: Percentages are percent distributions and may not sum to 100 because of rounding and missing information. Data are weighted to reflect randomization ratios.
Square brackets indicate the source of the outcome information: SR = self-report. CEN = 1990 and 2000 decennial census data.
Data source and sample: Moving To Opportunity (MTO) Participant Baseline Survey. "Sample adult" refers to the 4,604 adults included in the interview sampling frame for the long-term evaluation (see section 1.6 for details). Selection of one adult from each household gave priority to female adults who were more likely to be the children's caretakers. The priority order for selecting both the interim and final evaluation samples was: female heads of the core household, female spouses of the core household head, female baseline heads, female spouses of the baseline head, and finally, non-female heads of the core households. The baseline head is often but not always the same person as the sample adult. The baseline head completed the baseline survey, providing information on both the household and its individual members.
Measures: Characteristics for the census tracts of addresses at which MTO families were living at baseline were linearly interpolated from the 1990 and 2000 decennial Censuses.

EXHIBIT 1.3. BASELINE CHARACTERISTICS OF THE YOUTH SAMPLE BY RANDOM ASSIGNMENT GROUP				
	EXPERIMENTAL Group	SECTION 8 Group	CONTROL GROUP	ALL GROUPS
AGE [PR]				
As of December 2007	16.1	16.2	16.1	16.1
At baseline	4.6	4.7	4.7	4.6
SEX OF YOUTH [PR]				
Female	50.3%	49.3%	47.9%	49.2%
Male	49.7%	50.7%	52.1%	50.8%
OLDER YOUTH CHARACTERISTICS [PR]				
Gifted student or did advanced coursework	13.0%	12.4%	12.4%	12.6%
Suspended or expelled from school in past two years	3.8%	4.3%	3.3%	3.8%
School called about behavior in past two years	19.9%	20.7%	19.6%	20.0%
Behavioral or emotional problems	5.5%	7.0%	5.1%	5.8%
Learning problem	11.8%	13.1%	12.3%	12.3%
YOUNGER YOUTH CHARACTERISTICS [PR]				
In hospital before first birthday	18.1%	18.0%	19.4%	18.5%
Weight at birth (pounds)	7.1	7.1	7.0	7.1
Weighed less than 6 pounds at birth	13.3%	14.5%	14.8%	14.1%
Adult read to youth more than once per day	24.1%	21.5%	26.9%	24.3%
ALL YOUTH CHARACTERISTICS [PR]				
Health problems that limited activity	6.1%	5.8%	5.4%	5.8%
Health problems that required special medicine or equipment	9.1%	9.3%	8.9%	9.1%
Sample Size	2,544	1,965	2,136	6,645

Notes: Percentages are percent distributions. Data are weighted to reflect randomization ratios. At baseline, older youth were ages 6 to 11 and younger youth were ages 0 to 5.
Square brackets indicate the source of the outcome information: PR = parent report.
Data source and sample: Moving To Opportunity (MTO) Participant Baseline Survey. N = 6,645 youth ages 10 to 20 as of December 2007 included in the interview sampling frame for the long-term evaluation (see section 1.6 for details) as reported by the baseline head of household.

Perhaps the most striking finding from the baseline surveys is that more than 40 percent of households that applied had a household member victimized by a crime during the previous six months. Prior qualitative work by several members of our present research team showed that the fear of violence among adults was "so deep-seated that their entire daily routine was focused on keeping their children safe" (Kling, Liebman, and Katz, 2005, p. 244). One applicant, describing her previous experiences living in Boston public housing, reported that, "Every time I looked out my window, there was dead bodies. So I didn't want my kids to grow up in that atmosphere…. They would see the dead bodies. And if they didn't die on the scene, they would see the blood" (p. 252). A Hispanic woman in the Boston site noted that, "In this entryway, a woman was raped. People have been robbed, beaten, and stabbed right here" (p. 253). Her son noted, "On the rooftops there were empty crack vials everywhere. It was pretty violent. Gun shots, fights every day. I saw someone die over there. Some guy was shot in the neck"(p. 253).

It is probably not surprising, then, that fully three-quarters of MTO families report that getting away from gangs and drugs—that is, crime—was the first or second most important reason for enrolling in the program. About half of the households wanted to move so that their children could attend a better school.

Eligible applicant households were randomly assigned to one of three groups:

> The *experimental group* received Section 8 rental assistance certificates or vouchers that they could use only in census tracts with 1990 poverty rates below 10 percent. In each city, a nonprofit organization under contract to the PHA provided mobility counseling to help families locate and lease suitable housing in a low-poverty area. After one year, families were able to use their voucher to relocate without any special MTO-imposed constraints on their moves. Families who stayed in their new low-poverty neighborhoods less than a year did not receive a new voucher. Aside from this requirement, experimental group families were required to abide by all of the regular rules and requirements of the

Section 8 certificate and voucher programs, including having a limited amount of time to search for housing and sign a lease before they lost the rights to their subsidy, being required to contribute 30 percent of their adjusted income toward rent (the same rent requirement as in public housing), and prohibitions on rental assistance to households that engage in certain types of criminal activity.[6]

> The *Section 8 only group* received regular Section 8 certificates or vouchers that they could use anywhere; these families received no mobility counseling.

> The *control group* received no certificates or vouchers, but continued to be eligible for project-based assistance and whatever other social programs and services to which families would otherwise be entitled.

It is important to keep in mind that the control group, to which the experimental and Section 8 only groups are being compared, was not required to stay in public housing during the entire study period. In fact, during the course of the 1990s, HUD sponsored another large-scale program called HOPE VI that ran in parallel to MTO and provided assistance to local PHAs to demolish tens of thousands of the nation's most distressed public housing units, including many of the housing projects in which MTO families were living (see Popkin et al., 2004 for more details about HOPE VI). Families displaced as a result of HOPE VI sometimes received housing vouchers through that alternative program, or they received assistance in moving into other housing projects or mixed-income housing developments in other areas.

Exhibits 1.2 and 1.3 show that the observable baseline characteristics of families are on average very similar across the three randomly assigned MTO groups, suggesting that the randomization in MTO was carried out correctly. Because of random assignment, we

---

6 HUD rules require PHAs to deny housing assistance to households that include someone currently engaged in illegal drug use or in alcohol abuse that affects other residents of the building, or someone who was evicted from federally assisted housing for drug-related criminal activity during the past three years. Local PHAs are also left with some discretion about the degree to which other types of criminal activity affect eligibility; see for example Kooklan (2008) at www.lahsa.org/docs/policyandplanning/section8.pdf.

expect even the unobserved attributes of families at baseline to be similar across MTO groups on average. Any differences in average outcomes across the three randomly assigned MTO groups can thus be attributed to the MTO intervention, as we discuss in greater detail in the next section.

1.3 ESTIMATING THE MTO PROGRAM EFFECTS

The great strength of MTO is the demonstration project's randomized experimental design, which as we noted above helps overcome concerns that previous nonexperimental studies may confound the causal effects of neighborhoods with those of difficult-to-measure individual- or family-level attributes that affect both outcomes and residential selection. For this reason, we focus in this report on results that fully exploit MTO's experimental design.[7] Estimates of MTO's effects correspond to two different questions. The first is the effect of the intervention on the average outcomes of those assigned to one of the MTO mobility groups (the experimental group or Section 8 only group), regardless of whether they relocated through the MTO program. This is known as the ***intention to treat*** (ITT) effect. The ITT effect is estimated by applying ordinary least squares to equation (1), 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 randomly assigned to the experimental or Section 8 groups. The control group is the omitted reference group.

$$(1) \quad Y_i = \alpha_0 + \alpha_1 Exp_i + \alpha_2 S8_i + \alpha_3 X_i + e_i$$

Our estimates for the ITT effects for the experimental and Section 8 treatments equal α_1 and α_2 in equation (1). In our analysis, we also control for a set of baseline characteristics, X_i, which are basically the same as those used in the interim MTO study (Orr et al., 2003) and are described in Supplemental Exhibit 1.2. Under random assignment, the weighted distribution of the baseline variables should be similar across the three MTO groups; therefore, controlling for baseline characteristics should not have much effect on the estimated coefficients α_1

and α_2. However, controlling for baseline characteristics accounts for additional variation in the outcomes in our data, and thus can improve the precision of our estimates for the ITT effects. The term e_i in equation (1) is a residual or error term, and reflects the fact that outcomes for MTO program participants will also be affected by variables that are not included in our analysis. However any unmeasured background characteristics of families that affect outcomes should be uncorrelated with the indicators for treatment group assignment in equation (1) by virtue of random assignment.

We use ordinary least squares to estimate equation (1) for dichotomous (binary) outcome variables as well as continuous outcome variables. In practice, least squares estimates with dichotomous dependent variables yield results quite similar to the average marginal effects that come from probit or logit models (see, for example, Angrist and Pischke, 2009). All estimates weight individuals by the inverse of their probability of assignment to their group to account for changes in the random assignment ratios over time, sampling of youth from large families and Section 8 adults, and, for those interviewed in the phase 2 stage, by the inverse of the likelihood of selection for phase 2 subsampling. Standard errors were adjusted for family clustering and heteroskedasticity using the Huber-White robust method. Analyses were performed using Stata 11.0 Special Edition.

If one is interested in the effectiveness of a program such as MTO in improving the situation of the entire class of families to whom it is offered, the ITT estimates are the appropriate results to examine. The overall effectiveness of such a policy depends on both the effects on families of using a voucher and the proportion of families in one of the MTO mobility groups who use a voucher. For continuous outcome variables, the ITT estimate is essentially equal to the difference in average outcomes between everyone assigned to the experimental or Section 8 only group and everyone assigned to the control group. If the dependent variable is dichotomous, the ITT estimate is essentially the difference in prevalence rates for that outcome between those assigned to the experimental or Section 8 only group versus the control group. Note that this ITT estimate is not biased by the

7 For different perspectives about the value of the MTO experimental design versus its nonexperimental alternatives, see Clampet-Lundquist and Massey (2008), Ludwig et al. (2008), and Sampson (2008).

fact that only some families relocate through the MTO program because we derive the ITT by comparing the average outcomes of everyone assigned to one of the mobility groups, whether they relocate through the program or not, with the average outcomes of everyone assigned to the control group.

If instead one is interested in our second question about the effects on family outcomes from actually moving, one should consult the **treatment on the treated** (TOT) results, which are the effect on those who actually leased up and moved through MTO. Under some assumptions, we can estimate the TOT effect by using information about the proportion of families assigned to the experimental or Section 8 only group that actually moved through the MTO program. More specifically, the TOT estimate will be approximately equal to the ITT effect divided by the share of the experimental or Section 8 only group that relocates using an MTO voucher (Angrist, Imbens, and Rubin, 1996; Bloom, 1984). Our analytic methods are similar to those in Orr et al. (2003) and Kling, Liebman, and Katz (2007), which included more detailed discussions.

With both the TOT and ITT effects, we reiterate that we are comparing the outcomes of the experimental and Section 8 only groups with the average outcomes of a set of families (the control group) that started out living in public housing. As we show in the next section, many control group families left public housing on their own, without any assistance from the MTO demonstration. Some control group families moved to the same kinds of neighborhoods as some of the treatment group families, and some even received vouchers through the regular Section 8 program. The estimates we present here represent the *incremental* effects of offering families MTO housing vouchers, relative to what was available to the control group, whose outcomes reflect what would have happened to the treatment group families in the absence of the MTO demonstration.

Although we can compare the ITT estimates for the experimental group with those of the Section 8 group, the TOT estimates for the two groups are *not* directly comparable. This is because families who signed leases in response to the experimental group voucher offer may differ from those who signed leases in response to the Section 8 group voucher. Differences in the TOT impacts between the two groups may reflect differences in the composition of families who relocate in the experimental versus Section 8 only group, rather than differences only in the types of changes in residential environments that families experience.

We must also bear in mind that even among the experimental group families who signed leases and initially moved to a low-poverty area with their MTO vouchers, not all of them spent the entire follow-up period in low-poverty areas. As we show in the next section, some experimental group families made a second or third move, sometimes to areas with higher poverty rates. And in some cases, even if the family stayed in the same neighborhood, the poverty level of that neighborhood increased over the course of the follow-up period. It is true, however, that experimental and Section 8 only participants spent significantly more time in low-poverty areas than did the control group families.

Finally, we must recognize that MTO is most informative about small-scale or incremental voluntary mobility programs serving families in very distressed areas. A much larger-scale voluntary mobility intervention may generate much different effects. For example, if a large number of public housing families receive vouchers, then it may be more difficult for families to find affordable apartments under the housing voucher program payment standards, at least in the short run until housing market supply responds. A much larger scale voucher mobility program also could potentially increase the general dispersion of poor families, making it harder for any given voucher recipient to find and lease an apartment located in a very low (less than 10 percent) poverty tract. Furthermore, MTO may not be a good guide to the impacts of potentially *involuntary* moves with vouchers such as those following HOPE VI public housing demolitions.[8]

---

8 See Jacob (2004) for a quasi-experimental analysis of the impact of moves induced by public housing demolitions in Chicago on children's schooling outcomes.

EXHIBIT 1.4. MTO VOUCHER LEASE-UP RATES FOR MTO EXPERIMENTAL AND SECTION 8 GROUPS
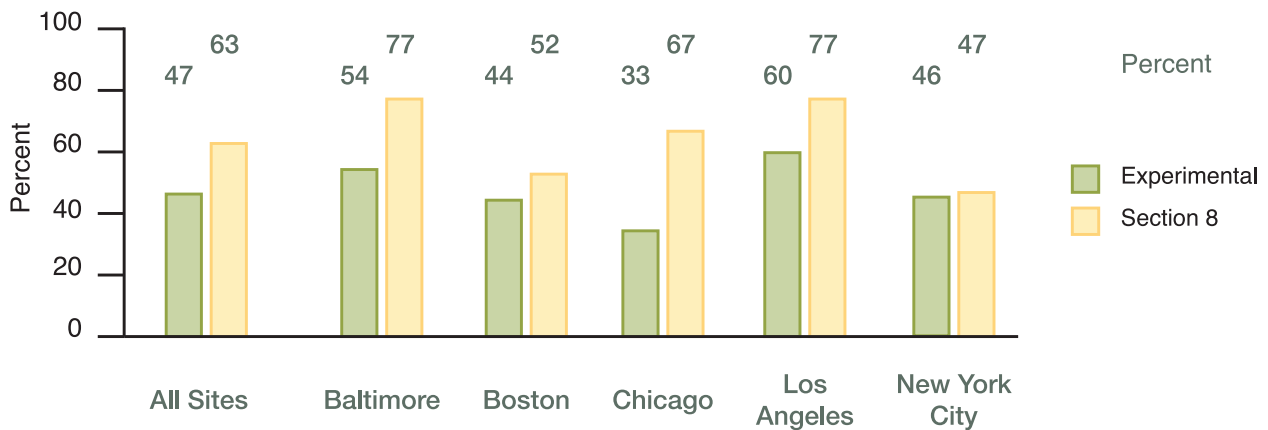
Sample: All adults in the experimental and Section groups included in the interview sampling frame for the long-term evaluation (N = 3,165). See section 1.6 for details. [MTO = Moving to Opportunity]

1.4 MTO MOBILITY OUTCOMES

Of those households assigned to the experimental group, 47 percent relocated using an MTO voucher (the "compliance rate"), whereas 63 percent of those assigned to the Section 8 group relocated through MTO. The compliance rate is higher for the Section 8 group presumably in part because their vouchers (unlike the vouchers offered to experimental group families) had no geographic restriction.

As Exhibit 1.4 shows, the lease-up or compliance rates differed substantially across the five cities, from a low of 33 percent in the Chicago experimental group to a high of 77 percent in the Los Angeles and Baltimore Section 8 groups. Compared with noncompliers, those families who moved with MTO vouchers tend to be younger, be relatively more dissatisfied with their original neighborhoods, and have fewer children (for details see Feins and Shroder, 2005; Shroder, 2002a).

MTO's explicit goal was to help move families into less economically distressed communities. As Exhibit 1.5 shows, by this measure MTO was successful. For the baseline census tracts in which families were residing,

53.1 percent of all tract residents had incomes below the federal poverty line.[9] The average census tract poverty rate for the experimental group families' initial MTO-assisted move was 10.7 percent. These differences in census tract poverty rates narrow considerably by the time of the long-term follow-up survey. By 10–15 years after random assignment, the average control group family is living in a census tract with a poverty rate of 31.3 percent, while the average family in the experimental group is in a tract with a poverty rate of 27.4 percent, and the average family who moved with an MTO experimental group voucher was in a tract with a 21.0 percent poverty rate.[10]

---

9 For addresses at which MTO families were living during intercensal years (between 1990 and 2000 or between 2000 and 2010), we interpolate the characteristics of the census tract in which the MTO family was living using data from the 1990 and 2000 decennial censuses together with census tract-level data from the 2005–09 American Community Survey, which we describe in more detail later in this chapter.

10 Most of the convergence in census tract characteristics across the three randomly assigned MTO groups over time seems to occur because of residential mobility, rather than change over time in the neighborhoods due to gentrification or neighborhood decline. When we reanalyzed the data in Exhibit 1.5 holding the characteristics of the census tracts constant by linking each address in a family's residential history to census tract characteristics measured in the 1990 or 2000 decennial census, we see qualitatively similar findings.

Despite this convergence, the difference is still sizable in the average tract poverty rate that families experience over the entire study period. Specifically, for the control group, the average duration-weighted tract poverty rate (which weights the tract poverty rate for each of a family's addresses by the amount of time the family spends at that address)[11] is equal to 39.6 percent compared with 30.6 percent for the average experimental group family and 20.0 percent for those who moved with an MTO voucher as part of the experimental group. The exhibit also shows that the effect of the Section 8 group on family neighborhood conditions is in the same general direction of the experimental treatment, but is generally more modest in magnitude.

Although MTO was explicitly designed to help public housing families move into lower-poverty census tracts, these MTO-induced moves changed other aspects of the socioeconomic composition of census tracts as well. For example, Exhibit 1.5 shows that the average duration-weighted tract share of adults with a college education was 16.1 percent for controls, 20.3 percent for all families assigned to the experimental group, and 24.7 percent for those in the experimental group who moved through MTO. Previous research by Duncan et al. (1994) suggests that the presence of affluent, college-educated neighbors may have distinct effects above and beyond the presence or absence of low-income families. On the other hand, unlike the findings from the Gautreaux program, which was designed explicitly to reduce racial segregation rather than just economic segregation, MTO engendered relatively modest changes in neighborhood racial composition. The average tract share during the study period was 88.0 percent minority for controls, 82.1 percent for all families assigned to the experimental group, and 75.0 percent for those in the experimental group who used an MTO voucher to move.

Chapter 2 will describe the mobility patterns of families over time in more detail, as well as the resulting characteristics of the neighborhoods in which they have resided. Naturally occurring mobility of families in the control group reduces somewhat the effect of the MTO mobility intervention on the differences in neighborhood environments across randomly assigned groups. But the moves out of the poorest neighborhoods by many in the control group may also make the MTO results more policy relevant: The neighborhood conditions that families in the experimental and Section 8 groups would have experienced had they not been assigned to the MTO experimental and Section 8 treatment groups are not restricted to just the highest poverty census tracts in which a relatively small share of public housing families reside.

1.5 PREVIOUS MTO FINDINGS

Initial studies of the MTO program examined its short-term effects (two to three years after assignment), and individual research teams each focused on just one of the MTO cities. These initial short-term findings are generally consistent with the predictions of previous research suggesting that moving to a less distressed community generally improves adult and child outcomes.[12]

Several years later, HUD sponsored a multisite "interim MTO study" to examine outcomes for adults and youth four to seven years after random assignment, gathered uniformly across the five MTO sites (Fortson and Sanbonmatsu, 2010; Kling, Ludwig, and Katz, 2005; Kling, Liebman, and Katz, 2007; Orr et al., 2003;

---

11 The duration-weighted tract poverty rate is calculated from random assignment through May 31st, 2008. We chose to use the same endpoint for everyone rather than the actual date that a family was surveyed because differences in exactly when families were surveyed may be systematically related to other characteristics that influence outcomes (if, for example, more disadvantaged families were harder to find and therefore were interviewed later during the survey fieldwork period). We use linear interpolations between 1990 and 2000 decennial censuses and from 2000 forward using Census 2000 and the 2005–09 ACS five-year averages to calculate a census tract's percent poverty at a specific point in time. To simplify the calculation, we divide each family's residential history by the time periods they lived at different addresses (or address "spells"), and we split any addresses that span April 2000 into the period before and after 2000 to allow for different interpolation slopes. Let T_i be the total amount of time between when family i was randomly assigned and May 31st, 2008; let k index the different addresses that families reported during the study period, up to K_i total addresses; let Tk_i be the amount of time that family i spent at address k according to our address tracking data; and let Pk_i be our estimate for the census tract poverty rate for that address measured at the midpoint of the relevant address spell. Our calculation for the duration-weighted tract poverty rate (Pi) for each MTO family is then equal to: $P_i = \sum K_i (Tk_i/T_i)*Pk_i$.

12 Results from the site-by-site evaluations are described in Katz, Kling, and Liebman (2001) for Boston; Ludwig, Duncan, and Hirschfield (2001), Ludwig, Ladd, and Duncan (2001), and Ludwig, Duncan, and Pinkston (2005) for Baltimore; Hanratty, McClanahan, and Pettit (1998) for Los Angeles; Rosenbaum, Harris, and Denton (1999) and Rosenbaum and Harris (2003) for Chicago; and Leventhal and Brooks-Gunn (2003, 2004) for New York. Syntheses of these results appear in Goering and Feins (2003).

Sanbonmatsu et al., 2006).[13] Results from the interim evaluation yielded a more complicated pattern than the "better neighborhoods, better outcomes" of previous research.

Exhibits 1.6 and 1.7 summarize selected results from the MTO interim evaluation. The subsequent exhibits in this report all use the same format. The first column shows the "Control Mean" or average outcomes of everyone assigned to the MTO control group, which (because of the randomized experimental design) tells us what would have happened to families in the experimental or Section 8 group had they not been randomly assigned to receive mobility assistance. The next two columns present the ITT and TOT effects for the experimental treatment and then the following two columns present these estimates for the Section 8 treatment. For example, at the time of the interim evaluation, 45 percent of the control group adults were working and not on Temporary Assistance for Needy Families (TANF). The experimental group ITT estimate is 1.8 percentage points. This implies that 46.8 percent of the experimental group was working. As we noted above, the TOT effect will essentially be equal to the ITT effect divided by the proportion of families in the experimental or Section 8 only group that moves as a result of MTO. Because approximately 47 percent of families assigned to the experimental group moved through MTO, the experimental TOT effect is about twice the size of the ITT effect.

The interim MTO data suggest that adults assigned to either of the mobility groups were safer and more satisfied with their housing and neighborhoods and were less likely to report a household member being a victim of a crime or seeing illicit drugs sold (see also Kling, Liebman, and Katz., 2007; and Orr et al., 2003, Exhibit 3.5). Compared with the control group, adults in the experimental group also had large beneficial

impacts on mental health outcomes and some physical health outcomes, such as obesity, but had few detectable impacts on labor market outcomes or social program participation.[14]

Exhibit 1.7 shows that MTO's impacts on youth differed substantially by gender, with generally beneficial effects on female youth and deleterious impacts for male youth. Female youth in the experimental group had better mental health outcomes than those in the control group and were less likely to engage in risky behaviors such as use of marijuana, whereas male youth in the experimental group had higher scores than control group males on an abbreviated index of 11 behavior problems including difficulty concentrating, cheating or lying, bullying, getting in trouble, etc. The MTO treatments also reduced violent behavior for male and female youth, as measured by arrests for violent crime. MTO had few detectable effects on child health aside from an increase in non-sports injuries for male youth (Fortson and Sanbonmatsu, 2009). MTO also had no detectable impacts on educational achievement test scores (Sanbonmatsu et al., 2006), although there were some limitations to the cognitive assessments that the interim study used, as well as some differences in MTO impacts across cities, with some potential signs of test score gains in Baltimore and Chicago (Burdick-Will et al., 2011).

Another goal of the interim study was to learn more about the mechanisms through which neighborhood environments influence youth behavior. The disruption of moving by itself does not appear to explain the gender differences in effects for youth. Mobility rates were actually slightly higher among households with female youth than among those with male youth, and the adverse effects of MTO on male risky behaviors begin to manifest themselves starting several years after the initial move, not right after as a simple mobility disruption model would predict (Kling, Liebman, and Katz, 2007;Kling, Ludwig, and Katz, 2005).

13 The sample universe for the interim MTO study included the household head of the 4,248 families randomly assigned in the MTO demonstration through December 31, 1997, and up to two children (n= 6,683) per household between the ages of 5 and 19 as of May 31, 2001. Surveys were conducted between January and September 2002 with a total of 3,526 adults, 1,780 children ages 8 to 11, and 2,829 youth ages 12 to 19 and achievement tests were administered to a total of 5,264 children and youth ages 5 to 19 (see Orr et al., 2003, Exhibit A.4, p. A-8). Effective survey response rates equaled 90 percent for adults and 89 percent for children and youth, and were quite similar across MTO treatment groups.

14 See Ludwig et al. (2008), Cove et al. (2008), and Clampet-Lundquist and Massey (2008) for additional discussions of MTO's interim effects on labor market outcomes; and see Comey, Briggs, and Weisman (2008) for a discussion of subsequent mobility among MTO families.

The observed pernicious effects of MTO moves on male youth are consistent with both relative deprivation and competition models that Jencks and Mayer (1990) discuss, which emphasize the potential adverse effects on behavior from experiencing a decline relative to one's peers in socio-economic status or academic achievement. However, the existing theoretical literature does not explain why the effects of the relative deprivation and competition models should dominate for male but not female youth. MTO families with male youth did not move into different types of neighborhoods than those with female youth, at least as can be measured by standard administrative data for census tracts and police beats and from survey questions about neighborhoods developed by the Project on Human Development in Chicago Neighborhoods (PHDCN) (Kling, Ludwig, and Katz, 2005). Even within households, brothers and sisters exhibit different responses to similar neighborhood changes. The interim data suggest that MTO reduced violent-crime arrests for both male and female youth; the reduction in violence seems to be more closely associated with lower rates of racial segregation in the treatment neighborhoods than with changes in other neighborhood attributes such as poverty or overall levels of violent crime, or sociological process measures such as disorder (Ludwig and Kling, 2007).

To further illuminate the underlying processes through which MTO affects youth outcomes, and in particular the source of the gender difference in program impacts, MTO researchers later conducted qualitative interviews with 14- to 19-year-old youth (see, for example, Clampet-Lundquist et al., 2006; Popkin, Leventhal, and Weismann, 2008).[15] These interviews uncovered subtle gender differences in how MTO affects youth interactions with families, peers, and neighborhood institutions such as schools and police that were not readily apparent in the interim MTO survey data analysis.

For example, the nature of how boys and girls interact socially with their peers enables girls to more successfully adapt to life in low-poverty areas. Girls were more likely to visit with friends on their porches or inside their

homes in part because some parents placed girls on a "shorter leash" than they did boys (see, for example, Popkin, Leventhal, and Weismann, 2008). Boys often hung out in public spaces, which put them at elevated risk for conflict with neighbors and police, and increased their exposure to delinquent peer groups as well as opportunities to engage in delinquent activities themselves (see also Clampet-Lundquist et al., 2006; Clampet-Lundquist, 2007). MTO may also have other impacts on youth interactions with families beyond the stringency of parental monitoring. For example, boys who moved were less likely to report a nonbiological father figure in their life than boys in the control group, a difference across groups that did not appear for girls. And, despite strong preferences to move so that their children could attend better schools, MTO parents did not subsequently feel informed or empowered to make decisions about enrolling their children in academically promising schools. Thus, many MTO children ended up in schools that were very similar to those in their initial neighborhood (Ferryman et al., 2008).

---

15 See also the Urban Institute's Three City Study of the Moving to Opportunity program at www.urban.org/projects/mto.cfm.

EXHIBIT 1.5. CENSUS TRACT CHARACTERISTICS FOR KEY ADDRESSES FOR MTO ADULTS SURVEYED IN THE LONG-TERM EVALUATION							
	CONTROL GROUP	EXPERIMENTAL GROUP			SECTION 8 GROUP		
OUTCOME	ALL	ALL	COMPLIERS	NON-COMPLIERS	ALL	COMPLIERS	NON-COMPLIERS
SHARE POOR [CEN]							
Baseline address	0.531	0.527	0.530	0.524	0.526	0.540	0.505
Initial MTO-assisted move address			0.107			0.287	
Address at time of MTO interim evaluation (4–7 years after baseline)	0.395	0.299	0.193	0.402	0.326	0.286	0.391
Address at time of MTO long-term evaluation (10–15 years after baseline)	0.313	0.274	0.210	0.334	0.283	0.244	0.345
Average (duration-weighted) of all addresses since Random Assignment	0.396	0.306	0.200	0.407	0.329	0.285	0.400
SHARE COLLEGE EDUCATED [CEN]							
Baseline address	0.103	0.101	0.095	0.106	0.105	0.103	0.110
Initial MTO-assisted move address			0.284			0.173	
Address at time of MTO interim evaluation (4–7 years after baseline)	0.154	0.197	0.243	0.152	0.175	0.186	0.157
Address at time of MTO long-term evaluation (10–15 years after baseline)	0.209	0.229	0.244	0.215	0.224	0.234	0.208
Average (duration-weighted) of all addresses since Random Assignment	0.161	0.203	0.247	0.162	0.182	0.194	0.163
SHARE MINORITY [CEN]							
Baseline address	0.912	0.916	0.912	0.920	0.915	0.906	0.930
Initial MTO-assisted move address			0.624			0.835	
Address at time of MTO interim evaluation (4–7 years after baseline)	0.883	0.836	0.768	0.901	0.875	0.860	0.900
Address at time of MTO long-term evaluation (10–15 years after baseline)	0.841	0.804	0.771	0.834	0.826	0.813	0.846
Average (duration-weighted) of all addresses since Random Assignment	0.880	0.821	0.750	0.888	0.860	0.841	0.892

Notes: Characteristics for the census tracts of addresses at which Moving To Opportunity (MTO) families were living at different points in time were linearly interpolated from the 1990 and 2000 decennial Censuses as well as the 2005–09 American Community Survey (ACS). Addresses at the time of the interim and long-term evaluations are as of the beginning of the fielding period for each study (December 31, 2001 for the interim evaluation and May 31, 2008 for the long-term evaluation). Square brackets indicate the source of the outcome information: CEN = 1990 and 2000 decennial census data as well as the 2005–09 ACS.
Data source and sample: Adult long-term survey. All adults interviewed (N = 3,273).

EXHIBIT 1.6. IMPACT OF MTO ON SELECTED ADULT OUTCOMES, INTERIM EVALUATION (4–7 YEARS AFTER BASELINE)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
NEIGHBORHOOD OUTCOMES						
Share persons in poverty [CEN]	0.385	– 0.078*	– 0.167	– 0.062*	– 0.102*	3,675
		(0.008)	(0.017)	(0.008)	(0.014)	
Saw illicit drugs sold in past 30 days [SR]	0.445	– 0.117*	– 0.248*	– 0.103*	– 0.171*	3,480
		(0.022)	(0.046)	(0.024)	(0.039)	
Household member victim of crime in past six months [SR]	0.209	– 0.040*	– 0.085*	– 0.053*	– 0.089*	3,499
		(0.017)	(0.036)	(0.018)	(0.030)	
Felt safe during the day [SR]	0.750	0.093*	0.198*	0.096*	0.161*	3,514
		(0.018)	(0.039)	(0.019)	(0.032)	
Satisfied or very satisfied with neighborhood [SR]	0.475	0.138*	0.293*	0.108*	0.180*	3,524
		(0.022)	(0.047)	(0.024)	(0.040)	
ADULT OUTCOMES [SR]						
Working and not on TANF	0.452	0.018	0.038	0.019	0.032	3,472
		(0.020)	(0.043)	(0.023)	(0.038)	
Psychological distress index for past month	0.329	– 0.034*	– 0.073*	– 0.012	– 0.020	3,521
		(0.015)	(0.032)	(0.016)	(0.028)	
Good or better health	0.669	– 0.016	– 0.033	– 0.007	– 0.013	3,523
		(0.020)	(0.042)	(0.022)	(0.036)	
Obese, Body Mass Index ≥ 30	0.471	– 0.051*	– 0.108*	– 0.047	– 0.079	3,405
		(0.022)	(0.047)	(0.025)	(0.042)	

Notes: * = p < 0.05 on two-tailed t-test. Robust standard errors are shown in parentheses. Square brackets indicate the source of the outcome information. The control mean is unadjusted. Intention-to-Treat estimate (ITT), or estimated impact of being offered an Moving To Opportunity (MTO) housing voucher; estimated effect of Treatment-on-the-Treated (TOT), or estimated impact of moving using an MTO housing voucher.
Square brackets indicate the source of the outcome information: CEN = 2000 Census, SR = self-report.
Date source and sample: MTO Interim Impacts Evaluation (Orr et al., 2003). Tract poverty measure: Exhibit D2.1. Other neighborhood measures: Exhibit 3.5. Employment/TANF measure: Exhibit 8.8. Physical and mental health measures: Exhibit 4.2.

EXHIBIT 1.7. IMPACT OF MTO ON SELECTED CHILD AND YOUTH OUTCOMES, INTERIM EVALUATION (4–7 YEARS AFTER BASELINE)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
WOODCOCK-JOHNSON REVISED BROAD READING SCORE [M]						
All youth (ages 5–19)	497.31	0.92	2.04	0.45	0.74	5,169
		(0.93)	(2.06)	(1.06)	(1.73)	
Female	499.69	1.84	3.96	– 0.19	– 0.30	
		(1.14)	(2.45)	(1.24)	(1.99)	
Male	494.94	– 0.03	– 0.06	1.13	1.89	
		(1.38)	(3.14)	(1.59)	(2.66)	
WOODCOCK-JOHNSON REVISED BROAD MATH SCORE [M]						
All youth (ages 5–19)	501.23	0.22	0.49	– 1.07	– 1.74	5,187
		(0.78)	(1.75)	(0.85)	(1.39)	
Female	502.30	0.81	1.73	– 1.21	– 1.92	
		(1.06)	(2.26)	(1.09)	(1.73)	
Male	500.18	– 0.36	– 0.82	– 0.91	– 1.52	
		(1.05)	(2.42)	(1.17)	(1.95)	
PSYCHOLOGICAL DISTRESS INDEX [SR]						
All youth (ages 12–19)	0.256	– 0.006	– 0.013	– 0.006	– 0.010	2,803
		(0.015)	(0.032)	(0.016)	(0.028)	
Female	0.304	– 0.045*	– 0.098*	– 0.043	– 0.072	
		(0.020)	(0.043)	(0.023)	(0.037)	
Male	0.208	0.034	0.077	0.031	0.057	
		(0.020)	(0.045)	(0.022)	(0.040)	
VERY GOOD OR BETTER HEALTH [SR]						2,822
All youth (ages 12–19)	0.680	0.001	0.001	– 0.006	– 0.010	
		(0.024)	(0.054)	(0.027)	(0.048)	
Female	0.635	0.030	0.065	0.008	0.013	
		(0.034)	(0.075)	(0.038)	(0.062)	
Male	0.725	– 0.029	– 0.066	– 0.020	– 0.036	
		(0.034)	(0.076)	(0.036)	(0.067)	
EVER USED MARIJUANA [SR]						
All youth (ages 15–19)	0.344	– 0.031	– 0.071	– 0.020	– 0.036	1,569
		(0.034)	(0.079)	(0.038)	(0.069)	

EXHIBIT 1.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EVER USED MARIJUANA [SR] (CONTINUED)						
Female	0.342	– 0.129*	– 0.276*	– 0.079	– 0.132	
		(0.044)	(0.095)	(0.050)	(0.085)	
Male	0.348	0.068	0.169	0.042	0.083	
		(0.049)	(0.122)	(0.052)	(0.104)	
ABBREVIATED BEHAVIOR PROBLEMS INDEX [SR]						
All youth (ages 12–19)	0.343	0.036*	0.080*	0.023	0.039	2,810
		(0.014)	(0.032)	(0.016)	(0.028)	
Female	0.352	– 0.002	– 0.004	– 0.007	– 0.012	
		(0.019)	(0.042)	(0.021)	(0.035)	
Male	0.336	0.075*	0.169*	0.052*	0.095*	
		(0.020)	(0.045)	(0.022)	(0.040)	
FRACTION OF CALENDAR QUARTERS WITH VIOLENT CRIME ARREST [ADMIN]						
All youth (ages 15–19)	0.0138	– 0.0038*	– 0.0091*	– 0.0010	– 0.0019	2,532
		(0.0019)	(0.0045)	(0.0022)	(0.0040)	
Female	0.0088	– 0.0022	– 0.0053	– 0.0030	– 0.0054	
		(0.0020)	(0.0049)	(0.0026)	(0.0047)	
Male	0.0190	– 0.0054	– 0.0129	0.0006	0.0011	
		(0.0030)	(0.0071)	(0.0034)	(0.0061)	
EVER ARRESTED FOR PROPERTY CRIME [ADMIN]						
All youth (ages 15–19)	0.118	0.022	0.051	0.014	0.026	2,646
		(0.015)	(0.036)	(0.017)	(0.031)	
Female	0.087	– 0.006	– 0.013	– 0.007	– 0.013	
		(0.018)	(0.042)	(0.021)	(0.037)	
Male	0.150	0.049*	0.115*	0.035	0.062	
		(0.024)	(0.055)	(0.027)	(0.048)	

Notes: * = $p < 0.05$ on two-tailed t-test. Robust standard errors are shown in parentheses. The control mean is unadjusted. Intention-to-Treat estimate (ITT), or estimated impact of being offered a Moving To Opportunity (MTO) housing voucher; estimated effect of Treatment-on-the-Treated (TOT), or estimated impact of moving using an MTO housing voucher.
Square brackets indicate the source of the outcome information: M = direct measurement, SR = self-report, ADMIN = administrative records.
Date source and sample: MTO Interim Impacts Evaluation (Orr et al., 2003). Test score measures: Exhibits 6.5 (overall) and 6.6 (by gender). Mental health measure: Exhibit 4.5. Health measure: Exhibit 4.4. Behavior measures: Exhibits 5.2–5.5.
Measures: The fraction of calendar quarters with violent crime arrest is based on the first through fourth year after random assignment.

1.6 THE LONG-TERM STUDY

This report presents results from following up with MTO families over the long-term: 10–15 years after the time of random assignment. This evaluation is the first opportunity to answer questions about the longer term effects of being offered a housing voucher, and resulting changes in housing and neighborhood conditions, on subsequent well-being and life outcomes in the following domains: housing and neighborhood conditions; physical health; mental health; economic outcomes; educational outcomes; and risky and criminal behavior.

What can we conclude about the effects of MTO on low-income families so far? It generates persistent differences in housing and neighborhood satisfaction and characteristics across randomized groups, despite subsequent mobility among all three groups. Its impacts on other behaviors through four to seven years after assignment are more complex than previous research would predict.

But of primary importance for public policy are the *long-term* effects on participants. Predicting these long-term impacts on the basis of the interim MTO results is a perilous business, as highlighted by the surprising differences in MTO impacts between the short-term evaluations in each of the sites, which supported the "better neighborhoods, better outcomes" hypotheses, versus the more mixed pattern of results found with the interim evaluation. Social science is filled with examples of long-term impacts that would have been hard to predict based on short- or medium-term impacts. Consider that the Head Start early childhood program yields short-term test score impacts that seem to fade out after a few years, yet Head Start nonetheless seems to generate long-term lasting benefits for participants such as higher rates of high school graduation (Currie and Thomas, 1995;Garces, Thomas, and Currie, 2002; Ludwig and Miller, 2007; Deming, 2009). In hindsight the operative mechanism or causal pathway behind these long-term "sleeper effects" is commonly thought to be Head Start impacts on social-cognitive skills that are not captured by reading and math tests. But this pattern of long-term benefits despite fadeout of short-term test score impacts was certainly not anticipated prior to the collection and analysis of long-term outcome data.

The final impact evaluation was designed to exploit the MTO's randomized experimental design to best address the following key questions:

What are the long-term effects of a housing mobility program intervention on participating families and their children, and how did these effects evolve over time?

If differences in average neighborhood characteristics across groups persist over time, MTO's effects on well-being and behavior may increase over time. Ties to old social networks will diminish, whereas social ties to new communities and use of new neighborhood institutional resources will increase. For example, the benefits of greater exposure to more prosocial and affluent social networks may improve if families become more socially integrated into their new communities, 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. Social integration itself might require families to learn new modes of dress, language, or interactions to "fit in," and families may also learn how to fully use better local services such as schools. Families may also learn how to better navigate the potential opportunities and pitfalls in low-poverty areas. To the extent that exposure to new resources and opportunities makes MTO adults and youth more competitive over time for prosocial rewards, any deleterious effects from competition or declines in relative position may diminish over time. Any deleterious "moving effects" may also attenuate.

The long-term evaluation is also an opportunity to address a number of important open questions that were raised by the interim study's findings. For example, the long-term MTO study enables us to investigate whether differences in the effects of MTO on male and female youth hold up over time, or if they emerge for the younger cohort of youth that the long-term study examines, and better understand why male and female youth may respond so differently to changing neighborhoods. Another example is determining whether MTO impacts on basic screening indicators of adult mental health found in the interim MTO study translate into clinically important changes in mental health, which impose the greatest costs to individuals and society at

large, and so are of greatest relevance for public health and public policy. A final example comes from the possibility that the interim mental health improvements among mothers in MTO translate into improved labor market outcomes over time.

What are MTO's long-term effects on those children who had not yet entered school when the study began?

MTO included many participants who were fairly young at the point of randomization, and so were too young at the time of the interim study to provide developmentally meaningful measures for a variety of key outcomes such as school dropout, mental health, or risky behavior. The long-term evaluation offers an important opportunity to study effects on this group of children (aged 10–20 as of December 2007), including their math and reading achievement, school completion and employment, mental and physical health, and engagement in risky behavior. Those children who are very young at baseline and now teenagers at the time of our long-term (10–15 year follow up) might exhibit even more pronounced responses to the MTO intervention than those who were teens at the time of the interim (4–7) year study for several reasons. One reason is that for children relatively younger at baseline and assessed now, MTO has generated longer-term exposure to less economically-distressed neighborhoods. Children who are relatively younger at baseline will also experience relatively more pronounced MTO-induced changes in exposure to violence, which has been shown to be potentially important for child development (Sharkey, 2010). In addition a growing body of research in developmental psychology, economics, neuroscience, and even animal studies suggests that younger children may be more responsive to social policy interventions than older people given the possibility of declining developmental "plasticity" as people age (Becker and Murphy, 2000; Carneiro and Heckman, 2003; Knudsen et al., 2006; Shonkoff and Phillips, 2000).

Children relatively young at baseline are a particularly important group to study because they most closely parallel the groups to which study effects would most reasonably be extrapolated if an MTO-like intervention were implemented universally. Under a universal version of some MTO-like program, children in eligible families would have the benefits of MTO early in life instead of experiencing the stresses of relocation that the older MTO youth experienced.

What are the mechanisms through which MTO affects long-term outcomes?

Information about the mechanisms through which MTO affects the behavior and life chances of participants is important for the design of tenant-based subsidy programs as well as place-based interventions to modify specific elements of community environments. As noted, because MTO changes multiple neighborhood attributes simultaneously, isolating the effects of specific mechanisms is complicated. However, the study design nonetheless can help us at least rule out some possible mediating mechanisms. If, for example, MTO has long-term beneficial effects on the mental health of female adults, yet has no detectable effect on access to health care services including mental health care, the pattern of findings would provide some evidence against the importance of that mechanism.

SAMPLE DEFINITION AND DESCRIPTION

The sample we used in the long-term evaluation includes all 4,604 families randomly assigned in the MTO demonstration.[16] This group includes the 356 families in the Los Angeles site who were randomized after December 31, 1997, and were excluded from the interim evaluation's sample frame because the corresponding increase in sample size was deemed insufficient to justify shortening the follow-up period. The 4,604 families include 19,932 baseline household members and 17,133 core household members (family members whom the head of household believed would move with the family if they received an MTO voucher). As in the interim MTO study, our analysis focuses on the core household members.

The adult survey sampling frame for the long-term evaluation included one adult from each of the original MTO families. We prioritized female adults from the

---

16 The interim MTO report indicated a total sample size of 4,608 families in the demonstration, but in the process of data collection it was discovered that four of the applicants in MTO were actually members of other MTO households.

core household when selecting our sample. The interview sample included all sample adults from the experimental and control groups, and (because of funding constraints) a random 66 percent subsample of adults from the Section 8 group, or a total of 4,142 adults.[17] The youth sampling frame consisted of all core household members from all three MTO groups who were ages 10–20 on December 31, 2007. Up to three youth per household were eligible for interviews. In those families that contained more than three eligible youth, we randomly selected three youth for the interview sample. The total number of people in our long-term survey sample was 6,308. We also collected parental reports on all core young adult household members who were not in the MTO long-term survey sample frame (under age 18 at baseline, age 21–30 as of December 2007). We refer to this group as MTO grown children.

The allocation of this sample among the treatment groups, by site and overall, is shown in Exhibit 1.8. The number of adults in each site ranges from 572 in Baltimore to 948 in New York City. Somewhat more were assigned to the experimental group than to the Section 8 group to ensure that enough families signed leases with an MTO voucher in the experimental group. The expectation was that the lease-up rate would be lower for experimental families whose vouchers initially required them to live in a low-poverty tract compared with families in the Section 8 only group, whose vouchers were not subject to any additional mobility constraints. Assignment rates within sites were further adjusted during implementation to compensate for the fact that the lease-up rate for the experimental group turned out to be higher than had been anticipated. (The sample weights used in the quantitative analyses adjusted for differences among sites and over time in the random assignment ratio, and our regression models control for site indicators; see the Technical Appendices for more details).

The adult surveys collected information about young adults who are not in the long-term survey sample frame (those who were over age 20 at the time of survey fieldwork) via adult proxy reports. That is, we ask the adults in the survey sample frame (who are almost all female) to report briefly on a few key outcomes for each member in the core MTO household who was not in the survey sample frame. We also asked MTO adults to provide proxy reports on other people who were currently living in the home at the time of the long-term survey, regardless of whether they were in the baseline core household or not. Administrative data are also available to measure outcomes for those not in the survey sample frame.

CONTENT OF PARTICIPANT DATA COLLECTION

Like the interim evaluation, the final impact evaluation focuses on measuring and assessing MTO's long-term effects on the following key outcome domains:

- Housing and mobility
- Neighborhoods and social networks
- Adult and youth physical and mental health
- Adult and youth economic self-sufficiency
- Adult education and youth schooling and educational achievement
- Adult and youth risky and criminal behavior.

These outcomes, as well as a variety of key possible mediating mechanisms, are measured through in-person surveys of MTO adults and youth, direct measurement of health biomarkers, achievement assessments, and administrative records. Exhibit 1.9 summarizes the topics about which data were collected, by method, and according to the age of sample members.

17 Because of funding constraints, the original survey sample fielding design excluded the sample of Section 8 group adults. Additional funding to expand the adult survey sample frame to include 66 percent of the eligible Section 8 adult sample came through in December 2008. The Section 8 adult sample was released in February 2009, eight months after survey fielding began. A small randomly-selected subsample of experimental and control group adults was released to the field survey sample at the same time. Our methods for analyzing data from the MTO Section 8 group will be described in more detail in the technical appendices, forthcoming.

	ALL GROUPS	CONTROL GROUP	EXPERIMENTAL GROUP		SECTION 8 GROUP	
	N	N	N	COMPLIANCE RATE (%)	N	COMPLIANCE RATE (%)
ADULTS						
Baltimore	572	197	252	53.5	123	79.8
Boston	868	326	366	43.6	176	51.1
Chicago	825	232	460	33.4	133	67.4
Los Angeles	929	389	340	60.5	200	71.6
New York City	948	295	401	46.4	252	45.2
All sites	4,142	1,439	1,819	47.4	884	61.6
YOUTH, AGES 10–20						
Baltimore	762	240	268	59.1	254	79.2
Boston	1,267	440	475	38.2	352	54.6
Chicago	1,363	328	701	31.7	334	72.0
Los Angeles	1,539	592	502	62.2	445	78.2
New York City	1,377	418	471	49.6	488	49.2
All sites	6,308	2,018	2,417	47.6	1,873	66.4

EXHIBIT 1.8. ALLOCATION AND COMPLIANCE OF THE LONG-TERM EVALUATION SAMPLES BY SITE AND TREATMENT GROUP

Notes: Percentages are weighted to reflect the randomization ratios and sampling of Section 8 adults and up to three youth per family. "Compliance" is defined as leasing a unit using a housing voucher provided by the MTO program.

Data source and sample: MTO data system. The samples are N = 4,142 adults and N = 6,308 youth ages 10 to 20 as of December 2007 selected for the long-term survey. Excluded from the samples are the N = 462 Section 8 group adults and youth from households with greater than three youth ages 10 to 20 who were not randomly selected for the long-term survey.

EXHIBIT 1.9. CONTENT OF PARTICIPANT DATA COLLECTED FOR THE MTO LONG-TERM EVALUATION			
	ADULT	YOUTH	GROWN CHILDREN
Survey sampling plan	One adult per household (female caregivers prioritized) Up to 4,142 adults total	Up to three youth ages 10–20 as of December 2007 per household Up to 6,308 youth total	Parent reports of household members under age 18 at baseline and up to age 30 as of December 2007
Survey content	Household roster Housing and mobility Neighborhoods Social networks Education Employment and earnings Income, public assistance Savings and assets Mental and physical health Decision making Relationships and parenting Reports on household outcomes	Neighborhoods Social networks Education and schooling Employment and earnings Risky behavior and behavior problems Mental and physical health Decision making Youth reports on parenting	Adult respondent reports on eight items measuring education, employment, mental health, and risky behavior
Direct measurement	Height, weight, waist circumference, and blood pressure	Height and weight	
Biomarkers	Dried Blood Spots to measure total cholesterol, high-density lipoproteins, C-reactive protein (CRP), glycosylated hemoglobin (HbA1c), and Epstein-Barr virus (EBV)		
Achievement assessments		Math and reading achievement tests based on ECLS-K 5th and 8th grade assessments	
Language assessments	Audio taping of open-ended questions and reading passage for pre-selected random subsample	Audio taping of open-ended question and reading passage for pre-selected random subsample	
Administrative data	Addresses, housing program participation, earnings, social program participation, arrests, post-secondary school enrollment, and mortality	Elementary and secondary school records, post-secondary school enrollment, social program participation, arrests, earnings, and mortality	Earnings, social program participation, arrests, post-secondary school enrollment, and mortality
Neighborhood indicators	Census tract data Interviewer observations from neighborhood walk-around	Census tract data Interviewer observations from neighborhood walk-around	

The long-term surveys draw as much as possible (and where appropriate) from the survey instruments used in the interim MTO study. This facilitates comparisons of impacts from interim to final evaluation. In the long-term evaluation, interviewers administered a different reading and math achievement test[18] and different survey measurement of mental and physical health, criminal offending, and victimization. They also collected additional key mediating factors, including basic features of decision-making and audio recordings of speech patterns. Most of the survey items in the interim study, as well as the new additions to the long-term MTO study, come from extensively pre- and field-tested national surveys, which has the advantage of facilitating comparisons between MTO and national samples. The final MTO survey instruments are located at www.mtoresearch.org.

SURVEY DATA COLLECTION

Between June 2008 and April 2010, the Survey Research Center (SRC) at the University of Michigan collected the following information for the long-term study:

- An adult respondent survey to gather data on outcomes and mediating factors for the respondent and other members of the household;
- A youth survey to gather information on outcomes and mediating factors for youth who were ages 10–20 at the end of 2007, just prior to the start of the survey data collection period, and who resided with MTO families at time of enrollment;
- Educational achievement data from standardized reading and math tests administered to MTO youth;
- Biomarker data, including height and weight

measurement. For adults, SRC also measured waist circumference and blood pressure and collected blood spots from finger pricks and sent them to a lab for analysis of various long-term disease precursors;
- Language assessments to measure speech patterns of adults and youth through audiotaping of open-ended survey questions and a reading passage;
- Observational data on the characteristics of the respondent's immediate neighborhood block through neighborhood "walk-arounds" by trained interviewers.

Trained interviewers using Computer-Assisted Personal Interviewing (CAPI) on laptop computers administered the two surveys, household and youth, primarily in the respondents' homes, with the session scheduled at the respondent's convenience.[19] Field interviewers also recorded their observations of the neighborhood environments.

As in the interim study, the survey fielding design employed two-phase sampling to obtain responses from a representative subsample of hard-to-locate respondents (Groves et al., 2004). In the main sample phase, SRC sought to contact and interview all of the adults and youth who were in the survey sample frame. They offered $50 to everyone selected for the survey sample frame to complete the surveys. They offered adults an additional $25 to provide biomarkers (like blood spot samples) and $10 for the audio recording. They offered youth an extra $25 to complete the reading and math achievement tests. Once SRC reached a response rate of approximately 75 percent for the adult or youth survey for a site, it selected a random subset of the 35 percent of the remaining cases for more intensive interviewing efforts. In the data analysis, they "weighted up" the interviews they conducted as part of the second phase by 1/.35 = 2.856 to represent the other hard-to-reach cases that they did not try to interview. The "effective response rate" for the study was 90 percent for the adult sample and 89 percent for the youth sample.[20] Exhibit 1.10

---

18 For the interim MTO study, children were administered standardized Woodcock-Johnson-Revised (WJ-R) reading and math tests. However, the average test scores on the WJ-R did not vary much among MTO adolescents by age; for example, the average scores of 13-year-olds were not very different from those of 18-year-olds. This pattern made us nervous for the long-term study about whether the WJ-R assessments adequately capture the sort of academic content that adolescents will (or at least should) be learning in school. Given these concerns, for the long-term MTO study we replaced the WJ-R with the reading and math tests developed for the U.S. Department of Education's Early Childhood Longitudinal Survey of Kindergarteners in 1998 (ECLS-K) for the fifth and eighth grade waves. To avoid "ceiling effects" (cases where some children know everything that is covered on the test and get everything right, so that their actual achievement level cannot be pinned down), the ECLS assessments were supplemented with additional reading and math items from the 10th grade test that was used in the U.S. Department of Education's National Education Longitudinal Study of 1988 (NELS).

19 A small number of adult and youth survey interviews were completed by telephone.

20 For example, if the phase one response rate was 74 percent, and a 3.5-in-10 subsample of hard-to-locate cases were worked in phase two with a response rate of, say, 50 percent, then the effective response rate would be .74+.26*.5=.87, or 87 percent.

shows response rates for the adult and youth survey, by treatment status. Response rates are statistically similar between the experimental and control groups for adults and youth, and the Section 8 and control group youth. The Section 8 group adult response rate is slightly lower, and statistically differs from the control group.

ADMINISTRATIVE DATA COLLECTION

The long-term study also includes an extensive effort to collect administrative data from everyone who was a member of the core MTO household at baseline, regardless of whether they were included in the long-term survey sampling frame. At baseline, all household heads signed a waiver granting HUD-authorized researchers permission to access government administrative data on themselves and their families for purposes of evaluating MTO.

The study collected administrative data on:[21]

- Housing assistance from HUD administrative data;
- Earnings and employment from state unemployment insurance records;
- Social program participation (welfare, food stamps) from state data records;
- Elementary and secondary school outcomes from student-level school records from local school districts and in some cases state education agencies;
- Post-secondary enrollment data from the National Student Clearinghouse;
- Adult and juvenile arrest histories from state criminal justice agencies (and in some cases county- or city-level juvenile justice agencies).

We collected administrative data for the five MTO sites and for a random sample of the other jurisdictions to which participants have moved. Sampling is necessary because participants have now resided in 40 states and attended schools in more than 300 public school districts. Sampling enables us to provide a representative picture of the outcomes of everyone in all three of the randomized MTO groups, but without having to collect administrative data from every agency that includes

MTO families. The details of the sampling scheme will appear in the forthcoming Technical Appendices.

We used census-tract-level data from the 1990 and 2000 decennial censuses and the 2005–09 waves of the American Community Survey (ACS) to measure MTO changes in the social and institutional environments in which families are living. A census tract is a small continuous geographic area comprising between 1,500 and 8,000 residents (600 to 3,200 housing units) that is delineated by observable physical objects (road, river, train tracks, state or county line).[22] Census tracts are often used in the social science research literature as a geographic measure of "neighborhood."
We are also currently in the process of collecting data on local-area crime rates from police departments serving the five main MTO demonstration cities.

1.7 OVERVIEW OF THE REPORT

MTO research provides direct evidence on the impact of housing mobility interventions on low-income families. The findings are critical to improving our understanding of the impacts of neighborhood context more generally on the contemporaneous well-being and longer-term outcomes for poor households. An experiment such as MTO that alters residential mobility and places families in different neighborhoods also can be useful for simulating the potential effects of wholesale neighborhood changes on individuals from disadvantaged backgrounds. It may be possible to identify specific mechanisms that will help target issues that can be directly addressed in today's high-poverty communities.

MTO findings are also central to a number of ongoing housing-policy debates. The contrast in youth outcomes between the Section 8 and the control groups will be directly relevant to debates about whether to increase the number of Section 8 vouchers, and particularly about whether to offer vouchers to current public housing residents rather than renovate the aging public housing stock (Olsen, 2003; Quigley, 2000).

---

21 The study also collected mortality records from the National Center for Health Statistics' National Death Index, although those outcomes are not included in this report.

22 See the U.S. Census Bureau's Participant Statistical Area Program Guidelines at www.census.gov/geo/www/psapage.html and Participant Statistical Area Program Criteria available at www.census.gov/geo/www/statarea.pdf. (accessed May 5, 2010).

In extrapolating these results to other housing programs, it is important to keep in mind that different people may respond differently to the same policy intervention—a possibility that researchers refer to as "treatment heterogeneity." We might be particularly concerned that people who volunteer for a housing mobility programs may be respond differently than would people who are involuntarily moved, for example because their public housing project is scheduled for demolition. MTO estimates will be most directly relevant to other voluntary mobility programs that involve similarly disadvantaged minority families living in very distressed public housing communities. In fact for MTO-type programs and populations, these MTO findings will be particularly valuable because most housing mobility programs are not implemented in a way that includes a valid control group, and so cannot be reliably evaluated directly themselves.

The treatment that the experimental group received does not correspond precisely to particular policies now under consideration, but this group may provide information about the value of more intensive counseling initiatives that could expand the number of neighborhoods into which voucher families move. Remember, though, MTO is a relatively small program, and the lessons from it are most directly applicable to strategies to incrementally expand or revise current programs, such as adding several hundred Section 8 vouchers in a city. For the most part, there are too few families who moved to a given low-poverty neighborhood in MTO to really change the character of the new place.[23] A large-scale program, such as eliminating all public housing and issuing vouchers to former tenants, may have different effects than a smaller-scale program.

This report is organized into eight chapters. Chapter 2 provides more detailed information about the MTO experiment's effect on mobility patterns for families, housing conditions and receipt of means-tested housing assistance from the government, and neighborhood characteristics and social networks. Chapters 3 and 4 provide results on the physical and mental health outcomes, respectively, of MTO adults and youth. Chapter 5 provides results on economic outcomes, including employment, earnings, and income from government social programs and other sources. Chapter 6 examines impacts of MTO on the risky and criminal behavior of participating adults and youth, while Chapter 7 examines impacts on educational outcomes. Chapter 8 concludes and discusses the implications of the long-term results of MTO for social science and public policy.

---

23 Measuring the effects that MTO families might have on the areas to which they move is not one of the goals of this evaluation.

EXHIBIT 1.10. MTO ADULT AND YOUTH LONG-TERM SURVEY INSTRUMENT RESPONSE RATES BY RANDOM ASSIGNMENT GROUP

	ADULT (%)	YOUTH (%)
Experimental Group	90.8	90.1
Section 8 Group	86.6	86.9
Control Group	90.0	88.9
All Groups	89.6	88.7

Notes: The response rate calculations presented above account for the change over time in the MTO random assignment ratios as well as the two-phase survey sampling design of the long-term evaluation. The weights equal the product of the random assignment ratio weight (described previously) and the sampling weight (equal to 1 for families interviewed in Phase 1, equal to 1/0.35 for families who were randomly selected for the Phase 2 survey sample, and equal to 0 for families who were not randomly selected for the Phase 2 survey sample. The effective response rate is equal to the weighted number of interviews divided by the weighted survey sample frame total minus the weighted number of decedents.
Data source and sample: MTO long-term evaluation survey. The survey sampling frame included 4,604 adults and 6,645 youth (ages 10 to 20 as of December 2007), and interviews were conducted with 3,273 adults and 5,101 youth.

SUPPLEMENTAL EXHIBIT 1.1. BASELINE CHARACTERISTICS OF THE GROWN CHILD SAMPLE BY RANDOM ASSIGNMENT GROUP

	EXPERIMENTAL GROUP	SECTION 8 GROUP	CONTROL GROUP	ALL GROUPS
AGE [PR]				
As of December 2007	24.6	24.4	24.4	24.5
At baseline	12.7	12.6	12.6	12.7
GENDER [PR]				
Female	50.3%	49.3%	47.9%	49.2%
Male	49.7%	50.7%	52.1%	50.8%
OTHER CHARACTERISTICS [PR]				
Gifted student or did advanced coursework	13.0%	12.4%	12.4%	12.6%
Suspended or expelled from school in past 2 years	3.8%	4.3%	3.3%	3.8%
School called about behavior in past 2 years	19.9%	20.7%	19.6%	20.0%
Behavioral or emotional problems	5.5%	7.0%	5.1%	5.8%
Learning problem	11.8%	13.1%	12.3%	12.3%
Health problems that limited activity	6.1%	5.8%	5.4%	5.8%
Health problems that required special medicine/ equipment	9.1%	9.3%	8.9%	9.1%
SAMPLE SIZE	1,887	1,336	1,420	4,643

Notes: Percentages are percent distributions. Data are weighted to reflect randomization ratios. At baseline, older youth were ages 6 to 11 and younger youth were ages 0 to 5.
Square brackets indicate the source of the outcome information: PR = parent report.
Data source and sample: MTO Participant Baseline Survey. N = 4,643 long-term survey sample grown children (ages 21 to 30 as of December 31, 2007, and ages 7 to 17 at baseline) from all 4,604 MTO families as reported by the baseline head of household.

SUPPLEMENTAL EXHIBIT 1.2. MEAN, STANDARD DEVIATION, AND SHARE MISSING FOR BASELINE COVARIATES BY MTO SAMPLE						
	ADULTS			YOUTH AGES 10–20		
DESCRIPTION	MEAN	STANDARD DEVIATION	SHARE MISSING	MEAN	STANDARD DEVIATION	SHARE MISSING
CHARACTERISTICS OF THE ADULT/YOUTH'S PARENT						
ADULT/PARENT'S AGE AS OF 12/31/07 (OMITTED CATEGORY: > 50)						
≤ 35	0.141	0.348	0	0.184	0.387	0
36–40	0.224	0.417	0	0.305	0.460	0
41–45	0.231	0.422	0	0.247	0.431	0
46–50	0.187	0.390	0	0.149	0.356	0
RACE (OMITTED CATEGORY: WHITE)						
African-American	0.646	0.474	0.019	0.662	0.469	0.016
Other race	0.279	0.444	0.019	0.273	0.441	0.016
HISPANIC ETHNICITY	0.318	0.464	0.009	0.306	0.459	0.010
MALE ADULT/PARENT	0.018	0.133	0	0.018	0.132	0
EDUCATIONAL ATTAINMENT AT BASELINE						
Adult had a GED	0.191	0.393	0.065[†]	0.196	0.397	0.063[†]
Adult reported having completed high school	0.391	0.488	0.065[†]	0.373	0.484	0.063[†]
Adult is missing information on GED and high school diploma status	0.065	0.247		0.063	0.243	
Adult was enrolled in school	0.167	0.366	0.036	0.182	0.380	0.034
NEVER MARRIED	0.628	0.476	0.031	0.663	0.465	0.032
UNDER AGE 18 AT BIRTH OF FIRST CHILD	0.256	0.426	0.044	0.296	0.447	0.043
WORKING	0.262	0.433	0.032	0.220	0.407	0.033
SITE (OMITTED CATEGORY: NEW YORK CITY)						
Baltimore	0.136	0.343	0	0.128	0.334	0
Boston	0.204	0.403	0	0.183	0.387	0
Chicago	0.206	0.404	0	0.231	0.422	0
Los Angeles	0.225	0.418	0	0.238	0.426	0
FLAG INDICATING ADULT INCLUDED IN FIRST SURVEY RELEASE	0.358	0.479	0			

SUPPLEMENTAL EXHIBIT 1.2. (CONTINUED)						
	ADULTS			YOUTH AGES 10–20		
DESCRIPTION	MEAN	STANDARD DEVIATION	SHARE MISSING	MEAN	STANDARD DEVIATION	SHARE MISSING
CHARACTERISTICS OF THE BASELINE HOUSEHOLD						
HOUSEHOLD MEMBER HAD A DISABILITY	0.152	0.358	0.008	0.143	0.348	0.009
HOUSEHOLD STRUCTURE						
No teen (ages 13–17) children in core household at baseline	0.621	0.485	0	0.754	0.431	0
Core household size is two or smaller	0.210	0.408	0	0.086	0.280	0
Core household size equals three	0.308	0.462	0	0.247	0.432	0
Core household size equals four	0.231	0.421	0	0.259	0.438	0
HOUSEHOLD MEMBER WAS BEATEN/ASSAULTED; THREATENED WITH A GUN OR KNIFE; OR HAD THEIR PURSE, WALLET, OR JEWELRY SNATCHED DURING THE 6 MONTHS PRIOR TO BASELINE	0.422	0.492	0.007	0.426	0.493	0.008
RECEIVING AFDC	0.756	0.429	0.005	0.815	0.387	0.005
OWNED A CAR	0.183	0.387	0.004	0.188	0.390	0.005
HEAD OF HOUSEHOLD AT BASELINE…						
Had lived in their neighborhood for 5 or more years	0.606	0.483	0.025	0.557	0.490	0.028
Stopped to chat with neighbor in street or hallway at least once a week	0.521	0.497	0.009	0.527	0.497	0.009
Was very dissatisfied with his/ her neighborhood	0.474	0.497	0.009	0.486	0.497	0.01
Reported he/she would be very likely to tell neighbor if saw neighbor's child getting into trouble	0.546	0.497	0.005	0.550	0.496	0.005
Did not have any family living in their neighborhood	0.631	0.481	0.005	0.638	0.479	0.005
Did not have any friends in the their neighborhood	0.401	0.488	0.008	0.402	0.488	0.008

SUPPLEMENTAL EXHIBIT 1.2. (CONTINUED)						
	ADULTS			YOUTH AGES 10–20		
DESCRIPTION	MEAN	STANDARD DEVIATION	SHARE MISSING	MEAN	STANDARD DEVIATION	SHARE MISSING
Felt streets near home were very unsafe at night	0.506	0.498	0.008	0.507	0.499	0.006
Was very sure he/she would find an apartment in a different area of the city	0.477	0.498	0.007	0.499	0.498	0.008
Had moved more than three times in the past 5 years	0.097	0.294	0.009	0.112	0.314	0.009
Had already previously applied for a Section 8 voucher or certificate at baseline	0.402	0.489	0.008	0.382	0.484	0.01
HEAD OF HOUSEHOLD'S PRIMARY OR SECONDARY REASON FOR WANTING TO MOVE WAS…						
To get away from gangs or drugs	0.773	0.414	0.023	0.766	0.418	0.024
To have access to better schools for children	0.505	0.494	0.027	0.527	0.492	0.03
YOUTH CHARACTERISTICS (AGES 10–20 IN 2007)						
YOUTH'S AGE AS OF 12/31/07 (OMITTED CATEGORY: 15)						
10				0.009	0.094	0
11				0.030	0.170	0
12				0.048	0.214	0
13				0.093	0.291	0
14				0.102	0.303	0
16				0.128	0.334	0
17				0.128	0.335	0
18				0.125	0.330	0
19				0.110	0.314	0
20				0.112	0.315	0
MALE YOUTH				0.495	0.500	0
FLAG INDICATING AGE 6–17 AT BASELINE (OMITTED CATEGORY: UNDER 6)				0.396	0.489	0
HEALTH PROBLEMS						
Problems requiring special medicine and/or equipment				0.084	0.269	0.059

SUPPLEMENTAL EXHIBIT 1.2. (CONTINUED)						
	ADULTS			YOUTH AGES 10–20		
DESCRIPTION	MEAN	STANDARD DEVIATION	SHARE MISSING	MEAN	STANDARD DEVIATION	SHARE MISSING
HEALTH PROBLEMS (CONTINUED)						
Health problems limiting activity				0.056	0.230	0.068[†]
Flag indicating missing information on health problems (see preceding item)				0.068	0.252	
CHARACTERISTICS APPLYING ONLY TO YOUTH AGES 0–5 AT BASELINE						
HOSPITALIZATION						
Hospitalized before first birthday				0.191	0.393	0.057[†]
Flag indicating missing information on hospitalization (see preceding item)				0.057	0.232	
BIRTHWEIGHT						
Weighed less than 6 pounds at birth				0.150	0.357	0.093[†]
Flag indicating missing information on birthweight (see preceding item)				0.093	0.291	
READ TO BY AN ADULT						
Read to by an adult more than once a day				0.264	0.441	0.091[†]
Flag indicating missing information on being read to (see preceding item)				0.091	0.287	
CHARACTERISTICS APPLYING ONLY TO YOUTH AGES 6–17 AT BASELINE						
BEHAVIORAL/EMOTIONAL PROBLEMS						
Behavioral/emotional problems in 2 years prior to baseline				0.067	0.250	0.157[†]
Flag indicating missing information on behavioral/ emotional problems (see preceding item)				0.157	0.364	

SUPPLEMENTAL EXHIBIT 1.2. (CONTINUED)						
	ADULTS			YOUTH AGES 10–20		
DESCRIPTION	MEAN	STANDARD DEVIATION	SHARE MISSING	MEAN	STANDARD DEVIATION	SHARE MISSING
SUSPENSIONS/ EXPULSIONS						
Suspended or expelled from school in 2 years prior to baseline				0.036	0.187	0.061[†]
Flag indicating missing information on suspensions/ expulsions (see preceding item)				0.061	0.239	
GIFTED STUDENT						
Gifted student or did advanced course work				0.143	0.351	0.079[†]
Flag indicating missing information on gifted student (see preceding item)				0.079	0.269	
LEARNING AND BEHAVIORAL PROBLEMS						
Learning problem in 2 years prior to baseline				0.129	0.336	0.050[†]
Flag indicating missing information on learning problems (see preceding item)				0.050	0.217	
School called about youth's behavioral problems in 2 years prior to baseline				0.204	0.396	0.040

Notes: Weighted means, standard deviations, and share missing are shown for the interviewed adults and youth (ages 10–20). For baseline information that only applied to children age 0–5 or 6–17 at baseline, we exclude children to whom the item did not apply before calculating the means and other statistics. Missing values are replaced with mean imputed values for covariates that were missing for less than 5 percent of the full sampling frame. The imputed means for adult and household covariates are conditional on site and randomization in 1998 versus earlier years. Youth imputations are conditional on site, randomization in 1998 versus earlier years, age at baseline, and gender. Missing values for covariates with at least 5 percent missing for the full sample are excluded from the statistics above and are flagged as having missing data and set to zero in the analysis.
Data source and sample: MTO Participant Baseline Survey and MTO data system. Adults interviewed at final survey (N = 3,273); Youth interviewed at final survey (N = 5,101).
† Indicates that baseline information (for the applicable sample) was missing on this covariate for at least 5 percent of the full sample. Due to the level of missing information, a missing flag was included in the analysis and the mean and standard deviation shown in this table are conditional on the baseline information being nonmissing (for example, the covariate is set to zero for individuals with missing information on this item however these zeros are not included in the means shown).

CHAPTER 2

IMPACTS ON MOBILITY, HOUSING, NEIGHBORHOODS, AND SOCIAL NETWORKS

This chapter describes the long-term impacts of MTO on the residential mobility patterns, housing conditions, neighborhood conditions, and social networks of participating families. These outcomes are measured 10 to 15 years after random assignment and are derived from survey self-reports by MTO participants, geocoded address histories of families linked to tract-level data from the Census Bureau, and interviewer observations about the respondents' housing units and surrounding neighborhoods.

We find that, on average, control group families made about two moves over the course of the 10–15-year study period; families in the experimental or Section 8 groups who moved with a program voucher made, on average, approximately one extra move over the course of the study. MTO improved the quality of the housing units in which families were living but had no detectable impacts on housing costs for families, although the confidence intervals around these latter estimates do not allow us to rule out modest impacts. MTO had no detectable impacts on being "literally homeless" (i.e., living at a shelter, on the street, in an abandoned building, in a car or van, or similar circumstances). However, being in the Section 8 group increased the chances of "doubling up" with friends or relatives. Families in the experimental and Section 8 groups were less likely than controls to report having trouble paying their rent on time, but were more likely to have trouble paying their utility bills.

MTO allowed many families to move to lower-poverty neighborhoods. During the 10 to 15 years after families were randomly assigned, the poverty rate of the census tracts in which the control group lived averaged approximately 40 percent. For those families that moved with MTO vouchers in the experimental and Section 8 groups, average tract poverty rates were about 18 and 11 percentage points lower, respectively. MTO had more modest effects on the degree of neighborhood

racial segregation that families experienced, but the program did have sustained effects on a variety of other measures of neighborhood socioeconomic composition. Despite concerns about potential social isolation of MTO families who moved to more affluent areas, program moves increased the likelihood of having close friends who had a college degree. MTO also made adult and female youth participants, but not male youth, feel safer in their new neighborhoods.

2.1 BASELINE AND CONTROL GROUP CONTEXT

The MTO demonstration recruited families from public housing or assisted housing projects in some of the poorest census tracts in the study's five cities. About 90 percent of families interviewed for the long-term evaluation lived in public housing at baseline. The other 10 percent lived in housing run by private operators with government assistance.[1]

Exhibit 2.1 shows baseline characteristics of the long-term survey respondents and the census tracts where they lived. At the point of enrollment in MTO, families expressed considerable dissatisfaction with their housing. Only one-quarter of adults rated the condition of their housing as excellent or good. At baseline, families lived in census tracts with poverty rates above 53 percent, and in which 91 percent of all residents were members of racial or ethnic minority groups. The tracts had low labor force participation and adult education levels. They also had high proportions of families headed by a single parent or on welfare.

---

1 Private developers built rental housing under a number of different federal programs from the 1960s to the 1980s. The developers received subsidies in various forms (such as below-market interest rates on mortgages) in exchange for providing some units affordable to low-income renters. Such developments were built under a number of programs (rent supplement, 221(d)(3), BMIR, Section 202, Section 236, Section 8 new construction or substantial or moderate rehabilitation). Units in such developments are typically referred to as private, "assisted" housing.

EXHIBIT 2.1. SELECTED BASELINE CHARACTERISTICS FOR ADULTS SURVEYED IN THE LONG-TERM EVALUATION: HOUSEHOLD AND NEIGHBORHOOD CHARACTERISTICS				
OUTCOME	CONTROL GROUP	EXPERIMENTAL GROUP	SECTION 8 GROUP	ALL GROUPS
HOUSING CONDITION				
Rated baseline housing as excellent or good [SR]	26.3%	29.3%	25.0%	27.1%
BASELINE TRACT CHARACTERISTICS INTERPOLATED USING 1990 AND 2000 CENSUS DATA TO THE FAMILY'S DATE OF ENROLLMENT BETWEEN 1994–1998				
Persons who are poor [CEN]	53.1%	52.7%	52.6%	52.8%
Persons above 200% of the poverty line [CEN]	24.8%	24.9%	24.9%	24.9%
Minority [CEN]	91.2%	91.6%	91.5%	91.4%
Employed, civilian [CEN]	74.2%	73.9%	74.3%	74.1%
College graduates [CEN]	10.3%	10.1%	10.6%	10.3%
More than high school education [CEN]	22.1%	21.7%	22.3%	22.0%
Single female-headed households [CEN]	63.4%	63.1%	63.3%	63.3%
Households on public assistance [CEN]	33.4%	33.8%	33.1%	33.4%
BASELINE TRACT CHARACTERISTICS MEASURED USING AMERICAN COMMUNITY SURVEY DATA (2005–09)				
Persons who are poor [CEN]	41.5%	41.5%	42.1%	41.7%
Persons above 200% of the poverty line [CEN]	28.5%	28.7%	28.2%	28.5%
Minority [CEN]	90.4%	90.5%	91.0%	90.6%
Employed, civilian [CEN]	79.5%	79.9%	79.6%	79.7%
College graduates [CEN]	18.6%	18.2%	18.6%	18.4%
More than high school education [CEN]	34.2%	33.7%	34.2%	34.0%
Single female-headed households [CEN]	60.0%	59.4%	61.0%	60.1%
Households on public assistance [CEN]	14.0%	13.6%	13.5%	13.7%
NEIGHBORHOOD SAFETY				
Any household member was a victim of crime in past 6 months [SR]	41.6%	43.4%	41.4%	42.2%
Streets are very unsafe at night [SR]	51.2%	49.3%	51.7%	50.6%
MOTIVATIONS FOR SIGNING UP TO MOVE THROUGH MTO				
PRIMARY OR SECONDARY REASON FOR MOVING WAS…				
To get away from drugs or gangs [SR]	77.9%	78.6%	74.9%	77.3%
For better schools for the children [SR]	48.1%	49.1%	55.3%	50.5%
To get a bigger or better apartment [SR]	45.7%	44.1%	43.8%	44.5%
HOPE VI PROJECT				
Housing project has been demolished through HOPE VI or another housing initiative [HUD]	41.6%	39.5%	44.8%	41.6%
INTERVIEWED N	1,139	1,456	678	3,273

Notes: Percentages are percent distributions and may not sum to 100 because of rounding and missing information. Data are weighted to reflect randomization ratios and the selection of adults into the interview sample and the two-phase sampling design of the long-term evaluation.

Square brackets indicate the source of the outcome information: SR = self-report from the MTO Participant Baseline Survey, CEN = 1990 and 2000 decennial census data as well as the 2005–09 American Community Survey, HUD = Administrative data on demolished housing projects through 2009.

Data source and sample: MTO Participant Baseline Survey. Adult long-term survey. All adults interviewed.

Measures: Baseline census tract is based on participant's address at baseline. Interpolated census tract characteristics are linearly interpolated from the 1990 and 2000 decennial census.

The third panel of Exhibit 2.1 shows the characteristics of the same baseline census tracts but measured more recently using the 2005 to 2009 American Community Survey's (ACS) 5-year averages. Comparing the demographic composition of these tracts at baseline (interpolated to the date families enrolled in the program, between 1994 and 1998, using 1990 and 2000 census data) with the 2005 to 2009 demographics of these tracts suggests that, over time, these tracts have become slightly less poor and have higher proportions of college-educated residents. The share of residents who are members of a racial or ethnic minority group has remained about the same over time in these census tracts.

The baseline neighborhoods in which families were living were also quite dangerous. Exhibit 2.1 shows that 42 percent of MTO household heads reported that someone in the home had been the victim of a crime during the six months prior to the survey. Around 50 percent reported that the streets in their neighborhood were unsafe at night. Perhaps not surprisingly, 77 percent said that "getting away from drugs and gangs" was the first or second most important reason for wanting to move. The next most common reason for wanting to move was better schools (approximately 50 percent reported this as the first or second most important reason for enrolling), followed by wanting a bigger or better apartment (approximately 45 percent said this was the first or second reason).

As MTO was taking place, the urban landscape itself was shifting. The overall share of people who were poor in the United States declined between 1990 and 2000, but then increased again slightly during the 2000s (Anderson, 2011; Bishaw and Macartney, 2010; Bishaw and Renwick, 2011). The geographic concentration of low-income people (that is, residential segregation by income) was increasing from 1970 through 2000, the last year for which decennial census data are available (Watson, 2009). At the time of this writing, trends beyond 2000 were unavailable, but there are reasons to suspect that residential segregation by income increased further.

Throughout the MTO study period, rents rose faster than wages for low-skilled people in metropolitan areas nationwide. The stock of housing units with low to moderate rents declined, both because the overall rent distribution shifted and because units were removed from the stock (demolition, conversion to for-sale status, or landlords opting out of the voucher programs) faster than affordable replacement units were built (U.S. Department of Housing and Urban Development, 1997, 2007). The cities where MTO families are living are particularly expensive. The majority of low-income renters in the five MTO sites—ranging from nearly 74 percent of low-income renters in California to 64 percent in Massachusetts—spend more than 30 percent of their income on rent, which is a commonly-used definition of "rent burdened" (see Center on Budget and Policy Priorities, 2011). More than one-third of low-income renters in these same states spend more than 50 percent of their income on rent ("severely rent burdened").

Housing markets in Boston, Chicago, Los Angeles, and New York were strongly affected by the recent housing market collapse. The resulting foreclosure of rental properties, tenant evictions, and the flood of new renters who were former homeowners appear to have increased competition in the rental market (Briggs, Popkin, and Goering, 2010). Increasing poverty rates during the 2000s presented further challenges to low-income families. The net result of these two trends is that between 2007 and 2009, the number of low-income renters who devoted more than 50 percent of their income to rent rose 20 percent, from 5.91 million to 7.10 million renters (U.S. Department of Housing and Urban Development, 2011).

Housing policies themselves changed in important ways as well. Cities demolished and redeveloped many of the most severely distressed public housing developments, mostly under the HOPE VI program operated by HUD. Some of the public housing projects in which MTO families were living at baseline were among those demolished as part of HOPE VI and other local and federal housing initiatives. The share of the MTO sample members from all three groups who at baseline were living in housing projects that were eventually demolished varied from 91.5 percent in Chicago to 6 percent in New York City. Overall, approximately 42 percent of the sample was living at baseline in a project that was eventually affected by a demolition program.

Families living in projects targeted for demolition were typically either relocated to other public housing properties or offered housing vouchers similar to those that were offered to the Section 8 group. Most families who relocated through these programs seem to now be in better housing and safer neighborhoods (see Popkin et al., 2004, for a review). Only a small share of families returned to the redeveloped projects, but those who did now live in dramatically better housing and lower-poverty environments (Turner and Kingsley, 2008).

Even housing projects untouched by HOPE VI have seen improvements in housing and neighborhood conditions over the last decade, owing to changes in HUD policy that encouraged income mixing and the exclusion of residents with criminal records. Crime rates in the United States also declined dramatically during the 1990s with the largest proportional drops in crime often coming in the highest-crime places including places like the baseline housing projects from which MTO families were drawn (Harcourt and Ludwig, 2006; Cook and Laub, 2002; and Levitt, 2004).

These demographic, housing market, and policy changes may have influenced the impacts of the MTO demonstration on housing and neighborhood conditions by changing the housing and neighborhood options available to families in the two treatment groups, and by changing the conditions that control group families experienced. For example, HOPE VI may have led to more mobility among control group families than would otherwise have been expected of public housing residents. This does not bias our estimates, given that the experience of the control group is still an accurate picture of what would have happened to families assigned to the treatment groups in the absence of MTO. But it is important to bear in mind that we are comparing treatment families with a set of control families who were likely affected by substantial changes in housing policy. Although housing-market-induced changes in housing units available to voucher holders do not bias our comparisons, they are relevant in thinking about the conditions under which we would expect to see similar impacts from receiving housing vouchers.

2.2 HYPOTHESES ABOUT EFFECTS ON MOBILITY, HOUSING, NEIGHBORHOODS, AND SOCIAL NETWORKS

This section reviews the different pathways through which the MTO offer of a housing voucher might influence the housing and neighborhood conditions of families in the demonstration. Subsequent chapters of this report each include some discussion of the different theoretical pathways through which the housing and neighborhood changes described in this chapter might lead to changes in health, earnings, or other outcomes.

In thinking about the potential effects of housing vouchers on housing and neighborhood conditions, it is crucial to keep in mind that the MTO study sample consists of families who were all living in public housing at baseline. Setting aside for the moment the changes described above in other housing policies over the course of the study period, the MTO experiment essentially compares the effects of being offered a housing voucher against the chance to stay in public housing. This is a very different intervention or "treatment" from what is examined by the HUD Welfare to Work (WtW) voucher study (Mills et al., 2006) or the Chicago housing voucher study by Jacob and Ludwig (2011), which compare the effects of being offered a voucher versus living in the private-housing market without a subsidy. We return to the issue of how MTO compares with other voucher experiments at the end of this chapter.

HOUSING UNIT QUALITY
Whether the MTO comparison of housing vouchers versus public housing should have any impact at all on housing-unit quality is unclear, given that both the public housing and housing voucher programs share the same ostensible goal articulated by Title II of the Housing Act of 1949, to ensure "a decent home and suitable living environment for every American family." If both public housing and housing vouchers provided families with "a decent home," then MTO vouchers could in principle have no net effect on housing unit quality.

Any MTO impact on housing-unit quality will depend on the specifics of how public housing and housing voucher programs are implemented in practice. Some economists worry that the lack of financial incentives for government housing authorities to operate public housing projects efficiently could lead them to provide low-quality housing units relative to private-market housing (Olsen, 2003; see also the historical account of Chicago public housing by Hunt, 2009). On the other hand, the quality of the nation's public housing stock may be improving over time (Fischer and Sard, 2008). The private housing market could itself also be operating inefficiently because of regulatory and other constraints on the available supply of low-income housing, or because the high costs of moving reduce the competitive pressure on landlords to provide high-quality housing. The degree to which housing vouchers actually improve the unit quality of housing for families will then depend in part on the degree of enforcement in the study sites of housing quality standards and inspections, which are required as part of the standard housing voucher program.

MOBILITY

If people respond to incentives, at least in the aggregate as standard rational choice theory predicts, then MTO should increase the number of moves that families make in the short-term. Families in public housing or other government-assisted housing projects receive large rental subsidies that help reduce their housing-cost burden, but only if they agree to live in the public housing unit offered to them. Normally, if a family in public housing would like to move to a different apartment or neighborhood, their only option would be to move into a private-market housing unit and give up their public housing subsidy.[2] We might therefore expect somewhat higher mobility rates among the MTO control families than among more representative samples of low-income families. MTO housing vouchers relaxed the location constraints that families faced in exchange for receiving a rental subsidy. Put differently, families who are offered an MTO housing voucher are able to move to different units

and neighborhoods without having to give up a large housing subsidy. Therefore, an MTO housing voucher substantially reduces the costs to families of relocating. And economic models predict that a lower cost of moving should lead families to make more moves.

The net effect of MTO on residential mobility rates over the longer term could be different from the program's short-term mobility effects. In the short-term, MTO could increase the number of "good" (voluntary) moves made by families in the experimental and Section 8 groups. Over time, control group families may enter into the private housing market on their own even without receiving a housing voucher subsidy. Control group families thereby could be placed at increased risk for housing instability if income fluctuations make them unable to meet monthly rent or utility expenses without a subsidy and if private-market landlords are quicker to evict families for nonpayment than are public housing authorities. The result could be additional involuntary, or "bad," moves by the control group, serving to offset the higher initial mobility of the experimental and Section 8 groups.[3]

HOMELESSNESS

The prospect of housing instability among control group families who move into the private housing market without a subsidy also raises the prospect that MTO vouchers could reduce homelessness. But even the net effect of MTO on homelessness is ambiguous from the perspective of social science theory. It could be the very fact of relying on the private housing market that puts a family at elevated risk for homelessness, regardless of whether the family has a voucher subsidy, because of the increased risk of landlord difficulties or sale or foreclosure of their rental property. Given that, by design, a larger share of the experimental and Section 8 groups versus controls will wind up in the private housing market, MTO could increase rather than decrease homelessness.

---

2 In principle some public housing families might be able to relocate to a different public housing project located in a different area, or they might be able to access a housing voucher through some other voucher program besides MTO. In practice in most cities there are long wait-lists for both public housing and housing voucher subsidies.

3 Previous research shows that, in general, American families tend to move quite a bit. Among black and Hispanic households living in poverty, fully 23 percent change residences each year (Schachter, 2004). Most of these moves are local moves to similar types of neighborhoods; poor families in particular are likely to move from one poor neighborhood to another (Crowder and South, 2005).

NEIGHBORHOOD ENVIRONMENTS

By relaxing the locational constraint that public housing families face to receive a housing subsidy, MTO vouchers should also increase the chances that families wind up living in lower-poverty neighborhoods. Public housing offers families limited choice over where they live. In exchange for a large housing subsidy, families must live in whatever public housing units are offered by the local housing authority. Given the politics of how local housing authorities first selected public housing locations, and given that the design and tenant selection policies often created high concentrations of poor families, many housing project residents wind up living in very poor, racially segregated neighborhoods (Olsen, 2003; Hunt, 2009; Schill and Wachter, 1995). Housing vouchers enable public-housing families to move into lower-poverty neighborhoods without having to give up their subsidy, and as such they reduce the costs of changing their neighborhood conditions.

The relatively greater locational flexibility afforded to voucher families has its limits, however. Many housing units will be unaffordable to voucher holders.[4] Affordable housing units will be even more difficult to find in low-poverty areas, given that standard economic theory predicts that any amenity associated with a housing unit—including the "quality" of the surrounding neighborhood—should be reflected in relatively higher monthly rents. For a given rent level, families must trade housing unit quality for neighborhood quality.

We expect the constraint that MTO imposes on experimental group families, which limits them to using their housing voucher in a low-poverty area at least initially, to lead to more pronounced changes in tract poverty for families who move with an MTO experimental group voucher compared with those who move with a Section 8 group voucher. Even though experimental group families could move out of low-

poverty census tracts after their initial one-year lease was up, MTO designers hoped that many experimental group families would end up liking their new areas and choose to stay. The costs of moving might also contribute to the "stickiness" of experimental group voucher holders in their initial low-poverty census tracts. Consistent with the hope of MTO program designers, previous research of families in the Gautreaux mobility program in Chicago found that many families who were initially assigned to relocate to low-poverty, mostly white Chicago suburbs wound up staying there (Keels et al., 2005).[5]

We expect MTO moves to lead to changes in a wide range of other neighborhood attributes that families experience as well, given the correlation between neighborhood poverty and a variety of other neighborhood attributes such as prevalence of high school dropout and joblessness, crime, and quality of local public services (Briggs, 2008). The one exception might be neighborhood racial composition, if the mostly minority program population in MTO has a preference for living in mostly minority neighborhoods, or encounters racial discrimination in the housing market. Another potential exception could be access to public transportation if higher-poverty inner-city neighborhoods are closer to train and bus lines than lower-poverty areas that may be concentrated more along the periphery of the demonstration cities.

SOCIAL ISOLATION

A large body of research in social psychology and sociology has documented a tendency of people to associate with other people who share the same social characteristics or preferences, known as "homophily" (see, for example, McPherson, Smith-Lovin, and Cook, 2001). One potential concern with MTO is that the families who moved with a housing voucher might wind up feeling socially isolated in their new low-poverty areas if the moves reduce their access to their baseline-neighborhood social ties. At the same time, families can have difficulty establishing new connections with their new, more affluent neighbors (see, for example,

---

4 The voucher subsidy amount is essentially equal to something like the difference between the HUD-defined "fair market rent," often set between the 40th and 50th percentile of the metropolitan area's rent distribution, and 30 percent of the family's adjusted income (total income minus some deductions allowed by the program rules; see Olsen, 2003). Voucher program rules limit the ability of families to spend their own money to live in units with rents that are much above the fair market rent, which means that many housing units will be unaffordable to voucher holders.

---

5 Keels (2008) found remarkable persistence in low rates of neighborhood poverty among Gautreaux children who are now adults, although many Gautreaux children have moved on to more racially balanced neighborhoods.

Briggs, 1998). The hope would be that, at the very least, experimental and Section 8 group movers would manage to make new social connections over time as they acculturate into their new neighborhood environments (Briggs, Popkin, and Goering, 2010). The risk of social isolation for MTO movers could be declining over our study period as technological advances have reduced the costs of staying in close contact with the social networks from their origin neighborhoods.

2.3 DATA SOURCES AND MEASURES

From June 2008 through April 2010, we carried out long-term follow-up surveys with adults and up to three youth from each family who were between 10 and 20 years old at the end of 2007. These follow-up surveys capture outcomes on average 12 years after baseline (range from 10 to 15 years). We collected and geocoded address histories and linked each address to census tract characteristics, and also asked survey interviewers to record their observations of the neighborhoods in which families were living. In what follows we discuss each of these data sources and measures in detail.

PARTICIPANT ADDRESS HISTORY

Any effort to understand the neighborhood conditions requires that we reconstruct each family's residential history from random assignment onward. Our strategy was to assemble a best guess of the family's residential history from administrative records and previous canvasses and surveys of MTO families and then ask MTO adults to confirm or correct their full history, beginning with their current address and moving back in time to the point of random assignment. We also asked adults to indicate where they stayed during any gaps in their residential history in order to measure spells of homelessness, as described in more detail below.

In constructing the histories, we drew from administrative data from housing agency data used to track families receiving any form of housing assistance. These include several HUD data systems, including the Public and Indian Housing Information Center (PIC), the Tenant Rental Assistance Certification System (TRACS), and the Multifamily Tenant Characteristics System (MTCS). In addition, we accessed the U.S. Postal Service's National Change of Address (NCOA) system.

We supplemented these sources with other tracking activities that HUD supported over the course of the study period, including periodic canvasses of families by Abt Associates, and the interim survey that Abt, together with several members of our current National Bureau of Economic Research (NBER) research team, carried out four to seven years after baseline (see Orr et al., 2003, for more details).

We used the reconciled administrative and survey-based address histories to construct a "spell" file that incorporated information on the amount of time that MTO families spent at different addresses. Our information about the sequence of families' addresses is probably more accurate than our data on the specific amounts of time that families spent at each address, given that it is difficult for people to accurately recall the timing of episodic events (Bound, Brown, and Mathiowetz, 2001). In cases of conflicting information about where a family was living at a given point in time, we prioritized address information that came from actual contact with the family. We built on the algorithm that Abt Associates developed for the interim MTO study to determine which addresses were reliable and which dates should be associated with each address.

We created address histories for youth using previously collected administrative and survey data and asking adult survey respondents about whether the youth had ever lived apart from the adult (and if so, when). If the adult reported that the youth had always lived with him or her, or that the youth had lived away just once and for less than one year, we applied all of the addresses the adult supplied during the final survey to the youth (in addition to the other addresses we already had for the youth). If, however, the youth had lived away from the adult for more than one year or more than once, or if the youth did not live with the adult at the long-term survey, then we only applied the adult's address history to the estimated point when the youth was no longer living with the adult.

HOUSING TENURE

Tenure refers to whether the respondent occupies housing as a renter, as an owner, is living doubled-up with others, or is homeless. We asked adult heads of households

whether they presently owned, rented, lived with family or friends, or if they lived in a group shelter. A small number of people did not fit into any of these categories and specified another living situation. We collected information on the top reason for moving out of the second most recent address as well as reasons for moving into the current neighborhood.

If respondents did not have their own place to stay, we determined the exact type and duration of homelessness. Respondents who reported staying with a friend or a relative were classified as "doubled-up." We classified respondents as "literally homeless" if they did not have a place to stay and were not living in a hotel or doubled-up but were instead living at a shelter, on the street, in an abandoned building, in a car or van, or other similar circumstance. Respondents who reported staying at a hotel or motel were not classified in either of those categories. This question also allowed for open-ended answers. Many of the open-ended answers were later coded as "doubled-up" or "literally homeless." There are five final outcome variables reported in the analysis. Respondents are flagged as "doubled-up" or "literally homeless" on the basis of whether they fell into the respective category at least once after randomization. They were "without a home of their own" if they were flagged as "doubled-up" or "literally homeless." "Number of literally homeless spells" and "number of doubled-up spells" are the counts of respective spells that respondents reported.

HOUSING STABILITY AND UTILITY PAYMENTS

The adult survey also asked current renters if they had been more than 15 days late in paying their rent in the past 12 months and if they had been threatened with eviction in the past 12 months. Current homeowners were asked if they were more than 15 days late paying their mortgage in the past 12 months and if they had

been threatened with foreclosure.[6] To measure the stability of households' utility service, we asked renters who paid utilities if they were 15 days late in paying their bill for gas, electricity, or water in any month during the past year.[7] Of renters who were late at least one month, we asked if they received a notice that their utilities would be shut off and also if their utilities were in fact shut off.

HOUSING CONDITIONS

Adult household heads reported the conditions of their current homes or apartments, including an overall assessment of their housing and whether a set of specific problems was big or small, or no problem at all. The list of potential housing problems that we asked about comes from the American Housing Survey (U.S. Census Bureau, 2004) and included different types of pests, poorly maintained paint or plaster, and broken heating or cooling systems. In addition, we created a measure of the number of people living in the household divided by the number of rooms in the home (excluding bathrooms and kitchens). We defined a situation as overcrowded if there was more than one person per room, and severely overcrowded if there were more than 1.5 people per room.[8]

HOUSING COSTS AND ASSISTANCE

Housing assistance refers to any financial subsidy for rental costs, such as whether the sample member received help in paying rent through one of the federal housing assistance programs. At the study's outset, every participant was receiving housing assistance. At the time of the long-term survey, we used a multistep "triangulation" method developed by the Urban Institute that compares two types of administrative data against

6 Our survey questions on evictions and foreclosures focused on the type of housing situation that the respondent was currently living in. Thus, current renters were asked about evictions and current homeowners were asked about foreclosures. Under this survey design, we cannot identify a current renter who may have had his or her home foreclosed on during the past 12 months. Thus, the eviction and foreclosure questions can most easily be thought of as reflecting housing stability at respondent's current address, but it also allows respondents to report occurrences at other addresses with the same type of housing situation in the past 12 months.

7 Like renters, homeowners can also be late on their utilities payments. However, because of a skip error in the survey instrument, we did not ask homeowners if they were late on their utility payments.

8 For more information on measures of overcrowding, see Blake, Kellerson, and Simic (2007).

the MTO survey responses. The results slot families into the following categories: public housing; tenant-based federal rental assistance; project-based non-public-housing federal rental assistance; and no federal rental assistance (including owners, unassisted renters, the homeless, and those with other statuses).[9]

Housing cost burden is the ratio of monthly housing costs (of current rent or mortgage plus utilities) to household monthly income (primarily based on reports of last year's income). Households are cost-burdened if they devote more than 30 percent of their income to housing costs. They are severely cost-burdened if the ratio exceeds 50 percent. We assessed monthly housing costs from survey data on adults' rent or mortgage payments and their most recent gas and electric bills. If a respondent did not know or refused to provide the amount of his or her utility bills, we used an average for that site and treatment group.[10] Although the survey did not directly ascertain current monthly income, we were able to estimate it using survey data on the components of the previous year's household income, adjusted for changes in employment status and receipt of government aid. For example, if a respondent had earnings the prior year but was currently not working, we excluded his or her earnings from our income estimate.

NEIGHBORHOOD CENSUS TRACT CHARACTERISTICS

We measured the sociodemographic composition of the "neighborhoods" in which MTO families were living by linking their address to census tract-level data from the Census Bureau. As the Census Bureau notes, census tracts "usually have between 2,500 and 8,000 persons and, when first delineated, are designed to be homogeneous with respect to population characteristics, economic status, and living conditions."[11] The census tract characteristics we examined included median income, poverty and thresholds of poverty, household composition, homeownership, race, ethnicity, employment, occupation, and education.

The tract-level data we used are from the 1990 and 2000 decennial censuses, together with data from the 2005–09 ACS. The ACS was designed to replace the census long form, which was discontinued after 2000, although the ACS has a smaller sample. The ACS contains many of the same questions as the census long form, although some variables are measured slightly differently between the two data sources.[12] We can estimate census tract characteristics at a specific point in time assuming that tract characteristics changed in a constant way in between census data collection points, that is, by assuming a

---

9 First, we use respondents' self-reports to determine each of eight possible housing assistance categories: renter with tenant-based assistance (TBA renter); renter in public housing (public housing); renter with project-based assistance (PBA renter); renter without housing assistance (unassisted renter); homeowner; homeless individual; individuals who live with family or friends and do not pay rent (doubled up); and individuals with another housing arrangement (other). Next, we analyze administrative data (PIC/MTCS and TRACS/MultiFamily) separately to determine the housing assistance status for any head of household in the administrative file. If the status in the administrative data coincides with that in the survey data, it is assigned to the respondent. For respondents whose statuses are not identical, we match (based on ZIP+4 location) against a set of locations of public and project-based housing obtained from HUD. If the status in HUD data is the same as in survey or administrative data, then we use that status. If no match is found, we use the administrative data status when the administrative records match the respondent with certainty; otherwise, we use the survey data status. Fully 86 percent of the survey analysis results agree with the administrative analysis results and 14 percent disagree. Of those that disagree, results for 3.6 percent of the respondents disagree on type of federal housing assistance, not the fact that they had federal housing assistance at all. Thus, receipt of housing assistance is in agreement for 89.5 percent of the respondents. Both rates are much higher than the 78 percent agreement from the interim survey.

10 Reported utility costs in the MTO survey data are higher than documented average utility costs in comparable locations over a relatively similar time period. We find that MTO's effects on the housing burden do not qualitatively differ under a variety of sensitivity checks that impose caps or minimum values on utility costs.

---

11 www.census.gov/geo/www/cen_tract.html, Accessed April 16, 2011.

12 For example, Census 2000 asked for income in the previous calendar year whereas the ACS asks for income over the previous 12 months. Estimates relying on measures of income in the last 12 months may be slightly lower on average than measures of income from the prior calendar year. (See, for example, Beaghan and Stern, 2009, available at www.amstat.org/sections/srms/Proceedings/y2009/Files/303932.pdf). This difference in reported income could contribute to differences in poverty measures for certain neighborhoods. In addition, the ACS and Census define residency in different ways. In the ACS, someone is counted as living in a census block if he or she presently resides there. However, for the census, people are only counted as presently residing if they "usually live" there. This may change the population of "residents" in areas with significant populations that are transitory, such as areas with colleges, or communities with many summer homes. The Census Bureau in 2000 changed its questions on race and ethnicity to allow for identification of multiple races. Another significant change is how the Census defined professions and careers, which changed with revisions to the Standard Occupational Classification and to the North American Industry Classification System. As a result, many of the major occupation groups in Census 2000 are not comparable to earlier censuses. For example, people who work for travel agencies are classified as part of the transportation industry in the 1990 census but are classified as "administrative, support, and waste management" for the 2000 census. (See www.census.gov/hhes/www/ioindex/pdfio/techpaper2000.pdf).

linear trend for the evolution of tract characteristics.[13] We present census tract characteristics for different points in time after random assignment for families, as well as the average duration-weighted tract characteristics of all the addresses that families lived at from the time of random assignment until just before the start of survey fielding (through May 31, 2008). We selected the start of survey fielding in order to have a common endpoint for all families who were interviewed at different points in time from June 2008 through April 2010. We duration-weighted the characteristics of each census tract in which a family lived during this time period by the number of days they lived at each address. This ensures that addresses where they lived longer count more heavily in the average.

NEIGHBORHOOD SATISFACTION AND CHARACTERISTICS

To gauge satisfaction with their neighborhoods of residence, we asked adults and youth to describe how much of a problem they have in their neighborhoods with things like litter or trash on the streets or sidewalks, graffiti or writing on walls, abandoned buildings, people drinking in public, and groups of people hanging out in their neighborhood. As a measure of access to neighborhood essentials (for example, shopping and parks) and employment, respondents also reported if they owned a working car or if they lived fewer than 15 minutes from public transportation.

NEIGHBORHOOD SOCIAL PROCESSES

Social science research has increasingly focused on the role of neighborhood social processes in individual outcomes, above and beyond the role of neighborhood structural characteristics such as social and demographic composition (see, for example, Sampson, Morenoff, and Gannon-Rowley, 2002). Our long-term survey asked adults to report on social processes such as "collective efficacy," defined by Sampson, Raudenbush, and Earls (1997) as the willingness of local adults to work together to enforce shared norms. Specifically, we asked

_____

13 For this purpose, we assumed that the ACS characteristics represent the midpoint of fielding of the ACS, which is July 1, 2007. ACS fielding occurred continuously from 2005 to 2009. It is possible that characteristics in each census tract changed in a nonlinear way from the time of the ACS to the time of the interview, particularly because of the most recent recession.

respondents to describe how likely it was (very likely, likely, unsure, unlikely, or very unlikely) that their neighbors would act if they saw a group of children or youth skipping school and "hanging out on the street," or if they saw youth spray-painting graffiti on local buildings.

As a measure of the strength and breadth of available social support, we also asked respondents about their close friends using modified questions from the Social Capital Community Benchmark Survey (Saguaro Seminar, 2002). Interviewers asked adults to report how many close friends they had and how many of them (all, most, some, a few, or none) were of a different race or ethnicity than the respondent. Additional questions asked how many of their close friends had college degrees or were employed full-time.

FAMILY AND NEIGHBORHOOD SAFETY

As noted above, one of the key reasons MTO families signed up for the demonstration was their desire to live in safer neighborhoods. A greater sense of safety can affect a family's quality of life and behaviors in a variety of ways, including direct effects on mental health, physical health outcomes (for example, because of changes in the willingness to walk around in a neighborhood), and economic outcomes (for example, increased willingness to use public transportation to commute to jobs, or to leave children unattended or in the company of other caregivers after school).

We asked respondents how safe they felt in the streets around their neighborhood during the day and at night (very safe, safe, unsafe, or very unsafe). Interviewers also asked about how responsive the local police were to calls (police not coming when called is a big problem, small problem, or no problem) and whether the respondent saw people selling or using illegal drugs in the past 30 days. As a measure of actual crime victimization experiences, the survey asked if any household members had been victims of crime (purse, wallet, or jewelry was snatched; threatened with a knife or gun; beaten up or assaulted; was stabbed or shot; and/or there was a break-in/attempted break-in to their house) in the past six months.

We asked youth about the prevalence of gangs in their neighborhood or by their school. This question is modified from the National Longitudinal Survey of Youth 1997 (Moore et al., 1999) and aims to measure the possibility of gang incidences in the neighborhood.

Previous studies have found that female youth in high-poverty urban areas may be affected (mentally and in overall well-being) by sexual harassment. In an effort to understand the level and type of such harassment that female participants in MTO might have experienced, we asked questions drawn from the Maryland Adolescent Development in Context Study (Goldstein, Davis-Kean, and Eccles, 2005) about how often (never, a couple of times each year, one or more times a month, once or twice a week, or every day) they faced unwanted or rude comments, unwanted sexual attention, or were afraid to go places because of unwanted attention or pressure.

INTERVIEWER OBSERVATIONS OF THE NEIGHBORHOOD BLOCK

In principle, different people might view conditions in the same neighborhood differently, and so ideally we would obtain reports about neighborhood conditions from other neighborhood residents as well, as was done in the Project on Human Development in Chicago Neighborhoods (PHDCN; Sampson, Raudenbush, and Earls, 1997; Raudenbush and Sampson, 1999). This was not feasible in the MTO study, so instead we asked interviewers to complete a checklist about different attributes of the respondent's block (defined as both sides of the street that the respondent's house is on or faces, from one street corner to the other). Most of the observations were gathered in a walking tour, although some were completed by driving around the block.[14]

Specifically, the interviewers reported on the condition of most of the buildings on the block, as well as the existence of window bars and any graffiti that was painted over. They also noted the types of institutions

---

14 For PHDCN, interviewers walked all around the block, while for logistical reasons, MTO interviewers only walked up and down the street (block face) on which the MTO respondent was living. In some unpublished analyses that were carried out for us by Michael Bader, a graduate student at the time at the University of Michigan, we find the correlations for variables that come from observing a single block-face versus walking around the entire block typically ranged from .5 to .8, depending on the measure.

and buildings on the block (storefront church, abandoned/burned buildings, boarded up housing or building, green grocer/delicatessen, supermarket/grocery store, fast-food and take-out place, or none of the above). In addition to buildings, interviewers rated the condition of the streets on the block (very good: recent resurfacing and smooth; moderate: evidence of keeping in good repair; fair: minor repairs needed but not rough surface; and poor: potholes and other evidence of neglect). They reported on the amount of garbage (including litter or broken glass) and density of trees on the street. Finally, interviewers noted the types (if any) of signage (neighborhood or crime watch, security warnings, advertising for beer/whiskey/other alcohol, "for sale" signs, or none of the above).

ADULT SOCIAL NETWORKS AND CIVIC ENGAGEMENT

We asked respondents a series of questions to determine social connectedness, such as the number of friends the adults have in their neighborhood and how many times they visit their friends (whether or not they are living in the same neighborhood) at their homes (every day, several days a week, twice a week, once a week, two or three times in the past month, once in the past month, or not at all in the past month). To gain a sense of the respondent's community involvement, we asked how often they chat with their neighbors (almost every day, once a week, once a month, a few times a year, or almost never) and go to church or religious services (never in the past 12 months, several times in the past 12 months, once a month, once a week, more than once a week, or no church/place of worship).

YOUTH SOCIAL NETWORKS AND SELF-ESTEEM

The MTO youth survey draws on questions from National Education Longitudinal Study of 1988 (NELS) (Ingels et al., 1994) and National Comorbidity Survey (Kessler et al., 2009) and asks youth about their friends' involvement in school activities, use of illegal drugs, and whether they are school dropouts. The goal is to gain an understanding of their peers' views on studying and continuing education beyond high school (whether it is very important, somewhat important, or not important at all). MTO youth were additionally asked about the

number of close friends they have (none, one, two, three to five, six to ten, or more than ten), how many of their close friends live in their neighborhood, and whether these friendships are new or holdovers from their neighborhoods of origin. Another aspect of a youth's social connections can come from church or other religious activities, so we asked them about how often they participated in activities such as youth groups, Bible classes, or choir (once a week or more, once a month or more, less than once a month, or never). Using items from the Ferguson Tripod Study (Ferguson, 2010) and NELS, we asked youth about speaking "proper" English (even when they are with their friends) and how often people would describe their behavior as "ghetto" (very often, somewhat often, or not at all), as well as a battery of items about their perception of themselves and their future.[15]

2.4 LONG-TERM IMPACTS ON MOBILITY AND HOUSING

Of those families randomly assigned to the experimental group, 48 percent "complied" or moved with an MTO voucher. Of those assigned to the Section 8 group, the compliance rate is 63 percent.

Exhibits 2.2 through 2.5 present MTO's effects on the residential mobility patterns and housing assistance of program participants, as well as their housing conditions. These estimates are calculated using the methods described in Chapter 1 (Section 1.3). The intention to treat (ITT) estimates reflect the effects of being offered the chance to relocate with an MTO housing voucher. For example, the ITT estimate for the experimental treatment compares the average outcome for everyone assigned to the experimental group (regardless of whether they moved or not with an MTO voucher) with the average outcome for everyone assigned to the control group. The ITT estimate for the Section 8 treatment group is analogously defined. Mechanically, the ITT estimates are calculated by running an ordinary least

squares regression of the outcome of interest against dichotomous indicators for random assignment to either the experimental group or the Section 8 group, controlling for a set of baseline control variables to improve statistical precision and are calculated using sample weights that account for changes over time in random-assignment ratios and for the random selection of some families but not others into the survey sample frame (see Section 1.3 for details). The estimated effects of treatment on the treated (TOT) capture the effects of MTO moves on those who initially moved with an MTO voucher. For each MTO treatment group the TOT is equal to the ITT effect for that treatment group divided by the share of that group that moved initially with an MTO voucher.

Because the remainder of the report uses tables with a similar structure, we spend a moment here to review how the table is laid out. Each row presents the results of running a regression for the variable listed on the left-hand side of the row, which is the outcome variable in the regression, on the two treatment-group assignment dummy variables as well as the baseline covariates listed in Supplemental Exhibit 1.2. The first number in each row is the control mean for that variable, that is, the average value of the outcome for the control group, calculated using the sample weights described in Chapter 1. The coefficients on the treatment-group assignment variables together with their standard errors are listed under the ITT columns for the experimental and Section 8 treatments respectively. Next to the ITT estimates are the estimated effects of the TOT and accompanying standard errors for that treatment group.

The ITT estimates capture the effect of what is most directly under the control of policymakers—offering families the opportunity to relocate with an MTO voucher. The TOT estimate is of additional interest because the share of families that would relocate if offered an MTO-like voucher could differ across settings. The ITT estimates require no statistical assumptions other than that random assignment was carried out correctly. The TOT estimates require some additional assumptions, the most important of which is that assignment to either of the treatment groups has no effect on those who do not move through MTO (the

---

15 Respondents were also asked how much they agree with the following statements: I feel good about myself, I am able to do things as well as most other people, and chance and luck are very important for what happens in my life. Possible answers are strongly agree, agree, disagree, or strongly disagree. Finally, youth were asked how they think other people see them in terms of popularity, athleticism, and academically (possible responses were: never, sometimes, or all the time).

noncompliers). All of the tables that follow in the report show both the ITT and TOT effects, allowing readers to choose their preferred estimate. The text in most of the chapters focuses on the ITT effects. In this chapter, we devote additional discussion to the TOT effects as well, which are particularly relevant for thinking about the strength of MTO's "treatment dose" on the housing and neighborhood conditions of participating families.

Over the course of the study period, which follows families on average for 12 years after baseline, adults in the control group moved just over two (2.17) times on average (as shown in Exhibit 2.2). For many of control group households, at least one of these moves was triggered by the demolition of their initial public housing through HOPE VI or a related program. Nonetheless, the total number of moves we observe for MTO control group families over the study period is, if anything, slightly lower than what we would predict from national data, which indicate that around 23 percent of low-income African-American and Hispanic families move each year (Schachter, 2004). If 23 percent of control group families in MTO moved each year, then over the course of the 12-year follow-up period, we would expect 2.76 moves. Mobility rates might be somewhat lower for the MTO control group than for national samples given that, as noted above, control families may be relatively more tied in place by their project-based housing subsidies.

Assignment to either the experimental or Section 8 group (the ITT effect) increases the average number of moves for families by about .6 during the 10–15 year study period. Among those who actually moved through MTO, the TOT effect of the experimental treatment was 1.16 additional moves for the experimental group compliers and 1.01 additional moves for Section 8 compliers (both statistically significant at the standard 5 percent cut-off). Learning more about the impacts of the MTO intervention on distances moved by families is an important question for future research.

Perhaps the most striking finding in Exhibit 2.2 is that approximately 20 percent of control group families report having been without a home of their own (that is, doubled-up or literally homeless) at some time during the course of the study period, underscoring the extreme level of economic disadvantage of the study sample. Typically, control group families would deal with the problem by doubling up with another family (19 percent of control families did this at least once during the study period), although around 5 percent of control group families were literally homeless at some point. Most families who were homeless were homeless just once, as revealed by the close correspondence between the share of control families who had been homeless and the average number of homelessness spells among the control group. There were no statistically significant differences between the control and treatment groups in the likelihood of being literally homeless; however, the Section 8 group was more likely to report having been doubled-up than the control group (ITT effect of 4.5 percentage points). The lack of detectable MTO impacts on literal homelessness might initially seem contradictory with the findings from the Welfare to Work (WtW) voucher study, which found that housing vouchers reduced the risk of homelessness by around one-third of the control group mean (Mills et al., 2006). But it is important to keep in mind that the "treatment" studied in WtW—giving housing vouchers to families who were initially living in the private housing market without a subsidy—is quite different from that examined in MTO, which involved offering housing vouchers to families who already had a housing subsidy, in the form of a public housing or other project-based assisted housing unit. The importance of the different control group conditions can be seen in the much higher rate of homelessness among the (unsubsidized) control group in the WtW study. There, approximately one-quarter of respondents did not have a place to stay during the last year prior to the final survey, which is higher than the rate of homelessness that MTO control-group families experienced cumulatively over the entire 10–15-year follow-up period.

Exhibit 2.3 shows that at the time of the long-term survey, 85 percent of adults in the control group were living in rental housing (including both public housing or private market) and 12 percent had become homeowners, which is more than double the prevalence of homeownership reported at the interim study from four to seven years after baseline (Orr et al., 2003, Exhibit C3.2, p. C–16). Being in the Section 8 treatment

group increased the likelihood of renting (the ITT effect was 4 percentage points, the TOT effect was 6 percentage points, compared with a control mean of 85 percent) and reduced the likelihood of owning a home compared with the control group (ITT and TOT effects of – 4 and – 6 percentage points, respectively, compared with a control group homeownership rate of 12 percent). The effects of being assigned to the experimental group on renting and owning are qualitatively similar to those of the Section 8 group, but are not statistically significant at the 5 percent level.

Exhibit 2.3 also shows the reasons that survey respondents left their previous address and moved to their current neighborhood. These results are somewhat complicated to interpret because for most families their most recent move was a secondary move (see Exhibit 2.2). Therefore, for example, most experimental group families will have already made their initial MTO move into a safer neighborhood (discussed further below), and so their most recent move will use that safer address as the starting location. Among the control group, 16 percent of families reported that getting away from gangs or drugs was the most important reason they moved from their previous address. Those in the experimental group, in contrast, were nearly 3 percentage points less likely to say they left their most recent address for reasons of safety (the TOT effect is around 7 percentage points). Exhibit 2.3 also shows that when voucher-holders made their most recent move, they were more likely than control group members to have moved because of problems navigating the private housing market, such as landlord problems or their house or apartment was sold.

About 62 percent of the control group adults were receiving housing assistance at the time of the long-term follow-up. This estimate does not seem to be sensitive to whether we measure receipt of housing assistance from a combination of administrative and survey data, which is what we report in Exhibit 2.4 using the methods described above, or if we calculate it from only survey self-reports (not shown). Receipt of any sort of housing assistance did not differ statistically at the 5 percent level between the controls and the experimental group or between the controls and the Section 8 group. However, MTO did change the type of housing program in which families were participating. Of those in the control group who were receiving housing assistance, about one-half were currently in public housing (30 percent of the

total control group) and 40 percent were using a voucher or certificate (25 percent of the total control group). Another 7.2 percent of the total control group was receiving other forms of housing assistance, such as the Rental Assistance Payment and Moderate Rehabilitation program. Families assigned to the experimental and Section 8 groups were 16 and 19 percentage points more likely than controls to be receiving a housing voucher at the time of the long-term follow-up survey (the TOT effects for both groups are nearer 30 percentage points).

Exhibit 2.4 shows that monthly housing costs (rent or mortgage plus utilities) as reported by families in the long-term surveys averaged around $680 for controls. Exhibit 2.4 also shows that nearly 68 percent of control group adults had a housing-cost burden greater than 30 percent, in line with the national statistics cited above. Given that MTO did not have statistically significant effects on the likelihood of receiving any sort of housing assistance (public housing plus housing vouchers), it is perhaps not surprising that MTO had no detectable impacts on the amount of money that families spent out-of-pocket on housing or on housing burdens. However, the confidence intervals around these estimates do not allow us to rule out effects of MTO moves on housing costs that may be as large as $100 per month. We do find that, compared with the control group adults, those in the experimental group were less likely to report being late with their rent or mortgage payments, but they were more likely to have had trouble paying their utility bills on time while renting.

Although MTO may not have had detectable effects on housing costs for families, the program did improve the quality of housing that families found. Exhibit 2.5 shows that adults assigned to the experimental group were about 5 percentage points more likely than controls to report that their housing conditions were excellent or good (TOT effect of approximately 11 percentage points compared with 57 percent of control group adults). Adults in the two treatment groups also reported fewer specific problems with their housing units. For example, 52 percent of control group adults reported that their current housing units had problems with vermin. Adults in the experimental or Section 8 group were 6 and 9 percentage points less likely than controls to report having problems with vermin (TOT effects of 13 or 14 percentage points). Results for additional housing quality measures are shown in Supplemental Exhibit 2.1.

EXHIBIT 2.2. ADULT MOBILITY AND HOMELESSNESS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
MOBILITY						
Number of moves since Random Assignment [SR]	2.165	0.561* (0.073)	1.157* (0.151)	0.629* (0.096)	1.014* (0.155)	3,273
Leased an apartment using an MTO voucher	0.000	0.484* (0.014)		0.639* (0.022)		3,273
HOMELESSNESS AFTER RANDOM ASSIGNMENT						
Without a home of their own at least once ("doubled-up" or "literally homeless") [SR]	0.214	0.020 (0.017)	0.040 (0.036)	0.056* (0.024)	0.090* (0.039)	3,273
"Doubled-up" at least once (stayed with family or friends) [SR]	0.191	0.008 (0.017)	0.017 (0.034)	0.045* (0.023)	0.073* (0.037)	3,273
"Literally homeless" at least once [SR]	0.049	0.009 (0.009)	0.018 (0.019)	0.018 (0.014)	0.030 (0.022)	3,273
Number of times "doubled-up" [SR]	0.257	0.014 (0.026)	0.028 (0.053)	0.074* (0.034)	0.120* (0.056)	3,273
Number of times "literally homeless" [SR]	0.061	0.003 (0.012)	0.006 (0.025)	0.024 (0.020)	0.038 (0.032)	3,273

Notes: * = p < 0 .05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: "Doubled up" is defined as staying with friends or family when respondents were without a home of their own. "Literally homeless" is defined as staying at a shelter, on the street, abandoned building, car/van, movie theater/laundromat, and so on, when respondents were without a home of their own.

EXHIBIT 2.3. ADULT HOUSING TENURE AND REASONS FOR MOVING						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
CURRENT HOUSING TENURE						
Currently rents house or apartment [SR]	0.850	0.024	0.049	0.040*	0.064*	3,265
		(0.015)	(0.032)	(0.020)	(0.032)	
Currently owns house or apartment [SR]	0.120	− 0.024~	− 0.050~	− 0.039*	− 0.063*	3,265
		(0.014)	(0.029)	(0.018)	(0.029)	
Currently doubled-up and pays part of rent/ mortgage [SR]	0.015	− 0.003	− 0.006	− 0.001	− 0.002	3,266
		(0.005)	(0.010)	(0.008)	(0.012)	
Currently doubled-up and does NOT pay part of rent/mortgage [SR]	0.009	0.005	0.010	− 0.003	− 0.005	3,267
		(0.004)	(0.009)	(0.005)	(0.009)	
Currently lives in a group shelter [SR]	0.005	− 0.003	− 0.006	0.000	0.000	3,268
		(0.002)	(0.005)	(0.004)	(0.007)	
REASONS FOR MOVING FROM PREVIOUS ADDRESS						
To get a bigger or better apartment [SR]	0.259	− 0.058*	− 0.116*	− 0.034	− 0.054	3,168
		(0.018)	(0.037)	(0.024)	(0.038)	
To get away from drugs or gangs [SR]	0.159	− 0.033*	− 0.067*	− 0.051*	− 0.080*	3,168
		(0.015)	(0.030)	(0.020)	(0.031)	
Due to problems with landlord [SR]	0.073	0.022~	0.045~	0.022	0.035	3,168
		(0.012)	(0.025)	(0.015)	(0.024)	
Because house or apartment was sold [SR]	0.051	0.036*	0.074*	0.043*	0.068*	3,168
		(0.011)	(0.022)	(0.015)	(0.024)	
REASONS FOR MOVING TO CURRENT NEIGHBORHOOD						
For better or bigger living space [SR]	0.201	− 0.034*	− 0.071*	0.005	0.008	3,261
		(0.017)	(0.034)	(0.023)	(0.036)	
Because it is safer [SR]	0.156	0.010	0.020	0.036	0.057	3,261
		(0.016)	(0.033)	(0.022)	(0.035)	
Because unit is affordable [SR]	0.134	− 0.037*	− 0.076*	− 0.048*	− 0.076*	3,257
		(0.015)	(0.030)	(0.018)	(0.029)	
Because it has fewer problems with gangs or drugs [SR]	0.085	− 0.010	− 0.020	− 0.013	− 0.021	3,261
		(0.011)	(0.023)	(0.015)	(0.025)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

EXHIBIT 2.4. ADULT HOUSING ASSISTANCE, COSTS, AND STABILITY						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
CURRENT HOUSING ASSISTANCE						
Any housing assistance [SR, HA]	0.620	0.026	0.054	0.045~	0.072~	3,273
		(0.021)	(0.043)	(0.027)	(0.044)	
Currently in public housing [SR, HA]	0.296	− 0.107*	− 0.220*	− 0.110*	− 0.177*	3273
		(0.017)	(0.035)	(0.022)	(0.036)	
Receiving housing voucher or certificate [SR, HA]	0.252	0.159*	0.328*	0.194*	0.312*	3,273
		(0.019)	(0.040)	(0.026)	(0.042)	
Using other types of housing assistance [SR, HA]	0.072	− 0.026*	− 0.054*	− 0.039*	− 0.063*	3,273
		(0.010)	(0.020)	(0.013)	(0.020)	
CURRENT HOUSING COSTS						
Total housing cost (2009 dollars), monthly [SR]	$678.73	19.50	39.67	− 6.26	− 10.00	3,180
		(23.30)	(47.39)	(30.73)	(49.10)	
Cost of rent or mortgage (2009 dollars), monthly [SR]	$493.04	3.16	6.43	− 24.44	− 39.05	3,180
		(20.16)	(41.01)	(26.14)	(41.76)	
Household is housing burdened (monthly housing cost/income > 30%) [SR]	0.676	0.011	0.022	0.020	0.032	3,169
		(0.020)	(0.041)	(0.027)	(0.043)	
Household is severely housing burdened (monthly housing cost/income > 50%) [SR]	0.426	− 0.004	− 0.007	0.017	0.027	3,169
		(0.021)	(0.043)	(0.029)	(0.046)	
HOUSING STABILITY						
More than 15 days late in paying rent/ mortgage at least once during past 12 months [SR]	0.258	− 0.055*	− 0.112*	− 0.067*	− 0.106*	3,242
		(0.018)	(0.037)	(0.024)	(0.038)	
Received eviction/foreclosure threat due to nonpayment at least once during the past 12 months [SR]	0.124	− 0.017	− 0.035	− 0.033~	− 0.053~	3,261
		(0.014)	(0.028)	(0.018)	(0.028)	
LATE UTILITY PAYMENTS (RENTERS)						
Rents and was more than 15 days late in paying utilities at least once during the past 12 months [SR]	0.333	0.052*	0.108*	0.033	0.053	3,236
		(0.020)	(0.041)	(0.027)	(0.043)	
Rents and received shut-off notice due to nonpayment at least once in the past 12 months [SR]	0.235	0.054*	0.112*	0.050*	0.081*	3,236
		(0.018)	(0.038)	(0.025)	(0.040)	
Rents and utilities have been shut off for nonpayment at least once in past 12 months [SR]	0.052	0.021*	0.043*	0.016	0.026	3,236
		(0.010)	(0.022)	(0.014)	(0.022)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, HA = Housing Assistance data from HUD.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

EXHIBIT 2.5. ADULT HOUSING QUALITY	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
OVERALL RATING OF CURRENT HOUSING CONDITION						
Rates current housing as excellent or good [SR]	0.570	0.053*	0.109*	0.031	0.050	3,267
		(0.021)	(0.044)	(0.029)	(0.046)	
CURRENT HOUSING PROBLEMS						
Vermin (mice, rats, and/or cockroaches) is a big or small problem [SR]	0.517	− 0.063*	− 0.130*	− 0.088*	− 0.141*	3,266
		(0.021)	(0.043)	(0.028)	(0.045)	
Heating or plumbing that does not work is a big or small problem [SR]	0.374	− 0.028	− 0.059	− 0.052~	− 0.083~	3,258
		(0.020)	(0.042)	(0.027)	(0.043)	
Peeling paint or plaster is a big or small problem [SR]	0.466	− 0.091*	− 0.187*	− 0.120*	− 0.192*	3,265
		(0.021)	(0.044)	(0.028)	(0.044)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

2.5 LONG-TERM IMPACTS ON NEIGHBORHOOD CHARACTERISTICS

As Chapter 1 noted, the initially large differences between treatment and control groups in census tract poverty rates narrowed over time in large part because of declines in the control group's average tract poverty rate. Exhibit 2.6 shows the effects of MTO on the average (duration-weighted) census tract characteristics that families experienced during the entire 10- to 15-year study period. As noted above, we calculate each family's average tract characteristics by first estimating the characteristics of the census tract when the family was living there, using information from the 1990 and 2000 decennial census together with data from the 2005–09 ACS, and then average over all the family's addresses during the study period, weighting each address by the share of the study period the family spends at that address. (Results for the youth sample are presented in Supplemental Exhibit 2.2.)

The average poverty rate of the census tracts in which the control group lived over the study period was about 40 percent poor. For families assigned to the experimental and Section 8 groups, duration-weighted average tract poverty rates were 9 and 7 percentage points lower, respectively. The TOT effects of the experimental and Section 8 treatments on average tract poverty rates equaled 18 and 11 percentage points.

Exhibit 2.6 shows that MTO changed a variety of other measures of neighborhood advantage and disadvantage as well. For example, 54 percent of households in the census tracts where control group families lived were headed by females; the figure was 8 percentage points lower for those in the experimental group (and 16 percentage points lower for those in the experimental group who moved with an MTO voucher). Median household income was almost $19,000 higher in the census tracts where experimental group compliers lived, a sizable change compared with the average control group census tract median income of $27,800.

On the other hand, MTO moves had more modest impacts on neighborhood racial segregation. Among control group members, 88 percent of residents in their census tracts were members of racial and ethnic minority groups. The figure was slightly lower for those in the Section 8 group. Families in the experimental group

experienced a statistically significant decline in census tract minority share equal to around 6 percentage points (TOT effect of nearly 13 percentage points). However, these families were still living in census tracts that were overwhelmingly populated by minority residents.

Exhibit 2.7 reports results for MTO's impacts on the same set of census tract characteristics shown in Exhibit 2.6, but now measured at the point in time just before we started the long-term survey data collection (May 2008) rather than duration-weighted averages in Exhibit 2.6. Most of these MTO effects on neighborhood sociodemographic characteristics are still statistically significant, particularly in the experimental group. However, given the gradual convergence over time between the two treatment groups and controls in their average neighborhood conditions, the treatment-control differences in tract characteristics for each family's May 2008 address tend to be smaller in magnitude than the duration-weighted averages.

The MTO families themselves report being more satisfied with their neighborhoods at the time of the long-term survey (as was also the case in the interim survey data). Exhibit 2.8 shows that approximately 52 percent of adults in the control group said they were satisfied or very satisfied with their current neighborhood; the figure was 9 percentage points higher for adults in the experimental group (TOT effect of 19 percentage points), and 8 percentage points higher for adults in the Section 8 group (TOT effect of 13 percentage points). Adults in the experimental and Section 8 group also tended to report fewer signs of disorder than adults in the control group, particularly for problems like trash and graffiti (Exhibit 2.8). The interviewer observations for these neighborhoods tend to confirm the reports (Exhibit 2.9). However, MTO did not seem to have any detectable impact on access to private or public transportation (last row of Exhibit 2.8). (See also Supplemental Exhibit 2.3 for additional results.)

MTO also led to persistent improvements in neighborhood safety, as measured by self-reports in our long-term surveys (Exhibit 2.10). Adults in the experimental and Section 8 groups were around 4 percentage points more likely than controls to feel safe

in their neighborhoods during the day (TOT effects of around 7 percentage points), compared with a control mean of 80 percent. Adults in the experimental and Section 8 groups were 7 percentage points less likely than controls to say that police do not come when called compared with 42 percent of controls and were also less likely to see drugs sold or used in the neighborhood (TOT effects of 14 and 12 percentage points). Although MTO did not change feelings of safety among male youth, relocation did increase feelings of safety among female youth. MTO moves also reduced the exposure of female youth in the experimental group to unwanted sexual attention (Exhibit 2.10). The qualitative interviews with MTO families done at three of the MTO demonstration sites also found important impacts of MTO moves in reducing "female fear," the concern that many female youth have about sexual harassment, coercion and rape (Popkin, Leventhal and Weismann, 2010; see also Briggs, Popkin, and Goering, 2010). Despite the improved feelings of safety, MTO had no statistically significant effects on self-reported crime victimization rates in the long-term surveys among adults, or among all youth pooled together. These results differ from what was found in the interim survey data, which showed declines in crime victimization (Orr et al., 2003). Somewhat surprisingly, the experimental group female youth—unlike adults or male youth— reported lower levels of household crime victimization, which raises the possibility that female youth may not share information about their own crime victimization experiences with others in the home.

2.6 LONG-TERM IMPACTS ON SOCIAL NETWORKS

MTO also helped families move into neighborhoods where neighbors were more willing to work together to support shared norms, a measure of informal social control that previous research suggests may be particularly important in improving the lives of neighborhood residents (Sampson, Raudenbush, and Earls, 1997; Sampson, Morenoff, and Gannon-Rowley, 2002). Exhibit 2.11 shows that 59 percent of control group adults reported that neighbors were likely or very likely to do something if neighborhood children were spray painting buildings with graffiti. The share is nearly 8 points higher for those adults in the experimental

group (and approximately 16 percentage points higher for the experimental group compliers, that is, the TOT effect). Adults in the Section 8 group were more likely than controls to say neighborhood adults would be likely or very likely to do something about local children skipping school.

The MTO experimental intervention increased the social connections to people who had completed college and may have increased connections to people employed full-time, with no net adverse impacts on social isolation. For example, 53 percent of controls said they have a friend who graduated from college, a figure that was 7 percentage points higher for those in the experimental group (see Supplemental Exhibit 2.4 for additional results). These findings are consistent with those from the MTO interim study and with qualitative investigations of housing mobility programs in Yonkers, New York (Orr et al., 2003; Briggs, 1998).

MTO effects on the social connections of youth differed somewhat for males and females, as shown in Supplemental Exhibits 2.5–2.7. More than 96 percent of control group youth reported having at least one close friend. Experimental and Section 8 youth (particularly male youth) were slightly less likely than controls to report having at least one close friend. Experimental and Section 8 youth were also less likely than controls to report visiting a friend from their original neighborhoods. Female youth in the experimental group were less likely to have friends who have ever used drugs than are girls in the control group, but otherwise, peers of MTO youth had similar characteristics across treatment groups (Supplemental Exhibit 2.6). Youth across treatment groups shared similar reports on how good they felt about themselves and whether they considered themselves as speaking proper English or if others would describe their behavior as "ghetto" (Supplemental Exhibit 2.7). We also found suggestive evidence (though somewhat inconsistent across treatment groups and gender) that youth in the two treatment groups may be less likely than controls to say that other people see them as popular or as good students.

EXHIBIT 2.6. AVERAGE (DURATION-WEIGHTED) NEIGHBORHOOD CENSUS TRACT CHARACTERISTICS OF ALL OF THE ADULTS' ADDRESSES FROM RANDOM ASSIGNMENT THROUGH MAY 2008

| OUTCOME | CONTROL MEAN | EXPERIMENTAL VERSUS CONTROL | | SECTION 8 VERSUS CONTROL | | |
		ITT	TOT	ITT	TOT	N
Share of persons in tract who are poor [CEN]	0.396	– 0.089*	– 0.184*	– 0.069*	– 0.111*	3,270
		(0.006)	(0.012)	(0.007)	(0.011)	
Share of persons who are above 200% of the poverty line [CEN]	0.361	0.108*	0.223*	0.068*	0.110*	3,270
		(0.006)	(0.013)	(0.008)	(0.012)	
Share minority (which is, non-White or Hispanic) [CEN]	0.880	– 0.061*	– 0.125*	– 0.018*	– 0.029*	3,270
		(0.007)	(0.014)	(0.009)	(0.015)	
Share Hispanic [CEN]	0.315	– 0.038*	– 0.078*	– 0.003	– 0.005	3,270
		(0.006)	(0.011)	(0.007)	(0.012)	
Share Black [CEN]	0.535	– 0.034*	– 0.069*	– 0.019~	– 0.031~	3,270
		(0.008)	(0.016)	(0.011)	(0.017)	
Share of households that are single female-headed families [CEN]	0.541	– 0.077*	– 0.159*	– 0.058*	– 0.093*	3,270
		(0.006)	(0.012)	(0.007)	(0.011)	
Share college graduates [CEN]	0.161	0.042*	0.087*	0.018*	0.029*	3,270
		(0.004)	(0.008)	(0.005)	(0.008)	
Share with more than high school education [CEN]	0.318	0.063*	0.130*	0.034*	0.055*	3,270
		(0.005)	(0.010)	(0.006)	(0.009)	
Share employed, civilian [CEN]	0.808	0.036*	0.075*	0.031*	0.050*	3,270
		(0.003)	(0.006)	(0.003)	(0.005)	
Share workers in managerial or professional occupations [CEN]	0.236	0.035*	0.072*	0.012*	0.019*	3,270
		(0.004)	(0.008)	(0.005)	(0.008)	
Median household income (2009 dollars) [CEN]	$27,808.85	9,148.91*	1,8848.48*	5,600.18*	9,027.10*	3,270
		(544.97)	(1122.75)	(660.90)	(1065.32)	
Per capita income (2009 dollars) [CEN]	$14,779.62	3,267.36*	6,731.38*	1,492.56*	2,405.91*	3,270
		(266.65)	(549.35)	(340.51)	(548.88)	
Share owner-occupied housing units [CEN]	0.225	0.103*	0.213*	0.072*	0.115*	3,270
		(0.007)	(0.015)	(0.009)	(0.014)	
Share households on public assistance [CEN]	0.176	– 0.047*	– 0.096*	– 0.039*	– 0.064*	3,270
		(0.003)	(0.007)	(0.004)	(0.006)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CEN = 1990 and 2000 decennial census data as well as the 2005–09 American Community Survey (ACS).
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: Census tract characteristics are linearly interpolated from the 1990 decennial census, 2000 decennial census, and 2005–09 ACS. Each census outcome is also duration-weighted by the amount of time the respondent spent at each address.

EXHIBIT 2.7. CENSUS TRACT CHARACTERISTICS OF ADULT'S CURRENT NEIGHBORHOOD (MAY 2008)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Share of persons in tract who are poor [CEN]	0.313	– 0.038* (0.007)	– 0.079* (0.014)	– 0.029* (0.009)	– 0.046* (0.014)	3,206
Share of persons who are above 200% of the poverty line [CEN]	0.444	0.046* (0.008)	0.095* (0.017)	0.026* (0.011)	0.043* (0.017)	3,206
Share minority (which is non-White or Hispanic) [CEN]	0.841	– 0.037* (0.010)	– 0.075* (0.021)	– 0.003 (0.014)	– 0.005 (0.022)	3,206
Share Hispanic [CEN]	0.303	– 0.015~ (0.008)	– 0.030~ (0.017)	– 0.001 (0.011)	– 0.002 (0.018)	3,206
Share Black [CEN]	0.498	– 0.024* (0.011)	– 0.049* (0.022)	– 0.005 (0.015)	– 0.007 (0.024)	3,206
Share of households which are single female-headed families [CEN]	0.487	– 0.042* (0.008)	– 0.086* (0.017)	– 0.021* (0.010)	– 0.034* (0.017)	3,206
Share college graduates [CEN]	0.209	0.021* (0.006)	0.043* (0.012)	0.009 (0.007)	0.014 (0.012)	3,206
Share with more than high school education [CEN]	0.388	0.028* (0.007)	0.059* (0.013)	0.017* (0.008)	0.027* (0.013)	3,206
Share employed, civilian [CEN]	0.848	0.016* (0.003)	0.033* (0.006)	0.011* (0.004)	0.018* (0.006)	3,206
Share workers in managerial or professional occupations [CEN]	0.241	0.016* (0.005)	0.033* (0.011)	0.001 (0.007)	0.002 (0.011)	3,206
Median household income in 2009 dollars [CEN]	$33,520.69	4,416.64* (754.05)	9,099.20* (1553.50)	2,544.68* (955.14)	4,121.36* (1546.94)	3,206
Per capita income (2009 dollars) [CEN]	$17,134.84	1474.30* (389.69)	3,037.36* (802.84)	581.65 (499.28)	942.04 (808.63)	3,206
Share owner-occupied housing units [CEN]	0.329	0.048* (0.010)	0.099* (0.021)	0.030* (0.013)	0.049* (0.021)	3,206
Share households on public assistance [CEN]	0.100	– 0.019* (0.003)	– 0.039* (0.006)	– 0.014* (0.004)	– 0.023* (0.006)	3,206

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CEN = 1990 and 2000 decennial census data as well as the 2005–09 American Community Survey (ACS).
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: Census tract characteristics are linearly interpolated from the 1990 decennial census, 2000 decennial census, and 2005–09 ACS.

EXHIBIT 2.8. ADULT NEIGHBORHOOD CHARACTERISTICS AND SATISFACTION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
NEIGHBORHOOD SATISFACTION						
Very satisfied or satisfied with current neighborhood [SR]	0.515	0.094* (0.022)	0.194* (0.044)	0.082* (0.028)	0.131* (0.045)	3,265
NEIGHBORHOOD CHARACTERISTICS						
Litter/trash/graffiti/abandoned buildings are a big or small problem in neighborhood [SR]	0.720	– 0.073* (0.020)	– 0.151* (0.041)	– 0.081* (0.027)	– 0.128* (0.043)	3,266
Public drinking/loitering are a big or small problem in neighborhood [SR]	0.625	– 0.051* (0.021)	– 0.105* (0.044)	– 0.020 (0.028)	– 0.031 (0.045)	3,258
TRANSPORTATION						
Has working car or lives less than 15 minutes to public transportation [SR]	0.942	– 0.001 (0.010)	– 0.002 (0.021)	– 0.002 (0.014)	– 0.003 (0.023)	3,254

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

EXHIBIT 2.9. INTERVIEWER OBSERVATIONS OF THE ADULT'S CURRENT NEIGHBORHOOD						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
BUILDINGS ON RESPONDENT'S BLOCK						
Most buildings on this block are well kept [NWA]	0.348	0.024	0.049	0.007	0.011	3,199
		(0.021)	(0.044)	(0.028)	(0.045)	
There is evidence of graffiti that has been painted over [NWA]	0.206	– 0.028~	– 0.057~	– 0.040~	– 0.063~	3,100
		(0.017)	(0.035)	(0.021)	(0.033)	
There are metal bars on windows above the basement level on at least one other building on this block [NWA]	0.436	– 0.022	– 0.045	0.015	0.025	3,203
		(0.020)	(0.042)	(0.027)	(0.044)	
There are abandoned buildings on this block [NWA]	0.172	– 0.005	– 0.011	– 0.020	– 0.032	3,245
		(0.016)	(0.033)	(0.021)	(0.034)	
There is a green grocer, delicatessen, supermarket, or grocery store on this block [NWA]	0.210	– 0.012	– 0.025	– 0.022	– 0.036	3,245
		(0.017)	(0.035)	(0.021)	(0.033)	
There are no store-front churches, green grocers or delis, supermarkets or grocery stores, fast-food or take-out, or abandoned, burned out, or boarded up houses or buildings on this block [NWA]	0.592	0.013	0.028	0.035	0.056	3,245
		(0.021)	(0.044)	(0.028)	(0.044)	
STREET CONDITION						
Street is in very good/moderate condition [NWA]	0.654	0.012	0.026	0.015	0.024	3,207
		(0.021)	(0.043)	(0.027)	(0.044)	
Street is in poor condition [NWA]	0.084	– 0.010	– 0.021	– 0.009	– 0.014	3,207
		(0.011)	(0.023)	(0.016)	(0.026)	
Moderate-heavy amount of litter on the streets [NWA]	0.217	– 0.041*	– 0.085*	– 0.023	– 0.036	3,194
		(0.017)	(0.036)	(0.023)	(0.036)	
Trees are on most/all of the block [NWA]	0.422	0.011	0.022	0.044	0.071	3,198
		(0.021)	(0.044)	(0.028)	(0.045)	
No signs visible for neighborhood or crime watch, security warnings, alcohol advertisements, or houses "For Sale" on this block [NWA]	0.540	– 0.015	– 0.031	0.001	0.002	3,245
		0.022	0.045	0.029	0.047	
		(0.022)	(0.045)	(0.029)	(0.047)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: NWA = neighborhood walk-around conducted by interviewers.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

EXHIBIT 2.10. ADULT AND YOUTH NEIGHBORHOOD SAFETY AND CRIME VICTIMIZATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ADULT RESPONSES						
Feels safe or very safe during the day [SR]	0.804	0.036*	0.074*	0.045*	0.072*	3,262
		(0.016)	(0.034)	(0.021)	(0.034)	
Feels safe or very safe during the night [SR]	0.596	0.043*	0.088*	0.073*	0.117*	3,246
		(0.021)	(0.043)	(0.027)	(0.043)	
Police don't respond when called [SR]	0.420	− 0.067*	− 0.138*	− 0.075*	− 0.118*	3,146
		(0.021)	(0.044)	(0.028)	(0.045)	
Saw drugs being sold or used in the neighborhood in the past 30 days [SR]	0.310	− 0.062*	− 0.128*	− 0.057*	− 0.090*	3,249
		(0.019)	(0.039)	(0.025)	(0.040)	
Any household member was a crime victim in the last 6 months [SR]	0.184	− 0.022	− 0.046	0.025	0.040	3,241
		(0.016)	(0.033)	(0.022)	(0.035)	
YOUTH RESPONSES						
Feels safe or very safe during the day [SR]						
All	0.801	0.018	0.037	− 0.012	− 0.018	4,863
		(0.016)	(0.033)	(0.018)	(0.026)	
Female	0.784	0.045*	0.090*	0.019	0.030	2,478
		(0.022)	(0.045)	(0.025)	(0.039)	
Male	0.817	− 0.009	− 0.019	− 0.043~	− 0.062~	2,385
		(0.022)	(0.047)	(0.025)	(0.035)	
Feels safe or very safe during the night [SR]						
All	0.540	0.035~	0.074~	0.019	0.028	4,862
		(0.021)	(0.043)	(0.022)	(0.033)	
Female	0.486	0.052~	0.104~	0.067*	0.105*	2,478
		(0.028)	(0.056)	(0.030)	(0.048)	
Male	0.591	0.020	0.043	− 0.030	− 0.043	2,384
		(0.028)	(0.060)	(0.031)	(0.043)	
Saw drugs being sold or used in the neighborhood in the past 30 days [SR]						
All	0.388	− 0.056*	− 0.116*	− 0.041*	− 0.062*	4,879
		(0.020)	(0.040)	(0.021)	(0.031)	
Female	0.393	− 0.087*	− 0.174*	− 0.085*	− 0.134*	2,486
		(0.026)	(0.053)	(0.029)	(0.046)	
Male	0.384	− 0.026	− 0.056	0.002	0.003	2,393
		(0.026)	(0.057)	(0.028)	(0.040)	
Any household member was a crime victim in the last 6 months (only asked of youth ages 13–20) [SR]						
All	0.246	− 0.027	− 0.056	− 0.020	− 0.030	4,618
		(0.018)	(0.038)	(0.020)	(0.029)	
Female	0.255	− 0.059*	− 0.119*	− 0.044~	− 0.068~	2,358
		(0.024)	(0.047)	(0.026)	(0.040)	
Male	0.238	0.005	0.011	0.002	0.003	2,260
		(0.025)	(0.053)	(0.027)	(0.038)	

EXHIBIT 2.10. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Gangs are present in neighborhood or school (only asked of youth ages 13–20) [SR]						
All	0.672	– 0.022	– 0.045	0.000	0.000	4,597
		(0.019)	(0.040)	(0.021)	(0.031)	
Female	0.673	– 0.025	– 0.051	– 0.051~	– 0.079~	2,347
		(0.026)	(0.052)	(0.029)	(0.045)	
Male	0.671	– 0.019	– 0.041	0.052~	0.074~	2,250
		(0.028)	(0.059)	(0.030)	(0.042)	
FEMALE YOUTH ONLY						
People have made unwanted or rude comments to youth at least a few times a year [SR]	0.749	0.008	0.017	0.026	0.041	2,584
		(0.023)	(0.046)	(0.024)	(0.038)	
People have given unwanted sexual attention to youth at least a few times a year [SR]	0.596	– 0.052*	– 0.104*	– 0.045	– 0.070	2,577
		(0.025)	(0.051)	(0.028)	(0.044)	
Is afraid to go places because of unwanted attention or pressure at least a few times a year [SR]	0.401	– 0.003	– 0.005	– 0.030	– 0.047	2,582
		(0.026)	(0.051)	(0.028)	(0.044)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed; Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: "Any household member was a crime victim in the past 6 months" refers to a series of questions in which the youth is asked if anyone in their household: had their purse, wallet, or jewelry snatched from them, threatened with a gun or knife, was beaten or assaulted, was stabbed or shot, or if anyone attempted to break into their home.

EXHIBIT 2.11. NEIGHBORHOOD SOCIAL PROCESS AND ADULT SOCIAL NETWORKS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
COLLECTIVE EFFICACY						
Likely or very likely neighbors would do something if neighborhood kids were skipping school and hanging out on a street corner [SR]	0.346	0.029 (0.021)	0.059 (0.043)	0.075* (0.028)	0.119* (0.045)	3,250
Likely or very likely neighbors would do something if they saw kids spray-painting graffiti on a local building [SR]	0.589	0.076* (0.021)	0.156* (0.043)	0.042 (0.028)	0.067 (0.045)	3,255
FRIENDS						
Has three or more close friends [SR]	0.432	0.006 (0.021)	0.013 (0.044)	− 0.026 (0.028)	− 0.042 (0.045)	3,265
Has at least one close friend who graduated from college [SR]	0.532	0.071* (0.021)	0.145* (0.044)	0.007 (0.029)	0.010 (0.046)	3,203
Has at least one close friend who works full-time [SR]	0.742	0.033~ (0.019)	0.068~ (0.038)	− 0.020 (0.025)	− 0.032 (0.041)	3,261
Has at least one close friend who is a different race or ethnicity from respondent [SR]	0.413	0.022 (0.021)	0.046 (0.043)	− 0.017 (0.027)	− 0.027 (0.043)	3,266

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

2.7 INTERPRETATION OF RESULTS

MTO offered housing vouchers to families living in public housing at baseline, with the primary goal of helping them move into lower-poverty areas. The results in Chapter 1 showed that in that regard MTO was clearly a success. Families in the experimental group used the MTO voucher to move to tracts with poverty rates that were on average 42 percentage points lower than their baseline tracts (Exhibit 1.5). Section 8 families used their vouchers to move to tracts with poverty rates 25 percentage points lower. These findings are consistent with the hypothesis that many families living in public housing are basically tied to high-poverty areas if they wish to receive a housing subsidy, and (absent gaining access to a housing voucher or another housing program) must give up their rent subsidy to move into a lower-poverty area. The MTO voucher reduces the cost to families of moving into a lower-poverty area.

These effects on neighborhood conditions attenuate over time for a combination of reasons, but primarily because some control families moved to lower-poverty areas over time even without benefit of an MTO housing voucher. In addition, many treatment group families did not stay in the very low-poverty neighborhoods into which they made their initial MTO moves, and the fact that poverty rates declined over time in the baseline neighborhoods. Nonetheless as we showed in this chapter, poverty rates averaged over the entire study period—as well as other indicators of neighborhood socioeconomic composition, and neighborhood safety as well—are still quite different between the MTO treatment groups and the control group.

MTO did not generate sizable changes in neighborhood racial composition, unlike the Gautreaux mobility program (Rubinowitz and Rosenbaum, 2000). In Gautreaux, moving families to less racially segregated neighborhoods was a key objective of the program. The non-profit organization that helped carry out Gautreaux was responsible for identifying rental apartments for families, which eliminated the need for program participants to search themselves for housing and at the same time also gave participants limited choice over where they would move. In MTO, program participants instead were involved in the housing search process with assistance from a local non-profit. What we cannot tell from the MTO data is the degree to which MTO families moved to low-poverty, mostly-minority neighborhoods because that was their preference or instead because of racial discrimination they encountered searching for housing in more racially integrated communities. Both of these explanations fit the MTO data equally well.

MTO did generate sizable and persistent differences in the quality of housing units as well, even though many families who moved with an MTO voucher were still living in units with problems such as vermin, peeling paint or plaster, or problems with their heating or plumbing. An even larger share of the control group families lived in units with these types of problems.

The findings reported in this chapter suggest that at least among the set of families who volunteered for MTO, changing the form of housing assistance they receive from public housing to vouchers leads to improved housing quality and changes in a range of neighborhood conditions as well. Our findings for MTO effects on neighborhood quality are quite different from studies such as HUD's Welfare to Work voucher experiment (Mills et al. 2006) or the Chicago housing voucher lottery study of Jacob and Ludwig (2011), which compare outcomes of families that receive housing vouchers with those who are living unsubsidized in the private housing market. Those studies find that providing housing vouchers to families who are already living (unsubsidized) in private-market housing leads to very modest if any changes in neighborhood conditions. The limited effect of vouchers on neighborhood conditions for families already in private-market housing is not what we would predict from standard economic theory, which suggests that families receiving generous voucher subsidies would want to spend more on higher unit quality and higher neighborhood "quality." Learning more about why vouchers change neighborhood conditions for public housing residents but not for families already in private-market housing is an important question for future research.

Whether these MTO-induced changes in housing and neighborhood conditions translate into changes in

outcomes such as labor market earnings, schooling, health or delinquency will depend on which aspects of housing and neighborhoods are most relevant to these outcomes and the degree to which these outcomes are affected by immediate versus accumulated exposure to different housing and neighborhood conditions. But, at the very least, these moves have left the families better off, as judged by the growing share of families that report being satisfied with their housing units and neighborhoods.

SUPPLEMENTAL EXHIBIT 2.1. ADULT CURRENT HOUSING CONDITION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
OVERCROWDING IN HOUSING						
Overcrowded household (ratio of more than one person per room not including bathrooms and hallways) [SR]	0.229	− 0.008 (0.017)	− 0.017 (0.034)	− 0.021 (0.021)	− 0.034 (0.033)	3,260
Severely overcrowded household (ratio of more than one and one-half people per room not including bathrooms and hallways) [SR]	0.080	0.012 (0.011)	0.024 (0.024)	0.019 (0.015)	0.030 (0.023)	3,260
SPECIFIC HOUSING PROBLEMS						
Walls with peeling paint or broken plaster are a big or small problem [SR]	0.466	− 0.091* (0.021)	− 0.187* (0.044)	− 0.120* (0.028)	− 0.192* (0.044)	3,265
Plumbing not working is a big or small problem [SR]	0.327	− 0.058* (0.020)	− 0.119* (0.040)	− 0.053* (0.026)	− 0.085* (0.041)	3,265
Rats or mice are a big or small problem [SR]	0.347	− 0.038~ (0.020)	− 0.079~ (0.041)	− 0.045~ (0.027)	− 0.072~ (0.043)	3,265
Cockroaches are a big or small problem [SR]	0.356	− 0.053* (0.019)	− 0.110* (0.040)	− 0.089* (0.025)	− 0.142* (0.039)	3,267
Broken or no locks on door to the unit are a big or small problem [SR]	0.144	− 0.029~ (0.015)	− 0.059~ (0.030)	− 0.019 (0.019)	− 0.031 (0.030)	3,267
Broken windows or missing screens are a big or small problem [SR]	0.233	− 0.065* (0.017)	− 0.134* (0.036)	− 0.078* (0.023)	− 0.124* (0.036)	3,267
Heating system that does not work is a big or small problem [SR]	0.179	− 0.013 (0.016)	− 0.027 (0.033)	− 0.037~ (0.020)	− 0.059~ (0.033)	3,252

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

SUPPLEMENTAL EXHIBIT 2.2. YOUTH NEIGHBORHOOD CENSUS TRACT CHARACTERISTICS FOR CURRENT ADDRESS (MAY 2008)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Share of persons in tract who are poor [CEN]						
All	0.309	− 0.032*	− 0.067*	− 0.032*	− 0.049*	4,974
		(0.008)	(0.017)	(0.008)	(0.012)	
Female	0.311	− 0.033*	− 0.065*	− 0.032*	− 0.050*	2,543
		(0.009)	(0.019)	(0.010)	(0.016)	
Male	0.306	− 0.032*	− 0.068*	− 0.033*	− 0.047*	2,431
		(0.010)	(0.022)	(0.010)	(0.014)	
Share of persons who are above 200% of the poverty line [CEN]						
All	0.450	0.034*	0.070*	0.030*	0.044*	4,974
		(0.010)	(0.020)	(0.010)	(0.015)	
Female	0.446	0.034*	0.069*	0.031*	0.048*	2,543
		(0.012)	(0.023)	(0.013)	(0.020)	
Male	0.454	0.033*	0.072*	0.029*	0.041*	2,431
		(0.012)	(0.026)	(0.012)	(0.018)	
Share minority (non-White or Hispanic) [CEN]						
All	0.832	− 0.016	− 0.034	− 0.019	− 0.028	4,974
		(0.012)	(0.026)	(0.014)	(0.021)	
Female	0.831	− 0.018	− 0.035	− 0.023	− 0.036	2,543
		(0.015)	(0.029)	(0.017)	(0.027)	
Male	0.834	− 0.015	− 0.033	− 0.014	− 0.020	2,431
		(0.016)	(0.033)	(0.017)	(0.025)	
Share Hispanic [CEN]						
All	0.299	− 0.003	− 0.007	0.015	0.023	4,974
		(0.009)	(0.019)	(0.009)	(0.014)	
Female	0.306	− 0.004	− 0.008	0.012	0.019	2,543
		(0.011)	(0.022)	(0.012)	(0.019)	
Male	0.292	− 0.003	− 0.007	0.019	0.027	2,431
		(0.011)	(0.024)	(0.011)	(0.016)	
Share Black [CEN]						
All	0.497	− 0.017	− 0.035	− 0.041*	− 0.062*	4,974
		(0.013)	(0.027)	(0.015)	(0.022)	
Female	0.488	− 0.019	− 0.039	− 0.045*	− 0.071*	2,543
		(0.015)	(0.031)	(0.019)	(0.029)	
Male	0.506	− 0.014	− 0.031	− 0.037*	− 0.053*	2,431
		(0.015)	(0.033)	(0.019)	(0.026)	
Share of households that are single female-headed families [CEN]						
All	0.483	− 0.036*	− 0.075*	− 0.041*	− 0.061*	4,974
		(0.010)	(0.020)	(0.010)	(0.015)	

SUPPLEMENTAL EXHIBIT 2.2. (CONTINUED)					
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL	
OUTCOME		ITT	TOT	ITT	TOT	N

OUTCOME	CONTROL MEAN	ITT	TOT	ITT	TOT	N
SHARE OF HOUSEHOLDS THAT ARE SINGLE FEMALE-HEADED FAMILIES [CEN] (CONTINUED)						
Female	0.479	– 0.032*	– 0.065*	– 0.039*	– 0.062*	2,543
		(0.011)	(0.022)	(0.012)	(0.019)	
Male	0.486	– 0.040*	– 0.085*	– 0.043*	– 0.061*	2,431
		(0.012)	(0.025)	(0.012)	(0.017)	
Share college graduates [CEN]						
All	0.209	0.015*	0.031*	0.015*	0.023*	4,974
		(0.007)	(0.014)	(0.007)	(0.011)	
Female	0.208	0.015*	0.031*	0.021*	0.033*	2,543
		(0.008)	(0.016)	(0.009)	(0.014)	
Male	0.209	0.015~	0.033~	0.009	0.013	2,431
		(0.008)	(0.018)	(0.009)	(0.013)	
Share with more than high school education [CEN]						
All	0.391	0.021*	0.044*	0.017*	0.026*	4,974
		(0.007)	(0.016)	(0.008)	(0.012)	
Female	0.391	0.020*	0.041*	0.021*	0.033*	2,543
		(0.009)	(0.018)	(0.010)	(0.015)	
Male	0.392	0.022*	0.048*	0.013	0.019	2,431
		(0.010)	(0.021)	(0.010)	(0.014)	
Share employed, civilian [CEN]						
All	0.848	0.012*	0.025*	0.014*	0.021*	4,974
		(0.004)	(0.008)	(0.004)	(0.006)	
Female	0.849	0.010*	0.020*	0.011*	0.018*	2,543
		(0.004)	(0.008)	(0.005)	(0.008)	
Male	0.846	0.014*	0.030*	0.017*	0.024*	2,431
		(0.005)	(0.010)	(0.005)	(0.007)	
Share workers in managerial or professional occupations [CEN]						
All	0.240	0.009	0.018	0.007	0.011	4,974
		(0.006)	(0.012)	(0.006)	(0.009)	
Female	0.238	0.011~	0.023~	0.015*	0.024*	2,543
		(0.007)	(0.014)	(0.007)	(0.012)	
Male	0.243	0.006	0.014	– 0.001	– 0.001	2,431
		(0.008)	(0.017)	(0.008)	(0.012)	
Median household income (2009 dollars) [CEN]						
All	$34,189.96	3,627.81*	7,526.02*	3,035.12*	4,543.18*	4,974
		(895.53)	(1,857.81)	(903.84)	(1,352.93)	
Female	$34,130.44	3,440.25*	6,920.75*	2,792.55*	4,385.01*	2,543
		(1,050.07)	(2,112.42)	(1,109.29)	(1,741.86)	
Male	$34,246.61	3,815.73*	8,193.22*	3,277.41*	4,677.22*	2,431
		(1,118.32)	(2,401.28)	(1,126.04)	(1,606.98)	

SUPPLEMENTAL EXHIBIT 2.2. (CONTINUED)					
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL	
OUTCOME		ITT	TOT	ITT	TOT	N
Per capita income (2009 dollars) [CEN]						
All	$17,257.40	918.75*	1,905.98*	622.10	931.21	4,974
		(451.78)	(937.24)	(461.82)	(691.28)	
Female	$17,150.40	945.79~	1,902.65~	897.02	1,408.54	2,543
		(543.00)	(1,092.34)	(585.07)	(918.70)	
Male	$17,359.24	897.28	1,926.66	339.91	485.08	2,431
		(563.61)	(1,210.20)	(545.94)	(779.11)	
Share owner-occupied housing units [CEN]						
All	0.338	0.042*	0.088*	0.041*	0.062*	4,974
		(0.012)	(0.025)	(0.013)	(0.019)	
Female	0.335	0.040*	0.081*	0.034*	0.054*	2,543
		(0.014)	(0.029)	(0.015)	(0.024)	
Male	0.342	0.045*	0.096*	0.048*	0.069*	2,431
		(0.015)	(0.032)	(0.015)	(0.022)	
Share of households on public assistance [CEN]						
All	0.098	− 0.015*	− 0.031*	− 0.014*	− 0.021*	4,974
		(0.004)	(0.008)	(0.003)	(0.005)	
Female	0.100	− 0.016*	− 0.031*	− 0.015*	− 0.024*	2,543
		(0.004)	(0.008)	(0.004)	(0.007)	
Male	0.097	− 0.015*	− 0.032*	− 0.013*	− 0.019*	2,431
		(0.005)	(0.010)	(0.004)	(0.006)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CEN = 1990 and 2000 decennial census data as well as the 2005–09 American Community Survey (ACS).
Model: Experimental and Section 8 impacts 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. Youth impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: Census tract characteristics are linearly interpolated from the 1990 decennial census, 2000 decennial census, and 2005–09 ACS.

SUPPLEMENTAL EXHIBIT 2.3. ADULT NEIGHBORHOOD CONDITIONS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DRUGS						
Saw drugs being used or sold in neighborhood in the past 30 days [SR]	0.310	– 0.062* (0.019)	– 0.128* (0.039)	– 0.057* (0.025)	– 0.090* (0.040)	3,249
Saw someone using drugs in neighborhood in past 30 days [SR]	0.198	– 0.050* (0.016)	– 0.103* (0.033)	– 0.050* (0.021)	– 0.078* (0.034)	3,247
Saw someone selling drugs in neighborhood in past 30 days [SR]	0.264	– 0.047* (0.018)	– 0.096* (0.037)	– 0.051* (0.024)	– 0.081* (0.038)	3,249
NEIGHBORHOOD PROBLEMS						
Litter or trash on the sidewalk is a big or small problem [SR]	0.646	– 0.075* (0.021)	– 0.155* (0.042)	– 0.069* (0.028)	– 0.111* (0.045)	3,265
Graffiti on the walls is a big or small problem [SR]	0.466	– 0.061* (0.021)	– 0.126* (0.042)	– 0.041 (0.028)	– 0.065 (0.044)	3,261
People drinking in public is a big or small problem [SR]	0.513	– 0.056* (0.022)	– 0.114* (0.045)	– 0.020 (0.029)	– 0.031 (0.046)	3,246
Abandoned buildings is a big or small problem [SR]	0.388	– 0.047* (0.020)	– 0.096* (0.042)	– 0.040 (0.027)	– 0.064 (0.043)	3,259
Groups of people hanging out is a big or small problem [SR]	0.557	– 0.049* (0.021)	– 0.101* (0.044)	– 0.012 (0.029)	– 0.019 (0.045)	3,258
HOUSEHOLD VICTIMIZATION (PAST 6 MONTHS)						
Household member had their purse, wallet, or jewelry snatched [SR]	0.077	– 0.005 (0.011)	– 0.010 (0.023)	0.008 (0.014)	0.012 (0.023)	3,234
Household member was threatened with a knife or gun [SR]	0.066	– 0.008 (0.010)	– 0.016 (0.021)	0.001 (0.013)	0.001 (0.021)	3,236
Household member was beaten up or assaulted [SR]	0.074	– 0.014 (0.011)	– 0.030 (0.022)	– 0.005 (0.014)	– 0.008 (0.023)	3,238
Household member was stabbed or shot [SR]	0.029	– 0.006 (0.007)	– 0.013 (0.015)	– 0.008 (0.008)	– 0.013 (0.013)	3,241
Someone attempted to break into the home [SR]	0.043	– 0.003 (0.009)	– 0.007 (0.019)	0.015 (0.013)	0.025 (0.021)	3,242

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed

SUPPLEMENTAL EXHIBIT 2.4. MEDIATORS: ADULT SOCIAL NETWORKS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Has no friends in the neighborhood [SR]	0.571	0.009 (0.021)	0.019 (0.044)	0.009 (0.028)	0.014 (0.045)	3,267
Visits friends in their home at least once a week [SR]	0.239	0.015 (0.018)	0.031 (0.038)	0.018 (0.024)	0.028 (0.039)	3,266
Stops to chat with neighbor in street or hallway at least once a week [SR]	0.532	0.019 (0.021)	0.039 (0.044)	0.030 (0.028)	0.048 (0.045)	3,269
Attended church or religious services at least once a month in the past year [SR]	0.460	− 0.028 (0.021)	− 0.057 (0.044)	0.013 (0.028)	0.020 (0.045)	3,264

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

SUPPLEMENTAL EXHIBIT 2.5. MEDIATORS: YOUTH SOCIAL NETWORK						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Youth has at least one close friend [SR]						
All	0.963	− 0.018* (0.008)	− 0.037* (0.016)	− 0.019* (0.009)	− 0.028* (0.014)	5,094
Female	0.967	-0.009 (0.010)	− 0.019 (0.021)	− 0.021~ (0.013)	− 0.034~ (0.020)	2,595
Male	0.959	− 0.027* (0.012)	− 0.057* (0.026)	− 0.016 (0.014)	− 0.022 (0.020)	2,499
Youth has at least three close friends [SR]						
All	0.640	− 0.023 (0.019)	− 0.048 (0.039)	− 0.032 (0.020)	− 0.047 (0.030)	5,094
Female	0.616	− 0.040 (0.026)	− 0.080 (0.051)	− 0.054~ (0.028)	− 0.085~ (0.045)	2,595
Male	0.661	− 0.006 (0.026)	− 0.013 (0.057)	− 0.009 (0.029)	− 0.013 (0.041)	2,499
Youth has at least one close friend in their neighborhood [SR]						
All	0.624	− 0.014 (0.019)	− 0.030 (0.040)	− 0.028 (0.021)	− 0.041 (0.031)	5,089
Female	0.608	− 0.009 (0.026)	− 0.018 (0.053)	− 0.052~ (0.029)	− 0.082~ (0.046)	2,594
Male	0.640	− 0.021 (0.027)	− 0.045 (0.058)	− 0.002 (0.029)	− 0.003 (0.042)	2,495

SUPPLEMENTAL EXHIBIT 2.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Youth has at least one friend that was also a friend at random assignment [SR]						
All	0.487	− 0.027	− 0.055	− 0.039~	− 0.058~	5,006
		(0.020)	(0.041)	(0.022)	(0.032)	
Female	0.496	− 0.024	− 0.049	− 0.037	− 0.059	2548
		(0.027)	(0.055)	(0.030)	(0.047)	
Male	0.479	− 0.029	− 0.062	− 0.040	− 0.057	2,458
		(0.027)	(0.058)	(0.029)	(0.042)	
Youth visits baseline friends or they visit youth at least a few times a week [SR]						
All	0.280	− 0.035*	− 0.073*	− 0.038*	− 0.057*	5,002
		(0.017)	(0.036)	(0.018)	(0.027)	
Female	0.257	− 0.030	− 0.060	− 0.016	− 0.025	2,546
		(0.023)	(0.047)	(0.025)	(0.040)	
Male	0.302	− 0.040~	− 0.086~	− 0.060*	− 0.086*	2,456
		(0.024)	(0.052)	(0.026)	(0.037)	
Youth attended youth activities at church at least once a month during past year [SR]						
All	0.360	− 0.023	− 0.049	0.001	0.002	5,086
		(0.019)	(0.039)	(0.020)	(0.030)	
Female	0.356	− 0.031	− 0.062	− 0.003	− 0.005	2,589
		(0.025)	(0.051)	(0.028)	(0.044)	
Male	0.363	− 0.016	− 0.035	0.005	0.007	2,497
		(0.026)	(0.056)	(0.028)	(0.040)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

SUPPLEMENTAL EXHIBIT 2.6. MEDIATORS: YOUTH'S PEERS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Close friends think that studying is very important [SR]						
All	0.428	– 0.022	– 0.046	– 0.016	– 0.024	5,083
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.474	– 0.009	– 0.019	– 0.029	– 0.046	2,589
		(0.026)	(0.053)	(0.029)	(0.045)	
Male	0.385	– 0.035	– 0.076	– 0.002	– 0.003	2,494
		(0.027)	(0.058)	(0.030)	(0.042)	
Close friends think that it is very important to continue their education past high school [SR]						
All	0.732	0.006	0.013	0.003	0.005	5,082
		(0.017)	(0.034)	(0.018)	(0.028)	
Female	0.787	0.024	0.048	0.010	0.016	2,589
		(0.021)	(0.042)	(0.024)	(0.037)	
Male	0.680	– 0.012	– 0.026	– 0.003	– 0.005	2,493
		(0.025)	(0.054)	(0.028)	(0.041)	
Close friends have ever been involved in school activities [SR]						
All	0.810	– 0.014	– 0.029	– 0.018	– 0.027	5,041
		(0.015)	(0.030)	(0.017)	(0.025)	
Female	0.820	– 0.016	– 0.033	– 0.027	– 0.042	2,568
		(0.020)	(0.041)	(0.023)	(0.036)	
Male	0.800	– 0.012	– 0.026	– 0.009	– 0.012	2,473
		(0.022)	(0.047)	(0.025)	(0.035)	
Close friends have ever used marijuana or other drugs [SR]						
All	0.357	– 0.045*	– 0.094*	– 0.025	– 0.038	4,955
		(0.019)	(0.039)	(0.021)	(0.031)	
Female	0.315	– 0.063*	– 0.126*	– 0.034	– 0.053	2,541
		(0.024)	(0.048)	(0.027)	(0.043)	
Male	0.397	– 0.028	– 0.059	– 0.017	– 0.024	2,414
		(0.026)	(0.057)	(0.028)	(0.041)	
Close friends have ever dropped out of school [SR]						
All	0.214	– 0.005	– 0.010	– 0.005	– 0.008	5,082
		(0.015)	(0.031)	(0.017)	(0.026)	
Female	0.200	– 0.018	– 0.037	– 0.040~	– 0.063~	2,591
		(0.020)	(0.040)	(0.022)	(0.035)	
Male	0.227	0.008	0.018	0.030	0.043	2,491
		(0.022)	(0.048)	(0.025)	(0.036)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

SUPPLEMENTAL EXHIBIT 2.7. YOUTH SELF-REFLECTION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Youth speaks proper English very often or somewhat often (versus never) [SR]						
All	0.894	0.001	0.001	0.004	0.005	5,092
		(0.011)	(0.024)	(0.012)	(0.018)	
Female	0.896	0.011	0.022	0.006	0.009	2,595
		(0.015)	(0.031)	(0.017)	(0.027)	
Male	0.891	− 0.010	− 0.021	0.002	0.002	2,497
		(0.017)	(0.036)	(0.018)	(0.026)	
Youth thinks people would never describe his or her behavior as "ghetto" (versus very often or somewhat often) [SR]						
All	0.613	0.000	0.000	− 0.012	− 0.018	5,077
		(0.019)	(0.039)	(0.020)	(0.030)	
Female	0.638	− 0.016	− 0.031	− 0.007	− 0.011	2,586
		(0.025)	(0.050)	(0.027)	(0.043)	
Male	0.589	0.016	0.034	− 0.018	− 0.026	2,491
		(0.026)	(0.056)	(0.029)	(0.041)	
Youth strongly agrees that they feel good about themselves (versus agree, disagree, or strongly disagree) [SR]						
All	0.675	0.013	0.027	− 0.011	− 0.016	5,095
		(0.018)	(0.037)	(0.020)	(0.029)	
Female	0.667	0.028	0.057	− 0.024	− 0.037	2,596
		(0.024)	(0.049)	(0.027)	(0.043)	
Male	0.683	− 0.003	− 0.007	0.003	0.004	2,499
		(0.025)	(0.054)	(0.027)	(0.039)	
Youth strongly agrees that they can do things as well as other people (versus agree, disagree, or strongly disagree) [SR]						
All	0.584	0.017	0.035	− 0.007	− 0.010	5,093
		(0.019)	(0.039)	(0.020)	(0.030)	
Female	0.590	0.032	0.065	0.005	0.008	2,594
		(0.026)	(0.052)	(0.028)	(0.044)	
Male	0.579	0.002	0.004	− 0.018	− 0.026	2,499
		(0.026)	(0.055)	(0.029)	(0.041)	
Youth strongly disagrees or disagrees that chance and luck are very important in life (versus strongly agree or agree) [SR]						
All	0.309	− 0.015	− 0.030	0.017	0.025	5,082
		(0.018)	(0.037)	(0.019)	(0.029)	
Female	0.308	− 0.070*	− 0.141*	− 0.013	− 0.020	2,590
		(0.024)	(0.048)	(0.026)	(0.041)	
Male	0.311	0.041	0.088	0.045	0.064	2,492
		(0.025)	(0.054)	(0.028)	(0.040)	
Youth thinks other people always see them as popular (versus some of the time or never) [SR]						
All	0.323	− 0.028	− 0.057	− 0.030	− 0.045	5,087
		(0.017)	(0.035)	(0.018)	(0.028)	
Female	0.315	− 0.038~	− 0.077~	− 0.057*	− 0.089*	2,593
		(0.023)	(0.047)	(0.025)	(0.040)	

SUPPLEMENTAL EXHIBIT 2.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
YOUTH THINKS OTHER PEOPLE ALWAYS SEE THEM AS POPULAR (VERSUS SOME OF THE TIME OR NEVER) [SR] (CONTINUED)						
Male	0.330	− 0.017	− 0.037	− 0.003	− 0.004	2,494
		(0.024)	(0.052)	(0.027)	(0.038)	
Youth thinks other people always see them as athletic (versus some of the time or never) [SR]						
All	0.318	− 0.020	− 0.041	0.002	0.003	5,089
		(0.017)	(0.036)	(0.019)	(0.028)	
Female	0.207	− 0.035~	− 0.071~	− 0.003	− 0.005	2,593
		(0.021)	(0.043)	(0.024)	(0.038)	
Male	0.422	− 0.004	− 0.009	0.006	0.008	2,496
		(0.027)	(0.058)	(0.030)	(0.043)	
Youth thinks other people always see them as a good student (versus some of the time or never) [SR]						
All	0.404	0.002	0.004	− 0.016	− 0.024	5,092
		(0.019)	(0.039)	(0.020)	(0.031)	
Female	0.455	0.027	0.055	0.014	0.022	2,595
		(0.026)	(0.052)	(0.029)	(0.045)	
Male	0.356	− 0.023	− 0.050	− 0.046~	− 0.066~	2,497
		(0.026)	(0.055)	(0.027)	(0.039)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

CHAPTER 3

IMPACTS ON ADULT AND YOUTH PHYSICAL HEALTH

In this chapter, we summarize MTO's impacts on the physical health of adults and youth. At the end of 2007, the adults were on average 45.2 years old, with 29 percent over age 50, while the youth in our study sample ranged in age from 10 to 20. We relied in part on adult and youth self-reports of health status, health behaviors, and access to health care. Because differing access to health services across neighborhoods could affect self-reported health outcomes, we collected physical measurements of health outcomes as well. Specifically, we measured height and weight, and for adults we also measured waist circumference and blood pressure and collected blood samples to measure physiological indicators of long-term disease risk. We also asked adults to report on the health outcomes of their grown children (those who were under age 18 at baseline but over age 20 at the end of 2007), who were too old to have been included in our youth survey sample frame.

Although MTO does not have detectable impacts on many of the physical health outcomes that we examined, particularly those focused on the health of the average adult in each randomized mobility group (central tendencies), we did find impacts on several measures of the prevalence of adverse health outcomes that are relevant for people with the most distressed health outcomes (those at the tails of the health outcome distribution). Specifically, we find that, compared with the control group, adults in the experimental group:

- Have a lower prevalence of extreme obesity (defined as a body mass index (BMI)[1] of greater than or equal to 35);
- Have a lower prevalence of diabetes (measured by glycosylated hemoglobin [HbA1c] greater than or equal to 6.5 percent);

---

1 Body mass index (BMI) is defined as weight in kilograms divided by height in meters squared.

- Have fewer self-reported physical limitations; and Show no statistically significant differences on other health outcomes such as self-rated overall health, asthma, or blood pressure.

Results for adults in the Section 8 group tend to point in the same direction, but the effects in general were smaller and not statistically significant.

We detect no significant differences across treatment groups in the physical health of youth. Similarly, we see few effects on grown children, with the possible exception of an increased prevalence of physical limitations among male grown children in the Section 8 group compared with those in the control group. We find few detectable differences across randomized groups on health-related behaviors, such as diet and moderate exercise, although we did find that a higher share of adults in the experimental group compared with the control group appear to engage in vigorous exercise. We also see signs of increased access to health care services for people in the Section 8 group versus those in the control group.

3.1 HYPOTHESES ABOUT EFFECTS ON ADULT AND YOUTH PHYSICAL HEALTH

Previous epidemiological research has found strong associations between living in disadvantaged neighborhoods and adverse health outcomes for adults, such as mortality, heart disease, obesity, depression, substance abuse, and poor overall health, even after controlling for individual health risk factors and protective factors. For example, Waitzman and Smith (1998) find that people living in federally designated poverty areas have higher rates of mortality even after controlling for individual characteristics. Ross and Mirowsky (2001) find that living in disadvantaged neighborhoods is associated with poorer self-reported health and physical functioning. Browning and Cagney

(2002) find that individuals residing in neighborhoods with greater collective efficacy report better overall health. Diez-Roux et al. (2001) find that adults living in disadvantaged neighborhoods are at significantly greater risk of developing coronary heart disease, even after controlling for income, occupation, and education (see Pickett and Pearl, 2001; Kawachi and Berkman, 2003b; Macintyre and Ellaway, 2003, for excellent reviews of this literature). More recently, Bird et al. (2010) find that "good cholesterol" (high-density lipoprotein or HDL) and lower blood pressure are associated with higher neighborhood socioeconomic status after controlling for other factors. For children, living in a high-poverty urban setting or unsafe neighborhood is also associated with adverse health outcomes (Curtis, Dooley, and Phipps, 2004; Lumeng et al., 2006).

People living in high-poverty neighborhoods differ in many ways from those living in lower-poverty neighborhoods. Pickett and Pearl (2001) observe that estimates of neighborhood effects on health tend to be smaller in studies employing a richer set of control variables for the individual's own (as opposed to neighborhood) socioeconomic condition. Because nonexperimental studies cannot adequately measure and control for all of the individual differences between people in different neighborhoods, the causal relationships between neighborhood and health remain unclear. The design of the MTO study overcomes these limitations and makes the estimation of causal effects possible.

MTO-assisted moves to lower-poverty neighborhoods could affect health by altering a participant's physical and social environments and access to health-related community resources. Exhibit 3.1 shows how these changes might affect health, directly or indirectly, by changing health-related behaviors, including stress and health care access.

EXHIBIT 3.1. HYPOTHESIZED PATHWAYS THROUGH WHICH MTO MAY IMPACT PHYSICAL HEALTH

MTO INTERVENTION ➡ RELOCATION TO LOW-POVERTY NEIGHBORHOODS

| COMMUNITY-LEVEL MEDIATORS | PERSON- AND FAMILY-LEVEL MEDIATORS | PHYSICAL HEALTH OUTCOMES |

Physical Environment
· Cleaner air, fewer allergens
· More walkable
· Fewer hazards

Community Resources
· Health care facilities
· Access to jobs with health benefits
· Access to fast food, grocery stores, and liquor stores
· Exercise facilities

Social Environment
· Reduced crime and violence
· Collective efficacy
· Peers who eat healthier and exercise more
· Different neighbors

Access to Care
· Health insurance
· Access to medical care

Exercise and Nutrition
· Healthier eating
· More exercise

Safety and Stress
· Less stress

Social Supports
· More/less isolation
· More/less support

Physical Health
· General health status
· Difficulties with daily activities
· Asthma
· Injuries
· Hypertension
· Obesity
· Diabetes
· Chronic pain

Unhealthy Behaviors
· Smoking
· Drinking

Exhibit 3.1 is a modified version of Exhibit 4.1 in the MTO interim impacts evaluation (see Orr et al., 2003).

PHYSICAL ENVIRONMENT

A drive through any major American city clearly shows that the physical environments of neighborhoods vary widely. The environment—both indoors and out—may affect health by exposing individuals to pollution, contaminants, allergens, and physical hazards like broken glass. Poor housing quality, which can increase exposure to household dangers such as vermin, toxins, mold, and poor ventilation, may lead to a higher incidence of asthma (Bryant-Stephens, 2009). Carpeting can harbor dust mites, and its removal or cleaning is often recommended as an asthma remediation strategy (Crain et al., 2002; van der Heide et al., 1997). 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 lead exposure as a result of lead dust from peeling paint and soil contamination. This can impair brain development and may be exacerbated by poor nutrition (Filippelli and Laidlaw, 2010). Exposure to secondhand smoke may also be greater in low-income areas because of a higher prevalence of smoking (Bombard et al., 2010). The prevalence of accidents and injuries may be higher among children living in distressed urban communities owing to unsafe playgrounds and other features of the environment (Scharfstein and Sandel, 1998; Quinlan, 1996). Accidents may be a particularly important way in which neighborhoods affect child health, since accidents are the most common cause of death among children ages 1 to 14 in the United States.

The "built environment" of a neighborhood—buildings, parks, and streets—may also influence health by affecting exercise patterns. Studies have found that neighborhood greenness is associated with lower BMI for youth (Bell, Wilson, and Liu, 2008), and the "walkability" of a neighborhood may be associated with more physical activity and lower rates of overweight and obesity (Mujahid et al., 2008; Lovasi et al., 2009; Sallis et al., 2009).

COMMUNITY RESOURCES

Community resources such as stores, employers, and transportation are another part of the built environment

that may affect health behaviors and outcomes. The presence of grocery stores could affect diet by reducing the purchase price or transportation costs associated with accessing fresh fruits and vegetables (Morland et al., 2002). The availability of liquor stores and fast food restaurants can also affect diet, by reducing the transportation costs associated with accessing inexpensive forms of alcohol or fast food (Zenk et al., 2005; Franco et al., 2008; Inagami et al., 2006; Dubowitz et al., 2008).

MTO's potential effects on access to quality health care are more difficult to predict in advance. Low-income families may have difficulty accessing health care in part because they lack health insurance. MTO might increase access to health care if families move to areas with more opportunities for employment that offers health insurance benefits. MTO might also help families move to areas with better transportation or more local primary care physicians, making it easier to receive preventive care and other services. On the other hand, the MTO demonstration took place in cities with some of the top-ranked hospitals in the country, many of them academic medical centers that for historical reasons are located near economically disadvantaged inner-city neighborhoods.[2] 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 health care facilities located in more affluent areas.

SOCIAL ENVIRONMENT

Exposure to crime, social supports, and peer influences are all parts of the social environment that may affect health. Dangerous neighborhoods can have deleterious direct effects on health from injuries from assaults. Exposure to violence can be traumatic. Living in a dangerous neighborhood can produce chronic stress that in turn may be associated with asthma attacks (Wright, 1998; Bloomberg and Chen, 2005), hypertension (Joint National Committee on Prevention, Detection, Evaluation, and Treatment of High Blood Pressure, 1997; Kornitzer, Dramaix, and De Backer, 1999), and a lower ability of the immune system to respond to foreign substances, such as viruses and bacteria. Medical

---

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

researchers also speculate that exposure to trauma and other stressors may lead to dysregulation across multiple physiological systems (Merkin et al., 2009). Stress may affect eating patterns, for example, by leading to increased intake of dense-calorie foods (Torres and Nowson, 2007), and stress is associated with increased use of alcohol, tobacco, and other substances. In addition, living in a dangerous neighborhood may limit walking and other forms of outdoor exercise and could even influence access to health care facilities, especially in areas with inadequate or dangerous public transportation systems. Reduced outdoor time in more dangerous neighborhoods could also increase exposure to indoor pollutants.

MTO-assisted moves could help improve people's health if they relocate to less distressed neighborhoods with higher levels of "collective efficacy," that is, the level of trust among neighborhood residents and their willingness to work together to support shared values (Sampson, Raudenbush, and Earls, 1997). Living in a neighborhood with higher levels of collective efficacy could help families build social supports that buffer the effects of stress (Cohen, 2004; Berkman and Glass, 2000). Of course, this assumes that families are willing and able to connect with their new neighbors. Moving to a lower-poverty area may not be helpful if families are unable to connect to their new neighbors, and could even potentially harm families' mental or physical health if families feel deprived relative to the material wealth and social status of their new neighbors (Jencks and Mayer, 1990; Luttmer, 2005; Lawler and Thye, 1999).

Peer influences are a final mechanism through which MTO-assisted moves may affect health behaviors and subsequent outcomes (Sallis, Prochaska, and Taylor, 2000; Raudsepp and Viira, 2000). Epidemiological studies find a socioeconomic gradient in diets, with higher socioeconomic groups tending to eat a higher-quality diet that includes more fresh fruits and vegetables, whole grains, fish, and lower-fat meats and dairy products and with lower socioeconomic groups eating on average higher-calorie, lower-nutrient diets (Darmon and Drewnoski, 2008). As a result, moves to lower-poverty neighborhoods may expose participants

to neighbors with healthier lifestyles, including better diet and exercise habits and less smoking (Lee and Cubbin, 2002; Berkman and Breslow, 1983). On the other hand, alcohol consumption may be higher in the more affluent areas into which MTO families move, since alcohol consumption has a positive association with socioeconomic status (SES) over at least part of the SES distribution (Substance Abuse and Mental Health Services Administration, 2010). The net effect on health could be positive or negative given that previous research suggests that alcohol consumption may have a u-shaped relationship with mortality (Lee et al., 2008).

The degree to which exposure to people with different health habits translates into changes in health-related behavior by participants may depend on how socially connected participants are with their new neighbors (Christakis and Fowler, 2007). This exposure may be less helpful for Hispanic women in our study sample, given previous research indicating that greater levels of acculturation (often associated with higher income) may actually lead to less healthy lifestyles (see Lara et al., 2005 for a review of this literature). This previous research implies MTO moves for Hispanic women into neighborhoods with relatively larger shares of non-Hispanic residents could lead to additional acculturation and potentially to increased prevalence of health-risk behaviors.

We hypothesize that moves to lower-poverty neighborhoods may improve physical health because the new neighborhoods may be safer and less stressful, have greater community resources, have residents who tend to smoke less, eat healthier, and exercise more, and have fewer environmental hazards. However, it is possible that health might not be affected at all or that it could be negatively affected if housing quality has deteriorated, participants feel socially isolated, new neighborhoods are less walkable, or access to health care—at least the sort that is responsive to the needs of low-income minority families—is more difficult. Most of these mechanisms suggest that the effects on health from MTO will be greater if families spend more time in low-poverty neighborhoods.

3.2 EARLIER RESEARCH: SHORT-TERM AND INTERIM IMPACTS ON PHYSICAL HEALTH

Previous studies of the MTO demonstration's impacts on various physical health outcomes for adults and youth have yielded a mixed pattern of results. The short-term study (on average two years after baseline) of MTO at the Boston site found that adults in the experimental group were more likely than control subjects to rate their health as good or better. Short-term data from Boston also showed a lower incidence of asthma and nonsports injuries (for example, physical injuries from falls, fights, needles, and broken glass) for children in the experimental group relative to the control group (Katz, Kling, and Liebman, 2001).

Findings from the interim evaluation (four to seven years after baseline, pooling data from all five sites) show a lower prevalence of obesity (BMI ≥ 30) for adults in the experimental group than those in the control group (42.0 percent vs. 47.1 percent), together with some signs of increased rates of exercise and improved diet (Orr et al., 2003, Exhibit 4.2, p. 77 and Exhibit E4.2, p. E–2). However, the interim study did not detect statistically significant effects for most other adult physical health outcomes, including self-rated health, hypertension, physical limitations, asthma, or a summary health index combining different health measures (Orr et al., 2003; Kling, Liebman, and Katz, 2007).

Among youth ages 15–20 at the time of the interim study, the overall health index revealed worse health for males in the experimental group relative to those in the control group, but beneficial effects on female youth (Kling, Liebman, and Katz, 2007). However, the interim study found no effects on any single health measure examined separately for children and youth ages 5 and up (Orr et al., 2003). More detailed analyses of impacts on children's health by age confirmed the lack of overall health effects and revealed sporadic adverse effects for specific health measures and age groups, despite some beneficial effects on children's diet, exercise, and safety (Fortson and Sanbonmatsu, 2010). The only health mediator with a significant adverse impact was the greater presence of wall-to-wall carpeting, which can make it more difficult to eliminate asthma triggers, in the homes of the experimental and Section 8 groups.

3.3 BASELINE AND CONTROL GROUP CONTEXT

The baseline survey that HUD administered to families when they applied to MTO included only a handful of questions about health status. From these questions, we know that approximately 15 percent of families had a disabled household member (Exhibit 1.2), 8.4 percent were receiving Social Security Disability Insurance (SSDI), and 16 percent were receiving Supplemental Security Income (SSI) (Exhibit 5.1). Roughly 6 percent of the youth sample had a health problem that limited their activities, and 9 percent had a health problem requiring special medicine or equipment. Among youth under age 6 at baseline, parents reported that 19 percent had been hospitalized before their first birthday (Exhibit 1.3). At the time they entered the program, the majority of households were receiving Temporary Assistance for Needy Families (TANF), and about 25 percent of the adults indicated they were currently working. Families also expressed concerns about safety. Around 77 percent of adults said getting away from gangs or drugs was their primary or secondary reason for signing up for MTO, while 43 percent indicated someone in the household had been a victim of crime in the last six months (Exhibit 1.2).

One of the main changes affecting adults' health between the interim and final evaluations is that the adults are at least six years older at the final evaluation. A comparison of self-reports at interim data collection (2002) with the final collection (2008–2010) shows an apparent decline in the health of the control group. For example, the prevalence of obesity and self-reported physical limitations increased by 11 percentage points, the rate of asthma increased by 7 percentage points, and the likelihood that a control group adult rated his or her health as good or excellent declined by 10 percentage points.

3.4 DATA SOURCES AND MEASURES

We used self-reported data from the final adult and youth surveys, physical measurements of different health indicators, and dried blood spot samples (drops of whole

blood collected on filter paper following a simple finger stick; McDade et al., 2000) to measure impacts on physical health. Using objective physical and biological measures of health outcomes (called "biomarkers") addresses potential concerns that neighborhood environments themselves may affect self-reporting errors. For example, people may be less willing to self-report some health problem when that problem is relatively less common in the local community, they may be more likely to find out about some health problem if they live in a community with better access to health care, or their assessment of whether their health is good or excellent may depend in part on the average health conditions of other people in their community.[3] Another value of biomarkers as a complement to self-reported health outcomes is the ability to measure effects on hidden or undiagnosed health problems and precursors to long-term health problems that could be asymptomatic at the time of the final data collection. Previous research shows that nearly one-third of all diabetes cases are undiagnosed (Cowie et al., 2006); such cases, therefore, are unlikely to be detected through self-report but would be evident in biomarker data obtained from blood spot samples.

ADULT AND YOUTH SURVEY RESPONSES

Interviewers asked respondents a variety of questions about their health status, including physical limitations, health-related behaviors, morbidity, injuries, dental health, and access to health care. We selected questions from large national surveys (in particular, from the National Health Interview Survey) to measure, for example, general health status, dental health, injuries, asthma, height and weight, and smoking. The complete list of survey questions (and their sources) is available at mtoresearch.org.

SELF-REPORTED HEALTH OUTCOMES

Self-Rated Health: Self-reported health is strongly related to life expectancy among adults (Idler and Kasl, 1995; Idler and Benyamini, 1997). We measured self-reported health by asking respondents, "In general, how is your health: excellent, very good, good, fair, or poor?" Recent studies have noted that the strength of the relationship between self-rated health and other health

measures such as mortality and functional limitations may differ by the education level of the self-reporter. Some researchers have questioned whether self-rated health categories represent the same objective levels of health among respondents of different socioeconomic status (Delpierre et al., 2009; Dowd and Zajacova, 2010). Thus, one potential challenge in comparing self-rated health across MTO groups is that the families living in more affluent neighborhoods may judge their own health by higher standards (for example, more negatively relative to others), and this might lead to impact estimates that understate any benefits on health status.[4]

Diabetes: We asked respondents if a doctor had ever told them that they have diabetes or high blood sugar, if they were currently taking medication to treat diabetes or to lower their blood sugar, and if in the past 12 months they had diabetes or high blood sugar or received treatment for it. We present two self-reported measures of diabetes: (1) ever being told by a doctor they had diabetes/high blood sugar, and (2) having diabetes or having been treated for diabetes in the past 12 months. As noted above, underreporting of health problems could present a challenge in relying on self-reports. Moreover, the amount of the underreporting could vary across groups if, for example, experimental or Section 8 group families had more opportunities to interact with a doctor because of the program. This could lead to a larger share of experimental and Section 8 group than control group adults who have diabetes to know they have diabetes, thereby leading us to understate any beneficial impacts of MTO on these health outcomes. Partly to address this potential problem, we also collected blood spots from respondents and assayed them for diabetes and other health risks. (Similar issues arise with self-reported hypertension, which is why we also directly measure blood pressure as discussed below).

---

3 See, for example, Mango and Park (forthcoming) for a discussion of local context and self-reported health status.

4 It is possible that Hispanics interpret questions about self-reported health differently from African-Americans on average, or choose differently from available response options. These sorts of self-reporting issues should balance out in our experimental analysis since, by virtue of random assignment, the share of MTO program participants who are Hispanic vs. African-American are similar across randomized mobility groups.

Asthma: We asked adults and youth if they have ever been told by a doctor or other health professional that they had asthma and if they had experienced wheezing or whistling sounds in their chest in the past 12 months and how many attacks they had. We asked youth additional follow-up questions about missed days of work and school due to wheezing or whistling in their chest.

Health limitations, chronic pain, and chronic health problems: Measures of activities of daily living (ADLs) are used in a number of national surveys to assess an individual's functional status and quality of life (Wiener et al., 1990). We focused on ADLs that were likely to be relevant for our sample of largely middle-aged adults and asked them if their health limited them "in climbing several flights of stairs or lifting or carrying groceries" and considered them to have limitations if they indicated they were limited "a lot" or "a little" rather than "not limited at all." We also asked adults if they ever had arthritis or rheumatism. To gain a better understanding of the severity of illness and injuries, we also asked adults "how many days did illness or injury keep you in bed more than half of the day?" We drew questions on chronic pain from the National Comorbidity Survey (Kessler et al., 1994, 2005) and asked adults about severe or frequent headaches and about back, neck, or other chronic pain in the past year or ever. We asked youth between ages 13 and 20 if they ever had chronic back or neck problems, frequent or very bad headaches, or other chronic pain. We also asked them if a doctor had ever told them they have diabetes or high blood sugar or a serious stomach or bowel problem, such as an ulcer or colitis. We also asked the parents if their grown children had a physical health problem that kept them from doing normal activities such as walking, getting dressed, housework, or working, and if their grown children had chronic physical health problems such as cancer, a heart problem, or any other serious health problem.

Accidents and injuries: We asked youth about both serious accidents or injuries requiring medical care in the past 12 months and, in the event they did not seek treatment for injuries, about any other serious accidents or injuries during the past 12 months that limited their usual activities but did not require medical attention. Youth categorized the causes of accidents or injuries,

and we used this information to create a measure of nonsports injuries (relocation can affect opportunities for involvement with extracurricular or other athletic activities). Isolating the causal effect of neighborhood context on serious nonsports injuries is particularly important, given that injuries account for the two leading causes of death (accidents and homicide) in the United States among 15- to 19-year-olds (Anderson and Smith, 2003).

Smoking and drinking (adults): We asked adults if they had ever smoked a cigarette and, if so, on how many of the past 30 days they had smoked and how many cigarettes they usually smoke each day. We also asked them if they ever had an alcoholic beverage (defined as beer, wine, a mixed drink, or a shot of liquor) and on how many of the past 30 days they had one or more. These questions were adapted from the 1997 National Longitudinal Survey of Youth (analyses of impacts on youth smoking and drinking are presented in Chapter 6).

Dental health (youth): Similar to self-reports of overall health, we asked youth to describe the condition of their teeth (excellent, very good, good, fair, or poor). This question was adapted from the National Health and Nutrition Examination Survey.

SELF-REPORTED MEASURES OF CANDIDATE MEDIATORS

Exercise and sedentary behavior: We asked adults how many times in a usual week they engaged in moderate activities for at least 10 minutes at a time (such as brisk walking, bicycling, vacuuming, gardening, or anything else that causes small increases in breathing or heart rate) and how many times they did vigorous activities for at least 10 minutes at a time (such as running, aerobics, heavy yard work, or anything else that causes large increases in breathing or heart rate). We asked youth how many days within the past seven days they engaged in aerobic exercise (physical activity for at least 20 minutes that made you sweat and breathe hard, such as basketball, soccer, running, swimming, fast bicycling, fast dancing, or similar aerobic activities) and how many days they engaged in moderate exercise (physical activity for at least 30 minutes that did not make you sweat and breathe hard, such as fast walking, slow bicycling, skating,

pushing a lawn mower, or mopping floors). We gauged the amount of television adults and youth watch in a typical week by having them select from the following categories: less than 2 hours per week, 3–10 hours, 11–20 hours, 21–30 hours, 31–40 hours, or 40 hours or more.

Diet: We asked adults how many times they eat fruits and vegetables in a typical week. The possible responses were: I do not typically eat fruit; one to three times per week; four to six times per week; one time per day; two times per day; three times per day; and four times or more per day. Adults also reported the number of days per week that they typically ate fast food. The youth answered the same questions about eating fruits and vegetables, as well as questions about the number of times each week they drank soda or other carbonated drinks, soft drinks, or juices that contained sugar (diet drinks are excluded); ate sweet snacks like cookies, chocolate bars, or candy; and ate fast food. The possible responses for these questions were: I do not typically eat/drink [item]; one to three times per week; four to six times per week; one time per day; two times per day; three times per day; and four times or more per day.

Health care access: We asked adults to specify the types of health insurance or health care coverage they currently have for themselves (they could select multiple types of health insurance) and where they usually go (a clinic, doctor's office, emergency room, or some other place) when they need routine or preventive care. We asked how long it had been since they last saw a doctor about their own health, and where their children usually go for health care. In addition, we asked how many times they had been to the emergency room in the past 12 months because of their own health, and whether in the past 12 months they or their children did not get needed medical care for any of the following reasons: can't afford it, didn't have transportation, didn't know whom to see, or had to take care of family or friends. We also asked youth if they had a routine physical exam or a dental exam in the past year.

PHYSICAL BIOMARKERS

We directly measured various health outcomes in part to address concerns that MTO moves may affect self-reports

of health status (if respondents use their new, more affluent neighbors as their benchmark) or affect diagnosis of specific health problems if respondents are more likely to see a doctor. Physical health measures can also help detect the early stages of health problems that may not yet be symptomatic.

Height, weight, and obesity: We used protocols developed for the Health and Retirement Survey (Institute for Social Research, 2008) to measure adult and youth height and weight. For height, respondents removed their shoes and stood on a smooth surface with their heels and shoulders against a wall. Interviewers placed a rafter square on the respondent's head, marked the height on a wall, and then measured it in inches (to the nearest quarter inch) using a tape measure. For weight, respondents removed their shoes and heavy objects from their pockets, as well as any heavy outer layers of clothing before stepping on the scale. Interviewers used a digital floor scale to measure weight to the nearest half pound. If weight or height could not be measured or if quality checks revealed an unusual value, we obtained self-reports from the respondents. Using their height and weight, we calculated respondents' BMI equal to their weight in kilograms divided by height in meters-squared (BMI=kg/m2).[5]

In addition to examining impacts on mean BMI, overweight (defined as a BMI ≥25), and obesity (BMI ≥30), we also look at different parts of the BMI distribution, given previous evidence that having very high BMI values (≥35 or ≥40) may be strongly associated with subsequent adverse health outcomes (National Institutes of Health, National Heart, Lung and Blood Institute, and Obesity Education Initiative, 1998).

The standard BMI measure tends to work less well for children and adolescents because BMI tends to increase from birth through about age 20 as a natural byproduct of physical development. Instead, we define overweight and obesity for youth using criteria developed by the International Obesity Task Force (IOTF) (Cole et

---

5 Additional details about the measurement protocols, as well as a description of the handling of outlier values, are in the technical appendices (forthcoming) of this report.

al., 2000).[6] The IOTF uses growth curves from an international sample to create youth obesity measures by age and gender that align with the adult standards. Curves are further broken down by gender because while BMI tends to follow a linear trend for males, the trend line has more of a concave shape for females, and also because puberty tends to affect female bodies at different ages than males (Cole et al., 2000).

Waist circumference: Waist circumference is a measure of central obesity (abdominal or central fat) and has also been shown to be an important anthropometric correlate of cardiovascular risk, even after controlling for BMI. How weight is carried matters for health (Srinivasan et al., 2009; Wildman et al., 2008; Schneider et al., 2006; Depres and Lemieux, 2006; Lee et al., 2008). Central obesity has also been shown to be a major risk factor for type II diabetes (for more information, see discussion in National Institutes of Health, National Heart, Lung and Blood Institute, and Obesity Education Initiative, 1998). Respondents removed bulky outer clothing, and interviewers took waist measurements over their regular clothing. Interviewers asked the respondents to wrap a flexible measuring tape around their waist at navel level, inhale and exhale normally, and, while holding the exhale, hold the tape measure where the two ends met and then hand it to the interviewer, who recorded the measurement in inches.

Blood pressure: Interviewers took respondents' blood pressure using a large-sized automated blood pressure cuff (Omron automated sphygmomanometer model HEM-711DLX) designed to accommodate arm circumferences from 22 to 42 centimeters. Respondents sat at a table with both feet flat on the floor and their arm resting, palm up, on the table. The cuff was placed on the respondent's upper left arm, about half an inch above the elbow. Interviewers tried to collect two separate readings from each adult respondent. If two valid diastolic and systolic readings were obtained, we used

the average of each measure.[7] If only one valid reading was obtained, we used the single-reading values. We use the definitions suggested by the National Institutes of Health, National Heart, Lung and Blood Institute, and Obesity Education Initiative (2004) for pre-hypertension (systolic blood pressure of 120 mmHg or higher or diastolic blood pressure of 80 mmHg or higher) and hypertension (systolic pressure of 140 mmHg or higher, or diastolic pressure of 90 mmHg or higher; see also Chobanian et al., 2003). Because our self-reported hypertension measure does not distinguish between having hypertension and being treated for hypertension, a limitation of our study is our inability to distinguish between people who are being effectively treated for hypertension versus those with no hypertension.

DRIED BLOOD SPOT ASSAYS

Blood assays can potentially capture otherwise undetected health problems, such as undiagnosed diabetes, as well as longer-term risks of disease, such as cardiovascular disease (CVD). Interviewers asked participants if they could collect a small sample of their blood by pricking their fingers with a small instrument and then collecting enough blood to fill six small circles on a collection card. Respondents were offered an additional $25 incentive (on top of the incentive provided for completing the interview) for this blood sample collection. To avoid risks to participants, anyone who indicated that they had hemophilia or were taking anticoagulants or blood thinners was excluded from the blood spot collection. Among those interviewed and eligible for blood spot collection, the consent rate was over 90 percent.

Interviewers used a sterile, single-use lancet to prick the finger of the participant and collected up to six drops of blood on specimen paper, with one spot pretreated for the analysis of glycosylated hemoglobin (HbA1c). Interviewers allowed the blood spots to dry for 15 minutes and then placed them in a foiled envelope with a desiccant packet. Interviewers mailed the samples once a week to a central location for proper freezer storage at − 20°C to − 30°C. Because the interviews for Section 8 adults started about eight months after experimental

---

6 The Centers for Disease Control and Prevention has developed an alternative set of growth curves that are typically used to assess whether children are at risk of obesity. However, the growth curves apply only up to age 20 and thus would not apply to about 25 percent of our youth respondents who were over age 20 at the time of interview. As a result, we chose to use the IOTF criteria instead.

---

7 We considered a reading as valid if diastolic blood pressure was over 40 mmHg, the systolic blood pressure was over 60 mmHg, and the systolic was at least 10 points higher than the diastolic.

and control interviews, more of the Section 8 assays were done in the later batches by the laboratory.[8]

HbA1c and diabetes: In addition to our survey questions about diabetes that were discussed above, we measured the concentration of HbA1c from the dried blood spot specimens. This assessment provides an integrated measure of average blood glucose levels over several months prior to blood collection without requiring the respondent to fast. The American Diabetes Association (2010) recently recommended guidelines for use of HbA1c to diagnose and monitor diabetes, with a proposed HbA1c threshold of 6.5 percent for diagnosis. In addition to our two self-reported measures of diabetes, we created two additional measures using the HbA1c assays: diabetes as indicated by an elevated HbA1c level (≥ 6.5 percent) and a combined survey and blood measure of diabetes, defined as having either an elevated HbA1c level or self-reporting either having had diabetes or having been treated for diabetes during the past year. The combined measure is defined only for respondents with both survey and dried blood spot information. Because the self-reported diabetes measure does not distinguish between having diabetes and being treated for diabetes, a limitation of our study is the inability to fully differentiate between well-controlled diabetes (defined as a diagnosed case with normal HbA1c concentrations due to medication or other factors) and no diabetes.

Hs-CRP: The dried blood spot specimens were also assayed for high-sensitivity C-reactive protein (hs-CRP), a biomarker of inflammation that has recently emerged as an important predictor of cardiovascular disease (McDade, 2003; Ridker et al., 2002, 2003; Arima et al., 2008).[9]

3.5 LONG-TERM IMPACTS ON ADULT PHYSICAL HEALTH

SELF-RATED HEALTH STATUS
Exhibit 3.2 shows estimates for long-term effects on adult

---

8 More details about the dried blood spot data collection protocol and laboratory assays and calibration are in technical appendices, forthcoming.

9 Additional details on the assays and recalibration of hs-CRP using the results of a validation study will be described in technical appendices, forthcoming.

physical health. Fifty-six percent of adults in the control group rate their health as good, very good, or excellent; the others rate their health as only fair or poor. There is little difference in self-rated health across randomly assigned mobility groups. The estimated intention-to-treat effect (ITT) for the experimental group treatment is just 0.2 percentage points, with a standard error of 2 percentage points. These estimates enable us to rule out an experimental ITT effect of any more than 4 percentage points in either direction around zero. The top panel of Exhibit 3.3 shows that the distribution of adults across each of the different possible self-reported health categories (poor, fair, good, very good, and excellent) is similar for the treatment and control groups. As noted above, this health measure has the potential limitation of confounding the effects of MTO moves on health status with effects on the benchmarks that families use to think about what counts as good or excellent health.

OBESITY AND WAIST CIRCUMFERENCE
Although we do not see statistically significant experimental treatment effects on waist circumference, we estimate an experimental group ITT effect on average BMI of – 0.64, (p value, .07), which is equal to about 2 percent of the control group's mean BMI (32.8). The experimental treatment had no detectable impact at long-term follow-up on the likelihood of having a BMI ≥ 30, although we observed significant differences between the experimental and control groups at more severe levels of obesity. Estimates show that those in the experimental group were 4.6 percentage points less likely than control subjects to have a BMI of ≥ 35 (an effect that is equal to about 13 percent of the control group's prevalence of 35.1 percent), and they were 3.4 percentage points less likely to have a BMI of ≥ 40 (an effect equal to around 19 percent of the control group's prevalence of 17.5 percent).

For the Section 8 group, impacts on extreme obesity are of a similar magnitude, but only the effect on BMI ≥ 35 is statistically significant. The bottom panel of Exhibit 3.3 shows the implied distribution of each treatment group by obesity category. There are no statistically significant impacts of either group membership on waist circumference. In results not presented here, we find significantly fewer Section 8 adults than control group (27 percent versus 33 percent) with a waist circumference

above 43 inches (the 90th percentile for women based on the third National Health and Nutrition Examination Survey; Ford, Mokdad, and Giles, 2003). However, we do not detect any significant effects for the experimental group.

DIABETES AND HbA1c

Compared with the control group, the experimental group had statistically significantly lower levels of diabetes as measured by blood assays for HbA1c ≥ 6.5 percent (ITT effect of − 5.2 percentage points versus a control mean of 20.4 percent; see Exhibit 3.2). They were also lower when diabetes is measured by a combination of blood sample results and self-reported data (ITT of − 3.4 percentage points, versus a control mean prevalence of 23.4 percent; p value, .073). From self-reports of ever having been diagnosed with diabetes by a doctor, we find a nearly significant ITT effect of − 2.8 percentage points for the experimental group (control mean of 19.3 percent). Our analyses of self-reports of having had diabetes, currently taking medication for it, or having been treated for it during the past year showed a nonsignificant experimental ITT effect of − 2.4 percentage points versus a control mean of 16 percent.[10] Exhibit 3.2 provides evidence of under-reporting with the self-reported diabetes measure, given that 16 percent of controls report having diabetes or being treated for diabetes, while over 20 percent have HbA1c of 6.5 percent or higher as measured from blood samples.[11]

Adults in the Section 8 group were less likely than those in the control group to report that a doctor told them

they had diabetes (ITT of − 5.7 percentage points, which represents a 30 percent relative decline compared with the control group prevalence). Although we observe no significant increase within the Section 8 group of elevated HbA1c levels, we cannot statistically rule out that the impacts using the survey measures are different (at a statistically significant level) from the impacts estimated using HbA1c.[12] In addition, the timing of the dried blood spot assays differed between the Section 8 group and the control group; interviews with adults in the Section 8 group occurred, on average, two months after the control group interviews.

HYPERTENSION AND hs-CRP

We detect no statistically significant differences between the treatment and control groups on systolic or diastolic blood pressure, prehypertension, or hypertension. However, we observe a nearly significant difference of − 4.2 percentage points (p value, .08) for the experimental group on hs-CRP levels >3 mg/L, relative to a control mean of 58.6 percent. Previous research suggests that the relative risk of a major coronary event may be twice as high for people with hs-CRP level of greater than 3 mg/L, relative to those with a level of <1.0 mg/L (Pearson et al., 2003). For the Section 8 group, we do not observe any statistically significant effects of MTO on hypertension or hs-CRP.

ASTHMA

We detect no statistically significant effects on self-reported asthma for either treatment group. Some of our estimates have fairly wide 95 percent confidence intervals. For example, the Section 8 group ITT interval for having an asthma or wheezing attack during the past year ranges from − 9 to +1 percentage points, relative to a control mean of 29 percent. Thus, we cannot rule out moderate effects.

HEALTH LIMITATIONS AND CHRONIC PAIN

We observe no statistically significant difference across treatment groups on reports of health limitations, arthritis

---

10 Our combined measure using self-reported and HbA1c data is restricted to individuals with information from both sources. Alternatively, if we were to include everyone who had either self-reported or HbA1c data, the ITT estimates are -.036 for the experimental group (p < .05) and -.011 for the Section 8 group (not statistically significant) compared with a control mean of .259.

11 There is even more under-reporting of uncontrolled diabetes. Exhibit 3.2 shows that 16 percent of controls report having diabetes or being treated for diabetes, 20 percent of controls have HbA1c of 6.5 percent or higher, and 23 percent have either self-reported diabetes or treatment for diabetes or have HbA1c of 6.5 percent or higher. These figures imply that around 3 percent of the control group say they have diabetes or are being treated for it, but do not have HbA1c of 6.5 percent or higher– these are presumably control group adults who are being treated for diabetes. So 13 percent of the control group says they have diabetes and have HbA1c of 6.5 percent or higher as measured by blood samples, equal to about two-thirds of the total share of controls (20 percent) who have HbA1c of 6.5 percent or higher.

12 Note that the impact estimates using our survey measure of diabetes are similar regardless of whether we use the full set of respondents interviewed or restrict the sample to only respondents with HbA1c data (results not shown).

or rheumatism, headaches, chronic pain, or bed days, with two exceptions. The experimental group reported fewer health limitations than the control group, and the Section 8 group reported a higher prevalence of arthritis or rheumatism. Whether this estimated adverse Section 8 effect on arthritis / rheumatism represents a real MTO impact, a statistical fluke, or is because Section 8 adults are more likely to see a doctor and be diagnosed with arthritis or rheumatism is not known, although below we report results indicating that the Section 8 treatment did seem to improve people's access to medical care.

SMOKING AND DRINKING

Roughly one-third of adults in the control group reported that they currently smoked, and about 44 percent reported having had an alcoholic drink during the past month. The estimate points toward greater smoking for the experimental group, but is not statistically significant. Respondents in the experimental group reported slightly more days in which they had an alcoholic drink. The frequency of alcohol consumption is low across all three groups: less than two days a month, on average. (In Chapter 4, we present analyses of dependence on alcohol or drugs for the adult sample, and in Chapter 6, we present results for youth smoking and alcohol use.)

EXHIBIT 3.2. ADULT PHYSICAL HEALTH OUTCOMES						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
GOOD OR BETTER HEALTH CURRENTLY (VERSUS FAIR OR POOR) [SR]	0.564	0.002 (0.020)	0.004 (0.042)	− 0.005 (0.027)	− 0.009 (0.044)	3,269
OBESITY AND WAIST CIRCUMFERENCE						
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
Body Mass Index (BMI) [M, SR]	32.803	− 0.636~ (0.351)	− 1.311~ (0.724)	− 0.489 (0.491)	− 0.792 (0.796)	3,221
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
Waist circumference, in inches [M, SR]	40.868	− 0.273 (0.302)	− 0.563 (0.623)	− 0.543 (0.401)	− 0.867 (0.641)	3,177
DIABETES						
Ever told by a doctor they had diabetes or high blood sugar [SR]	0.193	− 0.028~ (0.016)	− 0.058~ (0.033)	− 0.057* (0.021)	− 0.091* (0.033)	3,251
Had diabetes or treated for it during the past year [SR]	0.160	− 0.024 (0.015)	− 0.049 (0.030)	− 0.061* (0.018)	− 0.098* (0.029)	3,251
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

EXHIBIT 3.2. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DIABETES (CONTINUED)						
HbA1c test detected diabetes or had or treated for diabetes during the past year [DBS, SR]	0.234	– 0.034~ (0.019)	– 0.071~ (0.040)	– 0.008 (0.025)	– 0.013 (0.039)	2,732
HYPERTENSION AND RISK OF CARDIOVASCULAR DISEASE						
Systolic blood pressure, mm Hg [M]	123.482	0.629 (0.877)	1.292 (1.802)	0.456 (1.121)	0.723 (1.778)	3,102
Diastolic blood pressure, mm Hg [M]	82.600	0.551 (0.577)	1.132 (1.186)	0.364 (0.744)	0.577 (1.180)	3,102
Currently has high blood pressure (systolic ≥ 140 mm Hg or diastolic ≥ 90 mm Hg) [M]	0.315	0.007 (0.020)	0.015 (0.042)	– 0.026 (0.027)	– 0.041 (0.043)	3,102
Currently has prehypertension or higher (systolic ≥ 120 mm Hg or diastolic ≥ 80 mm Hg) [M]	0.616	0.019 (0.021)	0.039 (0.043)	0.020 (0.027)	0.032 (0.043)	3,102
Ever prescribed high blood pressure medicine [SR]	0.382	0.013 (0.019)	0.027 (0.040)	0.018 (0.026)	0.029 (0.042)	3,261
Currently has high blood pressure or taking blood pressure medicine [M, SR]	0.479	0.009 (0.021)	0.018 (0.042)	– 0.012 (0.027)	– 0.019 (0.043)	3,158
High sensitivity C-reaction Protein at high risk level (> 3 mg/L) [DBS]	0.586	– 0.042~ (0.024)	– 0.087~ (0.049)	0.010 (0.030)	0.016 (0.048)	2,693
ASTHMA						
Has ever been told by a doctor that they had asthma [SR]	0.275	– 0.022 (0.019)	– 0.044 (0.039)	– 0.022 (0.025)	– 0.036 (0.040)	3,268
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
LIMITATIONS AND CHRONIC PAIN						
Health limits respondent's ability to climb several flights of stairs or lift or carry groceries [SR]	0.510	– 0.048* (0.021)	– 0.100* (0.043)	– 0.023 (0.028)	– 0.038 (0.045)	3,270
Ever had arthritis or rheumatism [SR]	0.304	0.017 (0.019)	0.036 (0.039)	0.051* (0.025)	0.081* (0.040)	3,259
Ever had severe or frequent headaches [SR]	0.410	– 0.006 (0.021)	– 0.013 (0.044)	– 0.039 (0.028)	– 0.063 (0.045)	3,267
Had severe or frequent headaches in past year [SR]	0.233	0.001 (0.019)	0.002 (0.038)	– 0.032 (0.025)	– 0.052 (0.039)	3,267
Ever had back, neck, or other chronic pain [SR]	0.440	– 0.024 (0.021)	– 0.050 (0.043)	– 0.010 (0.028)	– 0.016 (0.045)	3,266

	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
LIMITATIONS AND CHRONIC PAIN (CONTINUED)						
Back, neck, or other chronic pain in the past year [SR]	0.295	– 0.024 (0.019)	– 0.050 (0.040)	– 0.003 (0.026)	– 0.004 (0.042)	3,265
Number of days injury/illness kept adult in bed more than half the day [SR]	9.698	0.573 (1.575)	1.188 (3.266)	2.024 (2.251)	3.215 (3.575)	3,234
SMOKING AND DRINKING						
Currently a smoker [SR]	0.341	0.020 (0.020)	0.042 (0.042)	– 0.007 (0.027)	– 0.011 (0.043)	3,235
Ever drank an alcoholic beverage [SR]	0.602	0.046* (0.021)	0.095* (0.043)	– 0.037 (0.028)	– 0.058 (0.044)	3,239
Had alcoholic beverage in past 30 days [SR]	0.435	0.024 (0.021)	0.049 (0.044)	– 0.013 (0.028)	– 0.021 (0.045)	3,238
Fraction of days adult had alcoholic drink in past 30 days [SR]	0.053	0.011* (0.005)	0.022* (0.010)	0.004 (0.006)	0.006 (0.010)	3,238

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement, DBS = dried blood spot assays.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: For obesity inputs (height and weight), only a very small percent of the sample self-reported their height or weight. 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.

EXHIBIT 3.3. ADULT SELF-RATED HEALTH AND BMI CATEGORIES BY TREATMENT GROUP

A. Self-Rated Health of Adults by Treatment Group

B. BMI Categories for Adults by Treatment Group
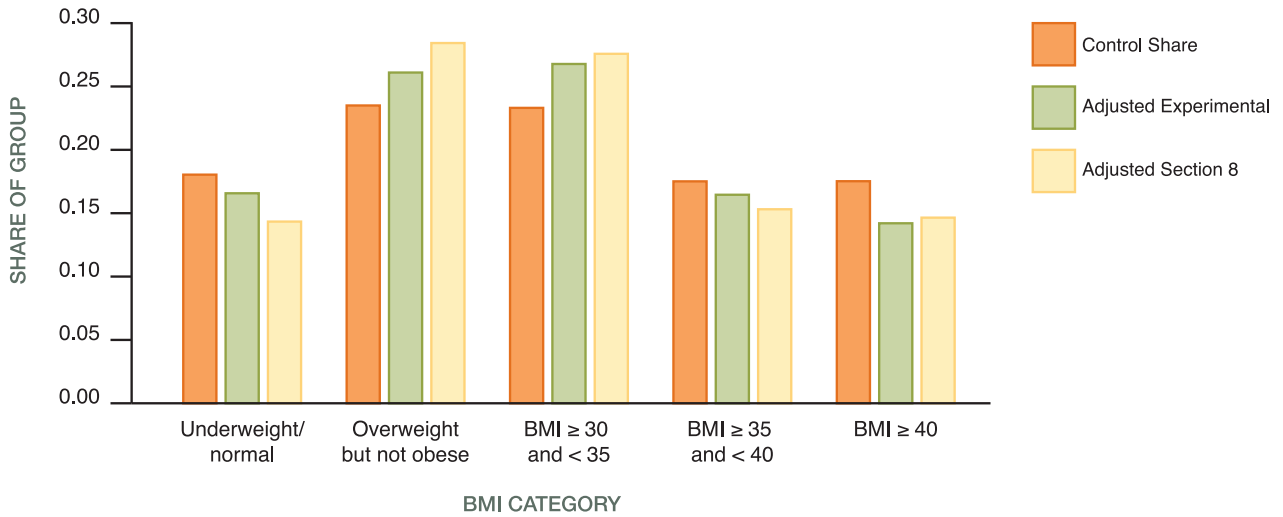
Notes: Share controls represents the unadjusted control mean. Adjusted experimental share represents the control mean plus the experimental ITT effect. Adjusted Section 8 share represents the control mean plus the Section 8 ITT effect. For obesity inputs (height and weight), only a very small percent of the sample self-reported their height or weight. BMI is measured as weight in kilograms divided by height in meters squared. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher.

Model: Experimental and Section 8 impacts were estimated jointly using an OLS regression model controlling for baseline covariates and field release, weighted, and clustering on family. Youth impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and technical appendices (forthcoming) for details.

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

3.6 LONG-TERM IMPACTS ON THE PHYSICAL HEALTH OF YOUTH AND GROWN CHILDREN

YOUTH AGES 10–20

Exhibit 3.4 shows the estimated effects of MTO on the physical health of youth.

Self-rated health status: Eighty-six percent of female youth and 90 percent of male youth in the control group rated their health as good, very good, or excellent. We detected no statistically significant differences between the treatment groups and control groups on this measure. As Exhibits 3.5 and 3.6 show, the distribution of categorical responses across treatment groups is similar for both females and males.

Obesity: Forty-five percent of youth in the control group met the IOTF criteria for overweight (designed to correspond to an adult BMI ≥25), and 23 percent met the criteria for obesity (designed to correspond to adult BMI ≥30). Control group females (27.4 percent) exhibited higher levels of obesity than do control group males (18.7 percent). There were no statistically significant effects of either treatment group overall or by gender on these outcomes, although we cannot rule out modest effects. For example, for female youth, we are only 95 percent confident that the experimental impact on being overweight is within the range of – 3.3 to +7.7 percentage points, and we are only 95 percent confident that the impact of obesity is between – 7.7 to +2.1 percentage points (that is, we cannot rule out that the treatment reduces obesity by as much as 28 percent relative to the control prevalence nor that it increases obesity by as much as 8 percent of the control mean).

Asthma: Nineteen percent of youth in the control group reported an asthma or wheezing attack during the past year, and 7 percent reported that wheezing limited their activities or led them to miss school or work. We find no statistically significant effects of either treatment on asthma and wheezing attacks, overall or by gender.

Accidents, injuries, chronic pain, and serious illness: We detect no significant differences between the

treatment and control groups on accidents and injuries in the past year, or on the prevalence of chronic pain, bad headaches, or serious illnesses such as diabetes or stomach problems. More than one-third of youth ages 13–20 in the control group reported chronic pain or frequent headaches. Although not statistically significant, MTO moves tend to reduce the prevalence of accidents and injuries for female youth relative to those in the control group. However, they tend to increase the prevalence for male youth, as we found in the interim study.

Dental health: We detect no differences across treatment groups in terms of whether youth report that their teeth are in "very good" or "excellent" condition rather than "good," "fair," or "poor" condition. We detect no impacts by gender on this measure. Across the full range of cut-points, we generally do not detect effects. However, a statistically significantly higher proportion of female youth in the experimental group reported that their teeth were in excellent condition than do controls.

EXHIBIT 3.4. YOUTH PHYSICAL HEALTH OUTCOMES						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
GENERAL HEALTH						
Good or better health currently (versus fair or poor) [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						
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)	
Wheezing caused limited activities or missed school or work in the past year [SR]						
All	0.071	0.003	0.007	− 0.012	− 0.018	5,091
		(0.010)	(0.021)	(0.010)	(0.015)	
Female	0.079	− 0.001	− 0.002	− 0.008	− 0.012	2,594
		(0.014)	(0.028)	(0.015)	(0.023)	
Male	0.064	0.008	0.017	− 0.017	− 0.025	2,497
		(0.014)	(0.029)	(0.013)	(0.018)	
OBESITY						
Currently overweight [M, SR]						
All	0.449	0.015	0.031	0.009	0.013	5,034
		(0.020)	(0.042)	(0.022)	(0.032)	
Female	0.482	0.022	0.045	0.039	0.061	2,560
		(0.028)	(0.056)	(0.030)	(0.048)	
Male	0.418	0.008	0.016	− 0.022	− 0.031	2,474
		(0.027)	(0.059)	(0.029)	(0.042)	
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)	

EXHIBIT 3.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
ACCIDENTS AND INJURIES						
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)	
Had serious accidents or injuries that required medical attention or limited activities in the past year [SR]						
All	0.273	0.001	0.002	0.006	0.009	5,092
		(0.017)	(0.035)	(0.019)	(0.028)	
Female	0.233	− 0.024	− 0.049	− 0.011	− 0.017	2,595
		(0.022)	(0.044)	(0.024)	(0.038)	
Male	0.309	0.026	0.056	0.022	0.031	2,497
		(0.025)	(0.054)	(0.028)	(0.040)	
Had a nonsports accident or injury requiring medical attention in the past year [SR]						
All	0.170	0.011	0.024	0.016	0.024	5,074
		(0.014)	(0.030)	(0.016)	(0.024)	
Female	0.176	− 0.015	− 0.030	0.001	0.002	2,584
		(0.020)	(0.039)	(0.022)	(0.034)	
Male	0.165	0.038~	0.082~	0.030	0.042	2,490
		(0.020)	(0.044)	(0.023)	(0.032)	
CHRONIC PAIN AND SERIOUS ILLNESS AMONG YOUTH 13–20						
Have ever had chronic back, neck, or other pain or frequent/very bad headaches [SR]						
All	0.371	− 0.002	− 0.004	0.017	0.025	4,628
		(0.019)	(0.039)	(0.021)	(0.032)	
Female	0.432	− 0.019	− 0.039	− 0.010	− 0.015	2,362
		(0.027)	(0.054)	(0.031)	(0.048)	
Male	0.313	0.015	0.033	0.043	0.062	2,266
		(0.027)	(0.057)	(0.030)	(0.042)	
Have ever had serious illness like diabetes, high blood sugar, and serious stomach problem [SR]						
All	0.044	− 0.013	− 0.026	− 0.010	− 0.015	4,627
		(0.008)	(0.016)	(0.009)	(0.014)	
Female	0.056	− 0.016	− 0.033	− 0.020	− 0.031	2,359
		(0.012)	(0.025)	(0.014)	(0.021)	
Male	0.033	− 0.009	− 0.019	0.000	0.001	2,268
		(0.009)	(0.020)	(0.012)	(0.017)	

EXHIBIT 3.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
DENTAL						
Teeth are in very good or better condition [SR]						
All	0.400	– 0.004	– 0.008	0.000	0.000	5,095
		(0.019)	(0.039)	(0.020)	(0.030)	
Female	0.379	0.021	0.042	0.026	0.040	2,596
		(0.025)	(0.051)	(0.028)	(0.043)	
Male	0.419	– 0.028	– 0.060	– 0.025	– 0.036	2,499
		(0.026)	(0.057)	(0.029)	(0.041)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: Overweight and obesity are defined according to the International Obesity Task Force.

SUBSET OF YOUTH AGES 15–20

Supplemental Exhibit 3.1 presents impacts on youth ages 15–20 at the end of 2007. This allows us to compare current results with interim results in 2001 reported by Kling, Liebman, and Katz (2007). This comparison holds constant the ages at which we measure the health outcomes, and examines whether the impact is different for children who were relatively younger at the time of MTO random assignment (than the sample analyzed by Kling, Liebman, and Katz) and thus had spent a relatively larger share of their lives in lower-poverty neighborhoods before we measured health outcomes. In principle, this comparison will confound differences in how MTO effects vary by someone's age at baseline with differences in how effects vary by cohort and the calendar years in which the health outcomes are measured.

With that last caveat in mind, we detect no effects on overall health or self-reported asthma among this older age group, but the results indicate less serious or chronic illness among youth in the experimental group (p value, .03), more accidents and injuries requiring medical attention for Section 8 males (p value, .03), and hint at possibly lower obesity (ITT of – 4.9 percentage points, p value, .10) for female youth in the experimental group compared with female controls. Estimates for the smaller sample of youth ages 10–14 (not shown) are imprecise, with some estimates having the opposite sign from the older youth.

GROWN CHILDREN AGES 21–30

Exhibit 3.7 presents our results on the physical health of grown children. Parents in the control group reported that 4.5 percent of their grown children had physical health problems that kept them from normal activities, and 7.9 percent had a chronic health problem, such as cancer or a heart problem. We do not detect any significant effects for the experimental group or for females in either group. However, we do observe a significantly higher prevalence of health limitations among Section 8 males relative to males in the control group (ITT of 4.9 percent relative to a control mean of 4.3 percent).

EXHIBIT 3.5. FEMALE YOUTH SELF-RATED HEALTH AND BMI CATEGORIES BY TREATMENT GROUP

A. Female Youth Self-Rated Health by Treatment Group

B. Female Youth BMI Categories by Treatment Group

Notes: Share controls represents the unadjusted control mean. Adjusted experimental share represents the control mean plus the experimental ITT effect. Adjusted Section 8 share represents the control mean plus the Section 8 ITT effect. For obesity inputs (height and weight), only a very small percent of the sample self-reported their height or weight. BMI is measured as weight in kilograms divided by height in meters squared. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher.
Data source and sample: Youth long-term survey. Interviewed female youth ages 10 to 20 as of December 31, 2007.
Model: Experimental and Section 8 impacts were estimated jointly using an OLS regression model controlling for baseline covariates and field release, weighted, and clustering on family. Youth impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and technical appendices (forthcoming) for details.

EXHIBIT 3.6. MALE YOUTH SELF-RATED HEALTH AND BMI CATEGORIES BY TREATMENT GROUP

A. Male Youth Self-Rated Health by Treatment Group

B. Male Youth BMI Categories by Treatment Group

Notes: Share controls represents the unadjusted control mean. Adjusted experimental share represents the control mean plus the experimental ITT effect. Adjusted Section 8 share represents the control mean plus the Section 8 ITT effect. For obesity inputs (height and weight), only a very small percent of the sample self-reported their height or weight. BMI is measured as weight in kilograms divided by height in meters squared. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher.

Data source and sample: Youth long-term survey. Interviewed female youth ages 10 to 20 as of December 31, 2007.

Model: Experimental and Section 8 impacts were estimated jointly using an OLS regression model controlling for baseline covariates and field release, weighted, and clustering on family. Youth impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and technical appendices (forthcoming) for details.

EXHIBIT 3.7. PARENT-REPORTED PHYSICAL HEALTH OUTCOMES OF GROWN CHILDREN AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
PHYSICAL LIMITATIONS						
Physical health problem keeps from normal activities like walking, housework, or working [PR]						
All	0.045	0.014	0.031	0.026~	0.047~	3,096
		(0.010)	(0.023)	(0.014)	(0.025)	
Female	0.048	0.013	0.029	0.001	0.001	1,525
		(0.014)	(0.032)	(0.018)	(0.031)	
Male	0.043	0.014	0.034	0.049*	0.093*	1,571
		(0.014)	(0.034)	(0.022)	(0.041)	
CHRONIC HEALTH PROBLEM						
Has a chronic physical health problem such as cancer, a heart problem, or any other serious health problem [PR]						
All	0.079	0.003	0.006	– 0.017	– 0.030	3,096
		(0.013)	(0.030)	(0.015)	(0.028)	
Female	0.080	0.008	0.018	– 0.023	– 0.040	1,526
		(0.018)	(0.040)	(0.021)	(0.036)	
Male	0.078	– 0.003	– 0.006	– 0.011	– 0.021	1,570
		(0.017)	(0.039)	(0.020)	(0.039)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. Grown children (who were under age 18 at baseline and over age 20 as of December 31, 2007) of interviewed adults.

3.7 IMPACTS ON HYPOTHESIZED MEDIATORS OF HEALTH

PHYSICAL ENVIRONMENT

Exhibits 2.2 through 2.9 in Chapter 2 showed statistically significant impacts on a range of housing and neighborhood conditions that could affect health. Adults in the experimental group rated the condition of their housing more favorably than did control subjects, and both treatment groups reported fewer problems with vermin (a problem that can aggravate asthma) and peeling paint or plaster, as well as greater satisfaction with their neighborhoods than their control group counterparts. Interviewers' observations of the neighborhoods suggest persons in the experimental group lived in neighborhoods with less litter than did control subjects, which was also statistically significant.

EXERCISE AND NUTRITION

Supplemental Exhibit 3.2 presents estimated effects of the MTO treatments on adult health behaviors related to exercise, television viewing, sleeping, and consumption of fruits and vegetables, soda, and sweets. We detect no statistically significant impacts on these mediators, with the exceptions that adults in the experimental group were 4.6 percentage points more likely to exercise vigorously at least once a week compared with controls, and adults in the Section 8 group were 4.8 percentage points less likely than control subjects to report long sleep (defined as >10 hours in the previous night). This is a large change compared with the control mean of 9.9 percent. There is no consistent pattern in the effects on healthy eating. In results not shown, we looked at effects for different cut points on the food consumption questions. In the rare instances that we detect differences across groups, the effects tend to be toward less healthy eating for the experimental group relative to the control group.

Supplemental Exhibit 3.3 shows little evidence of a clear effect of moving to different neighborhoods on youth exercise levels, eating habits, and sleep patterns. To the extent to which any of these impacts are statistically robust at 90 percent significance, they point in contradictory directions. For example, compared with those in the control group, youth in the experimental group may be somewhat less likely to eat fruits and vegetables four times per week (a nonsignificant effect suggesting less healthy eating), but male youth may be less likely to eat sweets four times per week (a nonsignificant effect suggesting more healthy eating).

ACCESS TO CARE

Supplemental Exhibit 3.4 shows how MTO affects access to health care for adults (top panel) and youth (bottom panel). Although there are no statistically significant effects of the experimental treatment on adult access to health care, we see some signs of improved access for Section 8 adults, relative to control adults. For example, the Section 8 ITT effect on whether the adult and his or her children have health insurance is 3.5 percentage points (p value, .096), relative to a control mean of 82.7 percent. The Section 8 group did not differ significantly from the control group in terms of whether they got medical care when needed. However, they were less likely to report not receiving health care during the past 12 months specifically because they could not afford it (ITT of – 2.6 percentage points, versus a control mean of 6.1 percent). For female youth, being in the experimental group seems to have lessened the likelihood of receiving a routine physical exam in the past year. Even though most of the estimates on access to health care are not statistically significant for adults or youth, our statistical power is somewhat limited, and so the 95 percent confidence interval of some of the ITTs encompasses moderate-sized effects.

SOCIAL SUPPORTS AND SOCIAL ISOLATION

The results presented in Chapter 2 (and Supplemental Exhibits 2.4 and 2.5) suggest that the experimental group adults could have increased exposure to more affluent peers, but that youth may be more socially isolated as a result of their move. Specifically, for adults, assignment to the experimental group rather than control group did not change the likelihood of having three or more friends, but did increase the share of adults who reported that they had a friend who was a college graduate. For youth, those assigned to the experimental group were more likely than controls to report they had no close friends.

SAFETY AND STRESS

Exhibit 2.10 suggests that adults in both treatment groups feel safer in their current neighborhoods than do control subjects. They are also less likely to have seen people selling or using drugs in their neighborhood in the past month. Similarly, female youth assigned to the treatment groups rather than to the control group reported improved neighborhood safety and less crime victimization, and those assigned to the experimental group reported less unwanted sexual attention. In contrast, there is no statistically significant effect on safety for male youth. In fact, the two effects with borderline significance (p values < .10) for males in the Section 8 group indicate that they are less safe and experience more gang activity in their new neighborhoods and schools compared with control group males.

3.8 INTERPRETATION OF PHYSICAL HEALTH RESULTS

We have examined the effects of MTO on the health outcomes of adults, youth, and grown children. For adults, our analyses of the long-term survey data and biomarkers suggest somewhat better health among the experimental group adults relative to control subjects in terms of fewer physical limitations and lower prevalence of extreme obesity and diabetes. We also observe higher levels of drinking for the experimental group adults compared with control subjects.[13] We do not detect any statistically significant effects on other health outcomes, such as self-rated health, hypertension, asthma, or chronic pain. In interpreting these null effects, it is important to bear in mind the size of effects that can be ruled out, which varies depending on the measure. For some measures, our 95 percent confidence intervals do not allow us to rule out moderately sized impacts in either direction (toward better or worse health as a result of MTO moves).

Whereas at the interim evaluation, four to seven years after random assignment, results revealed lower obesity

(BMI ≥ 30) for adults in the experimental group relative to the control group (Kling, Liebman, and Katz, 2007), results at 10–15 years show statistically significant experimental group impacts only at more severe levels of obesity (BMI ≥ 35 and BMI ≥ 40). This may be because obesity is now so common across the MTO adult sample—fully 58 percent of adults in the control group were obese at the time of our long-term data collection, whereas 47 percent of controls were obese at the time of the interim follow-up (Orr et al., 2003). We know that obesity is associated with other chronic health problems, including heart disease, cancer, and, in particular, diabetes. These comorbidities were not measured at interim, but the blood spots collected as part of our long-term study now enable us to confirm that reducing extreme obesity is accompanied by reductions in the prevalence of diabetes as well.

Our ability to physically measure a variety of health outcomes as part of the long-term study is an important enhancement over previous waves of MTO research. In general, we might prefer measured health outcomes to self-reported outcomes because of the possibility of self-report error. In the case of MTO, we might also be concerned that neighborhoods could affect self-report error. One issue is that access to doctors might vary across neighborhoods and lead to differences in diagnosis. In part to address this problem, we collected biomarker data and for self-reported measures such as asthma and injuries. We also asked respondents about symptoms rather than simply medical diagnoses in an effort to minimize any potential bias. Another concern with self-reports is that neighborhoods could affect one's frame of reference or expectations regarding one's health. In theory, a person's rating of her health status may reflect both her objective health status and the health level she could expect to achieve (Adams and White, 2006). Although adults in the experimental group are more likely than control subjects to report having a close friend who is a college graduate, it seems unlikely that these types of changes in peer networks are large enough to explain the lack of differences observed between treatment and control groups on self-rated health.

The possibility of differing mortality or other health-related attrition across groups is another source of

---

13 The control group's reports of ever drinking alcohol (60 percent) and drinking in the past month (44 percent) are low but consistent with published rates for minorities and for people with lower levels of education (for example, see Substance Abuse and Mental Health Services Administration, 2010).

potential bias. We think it is unlikely that mortality affects our impact estimates because our survey response rates are high, our extensive tracking efforts reveal similar rates of mortality across the treatment groups, and interviewers traveled to participants rather than requiring participants to travel to them (making it easier for participants with health limitations to participate in the follow-up).

In our analyses of health outcomes for youth ages 10–20, we do not detect statistically significant effects on self-rated health, asthma, obesity, injuries, chronic pain, serious illness, or very good dental health. Echoing the interim results, analyses restricted to only the older youth (ages 15–20) suggest some beneficial effects for the females in the experimental group and negative effects for the males in the Section 8 group, although these results should be interpreted with caution, as some of the results are in the opposite direction of those found for youth ages 10–14. In our analyses of parental reports on grown children who are now ages 21–30, we observe no detectable effects on chronic health problems of moving to a lower-poverty neighborhood, but we do observe a higher rate of health limitations for males in the Section 8 group relative to control-group males.

We find little effect on most health mediators, except for safety and stress, which makes us think that safety and stress could potentially be the key mechanisms for the effects on obesity and diabetes that we observe for adults. At the interim report, we saw various effects on exercise and eating of fruits and vegetables of moving to lower-poverty neighborhoods, whereas at the final evaluation we only see an impact on vigorous exercise for the experimental group. Of note, the wording and response categories on some of the exercise and diet questions differ between the interim and final surveys.[14]

As with the adult health mediators, we detect few differences on the eating habits, exercise, or sedentary

behaviors of youth across treatment groups. Female youth in the treatment groups felt greater safety relative to control subjects, and the experimental group experienced less psychological distress. For male youth in the Section 8 group, some of our estimates point toward lower perceptions of safety. (See Chapter 2 for the safety results and Chapter 4 for the mental health results.)

In conclusion, the effects of MTO on adult or youth health outcomes are mixed, with relatively few statistically significant health impacts across a range of outcomes. However, the impacts that we do observe for adults involve some of the most important health outcomes—specifically, obesity and diabetes—and as such speak to the potential public health importance of these findings.

---

14 At interim, we asked adults and youth, "In a typical week, how many days do you eat at least some green vegetables or fruit?" At final, we asked adults, "How many times do you eat fruit and vegetables other than french fries or potato chips" (from the National Longitudinal Survey of Youth 1997). The possible responses were "I do not typically eat fruit," "1 to 3 times per week," "4 to 6 times per week," "1 time per day," "2 times per day," "3 times per day," and "4 times or more per day."

SUPPLEMENTAL EXHIBIT 3.1. PHYSICAL HEALTH OUTCOMES OF YOUTH AGES 15–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
GENERAL HEALTH						
Good or better health currently (versus fair or poor) [SR]						
All	0.871	0.010 (0.016)	0.020 (0.033)	0.008 (0.017)	0.012 (0.026)	3,621
Female	0.851	0.015 (0.022)	0.031 (0.045)	0.016 (0.025)	0.025 (0.039)	1,845
Male	0.890	0.004 (0.020)	0.009 (0.043)	0.001 (0.023)	0.001 (0.033)	1,776
ASTHMA						
Asthma or wheezing attack during the past year [SR]						
All	0.187	0.000 (0.018)	0.000 (0.037)	− 0.017 (0.019)	− 0.025 (0.029)	3,616
Female	0.217	− 0.025 (0.025)	− 0.051 (0.050)	− 0.031 (0.027)	− 0.048 (0.043)	1,842
Male	0.159	0.025 (0.024)	0.054 (0.051)	− 0.003 (0.026)	− 0.005 (0.038)	1,774
Wheezing caused limited activities or missed school/work for youth in past year [SR]						
All	0.067	− 0.003 (0.012)	− 0.006 (0.024)	− 0.018 (0.011)	− 0.027 (0.017)	3,616
Female	0.077	− 0.009 (0.016)	− 0.019 (0.032)	− 0.013 (0.017)	− 0.020 (0.026)	1,842
Male	0.057	0.004 (0.016)	0.009 (0.034)	− 0.023 (0.015)	− 0.033 (0.021)	1,774
OBESITY						
Currently overweight [M, SR]						
All	0.463	0.012 (0.023)	0.026 (0.049)	− 0.001 (0.026)	− 0.002 (0.039)	3,580
Female	0.495	0.025 (0.032)	0.050 (0.065)	0.031 (0.036)	0.047 (0.056)	1,819
Male	0.432	0.000 (0.032)	0.001 (0.070)	− 0.032 (0.035)	− 0.047 (0.051)	1761
Currently obese [M, SR]						
All	0.246	− 0.016 (0.020)	− 0.034 (0.041)	− 0.013 (0.022)	− 0.020 (0.033)	3,580
Female	0.301	− 0.049 (0.030)	− 0.098 (0.060)	− 0.032 (0.032)	− 0.050 (0.049)	1,819
Male	0.193	0.017 (0.025)	0.036 (0.054)	0.005 (0.028)	0.007 (0.040)	1,761

SUPPLEMENTAL EXHIBIT 3.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
ACCIDENTS AND INJURIES						
Had accidents/injuries requiring medical attention in the past year [SR]						
All	0.166	0.012	0.024	0.025	0.038	3,618
		(0.017)	(0.035)	(0.019)	(0.028)	
Female	0.156	− 0.022	− 0.045	− 0.014	− 0.022	1,842
		(0.022)	(0.044)	(0.024)	(0.037)	
Male	0.175	0.045~	0.097~	0.064*	0.094*	1,776
		(0.025)	(0.054)	(0.028)	(0.041)	
Had serious accidents/injuries that required medical attention or limited activities in the past year [SR]						
All	0.247	0.014	0.029	0.034	0.050	3,615
		(0.019)	(0.040)	(0.022)	(0.034)	
Female	0.210	− 0.015	− 0.031	0.020	0.032	1,841
		(0.025)	(0.051)	(0.029)	(0.045)	
Male	0.282	0.044	0.094	0.046	0.066	1,774
		(0.029)	(0.062)	(0.032)	(0.046)	
Had a non-sports accident/injury requiring medical attention in the past year [SR]						
All	0.159	0.015	0.032	0.035~	0.053~	3,603
		(0.017)	(0.034)	(0.019)	(0.029)	
Female	0.164	− 0.006	− 0.012	0.020	0.031	1,832
		(0.023)	(0.046)	(0.026)	(0.040)	
Male	0.153	0.036	0.078	0.050~	0.072~	1,771
		(0.023)	(0.050)	(0.027)	(0.039)	
CHRONIC PAIN AND SERIOUS ILLNESS						
Ever had chronic back, neck or other pain or frequent/very bad headaches [SR]						
All	0.366	− 0.001	− 0.003	0.011	0.016	3,612
		(0.021)	(0.044)	(0.024)	(0.036)	
Female	0.432	− 0.028	− 0.057	− 0.027	− 0.043	1,839
		(0.031)	(0.062)	(0.035)	(0.054)	
Male	0.305	0.025	0.054	0.049	0.070	1,773
		(0.029)	(0.063)	(0.033)	(0.047)	
Ever had serious illness like diabetes, high blood sugar, and serious stomach problem [SR]						
All	0.051	− 0.021*	− 0.043*	− 0.018	− 0.026	3,610
		(0.009)	(0.019)	(0.011)	(0.016)	
Female	0.066	− 0.027~	− 0.054~	− 0.030~	− 0.047~	1,836
		(0.015)	(0.029)	(0.016)	(0.025)	
Male	0.037	− 0.015	− 0.031	− 0.005	− 0.007	1,774
		(0.011)	(0.024)	(0.014)	(0.020)	

SUPPLEMENTAL EXHIBIT 3.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
DENTAL						
Teeth are in very good or better condition [SR]						
All	0.390	0.004	0.009	0.023	0.034	3,617
		(0.022)	(0.046)	(0.024)	(0.036)	
Female	0.374	0.018	0.036	0.035	0.055	1,842
		(0.030)	(0.060)	(0.033)	(0.052)	
Male	0.404	− 0.010	− 0.020	0.010	0.015	1,775
		(0.032)	(0.068)	(0.034)	(0.050)	

Notes: * = p < 0.05, ~ = p <0 .10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 15 to 20 as of December 31, 2007.
Measures: Overweight and obesity are defined according to the International Obesity Task Force.

SUPPLEMENTAL EXHIBIT 3.2. ADULT HEALTH BEHAVIORS: EXERCISE, NUTRITION, AND SLEEP						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EXERCISE AND INACTIVITY						
Number of times per week engages in moderate physical activity [SR]	3.877	– 0.144	– 0.298	– 0.292	– 0.465	3,253
		(0.143)	(0.294)	(0.189)	(0.302)	
Engages in moderate physical activity ≥ four times a week [SR]	0.487	– 0.014	– 0.029	– 0.012	– 0.020	3,253
		(0.022)	(0.045)	(0.029)	(0.046)	
Number of times per week engages in vigorous physical activity [SR]	1.483	0.010	0.021	0.006	0.010	3,263
		(0.098)	(0.203)	(0.133)	(0.212)	
Engages in 10 or more minutes of vigorous physical activity at least one time per week [SR]	0.386	0.046*	0.094*	0.010	0.015	3,263
		(0.021)	(0.043)	(0.028)	(0.045)	
Watches ≥ 11 hours of television per week [SR]	0.483	– 0.002	– 0.003	– 0.037	– 0.059	3,250
		(0.022)	(0.045)	(0.029)	(0.046)	
NUTRITION						
Eats fruit or vegetables ≥ four times per week [SR]	0.566	0.016	0.034	0.038	0.061	3,268
		(0.021)	(0.044)	(0.028)	(0.045)	
Eats fast food ≥ one times per week [SR]	0.614	0.005	0.011	0.004	0.007	3,262
		(0.020)	(0.042)	(0.027)	(0.043)	
SLEEP						
Short sleep the previous night (< 6 hours) [SR]	0.194	– 0.017	– 0.035	– 0.004	– 0.006	3,241
		(0.017)	(0.034)	(0.022)	(0.036)	
7–8 hours of sleep the previous night ("normal sleep") [SR]	0.291	0.015	0.031	0.020	0.033	3,241
		(0.020)	(0.042)	(0.027)	(0.044)	
Long sleep the previous night (> 10 hours) [SR]	0.099	– 0.013	– 0.026	– 0.048*	– 0.077*	3,241
		(0.013)	(0.026)	(0.015)	(0.024)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

SUPPLEMENTAL EXHIBIT 3.3. YOUTH HEALTH BEHAVIORS: EXERCISE, NUTRITION, AND SLEEP						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EXERCISE AND INACTIVITY						
Fraction of past 7 days engaged in physical activity for ≥ 20 minutes that made youth sweat/breathe hard [SR]						
All	0.440	0.006	0.012	0.013	0.019	5,082
		(0.012)	(0.025)	(0.014)	(0.020)	
Female	0.350	0.010	0.021	0.019	0.030	2,589
		(0.017)	(0.034)	(0.019)	(0.030)	
Male	0.525	0.001	0.003	0.006	0.009	2,493
		(0.018)	(0.038)	(0.020)	(0.028)	
Fraction of past 7 days engaged in physical activity for ≥ 30 minutes that did not make youth sweat/breathe hard [SR]						
All	0.393	− 0.005	− 0.010	0.006	0.010	5,076
		(0.014)	(0.029)	(0.015)	(0.023)	
Female	0.397	− 0.009	− 0.018	− 0.006	− 0.009	2,587
		(0.019)	(0.038)	(0.021)	(0.032)	
Male	0.390	0.000	− 0.001	0.019	0.027	2,489
		(0.020)	(0.042)	(0.022)	(0.031)	
Watches ≥ 11 hours of television per week [SR]						
All	0.317	0.003	0.006	− 0.009	− 0.014	5,052
		(0.018)	(0.038)	(0.020)	(0.030)	
Female	0.302	0.014	0.029	− 0.012	− 0.018	2,577
		(0.024)	(0.049)	(0.027)	(0.042)	
Male	0.330	− 0.009	− 0.019	− 0.007	− 0.009	2,475
		(0.026)	(0.057)	(0.029)	(0.041)	
NUTRITION						
Eats fruit or vegetables ≥ four times per week [SR]						
All	0.536	− 0.036~	− 0.075~	− 0.017	− 0.026	5,091
		(0.020)	(0.041)	(0.021)	(0.032)	
Female	0.528	− 0.039	− 0.078	0.018	0.028	2,595
		(0.027)	(0.055)	(0.030)	(0.047)	
Male	0.544	− 0.032	− 0.069	− 0.054~	− 0.077~	2,496
		(0.028)	(0.060)	(0.030)	(0.042)	
Drinks soda or other sugary drinks like juice (diet soda excluded) ≥ four times per week [SR]						
All	0.639	− 0.017	− 0.035	0.018	0.027	5,094
		(0.019)	(0.040)	(0.020)	(0.030)	
Female	0.627	0.001	0.001	0.011	0.017	2,596
		(0.026)	(0.053)	(0.028)	(0.044)	
Male	0.650	− 0.035	− 0.075	0.027	0.038	2,498
		(0.026)	(0.056)	(0.028)	(0.040)	
Eats sweet snacks ≥ 4 times per week [SR]						
All	0.598	− 0.018	− 0.038	− 0.010	− 0.015	5,093
		(0.019)	(0.039)	(0.021)	(0.031)	

SUPPLEMENTAL EXHIBIT 3.3. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Eats sweet snacks ≥ four times per week [SR] (CONTINUED)						
Female	0.581	0.009	0.017	0.010	0.015	2,596
		(0.026)	(0.052)	(0.028)	(0.044)	
Male	0.614	− 0.045~	− 0.097~	− 0.029	− 0.041	2,497
		(0.026)	(0.056)	(0.029)	(0.042)	
Eats fast food ≥ four days per week [SR]						
All	0.259	− 0.012	− 0.024	0.010	0.015	5,093
		(0.018)	(0.037)	(0.019)	(0.028)	
Female	0.233	0.000	− 0.001	− 0.003	− 0.005	2,595
		(0.023)	(0.046)	(0.025)	(0.040)	
Male	0.283	− 0.023	− 0.050	0.024	0.034	2,498
		(0.025)	(0.054)	(0.027)	(0.039)	
SLEEP						
Hours of sleep last night [SR]						
All	8.479	− 0.011	− 0.023	0.053	0.080	5,073
		(0.081)	(0.169)	(0.091)	(0.135)	
Female	8.597	0.065	0.131	0.072	0.113	2,587
		(0.111)	(0.223)	(0.128)	(0.200)	
Male	8.368	− 0.087	− 0.189	0.038	0.054	2,486
		(0.117)	(0.253)	(0.125)	(0.177)	
Short sleep (less than < seven hours or less than < six hours if over age 17) [SR]						
All	0.129	− 0.015	− 0.031	− 0.011	− 0.016	5,073
		(0.012)	(0.026)	(0.014)	(0.020)	
Female	0.121	− 0.027	− 0.055	− 0.001	− 0.002	2,587
		(0.017)	(0.034)	(0.019)	(0.030)	
Male	0.136	− 0.002	− 0.003	− 0.021	− 0.029	2,486
		(0.019)	(0.041)	(0.019)	(0.027)	
Long sleep (> 11 hours or > 10 hours if over age 17) [SR]						
All	0.129	− 0.012	− 0.026	− 0.002	− 0.002	5,073
		(0.012)	(0.025)	(0.014)	(0.021)	
Female	0.141	0.002	0.004	0.016	0.025	2,587
		(0.018)	(0.037)	(0.020)	(0.032)	
Male	0.117	− 0.027~	− 0.058~	− 0.019	− 0.027	2,486
		(0.016)	(0.035)	(0.018)	(0.026)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, M = direct measurement.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

SUPPLEMENTAL EXHIBIT 3.4. SELF-REPORTED HEALTH CARE ACCESS FOR ADULTS AND YOUTH						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ADULTS						
Adult and children have health insurance [SR]	0.827	0.019 (0.017)	0.039 (0.034)	0.035~ (0.021)	0.056~ (0.034)	3,253
Adult or children did not get medical care when needed during the past 12 months [SR]	0.101	− 0.002 (0.013)	− 0.005 (0.027)	− 0.022 (0.016)	− 0.035 (0.025)	3,261
Adult or children did not get needed medical care in the past 12 months because they could not afford it [SR]	0.061	− 0.002 (0.010)	− 0.005 (0.022)	− 0.026* (0.012)	− 0.041* (0.019)	3,261
Adult has been to emergency room for own health reasons at least once in the past 12 months [SR]	0.446	0.036~ (0.022)	0.074~ (0.045)	− 0.002 (0.029)	− 0.003 (0.046)	3,262
Adult usually goes to emergency room for routine care [SR]	0.051	0.015 (0.010)	0.031 (0.020)	− 0.011 (0.012)	− 0.018 (0.019)	3,264
Adult usually goes to a clinic or health center for routine care [SR]	0.548	− 0.021 (0.021)	− 0.043 (0.044)	− 0.008 (0.028)	− 0.012 (0.045)	3,264
Adult usually goes to a doctor's office or HMO for routine care [SR]	0.298	0.027 (0.020)	0.055 (0.040)	0.027 (0.026)	0.043 (0.042)	3,264
Adult saw or talked to a health professional about their own health in the past 6 months [SR]	0.792	0.023 (0.017)	0.048 (0.036)	0.014 (0.022)	0.023 (0.036)	3,263
Adult has children in the household and they usually go to emergency room for routine care [SR]	0.020	0.005 (0.006)	0.010 (0.012)	− 0.004 (0.007)	− 0.007 (0.011)	3,250
Adult has children in the household and they usually go to a clinic or health center for routine care [SR]	0.412	− 0.010 (0.020)	− 0.020 (0.042)	0.009 (0.027)	0.014 (0.044)	3,250
Adult has children in the household and they usually go to a doctor's office or HMO for routine care [SR]	0.210	0.014 (0.017)	0.029 (0.035)	0.014 (0.022)	0.023 (0.035)	3,250

SUPPLEMENTAL EXHIBIT 3.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
YOUTH						
Youth had routine physical exam in the past 12 months [SR]						
All	0.719	− 0.036*	− 0.075*	0.002	0.002	5,060
		(0.017)	(0.035)	(0.019)	(0.028)	
Female	0.795	− 0.064*	− 0.129*	− 0.019	− 0.029	2,578
		(0.022)	(0.044)	(0.024)	(0.037)	
Male	0.647	− 0.008	− 0.017	0.021	0.030	2,482
		(0.026)	(0.055)	(0.029)	(0.041)	
Youth had dental exam in the past 12 months [SR]						
All	0.610	− 0.012	− 0.026	0.000	0.000	5,086
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.640	− 0.023	− 0.046	− 0.003	− 0.005	2,592
		(0.025)	(0.050)	(0.029)	(0.045)	
Male	0.582	− 0.002	− 0.003	0.003	0.004	2,494
		(0.027)	(0.059)	(0.029)	(0.042)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed. Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

CHAPTER 4

IMPACTS ON ADULT AND YOUTH MENTAL HEALTH

This chapter discusses MTO's effects on the mental health of participating adults and youth. For adults, we find that MTO produces statistically significant improvements in important mental health outcomes, including general psychological distress and depression. We also find some evidence for an increase in substance dependence among adults in the MTO experimental group compared with controls.

We detect few significant effects on the mental health of youth ages 13–20. However, we observe statistically significant beneficial effects for female youth in the experimental group relative to controls on a subset of our measures of mental health: lower prevalence of any lifetime mood disorder, lower prevalence of lifetime oppositional defiant disorder, fewer serious emotional or behavioral difficulties, less psychological distress, and fewer panic attacks in the past year. For male youth in the Section 8 group, our analyses suggest that MTO may be associated with a higher prevalence of post-traumatic stress disorder. Parental reports on their grown children's mental health show no significant differences across treatment groups.

4.1 HYPOTHESES ABOUT EFFECTS ON ADULT AND YOUTH MENTAL HEALTH

A substantial mental health literature documents that both adults and children who live in high-poverty, high-crime urban settings have elevated rates of a wide range of adverse mental health outcomes (for example, Bagley, Jacobson, and Palmer, 1973; Rezaeian et al., 2005; Whitley et al., 1999). There is continuing debate, however, about the relative importance of "geographic drift," that is, differential selection of people with a predisposition to mental illness into these environments rather than causal influences of the environment on mental illness (van Kamp et al., 2004). To the extent that these associations reflect causal effects of the environment, we would expect the MTO demonstration to improve mental health outcomes. Exhibit 4.1 describes some of the suggested mechanisms by which relocating to a lower-poverty neighborhood might lead to improved mental health.

EXHIBIT 4.1. HYPOTHESIZED PATHWAYS THROUGH WHICH MTO MAY AFFECT MENTAL HEALTH

MTO INTERVENTION → RELOCATION TO LOW-POVERTY NEIGHBORHOODS

COMMUNITY-LEVEL MEDIATORS

PERSON- AND FAMILY-LEVEL MEDIATORS

HEALTH OUTCOMES

Physical Environment
· Less noise, light pollution
· Safer environment

Community Norms and Values
· More positive peer groups, adult role models

Community Resources
· Mental health care facilities
· Access to job with health benefits

Social Environment
· Reduced crime and violence
· Different neighbors

Exercise
· More exercise

Access to Care
· Health insurance
· Access to medical care

Safety and Stress
· Increased sense of security, confidence
· Less stress, anxiety

Social Isolation
· Feelings of isolation

Mental Health
· Psychological distress
· Mental calm
· General happiness
· Major depression
· Bipolar Disorder
· Generalized Anxiety Disorder
· Panic Attacks/Disorder
· Intermittent-Explosive Disorder
· Oppositional-Defiant Disorder
· Post-Traumatic Stress Disorder

Substance Use
· Drinking
· Drug use

Exhibit 4.1 is a modified version of Exhibit 4.1 in the MTO interim impacts evaluation (see Orr et al., 2003).

CHANGES IN PHYSICAL ENVIRONMENT

A wide range of childhood adversities associated with the physical environment, such as poverty, poor nutrition, abuse, and neglect, have been linked to a wide range of mental disorders (Anda et al., 2006; Green et al., 2010). Animal studies suggest that the effects of early environments on the body's stress response system and brain development mediate these associations to some extent (Weaver, 2009). However, the social environments of neighborhoods also seem to be important by varying exposure to stressful experiences that can provoke mental disorders as well as in access to supportive resources for mental disorders (Kloos and Shah, 2009).

CHANGES IN THE SOCIAL ENVIRONMENT

Exposure to violence can cause long-term behavioral and psychological harm for both youth and adults (Famularo et al., 1996; Groves et al., 1993; Zapata et al., 1992). This is especially true in predicting depression, generalized anxiety disorder, and post-traumatic stress disorder (PTSD). As a result, 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).

It is also possible that moving to lower-poverty neighborhoods could influence externalizing disorders, including oppositional-defiant disorder, intermittent-explosive disorder, conduct disorder, and substance use disorder among youth, as these are strongly related to peer environments and norms regarding the appropriateness of violence and antisocial behaviors (Deater-Deckard, 2001; Gifford-Smith et al., 2005). The direction of these effects, however, is unknown, as there is no necessarily positive association between a decrease in neighborhood poverty levels and a decrease in exposure to maladaptive peer environments. It is possible that moves could have adverse effects by leaving MTO family members socially or culturally isolated in their new neighborhoods.

Previous research suggests that the local social environment could act differently on the mental health outcomes of male and female youth, in part because males may have higher exposure to neighborhood risk factors. Adolescent males tend to be subject to less parental supervision than females and also tend to be greater risk-takers (Block, 1983; Bottcher, 2001; LaGrange and Silverman, 1999). Male and female youth also appear to have different coping styles and capacities. Psychosocial stress sometimes can have more pronounced effects on males than females in part because males are more likely to use confrontational techniques 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 gender difference in the use of adult supports could be magnified in situations like MTO where households are disproportionately headed by females.

CHANGES IN COMMUNITY RESOURCES

MTO-assisted moves to lower-poverty neighborhoods could improve access to mental health service providers, or improve access to job opportunities that include health benefits that would cover mental health services. On the other hand, free or very low-cost mental health services may be concentrated in high-poverty areas, and so access to low-cost mental health care services could decrease with moves to better neighborhoods.

EARLIER RESEARCH

Some nonexperimental studies suggest a possible relationship between neighborhood of residence and mental health status (for a recent review, see Diez-Roux and Mair, 2010). Although almost entirely cross-sectional, these studies have consistently found that important aspects of neighborhood environment are significantly related to the mental health of residents. Gidlow et al. (2010), for example, found that consensus perceptions of neighborhood quality were significantly related to the self-reported mental health of respondents in a sample of neighborhoods in the United Kingdom, with perceived social support the most important aspect of the neighborhood. Van den Berg et al. (2010) find evidence consistent with the idea that the presence of green space may buffer the effects of stressful experiences on mental health.

EXHIBIT 4.2. ADULT PSYCHOLOGICAL WELL-BEING						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
PSYCHOLOGICAL DISTRESS INDEX (K6), PAST MONTH [SR]						
K6 Z-score (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
Serious mental illness (K6 raw score > =13)	0.133	0.001 (0.015)	0.001 (0.030)	– 0.015 (0.019)	– 0.024 (0.031)	3,273
SUBSTANCE DEPENDENCE, PAST MONTH [SR]						
Severity of dependence scale raw score (higher score indicates greater dependence)	0.329	0.117* (0.055)	0.241* (0.114)	0.048 (0.077)	0.076 (0.122)	3,269
Dependence on drugs or alcohol (severity of dependence score of 3 or higher)	0.055	0.029* (0.011)	0.060* (0.022)	0.015 (0.015)	0.024 (0.023)	3,269
MENTAL CALM [SR]						
Calm and peaceful most of the time, past month	0.487	0.018 (0.022)	0.037 (0.045)	– 0.025 (0.029)	– 0.040 (0.047)	3,272

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: Psychological distress consists of 6 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.
Serious mental illness is defined as a raw score of 13 or higher on the K6. Substance dependence consists of 5 items about drug and/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. See Section 4.3 for additional details.

4.2 EARLIER RESEARCH: SHORT-TERM AND INTERIM MTO IMPACTS ON MENTAL HEALTH

Consistent with evidence from nonexperimental studies, short-term evidence from the MTO sites showed that adults in both treatment groups in the Boston site were more likely to feel calm and peaceful (Katz, Kling, and Liebman, 2001). Short-term data from both Boston and New York showed that experimental group children experienced less fearfulness than the control group (Katz, Kling, and Liebman, 2001; Leventhal and Brooks-Gunn, 2003). Younger children in the Section 8 group also experienced fewer unhappy, sad, or depressed feelings (Leventhal and Brooks-Gunn, 2003).

The MTO interim study four to seven years after random assignment found that among adults, assignment to the experimental group rather than control group reduced psychological distress (as measured by the Kessler 6 [K6] measure of psychological distress) and increased reports of mental calm (Orr et al., 2003, Exhibit 4.2, p. 77). The point estimate of the impact on the incidence of depression for the experimental group was substantial but only statistically significant when boundary cases were included, and the impact on anxiety (measured using two questions) was undistinguishable from zero. The estimates for the Section 8 group were not statistically distinguishable from zero for any mental health measure for adults.

For youth, the interim study found few statistically significant changes in mental health when examining male and female youth together. The main mental health measures on the interim surveys for respondents ages 12–19 included the K6 and scales to measure lifetime depression and generalized anxiety disorder developed for the National Comorbidity Survey Replication: Adolescent Supplement (NCSR-AS). There was some sign of a decline in generalized anxiety disorder for the Section 8 group relative to controls. However, for all three mental health measures, the point estimates for females in both treatment groups suggest substantively large improvements in mental health, with four of the six estimates (three outcomes for each of two treatment groups) statistically significant. In contrast, for males,

none of the estimates were statistically significant, and all estimates were in the direction of small declines in mental health. A qualitative follow-up to the interim survey suggests that these gender differences could be related to differences in how female and male youth in the experimental group navigated their new social environments, with distance from father figures and increased exposure to neighborhood public spaces being perhaps particularly important for male youth (Clampet-Lundquist et al., 2011).

4.3 DATA SOURCES AND MEASURES

All of the mental health outcomes we describe were measured with data from the long-term evaluation surveys. These measures have all been used before in large national surveys. The data on adult and youth health outcomes are self-reported, whereas the data on grown children (ages 21–30) are from parental or primary caregiver reports.

Adults responded to questions about their mental health that included the same set of questions from the MTO interim survey (K6 indicator of distress and a measure of calm). The K6 scale (Furukawa et al., 2003; Kessler et al., 2003) consists of six questions and is the most widely used scale of nonspecific psychological distress in the literature. It is a core part of all three of the major ongoing national health tracking surveys in the United States: the National Health Interview Survey (NHIS), the Behavioral Risk Factor Surveillance Survey (BRFSS), and the Substance Abuse and Mental Health Services Administration's National Household Survey on Drug Use and Health. We also asked adults to complete disorder-specific mental health measures (for example, measures of depression, anxiety, intermittent explosive disorder) from the World Health Organization's (WHO) Composite International Diagnostic Interview (CIDI) (Kessler and Üstün, 2004), the most widely used epidemiological interview for mental disorders in the world. The CIDI is used in the WHO World Mental Health Survey Initiative (Kessler and Üstün, 2008), the world's largest series of coordinated epidemiological surveys of mental disorders, as well as in the U.S. National Comorbidity Surveys (Kessler et al., 1994, 2005). MTO adults completed the CIDI sections designed to generate diagnoses of specific disorders

defined by the *Diagnostic and Statistical Manual of Mental Disorders*, 4th edition (DSM-IV), such as major depression, generalized anxiety disorder, and post-traumatic stress disorder.

Interviews with youth ages 10–20 included questions about their mental calm, distress, and strengths and difficulties. The latter questions come from the Strengths and Difficulties Questionnaire (SDQ) (Goodman and Goodman, 2009), a widely used screening scale to detect clinically significant mental disorders among youth. The MTO screening version of the SDQ is identical to the screening scale developed for use in the Health Interview Survey to assess DSM-IV "serious mental illness" (Simpson et al., 2005). Interviews with youth 13 or older also included the CIDI diagnostic scales. We use parent reports on whether any of their grown children (ages 21–30) suffer from serious mental health problems or an alcohol or drug problem. We focus our primary analyses on youth ages 13–20 because they were administered the CIDI diagnostic scales as well as the other mental health measures.[1]

MEASURES OF DISTRESS AND DIFFICULTIES

Kessler 6 (K6) measure of psychological distress: We asked adult and youth respondents how often in the past 30 days they felt sad, nervous, restless, hopeless, that everything was an effort, or worthless.[2] The raw score on the K6 can range from 0 (no distress) to 24 (highest distress). We report K6 scores transformed to z-scores, standardized using the control group's mean and standard deviation. K6 scores of 13 or higher have been shown to be strongly predictive of serious mental illness (Kessler et al., 2003).

Strengths and Difficulties Questionnaire (SDQ): We administered youth ages 10–20 a brief version of the SDQ developed by Robert Goodman. It consists of five behavioral and emotional items: obedience, worry/anxiety, unhappiness, getting along better with adults

than with peers, and attention span. The raw score can range from 0 (no behavioral or emotional problems) to 10 (severe emotional or behavioral problems). A score of 6 or higher is considered indicative of serious behavioral or emotional problems.

MEASURES OF MENTAL CALM

Calm and peaceful: We replicated the question from the MTO interim evaluation that asked adults how much of the time in the past 30 days they felt calm and peaceful. Response categories match those for the K6 items. Our measure reflects being calm and peaceful most of the time during the past month.

MEASURES OF SUBSTANCE DEPENDENCE

Severity of Dependence Scale (SDS): The scale consists of five items about the use of drugs or alcohol: use out of control, anxiety/worry about missing a fix or drink, worry about use, frequency of desire to end use, and difficulty of going without use. The raw score on the SDS can range from 0 (no dependence) to 15 (highest level of dependence). A score of 3 or higher indicates that the respondent is likely to meet DSM-IV criteria for a diagnosis of substance dependence.

MEASURES OF DSM-IV DISORDERS

We used a fully structured diagnostic interview to assess the prevalence of disorders such as depression, bipolar disorder, anxiety, panic disorder, PTSD, and intermittent-explosive disorder (IED) among MTO participants. Our diagnostic instrument was the CIDI, which was revised to make diagnoses according to the definitions and criteria of the DSM-IV (American Psychiatric Association, 1994) and expanded and updated for the WHO World Mental Health Survey Initiative (2004). The CIDI was developed with careful attention to instruction, comprehension, and motivation to increase the accuracy of reports (Kessler and Üstün, 2004) and has been validated through clinical reappraisals (Haro et al., 2006). The CIDI forms the basis for the National Comorbidity Survey (NCS) for adults and the NCS-Adolescent Supplement for youth, which allows us to compare MTO participant responses to nationally representative samples. In addition to assessing lifetime occurrence of disorders, we also

---

1 In Supplemental Exhibit 4.2, we report results expanding the sample to include 10- to 12-year-olds on the limited set of mental health measures that apply to youth of all ages (10–20 years old).

2 Response categories on the K6 items are: all of the time; most of the time; some of the time; a little of the time; and none of the time.

assessed the 12-month prevalence and collected data on age of onset and recency. We administered the structured diagnostic interview only to older youth, ages 13–20.

Major depression: Major depression is diagnosed if the respondent has experienced a major depressive episode, defined as a two-week or longer period where at least one symptom is depressed mood or loss of interest or pleasure and where the respondent had at least five of the following nine symptoms: depressed mood, markedly diminished interest or pleasure, significant weight loss or gain (unrelated to dieting), insomnia, psychomotor agitation (for example, physical restlessness, pacing) or retardation (for example, being physically slowed down), fatigue or loss of energy, feelings of worthlessness or excessive or inappropriate guilt, diminished ability to think or concentrate or indecisiveness, and recurrent thoughts of death. In addition, the symptoms must cause clinically significant distress or impair social, occupational, or other functioning. Depression with hierarchy is diagnosed where mania or hypomania is not also diagnosed.

Bipolar disorders (BPD) I and II: These are diagnosed if the respondent has experienced a manic (BPD I) or hypomanic (BPD II) episode, a distinct period in which the respondent has an abnormally and persistently elevated, expansive, or irritable mood. During that period, the respondent must experience at least three (or four if the mood is irritable only) of the following seven symptoms: inflated self-esteem or grandiosity, decreased need for sleep, more talkative than usual or pressure to keep talking, flight of ideas or subjective sense of racing thoughts, distractibility, increase in goal-oriented activity (often in the form of marathon writing sessions or other very intensive work-related activities) or psychomotor agitation (for example, inability to sit still without constant fidgeting), and excessive involvement in pleasurable but risky activities. In addition, a manic episode requires the mood disturbance to impair normal functioning, require hospitalization to prevent self-harm, or have psychotic features. The length and severity of the episode distinguish the manic episode from the less severe hypomania episode. Bipolar II also requires a history of at least one lifetime major depressive episode.

Generalized anxiety disorder (GAD): GAD includes excessive anxiety about multiple events or activities that the respondent finds difficult to control on more days than not over the course of at least six months. The anxiety must also be associated with at least three of the following six symptoms: restlessness, easy fatigue, difficulty concentrating, irritability, muscle tension, and sleep disturbance. Furthermore, the anxiety must cause clinically significant distress or impair social, occupational, or other functioning. Finally, the disturbance cannot occur exclusively during a mood disorder such as bipolar or depression. GAD with hierarchy is diagnosed when neither major depressive disorder nor mania is also diagnosed or where the onset of GAD is earlier than the onset of major depressive disorder and mania.

Panic attacks: These occur when a respondent experiences a discrete period of intense fear or discomfort where at least four of the following 13 symptoms developed abruptly and reached a peak within 10 minutes: palpitations, pounding heart, or accelerated heart rate; sweating; trembling or shaking; sensation of shortness of breath or smothering; feeling of choking; chest pain, or discomfort; nausea or abdominal distress; feeling dizzy, unsteady, or lightheaded; feelings of unreality or depersonalization; fear of losing control or going crazy; fear of dying; numbing or tingling sensations; and chills or hot flushes.

Panic disorder: This requires recurrent, unexpected panic attacks where at least one of the attacks is followed by a month or more of persistent concern about having additional attacks, worry about the implications of the attack or its consequences, or a significant change in behavior related to the attacks. Finally, the panic attacks cannot be better accounted for by another mental disorder, such as PTSD, which we operationalize in the MTO study by focusing on "out of the blue" attacks.

Intermittent-explosive disorder: IED requires at least three discrete episodes of failure to resist aggressive impulses that result in serious assaultive acts or destruction of property in which the degree of aggressiveness expressed is grossly out of proportion to any precipitating psychosocial stressors. Additionally,

it must not be the case that the aggressive episodes are better accounted for by another mental disorder (such as major depressive disorder or mania) or are due to the direct physiological effects of a substance or a general medical condition. The IED with hierarchy diagnosis takes into account major depressive disorder and mania/hypomania, where the anger attacks cannot only have occurred when depressive or manic symptoms were also present and where the age of onset and recency for IED is compared with the age of onset and recency for major depressive disorder and mania/hypomania.

Oppositional defiant disorder (ODD): ODD, which is diagnosed only for youth, requires a pattern of negative, hostile, and defiant behavior lasting at least six months, during which at least four of the following eight symptoms are often present: loses temper, argues with adults, actively defies or refuses to comply with adult's requests or rules, deliberately annoys people, blames others for his or her mistakes or misbehavior, touchy or easily annoyed by others, angry and resentful, and spiteful and vindictive. Furthermore, the disturbance in behavior must cause clinically significant impairment in social, academic, or occupational functioning, and the behaviors must not occur exclusively during the course of a mood disorder.

Post-traumatic stress disorder: PTSD occurs when a respondent has been exposed to and subsequently suffers because of a traumatic event in which the person experienced, witnessed, or was confronted with an event or events that involved actual or threatened death or serious injury to the respondent or to others.[3] Furthermore, the respondent must exhibit three of the following five behaviors: avoiding activities, places, or people that arouse recollections of the trauma; markedly diminished interest or participation in significant activities; feeling of detachment or estrangement from others; restricted range of affect (for example, unable to have loving feelings); and a sense of a foreshortened

future (for example, does not expect to have a career, marriage, children, or a normal life span). Finally, the respondent must indicate difficulty falling or staying asleep and an exaggerated startle response, and the disturbance in behavior must cause clinically significant impairment in social, academic, or occupational functioning. The MTO version of the CIDI PTSD module contained fewer questions than the full CIDI battery, so we used responses to the included questions to generate a predicted probability of meeting the disorder among respondents who meet the disorder in a nationally representative sample.

Summary measures: In addition to the disorders listed above, we include four summary measures of disorders. Any mood disorder includes depression (without hierarchy), bipolar I/II/subthreshold, and mania/hypomania/hypomania subthreshold. Any anxiety disorder includes panic disorder, GAD (without hierarchy), and PTSD. Any disorder includes any mood disorder, any anxiety disorder, IED (without hierarchy), and for youth ODD (without hierarchy). Number of disorders is a count of the following disorders: depression (without hierarchy), bipolar I/II/subthreshold, panic disorder, GAD (without hierarchy), PTSD, IED (without hierarchy), and for youth ODD (without hierarchy).

Absence of mental health problems: This measure is based on an index we constructed for adults and youth as developed by Kling, Liebman, and Katz (2007).[4] The adult version includes the K6; CIDI diagnosis of major depressive disorder and GAD for the past year; mental calm; and a measure of normal sleep (between seven and eight hours per night). The youth version includes the K6, lifetime CIDI diagnosis of major depressive disorder, and CIDI lifetime diagnosis of GAD. We standardized each item in the index by the control group mean and standard deviation. We standardized items separately for male and female youth. We reversed measures of problems so that higher scores indicate an absence of

---

3 The MTO survey asked about the following events: beaten up as a child by a primary caregiver, a spouse/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; and an unspecified other event (open-ended response option).

---

4 We measure some of the mental health items included in the index differently than Kling, Liebman, and Katz (2007) did. As we described above, the adult long-term survey included the full CIDI modules for depression and generalized anxiety disorder, whereas the interim survey contained a CIDI short-form for depression and only two questions for anxiety. Also, the interim sleep question asked about how much the adult typically slept at night, whereas the long-term measure is based on how much the adult slept last night.

problems, replaced missing values on individual measures with the mean for the treatment group, and then averaged standardized scores to create the index score.

MEASURES OF MENTAL HEALTH SERVICES

We asked adults a series of questions about any services that they have received in the past year for problems with mental health or substance abuse. We asked those who reported receiving any services how many sessions they received. We also asked all respondents whether they had taken any prescription medicine for these problems. Finally, we asked all respondents who neither received services nor took prescription medicine if there was a time in the past year when they felt they needed to see a medical professional for mental health or substance abuse issues.

4.4 CONTEXT AND BASELINE STATUS OF THE SAMPLE

We do not have baseline survey information on mental health status. However, the baseline surveys did indicate that half of all MTO heads of household reported feeling very unsafe in their neighborhoods at night. Fully 43 percent reported that someone in the home had been the victim of a crime during the six months before enrolling. As noted in prior chapters, more than three-quarters of all household heads reported that safety (getting away from gangs and drugs) was the first or second most important reason for wanting to enroll in MTO and move out of their baseline neighborhoods.

4.5 LONG-TERM IMPACTS ON MENTAL HEALTH

ADULTS

We assessed the intention-to-treat (ITT) effects and treatment-on-the-treated (TOT) effects on adult mental health proxied by our measures of psychological well-being and mental disorders using the regression models described in Chapter 1. Exhibit 4.2 shows the estimated impacts of the MTO treatments on psychological distress (K6), substance dependence, and mental calm.

We observe a few statistically significant impacts on the mental health of adults. The experimental group

adults have scores on the K6 that are, on average, a tenth of a standard deviation lower than the control group; however, the prevalence of serious mental illness, as measured by a K6 score of at least 13, is similar for both groups. The prevalence of dependence on drugs or alcohol for the experimental group is an estimated 2.9 percentage points higher than among controls. On mental calm (Exhibit 4.2) and normal sleep (Supplemental Exhibit 3.2), we detect no statistically significant differences between adults in the experimental and control groups. We observe no statistically significant differences between the Section 8 group and the control group on the K6 measure of serious mental illness, substance dependence, mental calm, or normal sleep and only a marginally statistically significant decrease on the K6 z-score.

Exhibit 4.3 shows the estimated impacts on adult mental disorders that we assessed using the CIDI diagnostic interview. The lifetime prevalence of depression for the experimental and Section 8 adults is lower by 3.2 (p < .10) and 4.8 percentage points, respectively, than the control mean of 20.3 percent. We observe no statistically significant treatment impacts for either the experimental or Section 8 groups on panic attacks, IED, PTSD, our measure of "any mental health disorder," the number of disorders, or the mental health index. However, the estimated MTO treatment effect on the mental health index for the experimental group is in the direction of improved mental health and on the margin of statistical significance with a p value of .095 (Exhibit 4.3). Some of the impact estimates have fairly wide confidence intervals. For example, the 95 percent confidence interval for "any lifetime mood disorder" for the Section 8 group ranges from − 8 to +1 percentage points compared with a control mean of 26 percent.

EXHIBIT 4.3. ADULT MENTAL DISORDERS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
MAJOR DEPRESSION WITH HIERARCHY [SR]						
Lifetime	0.203	– 0.032~	– 0.066~	– 0.048*	– 0.077*	3,269
		(0.017)	(0.035)	(0.021)	(0.034)	
Past year	0.119	– 0.021	– 0.043	– 0.013	– 0.021	3,269
		(0.013)	(0.028)	(0.017)	(0.027)	
BIPOLAR I [SR]						
Lifetime	0.013	– 0.003	– 0.007	– 0.001	– 0.002	3,269
		(0.004)	(0.009)	(0.006)	(0.010)	
Past year	0.009	– 0.003	– 0.006	0.001	0.001	3,269
		(0.004)	(0.007)	(0.006)	(0.009)	
BIPOLAR II [SR]						
Lifetime	0.009	0.006	0.012	0.000	– 0.001	3,269
		(0.005)	(0.010)	(0.007)	(0.011)	
Past Year	0.006	0.006	0.013	0.004	0.007	3,269
		(0.004)	(0.009)	(0.006)	(0.010)	
GENERALIZED ANXIETY DISORDER WITH HIERARCHY [SR]						
Lifetime	0.065	– 0.003	– 0.005	– 0.020~	– 0.033~	3,273
		(0.010)	(0.021)	(0.011)	(0.017)	
Past year	0.033	0.006	0.012	– 0.012	– 0.019	3,273
		(0.008)	(0.016)	(0.008)	(0.012)	
PANIC DISORDER [SR]						
Lifetime	0.081	0.003	0.007	0.002	0.003	3,269
		(0.012)	(0.024)	(0.016)	(0.025)	
Past year	0.063	0.001	0.001	0.000	0.000	3,269
		(0.010)	(0.021)	(0.014)	(0.022)	
PANIC ATTACKS [SR]						
Lifetime	0.407	0.004	0.009	– 0.022	– 0.035	3,269
		(0.021)	(0.044)	(0.028)	(0.045)	
Past year	0.087	0.004	0.009	0.004	0.006	3,269
		(0.012)	(0.026)	(0.016)	(0.026)	
INTERMITTENT-EXPLOSIVE DISORDER WITH HIERARCHY [SR]						
Lifetime	0.062	0.001	0.003	0.004	0.007	3,269
		(0.010)	(0.022)	(0.015)	(0.024)	
Past year	0.051	– 0.008	– 0.015	– 0.008	– 0.013	3,269
		(0.009)	(0.019)	(0.013)	(0.021)	
POST-TRAUMATIC STRESS DISORDER [SR]						
Lifetime	0.219	– 0.012	– 0.024	0.004	0.006	3,269
		(0.018)	(0.037)	(0.024)	(0.038)	
Past year	0.164	– 0.016	– 0.032	– 0.023	– 0.036	3,269
		(0.015)	(0.032)	(0.020)	(0.032)	

EXHIBIT 4.3 (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANY ANXIETY DISORDER [SR]						
Lifetime	0.308	– 0.020	– 0.042	– 0.005	– 0.008	3,269
		(0.020)	(0.041)	(0.026)	(0.042)	
Past year	0.220	– 0.012	– 0.024	– 0.021	– 0.034	3,269
		(0.018)	(0.036)	(0.023)	(0.037)	
ANY MOOD DISORDER [SR]						
Lifetime	0.255	– 0.028	– 0.058	– 0.036	– 0.058	3,270
		(0.018)	(0.038)	(0.024)	(0.039)	
Past year	0.153	– 0.015	– 0.032	– 0.001	– 0.001	3,269
		(0.015)	(0.031)	(0.020)	(0.033)	
ANY DISORDER [SR]						
Lifetime	0.412	– 0.026	– 0.053	– 0.022	– 0.036	3,270
		(0.021)	(0.044)	(0.028)	(0.045)	
Past year	0.286	– 0.021	– 0.043	– 0.019	– 0.031	3,269
		(0.019)	(0.040)	(0.026)	(0.041)	
NUMBER OF DISORDERS [SR]						
Lifetime	0.786	– 0.049	– 0.101	– 0.062	– 0.099	3,269
		(0.049)	(0.100)	(0.065)	(0.104)	
Past year	0.535	– 0.031	– 0.064	– 0.041	– 0.066	3,269
		(0.042)	(0.087)	(0.056)	(0.090)	
MENTAL HEALTH INDEX (Z-SCORE) [SR]						
Absence of mental health problems: depression, anxiety, calm, distress and sleep (higher score indicates fewer mental health problems)	0.000	0.042~	0.086~	0.040	0.065	3,273
		(0.025)	(0.052)	(0.034)	(0.055)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.
Measures: Disorders with hierarchy take into account the comorbidity of other disorders: Depression with hierarchy takes into account Mania and Hypomania; GAD takes into account Depression and Mania; IED takes into account Depression, Mania, and Hypomania. Any Anxiety Disorder includes Panic Disorder, GAD (without hierarchy), and PTSD. Any Mood Disorder includes Depression (without hierarchy), Bipolar I/II/Subthreshold, and Mania/Hypomania/Hypomania Subthreshold. Any Disorder includes all disorders from Any Anxiety Disorder and Any Mood Disorder as well as IED (without hierarchy). Index of mental health problems is the average of the z-scores for depression (past year), anxiety (past year), mental calm, distress (K6), and normal sleep (7 to 8 hours last night) after standardizing by the control mean and standard deviation. A higher mental health index score indicates better mental health. See Section 4.3 for additional details.

YOUTH

Exhibit 4.4 shows the estimated impacts on psychological well-being for youth ages 13–20. Supplemental Exhibit 4.2 shows these same impacts after expanding the age range to youth ages 10–20. Impacts on mental disorders such as depression and generalized anxiety are shown in Exhibits 4.5 and 4.6. All the exhibits provide separate estimates for all, female, and male youth.

We detect very few significant treatment impacts for all youth taken together in either the experimental or Section 8 groups. However, we see some significant effects for experimental group females compared with control group females. Females in the experimental group have K6 scores that are over a tenth of a standard deviation lower than the control group, are 4.8 percentage points less likely than controls to have had a mood disorder during their lifetime (control mean of 21.8 percent), have fewer serious behavioral and emotional problems as indicated by the SDQ, reported fewer panic attacks, and had a lower prevalence of ODD during the past year. We find no statistically significant differences between the experimental and control group females' mental calm, bipolar, panic disorder, PTSD, or IED and only marginally significant effects (towards better mental health) on our absence of mental health problems index, depression, and a K6 score indicative of serious mental illness and a marginally significant effect towards greater past year GAD.[5] For females in the Section 8 group, we detect no statistically significant differences from controls on any mental health measure, and the only impact that is marginally statistically significant is a higher level of past year GAD.

For male youth, we detect no statistically significant effects on our mental health index, the K6, strengths and difficulties, mental calm, depression, bipolar, panic disorder, panic attacks, IED, or ODD. While males in the experimental group appear to have similar levels of lifetime and past-year PTSD as males in the control group, Section 8 group males have higher levels of

lifetime PTSD. The estimated impact is a prevalence that is 3.0 percentage points higher than the controls, who have a prevalence of 4.1 percent. And, although not statistically significant, the estimated MTO impacts for males in the Section 8 groups (and to some extent the experimental group) were in the direction of worse mental health than controls, and the Section 8 males showed marginally significant impacts towards higher prevalences of bipolar, any mood disorder, and any mental health disorder.

GROWN CHILDREN

As reported by the sample adults, the mental health of MTO grown children (ages 21–30) appears to be similar across treatment groups (see Exhibit 4.7). Approximately 11 percent of the grown children in the control group suffer from depression or another serious mental illness, and about 5 percent have an alcohol or drug problem. Adults report higher rates of alcohol and drug problems for male children (8.2 percent) than female children (2.6 percent).

---

5 However, the bottom of Supplemental Exhibit 4.2 shows that, when limited to youth ages 15–20, we find a positive and statistically significant effect on the mental health index for female youth in the experimental group relative to those in the control group, a result similar to findings for female youth aged 15–20 at the interim survey by Kling, Liebman, and Katz (2007).

EXHIBIT 4.4. YOUTH PSYCHOLOGICAL WELL-BEING, AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
PSYCHOLOGICAL DISTRESS INDEX (K6), PAST MONTH [SR]						
K6 Z-SCORE (HIGHER SCORE INDICATES GREATER DISTRESS)						
All	0.000	– 0.040	– 0.084	0.036	0.054	4,644
		(0.041)	(0.085)	(0.047)	(0.070)	
Female	0.115	– 0.119*	– 0.241*	– 0.013	– 0.021	2,371
		(0.058)	(0.116)	(0.066)	(0.104)	
Male	– 0.110	0.040	0.085	0.084	0.120	2,273
		(0.054)	(0.116)	(0.060)	(0.086)	
SERIOUS MENTAL ILLNESS (K6 RAW SCORE ≥ 13)						
All	0.067	– 0.012	– 0.025	0.009	0.013	4,644
		(0.010)	(0.021)	(0.011)	(0.017)	
Female	0.085	– 0.026~	– 0.053~	0.001	0.002	2,371
		(0.015)	(0.030)	(0.017)	(0.026)	
Male	0.050	0.002	0.005	0.016	0.023	2,273
		(0.013)	(0.028)	(0.015)	(0.021)	
BRIEF STRENGTHS AND DIFFICULTIES QUESTIONNAIRE (SDQ) [SR]						
SDQ RAW SCORE (HIGHER SCORE INDICATES MORE DIFFICULTIES)						
All	3.149	– 0.102	– 0.211	0.071	0.106	4,644
		(0.071)	(0.147)	(0.079)	(0.119)	
Female	3.245	– 0.174~	– 0.351~	0.150	0.234	2,371
		(0.100)	(0.202)	(0.114)	(0.178)	
Male	3.057	– 0.026	– 0.057	– 0.012	– 0.018	2,273
		(0.093)	(0.200)	(0.101)	(0.145)	
SERIOUS BEHAVIORAL OR EMOTIONAL PROBLEMS (SDQ RAW SCORE ≥ 6)						
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)	

EXHIBIT 4.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
CALM AND PEACEFUL MOST OF THE TIME OR MORE OFTEN IN THE PAST MONTH [SR]						
All	0.595	0.017	0.034	− 0.010	− 0.014	4,642
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.512	0.043	0.087	0.026	0.040	2,371
		(0.028)	(0.057)	(0.031)	(0.048)	
Male	0.675	− 0.010	− 0.022	− 0.045	− 0.064	2,271
		(0.026)	(0.056)	(0.030)	(0.042)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: Psychological distress consists of 6 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 for of control group youth (with male and female youth standardized separately). Serious mental illness is defined as a raw score of 13 or higher on the K6. Strengths and Difficulties consists of 5 behavioral and emotional items (obedience, worry/anxiety, unhappiness, getting along better with adults than peers, attention span) scaled on score from 0 (no behavioral/emotional problems) to 10 (severe behavioral or emotional problems). A score of 6 or higher indicates serious behavioral/emotional problems. See Section 4.3 for additional details.

EXHIBIT 4.5. YOUTH MENTAL DISORDERS, AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
MAJOR DEPRESSION WITH HIERARCHY [SR]						
LIFETIME						
All	0.105	– 0.015 (0.012)	– 0.031 (0.025)	– 0.014 (0.013)	– 0.021 (0.019)	4,639
Female	0.128	– 0.032~ (0.017)	– 0.065~ (0.035)	– 0.029 (0.019)	– 0.045 (0.029)	2,367
Male	0.084	0.003 (0.016)	0.006 (0.034)	0.000 (0.017)	0.000 (0.025)	2,272
PAST YEAR						
All	0.059	– 0.005 (0.010)	– 0.010 (0.020)	– 0.004 (0.011)	– 0.005 (0.016)	4,639
Female	0.080	– 0.024~ (0.014)	– 0.049~ (0.029)	– 0.021 (0.015)	– 0.033 (0.024)	2,367
Male	0.038	0.015 (0.012)	0.033 (0.026)	0.014 (0.014)	0.019 (0.019)	2,272
BIPOLAR I [SR]						
LIFETIME						
All	0.014	0.004 (0.005)	0.009 (0.011)	0.004 (0.005)	0.006 (0.008)	4,639
Female	0.010	0.010 (0.006)	0.020 (0.013)	– 0.001 (0.005)	– 0.001 (0.008)	2,367
Male	0.017	– 0.002 (0.008)	– 0.004 (0.017)	0.008 (0.009)	0.012 (0.013)	2,272
PAST YEAR						
All	0.009	0.004 (0.004)	0.008 (0.008)	0.001 (0.004)	0.001 (0.006)	4,639
Female	0.008	0.006 (0.005)	0.013 (0.011)	– 0.002 (0.004)	– 0.004 (0.007)	2,367
Male	0.011	0.001 (0.006)	0.002 (0.013)	0.004 (0.007)	0.005 (0.011)	2,272
BIPOLAR II [SR]						
LIFETIME						
All	0.006	0.001 (0.003)	0.002 (0.006)	0.008~ (0.004)	0.011~ (0.006)	4,639
Female	0.010	– 0.001 (0.005)	– 0.003 (0.011)	0.008 (0.007)	0.012 (0.012)	2,367
Male	0.001	0.004 (0.002)	0.008 (0.005)	0.007~ (0.004)	0.010~ (0.005)	2,272

EXHIBIT 4.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
BIPOLAR II [SR] (CONTINUED)						
PAST YEAR (CONTINUED)						
All	0.006	0.000	0.000	0.007	0.010	4,639
		(0.003)	(0.006)	(0.004)	(0.006)	
Female	0.010	− 0.003	− 0.006	0.007	0.011	2,367
		(0.005)	(0.010)	(0.007)	(0.012)	
Male	0.001	0.003	0.006	0.006~	0.009~	2,272
		(0.002)	(0.005)	(0.004)	(0.005)	
GENERALIZED ANXIETY DISORDER WITH HIERARCHY [SR]						
LIFETIME						
All	0.019	− 0.002	− 0.004	0.010~	0.015~	4,644
		(0.005)	(0.010)	(0.006)	(0.009)	
Female	0.022	− 0.002	− 0.003	0.010	0.016	2,371
		(0.007)	(0.014)	(0.008)	(0.013)	
Male	0.016	− 0.002	− 0.005	0.010	0.015	2,273
		(0.006)	(0.013)	(0.009)	(0.013)	
PAST YEAR						
All	0.007	0.005	0.010	0.008~	0.012~	4,644
		(0.003)	(0.007)	(0.005)	(0.007)	
Female	0.007	0.009~	0.019~	0.010~	0.015~	2,371
		(0.005)	(0.010)	(0.006)	(0.009)	
Male	0.007	0.001	0.002	0.006	0.008	2,273
		(0.005)	(0.010)	(0.008)	(0.011)	
PANIC DISORDER [SR]						
LIFETIME						
All	0.047	0.000	0.000	0.001	0.002	4,639
		(0.008)	(0.017)	(0.009)	(0.013)	
Female	0.052	− 0.013	− 0.027	− 0.004	− 0.006	2,367
		(0.010)	(0.021)	(0.012)	(0.018)	
Male	0.042	0.014	0.030	0.006	0.008	2,272
		(0.012)	(0.027)	(0.013)	(0.018)	
PAST YEAR						
All	0.031	− 0.004	− 0.008	0.001	0.002	4,639
		(0.007)	(0.014)	(0.007)	(0.011)	
Female	0.031	− 0.011	− 0.022	0.002	0.003	2,367
		(0.008)	(0.016)	(0.009)	(0.015)	
Male	0.031	0.004	0.008	0.000	0.000	2,272
		(0.011)	(0.023)	(0.011)	(0.015)	

EXHIBIT 4.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
PANIC ATTACKS [SR]						
LIFETIME						
All	0.387	– 0.017	– 0.035	0.014	0.021	4,639
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.423	– 0.039	– 0.079	– 0.005	– 0.008	2,367
		(0.027)	(0.054)	(0.029)	(0.045)	
Male	0.354	0.006	0.012	0.033	0.047	2,272
		(0.027)	(0.057)	(0.030)	(0.043)	
PAST YEAR						
All	0.058	– 0.020*	– 0.042*	– 0.007	– 0.010	4,639
		(0.009)	(0.019)	(0.010)	(0.015)	
Female	0.060	– 0.029*	– 0.059*	– 0.003	– 0.005	2,367
		(0.012)	(0.025)	(0.014)	(0.021)	
Male	0.056	– 0.010	– 0.022	– 0.011	– 0.016	2,272
		(0.013)	(0.029)	(0.014)	(0.020)	
INTERMITTENT-EXPLOSIVE DISORDER WITH HIERARCHY [SR]						
LIFETIME						
All	0.164	– 0.001	– 0.003	0.006	0.009	4,639
		(0.015)	(0.032)	(0.017)	(0.025)	
Female	0.162	– 0.008	– 0.016	– 0.006	– 0.009	2,367
		(0.021)	(0.042)	(0.022)	(0.034)	
Male	0.166	0.005	0.011	0.017	0.025	2,272
		(0.021)	(0.045)	(0.023)	(0.033)	
PAST YEAR						
All	0.133	– 0.012	– 0.025	0.012	0.018	4,639
		(0.014)	(0.029)	(0.016)	(0.023)	
Female	0.142	– 0.020	– 0.041	– 0.001	– 0.002	2,367
		(0.019)	(0.038)	(0.021)	(0.032)	
Male	0.125	– 0.004	– 0.008	0.025	0.036	2,272
		(0.018)	(0.039)	(0.022)	(0.031)	
OPPOSITIONAL-DEFIANT DISORDER WITH HIERARCHY [SR]						
LIFETIME						
All	0.120	– 0.001	– 0.002	0.009	0.013	4,639
		(0.012)	(0.025)	(0.014)	(0.021)	
Female	0.118	– 0.014	– 0.028	0.001	0.001	2,367
		(0.016)	(0.032)	(0.018)	(0.028)	

EXHIBIT 4.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
OPPOSITIONAL-DEFIANT DISORDER WITH HIERARCHY [SR] (CONTINUED)						
LIFETIME (CONTINUED)						
Male	0.122	0.012	0.026	0.016	0.023	2,272
		(0.018)	(0.038)	(0.021)	(0.030)	
PAST YEAR						
All	0.061	− 0.008	− 0.017	− 0.009	− 0.013	4,639
		(0.009)	(0.018)	(0.010)	(0.015)	
Female	0.067	− 0.023*	− 0.046*	− 0.010	− 0.015	2,367
		(0.012)	(0.023)	(0.013)	(0.021)	
Male	0.055	0.006	0.014	− 0.008	− 0.012	2,272
		(0.014)	(0.030)	(0.015)	(0.022)	
POST-TRAUMATIC STRESS DISORDER [SR]						
LIFETIME						
All	0.066	0.006	0.012	0.005	0.007	4,639
		(0.010)	(0.020)	(0.011)	(0.016)	
Female	0.092	0.002	0.003	− 0.019	− 0.030	2,367
		(0.016)	(0.032)	(0.016)	(0.026)	
Male	0.041	0.010	0.021	0.030*	0.043*	2,272
		(0.012)	(0.026)	(0.015)	(0.021)	
PAST YEAR						
All	0.045	0.010	0.021	0.000	0.000	4,639
		(0.009)	(0.018)	(0.009)	(0.013)	
Female	0.063	0.007	0.013	− 0.019	− 0.030	2,367
		(0.014)	(0.027)	(0.013)	(0.020)	
Male	0.028	0.014	0.029	0.020	0.028	2,272
		(0.011)	(0.024)	(0.013)	(0.018)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: Disorders with hierarchy take into account the comorbidity of other disorders: Depression with hierarchy takes into account Mania and Hypomania; GAD and ODD take into account Depression and Mania; and IED takes into account Depression, Mania, and Hypomania. See Section 4.3 for additional details.

EXHIBIT 4.6. SUMMARY MEASURES OF YOUTH MENTAL HEALTH, AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
ANY ANXIETY DISORDER [SR]						
LIFETIME						
All	0.121	0.001 (0.013)	0.002 (0.027)	0.005 (0.014)	0.007 (0.020)	4,639
Female	0.149	− 0.011 (0.019)	− 0.023 (0.038)	− 0.014 (0.020)	− 0.021 (0.031)	2,367
Male	0.095	0.013 (0.017)	0.028 (0.037)	0.023 (0.019)	0.033 (0.027)	2,272
PAST YEAR						
All	0.083	0.007 (0.012)	0.014 (0.024)	0.003 (0.012)	0.004 (0.018)	4,639
Female	0.098	0.002 (0.016)	0.003 (0.032)	− 0.006 (0.016)	− 0.010 (0.026)	2,367
Male	0.069	0.011 (0.016)	0.024 (0.034)	0.011 (0.017)	0.016 (0.024)	2,272
ANY MOOD DISORDER [SR]						
LIFETIME						
All	0.178	− 0.013 (0.015)	− 0.027 (0.031)	0.003 (0.016)	0.004 (0.024)	4,644
Female	0.218	− 0.048* (0.022)	− 0.096* (0.044)	− 0.032 (0.024)	− 0.050 (0.038)	2,371
Male	0.140	0.022 (0.020)	0.047 (0.043)	0.038~ (0.022)	0.054~ (0.032)	2,273
PAST YEAR						
All	0.104	0.003 (0.012)	0.005 (0.026)	0.006 (0.014)	0.008 (0.020)	4,643
Female	0.141	− 0.025 (0.018)	− 0.051 (0.037)	− 0.020 (0.020)	− 0.030 (0.031)	2,370
Male	0.070	0.030~ (0.016)	0.065~ (0.034)	0.030~ (0.017)	0.043~ (0.025)	2,273
ANY MENTAL HEALTH DISORDER [SR]						
LIFETIME						
All	0.370	− 0.001 (0.020)	− 0.002 (0.041)	0.011 (0.021)	0.017 (0.032)	4,640
Female	0.402	− 0.039 (0.027)	− 0.080 (0.055)	− 0.025 (0.030)	− 0.038 (0.046)	2,368
Male	0.339	0.037 (0.026)	0.080 (0.057)	0.047 (0.030)	0.066 (0.042)	2,272

EXHIBIT 4.6. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANY MENTAL HEALTH DISORDER [SR] (CONTINUED)						
PAST YEAR						
All	0.259	− 0.001	− 0.003	0.019	0.028	4,639
		(0.018)	(0.037)	(0.020)	(0.030)	
Female	0.298	− 0.032	− 0.065	− 0.012	− 0.019	2,367
		(0.025)	(0.050)	(0.027)	(0.042)	
Male	0.222	0.029	0.062	0.050~	0.070~	2,272
		(0.024)	(0.051)	(0.027)	(0.039)	
NUMBER OF MENTAL HEALTH DISORDERS [SR]						
LIFETIME						
All	0.661	− 0.007	− 0.015	0.042	0.062	4,639
		(0.044)	(0.091)	(0.048)	(0.071)	
Female	0.738	− 0.070	− 0.141	− 0.033	− 0.052	2,367
		(0.063)	(0.127)	(0.069)	(0.107)	
Male	0.587	0.055	0.118	0.117~	0.166~	2,272
		(0.057)	(0.123)	(0.063)	(0.090)	
PAST YEAR						
All	0.425	0.000	0.001	0.020	0.030	4,639
		(0.037)	(0.076)	(0.039)	(0.057)	
Female	0.504	− 0.061	− 0.122	− 0.026	− 0.041	2,367
		(0.051)	(0.104)	(0.055)	(0.086)	
Male	0.351	0.062	0.133	0.065	0.092	2,272
		(0.048)	(0.103)	(0.050)	(0.070)	
MENTAL HEALTH INDEX (Z-SCORE) [SR]						
ABSENCE OF MENTAL HEALTH PROBLEMS: DEPRESSION, ANXIETY, AND DISTRESS (HIGHER SCORE INDICATES FEWER MENTAL HEALTH PROBLEMS)						
All	0.000	0.031	0.065	− 0.016	− 0.024	4,644
		(0.027)	(0.056)	(0.030)	(0.045)	
Female	0.000	0.069~	0.139~	0.016	0.025	2,371
		(0.036)	(0.073)	(0.042)	(0.065)	
Male	0.000	− 0.006	− 0.013	− 0.048	− 0.068	2,273
		(0.038)	(0.081)	(0.042)	(0.060)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: Any Anxiety Disorder includes Panic Disorder, GAD (without hierarchy), and PTSD. Any Mood Disorder includes Depression (without hierarchy), Bipolar I/II/Subthreshold, and Mania/Hypomania/Hypomania Subthreshold. Any Disorder includes all disorders from Any Anxiety Disorder and Any Mood Disorder as well as IED (without hierarchy) and ODD (without hierarchy). Index of mental health problems is the average of the z-scores for depression (lifetime), anxiety (lifetime), and distress (K6) after standardizing by the control mean and standard deviation (with male and female youth standardized separately). A higher mental health index score indicates better mental health. See Section 4.3 for additional details.

EXHIBIT 4.7. PARENT-REPORTED MENTAL HEALTH OF GROWN CHILDREN, AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DEPRESSION OR OTHER SERIOUS MENTAL HEALTH PROBLEM [PR]						
All	0.110	– 0.001	– 0.001	0.014	0.026	3,066
		(0.014)	(0.033)	(0.020)	(0.036)	
Female	0.096	– 0.013	– 0.029	0.015	0.026	1,518
		(0.019)	(0.041)	(0.027)	(0.046)	
Male	0.123	0.012	0.028	0.015	0.028	1,548
		(0.021)	(0.050)	(0.028)	(0.052)	
ALCOHOL OR DRUG PROBLEM [PR]						
All	0.054	– 0.012	– 0.027	0.017	0.031	3,056
		(0.010)	(0.023)	(0.015)	(0.028)	
Female	0.026	– 0.002	– 0.005	0.010	0.017	1,514
		(0.011)	(0.024)	(0.017)	(0.030)	
Male	0.082	– 0.022	– 0.050	0.023	0.043	1,542
		(0.016)	(0.038)	(0.024)	(0.046)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. Grown children (who were under age 18 at baseline and over age 20 as of December 31, 2007) of interviewed adults.

4.6 IMPACTS ON HYPOTHESIZED MEDIATORS

This section reviews effects on the mediating factors most directly relevant to the hypotheses we described in Section 4.1. The exhibits containing these results are in supplemental tables and Chapter 2.

SOCIAL ISOLATION

We analyzed a variety of measures of social networks, friendships, and social capital (Exhibit 2.11, Supplemental Exhibits 2.4–2.7). MTO has no detectable effect on adult reports of having friends in the neighborhood, visitations in their home, or casual chatting with neighbors, although experimental group adults are more likely to report having at least one friend who graduated from college. Effects on youth differ somewhat by gender. Female youth in the experimental group are less likely to have friends who have ever used drugs than controls, but otherwise, peers have similar characteristics for MTO youth across the randomly assigned treatment groups. Male youth in the experimental group are less likely to have at least one close friend and have less contact with friends from their baseline neighborhoods. Youth across treatment groups have similar reports on how good they feel about themselves or how others see them, except for some scattered, marginally significant effects toward others being less likely to see them as popular, athletic, or good students.

SAFETY AND EXPOSURE TO VIOLENCE

The MTO intervention appears to have produced improvements in safety and reductions in exposure to violence (see Exhibit 2.10). Compared with the control group, experimental and Section 8 adults report improved feelings of safety, a lower likelihood of seeing drugs sold or used, and a higher likelihood of police response to 911 emergency calls for service.

An apparent gender difference seems to emerge for youth in the extent to which MTO moves shape perceptions of safety in the local neighborhood, which could potentially help explain some of the gender differences in impacts on mental health. Assignment to the MTO experimental or Section 8 groups rather than to the control group improves feelings of safety and reduces the likelihood of seeing drugs in the neighborhood for female youth, but generally not for male youth. Female youth in the experimental group also report less unwanted sexual attention than those in the control group, a finding that is consistent with qualitative research collected during the interim study. These findings are mirrored by results suggesting that MTO moves are more likely to shift female youth than male youth into more prosocial peer groups. Whatever potential benefits MTO might generate from moving into areas with a lower prevalence of antisocial peers could be partially offset for male youth if they wind up in antisocial cliques in their new areas.

MENTAL HEALTH SERVICES

Supplemental Exhibit 4.1 shows MTO's estimated impacts on mental health services. We observe no statistically significant effects on receiving services, the number of counseling sessions, being prescribed medicine, or feeling the need for services for mental health or substance use issues. Approximately 30 percent of control group adults report receiving or needing mental health services or prescription medicines; this number is 3.6 percentage points lower for adults in the experimental group. This difference is large as a share of the control mean but not quite statistically significant at the conventional 5 percent cutoff (*p* value of .063).

4.7 INTERPRETATION OF MENTAL HEALTH RESULTS

The results for adults show that MTO is associated with significant or nearly significant effects in reducing recent psychological distress and improving overall mental health, but largely by having effects on mild manifestations of mental disorder rather than on serious mental disorders. This is clearest in the K6 results, where the mean score in both intervention groups is significantly or close to significantly lower than the control group but the percentage of respondents with extremely high scores does not differ between groups.

Results such as this are consistent with evidence from other community-level mental health interventions that low-intensity universal interventions are often successful in preventing relatively mild disorders that are comparatively easy to prevent but are ineffective in preventing more severe disorders (Offord, 2000). The standard approach to address this problem in community mental health intervention research is to use tiered interventions that add to the universal component a targeted component that provides more intensive intervention for people with high risk of disorders (Merry et al., 2004; Multisite Violence Prevention Project, 2004).

The results are somewhat different for youth, where the beneficial mental health effects of the intervention are limited to females and there is an adverse effect on Section 8 males with regard to PTSD. In contrast to the adults, female youth in the experimental group experienced reductions not only in mild distress but also for serious disorders. As a result, the same basic considerations apply in interpreting the results for female youth as for adults, with a special emphasis on the importance of finding individual-level indicators that can distinguish between females who are likely to profit from a universal MTO-like intervention and those who need more targeted interventions in conjunction with a universal intervention to prevent mental illness from occurring. Developing reliable algorithms to target indicated interventions is a major challenge in implementing effective multi-tiered community mental health interventions (Dodge, 2009).

Finally, the finding that MTO had no aggregate long-term effect on the mental health of grown children is unsurprising. We would expect a universal intervention such as this one to have no dramatic effect on the mental health of youth who had already spent a good part of their developmental years in more disadvantaged neighborhoods. Indeed, one might even expect long-term adverse mental health effects on these young adults, as they had established ties with their old neighborhoods before the MTO moves and were probably too old to benefit from the relocations.

SUPPLEMENTAL EXHIBIT 4.1. ADULT MENTAL HEALTH SERVICES IN PAST 12 MONTHS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Received any professional mental health or substance use services [SR]	0.160	– 0.019 (0.016)	– 0.039 (0.032)	0.003 (0.021)	0.006 (0.033)	3,265
Number of sessions of mental health or substance use counseling or therapy [SR]	2.478	– 0.128 (0.379)	– 0.263 (0.780)	– 0.680 (0.456)	– 1.078 (0.723)	3,256
Prescribed medicine for mental health or substance use problems [SR]	0.162	– 0.021 (0.015)	– 0.043 (0.031)	– 0.011 (0.021)	– 0.018 (0.033)	3,264
Felt the need for services for mental health or substance use problems but did not get it [SR]	0.088	– 0.003 (0.012)	– 0.007 (0.025)	– 0.022 (0.016)	– 0.035 (0.026)	3,265
Received therapy, prescribed medicine, or felt the need for services for mental health or substance use problems [SR]	0.297	– 0.036~ (0.019)	– 0.075~ (0.040)	– 0.022 (0.026)	– 0.036 (0.041)	3,264

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All adults interviewed.

SUPPLEMENTAL EXHIBIT 4.2. YOUTH PSYCHOLOGICAL WELL-BEING, AGES 10–20 AND AGES 15–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
YOUTH AGES 10–20						
PSYCHOLOGICAL DISTRESS INDEX (K6), PAST MONTH [SR]						
K6 Z-SCORE (HIGHER SCORE INDICATES GREATER DISTRESS)						
All	0.000	– 0.022 (0.040)	– 0.046 (0.083)	0.039 (0.045)	0.059 (0.068)	5,101
Female	0.101	– 0.080 (0.056)	– 0.161 (0.112)	0.010 (0.064)	0.016 (0.100)	2,600
Male	– 0.096	0.036 (0.052)	0.077 (0.113)	0.067 (0.058)	0.096 (0.082)	2,501
SERIOUS MENTAL ILLNESS (K6 RAW SCORE ≥ 13)						
All	0.064	– 0.008 (0.010)	– 0.016 (0.020)	0.009 (0.011)	0.013 (0.016)	5,101
Female	0.081	– 0.021 (0.014)	– 0.042 (0.028)	0.004 (0.016)	0.006 (0.025)	2,600
Male	0.048	0.005 (0.012)	0.012 (0.027)	0.013 (0.013)	0.019 (0.019)	2,501

SUPPLEMENTAL EXHIBIT 4.2. (CONTINUED)	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
BRIEF STRENGTHS AND DIFFICULTIES QUESTIONNAIRE (SDQ) [SR]						
SDQ RAW SCORE (HIGHER SCORE INDICATES MORE DIFFICULTIES)						
All	3.123	– 0.092	– 0.191	0.083	0.125	5,101
		(0.070)	(0.145)	(0.077)	(0.115)	
Female	3.211	– 0.141	– 0.284	0.166	0.260	2,600
		(0.097)	(0.197)	(0.111)	(0.174)	
Male	3.040	– 0.039	– 0.085	– 0.004	– 0.006	2,501
		(0.090)	(0.195)	(0.096)	(0.138)	
SERIOUS BEHAVIORAL OR EMOTIONAL PROBLEMS (SDQ RAW SCORE ≥ 6)						
All	0.102	– 0.018	– 0.037	0.017	0.026	5,101
		(0.011)	(0.024)	(0.013)	(0.020)	
Female	0.123	– 0.027	– 0.054	0.031	0.048	2,600
		(0.016)	(0.033)	(0.020)	(0.032)	
Male	0.081	– 0.008	– 0.018	0.004	0.005	2,501
		(0.015)	(0.032)	(0.016)	(0.023)	
CALM AND PEACEFUL MOST OF THE TIME OR MORE OFTEN IN THE PAST MONTH [SR]						
All	0.599	0.020	0.042	– 0.007	– 0.010	5,099
		(0.018)	(0.038)	(0.020)	(0.030)	
Female	0.527	0.035	0.070	0.021	0.032	2,600
		(0.027)	(0.053)	(0.029)	(0.046)	
Male	0.667	0.006	0.013	– 0.035	– 0.050	2,499
		(0.025)	(0.054)	(0.028)	(0.041)	
YOUTH AGES 15–20						
MENTAL HEALTH INDEX (Z-SCORE) [SR]						
ABSENCE OF MENTAL HEALTH PROBLEMS: DEPRESSION, ANXIETY, AND DISTRESS (HIGHER SCORE INDICATES FEWER MENTAL HEALTH PROBLEMS)						
All	0.000	0.051~	0.106~	– 0.003	– 0.005	3,621
		(0.030)	(0.062)	(0.033)	(0.050)	
Female	0.000	0.096*	0.193*	0.048	0.075	1,845
		(0.041)	(0.082)	(0.045)	(0.071)	
Male	0.000	0.007	0.014	– 0.054	– 0.078	1,776
		(0.042)	(0.090)	(0.047)	(0.069)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.

Measures: Psychological distress consists of 6 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 for control group youth (with male and female youth standardized separately). Serious mental illness is defined as a raw score of 13 or higher on the K6. Strengths & Difficulties consists of 5 behavioral and emotional items (obedience, worry/anxiety, unhappiness, getting along better with adults than peers, attention span) scaled on score from 0 (no behavioral/emotional problems) to 10 (severe behavioral or emotional problems). A score of 6 or higher indicates serious behavioral/emotional problems. Index of mental health problems is the average of the z-scores for depression (lifetime), anxiety (lifetime), and distress (K6) after standardizing by the control mean and standard deviation (with male and female youth standardized separately). A higher mental health index score indicates better mental health. See Section 4.3 for additional details.

CHAPTER 5

IMPACTS ON ECONOMIC SELF-SUFFICIENCY

This chapter summarizes the MTO demonstration's impacts on employment, earnings, income and poverty, receipt of social assistance, and savings and assets. We examine economic self-sufficiency outcomes derived from the long-term survey based on adult and older youth self-reports and parental reports on their grown children. We also use administrative data from state unemployment insurance (UI) earnings records, Temporary Assistance for Needy Families (TANF) records, and food stamp records to capture quarterly earnings and employment levels, and receipt of public assistance.

Generally, we find no persistent systematic MTO impacts on employment, earnings, income, and other economic outcomes in our comparisons of the treatment groups. Our analysis of administrative UI data finds that experimental and Section 8 adults had significantly lower employment and earnings than the control adults in the first two years following random assignment, which suggests that moving may have had an initially disruptive effect. The effects on adult employment reversed and became positive—but not statistically significant—by five to six years after random assignment (seen in the interim follow-up).

The overall earnings and employment impacts measured using UI data remain small and positive (but still not statistically significant) for 10 years following random assignment. The experimental and Section 8 groups experience few detectable effects on income, social assistance receipt, savings, and assets, although compared with controls, adults in both of the treatment groups report higher levels of food sufficiency in the long-term survey. There are no strong systematic impacts on the employment and earnings of older youth (15–20 years old as of the end of 2007) or grown children (under 18 years at baseline, but 21–30 years old at the end of 2007).

5.1 BASELINE AND CONTROL GROUP CONTEXT

Exhibit 5.1 presents baseline characteristics for adults interviewed in the long-term survey, overall and separately by randomized treatment group. This exhibit (which is analogous to Exhibit 1.2) focuses on several baseline economic measures. At the time of random assignment, 25 percent (24 percent for the control group) of adults reported being employed. This low employment rate is not surprising given that the sample consisted largely of female household heads with limited education, most of whom were receiving public assistance (Aid to Families with Dependent Children [AFDC]; now called TANF) and living in public housing projects in high-poverty inner-city neighborhoods. Seventy-six percent of sample members were receiving AFDC at baseline and 81 percent were receiving food stamps (now called Supplemental Nutrition Assistance Program or SNAP). Further, nearly all adults (93 percent) had received AFDC at some point in the past. The average annual household income at baseline was $12,709 (in 2009 dollars),[1] well below the official poverty line of $17,285 for a single parent with two children (Denavas-Walt, Proctor, and Smith, 2010). Moreover, median annual income at baseline was even lower—approximately $10,614.

The labor market conditions, tax-transfer system incentives, and constraints facing sample members have changed dramatically since they were enrolled in the demonstration in the mid-1990s. The national unemployment rate, which was 5.6 percent at the start of the demonstration in the last quarter of 1994, declined during the sustained 1990s boom, falling to 3.9 percent in the last quarter of 2000. Labor market improvements during the late 1990s, which were strong in some large U.S. cities, including Boston, were particularly great for disadvantaged workers. But

---

1 All dollar values reported in this chapter are in constant 2009 dollars, unless otherwise noted.

the labor market deteriorated in the early 2000s, with the national unemployment rate rising to 6.0 percent in 2003, recovering to 4.6 percent in 2007, skyrocketing during the recession of 2008–2009, reaching 10.0 percent in the last quarter of 2009 and remaining at 9.4 percent as of December 2010. (The median household income followed a generally similar pattern over this period.)[2] The recent economic downturn, occurring at about the time of long-term survey data collection in 2008 and 2009, may have hit those at the lowest end of the income distribution the hardest. Data from the Census Bureau show that the official poverty rate increased from 12.5 percent in 2007 to 14.3 percent in 2009, with nearly 43.6 million people living below the federal poverty threshold in 2009 (Denavas-Walt, Proctor, and Smith, 2010).

State welfare systems also changed substantially over the course of the study period, starting with state welfare waivers in the early 1990s and then federal welfare reform in 1996. These changes, combined with the expansion of the Earned Income Tax Credit from 1993 to 1996, increased the financial and social incentives for female heads of household to move off public assistance and into employment. National data on single female household heads indicate large declines in welfare receipt and large increases in employment rates and labor market earnings from the mid-1990s to 2000 (Blank, 2002). Much of the decline in TANF income support was the result of a decrease in the share of eligible families who were receiving assistance, rather than the result of any decrease in the share of families eligible for TANF. In 2005, only 40 percent of eligible families received TANF cash assistance. By comparison, about 80 percent of eligible families received AFDC during the two decades prior to welfare reform (Schott, 2009). Food stamp caseloads increased dramatically during the recent economic downturn (by 37 percent through February 2010). In contrast, TANF caseloads have increased only by 10 percent overall, and 23 states have caseloads that either increased by less than 5 percent or declined (Pavetti and Rosenbaum, 2010).

The changing economic and policy environment from the mid-1990s to the beginning of the 2000s improved labor market opportunities for single mothers and disadvantaged workers with concurrent large reductions in welfare caseloads. The recession of 2008–2009 and its aftermath (including a tightening of state and local government budgets) are expected to have created labor market difficulties and substantial fluctuations in labor market participation for adults during the long-term survey period. The weakening of labor market opportunities at the time of long-term follow-up in the late 2000s coincides with many of the MTO adults—largely women from disadvantaged backgrounds—aging into their 40s and 50s and "naturally" cutting back on employment and hours worked, particularly in the face of mounting health problems.

---

2 Median income increased in the mid- to late-1990s, peaked in 1999, declined in the early 2000s, recovered modestly from 2004 to 2007, and declined again through 2009, during the recent recession.

EXHIBIT 5.1. SELECTED BASELINE CHARACTERISTICS OF ADULTS SURVEYED IN THE LONG-TERM EVALUATION: WORK, PUBLIC ASSISTANCE, AND INCOME				
OUTCOME	CONTROL GROUP	EXPERIMENTAL GROUP	SECTION 8 GROUP	ALL GROUPS
ADULT EMPLOYMENT STATUS				
Working at baseline [SR]	23.8%	25.7%	26.5%	25.3%
Not working, but previously worked [SR]	56.4%	52.5%	55.1%	54.5%
Never worked for pay [SR]	19.8%	21.8%	18.4%	20.2%
HOUSEHOLD PUBLIC ASSISTANCE RECEIPT				
Receiving aid to families with dependent Children (AFDC) [SR]	76.3%	76.3%	73.6%	75.6%
Ever received AFDC [SR]	93.6%	94.3%	92.1%	93.4%
Working and not receiving AFDC [SR]	14.7%	15.9%	17.4%	15.9%
Receiving food stamps [SR]	81.8%	81.6%	80.7%	81.4%
Receiving Women, Infants and Children (WIC) Nutrition Program [SR]	36.7%	36.6%	37.7%	36.9%
Receiving Supplemental Security Income (SSI) [SR]	16.4%	16.1%	17.0%	16.4%
Receiving Social Security Disability Insurance (SSDI) [SR]	8.4%	9.0%	7.3%	8.4%
HOUSEHOLD INCOME (2009 DOLLARS)				
Average income [SR]	$12,438.64	$12,865.83	$12,788.32	$12,709.32
Median income [SR]	$10,353.36	$10,629.45	$10,891.88	$10,613.94
MISCELLANEOUS				
Owned a car at baseline [SR]	17.0%	19.0%	19.0%	18.3%
Very sure would like living in a neighborhood with neighbors who earn more than them [SR]	45.5%	41.5%	46.4%	44.2%
INTERVIEWED N	1,139	1,456	678	3,273

Notes: The numbers represent mean values on baseline characteristics for each treatment group and overall, weighted to reflect randomization ratios and the selection of adults into the interview sample and the two-phase sampling design of the long-term evaluation.
Square brackets indicate the source of the outcome information: SR = self-report.
Data source and sample: MTO Participant Baseline Survey. Adult long-term survey. All adults interviewed.

5.2 HYPOTHESES ABOUT EFFECTS ON ECONOMIC SELF-SUFFICIENCY

One primary motivation for the demonstration is to measure the impacts of neighborhood environments on the economic self-sufficiency outcomes of low-income families. These outcomes include employment and earnings, income and public assistance receipt, and savings and assets. Residential mobility might directly affect these outcomes in several ways (see Exhibit 5.2), described below.[3]

- Lower-poverty areas are likely to have lower unemployment rates and faster job growth than higher-poverty areas. Improved opportunities may generate higher employment and earnings, an increase in reservation wages (the lowest wage at which an individual will accept a job), and better jobs in terms of wages and benefits. Increased employment options for the experimental and Section 8 groups may also reduce welfare eligibility and participation and free up financial resources for savings.

- Access to housing vouchers through MTO increases the ability to relocate nearer potential jobs, rather than being tied to the location of a public housing project. This in turn reduces job-search costs and, once employed, commuting costs. This may lead to increased employment and earnings, lower reservation wages, and less dependence on public assistance.[4] If relocation leads to a broader range of employment opportunities, it may also lead to increased wages and fringe benefits (Weinberg, Reagan, and Yankow, 2004).

- Relocation to a safer neighborhood may reduce stress and anxiety and, more generally, may improve mental health and sense of control over one's life. This in turn could result in more active job searching, lower absenteeism, improved workplace relations and productivity, and,

therefore, increased employment and earnings and lower welfare dependence. Reduced stress and anxiety due to less crime and violence in a person's neighborhood may also free up cognitive space for evaluating financial decisions and thinking about long-term financial planning.

- Community members in low-poverty areas may be more supportive of work and less accepting of welfare than those in public housing projects. If sample members are surrounded by social norms that encourage work over welfare, this might increase job-search effort, employment, and earnings, and may also reduce welfare participation.

- Social networks in less economically disadvantaged locations may provide access to new job opportunities. Although relocation may lead to an initial disruption of social networks, these effects may diminish as networks are established in new neighborhoods. Recent research (for example, Bayer, Ross, and Topa, 2008; Ioannides and Loury, 2004; and Maurin and Moschion, 2009) points to potentially important effects of neighborhood social networks on labor market outcomes.

- Relocation may result in improved physical health potentially through fewer environmental hazards, more opportunities to walk or exercise in public spaces, or improved access to health care. Improved health can reduce rates of absenteeism and raise job productivity, leading to enhanced job prospects and earnings.

- Relocation to new lower-poverty neighborhoods may lead to increased access to and reliance on formal banks, as opposed to high-cost alternatives more prevalent in high-poverty areas, such as check cashers and payday lenders.

The disruptions of social networks could make it more difficult for MTO movers to find work and arrange informal and affordable child care in the short run and thus lead to higher public assistance receipt. However, the impact of moves on access to job search assistance, affordable child care, and transportation is ambiguous over the long term.

---

3 See Orr et al. (2003) for the original conception of this logic model. Shroder (2002b) and Corcoran and Heflin (2003) provide a discussion and hypotheses on the more specific effects on labor supply of federal housing programs, compared with neighborhood environments more generally.

4 This is a formulation of the "spatial mismatch" hypothesis, for which an extensive literature exists (for example, see Ihlanfeldt and Sjoquist, 1998).

EXHIBIT 5.2. HYPOTHESIZED PATHWAYS THROUGH WHICH MTO MAY IMPACT ECONOMIC SELF-SUFFICIENCY

MTO INTERVENTION → RELOCATION TO LOW-POVERTY NEIGHBORHOODS

COMMUNITY-LEVEL MEDIATORS

PERSON- AND FAMILY-LEVEL MEDIATORS

ECONOMIC SELF-SUFFICIENCY

Community Norms and Values
· Shared community value of work
· Peer pressure to work/not depend on welfare
· Welfare agency attitudes toward work
· Shared community value of saving
· Peer pressure to build assets

Social and Physical Environment
· Reduced crime and violence
· Lower delinquency rate
· Welfare agency resources

Economic Opportunities
· Lower unemployment rates
· Better access to the labor market
· Better wages and fringe benefits
· Greater proximity to jobs
· Change in state welfare system policies
· Access to formal banks
· Cheaper financial services

Positive Effects
· Improved mental health
· Improved control over lives
· Reduced stress/anxiety
· Improved outlook
· Development of new social networks capable of reliable financial support
· More active job search
· More use of networks for labor market information
· Feelings of safety and security
· Higher wages

Negative Effects
· Short-run disruption of existing social networks
· Less informal childcare in the short run
· Social connections

· Earnings
· Employment
· Decrease/stop monthly receipt of TANF, AFDC, Food Stamps/SNAP, SSI benefits
· Bank account
· Car ownership
· Less debt
· Credit card ownership

Exhibit 5.1 is a modified version of Exhibit 7.1 in the MTO interim impacts evaluation (see Orr et al., 2003)

EMPLOYMENT

The existing evidence about the effects on adult employment from residential mobility programs is somewhat mixed. Rosenbaum and Popkin's (1991) quasi-experimental analysis of female household heads in Chicago's Gautreaux Assisted Housing Program found substantially higher employment rates (14 percentage points) for those who moved to the suburbs than for those who moved within the city. The longer-term and more comprehensive follow-up analysis of Gautreaux's impacts on low-income, black female household heads by Mendenhall and colleagues (2006) finds little systematic impact on employment of moves to suburbs versus the city but persistent positive employment effects of being initially placed in less segregated neighborhoods (a lower percentage of black residents) with greater neighborhood resources. A recent study by Anil and colleagues (2010) also finds that moves through HOPE VI and similar programs had significant positive effects on employment for residents who moved, relative to other public housing tenants in Atlanta, Georgia.[5]

However, early analyses of MTO on adult labor market outcomes for Boston and Baltimore found no significant effects on adult household head employment in the first few years after randomization (Katz, Kling and Liebman, 2001; Ludwig, Duncan and Pinkston, 2005). A survey of early enrollees in Los Angeles found no significant short-term employment effects but did find modest evidence of increases in hours of work and weekly earnings for the experimental and Section 8 group household heads, relative to those in the control group (Hanratty, McLanahan and Pettit, 2003).

---

5 In related research, Jacob and Ludwig (2011) find negative effects of access to means-tested housing vouchers on adult labor supply (earnings and employment) using data from a randomized housing voucher wait-list lottery in Chicago. Their study sample consists of adults who were already living in private-market housing at the time they applied for a housing voucher. Those families (unlike MTO households) do not experience very much change in neighborhood "types," and—also unlike MTO—do end up experiencing large increases in marginal tax rates on earnings and large increases in disposable income as a result of receiving a housing voucher. So the "treatment" in that study is quite different from the one examined in MTO. However, they also present results for the subset of Chicago housing voucher applicants who live in public housing at baseline, essentially a replication of our comparisons of the MTO Section 8 and control groups. For this subsample, Jacob and Ludwig find no significant impacts of access to means-tested housing vouchers on either employment or public assistance receipt in an analysis based on Illinois administrative UI and TANF data.

At the interim evaluation, analysts found no systematic, significant effects on adult employment, earnings, or welfare participation, even though experimental and Section 8 household heads moved to and remained in neighborhoods with higher employment rates than those of control families (Orr et al., 2003; Kling, Liebman and Katz, 2007; Ludwig et al., 2008). In a subsequent nonexperimental analysis of survey data from the interim evaluation, Clampet-Lundquist and Massey (2008) re-estimate MTO impacts on the basis of the cumulative time families spent in low-poverty neighborhoods and find that families with longer stays in new neighborhoods saw positive impacts on employment and earnings. These results are likely subject to significant selection biases because within each treatment group families were not randomly assigned to different types of neighborhoods and since persistence in low-poverty neighborhoods also is likely to be correlated with unobserved family attributes that may lead to improved outcomes (as discussed by Ludwig et al., 2008). Moreover, the interim evaluation found no evidence of effects on average household income, poverty, or food insecurity.

It is possible that living in a less-disadvantaged neighborhood during childhood could have more pronounced effects on labor market outcomes. A large body of nonexperimental research finds that children who grow up in poor neighborhoods have worse economic outcomes during adolescence and adulthood than children who grow up around more affluent neighbors, although the effects are somewhat attenuated when controlling for other aspects of family background (see, for example, Corcoran et al., 1992; Page and Solon, 2003; Sharkey, 2008). Research using quasi-experimental designs (natural experiments) to evaluate the long-term impacts of housing mobility interventions on child economic outcomes yields mixed results. Oreopolous (2003) finds no systematic impacts of child neighborhood environments on adult labor market outcomes for children assigned to public housing projects in substantially different neighborhoods of Toronto. Rosenbaum's (1995) analysis of a 1988 follow-up survey of families who moved with Gautreaux vouchers finds strong positive gains in educational and economic outcomes for the children of suburban movers relative to city movers, but longer-term follow-ups find less striking

contrasts between suburban and city movers (DeLuca et al., 2010). Gould and colleagues (2009) find positive impacts on long-term adult economic outcomes for Yemenite refugees to Israel who as children were initially placed in more prosperous neighborhoods with better infrastructure.

At the interim evaluation, the MTO experimental treatment was associated with a substantial increase in female youth school enrollment and a substantial decrease in female youth idleness (defined as being neither employed nor enrolled in school) compared with the controls. The school enrollment rate increased by 16 percentage points (p=.096) and the idleness rate declined by 16 percentage points (p=.074) for female youth in experimental group families who moved with an MTO voucher (Orr et al., 2003, p. 132, Exhibit 7.6, p. 133, and Exhibit D7.1b, p. D-11). MTO had no comparable treatment effects on males in the interim evaluation.

Other factors that vary across neighborhoods can also affect youth economic outcomes. School-to-work programs administered by local public schools may help youth secure internships while still enrolled in high school and help non-college-bound youth secure employment after high school. Local social norms about 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 awards for behavior that supports schooling or employment can also affect youth decisions about whether to participate in the formal labor market or pursue underground or informal work.

PUBLIC ASSISTANCE AND INCOME

The MTO demonstration's impacts on welfare receipt are likely to mirror the impacts on employment and earnings. Moves from public housing to private housing in lower-poverty areas may affect welfare participation (TANF, food stamps, and Supplemental Security Income). To the extent that the demonstration increases employment and income, it can be expected to reduce eligibility for and reliance on social assistance. Changes in norms and attitudes toward the use of public assistance in new neighborhoods may also play a role.

MTO impacts on household income depend primarily on earnings and public assistance outcomes, because these are the main components of income for most sample members. An increase in earnings can lead to a decrease in welfare benefits, depending on the thresholds set for earned income disregards; the overall effect on cash income is expected to be positive. However, the size of the effect on overall household income depends on the reduction in welfare benefits for a given increase in earnings and the proportion of sample members that receives cash assistance.

SAVINGS AND ASSETS

The MTO demonstration's impacts on savings and assets are influenced by impacts on employment, earnings, and income, as well as access to community-based financial institutions, new social norms, and the ways in which these changes alter an individual's financial and savings decision-making. The initial MTO moves may disrupt informal networks available for instrumental support and financial assistance in emergencies. If the demonstration improves economic conditions for families, this may lead to changes in their financial behavior or net worth simply by increasing their capacity to save or acquire assets.

The demonstration might also affect savings and asset outcomes conditional on earnings or income levels if moves to low-poverty neighborhoods provide increased access to formal banking opportunities, better information or knowledge about returns on investments through social networks and local institutions, or exposure to neighborhood residents who utilize mainstream financial services. Formal financial services might also be more readily available in communities where a larger proportion of residents are employed in the formal labor market (Vermilyea and Wilcox, 2002). High-poverty areas may be less likely to have local bank branches, although the evidence on this point is mixed (Caskey, 1997; Vermilyea and Wilcox, 2002). However, high-poverty areas are more likely to have check cashing services, payday or auto title lenders, pawnshops, and rent-to-own stores that charge very high interest rates and may, as Carr and Schuetz argue, "undermine the ability of individual households to accumulate assets and build wealth" (2001, p. 5; see also Temkin and Sawyer, 2004; Rhine et al., 2001). Moves to a low-poverty neighborhood may

affect financial decisions, because movers are exposed to information about the importance of saving. Social values in destination neighborhoods may encourage saving; moreover, individuals may realize they can save more using formal financial institutions, compared with informal institutions like check cashers and payday lenders, and use them more as a result. New surroundings may lead movers to see future consumption through a different psychological frame, and decreases in stress may free up some mental space that could be used for making financial decisions (Loewenstein and O'Donoghue, 2004; Gennetian, Mullainathan, and Shafir, forthcoming).

MOVES AND ECONOMIC SELF-SUFFICIENCY

The mobility experiences of the experimental and Section 8 groups can help us understand how the demonstration might affect economic self-sufficiency. Treatment group differences in economic behavior over the long-term may not be detectable at a shorter-term follow-up. MTO moves may initially be disruptive, so any improvements in labor market behavior may not be observed until the effects of this initial disruption dissipate. The impact of an initial disruption will also be sensitive to the overall number of moves and whether the initial move precipitated high subsequent mobility, potentially including a move back to the participant's neighborhood of origin. Labor market outcomes may also be responsive to threshold effects based on the cumulative environmental and social improvements that result from moves to lower-poverty neighborhoods and not be apparent until after a critical mass of exposure to improved neighborhood environments.

At the long-term follow-up, the experimental and Section 8 groups on average had moved into neighborhoods that appear to have more favorable employment opportunities, as well as norms more supportive of work, than do the neighborhoods of the control group. The experimental and Section 8 group members reside in neighborhoods with substantially lower poverty rates, higher adult employment rates, and higher proportions of two-parent families (Exhibit 2.7). Adults in the experimental group are significantly more likely to have at least one close friend who graduated from college and, at a marginally significant level, more likely to have a close friend who works full-time (Exhibit 2.11). Experimental and Section 8 adults report high satisfaction with their neighborhoods

and improvements in neighborhood safety, compared with reports from control group adults. Observed improvements in safety (Chapter 2) and increases in potential adult role models with stable jobs in the new neighborhoods provide a potentially favorable context for improving labor market opportunities and the attitudes of youth. Gender differences for youth on some mental health outcomes (Chapter 4) and delinquency behaviors (Chapter 6) lead one to expect more beneficial impacts of the demonstration on the labor market experiences of female youth than male youth. The MTO demonstration did not significantly strengthen social networks or improve adult access to transportation.

5.3 DATA SOURCES FOR MEASURES OF EMPLOYMENT AND EARNINGS

Employment and earnings outcomes are derived from respondent self-reports from the long-term evaluation survey and quarterly earnings data of core household members come from state UI records. Administrative data is available for core adult household heads, children who are now grown adults, and older youth.

The long-term survey includes a series of questions to adults about their current employment status, hours of work, earnings from their current main job, total earnings for the most recent past calendar year, employee benefits, job-search behavior, informal work, and the duration and characteristics of their current main job.[6] These employment questions are the same as the standard employment status questions from the Current Population Survey used by the Bureau of Labor Statistics to measure employment, unemployment, and labor force participation. Respondent descriptions of their industry and occupation are coded using Census 2000 three-digit industry and occupation codes. Older youth are similarly asked a battery of standard employment status questions, as well as a further series of questions on earnings, informal employment arrangements, and enrollment status and participation in job training programs. A host of items on the long-term survey cover potential mediators such as social networks,

---

6 All survey and administrative records outcomes are adjusted for extremely high and unreasonably low outliers and any logical inconsistencies. Outliers on survey outcomes are adjusted based on a case-by-case review and then any value above the 99th percentile is set to missing. Zero values of total income are assigned the fifth percentile value. Sensitivity analyses show that these adjustments to outliers do not qualitatively alter the results. We've also conducted sensitivity analyses limiting analyses of survey data outcomes to the available sites with UI data, and, separately for the sites with available TANF/food stamp data (not shown).

neighborhood satisfaction, and household composition. Census data on the characteristics of current neighborhoods provide information on mediating factors for employment, which can be used to test alternative hypotheses about MTO impacts on labor market outcomes.

Administrative data from state UI records collected and processed from each of the states of the five main demonstration sites—California (for the Los Angeles site), Illinois (for the Chicago site), Maryland (for the Baltimore site), Massachusetts (for the Boston site), and New York (for the New York City site)—and the state of Florida provide quarterly earnings information for core household members (adults, youth, and grown children). Agencies matched these administrative records to core members by social security numbers. Adult data are collapsed into years since random assignment and grouped into two-year periods. The results are annualized to illustrate the impact over time from 1 to 10 years since random assignment.

Three of the five main site states provided individual-level earnings information on each sample member who matched to the UI records. Data from Massachusetts and New York are not available at the individual level but rather collapsed at a group level.[7] For Massachusetts, cell sizes were restricted to 10 or more individuals; for New York, each cell was required

to have 10 or more matches, resulting in larger cell sizes to ensure a reportable match rate. The most recent quarter of data varies by state: quarter 4 of 2008 for Maryland, quarter 3 of 2008 for Illinois, quarter 1 of 2009 for California, and quarter 4 of 2009 for both Massachusetts and New York.

We focus on two primary labor market outcomes using the UI administrative data: annual earnings and the proportion of quarters that individuals were employed. Annual earnings were calculated by adding quarterly earnings data. For adults, annual earnings were then averaged across two-year periods.[8] The share of quarters employed indicates the portion of the period for which a person had recorded earnings more than zero. The number of quarters with positive earnings is divided by eight (the number of quarters in the two-year period).

More generally, the administrative data serve an important complement to the survey data on employment by allowing us to analyze the evolution of employment and earnings impacts over time, since random assignment for the full sample of core participants (whether or not they were eligible for or responded to the long-term survey). However, UI data does have some disadvantages. Specifically, informal and uncovered employment are not measured, errors in social security numbers may lead to incorrect matches, earnings are not captured for out-of-state movers, information is not provided about job quality and employee benefits, and it is not possible to disentangle the contributions of weekly hours, quarterly weeks worked, and hourly wages to quarterly earnings.

Traditional measures of employment, labor force participation, and earnings gauge the labor market success of sample adults. But the interpretation of employment outcomes for youth also needs to take into account the importance of schooling as an alternative use of time that can improve eventual adult labor market outcomes. We use a summary measure of "idleness"— being out of work and not enrolled in school—to provide a more accurate picture of youth labor market outcomes.

---

7 To maximize use of the individual and aggregate cell data, UI analyses use the weighted share of participants in each site to create the combined impact estimates. To create the combined estimates, we use the weighted average of the estimate from each site. To calculate the combined standard error, we used the formula SE = sqrt(se1^2*share1^2 + se2^2*share2^2+…+ se4^2*share4^2) where "se1" is the standard error of the ITT estimate of the first state, "share1" is the weighted share of the sample for the first state, etc. In our adult analyses for sites with individual-level data (i.e., Baltimore, Chicago, and Los Angeles) we control for our standard set of covariates (except for the survey release flag) and also include dummies for quarter of random assignment, and earnings in the year prior to random assignment and weighted to account for changes in randomization ratios. The analyses with Boston aggregate adult data control for the weighted cell means for baseline work status, baseline high school completion status, and dummies for quarter of random assignment. The New York City aggregate analyses do not control for any baseline covariates. Youth analyses for sites with individual-level data include youth ages 15 to 20 as of December 31, 2007 and control for our standard set of covariates. The Boston aggregate youth analyses include youth ages 12 to 20 as of December 2007 (because our request file to Massachusetts did not split out ages 15-20) and include only the following covariates: gender, age as of December 2007, and the following baseline characteristics of the youth: required special medicine and/or equipment and had health problems that limited activity. The New York site youth analyses focus on youth ages 15 to 20 and do not control for any baseline covariates other than gender. In the aggregate youth analyses we are unable to cluster on family. All adult and youth UI analyses use only participants for whom we had a social security number from prior to random assignment that met a minimum level of verification by the Social Security Administration (a "best SSN" determined based on an algorithm developed by Jeffrey Liebman for the MTO interim evaluation). Participants without best SSNs were omitted from the main results, but included in sensitivity tests.

8 Any quarterly wages over $45,000 are considered outliers and first checked by examining the two surrounding quarters to try to rectify any outliers caused by single-quarter data entry error, and otherwise top coded at $45,000.

5.4 LONG-TERM IMPACTS ON EMPLOYMENT AND EARNINGS OF ADULTS

Exhibit 5.3 plots the employment rate of sample adults by random assignment group from 1995 to 2008 using data from state UI records covering all five demonstration sites. For adults in the control group, the employment rate (the percentage with positive quarterly earnings) more than doubled, from 23.7 in the first quarter of 1995 to 51.5 in 2000, in the face of welfare reform and the rapid economic expansion of the second half of the 1990s. However, the employment gains for adults in the control group somewhat eroded in the weaker labor market of the 2000s, with some employment decline in the 2001 recession and at the start of the recession of 2008–2009.

Part of the employment gains in the late 1990s may have reflected life-cycle work patterns that ceased as the typical MTO adult entered her 40s in the 2000s.

The experimental and Section 8 adults show broad patterns quite similar to the control group, with rapidly rising employment rates from 1995 to 2000 and modest employment declines in the 2000s. By 2000, higher employment rates are apparent for experimental and especially Section 8 adults, relative to control group adults. However, this pattern of treatment group employment advantage eroded by 2003. Adults in all three random assignment groups had similar employment trends and ended up with much higher employment rates by the final survey in 2008 (around 45 percent) than at the time of random assignment in 1995 (around 25 percent).

EXHIBIT 5.3. ADULT FIVE-SITE QUARTERLY EMPLOYMENT RATE BY RANDOM ASSIGNMENT GROUP AND CALENDAR QUARTER
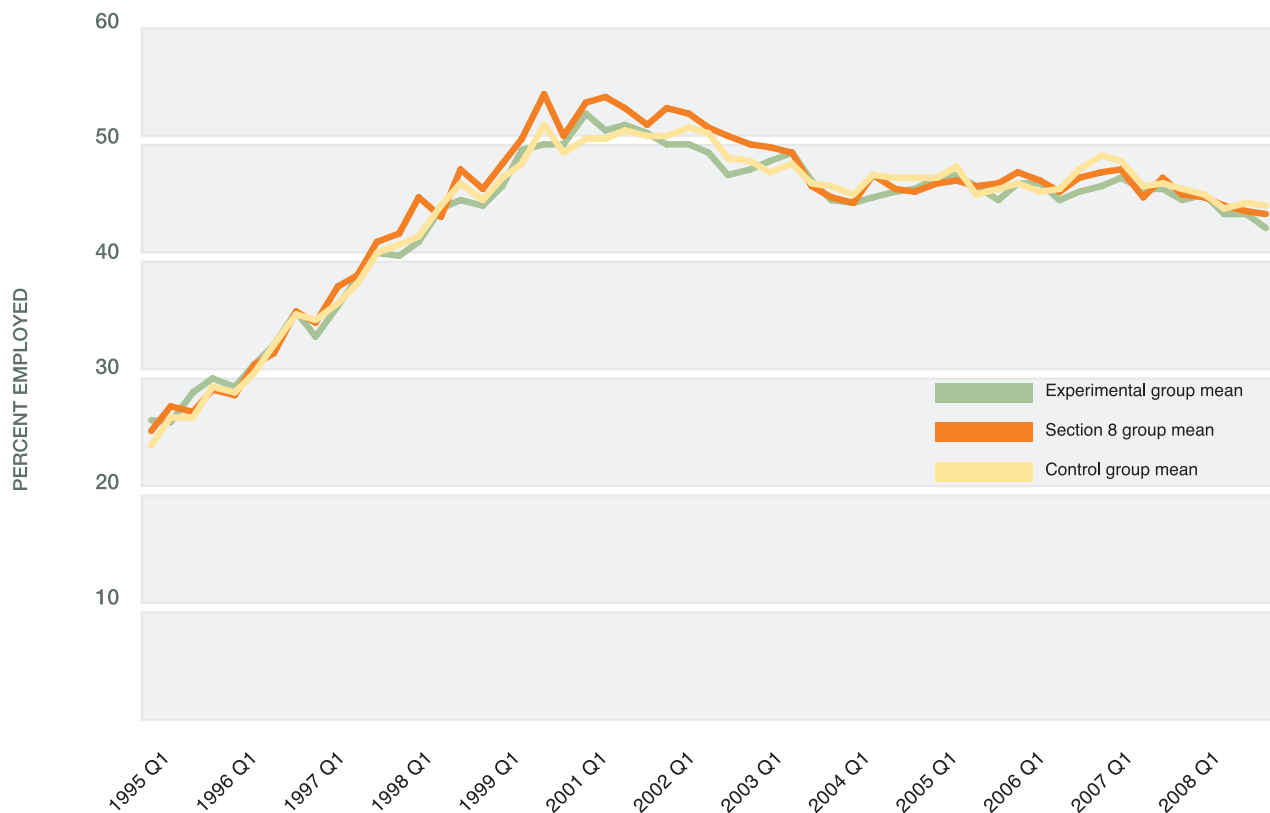
Data source and sample: These analyses use individual data from Unemployment Insurance records from Maryland, Illinois, California, and Florida for those whose random assignment sites are Baltimore, Chicago, or Los Angeles. It also incorporates aggregated Unemployment Insurance data from Massachusetts and New York, which represent individuals whose random assignment sites are Boston or New York City. All sample adults with baseline consent (N = 4,194).

Exhibit 5.4 plots the group employment rates by time since random assignment using quarterly data for all five sites. The patterns suggest a small disruption effect of moves on employment during the first two years of follow-up, as evidenced by the slightly higher employment rate for the control group (shown in the solid gray line) than for either the experimental or Section 8 groups. The adult employment rates for the experimental and Section 8 groups increased relative to the control group in the second year after random assignment and were slightly higher than the control group roughly four years after random assignment. The employment rates for all three groups followed the same trend and remain generally similar over the remaining follow-up period through 43 quarters, or roughly 10.5 to 11 years after random assignment.

EXHIBIT 5.4. ADULT FIVE-SITE EMPLOYMENT RATE BY RANDOM ASSIGNMENT GROUP AND THE NUMBER OF QUARTERS SINCE RANDOM ASSIGNMENT
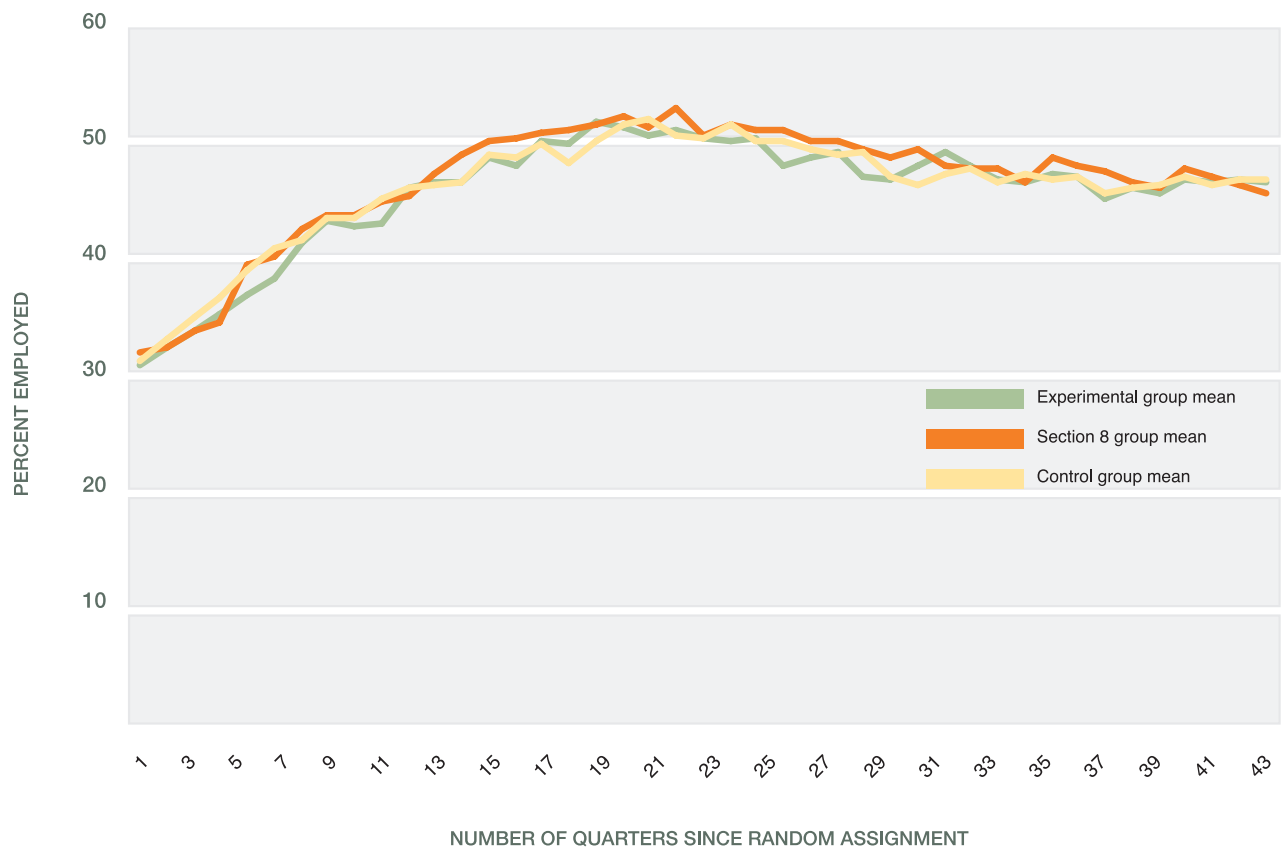
PERCENT EMPLOYED

NUMBER OF QUARTERS SINCE RANDOM ASSIGNMENT

Experimental group mean

Section 8 group mean

Control group mean

Data source and sample: These analyses use individual data from Unemployment Insurance records from Maryland, Illinois, California, and Florida for those whose random assignment sites are Baltimore, Chicago, or Los Angeles. It also incorporates aggregated Unemployment Insurance data from Massachusetts and New York, which represent individuals whose random assignment sites are Boston or New York City. All sample adults with baseline consent (N = 4,194).

Exhibit 5.5 more formally presents estimates of the demonstration impacts on employment and earnings of sample adults using the state UI records from a pre-random assignment period to the ninth and tenth year of follow-up for the four sites—Baltimore, Boston, Los Angeles, and Chicago—with complete data. The UI administrative data indicate modest negative impacts on employment and earnings for the experimental and Section 8 groups (marginally significant for the experimental group) in the first two years following random assignment.[9] This pattern may reflect initial disruption effects of MTO moves on employment relations and child care arrangements.

The experimental and Section 8 groups gain on the control group in employment over the next several years with slightly beneficial (especially for Section 8 adults), but not statistically significant, employment impact estimates by nine to ten years after random assignment. The impacts on average annual earnings over the first ten years from random assignment are modestly negative but insignificant for both the experimental and Section 8 groups, relative to the control group. Supplemental Exhibit 5.1 presents these same estimates separately by site, and suggests beneficial (but generally not significant) impacts on employment for the Los Angeles and Chicago sites, and more negative impacts for the Baltimore site and especially for the experimental group from the Boston site. The bottom line is one of little systematic MTO impact on employment and earnings averaged across sites during the first ten years following random assignment.

Exhibit 5.6 incorporates UI data on employment and earnings from all five demonstration sites for 2007, the most recent calendar year for which comparable information is available for all demonstrations sites.

There are no sizable or significant differences in employment and earnings in 2007 by random assignment group.[10]

Exhibit 5.7 shows estimates of impacts on employment outcomes based on adult self-reports from the long-term survey. The survey data mirror the administrative data in that there are no significant differences in employment or earnings outcomes for the experimental and control groups. But the long-term survey-based estimates do show a significant negative impact on current employment and a sizable negative (but not significant) impact on annual earnings for the Section 8 adults—findings not corroborated by the patterns observed in the UI data.

The observed negative effects on work and earnings in the self-reported survey data among those in the Section 8 group is a likely artifact that results from the slightly later timing of when those adults were interviewed, compared with the experimental and control groups. The supplemental exhibits examine, in more detail, the differences in employment impact estimates for the Section 8 adults between the long-term survey and UI administrative data. By the time the Section 8 adults were interviewed (starting February 2009), they were facing higher national unemployment compared with the experimental and control group adults.

A number of additional analyses examine possible explanations for the varying results by data source related to calendar timing of data collection and differences in the type and location of employment captured by source. Supplemental Exhibit 5.2 compares intention-to-treat (ITT) and treatment-on-the-treated (TOT) effects for survey data and UI administrative data for current employment around the time of the long-term survey. The estimated impacts are quite similar for the two data sources for the experimental group relative to the control group, but the Section 8 group impacts are substantially more negative for current employment in the survey than in the UI

---

9 This initial disruption effect is also described in Orr et al. (2003, p. 128): "The employment rate (fraction of quarters employed) of the experimental group moves from being modestly (but statistically significantly) lower than the control group (by 2.5 percentage points) in the first two years after random assignment to being only slightly lower (by 0.9 percentage points) and statistically indistinguishable from the control group on average in the third and fourth years after random assignment." The estimates in Exhibit 5.5 slightly differ from Orr et al. in that data are available for only four sites—New York is excluded. Exhibit 5.6 suggests that adding data from New York does not qualitatively alter the main conclusions.

10 UI data from Boston and New York were delivered as aggregate cell data. Data from Boston included means and standard deviations for each quarter, as well as annualized. The New York aggregated cell data did not include annualized standard deviations. We estimated an upper bound to employment levels using the individual level data from Baltimore, Chicago, and Los Angeles to calculate the annualized New York share employed outcome, and took the average correlations of earnings from these same individual UI sources to calculate the standard deviation for the New York annual earnings measure.

data. Supplemental Exhibit 5.3 explores this discrepancy by examining employment in the previous calendar year and including only the sample of respondents who were living in the state of the initial demonstration site at the time of the survey interview (that is, out-of-state movers were excluded). The gap between the survey and UI data results is modestly reduced overall, and specifically in California and Illinois. In fact, for Illinois, the Section 8 results for survey and UI employment are quite similar in Supplemental Exhibit 5.3, excluding out-of-state movers, as opposed to Supplemental Exhibit 5.2 where the direction of the effect on employment measured via survey is opposite the direction of the effect on employment measured via UI data. As it turns out, Chicago has the highest number of out-of-state movers compared with the other sites. Thus, this closer aligning of survey and administrative records points to a similar negative effect on current employment among Section 8 adults.

Supplemental Exhibit 5.4 further shows the industry and occupation of employed adults, measured at the time of the survey interview.[11] Adults in the experimental and control groups have similar distributions of employment across the retail, health care, education, and social service sectors and have similar broad occupational distributions. Section 8 adults, however, generally show patterns of lower employment rates in the long-term survey than the control group, across all sectors including retail and social services, and occupation levels. And, as shown in Supplemental Exhibit 5.5, experimental group adults were slightly (but not significantly) more likely than control group adults to be employed at the time of the long-term survey in jobs offering health insurance, vacation, and paid sick leave, while the Section 8 adults were less likely to be employed in jobs with such benefits.

---

11 Industry and occupation are coded using 1990 census codes.

EXHIBIT 5.5. ADULT EMPLOYMENT AND EARNINGS, ADMINISTRATIVE DATA BY YEARS SINCE RANDOM ASSIGNMENT (RA)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EMPLOYED [UI]						
THREE-SITE WEIGHTED AVERAGE (MD, IL, CA ONLY)						
Pre-RA year 1	0.276	– 0.001 (0.015)	– 0.003 (0.031)	0.005 (0.017)	0.006 (0.024)	2,365
FOUR-SITE WEIGHTED AVERAGE (MD, IL, CA, MA)						
RA years 1 and 2	0.408	– 0.023~ (0.012)	– 0.049~ (0.026)	– 0.017 (0.014)	– 0.025 (0.020)	3,250
RA years 3 and 4	0.499	– 0.003 (0.016)	– 0.006 (0.033)	0.005 (0.017)	0.008 (0.025)	3,250
RA years 5 and 6	0.519	0.005 (0.016)	0.010 (0.034)	0.015 (0.018)	0.022 (0.027)	3,250
RA years 7 and 8	0.485	0.002 (0.017)	0.005 (0.036)	0.029 (0.020)	0.043 (0.029)	3,250
RA years 9 and 10	0.465	0.007 (0.017)	0.014 (0.036)	0.026 (0.019)	0.038 (0.028)	3,250
RA years 1–10	0.475	– 0.003 (0.012)	– 0.005 (0.026)	0.012 (0.013)	0.017 (0.020)	3,250

EXHIBIT 5.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANNUAL WAGES (2009 DOLLARS) [UI]						
THREE-SITE WEIGHTED AVERAGE (MD, IL, AND CA ONLY)						
Pre-RA Year 1	$3,163.83	94.20	191.41	56.61	76.72	2,365
		(240.38)	(488.46)	(274.91)	(372.61)	
FOUR-SITE WEIGHTED AVERAGE (MD, IL, CA, MA)						
RA years 1 and 2	$5,781.17	– 377.39~	– 786.86~	– 214.97	– 317.03	3,250
		(221.62)	(462.10)	(255.44)	(376.71)	
RA years 3 and 4	$8,398.43	– 422.45	– 880.83	– 541.97	– 799.28	3,250
		(348.14)	(725.88)	(368.47)	(543.40)	
RA years 5 and 6	$10,111.07	– 274.30	– 571.93	– 24.84	– 36.63	3,250
		(436.74)	(910.63)	(459.28)	(677.33)	
RA years 7 and 8	$10,526.24	– 358.94	– 748.41	– 131.96	– 194.60	3,250
		(518.30)	(1,080.68)	(570.49)	(841.34)	
RA years 9 and 10	$10,643.49	-179.70	– 374.68	90.45	133.39	3,250
		(550.89)	(1,148.63)	(617.70)	(910.96)	
RA years 1–10	$9,092.08	– 322.56	– 672.54	– 164.66	– 242.83	3,250
		(343.66)	(716.54)	(369.25)	(544.56)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All sample adults with baseline consent.

EXHIBIT 5.6. ADULT EMPLOYMENT IN 2007, ADMINISTRATIVE DATA BY SITE						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
EMPLOYED [UI]						
Maryland	0.593	− 0.097*	− 0.181*	− 0.090~	− 0.115~	629
		(0.044)	(0.082)	(0.048)	(0.061)	
Illinois	0.471	0.021	0.064	0.020	0.030	883
		(0.038)	(0.112)	(0.045)	(0.067)	
California	0.424	0.059	0.095	0.064	0.082	853
		(0.037)	(0.060)	(0.039)	(0.050)	
Massachusetts	0.444	− 0.033	− 0.074	− 0.003	− 0.006	885
		(0.036)	(0.081)	(0.044)	(0.085)	
New York	0.431	0.004	0.010	− 0.015	− 0.033	944
		(0.040)	(0.087)	(0.040)	(0.088)	
Five-site weighted average	0.465	− 0.004	− 0.009	0.000	0.000	4,194
		(0.017)	(0.036)	(0.019)	(0.030)	
ANNUAL WAGES (2009 DOLLARS) [UI]						
Maryland	$14,420.03	− 3,090.98*	− 5,770.22*	− 1472.88	− 1,884.70	629
		(1,316.30)	(2,457.27)	(1,607.95)	(2,057.55)	
Illinois	$9,733.91	696.73	2,070.28	180.33	267.88	883
		(1,121.28)	(3,331.79)	(1,297.35)	(1,927.19)	
California	$9,649.18	709.15	1,141.41	971.73	1,256.06	853
		(1,127.95)	(1,815.48)	(1,125.39)	(1,454.68)	
Massachusetts	$12,245.56	− 755.34	− 1692.58	745.68	1429.99	885
		(1,397.82)	(3,132.25)	(1,599.76)	(3,067.87)	
New York	$11,448.08	− 73.08	− 158.73	− 260.76	− 580.57	944
		(887.41)	(1,927.62)	(880.92)	(1,961.30)	
Five-site weighted average	$11,325.14	− 347.83	− 731.73	112.93	180.50	4,194
		(523.80)	(1101.92)	(580.69)	(928.11)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from New York and Massachusetts (representing individuals whose random assignment sites are New York City and Boston). All sample adults with baseline consent.

EXHIBIT 5.7. ADULT SELF-REPORTED EMPLOYMENT AND EARNINGS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Currently employed [SR]	0.525	– 0.007 (0.021)	– 0.014 (0.042)	– 0.077* (0.028)	– 0.124* (0.045)	3,264
Currently employed full time [SR]	0.367	– 0.016 (0.020)	– 0.033 (0.042)	– 0.047~ (0.027)	– 0.076~ (0.043)	3,232
Currently employed at a job with weekly earnings above the poverty line [SR]	0.267	0.014 (0.019)	0.029 (0.038)	0.009 (0.026)	0.015 (0.042)	3,068
Currently employed at a job with weekly earnings above the federal minimum wage [SR]	0.345	0.013 (0.020)	0.026 (0.042)	– 0.025 (0.028)	– 0.040 (0.044)	3,068
Currently employed at a job with weekly earnings above $10/hr [SR]	0.243	0.005 (0.018)	0.010 (0.037)	– 0.010 (0.024)	– 0.016 (0.038)	3,068
Annual individual earnings (previous calendar year, 2009 dollars) [SR]	$12,288.51	326.94 (583.44)	677.92 (1,209.79)	– 613.60 (807.20)	– 982.43 (1,292.40)	3,141
Current weekly earnings at main job (2009 dollars) [SR]	$222.98	0.56 (11.69)	1.15 (24.03)	– 21.03 (15.82)	– 33.54 (25.23)	3,068
Employed over 1 year at current main job [SR]	0.469	– 0.007 (0.021)	– 0.014 (0.043)	– 0.063* (0.028)	– 0.101* (0.045)	3,225
In past 2 years, worked for pay [SR]	0.677	0.011 (0.019)	0.023 (0.038)	– 0.048~ (0.026)	– 0.077~ (0.041)	3,270
Currently working and household is not receiving Temporary Assistance for Needy Families [SR]	0.499	– 0.020 (0.021)	– 0.041 (0.043)	– 0.071* (0.028)	– 0.115* (0.045)	3,260

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.
Measures: Weekly earnings are first adjusted to 2009 dollars and then applied to the appropriate statistic. Poverty thresholds use the 2009 values and adjusts for head of household's age, household size, and number of kids. For the federal minimum wage, the 2009 value of $7.25 was used to measure whether respondents earned more than the minimum wage. All comparisons with hourly wage rates assumed a 40 hour work week at the hourly wage rate. Job tenure looks at the year in which respondent started their current job. If the interview year is a different year than the year they started their job, we assume that they have been at their job for more than 1 year.

5.5 LONG-TERM IMPACTS ON EMPLOYMENT AND EARNINGS OF YOUTH AND GROWN CHILDREN

Exhibit 5.8 shows the employment and school enrollment rates of grown children in the control group and summarizes the estimated impacts on the labor force status and earnings of these grown children. The top panel of the exhibit displays parental reports in the long-term survey on the current situation of their grown children. The bottom panel presents demonstration impact estimates based on administrative UI data covering grown children for quarter 4, 2007, to quarter 3, 2008, for four sites.

Compared with the control group, adult respondents in the experimental group report that their male grown children are more likely to be currently employed and less likely to be idle at the time of the long-term survey than those in the control group. Female grown children in the Section 8 group are less likely to be employed and more likely to be idle than those in the control group. Supplemental Exhibit 5.6 provides further detail on these patterns by presenting treatment group impact estimates for four mutually exclusive categories of work and schooling for grown children.

In contrast to the parental reports in the long-term survey, the UI data show that male grown children in the experimental group have negative employment and earnings outcomes, compared with the control group. Supplemental Exhibit 5.7 shows that the positive employment outcomes in the survey data are driven by the New York site, which unfortunately had to be excluded from the UI analyses because of the aggregate nature of the data and our inability to specifically examine the grown children subgroup. Los Angeles and Chicago have consistent estimated effects on male grown children from UI and parental reports. However, for the Boston site, the UI and survey data yield substantially different experimental employment impacts of opposite sign. Unfortunately, the Massachusetts UI agency did not provide individual level data for the Boston site limiting further analysis of this discrepancy.

MTO impacts on the labor market activities of youth are summarized in Exhibits 5.9 and 5.10. Exhibit 5.9 shows employment and annual earnings impacts for youth based on state UI administrative data covering the time from quarter 4, 2007, to quarter 3, 2008, for the four sites with complete administrative data: Baltimore, Boston, Chicago, and Los Angeles. The UI data indicate that youth in the experimental group have lower employment rates and earnings than youth in the control group (this finding is statistically significant overall and for males). There were no significant differences between the Section 8 and control group youth. Exhibit 5.10 presents estimates based on youth self-reports of employment, earnings, and enrollment outcomes. The survey data indicate that experimental group youth are significantly less likely to be employed than control group youth. The impact estimates on employment for female and male youth are similarly negative, although gender-specific estimates are not statistically significant. Site-by-site analyses presented in Supplemental Exhibit 5.8 suggest that experimental group youth, especially in Baltimore, are more likely to be currently enrolled in school, compared with the control group. There are no statistically significant differences by random assignment group in the long-term survey in idleness, as seen in Exhibit 5.10. A further decomposition of impacts on four mutually exclusive youth labor market and enrollment categories is given in Supplemental Exhibit 5.9.

EXHIBIT 5.8. EMPLOYMENT AND SCHOOL ENROLLMENT OF GROWN CHILDREN: PARENT REPORTS AND ADMINISTRATIVE DATA						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SURVEY DATA						
CURRENTLY EMPLOYED [PR]						
All	0.562	0.017	0.039	− 0.049	− 0.089	2,953
		(0.023)	(0.051)	(0.031)	(0.056)	
Female	0.615	− 0.030	− 0.066	− 0.086*	− 0.149*	1,480
		(0.031)	(0.067)	(0.040)	(0.070)	
Male	0.505	0.066*	0.156*	− 0.009	− 0.018	1,473
		(0.032)	(0.076)	(0.041)	(0.078)	
CURRENTLY ENROLLED IN SCHOOL [PR]						
All	0.158	− 0.007	− 0.016	− 0.001	− 0.002	3,089
		(0.016)	(0.036)	(0.023)	(0.042)	
Female	0.205	− 0.015	− 0.033	0.006	0.010	1,527
		(0.025)	(0.054)	(0.036)	(0.062)	
Male	0.110	0.000	0.000	− 0.007	− 0.013	1,562
		(0.020)	(0.047)	(0.025)	(0.047)	
CURRENTLY IDLE (NEITHER ENROLLED IN SCHOOL NOR WORKING) [PR]						
All	0.372	− 0.012	− 0.028	0.026	0.047	2,958
		(0.022)	(0.049)	(0.029)	(0.054)	
Female	0.299	0.036	0.079	0.063	0.109	1,483
		(0.029)	(0.063)	(0.039)	(0.067)	
Male	0.451	− 0.062~	− 0.147~	− 0.015	− 0.028	1,475
		(0.032)	(0.075)	(0.040)	(0.076)	
UNEMPLOYMENT INSURANCE RECORDS (FROM THE FOURTH QUARTER OF 2007 THROUGH THE THIRD QUARTER OF 2008 FROM MD, MA, IL, AND CA)						
EMPLOYED [UI]						
All	0.484	− 0.027	− 0.063	− 0.010	− 0.016	3,052
		(0.020)	(0.047)	(0.024)	(0.037)	
Female	0.551	− 0.004	− 0.008	− 0.002	− 0.002	1,507
		(0.017)	(0.038)	(0.027)	(0.042)	
Male	0.418	− 0.040*	− 0.095*	− 0.018	− 0.029	1,545
		(0.016)	(0.039)	(0.023)	(0.037)	
ANNUAL WAGES (2009 DOLLARS) [UI]						
All	$9,732.33	− 967.89	− 2,256.02	− 381.43	− 595.62	3,052
		(634.09)	(1,477.98)	(718.02)	(1,121.24)	

EXHIBIT 5.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
UNEMPLOYMENT INSURANCE RECORDS (FROM THE FOURTH QUARTER OF 2007 THROUGH THE THIRD QUARTER OF 2008 FROM MD, MA, IL, AND CA) (CONTINUED)						
ANNUAL WAGES (2009 DOLLARS) [UI] (CONTINUED)						
Female	$10,661.72	335.84 (682.66)	760.18 (1,545.23)	285.00 (922.52)	441.40 (1,428.76)	1,507
Male	$8,781.12	– 1,839.40* (533.73)	– 4,434.65* (1,286.78)	– 872.72 (735.46)	– 1,375.28 (1,158.97)	1,545

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report, UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For parent reports, data source is the adult long-term survey. Grown children (who were under age 18 at baseline and over age 20 as of December 31, 2007) of interviewed adults. Unemployment Insurance data use individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All core grown children whose parent has a baseline consent.

EXHIBIT 5.9. YOUTH EMPLOYMENT AND EARNINGS, ADMINISTRATIVE DATA BY SITE FOR THE FOURTH QUARTER OF 2007 THROUGH THE THIRD QUARTER 0F 2008						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EMPLOYED [UI]						
All	0.256	– 0.036* (0.015)	– 0.076* (0.032)	0.004 (0.018)	0.006 (0.025)	3,490
Female	0.272	– 0.030 (0.024)	– 0.063 (0.051)	0.012 (0.026)	0.017 (0.037)	1,691
Male	0.241	– 0.045* (0.021)	– 0.093* (0.044)	– 0.002 (0.023)	– 0.003 (0.032)	1,799
ANNUAL WAGES (2009 DOLLARS) [UI]						
All	$2,371.26	– 487.12* (225.21)	– 1,025.87* (474.29)	30.26 (247.74)	42.34 (346.68)	3,490
Female	$2,174.63	– 291.02 (297.80)	– 616.18 (630.55)	370.47 (326.36)	528.51 (465.58)	1,691
Male	$2,552.76	– 681.06* (323.22)	– 1,426.76* (677.12)	– 293.28 (358.88)	– 403.26 (493.46)	1,799

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All core youth ages 15 to 20 as of December 31, 2007, whose parent has a baseline consent. Massachusetts includes all youth ages 12 to 20. Since our request file did not split by age, we cannot look separately at those who were 15 to 20 in Massachusetts.

EXHIBIT 5.10. YOUTH SELF-REPORTED EMPLOYMENT, EARNINGS, AND SCHOOL ENROLLMENT						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
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)	
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 IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [SR]						
All	0.215	0.007	0.014	0.026	0.039	3,604
		(0.018)	(0.038)	(0.020)	(0.031)	
Female	0.194	0.024	0.049	0.031	0.048	1,838
		(0.024)	(0.048)	(0.027)	(0.043)	
Male	0.235	– 0.011	– 0.023	0.022	0.032	1,766
		(0.027)	(0.058)	(0.031)	(0.045)	
CURRENT WEEKLY EARNINGS (2009 DOLLARS) [SR]						
All	$88.98	– 11.81~	– 24.59~	– 8.55	– 12.74	3,463
		(6.51)	(13.55)	(7.17)	(10.68)	
Female	$88.76	– 14.31~	– 28.89~	– 12.16	– 18.74	1,767
		(8.44)	(17.05)	(9.59)	(14.78)	
Male	$89.19	– 9.35	– 20.16	– 5.00	– 7.21	1,696
		(9.72)	(20.95)	(10.84)	(15.63)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 15 to 20 as of December 31, 2007.
Measures: "Currently enrolled in school" also includes youth who are on summer vacation.

5.6 DATA SOURCES FOR AND MEASURES OF INCOME, POVERTY, FOOD CONSUMPTION, AND SOCIAL ASSISTANCE

Information about public assistance receipt and income is available from respondent self-reports on the long-term survey and administrative records from state TANF agencies. In the long-term survey, we asked adults about current receipt of TANF, food stamps, Supplemental Security Income, and Medicaid; the Earned Income Tax Credit (EITC),[12] and to recall their total household income for the most recent calendar year preceding the survey—2007, 2008, or 2009 depending on their interview date. The long-term survey used an innovative technique for collecting retrospective reports of total household income by embedding an income calculator.

Respondents are first asked about specific sources of income in the most recent calendar year for earnings, government sources, and then income from other sources. If a respondent does not know the total value of these income sources, the interviewer asks a series of yes-no questions about different income amounts to determine the income category.[13] These income sources are then added up to calculate for the respondent a total household income level. The respondent then has the opportunity to agree or adjust the total household income. Moreover, survey data is available on food sufficiency,[14] constructed measures of self-sufficiency (the proportion of adults who were working and not receiving TANF at the time of the survey), and food expenditure. Individual-level administrative data on monthly TANF and food stamp receipt are used to

construct the proportion of months receiving benefits and the amount received from both AFDC/TANF and food stamps.

Administrative records were collected and analyzed from seven state agencies and one county agency:[15] Massachusetts, Illinois, and Los Angeles County, plus five additional states (Indiana, Georgia, South Carolina, North Carolina, and Florida) where participants have moved since enrolling in the demonstration. Participants were matched to administrative records using different personal information, depending on the state. Massachusetts residents were matched strictly on social security number, Los Angeles County residents on social security number and date of birth, and Illinois residents on social security number, date of birth, and name. Once matched, government agencies returned both benefit amounts and a status variable indicating whether a case was active for TANF and food stamps.

We present results for MTO participants from the Boston, Chicago and Los Angeles site for the 24-month interval from July 2007 through June 2009. We construct variables for two units of analysis: the entire household and grown children. The household unit variables are designed to capture all TANF and food stamps received by all core members in each family by summing all distinct monthly benefit amounts received by family members, whether that person was active or not. This creates an upper bound on welfare receipt. This measure includes members who have started their own households and captures benefit amounts transferred to children born after randomization under any core member's care.

The subgroup of grown children captures those who enter adulthood and receive TANF and food stamps as household heads. Food stamp and TANF receipt are examined at the individual level. These results present two measures for grown children: the share of months *actively* on food stamps and TANF, and the total amount of food stamps and TANF benefits received *while active on their public assistance case.* We limit this analysis to only those grown children who are actively on TANF to prevent attributing benefits that are received in their

---

12 EITC refunds were ascertained using the following wording: Workers sometimes receive a tax refund check- early in the year- from the Earned Income Tax Credit or because they overpaid taxes in the previous year. Did you receive a tax refund check from the federal government early in [year of survey]? How much was your tax refund?

13 The "unfolding scale" (see Duncan and Petersen, 2001) for income consisted of the following questions: Would it amount to $10,000 or more? Would it amount to $20,000 or more? Would it amount to $25,000 or more? Would it amount to $15,000 or more? Would it amount to $5,000 or more?

14 Food sufficiency is measured using a modified version of a single item used by the U.S. Department of Agriculture (see Bickel et al., 2000).We asked respondents: Which of these statements best describes the foot eaten in your household in the last 12 months: I/we always had enough to eat, sometimes I/we did not have enough to eat, or often I/we did not have enough to eat.

15 The analyses currently exclude New York and Maryland.

parent's household to their newly formed household. This also implies that information might be missing about child-only cases for young adults with children.

5.7 LONG-TERM IMPACTS ON INCOME, POVERTY, FOOD CONSUMPTION, AND SOCIAL ASSISTANCE

Exhibits 5.11, 5.12, and 5.13 present estimates of MTO effects on income, TANF, and food stamp receipt. Exhibit 5.11 shows household self-reported responses on income, food security, and poverty status. Exhibit 5.12 shows welfare receipt information for grown children culled from administrative data from July 2007 to June 2009. Exhibit 5.13 shows self-reported data on receipt of public assistance at the time of interview and administrative data indicating the share of months that households received public assistance during the same two-year time period.

We find no systematic, detectable MTO treatment effects on total reported income or social assistance receipt. This is further shown in an analysis that more closely aligns the timing of social assistance receipt as reported in the survey and documented in the social assistance administrative data in Supplemental Exhibits 5.10 and 5.11. Experimental households are less likely to be living in poverty—defined as having an income at or below the poverty line[16]—than those in the control group in the calendar year before the long-term survey, but this finding is not statistically significant. The opposite pattern of a modest (also statistically insignificant) rise in poverty is found for the Section 8 group, relative to the control group (Exhibit 5.11). However, we worry here about the later timing of the survey for the Section 8 group. Supplemental Exhibit 5.12 shows similar rates of earned income tax credit receipt in the previous calendar year among the experimental, Section 8, and control group adults, although Section 8 adults claimed higher total refund amounts (above $2,500) compared with the control group.

Adults in the experimental and Section 8 groups report decreased food insufficiency (roughly 3.5 percentage points lower for experimental adults and 7 percentage points lower for Section 8 adults) compared with the control group rate of 34 percent. Experimental and Section 8 adults also report lower food expenditures— roughly $7 per week less than control group adults. These effects are difficult to explain or have confidence in without further investigation. The results may be chance findings or may be in part the result of a reduction in grocery expenditure as measured in the long-term survey or lower food prices in the new neighborhoods among experimental and Section 8 families.

The administrative data show that experimental group adults have significantly higher rates of food stamp receipt but similar TANF receipt, compared with control group adults (see Exhibit 5.13). Rates of TANF receipt generally follow national patterns of declines over time, with nearly 75 percent of the sample reporting receipt of welfare at baseline (measured during the earlier 1990s), 28.6 percent on average as reported at the interim survey interview (roughly during the early 2000s), and 16 percent reporting TANF receipt at the time of the long-term survey interview (in the later 2000s). The long-term survey data reveals a qualitatively similar but statistically insignificant pattern of slightly higher TANF and food stamp receipt for the experimental group. There are no significant differences between Section 8 and control group adults in terms of social assistance receipt in either the administrative or survey data. In addition, the administrative data indicate that grown children in the experimental group receive more total TANF benefits than the controls (this is marginally statistically significant). There is a similar (but statistically insignificant) pattern for the Section 8 grown children (Exhibit 5.12).

---

16 Poverty status was determined by comparing household income in the previous year to the U.S. Census Bureau's poverty thresholds for the adult respondent's current household size, number of children under 18, and age of the head of household (Denavas-Walt, Proctor, and Smith, 2010).

EXHIBIT 5.11. ADULT SELF-REPORTED HOUSEHOLD INCOME, POVERTY STATUS, AND FOOD SUFFICIENCY						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
HOUSEHOLD INCOME, LAST CALENDAR YEAR						
Total earnings of sample adult and spouse/partner (2009 dollars) [SR]	$14,518.70	– 483.91 (744.03)	– 998.61 (1,535.41)	– 1,752.38~ (1,015.66)	– 2,826.29~ (1,638.08)	3,266
Earnings from other household members (2009 dollars) [SR]	$2,672.24	– 17.56 (302.68)	– 36.16 (623.35)	427.21 (465.34)	675.34 (735.61)	3,079
Government income (2009 dollars) [SR]	$3,542.62	244.44 (217.77)	505.49 (450.33)	323.53 (303.01)	511.54 (479.08)	3,139
Other sources of income (2009 dollars) [SR]	$464.94	57.05 (72.58)	117.67 (149.71)	212.05* (103.04)	339.02* (164.74)	3,176
Total household income (2009 dollars) [SR]	$20,025.90	607.58 (727.58)	1,255.56 (1,503.54)	– 41.67 (1,009.18)	– 67.33 (1,630.66)	3,258
POVERTY STATUS, LAST CALENDAR YEAR						
Household income is at or below 50% of poverty line [SR]	0.298	– 0.012 (0.019)	– 0.024 (0.040)	0.036 (0.026)	0.058 (0.043)	3,258
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
Household income is at or below 150% of poverty line [SR]	0.756	– 0.007 (0.018)	– 0.014 (0.038)	0.008 (0.024)	0.013 (0.039)	3,258
FOOD SUFFICIENCY						
Adult reported that their household sometimes/often did not have enough to eat in the past 12 months [SR]	0.336	– 0.035~ (0.020)	– 0.072~ (0.042)	– 0.067* (0.026)	– 0.106* (0.042)	3,266
Current total weekly food expenditure (2009 dollars) [SR]	$113.05	– 7.01* (2.88)	– 14.39* (5.92)	– 7.27~ (3.82)	– 11.59~ (6.09)	3,165

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.
Measures: Poverty levels are determined by household size and age of household head. Government income includes welfare, SSI, unemployment benefits, and social security. "Other sources of income" includes money from any businesses, help from friends or relatives, child support, and any other money income. "Food Insufficiency" reflects the adult's response that in the past 12 months, there are some times or always not enough to eat.

EXHIBIT 5.12. WELFARE AND FOOD STAMP RECEIPT OF GROWN CHILDREN, ADMINISTRATIVE DATA						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
TEMPORARY ASSISTANCE FOR NEEDY FAMILIES (TANF) BENEFITS, JULY 2007–JUNE 2009 [TANF]						
FRACTION OF MONTHS ACTIVELY RECEIVING TANF						
All	0.046	0.012 (0.008)	0.029 (0.019)	0.010 (0.009)	0.017 (0.015)	2,833
Female	0.083	0.021 (0.016)	0.046 (0.034)	0.016 (0.017)	0.026 (0.027)	1,398
Male	0.010	0.004 (0.006)	0.009 (0.014)	0.004 (0.007)	0.007 (0.012)	1,435
TOTAL TANF BENEFITS WHILE ACTIVE ON CASE (2009 DOLLARS)						
All	$270.13	98.32~ (55.54)	230.41~ (130.15)	88.60 (59.30)	145.10 (97.12)	2,833
Female	$499.05	139.69 (103.06)	308.15 (227.36)	127.25 (109.64)	204.59 (176.28)	1,398
Male	$43.11	57.62 (38.61)	143.69 (96.28)	50.16 (41.26)	83.62 (68.78)	1,435
FOOD STAMPS BENEFITS, JULY 2007–JUNE 2009 [FS]						
FRACTION OF MONTHS ACTIVELY RECEIVING FOOD STAMPS						
All	0.228	0.004 (0.016)	0.010 (0.038)	0.009 (0.018)	0.014 (0.029)	2,833
Female	0.322	0.013 (0.026)	0.029 (0.057)	0.006 (0.028)	0.010 (0.045)	1,398
Male	0.134	− 0.004 (0.019)	− 0.010 (0.047)	0.011 (0.021)	0.018 (0.034)	1,435
TOTAL FOOD STAMPS BENEFITS WHILE ACTIVE ON CASE (2009 DOLLARS)						
All	$879.05	13.20 (76.64)	30.93 (179.59)	0.21 (82.65)	0.35 (135.36)	2,833
Female	$1,388.81	− 20.51 (134.74)	− 45.24 (297.23)	− 82.65 (140.46)	− 132.89 (225.83)	1,398
Male	$373.56	46.19 (74.85)	115.18 (186.66)	79.65 (78.66)	132.78 (131.13)	1,435

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: TANF = Temporary Assistance for Needy Families, FS = Food Stamps.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: FS and TANF analyses use individual data from Massachusetts, Illinois, Los Angeles County, and South Carolina and represent individuals whose random assignment sites are: Boston, Chicago and Los Angeles. We received TANF and food stamps data from Maryland, but the data quality is suspect. Sample for UI, TANF, and FS are all sample adults with baseline consent. All core grown children.
Measures: "Active" means the family is receiving welfare benefits directly through the grown child. The measures presented in this table include only payments made to the grown child while they were active on the case. We restrict this measure to "active" status to avoid picking up payments made to a youth's parent even though the youth is no longer part of the case (due to their age). This measure would not capture "child-only" payments made to children of the grown child.

EXHIBIT 5.13. ADULT GOVERNMENT ASSISTANCE, SELF REPORTS AND ADMINISTRATIVE DATA						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
SURVEY DATA [SR]						
Currently receiving Temporary Assistance for Needy Families (TANF)	0.158	0.010	0.021	0.026	0.041	3,262
		(0.015)	(0.031)	(0.021)	(0.033)	
Currently receiving food stamps	0.470	0.016	0.032	0.029	0.046	3,253
		(0.021)	(0.043)	(0.028)	(0.045)	
Currently receiving Supplemental Security Income	0.293	0.004	0.008	0.019	0.030	3,252
		(0.019)	(0.039)	(0.026)	(0.041)	
Currently receiving Medicaid	0.367	– 0.003	– 0.005	0.037	0.060	3,273
		(0.019)	(0.040)	(0.026)	(0.043)	
ADMINISTRATIVE DATA						
FAMILY TANF BENEFITS [TANF]						
Fraction of months received TANF, July 2007–June 2009	0.238	0.003	0.005	– 0.027	– 0.042	2,708
		(0.017)	(0.037)	(0.018)	(0.028)	
Total TANF benefits received, July 2007–June 2009 (2009 dollars)	$1,402.33	56.10	120.29	– 94.47	– 144.57	2,708
		(114.48)	(245.44)	(123.82)	(189.49)	
FAMILY FOOD STAMPS BENEFITS [FS]						
Fraction of months received food stamps (July 2007–June 2009)	0.594	0.046*	0.098*	– 0.007	– 0.011	2,708
		(0.019)	(0.040)	(0.020)	(0.031)	
Total food stamps benefits received, July 2007–June 2009 (2009 dollars)	$3,074.08	309.94*	664.54*	171.07	261.80	2,708
		(156.50)	(335.54)	(184.98)	(283.09)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: TANF = Temporary Assistance for Needy Families, FS = Food Stamps records, SR = self report.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: FS and TANF analyses use individual data from Massachusetts, Illinois, Los Angeles County, and South Carolina and represent individuals whose random assignment sites are: Boston, Chicago and Los Angeles. We received TANF and food stamps data from Maryland, but the data quality is suspect. Sample for UI, TANF, and FS are all sample adults with baseline consent. All sample adults with a baseline consent.

Measures: The measures represented in this table are at the family level. The outcome reflects the fraction of months during the 12 month period that any family members' case received TANF or Food Stamps benefits. The benefit amount represents 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.

5.8 DATA SOURCES FOR MEASURES OF SAVINGS AND ASSETS

The inclusion of savings and assets measures in the long-term survey is a new addition, compared with measures available from the interim survey. The long-term survey items are drawn from common sources of savings and asset questions, such as the Survey of Consumer Finances, the Health and Retirement Survey, and the Panel Study of Income Dynamics. Information is collected on savings or checking accounts, home or vehicle ownership, credit cards, and debt on any of these assets and also unpaid medical bills. Detailed questions about matters related to portfolio composition or use of more sophisticated savings vehicles are not included, given the low incomes of the demonstration participants and the first-order hypothesized effect of reducing the share of adults who are unbanked.

5.9 LONG-TERM IMPACTS ON SAVINGS AND ASSETS

Exhibit 5.14 shows that the demonstration had few detectable effects on measures of savings, assets, and debt—including owning a car or having a credit card or bank account—for both the experimental and Section 8 groups, relative to the control group. The experimental group adults are more likely to report having any household debt, and, at marginal statistical significance, the Section 8 adults are somewhat more likely to report having any household debt and to report owing more than $5,000 in credit card debt, compared with control group adults. As shown in Chapter 2, experimental and Section 8 participants are less likely to be homeowners than control group adults.

5.10 INTERPRETATION OF ECONOMIC SELF-SUFFICIENCY RESULTS

MTO has few systematic, detectable long-term effects on economic self-sufficiency outcomes for adults, youth, and grown children, as measured through survey and administrative records data up to the long-term follow-up survey period. Experimental and Section 8 group adults have similar employment, earnings, and income levels as control group adults. Experimental group adults are more likely to receive TANF benefits. Both experimental

and Section 8 adults are less likely to experience food insufficiency than control group adults. Ownership of assets, such as cars and homes, are the same or lower among experimental and Section 8 families, compared with those in the control group.

Yet debt levels are slightly higher for the experimental and Section 8 groups. Youth idleness rates are similar among random assignment groups at the long-term survey. Parental reports are suggestive of beneficial impacts on employment and reduction in idleness for male grown children, but the limited administrative UI data do not provide support for this finding.

On average, experimental and Section 8 households moved to neighborhoods with a greater share of "middle-class" (higher income, more educated, and steadily employed) neighbors and greater safety than control households. The advantages of these neighborhood attributes felt by the treatment groups eroded a bit from the time of initial moves to interim to the long-term survey. Still, these changes in environment are associated with persistent improvements in some mental health and physical health outcomes for adults in the experimental and Section 8 groups, relative to the control group adults (Chapters 3 and 4).But such neighborhood changes initially are associated with (if anything) an adverse initial disruption effect on labor market outcomes and do not appear to generate any sustained improvement in labor market outcomes or other measures of adult economic self-sufficiency.

The lack of impacts of a housing mobility intervention on economic outcomes for adult residents of housing developments contrasts with recent evaluations showing sustained positive impacts on earnings of disadvantaged adults from the direct provision of high-quality training and employment services. Examples of such successful employment programs include the Jobs-Plus demonstration, which provides training, outreach, and improved work incentives for public housing residents (Riccio, 2010), and sectoral employment programs, which provide training with strong links to local employer demands (Maguire et al., 2010). Training, education, and employment services that directly enhance marketable skills and changes in work incentives appear

to more directly impact labor market and economic outcomes of low-income adults than do the indirect effects of changes in neighborhood environments, at least in the range observed in the MTO demonstration.

Furthermore, the demonstrations have not generated systematic detectable gains in economic outcomes for youth and grown children in the experimental and Section 8 groups, compared with those in the control group. These results contrast with the long-term positive impacts on youth economic outcomes of some early childhood education programs (Almond and Currie, 2010) and improvements in school quality in early grades (Chetty et al., 2010). Housing mobility interventions that affect neighborhood environments by a similar magnitude as MTO may not be powerful enough to greatly affect youth long-term economic outcomes without being combined with further direct services for the children and training and education programs to improve parental earnings opportunities.

EXHIBIT 5.14. ADULT SELF-REPORTED FAMILY SAVINGS AND ASSETS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
Household currently owns a car [SR]	0.436	− 0.023	− 0.047	− 0.033	− 0.053	3,239
		(0.020)	(0.041)	(0.026)	(0.041)	
Adult currently has a bank account [SR]	0.559	0.014	0.029	− 0.039	− 0.062	3,191
		(0.021)	(0.043)	(0.028)	(0.044)	
Any household debt [SR]	0.410	0.055*	0.113*	0.056~	0.089~	3,209
		(0.021)	(0.044)	(0.029)	(0.045)	
Household owes ≥ $5,000 on medical bills (2009 dollars) [SR]	0.058	0.001	0.003	0.014	0.023	3,218
		(0.010)	(0.020)	(0.016)	(0.025)	
Adult has a credit card [SR]	0.259	0.025	0.051	0.002	0.004	3,238
		(0.019)	(0.038)	(0.024)	(0.038)	
Household owes ≥ $5,000 in credit card debt (2009 dollars) [SR]	0.065	0.016	0.033	0.028~	0.044~	3,211
		(0.011)	(0.022)	(0.015)	(0.023)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.

SUPPLEMENTAL EXHIBIT 5.1. ADULT EMPLOYMENT, ADMINISTRATIVE DATA, BY SITE AND RANDOM ASSIGNMENT (RA) YEARS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SHARE EMPLOYED [UI]						
PRE-RA YEAR 1						
Maryland	0.286	– 0.037	– 0.070	– 0.036	– 0.046	629
		(0.031)	(0.057)	(0.033)	(0.042)	
Illinois	0.270	0.009	0.026	0.018	0.027	883
		(0.026)	(0.076)	(0.030)	(0.045)	
California	0.274	0.017	0.027	0.005	0.007	853
		(0.025)	(0.041)	(0.027)	(0.035)	
Three-site weighted average	0.276	– 0.001	– 0.003	0.005	0.006	2,365
		(0.015)	(0.031)	(0.017)	(0.024)	
RA YEARS 1 AND 2						
Maryland	0.414	– 0.013	– 0.024	– 0.050	– 0.064	629
		(0.028)	(0.053)	(0.032)	(0.041)	
Illinois	0.408	– 0.058*	– 0.173*	– 0.012	– 0.017	883
		(0.025)	(0.073)	(0.029)	(0.043)	
California	0.401	– 0.021	– 0.034	– 0.042	– 0.054	853
		(0.025)	(0.041)	(0.028)	(0.036)	
Massachusetts	0.410	0.001	0.003	0.024	0.047	885
		(0.021)	(0.047)	(0.022)	(0.042)	
Four-site weighted average	0.408	– 0.023~	– 0.049~	– 0.017	– 0.025	3,250
		(0.012)	(0.026)	(0.014)	(0.020)	
RA YEARS 3 AND 4						
Maryland	0.549	– 0.011	– 0.021	– 0.025	– 0.032	629
		(0.035)	(0.065)	(0.038)	(0.049)	
Illinois	0.476	0.021	0.062	0.051	0.076	883
		(0.030)	(0.090)	(0.036)	(0.054)	
California	0.455	0.035	0.056	0.000	0.000	853
		(0.031)	(0.050)	(0.033)	(0.042)	
Massachusetts	0.530	– 0.058~	– 0.131~	– 0.013	– 0.025	885
		(0.030)	(0.067)	(0.029)	(0.055)	
Four-site weighted average	0.499	– 0.003	– 0.006	0.005	0.008	3,250
		(0.016)	(0.033)	(0.017)	(0.025)	
RA YEARS 5 AND 6						
Maryland	0.615	0.012	0.022	– 0.055	– 0.070	629
		(0.035)	(0.065)	(0.039)	(0.050)	
Illinois	0.501	0.025	0.075	0.040	0.060	883
		(0.033)	(0.099)	(0.039)	(0.058)	
California	0.454	0.052	0.083	0.039	0.050	853
		(0.033)	(0.053)	(0.034)	(0.044)	

SUPPLEMENTAL EXHIBIT 5.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
SHARE EMPLOYED [UI] (CONTINUED)						
RA YEARS 5 AND 6 (CONTINUED)						
Massachusetts	0.534	– 0.066*	– 0.149*	0.017	0.034	885
		(0.030)	(0.067)	(0.032)	(0.062)	
Four-site weighted average	0.519	0.005	0.010	0.015	0.022	3,250
		(0.016)	(0.034)	(0.018)	(0.027)	
RA YEARS 7 AND 8						
Maryland	0.610	– 0.039	– 0.072	– 0.037	– 0.047	629
		(0.038)	(0.070)	(0.042)	(0.054)	
Illinois	0.462	0.050	0.149	0.041	0.061	883
		(0.034)	(0.101)	(0.041)	(0.060)	
California	0.436	0.027	0.044	0.062~	0.080~	853
		(0.035)	(0.056)	(0.035)	(0.046)	
Massachusetts	0.468	– 0.041	– 0.092	0.032	0.062	885
		(0.032)	(0.071)	(0.039)	(0.075)	
Four-site weighted average	0.485	0.002	0.005	0.029	0.043	3,250
		(0.017)	(0.036)	(0.020)	(0.029)	
RA YEARS 9 AND 10						
Maryland	0.579	– 0.058	– 0.108	– 0.040	– 0.051	629
		(0.041)	(0.076)	(0.044)	(0.057)	
Illinois	0.445	0.047	0.140	0.059	0.088	883
		(0.034)	(0.100)	(0.040)	(0.060)	
California	0.425	0.042	0.067	0.060~	0.077~	853
		(0.035)	(0.056)	(0.036)	(0.046)	
Massachusetts	0.443	– 0.021	– 0.048	0.006	0.011	885
		(0.032)	(0.073)	(0.033)	(0.063)	
Four-site weighted average	0.465	0.007	0.014	0.026	0.038	3,250
		(0.017)	(0.036)	(0.019)	(0.028)	
RA YEARS 1–10						
Maryland	0.553	– 0.022	– 0.041	– 0.041	– 0.053	629
		(0.028)	(0.053)	(0.031)	(0.040)	
Illinois	0.458	0.017	0.051	0.036	0.053	883
		(0.025)	(0.075)	(0.029)	(0.044)	
California	0.434	0.027	0.043	0.024	0.031	853
		(0.025)	(0.040)	(0.026)	(0.034)	
Massachusetts	0.477	– 0.037~	– 0.083~	0.013	0.026	885
		(0.022)	(0.049)	(0.021)	(0.041)	
Four-site weighted average	0.475	– 0.003	– 0.005	0.012	0.017	3,250
		(0.012)	(0.026)	(0.013)	(0.020)	

SUPPLEMENTAL EXHIBIT 5.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANNUAL WAGES (2009 DOLLARS) [UI]						
PRE-RA YEAR 1						
Maryland	$2,885.21	– 180.81	– 337.54	– 335.68	– 429.54	629
		(402.44)	(751.27)	(423.82)	(542.32)	
Illinois	$3,095.51	371.97	1,105.29	404.78	601.29	883
		(400.14)	(1,188.98)	(480.48)	(713.75)	
California	$3,432.36	293.02	471.62	– 211.87	– 273.86	853
		(425.58)	(684.98)	(453.71)	(586.47)	
Three-site weighted average	$3,163.83	94.20	191.41	56.61	76.72	2,365
		(240.38)	(488.46)	(274.91)	(372.61)	
RA YEARS 1 AND 2						
Maryland	$4,786.21	96.59	180.31	– 346.50	– 443.38	629
		(402.81)	(751.96)	(449.83)	(575.60)	
Illinois	$5,232.02	– 603.87	– 1,794.35	– 268.39	– 398.68	883
		(417.76)	(1,241.33)	(490.36)	(728.42)	
California	$5,670.21	– 924.22*	– 1,487.57*	– 1,032.71~	– 1,334.88~	853
		(435.28)	(700.61)	(527.91)	(682.38)	
Massachusetts	$7,161.93	44.67	100.10	723.97	1,388.36	885
		(480.79)	(1,077.37)	(528.41)	(1,013.34)	
Four-site weighted average	$5,781.17	– 377.39~	– 786.86~	– 214.97	– 317.03	3,250
		(221.62)	(462.10)	(255.44)	(376.71)	
RA YEARS 3 AND 4						
Maryland	$8,125.47	– 166.07	– 310.02	– 353.63	– 452.51	629
		(765.92)	(1,429.82)	(792.12)	(1,013.60)	
Illinois	$7,504.97	– 124.31	– 369.38	– 481.40	– 715.11	883
		(660.57)	(1,962.83)	(762.10)	(1,132.08)	
California	$7,477.82	– 88.18	– 141.93	– 997.51	– 1,289.38	853
		(639.05)	(1,028.57)	(718.58)	(928.83)	
Massachusetts	$10,418.11	– 1,230.45~	– 2,757.22~	– 295.14	– 565.99	885
		(725.29)	(1,625.24)	(675.26)	(1,294.95)	
Four-site weighted average	$8,398.43	– 422.45	– 880.83	– 541.97	– 799.28	3,250
		(348.14)	(725.88)	(368.47)	(543.40)	
RA YEARS 5 AND 6						
Maryland	$10,509.31	332.50	620.72	– 284.52	– 364.07	629
		(874.49)	(1,632.50)	(1,019.74)	(1,304.87)	
Illinois	$9,011.70	492.27	1,462.74	27.38	40.67	883
		(804.95)	(2,391.85)	(902.33)	(1,340.39)	
California	$8,824.30	– 85.27	– 137.24	– 652.90	– 843.94	853
		(834.91)	(1,343.82)	(855.17)	(1,105.39)	
Massachusetts	$12,222.01	– 1,658.96~	– 3,717.42~	715.98	1,373.03	885
		(946.40)	(2,120.72)	(911.08)	(1,747.18)	
Four-site weighted average	$10,111.07	– 274.30	– 571.93	– 24.84	– 36.63	3,250
		(436.74)	(910.63)	(459.28)	(677.33)	

SUPPLEMENTAL EXHIBIT 5.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANNUAL WAGES (2009 DOLLARS) [UI] (CONTINUED)						
RA YEARS 7 AND 8						
Maryland	$11,947.92	– 586.02	– 1,093.97	– 358.35	– 458.55	629
		(1,033.78)	(1,929.86)	(1,193.35)	(1,527.02)	
Illinois	$9,264.16	699.30	2,077.93	– 181.69	– 269.89	883
		(911.22)	(2,707.61)	(1,070.05)	(1,589.53)	
California	$9,489.59	– 282.26	– 454.30	– 670.89	– 867.20	853
		(997.39)	(1,605.34)	(980.19)	(1,266.99)	
Massachusetts	$11,812.85	– 1,329.14	– 2,978.36	600.70	1,151.96	885
		(1,155.54)	(2,589.36)	(1,280.68)	(2,455.95)	
Four-site weighted average	$10,526.24	– 358.94	– 748.41	– 131.96	– 194.60	3,250
		(518.30)	(1,080.68)	(570.49)	(841.34)	
RA YEARS 9 AND 10						
Maryland	$13,334.45	– 2,569.26*	– 4,796.29*	– 931.10	– 1,191.45	629
		(1,170.38)	(2,184.85)	(1,402.79)	(1,795.02)	
Illinois	$9,048.23	1,227.64	3,647.83	395.85	588.03	883
		(1,010.41)	(3,002.36)	(1,205.51)	(1,790.75)	
California	$9,415.62	203.63	327.76	93.96	121.45	853
		(1,036.58)	(1,668.42)	(1,059.42)	(1,369.40)	
Massachusetts	$11,543.13	– 252.62	– 566.08	508.26	974.69	885
		(1,178.90)	(2,641.70)	(1,290.40)	(2,474.59)	
Four-site weighted average	$10,643.49	– 179.70	– 374.68	90.45	133.39	3,250
		(550.89)	(1,148.63)	(617.70)	(910.96)	
RA YEARS 1–10						
Maryland	$9,740.67	– 578.45	– 1,079.85	– 454.82	– 581.99	629
		(704.70)	(1,315.52)	(806.73)	(1,032.30)	
Illinois	$8,012.22	338.21	1,004.95	– 101.65	– 151.00	883
		(633.52)	(1,882.45)	(720.37)	(1,070.10)	
California	$8,175.51	– 235.26	– 378.66	– 652.01	– 842.79	853
		(644.21)	(1,036.88)	(684.88)	(885.27)	
Massachusetts	$10,631.61	– 885.30	– 1,983.79	450.75	864.41	885
		(747.31)	(1,674.60)	(747.08)	(1,432.67)	
Four-site weighted average	$9,092.08	– 322.556	– 672.542	– 164.660	– 242.834	3,250
		(343.663)	(716.550)	(369.255)	(544.564)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All sample adults with baseline consent.

SUPPLEMENTAL EXHIBIT 5.2. COMPARISON OF SELF-REPORTED AND ADMINISTRATIVE DATA ON ADULT CURRENT EMPLOYMENT BY SITE						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
"CURRENTLY EMPLOYED," BY SITE AND SOURCE						
MASSACHUSETTS						
Self-report	0.502	– 0.046	– 0.107	– 0.086	– 0.153	678
		(0.045)	(0.105)	(0.059)	(0.106)	
Unemployment Insurance records	0.413	– 0.068~	– 0.152~	– 0.020	– 0.039	885
		(0.039)	(0.087)	(0.049)	(0.094)	
THREE SITE (MARYLAND, ILLINOIS, CALIFORNIA)						
Self-report	0.533	– 0.005	– 0.011	– 0.078*	– 0.109*	1,856
		(0.027)	(0.054)	(0.037)	(0.052)	
Unemployment Insurance records	0.467	– 0.013	– 0.026	0.008	0.010	2,365
		(0.024)	(0.049)	(0.027)	(0.037)	
MARYLAND						
Self-report	0.610	– 0.115*	– 0.211*	– 0.138*	– 0.180*	450
		(0.055)	(0.100)	(0.070)	(0.091)	
Unemployment Insurance records	0.582	– 0.121*	– 0.226*	– 0.082	– 0.105	629
		(0.046)	(0.087)	(0.052)	(0.066)	
ILLINOIS						
Self-report	0.545	0.011	0.033	– 0.074	– 0.109	670
		(0.046)	(0.138)	(0.068)	(0.101)	
Unemployment Insurance records	0.436	0.033	0.099	0.060	0.088	883
		(0.041)	(0.122)	(0.049)	(0.072)	
CALIFORNIA						
Self-report	0.478	0.061	0.097	– 0.028	– 0.039	736
		(0.043)	(0.068)	(0.054)	(0.075)	
Unemployment Insurance records	0.415	0.046	0.074	0.033	0.043	853
		(0.041)	(0.066)	(0.042)	(0.054)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For self-reports, data source is the adult long-term survey and the sample is all adults interviewed. Unemployment Insurance data analyses use individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All sample adults with baseline consent.
Measures: Time period for administrative data is June 2008–December 2009.

SUPPLEMENTAL EXHIBIT 5.3. COMPARISON OF SELF-REPORTED AND ADMINISTRATIVE DATA ON ADULT EMPLOYMENT IN MARYLAND, ILLINOIS, AND CALIFORNIA (IN-STATE ONLY)

OUTCOME	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
		ITT	TOT	ITT	TOT	*N*
EMPLOYMENT IN THE PREVIOUS CALENDAR YEAR, BY SITE AND SOURCE						
ALL THREE SITES						
Self-report	0.664	0.021	0.040	– 0.077*	– 0.106*	1,546
		(0.028)	(0.054)	(0.039)	(0.054)	
Unemployment Insurance records	0.607	0.004	0.009	– 0.047	– 0.064	1,546
		(0.029)	(0.057)	(0.041)	(0.056)	
MARYLAND						
Self-report	0.731	– 0.100~	– 0.187~	– 0.190*	– 0.243*	408
		(0.055)	(0.102)	(0.079)	(0.101)	
Unemployment Insurance records	0.711	– 0.119*	– 0.220*	– 0.116	– 0.149	408
		(0.055)	(0.103)	(0.074)	(0.094)	
ILLINOIS						
Self-report	0.667	0.060	0.172	– 0.075	– 0.109	576
		(0.045)	(0.130)	(0.065)	(0.094)	
Unemployment Insurance records	0.610	0.036	0.102	– 0.082	– 0.120	576
		(0.049)	(0.140)	(0.071)	(0.104)	
CALIFORNIA						
Self-report	0.613	0.118*	0.182*	0.067	0.091	562
		(0.047)	(0.072)	(0.057)	(0.078)	
Unemployment Insurance records	0.529	0.122*	0.188*	0.105~	0.144~	562
		(0.049)	(0.075)	(0.061)	(0.083)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Sample is restricted to adults with annual earnings available in both survey and administrative data for the year prior to the interview and who are currently living in their random assignment state. For self-reports, data source is the adult long-term survey and the sample is all adults interviewed. Unemployment Insurance data use individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All sample adults with baseline consent.
Measures: Time period for administrative data is from June 2008-December 2009.

SUPPLEMENTAL EXHIBIT 5.4. ADULT SELF-REPORTED CURRENT INDUSTRY AND OCCUPATION						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
INDUSTRY						
Retail [SR]	0.060	− 0.003	− 0.006	− 0.022~	− 0.036~	3,250
		(0.010)	(0.021)	(0.013)	(0.021)	
Medical/health care [SR]	0.122	0.010	0.020	− 0.005	− 0.008	3,250
		(0.014)	(0.029)	(0.018)	(0.029)	
Education [SR]	0.059	0.002	0.004	− 0.012	− 0.019	3,250
		(0.011)	(0.023)	(0.013)	(0.021)	
Social services [SR]	0.075	− 0.002	− 0.005	− 0.017	− 0.027	3,250
		(0.011)	(0.023)	(0.014)	(0.023)	
Industry other than retail, medical, education, or social services [SR]	0.207	− 0.013	− 0.026	− 0.025	− 0.040	3,250
		(0.017)	(0.036)	(0.024)	(0.038)	
OCCUPATION						
Blue collar [SR]	0.062	− 0.009	− 0.018	− 0.015	− 0.024	3,253
		(0.010)	(0.021)	(0.013)	(0.020)	
White collar [SR]	0.202	0.001	0.003	− 0.032	− 0.050	3,253
		(0.016)	(0.033)	(0.021)	(0.034)	
Professional or managerial [SR]	0.061	0.002	0.005	− 0.001	− 0.001	3,253
		(0.010)	(0.020)	(0.013)	(0.021)	
Service [SR]	0.260	− 0.001	− 0.003	− 0.034	− 0.055	3,253
		(0.019)	(0.040)	(0.025)	(0.040)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.
"Measures: Industry and occupation outcomes indicate that the adult respondent was currently working and was working in that industry or occupation. Occupation categories are not mutually exclusive. Industry coding is based on CPS codes: Retail trade 580–691; Medical trade 812–840, Education trade 842–860, and Social services 861–871. Occupation is coded as follows: Blue collar 473–889 (production, craft, repair, machine operators, assemblers, inspectors, transportation, handlers, cleaners, helpers, and laborers); White collar 3–389 (executive/managerial, professional specialty, technical/administrative support, or sales), Managerial/professional 3–199 (executive, administrative, and managerial or professional specialty), and Service occupation 403–469 (private household service, protective service, and all other services).

SUPPLEMENTAL EXHIBIT 5.5. ADULT SELF-REPORTED CURRENT EMPLOYMENT BENEFITS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EMPLOYMENT BENEFITS						
Health insurance [SR]	0.342	0.013	0.027	– 0.044~	– 0.070~	3,229
		(0.020)	(0.041)	(0.026)	(0.042)	
Paid sick leave [SR]	0.307	0.016	0.034	– 0.035	– 0.056	3,220
		(0.019)	(0.040)	(0.025)	(0.040)	
Paid vacation [SR]	0.350	0.004	0.007	– 0.039	– 0.062	3,227
		(0.020)	(0.041)	(0.027)	(0.043)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.

SUPPLEMENTAL EXHIBIT 5.6. FOUR MUTUALLY EXCLUSIVE CATEGORIES OF WORK AND SCHOOLING OF GROWN CHILDREN, PARENT REPORTS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SURVEY DATA						
CURRENTLY EMPLOYED ONLY [PR]						
All	0.456	0.021	0.047	– 0.020	– 0.037	2,958
		(0.023)	(0.051)	(0.031)	(0.056)	
Female	0.486	– 0.024	– 0.052	– 0.062	– 0.108	1,485
		(0.032)	(0.069)	(0.041)	(0.071)	
Male	0.424	0.067*	0.158*	0.024	0.046	1,473
		(0.032)	(0.076)	(0.041)	(0.078)	
CURRENTLY ENROLLED IN SCHOOL ONLY [PR]						
All	0.059	– 0.006	– 0.014	0.024	0.043	3,084
		(0.010)	(0.022)	(0.016)	(0.029)	
Female	0.080	– 0.008	– 0.018	0.024	0.042	1,522
		(0.016)	(0.036)	(0.025)	(0.044)	
Male	0.037	– 0.004	– 0.009	0.024	0.045	1,562
		(0.011)	(0.026)	(0.018)	(0.034)	
CURRENTLY EMPLOYED AND ENROLLED IN SCHOOL [PR]						
All	0.097	– 0.004	– 0.009	– 0.028	– 0.050	3,084
		(0.013)	(0.031)	(0.018)	(0.032)	
Female	0.122	– 0.008	– 0.018	– 0.020	– 0.034	1,524
		(0.020)	(0.046)	(0.028)	(0.048)	
Male	0.071	0.000	0.001	– 0.035~	– 0.066~	1,560
		(0.017)	(0.040)	(0.019)	(0.035)	

SUPPLEMENTAL EXHIBIT 5.6. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SURVEY DATA						
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [PR]						
All	0.372	– 0.012	– 0.028	0.026	0.047	2,958
		(0.022)	(0.049)	(0.029)	(0.054)	
Female	0.299	0.036	0.079	0.063	0.109	1,483
		(0.029)	(0.063)	(0.039)	(0.067)	
Male	0.451	– 0.062~	– 0.147~	– 0.015	– 0.028	1,475
		(0.032)	(0.075)	(0.040)	(0.076)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. Grown children (who were under age 18 at baseline and over age 20 as of December 31, 2007) of interviewed adults.

SUPPLEMENTAL EXHIBIT 5.7. EMPLOYMENT AND SCHOOLING OUTCOMES OF GROWN CHILDREN, PARENT REPORTS AND ADMINISTRATIVE DATA BY SITE						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY EMPLOYED [PR]						
ALL						
Maryland	0.594	0.010	0.021	– 0.063	– 0.089	430
		(0.059)	(0.130)	(0.076)	(0.108)	
Massachusetts	0.662	– 0.050	– 0.133	– 0.111	– 0.223	553
		(0.056)	(0.148)	(0.071)	(0.144)	
Illinois	0.511	– 0.072	– 0.255	– 0.055	– 0.095	631
		(0.048)	(0.170)	(0.059)	(0.102)	
California	0.554	0.026	0.042	0.007	0.009	662
		(0.050)	(0.078)	(0.061)	(0.085)	
New York	0.513	0.099~	0.233~	– 0.013	– 0.039	677
		(0.052)	(0.121)	(0.063)	(0.194)	
FEMALE						
Maryland	0.610	0.089	0.189	– 0.032	– 0.041	215
		(0.076)	(0.162)	(0.102)	(0.129)	
Massachusetts	0.739	– 0.152*	– 0.378*	– 0.159~	– 0.280~	297
		(0.072)	(0.180)	(0.085)	(0.150)	
Illinois	0.556	– 0.093	– 0.311	– 0.098	– 0.182	307
		(0.070)	(0.233)	(0.091)	(0.169)	
California	0.636	– 0.066	– 0.091	– 0.072	– 0.093	326
		(0.067)	(0.092)	(0.076)	(0.098)	
New York	0.537	0.031	0.077	– 0.045	– 0.154	335
		(0.069)	(0.171)	(0.087)	(0.298)	

SUPPLEMENTAL EXHIBIT 5.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY EMPLOYED [PR] (CONTINUED)						
MALE						
Maryland	0.580	− 0.075	− 0.172	− 0.089	− 0.143	215
		(0.089)	(0.204)	(0.117)	(0.186)	
Massachusetts	0.560	0.076	0.215	− 0.035	− 0.083	256
		(0.079)	(0.225)	(0.094)	(0.225)	
Illinois	0.469	− 0.051	− 0.196	− 0.017	− 0.028	324
		(0.076)	(0.292)	(0.079)	(0.127)	
California	0.465	0.120~	0.212~	0.094	0.147	336
		(0.069)	(0.122)	(0.084)	(0.130)	
New York	0.487	0.173*	0.384*	0.025	0.069	342
		(0.069)	(0.155)	(0.084)	(0.236)	
CURRENTLY ENROLLED IN SCHOOL [PR]						
ALL						
Maryland	0.106	− 0.010	− 0.023	− 0.032	− 0.044	457
		(0.037)	(0.086)	(0.051)	(0.071)	
Massachusetts	0.182	0.003	0.007	− 0.038	− 0.076	577
		(0.039)	(0.106)	(0.057)	(0.113)	
Illinois	0.118	0.011	0.037	0.058	0.100	659
		(0.032)	(0.114)	(0.044)	(0.076)	
California	0.196	− 0.012	− 0.020	− 0.058	− 0.082	688
		(0.038)	(0.060)	(0.051)	(0.073)	
New York	0.172	0.017	0.039	0.053	0.160	708
		(0.035)	(0.082)	(0.049)	(0.147)	
FEMALE						
Maryland	0.158	− 0.005	− 0.011	− 0.061	− 0.077	228
		(0.055)	(0.124)	(0.065)	(0.081)	
Massachusetts	0.227	− 0.069	− 0.177	− 0.071	− 0.125	305
		(0.052)	(0.135)	(0.077)	(0.135)	
Illinois	0.188	0.006	0.021	0.129~	0.242~	311
		(0.049)	(0.164)	(0.072)	(0.136)	
California	0.203	0.025	0.034	− 0.011	− 0.014	336
		(0.055)	(0.077)	(0.073)	(0.094)	
New York	0.232	0.012	0.031	0.006	0.021	347
		(0.059)	(0.146)	(0.069)	(0.232)	
MALE						
Maryland	0.059	− 0.017	− 0.040	− 0.003	− 0.005	229
		(0.053)	(0.122)	(0.069)	(0.106)	
Massachusetts	0.124	0.089	0.250	0.010	0.023	272
		(0.055)	(0.155)	(0.067)	(0.154)	
Illinois	0.057	0.014	0.053	0.003	0.005	348
		(0.039)	(0.147)	(0.046)	(0.074)	

SUPPLEMENTAL EXHIBIT 5.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
CURRENTLY ENROLLED IN SCHOOL [PR] (CONTINUED)						
FEMALE (CONTINUED)						
California	0.189	− 0.049	− 0.087	− 0.109~	− 0.173~	352
		(0.050)	(0.090)	(0.060)	(0.096)	
New York	0.109	0.023	0.051	0.097~	0.267~	361
		(0.044)	(0.096)	(0.056)	(0.155)	
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [PR]						
ALL						
Maryland	0.367	0.006	0.014	0.021	0.030	431
		(0.056)	(0.125)	(0.071)	(0.101)	
Massachusetts	0.284	0.071	0.189	0.101	0.202	556
		(0.052)	(0.139)	(0.069)	(0.137)	
Illinois	0.435	0.055	0.197	0.012	0.021	630
		(0.050)	(0.177)	(0.061)	(0.106)	
California	0.351	− 0.014	− 0.021	0.035	0.049	665
		(0.046)	(0.073)	(0.057)	(0.081)	
New York	0.416	− 0.113*	− 0.264*	− 0.063	− 0.191	676
		(0.050)	(0.116)	(0.061)	(0.185)	
FEMALE						
Maryland	0.306	− 0.054	− 0.115	0.047	0.059	215
		(0.068)	(0.144)	(0.103)	(0.130)	
Massachusetts	0.204	0.185*	0.467*	0.176*	0.310*	299
		(0.067)	(0.168)	(0.083)	(0.147)	
Illinois	0.360	0.075	0.253	− 0.002	− 0.003	305
		(0.072)	(0.240)	(0.092)	(0.172)	
California	0.268	0.044	0.061	0.091	0.117	329
		(0.065)	(0.088)	(0.071)	(0.092)	
New York	0.359	− 0.041	− 0.102	− 0.016	− 0.054	335
		(0.064)	(0.158)	(0.081)	(0.273)	
MALE						
Maryland	0.420	0.072	0.169	− 0.005	− 0.009	216
		(0.087)	(0.205)	(0.112)	(0.179)	
Massachusetts	0.389	− 0.072	− 0.204	− 0.006	− 0.013	257
		(0.076)	(0.218)	(0.091)	(0.210)	
Illinois	0.506	0.037	0.140	0.021	0.033	325
		(0.077)	(0.291)	(0.080)	(0.129)	
California	0.441	− 0.072	− 0.127	− 0.027	− 0.043	336
		(0.065)	(0.116)	(0.080)	(0.127)	
New York	0.480	− 0.190*	− 0.421*	− 0.115	− 0.322	341
		(0.070)	(0.154)	(0.083)	(0.233)	

SUPPLEMENTAL EXHIBIT 5.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
UNEMPLOYMENT INSURANCE DATA, FOURTH QUARTER OF 2007 THROUGH THIRD QUARTER OF 2008 [UI]						
EMPLOYED						
ALL						
Maryland	0.540	− 0.055	− 0.120	0.018	0.024	650
		(0.043)	(0.093)	(0.052)	(0.070)	
Illinois	0.405	− 0.021	− 0.074	− 0.052	− 0.088	826
		(0.038)	(0.135)	(0.048)	(0.081)	
California	0.486	− 0.009	− 0.015	− 0.010	− 0.013	829
		(0.039)	(0.066)	(0.045)	(0.062)	
Massachusetts	0.513	− 0.028	− 0.071	0.010	0.019	747
		(0.039)	(0.099)	(0.043)	(0.085)	
All four sites	0.484	− 0.027	− 0.063	− 0.010	− 0.016	3,052
		(0.020)	(0.047)	(0.024)	(0.037)	
FEMALE						
Maryland	0.606	− 0.071	− 0.162	− 0.004	− 0.005	316
		(0.059)	(0.134)	(0.072)	(0.093)	
Illinois	0.445	0.032	0.108	0.005	0.009	404
		(0.056)	(0.188)	(0.067)	(0.121)	
California	0.594	− 0.090	− 0.140	− 0.073	− 0.097	401
		(0.058)	(0.089)	(0.062)	(0.081)	
Massachusetts	0.564	0.090	0.183	0.060	0.079	386
		(0.084)	(0.172)	(0.159)	(0.211)	
All four sites	0.551	− 0.004	− 0.008	− 0.002	− 0.002	1,191
		(0.017)	(0.038)	(0.027)	(0.042)	
MALE						
Maryland	0.478	− 0.039	− 0.081	0.039	0.054	334
		(0.063)	(0.129)	(0.075)	(0.102)	
Illinois	0.368	− 0.072	− 0.277	− 0.109~	− 0.174~	422
		(0.055)	(0.210)	(0.063)	(0.101)	
California	0.388	0.066	0.123	0.057	0.081	428
		(0.051)	(0.094)	(0.060)	(0.086)	
Massachusetts	0.458	− 0.152	− 0.537	− 0.056	− 0.209	361
		(0.108)	(0.384)	(0.177)	(0.652)	
All four sites	0.418	− 0.040*	− 0.095*	− 0.018	− 0.029	1,545
		(0.016)	(0.039)	(0.023)	(0.037)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report, UI = Unemployment Insurance administrative records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For parent reports, data source is the adult long-term survey. Grown children (who were under age 18 at baseline and over age 20 as of December 31, 2007) of interviewed adults. Unemployment Insurance data use individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All core grown children whose parent has a baseline consent.
Measures: The administrative data only look at the last four common calendar quarters (fourth quarter of 2007 through the third quarter of 2008).

SUPPLEMENTAL EXHIBIT 5.8. YOUTH EMPLOYMENT AND SCHOOL ENROLLMENT, SELF-REPORTED AND ADMINISTRATIVE DATA BY SITE						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY EMPLOYED [SR]						
ALL						
Maryland	0.450	− 0.158*	-0.271*	− 0.116~	− 0.155~	486
		(0.062)	(0.107)	(0.060)	(0.080)	
Massachusetts	0.456	− 0.032	− 0.081	− 0.032	− 0.057	716
		(0.046)	(0.117)	(0.047)	(0.084)	
Illinois	0.339	0.002	0.006	− 0.006	− 0.009	757
		(0.046)	(0.159)	(0.050)	(0.068)	
California	0.321	− 0.035	− 0.054	− 0.069~	− 0.088~	874
		(0.039)	(0.059)	(0.041)	(0.052)	
New York	0.439	− 0.070	− 0.142	− 0.041	− 0.080	771
		(0.046)	(0.094)	(0.050)	(0.098)	
FEMALE						
Maryland	0.430	− 0.148~	− 0.247~	− 0.109	− 0.146	244
		(0.087)	(0.145)	(0.086)	(0.115)	
Massachusetts	0.453	− 0.004	− 0.008	0.004	0.008	376
		(0.063)	(0.136)	(0.065)	(0.127)	
Illinois	0.368	− 0.051	− 0.180	− 0.018	− 0.025	392
		(0.063)	(0.225)	(0.072)	(0.100)	
California	0.371	− 0.057	− 0.086	− 0.151*	− 0.187*	428
		(0.055)	(0.082)	(0.060)	(0.074)	
New York	0.439	− 0.029	− 0.058	− 0.023	− 0.049	398
		(0.067)	(0.135)	(0.069)	(0.148)	
MALE						
Maryland	0.468	− 0.168*	− 0.297*	− 0.122	− 0.163	242
		(0.085)	(0.151)	(0.085)	(0.113)	
Massachusetts	0.459	− 0.061	− 0.189	− 0.073	− 0.117	340
		(0.069)	(0.213)	(0.072)	(0.115)	
Illinois	0.313	0.056	0.187	0.003	0.004	365
		(0.067)	(0.224)	(0.075)	(0.103)	
California	0.274	− 0.011	− 0.017	0.010	0.013	446
		(0.050)	(0.079)	(0.060)	(0.079)	
New York	0.439	− 0.112	− 0.230	− 0.058	− 0.102	373
		(0.070)	(0.142)	(0.071)	(0.126)	
CURRENTLY ENROLLED IN SCHOOL [SR]						
ALL						
Maryland	0.423	0.143*	0.245*	0.040	0.053	486
		(0.057)	(0.098)	(0.062)	(0.082)	

SUPPLEMENTAL EXHIBIT 5.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY ENROLLED IN SCHOOL [SR] (CONTINUED)						
ALL (CONTINUED)						
Massachusetts	0.685	− 0.023	− 0.059	− 0.007	− 0.012	722
		(0.046)	(0.117)	(0.046)	(0.083)	
Illinois	0.666	− 0.031	− 0.108	0.043	0.060	760
		(0.041)	(0.143)	(0.048)	(0.065)	
California	0.624	0.051	0.078	− 0.046	− 0.059	877
		(0.037)	(0.057)	(0.042)	(0.054)	
New York	0.609	0.022	0.044	− 0.038	− 0.074	776
		(0.043)	(0.086)	(0.045)	(0.088)	
FEMALE						
Maryland	0.501	0.107	0.179	− 0.081	− 0.108	244
		(0.073)	(0.121)	(0.077)	(0.103)	
Massachusetts	0.730	− 0.056	− 0.123	− 0.030	− 0.059	380
		(0.061)	(0.134)	(0.064)	(0.125)	
Illinois	0.673	− 0.023	− 0.082	0.068	0.093	394
		(0.051)	(0.183)	(0.063)	(0.086)	
California	0.636	0.043	0.065	0.032	0.039	428
		(0.051)	(0.076)	(0.054)	(0.067)	
New York	0.598	− 0.005	− 0.011	0.032	0.069	399
		(0.062)	(0.124)	(0.066)	(0.141)	
MALE						
Maryland	0.355	0.172*	0.303*	0.156~	0.207~	242
		(0.081)	(0.144)	(0.083)	(0.111)	
Massachusetts	0.636	0.013	0.040	0.020	0.032	342
		(0.063)	(0.194)	(0.063)	(0.101)	
Illinois	0.659	− 0.039	− 0.129	0.020	0.028	366
		(0.064)	(0.214)	(0.067)	(0.092)	
California	0.613	0.056	0.088	− 0.118~	− 0.155~	449
		(0.053)	(0.083)	(0.061)	(0.081)	
New York	0.619	0.055	0.111	− 0.117*	− 0.209*	377
		(0.056)	(0.113)	(0.056)	(0.100)	
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [SR]						
ALL						
Maryland	0.297	− 0.039	− 0.066	0.022	0.029	486
		(0.059)	(0.101)	(0.059)	(0.078)	
Massachusetts	0.167	0.030	0.076	0.009	0.017	716
		(0.038)	(0.097)	(0.038)	(0.068)	

SUPPLEMENTAL EXHIBIT 5.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [SR] (CONTINUED)						
ALL (CONTINUED)						
Illinois	0.206	0.028	0.097	– 0.020	– 0.028	757
		(0.037)	(0.128)	(0.043)	(0.059)	
California	0.201	0.018	0.027	0.071~	0.091~	874
		(0.036)	(0.055)	(0.040)	(0.051)	
New York	0.230	– 0.039	– 0.079	0.002	0.004	771
		(0.038)	(0.077)	(0.042)	(0.082)	
FEMALE						
Maryland	0.278	0.002	0.003	0.102	0.136	244
		(0.072)	(0.121)	(0.078)	(0.104)	
Massachusetts	0.133	0.053	0.114	0.045	0.088	376
		(0.052)	(0.112)	(0.052)	(0.100)	
Illinois	0.191	0.030	0.108	– 0.024	– 0.033	392
		(0.050)	(0.178)	(0.061)	(0.084)	
California	0.158	0.043	0.065	0.040	0.050	428
		(0.047)	(0.071)	(0.050)	(0.062)	
New York	0.239	– 0.031	– 0.063	– 0.013	– 0.029	398
		(0.052)	(0.106)	(0.060)	(0.129)	
MALE						
Maryland	0.315	– 0.076	– 0.133	– 0.054	– 0.072	242
		(0.088)	(0.154)	(0.086)	(0.115)	
Massachusetts	0.202	0.007	0.021	– 0.031	– 0.050	340
		(0.055)	(0.170)	(0.054)	(0.087)	
Illinois	0.220	0.026	0.086	– 0.017	– 0.024	365
		(0.061)	(0.203)	(0.065)	(0.089)	
California	0.241	– 0.006	– 0.010	0.099~	0.131~	446
		(0.049)	(0.077)	(0.059)	(0.079)	
New York	0.222	– 0.048	– 0.099	0.020	0.035	373
		(0.055)	(0.113)	(0.061)	(0.108)	

SUPPLEMENTAL EXHIBIT 5.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
UNEMPLOYMENT INSURANCE DATA, FOURTH QUARTER OF 2007 THROUGH THIRD QUARTER OF 2008 [UI]						
EMPLOYED						
All						
Maryland	0.338	− 0.042	− 0.069	− 0.031	− 0.039	588
		(0.043)	(0.071)	(0.047)	(0.060)	
Illinois	0.134	0.016	0.047	0.040	0.055	896
		(0.027)	(0.083)	(0.036)	(0.049)	
California	0.293	− 0.036	− 0.056	0.001	0.002	923
		(0.031)	(0.048)	(0.034)	(0.043)	
Massachusetts	0.278	− 0.075*	− 0.198*	− 0.004	− 0.006	1,083
		(0.027)	(0.070)	(0.030)	(0.053)	
Four-site average	0.256	− 0.036*	− 0.076*	0.004	0.006	3,490
		(0.015)	(0.032)	(0.018)	(0.025)	
FEMALE						
Maryland	0.394	− 0.019	− 0.034	− 0.038	− 0.050	284
		(0.058)	(0.102)	(0.062)	(0.081)	
Illinois	0.183	0.020	0.063	0.032	0.042	440
		(0.042)	(0.132)	(0.051)	(0.067)	
California	0.282	− 0.009	− 0.015	0.031	0.039	423
		(0.042)	(0.066)	(0.047)	(0.059)	
Massachusetts	0.276	− 0.091~	− 0.219~	0.009	0.017	544
		(0.049)	(0.117)	(0.047)	(0.091)	
Four-site average	0.272	− 0.030	− 0.063	0.012	0.017	1,691
		(0.024)	(0.051)	(0.026)	(0.037)	
MALE						
Maryland	0.250	− 0.043	− 0.066	− 0.022	− 0.027	353
		(0.047)	(0.071)	(0.053)	(0.064)	
Illinois	0.073	0.008	0.022	0.046	0.064	545
		(0.027)	(0.079)	(0.037)	(0.051)	
California	0.232	− 0.047	− 0.072	− 0.012	− 0.015	646
		(0.033)	(0.051)	(0.034)	(0.044)	
Massachusetts	0.280	− 0.064~	− 0.184~	− 0.015	− 0.024	539
		(0.038)	(0.111)	(0.044)	(0.070)	
Four-site average	0.207	− 0.037*	− 0.075*	0.001	0.001	2,083
		(0.018)	(0.037)	(0.020)	(0.027)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, UI = Unemployment Insurance administrative records.
Model: Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For self-reports, data source is youth long-term survey. Interviewed youth ages 15 to 20 as of December 31, 2007. Unemployment Insurance data use individual level data from Maryland, Illinois, California, and Florida (representing individuals whose random assignment sites are Baltimore, Chicago, and Los Angeles) and aggregate data from Massachusetts (representing individuals whose random assignment site is Boston). All core youth ages 15 to 20 as of December 31, 2007 whose parent has a baseline consent. Massachusetts includes all youth ages 12 to 20. Since our request file did not split by age, we cannot look separately at those who were 15 to 20 in Massachusetts.
Measures: The administrative data only looks at the last four common calendar quarters (fourth quarter of 2007 through the third quarter of 2008).

SUPPLEMENTAL EXHIBIT 5.9. SELF-REPORTED YOUTH EMPLOYMENT AND SCHOOL ENROLLMENT						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
CURRENTLY EMPLOYED ONLY [SR]						
All	0.169	− 0.023	− 0.048	− 0.005	− 0.008	3,604
		(0.015)	(0.031)	(0.018)	(0.027)	
Female	0.165	− 0.023	− 0.046	− 0.028	− 0.043	1,838
		(0.021)	(0.043)	(0.023)	(0.037)	
Male	0.173	− 0.024	− 0.051	0.017	0.025	1,766
		(0.022)	(0.047)	(0.026)	(0.038)	
CURRENTLY ENROLLED IN SCHOOL ONLY [SR]						
All	0.390	0.039*	0.081*	0.007	0.011	3,604
		(0.019)	(0.040)	(0.021)	(0.031)	
Female	0.396	0.027	0.055	0.011	0.017	1,838
		(0.027)	(0.054)	(0.028)	(0.044)	
Male	0.384	0.052~	0.111~	0.003	0.004	1,766
		(0.028)	(0.061)	(0.031)	(0.045)	
CURRENTLY ENROLLED IN SCHOOL AND EMPLOYED [SR]						
All	0.226	− 0.023	− 0.048	− 0.028	− 0.042	3,604
		(0.019)	(0.040)	(0.021)	(0.031)	
Female	0.245	− 0.029	− 0.058	− 0.014	− 0.022	1,838
		(0.027)	(0.054)	(0.029)	(0.046)	
Male	0.208	− 0.017	− 0.036	− 0.042	− 0.061	1,766
		(0.026)	(0.057)	(0.028)	(0.040)	
CURRENTLY IDLE (NEITHER EMPLOYED NOR ENROLLED IN SCHOOL) [SR]						
All	0.215	0.007	0.014	0.026	0.039	3,604
		(0.018)	(0.038)	(0.020)	(0.031)	
Female	0.194	0.024	0.049	0.031	0.048	1,838
		(0.024)	(0.048)	(0.027)	(0.043)	
Male	0.235	− 0.011	− 0.023	0.022	0.032	1,766
		(0.027)	(0.058)	(0.031)	(0.045)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 15 to 20 as of December 31, 2007.
Measures: "Currently enrolled in school" also includes youth who are on summer vacation.

	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SURVEY DATA						
Currently receiving TANF [SR]	0.174	0.020 (0.020)	0.042 (0.043)	0.020 (0.028)	0.032 (0.044)	1,985
Currently receiving food stamps [SR]	0.498	0.008 (0.027)	0.017 (0.056)	0.010 (0.036)	0.016 (0.055)	1,977
ADMINISTRATIVE DATA						
Received TANF in the month closest to the survey month [TANF]	0.244	– 0.009 (0.023)	– 0.019 (0.048)	– 0.004 (0.030)	– 0.006 (0.047)	1,985
Active and received TANF in the month closest to the survey month [TANF]	0.187	0.011 (0.020)	0.024 (0.043)	0.006 (0.027)	0.009 (0.041)	1,985
Received food stamps in the month closest to the survey month [FS]	0.632	0.029 (0.025)	0.060 (0.052)	0.040 (0.033)	0.061 (0.051)	1,977
Active and received food stamps in the month closest to the survey month [FS]	0.557	0.022 (0.027)	0.045 (0.056)	0.033 (0.037)	0.051 (0.056)	1,977

SUPPLEMENTAL EXHIBIT 5.10. FAMILY TANF AND FOOD STAMPS BENEFITS IN MASSACHUSETTS, ILLINOIS, AND LOS ANGELES COUNTY, ADULT REPORTED AND ADMINISTRATIVE DATA BY MONTH CLOSEST TO INTERVIEW MONTH

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, TANF = Temporary Assistance for Needy Families records, FS = Food Stamps records.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: For self-reports, data source is the adult long-term survey and the sample is all adults interviewed. FS and TANF analyses use individual data from Massachusetts, Illinois, Los Angeles County, and South Carolina and represent individuals whose random assignment sites are: Boston, Chicago and Los Angeles. We received TANF and food stamps data from Maryland, but the data quality is suspect. Samples for TANF, and FS are all sample adults with baseline consent.

Measures: The time period is the month closest to the month of interview. "Active" means the family is receiving welfare benefits directly through the adult. Otherwise, general receipt of the welfare benefits means that the family is receiving welfare benefits either through the adult or another family member.

SUPPLEMENTAL EXHIBIT 5.11. FAMILY TANF AND FOOD STAMPS BENEFITS IN MASSACHUSETTS, ILLINOIS, AND LOS ANGELES COUNTY, ADULT REPORTED AND ADMINISTRATIVE DATA FOR THE SAME MONTH

OUTCOME	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
		ITT	TOT	ITT	TOT	N
SURVEY DATA						
Currently receiving TANF [SR]	0.178	0.034~	0.071~	− 0.007	− 0.011	1,875
		(0.021)	(0.043)	(0.027)	(0.042)	
Currently receiving food stamps [SR]	0.501	0.029	0.061	0.003	0.005	1,871
		(0.027)	(0.056)	(0.037)	(0.057)	
ADMINISTRATIVE DATA						
Received TANF in the month of survey [TANF]	0.253	− 0.006	− 0.013	− 0.011	− 0.016	1,875
		(0.023)	(0.048)	(0.031)	(0.047)	
Active and received TANF in the month of survey [TANF]	0.194	0.011	0.023	0.013	0.020	1,875
		(0.021)	(0.043)	(0.028)	(0.043)	
Received food stamps in the month of survey [FS]	0.650	0.034	0.070	0.028	0.044	1,871
		(0.025)	(0.051)	(0.034)	(0.051)	
Active and received food stamps in the month of survey [FS]	0.575	0.023	0.049	0.021	0.032	1,871
		(0.027)	(0.055)	(0.038)	(0.058)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, TANF = Temporary Assistance for Needy Families, FS = Food Stamps.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: For self-reports, data source is the adult long-term survey and the sample is all adults interviewed. FS and TANF analyses use individual data from Massachusetts, Illinois, Los Angeles County, and South Carolina and represent individuals whose random assignment sites are: Boston, Chicago and Los Angeles. We received TANF and food stamps data from Maryland, but the data quality is suspect. Samples for TANF and FS are all sample adults with baseline consent.
Measures: The time period is the month of survey. "Active" means the family is receiving welfare benefits directly through the adult. Otherwise, general receipt of welfare benefits means that the family is receiving welfare benefits either through the adult or another family member.

SUPPLEMENTAL EXHIBIT 5.12. ADULT SELF-REPORTED EITC

OUTCOME	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
		ITT	TOT	ITT	TOT	N
Received EITC for the previous calendar year [SR]	0.421	0.011	0.023	0.020	0.033	3,242
		(0.021)	(0.043)	(0.028)	(0.045)	
Received EITC and amount was more than $2,500 (2009 dollars) [SR]	0.194	0.007	0.014	0.042~	0.067~	3,119
		(0.017)	(0.035)	(0.024)	(0.038)	
Received EITC and used it to pay bills (credit card, housing costs, tuition) [SR]	0.303	− 0.007	− 0.015	− 0.029	− 0.047	3,231
		(0.020)	(0.040)	(0.026)	(0.041)	
Received EITC and put money into savings account [SR]	0.019	− 0.001	− 0.002	0.014	0.023	3,231
		(0.006)	(0.012)	(0.009)	(0.015)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. All interviewed adults.

CHAPTER 6

IMPACTS ON RISKY AND CRIMINAL BEHAVIOR

This chapter discusses the reasons why moving to better neighborhoods might affect the risky or criminal behavior of youth and adults. We present data on risky and criminal behavior from combinations of survey self-reports, proxy reports, and administrative arrest records for those who were youth at the time of our long-term follow-up (ages 13 to 20 or 15 to 20), grown children (ages 21 to 30), or adults (mostly household heads).

The long-term follow-up data show slightly more favorable treatment impacts for females than for males. These gender differences in long-term MTO impacts are more muted than the interim evaluation results, but are qualitatively similar. Assignment to the experimental and Section 8 groups increased the prevalence of smoking for male youth in the long-term MTO survey. Section 8 males also exhibited increased scores on a behavior problems index by a marginally significant amount equal to about 7 percent of the control group's mean. For female youth, assignment to the experimental group seemed to reduce the prevalence of drinking by around 11 percent of the control mean. We found few statistically significant impacts for most other measures of risky or criminal behavior among the different age groups we examined. One potentially important exception is arrests for drug distribution, for which we find mixed but suggestive indications of lower arrest rates among youth, grown children, and adults assigned to the MTO treatment groups relative to those assigned to the control group.

6.1 HYPOTHESES ABOUT EFFECTS ON RISKY AND CRIMINAL BEHAVIOR

This section begins with a brief review of age and developmental patterns of risky and criminal behavior. These age patterns are relevant for thinking about what sort of specific anti-social behaviors may be most susceptible to influence from MTO and for which particular age sub-groups within the study sample. We then discuss what previous theories in the "neighborhood effects" literature predict regarding MTO's impacts on risky and delinquent behaviors.

DEVELOPMENTAL PATTERNS OF RISKY AND CRIMINAL BEHAVIOR[1]

Criminal behavior is disproportionately concentrated during adolescence and early adulthood. Exhibit 6.1 provides an example of the "age-crime curve," which shows the fraction of all arrests for the Federal Bureau of Investigation's Uniform Crime Report "part 1" violent crime offenses (murder, rape, robbery, aggravated assault) and property offenses (burglary, motor vehicle theft, larceny) committed by different age groups using national data from 2009. Rates of criminal involvement increase during adolescence, starting around middle-school age, and peak during late adolescence and early adulthood (depending on the exact type of crime being examined). This age pattern is observed for both males and females, and has been documented across different countries and times (Hirschi and Gottfredson, 1983).

---

1 This section draws heavily on material from Ander et al. (2010).

EXHIBIT 6.1. THE "AGE-CRIME CURVE" FOR CRIMINAL BEHAVIOR: NATIONAL DATA ON PERCENT OF CRIME COMMITTED BY AGE GROUP

Notes: Percentages represent the fraction of arrests involving different age groups based on 2009 data from the Federal Bureau of Investigation's Uniform Crime Report. Violent offenses include murder, rape robbery, and aggravated assault. Property crimes include burglary, motor vehicle theft, and larceny.
Data source: Bureau of Justice Statistics (2011).

More generally, there is a substantial concentration of anti-social behavior within the population—particularly the most serious forms of anti-social behavior. In a landmark longitudinal study of 10,000 boys born in Philadelphia in 1945, University of Pennsylvania criminologist Marvin Wolfgang found that 6 percent of all boys accounted for one-half of all arrests and fully two-thirds of all arrests for violent crimes (Wolfgang, Figlio, and Sellin, 1972; Wolfgang, 1983). This pattern has also been widely replicated in other places and time periods (see, for example, Tracy, Wolfgang, and Figlio, 1985; Shannon, 1982; Tillman, 1987; Farrington, 2002).

A growing body of research building on Wolfgang's finding suggests that the age-crime curve shown in Exhibit 6.1 reflects the behavior of two largely distinct "types" of people. For a small share of all youth, anti-social and aggressive behavior begins during early childhood, and persists through adolescence into adulthood, although the manifestations of this behavior change over time. Psychologist Terrie Moffitt (1993) calls this group the *life-course-persistent offenders*, and

argues that their behavior stems from ill health, difficult temperament, and subtle neuropsychological and cognitive deficits, which are likely to be compounded by challenging family backgrounds (in part because many of these traits are heritable). More common are *adolescent-limited* offenders, who engage in delinquency because of "social mimicry." For some youth in this latter group, adolescent offending can be "ensnaring"—as when a youthful offense contributes to school dropout and harms future employment prospects, thereby making persistent offending more likely.

Gender is one important predictor of involvement in risky or criminal behavior, whether of the adolescent-limited or life-course persistent varieties. Male youth are much more likely than females to engage in most anti-social behaviors, particularly delinquency and violence. For example, national data from 1994 show that for youth under age 18, males are arrested more than four times as often as females for aggravated assault, and seven or eight times as often for robbery (Cook and Laub, 2002).

A variety of individual- and family-level characteristics help distinguish life-course persistent and adolescent-limited offenders from people who never engage in crime. These characteristics include poor academic performance, particularly on verbal tasks (Maguin and Loeber, 1996), and levels of warmth and consistent discipline within the home as opposed to hostility and conflict (Widom, 1989; Gonzalez and Dodge, 2009; Steinberg, Blatt-Eisengart, and Cauffman, 2006). Low levels of parental monitoring are also predictive of the risk of involvement in drug selling (Little and Steinberg, 2006). Since so many of these family-level risk factors are correlated with economic disadvantage, rates of anti-social behavior in the low-income MTO study sample are likely to exceed what we see in more nationally representative populations.

EXHIBIT 6.2. DEVELOPMENTAL PATHWAYS TO SERIOUS AND VIOLENT OFFENDING

AGE OF ONSET:
Late

PERCENTAGE OF BOYS:
Few

VIOLENCE
rape, attack, strong-arm, homicide

SERIOUS DELINQUENCY
auto theft, burglary

PHYSICAL FIGHTING
physical fighting, gang fighting

MODERATELY SERIOUS DELINQUENCY
fraud, pick pocketing

AUTHORITY AVOIDANCE
truancy, running away, staying out late

PROPERTY DAMAGE
vandalism, fire setting

MINOR AGGRESSION
annoying others, bullying

MINOR COVERT BEHAVIOR
shoplifting, frequent lying

DEFIANCE/DISOBEDIENCE

Overt Pathway

Covert Pathway
(before age 15)

STUBBORN BEHAVIOR

Early

Authority Conflict Pathway
(before age 12)

Many

Criminal behavior and particularly violence are perhaps of greatest public policy concern, given their disproportionately large social costs (Anderson, 1999; Cook and Ludwig, 2000; Cohen, 2005; Ludwig, 2006). But serious forms of anti-social behavior are almost always preceded by less-serious risky or anti-social behaviors. Exhibit 6.2 (reproduced from Thornberry, Huizinga, and Loeber, 2004) shows that most youth who wind up engaging in violent behavior have a history of engaging in less-serious problem behavior previously—suggesting a "developmental ordering to seriousness" of anti-social behavior. Exhibit 6.2 also makes clear that more youth engage in less-serious risky or anti-social behaviors than in more-serious forms of delinquency such as violence.[2] One important additional behavioral marker for youth-violence risk not reflected

2 Criminal offenders in general "tend to engage in a diversity of crime types [but] with a somewhat greater tendency to repeat the same crime or to repeat within the group of property crimes or the group of violent crimes" (Piquero, Farrington, and Blumstein, 2003).

in Exhibit 6.2 is substance use, including levels of use for intoxicating substances that fall below clinical thresholds for a substance-abuse disorder.

One implication of these developmental patterns is that less-serious forms of anti-social behavior by youth (such as disobeying parents and teachers or hanging out with other youth who tend to get into trouble), even if not of great social policy concern for their own sake, are important outcomes to examine because they are indicative of developmental trajectories that may lead to more serious and socially costly behaviors in the future.

These patterns also help guide our decisions about how to analyze the data from the MTO long-term study. The large differences in base rates for risky and criminal behavior by gender help motivate our decision to present results separately for male and female youth. Given the age pattern to anti-social behavior, we will tend to focus our analysis of more serious forms of criminal behavior on youth who are in their peak offending ages at the time of our survey (15 to 20 at the end of 2007) and also look separately at grown children in MTO who were under 18 at baseline but have now reached young adulthood (ages 21 to 30 at the end of 2007). Our outcomes of interest include measures that are primarily related to onset (for example, ever arrested or lifetime prevalence of drug use) as well as those that capture duration and desistance of the behavior of interest (for example, total number of arrests).

HYPOTHESES ABOUT POTENTIAL NEIGHBORHOOD INFLUENCES

Neighborhood context (and by extension neighborhood mobility) may influence youth involvement with risky and criminal behavior through the physical and institutional environments, as well as through the social environment (Exhibit 6.3). Most of the same mechanisms may be relevant for the behavior of MTO grown children and adults as well, although for convenience in this sub-section we just refer to "youth."

EXHIBIT 6.3. HYPOTHESIZED PATHWAYS THROUGH WHICH MTO MAY IMPACT DELINQUENCY AND RISKY BEHAVIOR

MTO INTERVENTION ➡ **RELOCATION TO LOW-POVERTY NEIGHBORHOODS**

COMMUNITY-LEVEL MEDIATORS

PERSON- AND FAMILY-LEVEL MEDIATORS

DELINQUENCY AND RISKY BEHAVIOR OUTCOMES

Community Norms and Values
· More pro-social peer group
· Better adult role models
· Better community monitoring of youth (collective efficacy)
· Possible adverse effects from increased competition with higher socio-economic status peers

Social and Physical Environment
· Reduced crime, violent activity
· Better school resources
· Increased recreational activities for youth
· More or higher quality policing

Economic Opportunities
· Better labor market opportunities

Adults
Positive Effects
· Increased sense of safety
· Less stress, anxiety

Negative Effects
· Feelings of isolation
· Depression caused by change

Youth
Positive Effects
· Increased sense of safety
· Increased perceived returns to "legitimate" activities (work, education)
· Increased orientation towards the future

Negative Effects
· Feelings of inadequacy, inability to compete

· Tobacco/alcohol use
· Use of marijuana and other drugs
· Self-reports of delinquent and illegal behaviors
· Gang involvement
· Fighting/violence
· Arrests/contact with the criminal justice system
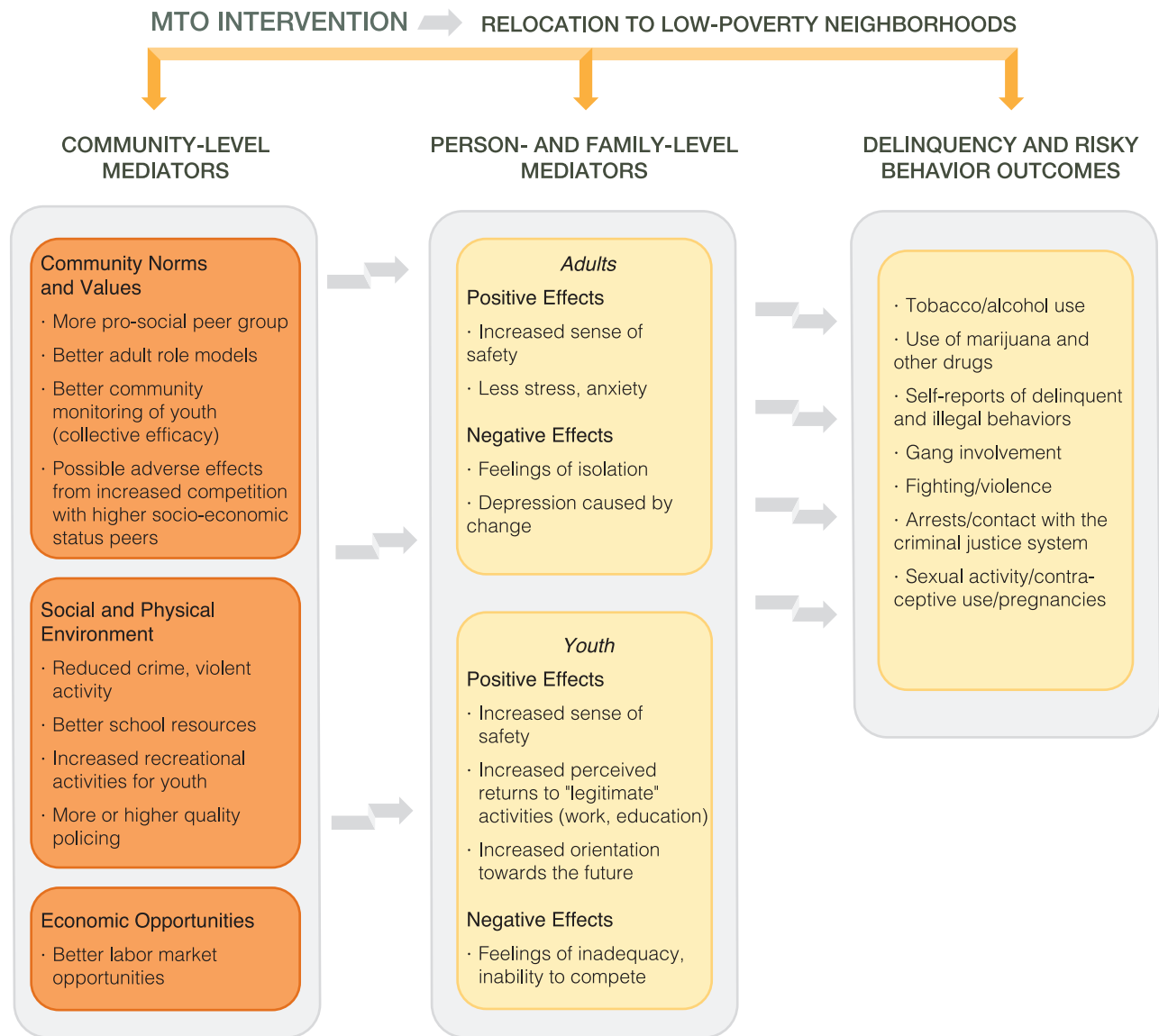· Sexual activity/contraceptive use/pregnancies

Exhibit 6.3 is a modified version of Exhibit 5.1 in the MTO interim impacts evaluation (Orr et al., 2003).

The physical features of a neighborhood might influence youth involvement with risky behavior or crime in a number of ways (Macintyre and Ellaway, 2003). For decades urban planners and criminologists have been concerned with the possibility that some building designs contribute to crime, for example, through the construction of enclosed stairwells in public housing buildings that are difficult for local residents to monitor. Other physical features of neighborhoods, such as access to public transportation and job opportunities, may influence the legal labor market opportunities that are available to people and that represent a key alternative to criminal behavior in the standard economic model of crime (Becker, 1968).

The quality or availability of local institutions may also be relevant for youth involvement in risky or criminal behavior. For example, previous research suggests that the quality of local public schools varies dramatically across areas (Rivkin, Hanushek, and Kain, 2005), which influences student success in school and consequently the relative attractiveness of pro- versus anti-social behavior. It is also possible that high-crime neighborhoods might have policing of lower quality or quantity compared to more affluent areas.[3] If youth move to neighborhoods with greater policing, MTO could increase the likelihood of a youth being arrested for any given level of actual criminal activity. Previous research raises the possibility that minorities may be at particularly elevated risk for being arrested, even after controlling for frequency of involvement with criminal behavior and other risk factors such as neighborhood type, family socio-economic status, and educational history (Huizinga et al., 2007).

Most of the theoretical work in the public health and social science literatures has been devoted to understanding the possible psychosocial mechanisms through which the neighborhood social context may influence behavior, including risky or criminal behavior. One influential typology of models in this literature comes from Jencks and Mayer (1990). *Epidemic models* are those that emphasize the power of peers to spread behaviors. Such contagion effects can arise from

learning criminal behavior from peers, pure preference externalities (individuals enjoy imitating their peers), stigma effects and social norms (the negative signal from criminal behavior declines when more people do them), and physical externalities (for example, higher rates of crime reduce the chances of getting arrested because of congestion effects in law enforcement); see Kleiman (1993), Cook and Goss (1996), Glaeser and Scheinkman (1999), Brock and Durlauf (2001), Manski (2000), and Moffitt (2001). Some epidemic models predict peer influences on youth criminal behavior that varies with the prevalence of peer criminal behavior within a community, which can lead to nonlinearities in peer effects or "tipping points." *Collective socialization* models concentrate on the way adults in a neighborhood influence young people who are not their children, for example, through human capital externalities that increase the opportunity costs of involvement with crime (Borjas, 1995), by acting as role models (Wilson, 1987), or the willingness of local adults to enforce shared values as in the "collective efficacy" model of Sampson, Raudenbush, and Earls (1997) (see also Coleman, 1988).

More recently new models from *behavioral economics* raise the possibility that moving to a less distressed area could reduce youth involvement with risky or criminal behavior by influencing future orientation, attitudes towards risk, and other aspects of decision making. For example, work by Loewenstein and O'Donoghue (2004) suggests that moves to less dangerous, less distressed areas could lead to more future-oriented, less risky, or violent behavior by reducing youth exposure to neighborhood stressors, which sap mental energy and reduce people's ability to control the more impulsive parts of their brains.

MTO could have additional effects on risky or criminal behavior that are mediated through MTO's impacts on household environments. For example, parental unemployment, substance use, poor mental health, exposure to community violence, and inadequate housing may all be risk factors for child maltreatment, and each of these risk factors could be affected by MTO. This possibility highlights the importance of measuring youth violence victimization that occurs within as well as outside of the home.

---

3 Whether or not such variation in policing exists depends in part on the degree to which policymakers choose to allocate additional police resources to the highest-crime neighborhoods (Sherman, 2002).

Although most behavioral models predict that MTO moves should reduce youth involvement with risky or criminal behavior, Jencks and Mayer's discussion implies that other outcomes are possible. *Competition models* emphasize the competition between neighbors for scarce resources like grades or jobs. Failure in the competition for pro-social rewards may lead youth to compete instead for anti-social rewards, which could elevate risk for criminal involvement. *Relative deprivation* models focus on negative psychological impacts from experiencing a decline in one's relative material or social standing in a community (Luttmer, 2005).

Additional uncertainty about MTO's net effects on youth risky or criminal behavior comes from the possibility that youth may choose their own social "micro-climates" from within their new neighborhood social "macro-climates" (Jencks and Mayer, 1990; Sharkey, 2006; Cicala, Fryer, and Spenkuch, 2011). For example, it is possible that any benefits from moving to less distressed areas through MTO could be lost if youth simply sort themselves back into peer groups that engage in and support anti-social behaviors that put youth at elevated risk for violent victimization or offending. However "fitting in" to new lower-poverty areas for MTO youth may entail learning new modes of language, dress, expected emotional responses (Lawler and Thye, 1999), and other aspects of social interactions, which could vary by gender, given evidence that boys and girls value different attributes and personal characteristics (Ferguson, 2001). Boys may have more difficulty integrating into their new neighborhoods, given evidence that boys are more likely than girls to "use aggressive or confrontational techniques to deal with interpersonal difficulties" (Coleman and Hendry, 1999, p. 218). Compared to girls, boys might also have more reputational or social capital to lose from moving to a new neighborhood, as suggested by Anderson's (1999) findings that many youth growing up in dangerous neighborhoods may develop violent social networks or strategic involvement in violence to help deter violence against themselves in the future.

Chapters 1 and 2 of this report show that the difference in average neighborhood environments between the MTO mobility groups (experimental and Section 8) and the MTO control group declines somewhat over time,

which raises the possibility that the behavioral impact of assignment to an MTO mobility group could decline over time.

On the other hand most of the psychosocial behavioral models discussed here suggest that the benefits of a given change in neighborhood environment may become more beneficial (or less detrimental) over time, as MTO families spend more time in lower-poverty areas. The benefits of exposure to higher-achieving peers, more pro-social adults, and higher-quality neighborhood schools, labor market options and policing environments may accumulate over time and lead to more pronounced positive effects on youth behavior through the epidemic, collective socialization, and institutional models, as well as more pronounced changes in youth decision-making processes. To the extent to which exposure to the opportunities available in lower-poverty areas makes MTO youth more competitive over time for getting good grades or jobs, any deleterious effects from competition or declines in relative position may also decline over time. We may also expect that any deleterious "moving effects" on families attenuate over time.

Moreover theories about developmentally "sensitive" or "critical" periods (Shonkoff and Phillips, 2000; Knudsen et al., 2006) predict that MTO impacts on risky or criminal behavior should be more pronounced for program participants who are adolescents at the long-term follow-up (and so were ages 10 or younger early in the study period, when MTO's impacts on neighborhood conditions were particularly large) compared to those who were adolescents at the interim follow-up (most of whom would already have been 10 or older at baseline). Sensitive periods are hypothesized to be ages in which development of certain skills or developmental processes are particularly susceptible to the social environment, but are not necessarily the only times in which those skills or processes can be modified. Critical periods are thought to be ages at which some skills or developmental processes are shaped, after which they are largely fixed.

The non-experimental empirical literature reveals mixed results on the importance of these theoretical neighborhood mechanisms in affecting risky and

criminal behaviors. Case and Katz (1991) found strong relationships between one's own risky and delinquent behaviors and that of one's peers for illegal drug use, alcohol use, and criminal offending in the Boston Youth Survey. However, Esbensen and Huizinga (1990) found that the level of disorganization of the neighborhood did not affect neighborhood-level prevalence or frequency of drug use. Studies of a sample of young black women in Chicago found some relationship between pregnancy risk and low neighborhood socioeconomic status (Hogan and Kitagawa, 1985) and evidence that this risk was related to lower contraceptive use (Hogan, Astone, and Kitagawa, 1985). The proportion of managerial workers in a census tract has been shown to be related to teen childbearing (Crane, 1991; Brooks-Gunn et al., 1993), but Case and Katz did not find direct evidence of peer influences on out-of-wedlock childbearing.

The non-experimental research seems to provide stronger support for an association between neighborhood attributes and involvement with crime or violence specifically. One of the best-known non-experimental studies on this question is Sampson, Raudenbush, and Earls' (1997) analysis of data from the Project on Human Development in Chicago Neighborhoods (PHDCN). Their analysis finds that one of the best predictors for variation in violence was the neighborhood's degree of informal social control together with social cohesion and trust, what they term "collective efficacy." Collective efficacy is found to have a strong association with violence even after controlling for a rich set of individual-level characteristics, and seems to mediate the effects of other neighborhood attributes such as socioeconomic composition (see also Sampson and Raudenbush, 1999, and Morenoff, Sampson, and Raudenbush, 2001). Qualitatively similar findings for violence offending rather than victimization that also draw on data from the PHDCN have been reported by Sampson, Morenoff, and Raudenbush (2005). Similar patterns of results for exposure to violence and violent behavior have been reported in a number of other studies (for example, Hirschfield and Bowers, 1997; Warner and Rountree, 1997; Rountree and Warner, 1999; Bellair, 2000; Peterson, Krivo, and Harris, 2000; Wikström and Loeber, 2000; Beyers et al., 2001; Simons et al., 2004;

for an excellent summary, see Sampson, Morenoff, and Gannon-Rowley, 2002).[4]

6.2 EARLIER RESEARCH: SHORT-TERM AND INTERIM IMPACTS ON RISKY AND CRIMINAL BEHAVIOR

Initial short-term findings from MTO drawing on data collected for individual demonstration sites showed that during the first few years after random assignment, males in both the experimental and Section 8 groups exhibited fewer behavior problems (disobedience, bullying, depression) than those in the control group in the Boston MTO site (Katz, Kling, and Liebman, 2001) and in the New York MTO site as well (Leventhal and Brooks-Gunn, 2003). Evidence from the Baltimore MTO site suggested that the experimental group males had fewer arrests for violent crimes relative to the control group (Ludwig, Duncan, and Hirschfield, 2001).

Results from the MTO interim evaluation, which measured outcomes for youth in a consistent way across all five MTO demonstration sites 4 to 7 years after random assignment, suggested that the impacts of moving to a less dangerous and distressed neighborhood on youth behavior might be more complex than the previous empirical literature would suggest. Although the results indicated that neighborhood influence on risky and criminal behavior by female youth follows a pattern predicted by most prior theoretical and non-experimental empirical studies, the pattern of program impacts is more complicated for male youth (Kling, Ludwig, and Katz, 2005). In the first few years after random assignment, male youth in the experimental group had fewer violentcrime arrests and behavior problems than those in the control group.[5] However by three to four years after randomization males in the experimental group were arrested more often than those in the control group, primarily for property crimes, and were also more likely to engage in a variety of other delinquent and risky behaviors.

---

4 A more recent observational study drawing on PHDCN data finds some evidence of a non-linear relationship between youth violence involvement and exposure to violent peers, where the effect of exposure to additional violent peers declines at higher levels of peer violence (Zimmerman and Messner, 2011).

5 These short-term findings are consistent with those reported for the Boston and Baltimore sites by Katz, Kling, and Liebman (2001) and Ludwig, Duncan, and Hirschfield (2001).

Evidence of a similar gender difference in how youth outcomes change in response to moving out of public housing into less distressed areas comes from a recent analysis of mortality records matched to an entirely separate sample of public housing families who applied for housing vouchers in Chicago in the late 1990s (Jacob, Ludwig, and Miller, 2011). That study found housing voucher offers produced declines in mortality rates for female youth compared to those who were not offered a voucher, but did not have the same protective effect on male youth.

Kling, Ludwig, and Katz (2005) argue that the most likely explanation for the unexpected gender difference in how youth risky and criminal behavior responds to residential mobility rests with a variant of the competition model discussed by Jencks and Mayer (1990). Over time some MTO males may have realized that they had a comparative advantage in the competition for anti-social rather than pro-social rewards within their new, lower-poverty neighborhoods.

6.3 BASELINE AND CONTROL GROUP CONTEXT

At baseline, three-quarters of control group families reported that their primary or secondary reason for moving was to get away from drugs and gangs and over 40 percent indicated that a member of their household had been victimized during the past 6 months (see Exhibit 1.2). Exhibit 6.4 shows that most forms of risky behaviors in the United States (other than marijuana use) have held steady or declined in prevalence since the MTO demonstration was planned in the early 1990s. The top panels report data from the Monitoring the Future survey on self-reported use rates over the past 30 days, and show that smoking and drinking have declined over time, and use of illicit drugs other than marijuana has been fairly constant. Marijuana use, in contrast, has increased over time. The bottom panel of Exhibit 6.4 shows that rates of teen pregnancy, births, and abortions have also declined over this period for 15- to 19-year-olds; this pattern holds for minority as well as white teens (not shown).

EXHIBIT 6.4. NATIONAL TRENDS IN SUBSTANCE USE AND TEEN PREGNANCY		
	YEAR	
SUBSTANCE USE IN THE LAST 30 DAYS	1991	2006
ANY ILLICIT DRUG (%)		
8th graders	5.7	9.5
12th graders	16.4	23.8
ANY ILLICIT DRUG OTHER THAN MARIJUANA (%)		
8th graders	3.8	3.5
12th graders	7.1	8.6
MARIJUANA/HASHISH (%)		
8th graders	3.2	8.0
12th graders	13.8	21.4
ALCOHOL (%)		
8th graders	25.1	13.8
12th graders	54.0	41.2
SMOKING (%)		
8th graders	14.3	7.1
12th graders	28.3	19.2
TEEN PREGNANCY (RATES PER 1,000 FEMALES AGES 15–19)	1991	2010
PREGNANCY RATE	115.3	71.5
BIRTHRATE	61.8	41.9
ABORTION RATE	37.4	19.3
Data source: Johnston et al. (2011) and Guttmacher Institute (2010).		

Crime rates, particularly for violent crime, were also declining dramatically over the MTO study period—although rates of violence had been surging in the MTO demonstration cities in the years during which MTO was being planned. While the overall homicide rate in the U.S. increased only modestly from 1985 to 1991 (from 8.2 to 10.4 per 100,000), the rate at which teen ages 13 to 17 were arrested for murder more than tripled over this time period (Cook and Laub, 2002). Most of this increase was driven by gun homicides, committed disproportionately by and against minority youth (Cook and Laub, 1998, 2002; Blumstein, 2000). Many criminologists believe that this surge in youth crime was driven by violence associated with the growth of crack cocaine, which may have contributed to growing gun use by teens involved in crack distribution and eventually other youth as well (Blumstein, 1995). The rate at which teens age 13 to 17 committed murder proceeded to drop by more than half over the next several years (Cook and Laub, 2002). The most likely explanations seem to be the ebbing of the crack epidemic, as well as increased spending on police and prisons (Levitt, 2004).

The declines in crime observed over the 1990s were most pronounced in those places that had the highest crime rates at the peak of the crack epidemic (Blumstein, 2000; Raphael and Ludwig, 2003; Levitt, 2004; Harcourt and Ludwig, 2006). The largest cities in particular experienced unusually pronounced declines in crime during the 1990s, and even within cities the highest-crime areas experienced above-average declines. This pattern will serve to narrow the differential in neighborhood crime rates between families assigned to the MTO experimental or Section 8 groups and the control group, because control group families were more likely to stay behind in the highest-crime neighborhoods, which subsequently experienced the largest drops in their crime rates.

Although crime dropped almost everywhere in the U.S. over the 1990s, the size of the drop was somewhat larger in some of our MTO demonstration sites than others, leading to some difference in crime levels across cities during our study period. In particular, Baltimore and Chicago have greatly elevated levels of crime and violence compared with the other three MTO cities. For

example, 1998 homicide rates per 100,000 equaled 47.1 in Baltimore and 25.6 in Chicago, compared with 6.1 in Boston, 11.8 in Los Angeles, and 8.6 in New York City.

6.4 DATA SOURCES AND MEASURES

A variety of survey-based and administrative data sources were used to measure MTO participants' involvement in risky or criminal behavior.

DATA SOURCES

One source of information about risky and criminal behavior is the long-term follow-up surveys we collected. Youth who were ages 13 to 20 at the end of 2007 were asked to self-report on their own risky behavior (youth ages 10 to 12 in our survey sample frame were asked a smaller subset of questions that were developmentally appropriate). For MTO participants who were children (under 18) at baseline but ages 21 to 30 at the time of our survey, and so not eligible for our survey sample frame, we asked MTO adult household heads to answer a few survey questions that provide proxy information about whether grown children in the study have been in prison or jail.

Our second major source of information about involvement in risky or criminal behavior is from official administrative arrest histories maintained by government agencies. We have obtained individual adult arrest histories from state-level criminal justice agencies in the five states that contain the original MTO demonstration sites (California, Illinois, Maryland, Massachusetts, and individual-level de-identified data from New York) as well as from eight other states in which MTO families have lived (Connecticut, Florida, Georgia, Iowa, Pennsylvania, South Carolina, Tennessee, and Virginia). These adult arrest histories capture 98 percent of all person-years for adults in the MTO study sample. We also obtained juvenile arrest histories with state-wide coverage for four of the five original site states (California, Illinois, Maryland, and Massachusetts), city-wide coverage for New York City, and state-wide coverage for two other states in which MTO families have lived (Florida and Iowa). Combined with the data from the adult arrest agencies, the juvenile arrest histories capture 97 percent and 95 percent of all person-years for grown children and youth, respectively, in the sample.

MEASURES

Most of the relevant survey questions about youth risky and criminal behavior are taken from the National Longitudinal Survey of Youth and replicate items used in the interim survey. These measures have been linked in the literature to many neighborhood characteristics, and they have been shown to be highly correlated with other measures of behavior problems (see Moore et al., 1999). Following standard practice in the research literature, we aggregate information on specific outcomes into several key indices:

- Abbreviated behavior problems index—Fraction of 11 behavior problems that youth respondent self-reported as "often or sometimes true" of themselves: has difficulty concentrating, cheats or lies, bullies or is cruel or mean to others, is disobedient at home, has trouble getting along with other children, is restless or overactive, has a very strong temper, is withdrawn/does not get involved with others, hangs around with kids who get into trouble, is disobedient at school, and has trouble getting along with teachers.
- Delinquency index—Fraction of 8 delinquent behaviors that the youth self-reported ever having engaged in: carrying a hand gun, belonging to a gang, purposely damaging or destroying property, stealing something worth less than $50, stealing something worth more than $50, engaging in other property crimes, attacking someone with idea of hurting them, having a situation end in serious fight or assault, and selling drugs.
- Risky behavior index—Fraction of 4 risky behaviors that a youth self-reported ever having engaged in: alcohol use, cigarette smoking, marijuana use, and sexual intercourse.

Both the delinquency and risky behavior indices measure having ever engaged in certain behaviors. This reference period has the advantage of encompassing events throughout the entire period since random assignment. Most youth were randomly assigned prior to the ages at which the behaviors tend to occur frequently. Some of these behaviors may have occurred before random assignment, but these should be both small in number

(since these youth were relatively young prior to random assignment) and approximately the same in prevalence in all random assignment groups.

We used our administrative arrest records to construct measures of whether the person was ever arrested (and the number of times arrested) between the time of MTO random assignment and December 31, 2007, which was the last date for which we could obtain arrest records from the major states containing most of the MTO families.[6] We also construct measures of ever arrested and number of arrests for specific types of crimes, namely violence (most commonly for aggravated or simple assault, and, less frequently, for more serious crimes like murder, rape, robbery, and kidnapping), property offenses (motor vehicle theft, larceny, burglary), drug offenses (drug possession and drug distribution), and all other offenses (a category that is dominated by relatively less serious crimes like disorderly conduct). If more than one charge was tied to that arrest date, each arrest was categorized using the most serious charge from that arrest event.[7] Arrests that occurred prior to randomization were controlled for in the statistical analysis (that is, used as explanatory variables), but were not included in the outcome measures.

6.5 LONG-TERM IMPACTS ON RISKY AND CRIMINAL BEHAVIOR

This section discusses the main results for the impacts of the MTO mobility experiment on risky and criminal behavior by MTO youth, grown children, and adult household heads.

---

6 Accessing administrative arrest records was more straightforward in some jurisdictions than in others, and so the point in calendar time at which we received data varied across jurisdictions—which means the last calendar year for which we have arrest data varies across areas. We chose to use administrative arrest records through December 31, 2007 as a common endpoint, which we have available for all jurisdictions mentioned in the text and is consistent with the original study design we planned before beginning data collection.

7 We select the most serious charge based on how New York State criminal law classifies offenses. We use New York state law because the criminal justice agency in that state only provides us with the most serious criminal charge per arrest, so using their selection criteria in the other states (where we have all charges associated with each arrest) helps improve consistency in the data across states. We have explored other systems for selecting the most serious charge per arrest and obtain quite similar results, in part because in the large majority of cases an arrestee is charged with a single criminal offense.

YOUTH OUTCOMES

The first panel of Exhibit 6.5 shows that the estimated effects of the MTO experimental and Section 8 treatments on the overall risky behavior index for all youth (boys and girls together) are not statistically significant. Sometimes statistically insignificant results are hard to interpret because they are not very precisely estimated—that is, the statistical uncertainty (confidence interval) around an estimate is so large that they cannot rule out even moderately large impacts. As a way to think about the precision of the insignificant estimates

presented in the top panel of Exhibit 6.5, the variation in the MTO experimental and Section 8 intent-to-treat (ITT) effects on the behavior problems index for all youth does not permit us to detect an impact if the difference from the control group is smaller than around .03, or roughly 6 percent of the control group's mean (.47). Looking at the individual components of this index reveals some signs of the same gender difference in youth responses to MTO mobility as were found in the interim study.

EXHIBIT 6.5. RISKY BEHAVIOR OUTCOMES FOR YOUTH AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
RISKY BEHAVIOR INDEX [SR]						
All	0.467	− 0.001	− 0.002	0.007	0.010	4,623
		(0.014)	(0.029)	(0.015)	(0.022)	
Female	0.442	− 0.027	− 0.054	− 0.017	− 0.026	2,358
		(0.019)	(0.037)	(0.020)	(0.031)	
Male	0.491	0.025	0.053	0.029	0.042	2,265
		(0.018)	(0.039)	(0.020)	(0.028)	
COMPONENTS OF THE RISKY BEHAVIOR INDEX						
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)	
EVER USED MARIJUANA [SR]						
All	0.363	− 0.008	− 0.017	0.003	0.005	4,609
		(0.020)	(0.042)	(0.022)	(0.032)	
Female	0.327	− 0.042	− 0.084	− 0.026	− 0.040	2,350
		(0.027)	(0.054)	(0.029)	(0.045)	

EXHIBIT 6.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EVER USED MARIJUANA [SR] (CONTINUED)						
Male	0.397	0.025	0.054	0.031	0.044	2,259
		(0.028)	(0.061)	(0.030)	(0.043)	
EVER HAD SEX [SR]						
All	0.662	− 0.003	− 0.005	− 0.001	− 0.001	4,580
		(0.017)	(0.036)	(0.019)	(0.029)	
Female	0.605	− 0.022	− 0.044	− 0.022	− 0.035	2,334
		(0.025)	(0.050)	(0.027)	(0.042)	
Male	0.716	0.017	0.036	0.020	0.029	2,246
		(0.023)	(0.050)	(0.026)	(0.037)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: The Risky Behavior Index is the fraction of the 4 risky behaviors listed above that the youth reports ever having exhibited.

The second panel of Exhibit 6.5 shows that compared to male youth assigned to the control group, those assigned to either the MTO experimental or Section 8 groups are about 13 percent more likely to smoke. In contrast, female youth assigned to the MTO experimental group are 6 percentage points less likely than those assigned to the control group to drink (about 11 percent of the control prevalence). The estimated impact for female youth assigned to the MTO Section 8 group is in the same direction but slightly smaller in magnitude and not statistically significant. And consistent with those gender differences, although not statistically significant, are the results for sexual activity and marijuana use, where the sign of the estimate for males in both treatment groups is positive (more sex and marijuana use) and the sign for females in both treatment groups is negative (less sex and marijuana use). These hints in the long-term MTO data of the same gender difference in youth response to MTO moves that were found in the interim MTO data are interesting in part because overlap between the two study samples is limited. The interim MTO study focused on delinquency and risky behavior outcomes among youth ages 12 to 19 as of the end of 2001, while the long-term youth survey examines youth ages 13 to 20 as of the end of 2007 (who would have been ages 7 to 14 at the end of 2001). The fact that a similar finding arises for two largely different samples of MTO youth would seem to increase our confidence that this gender difference in how youth respond to MTO-induced moves is not a chance finding.

Exhibit 6.6 shows that there are no statistically significant impacts of assignment to either the MTO experimental or Section 8 groups on more serious forms of anti-social or criminal behavior, including a measure of 11 problem behaviors the youth self-reports ever having engaged in (the behavior problems index) or 8 delinquent behaviors the youth self-reports ever having engaged in (the delinquency index) when we pool male and female youth together. However the estimated MTO impacts are all negative for female youth and positive for male youth, even if most of these estimates are not statistically significant. (The one exception is the positive Section 8 impact on the behavioral problems index for male youth, which is marginally significant.)

EXHIBIT 6.6. BEHAVIOR PROBLEMS AND DELINQUENCY FOR YOUTH AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
BEHAVIOR PROBLEMS INDEX [SR]						
All	0.379	0.004	0.008	0.009	0.013	4,629
		(0.010)	(0.022)	(0.011)	(0.017)	
Female	0.371	– 0.007	– 0.014	– 0.010	– 0.015	2,361
		(0.014)	(0.028)	(0.015)	(0.024)	
Male	0.387	0.015	0.032	0.027~	0.038~	2,268
		(0.015)	(0.032)	(0.016)	(0.023)	
DELINQUENCY INDEX [SR]						
All	0.146	– 0.002	– 0.004	0.008	0.012	4,625
		(0.008)	(0.017)	(0.009)	(0.014)	
Female	0.110	– 0.006	– 0.011	– 0.005	– 0.008	2,360
		(0.009)	(0.019)	(0.010)	(0.016)	
Male	0.181	0.002	0.004	0.021	0.030	2,265
		(0.013)	(0.029)	(0.016)	(0.022)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: 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 8 delinquent behaviors (for example, carrying a gun and destroying property) that the youth reported ever having exhibited.

Assignment to either of the MTO treatment groups did not have any statistically significant impacts on overall arrest rates of male or female youth ages 15 to 20, but impacts on these outcomes are somewhat imprecisely estimated (Exhibit 6.7). For example, the top panel of Exhibit 6.7 shows that for the MTO experimental ITT effect, for female youth we cannot rule out impacts on number of arrests that range from – .03 to +.24, that is, – 10 percent to +66 percent of the control group's mean number of arrests (0.36). For male youth, we cannot rule out impacts that range from around – 16 percent to +26 percent of the control group mean. Exhibit 6.7 suggests that the MTO experimental treatment may have increased propertycrime arrest rates, with positive point estimates for both male and female youth that are not quite statistically significant when disaggregated by gender. The MTO experimental treatment may have reduced arrest rates for drug selling among male youth (marginally significant), with an ITT effect that is equal to over one-third of the control group mean.

EXHIBIT 6.7. NUMBER OF POST-RANDOM ASSIGNMENT ARRESTS FOR YOUTH AGES 15–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ARRESTS BY CRIME TYPE						
NUMBER OF…						
ANY CRIME ARRESTS [CJR]						
All	1.078	0.093	0.195	– 0.170	– 0.259	4,717
		(0.101)	(0.212)	(0.112)	(0.170)	
Female	0.356	0.101	0.207	– 0.054	– 0.082	2,300
		(0.069)	(0.142)	(0.071)	(0.109)	
Male	1.741	0.087	0.187	– 0.283	– 0.425	2,417
		(0.185)	(0.397)	(0.204)	(0.306)	
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)	
DRUG CRIME ARRESTS [CJR]						
All	0.208	– 0.015	– 0.032	– 0.048	– 0.073	4,717
		(0.030)	(0.063)	(0.034)	(0.051)	
Female	0.019	0.016	0.032	– 0.017	– 0.026	2,300
		(0.016)	(0.033)	(0.016)	(0.025)	
Male	0.381	– 0.044	– 0.094	– 0.077	– 0.116	2,417
		(0.057)	(0.121)	(0.064)	(0.096)	
OTHER CRIME ARRESTS [CJR]						
All	0.306	0.000	– 0.001	– 0.048	– 0.072	4,717
		(0.034)	(0.071)	(0.037)	(0.055)	
Female	0.090	0.015	0.030	0.021	0.032	2,300
		(0.023)	(0.047)	(0.026)	(0.040)	
Male	0.503	– 0.014	– 0.029	– 0.113~	– 0.170~	2,417
		(0.064)	(0.136)	(0.067)	(0.100)	

EXHIBIT 6.7. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DRUG CRIME ARRESTS BY TYPE						
NUMBER OF…						
DRUG POSSESSION ARRESTS [CJR]						
All	0.117	0.010	0.020	− 0.034~	− 0.052~	4,717
		(0.019)	(0.039)	(0.019)	(0.028)	
Female	0.013	0.005	0.009	− 0.011	− 0.017	2,300
		(0.010)	(0.021)	(0.010)	(0.016)	
Male	0.213	0.015	0.032	− 0.057	− 0.085	2,417
		(0.035)	(0.075)	(0.035)	(0.052)	
DRUG DISTRIBUTION ARRESTS [CJR]						
All	0.091	− 0.025	− 0.052	− 0.014	− 0.021	4,717
		(0.017)	(0.036)	(0.022)	(0.033)	
Female	0.006	0.011	0.023	− 0.006	− 0.009	2,300
		(0.009)	(0.020)	(0.010)	(0.015)	
Male	0.169	− 0.059~	− 0.127~	− 0.020	− 0.031	2,417
		(0.032)	(0.069)	(0.042)	(0.063)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual criminal justice system arrest data. Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. All core household members ages 15 to 20 as of December 31, 2007.
Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

GROWN CHILDREN OUTCOMES

Exhibit 6.8 presents results for administratively-recorded arrest rates of grown children—MTO participants who were ages 21 to 30 at the end of 2007 and would have been roughly ages 15 to 25 at the time of the interim study. Data from the interim study showed that MTO reduced violent-crime arrests for male and female youth through 4 to 7 years after random assignment, but increased property-crime arrests for male youth (Kling, Ludwig, and Katz, 2005). The administrative arrest records that we have collected for the long-term follow-up show no statistically significant impacts on either violent or property crimes, although we do see some suggestive evidence for a decline in arrests for drug selling that echoes what we find for MTO youth in the long-term follow-up. The last row of Exhibit 6.8 shows that the Section 8 ITT effect for males on number of arrests since random assignment for drug selling is negative and equal to around 23 percent of the control group mean. The experimental ITT effect is in the same direction but half as large (in absolute value) and not statistically significant.

EXHIBIT 6.8. NUMBER OF POST-RANDOM ASSIGNMENT ARRESTS FOR GROWN CHILDREN AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ARRESTS BY CRIME TYPE						
NUMBER OF…						
ANY CRIME ARRESTS [CJR]						
All	2.862	− 0.090	− 0.212	− 0.116	− 0.202	4,641
		(0.171)	(0.404)	(0.192)	(0.332)	
Female	0.967	− 0.054	− 0.126	− 0.037	− 0.063	2,277
		(0.131)	(0.306)	(0.157)	(0.269)	
Male	4.673	− 0.124	− 0.296	− 0.192	− 0.335	2,364
		(0.303)	(0.726)	(0.347)	(0.607)	
VIOLENT CRIME ARRESTS [CJR]						
All	0.626	− 0.055	− 0.129	− 0.042	− 0.072	4,641
		(0.047)	(0.112)	(0.054)	(0.093)	
Female	0.279	− 0.038	− 0.089	− 0.047	− 0.080	2,277
		(0.045)	(0.105)	(0.051)	(0.087)	
Male	0.958	− 0.071	− 0.169	− 0.038	− 0.066	2,364
		(0.082)	(0.196)	(0.092)	(0.161)	
PROPERTY CRIME ARRESTS [CJR]						
All	0.633	− 0.006	− 0.014	− 0.042	− 0.072	4,641
		(0.054)	(0.128)	(0.061)	(0.105)	
Female	0.297	− 0.057	− 0.132	− 0.016	− 0.027	2,277
		(0.046)	(0.107)	(0.065)	(0.112)	
Male	0.953	0.044	0.105	− 0.064	− 0.112	2,364
		(0.093)	(0.222)	(0.100)	(0.175)	
DRUG CRIME ARRESTS [CJR]						
All	0.799	− 0.089	− 0.211	− 0.075	− 0.129	4,641
		(0.065)	(0.154)	(0.076)	(0.131)	
Female	0.127	− 0.022	− 0.052	0.033	0.057	2,277
		(0.044)	(0.103)	(0.051)	(0.087)	
Male	1.440	− 0.153	− 0.366	− 0.177	− 0.310	2,364
		(0.119)	(0.285)	(0.144)	(0.251)	
OTHER CRIME ARRESTS [CJR]						
All	0.804	0.060	0.141	0.042	0.072	4,641
		(0.066)	(0.155)	(0.074)	(0.128)	
Female	0.263	0.063	0.147	− 0.008	− 0.013	2,277
		(0.052)	(0.121)	(0.054)	(0.092)	
Male	1.321	0.056	0.134	0.087	0.153	2,364
		(0.116)	(0.278)	(0.137)	(0.239)	

EXHIBIT 6.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DRUG CRIME ARRESTS BY TYPE						
NUMBER OF…						
DRUG POSSESSION ARRESTS [CJR]						
All	0.461	– 0.042	– 0.100	– 0.011	– 0.020	4,641
		(0.044)	(0.104)	(0.053)	(0.093)	
Female	0.069	0.001	0.002	0.015	0.026	2,277
		(0.030)	(0.070)	(0.035)	(0.060)	
Male	0.835	– 0.084	– 0.202	– 0.037	– 0.065	2,364
		(0.082)	(0.196)	(0.103)	(0.179)	
DRUG DISTRIBUTION ARRESTS [CJR]						
All	0.338	– 0.047	– 0.111	– 0.063	– 0.110~	4,641
		(0.035)	(0.082)	(0.037)	(0.064)	
Female	0.058	– 0.023	– 0.055	0.018	0.032	2,277
		(0.024)	(0.056)	(0.028)	(0.049)	
Male	0.605	– 0.069	– 0.165	– 0.140*	– 0.245*	2,364
		(0.063)	(0.151)	(0.067)	(0.116)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Individual criminal justice system arrest data. Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. All core household members who are now grown children (under age 18 at baseline and ages 21 to 30 as of December 31, 2007).

Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

Exhibit 6.9 shows that there are no statistically significant effects of the experiment based on household head proxy reports about whether grown children were ever in jail or prison.

EXHIBIT 6.9. INCARCERATION OF GROWN CHILDREN AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EVER IN JAIL/PRISON [PR]						
All	0.205	0.007	0.016	– 0.008	– 0.014	3,087
		(0.018)	(0.041)	(0.023)	(0.041)	
Female	0.057	0.014	0.031	– 0.003	– 0.006	1,526
		(0.017)	(0.038)	(0.022)	(0.038)	
Male	0.357	– 0.001	– 0.001	– 0.013	– 0.024	1,561
		(0.030)	(0.072)	(0.037)	(0.070)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult long-term survey. Grown children (who were under age 18 at baseline and ages 21 to 30 as of December 31, 2007) of interviewed adults.

ADULT OUTCOMES

Exhibit 6.10 presents results for number of arrests following random assignment for the adults in our longterm follow-up survey sample. The vast majority of adults in MTO are female heads of household, so the exhibit does not present results separately by gender. The exhibit shows that adults assigned to the experimental group are arrested more often than controls for property crimes,

with an ITT effect that is marginally significant (p<.10) and equal to around one-third of the control group mean. The experimental ITT effect for drug selling is negative and not significant when we look at total number of post-randomization arrests (Exhibit 6.10), but marginally significant (p<.10) when we look at a measure of "ever arrested" for drug selling (Supplemental Exhibit 6.3).

EXHIBIT 6.10. NUMBER OF POST-RANDOM ASSIGNMENT ARRESTS FOR ADULTS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ARRESTS BY CRIME TYPE						
NUMBER OF…						
Any crime arrests [CJR]	0.567	0.052	0.110	− 0.016	− 0.026	4,376
		(0.064)	(0.135)	(0.060)	(0.096)	
Violent crime arrests [CJR]	0.149	0.001	0.002	− 0.009	− 0.014	4,376
		(0.020)	(0.043)	(0.022)	(0.035)	
Property crime arrests [CJR]	0.133	0.043~	0.091~	− 0.002	− 0.002	4,376
		(0.025)	(0.052)	(0.022)	(0.036)	
Drug crime arrests [CJR]	0.114	− 0.001	− 0.002	− 0.003	− 0.005	4,376
		(0.022)	(0.047)	(0.023)	(0.037)	
Other crime arrests [CJR]	0.171	0.009	0.020	− 0.003	− 0.005	4,376
		(0.027)	(0.056)	(0.025)	(0.040)	
DRUG CRIME ARRESTS BY TYPE						
NUMBER OF…						
Drug possession arrests [CJR]	0.080	0.006	0.013	− 0.004	− 0.006	4,376
		(0.018)	(0.038)	(0.019)	(0.030)	
Drug distribution arrests [CJR]	0.035	− 0.007	− 0.016	0.001	0.001	4,376
		(0.008)	(0.017)	(0.009)	(0.015)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; and adult or juvenile records from 8 additional states in which participants have lived. All long-term survey sampling frame adults with a baseline consent form.
Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

6.6 INTERPRETATION OF RESULTS FOR RISKY AND CRIMINAL BEHAVIOR

Overall the estimated effects of the MTO residential-mobility intervention on risky and criminal behavior that we found in the long-term MTO data (measuring outcomes 10 to 15 years after baseline) were more muted than what was found in the interim MTO study (4 to 7 years after baseline). Because MTO treatment impacts on neighborhood environments have been declining over the course of the study period (Chapter 2), the pattern of impacts on risky and criminal behavior over time is consistent with the idea that contemporaneous neighborhood conditions may be more important than accumulated exposure for outcomes in this domain.

We do not see signs of any sustained decline in violent criminal behavior of the sort found in the interim data, although the confidence intervals around our longterm estimates are fairly sizable and so we cannot rule out either moderately-sized declines (or increases) in that outcome. Our long-term follow-up data also yield suggestive evidence that arrests for drug selling may have declined for male youth, male grown children, and perhaps adults as well as a result of the MTO intervention. It is not clear why a residential mobility intervention would reduce this type of criminal behavior, but perhaps drug selling is a crime that requires cooperation among numerous people.

As with the interim MTO data, we find some signs that assignment to the MTO experimental rather than the control group might increase risky behaviors for male youth (smoking) while at the same time decreasing the prevalence of at least some of these behaviors (drinking) for female youth. A concern in interpreting the administrative arrest data is that the likelihood of arrest could vary across neighborhoods. In particular, if the quality of policing is relatively higher in lower-poverty areas, then any evidence of an increase in arrests in administratively-recorded arrest records may overstate any adverse effects of MTO in the direction of increasing criminal behavior. However the fact that we see signs that MTO moves increased self-reported smoking among male youth as well as administratively-recorded property-crime arrests suggests the (muted) gender difference in how youth respond to MTO in the long-term follow-up is not simply an artifact of varying policing quality across areas.

We now have at least suggestive evidence of gender differences in how youth respond to residential mobility from three different samples. This pattern was found very strongly in the interim MTO youth sample, and now in a more muted way for those program participants who were youth at the time of our long-term follow-up—a largely different cohort of youth compared to those examined in the interim study. In addition a qualitatively similar pattern has been found in an independent sample of public housing families in Chicago who applied for housing vouchers, using information from mortality records (Jacob, Ludwig, and Miller, 2011). Understanding the mechanisms behind this pattern and possible policy responses remain important priorities for social science research.

SUPPLEMENTAL EXHIBIT 6.1. PREVALENCE OF POST-RANDOM ASSIGNMENT ARRESTS FOR YOUTH AGES 15–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ARRESTS BY CRIME TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
ANY CRIME [CJR]						
All	0.297	0.013	0.027	– 0.016	– 0.024	4,717
		(0.016)	(0.034)	(0.018)	(0.028)	
Female	0.189	0.012	0.025	– 0.012	– 0.019	2,300
		(0.021)	(0.044)	(0.023)	(0.035)	
Male	0.397	0.014	0.029	– 0.019	– 0.029	2,417
		(0.024)	(0.052)	(0.028)	(0.041)	
VIOLENT CRIME [CJR]						
All	0.167	0.015	0.032	– 0.023	– 0.035	4,717
		(0.014)	(0.029)	(0.015)	(0.023)	
Female	0.103	0.006	0.012	– 0.030~	– 0.046~	2,300
		(0.017)	(0.034)	(0.017)	(0.026)	
Male	0.226	0.025	0.053	– 0.016	– 0.025	2,417
		(0.022)	(0.047)	(0.024)	(0.035)	
PROPERTY CRIME [CJR]						
All	0.132	0.019	0.040	– 0.001	– 0.001	4,717
		(0.012)	(0.026)	(0.014)	(0.021)	
Female	0.070	0.011	0.023	– 0.003	– 0.005	2,300
		(0.014)	(0.029)	(0.015)	(0.023)	
Male	0.189	0.027	0.057	0.001	0.002	2,417
		(0.020)	(0.043)	(0.023)	(0.034)	
DRUG CRIME [CJR]						
All	0.091	0.004	0.008	– 0.014	– 0.021	4,717
		(0.010)	(0.022)	(0.011)	(0.017)	
Female	0.016	0.009	0.018	– 0.008	– 0.012	2,300
		(0.008)	(0.016)	(0.007)	(0.011)	
Male	0.161	– 0.001	– 0.002	– 0.020	– 0.030	2,417
		(0.019)	(0.040)	(0.020)	(0.030)	
OTHER CRIME [CJR]						
All	0.159	– 0.010	– 0.020	– 0.016	– 0.025	4,717
		(0.013)	(0.027)	(0.014)	(0.021)	
Female	0.078	– 0.001	– 0.002	0.009	0.014	2,300
		(0.015)	(0.030)	(0.016)	(0.025)	
Male	0.234	– 0.017	– 0.037	– 0.041~	– 0.061~	2417
		(0.021)	(0.045)	(0.023)	(0.034)	

SUPPLEMENTAL EXHIBIT 6.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DRUG CRIME ARRESTS BY TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
DRUG POSSESSION [CJR]						
All	0.065	0.008	0.017	− 0.008	− 0.012	4,717
		(0.009)	(0.019)	(0.009)	(0.014)	
Female	0.012	0.004	0.008	− 0.007	− 0.010	2,300
		(0.007)	(0.014)	(0.006)	(0.010)	
Male	0.113	0.012	0.026	− 0.010	− 0.015	2,417
		(0.016)	(0.034)	(0.017)	(0.026)	
DRUG DISTRIBUTION [CJR]						
All	0.048	− 0.004	− 0.009	− 0.005	− 0.008	4,717
		(0.007)	(0.016)	(0.008)	(0.012)	
Female	0.005	0.009	0.018	− 0.003	− 0.004	2,300
		(0.005)	(0.011)	(0.005)	(0.007)	
Male	0.087	− 0.016	− 0.035	− 0.007	− 0.011	2,417
		(0.014)	(0.029)	(0.015)	(0.023)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual criminal justice system arrest data. Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. All core household members ages 15 to 20 as of December 31, 2007.
Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

SUPPLEMENTAL EXHIBIT 6.2. PREVALENCE OF POST-RANDOM ASSIGNMENT ARRESTS FOR GROWN CHILDREN AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
ARRESTS BY CRIME TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
ANY CRIME [CJR]						
All	0.511	– 0.003	– 0.006	– 0.017	– 0.030	4,641
		(0.018)	(0.043)	(0.019)	(0.034)	
Female	0.348	0.009	0.022	– 0.020	– 0.034	2,277
		(0.025)	(0.059)	(0.027)	(0.047)	
Male	0.666	– 0.014	– 0.034	– 0.015	– 0.026	2,364
		(0.025)	(0.059)	(0.027)	(0.046)	
VIOLENT CRIME [CJR]						
All	0.279	– 0.007	– 0.017	– 0.017	– 0.030	4,641
		(0.016)	(0.038)	(0.018)	(0.031)	
Female	0.152	0.004	0.010	– 0.026	– 0.045	2,277
		(0.019)	(0.045)	(0.021)	(0.036)	
Male	0.400	– 0.018	– 0.044	– 0.009	– 0.016	2,364
		(0.025)	(0.060)	(0.028)	(0.049)	
PROPERTY CRIME [CJR]						
All	0.274	0.008	0.019	– 0.016	– 0.027	4,641
		(0.017)	(0.039)	(0.018)	(0.031)	
Female	0.164	– 0.006	– 0.013	– 0.021	– 0.036	2,277
		(0.020)	(0.047)	(0.022)	(0.037)	
Male	0.379	0.022	0.052	– 0.010	– 0.018	2,364
		(0.025)	(0.060)	(0.027)	(0.048)	
DRUG CRIME [CJR]						
All	0.255	– 0.012	– 0.029	– 0.034*	– 0.059*	4,641
		(0.015)	(0.035)	(0.016)	(0.028)	
Female	0.077	– 0.005	– 0.011	– 0.005	– 0.009	2,277
		(0.015)	(0.035)	(0.016)	(0.028)	
Male	0.425	– 0.020	– 0.047	– 0.061*	– 0.107*	2,364
		(0.025)	(0.059)	(0.028)	(0.048)	
OTHER CRIME [CJR]						
All	0.311	0.024	0.056	0.008	0.014	4,641
		(0.017)	(0.039)	(0.018)	(0.031)	
Female	0.169	0.019	0.044	– 0.014	– 0.025	2,277
		(0.020)	(0.047)	(0.022)	(0.037)	
Male	0.447	0.029	0.068	0.029	0.050	2,364
		(0.026)	(0.062)	(0.028)	(0.049)	

SUPPLEMENTAL EXHIBIT 6.2. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
DRUG CRIME ARRESTS BY TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
DRUG POSSESSION [CJR]						
All	0.198	– 0.011	– 0.026	– 0.015	– 0.026	4,641
		(0.014)	(0.032)	(0.015)	(0.026)	
Female	0.053	– 0.003	– 0.008	– 0.007	– 0.012	2,277
		(0.013)	(0.030)	(0.014)	(0.024)	
Male	0.336	– 0.019	– 0.044	– 0.023	– 0.040	2,364
		(0.024)	(0.056)	(0.027)	(0.047)	
DRUG DISTRIBUTION [CJR]						
All	0.155	– 0.018	– 0.042	– 0.032*	– 0.055*	4,641
		(0.013)	(0.030)	(0.013)	(0.023)	
Female	0.037	– 0.009	– 0.021	0.001	0.002	2,277
		(0.011)	(0.026)	(0.012)	(0.021)	
Male	0.268	– 0.026	– 0.062	– 0.062*	– 0.108*	2,364
		(0.022)	(0.053)	(0.024)	(0.041)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Individual criminal justice system arrest data. Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; juvenile data from New York City; and adult or juvenile records from 8 additional states in which participants have lived. All core household members who are now grown children (under age 18 at baseline and ages 21 to 30 as of December 31, 2007).
Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

SUPPLEMENTAL EXHIBIT 6.3. PREVALENCE OF POST-RANDOM ASSIGNMENT ARRESTS FOR ADULTS						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ARRESTS BY CRIME TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
Any crime [CJR]	0.235	0.004	0.009	− 0.016	− 0.026	4,376
		(0.015)	(0.031)	(0.016)	(0.025)	
Violent crime [CJR]	0.095	0.007	0.014	0.000	0.000	4,376
		(0.010)	(0.022)	(0.012)	(0.018)	
Property crime [CJR]	0.080	0.020~	0.041~	0.004	0.007	4,376
		(0.010)	(0.022)	(0.011)	(0.017)	
Drug crime [CJR]	0.061	− 0.008	− 0.017	− 0.002	− 0.003	4,376
		(0.008)	(0.018)	(0.010)	(0.015)	
Other crime [CJR]	0.109	− 0.005	− 0.010	− 0.008	− 0.012	4,376
		(0.011)	(0.023)	(0.012)	(0.019)	
DRUG CRIME ARRESTS BY TYPE						
EVER ARRESTED AFTER RANDOM ASSIGNMENT FOR…						
Drug possession [CJR]	0.041	0.004	0.007	0.000	0.000	4,376
		(0.007)	(0.015)	(0.008)	(0.013)	
Drug distribution [CJR]	0.029	− 0.010~	− 0.020~	− 0.003	− 0.004	4,376
		(0.006)	(0.012)	(0.006)	(0.010)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT= Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: CJR = criminal justice records.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Adult and juvenile data from California, Illinois, Maryland, and Massachusetts; de-identified adult data from New York State; and adult or juvenile records from 8 additional states in which participants have lived. All long-term survey sampling frame adults with a baseline consent form.

Measures: 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. Drug crime arrests are classified as either possession or distribution arrests. "Other" arrest charges cover any charges not classified as violent, property, or drug crimes and include disorderly conduct and moving violations.

CHAPTER 7
IMPACTS ON EDUCATIONAL OUTCOMES

By providing opportunities for children and their families to move to less-distressed neighborhoods, MTO was expected to provide access to superior educational opportunities in the form of better schools and increased exposure to peers, neighbors, and communities that value academic achievement. This in turn could lead to improved educational outcomes for these youth, including increased achievement test scores, completion rates of high school, and advanced educational degrees, as well as higher earnings.

This chapter describes the educational experiences and behaviors of the MTO youth and the long-term impacts of the program on their educational outcomes. The results include effects on post-secondary school participation among children who were already in school when the program started, as well as the first estimates of MTO's effects on the youngest children at baseline—those between infancy and age 5—all of whom were too young to provide reliable measures of educational success at the MTO interim evaluation. These children are of particular interest because their opportunity to move through MTO to better neighborhoods and potentially better schools occurred at the earliest, and most malleable, stage of their cognitive and socio-emotional development.

We find that relative to youth in the control group, youth in the experimental and Section 8 groups experienced mixed impacts on school quality—they attended schools with lower proportions of low-income and minority students and with slightly higher average test scores, but the schools serving treatment-group youth also had larger overall student bodies and the experimental group's most recent school had higher student-teacher ratios. School mobility is similar across treatment and controls groups, although youth in the experimental group may have been distributed across a slightly larger number of school districts compared with the control group. Youth in the experimental group,

especially females, reported more hospitable school climates than the control group.

In keeping with the results from the interim study (Orr et al., 2003; Sanbonmatsu et al., 2006), MTO has few detectable effects on the school performance of youth. There are no statistically significant differences across randomized groups in achievement test scores. We see signs of reduced rates of grade retention for Section 8 female youth compared with controls, but on a few other measures such as high school graduation we see statistically significant or marginally significant declines in educational outcomes for youth in one or both of the treatment groups compared with controls. Given the large number of comparisons made, these impacts could potentially be chance findings. Disappointingly, the generally null results also extended to the younger children who had not yet entered school at baseline. Results for grown children are mixed.

7.1 BASELINE AND CONTROL GROUP CONTEXT

The plight of children in urban schools is widely documented. Children in urban communities perform well below their counterparts in suburban schools on standardized tests, are more likely to drop out of school, and are much less likely to attend college (Casserly, 2002; Swanson, 2009). Drop-out rates among low-income students are almost three times the rate of students in the middle three income quintiles (Chapman, Laird, and Kewal-Ramani, 2010). Graduation rates for whites and Asians are approximately 50 percent greater than those for blacks, Hispanics, and American Indians (Swanson, 2004). Ninth graders in the richest 20 percent of American households are almost seven times more likely than those in the poorest 20 percent to place in the top fifth of math percentile scores (Ingels et al., 2011). These poor educational outcomes have been linked to both low-quality schools and home environments that do not support educational achievement.

The experiences of MTO children are not unlike the experiences of low-income children in urban communities nationwide. Although the measures used in MTO do not correspond exactly to those used in studies of nationally representative samples of children, available data indicate elevated rates of problem behaviors among MTO children compared with the average child in America. Exhibit 7.1 presents summary statistics for the education-related variables collected at baseline for MTO children.[1] Approximately 6 percent of the older youth (ages 6 to 11 at baseline) had behavioral problems and 12 percent had learning problems at baseline, and 3 percent had been suspended or expelled from school in the two years prior to baseline. Over 20 percent of parents indicated that during the two years prior to baseline someone from their child's school had asked them to come in to talk about behavior or other problems the child was having at school.

MTO children attended schools at baseline that were characterized by both low achievement and high poverty. Exhibit 7.1 shows that 86 percent of their classmates were eligible for free or reduced lunch and 94 percent were members of minority groups. On average, students at these schools ranked at the 15th percentile on state assessments of academic skills. As discussed in Chapter 1, over half of MTO families indicated that the primary or secondary reason they wanted to move was to send their children to better schools.

The parents of MTO children also had relatively low levels of school attainment themselves at baseline compared with national samples, which is relevant given the strong documented association between parental schooling and children's learning outcomes (Jencks and Phillips, 1998). Only 35 percent of the sample adults had completed a high school degree at the time of random assignment (another 18 percent had earned a GED).

The MTO demonstration was implemented during the mid- to late 1990s, a time of increased emphasis on urban school reform. Since then, all five of the MTO cities have initiated high-profile efforts aimed at improving the educational outcomes for students in city schools. As a result, although experimental and Section 8 children might have moved to lower-poverty neighborhoods, the differences between the quality of the public schools in their new and original neighborhoods may have been attenuated by urban school reform and any new public and private investments that came as a result.

One example is the small high schools initiative that occurred in one of the five MTO demonstration sites—New York City—which together with other reforms boosted achievement test scores and graduation rates (Bloom, Thompson, and Unterman, 2010). Another example comes from the accountability efforts in the Chicago Public Schools, which has increased substantially the number of children performing at grade level in reading and mathematics over the last ten years, with the most dramatic increases occurring in some of the city's lowest performing schools (Roderick et al., 1999). The performance of students in the Chicago Public Schools—where a majority of MTO youth in the Chicago site attended schools throughout their school careers—has increased relative to other schools in the state (Jacob, 2003). Boston saw similar improvements across all grades and subject areas, including some narrowing of racial test-score gaps as well (Office of Research Assessment and Evaluation, 2010a). In Baltimore, from 2006 to 2010 the percentage of elementary school students achieving a level of proficient increased from 82 percent to 89 percent in math and from 78 to 88 percent in reading (Baltimore City Public Schools, 2011).

7.2 HYPOTHESES ABOUT EFFECTS ON EDUCATIONAL OUTCOMES

The explicit goal of MTO was to help families residing in public housing move to lower-poverty areas. And, as demonstrated in Chapter 2, following random assignment MTO experimental and Section 8 families were substantially more likely than controls to live in low-poverty neighborhoods—even measured 10 to 15 years after baseline. A large literature in the social sciences suggests multiple pathways through which neighborhood socioeconomic composition could directly or indirectly influence the amount that students learn in school, or their persistence in school and ultimate schooling attainment.

---

1 No information on standardized achievement test scores was collected at baseline.

Exhibit 7.2 presents a conceptualization of these pathways. Four important community characteristics are associated with the level of affluence of the neighborhood: the quality of the schools, community values and the community as a socializing agent for these values, the safety of the community, and the economic opportunities available in the community. As the model shows, one of these community characteristics—higher quality schools—may directly influence students' educational outcomes. In addition, in response to living in more affluent communities that may (a) have better educational institutions, (b) be safer and more cohesive communities, and (c) provide employment opportunities for adults, the attitudes and behavior of the children and their families are expected to change in ways that better support learning and achievement.

The first, and most direct, mediator of educational outcomes is school quality. Previous research typically finds that higher-income neighborhoods have better schools (Connell and Halpern-Felsher, 1997). Typically, "better" schools are defined as having higher teaching quality, greater educational resources, more rigorous course offerings, smaller class sizes, and a school climate that values learning and achievement and holds high expectations for students (Darling-Hammond, 1996). These school characteristics are, in turn, hypothesized to increase students' commitment to academic achievement and promote behaviors that produce achievement— studying, attendance, and engagement in school.

However, changing schools could have unintended negative consequences on school performance as well. One reason stems from whatever initial disruption may arise as a result of residential mobility induced by MTO. In addition, there may be increased competition for grades and academic success in more affluent schools, which could lead children who move through MTO to develop low self-confidence in the midst of a more affluent, higher-achieving peer group. A recent survey found a consistent negative association between school mobility and school achievement or high school graduation rates, especially for minority groups such as Hispanics and African-Americans (Beatty, 2010).

Children growing up in relatively more affluent communities may also be exposed to adults and peers who place relatively greater importance on educational achievement, learning, and pro-social behavior. More affluent neighborhoods also tend to have relatively higher rates of professional and managerial employment; people in these jobs may act as positive role models who help signal the returns to educational completion and academic success. In addition, peers could directly influence learning outcomes by providing children with opportunities to participate in more developmentally productive study groups, or by influencing the content or pace of teaching within the classroom environment.

A third community characteristic that may be relevant for children's educational outcomes is the neighborhood's physical environment—including safety. Parents who believe that their children are safe may feel less stress and anxiety, resulting in improved mental health and improved parenting (McLoyd and Flanagan, 1990). Children who feel safe in their physical surroundings may be more likely to flourish academically and personally. Previous observational research has shown a negative association between exposure to serious violence and children's short-term academic outcomes (Sharkey, 2010).

Finally, the economic opportunities within a neighborhood could influence children's schooling outcomes for several reasons. Communities with improved economic opportunities may increase the resources of parents and the local parent-teacher association enabling them to supplement public school resources or offering enrichment activities (for example, by raising money to hire teacher's aides or by offering after school programs). Living in a community with improved job opportunities could also improve the economic conditions of MTO families themselves, which may enable parents to increase their own private investments in their children's learning. And the presence of formal labor market opportunities in an area could influence perceptions of the economic returns to schooling attainment and achievement.

EXHIBIT 7.1. BASELINE CHARACTERISTICS OF THE YOUTH SAMPLE BY RANDOM ASSIGNMENT GROUP					
	CONTROL GROUP	EXPERIMENTAL GROUP	SECTION 8 GROUP	ALL GROUPS	*N*
YOUTH CHARACTERISTICS AT BASELINE					
Age (years) [PR]	4.7	4.6	4.7	4.7	5,101
Gender (%) [PR]					
Female	48.7	51.7	51.2	50.5	5,101
Male	51.3	48.3	48.8	49.5	5,101
YOUNGER YOUTH CHARACTERISTICS–AGES 0-5 AT BASELINE (%)					
Someone read to youth more than once per day [PR]	26.9	23.7	21.0	24.0	3,085
Enrolled in Head Start [PR]	17.3	22.8	21.7	20.7	2,674
OLDER YOUTH CHARACTERISTICS–AGES 6-11 AT BASELINE (%)					
Gifted student/did advanced coursework [PR]	14.5	12.3	12.9	13.2	2,016
Suspended or expelled from school in past two years [PR]	3.2	3.1	4.1	3.4	2,016
School called about behavior or schoolwork in past two years [PR]	19.6	20.0	21.8	20.4	2,016
Behavioral or emotional problems [PR]	6.1	5.1	5.9	5.7	2,016
Learning problem [PR]	13.4	10.1	13.7	12.3	2,016
OLDER YOUTHS' SCHOOL CHARACTERISTICS AT BASELINE					
Baseline grade [PR]	1.50	1.37	1.49	1.45	2,006
Percent free/reduced-price lunch [CCD]	85.4	85.9	86.5	85.9	1,534
Percent minority [CCD]	94.4	92.9	93.5	93.6	1,621
Number of students per teacher [CCD]	19.4	19.1	18.8	19.1	1,625
School's percentile rank on state exam [SLAD]	14.2	14.9	15.3	14.8	1,530
SAMPLE ADULT/PARENT CHARACTERISTICS AT BASELINE (%)					
High school diploma [PR]	34.0	35.7	35.1	35.0	5,101
GED [PR]	21.9	15.1	18.7	18.4	5,101
Enrolled in school [PR]	18.6	17.0	19.2	18.2	5,101
PARENT SCHOOL INVOLVEMENT AT BASELINE (%)					
IN THE PAST 12 MONTHS, A HOUSEHOLD ADULT HAD…					
Attended a school meeting [PR]	80.2	79.2	78.9	79.4	5,078
Attended a school event [PR]	69.2	67.7	69.2	68.6	5,080
Volunteered at school [PR]	47.8	47.9	46.2	47.4	5,081

EXHIBIT 7.1. (CONTINUED)					
	CONTROL GROUP	EXPERIMENTAL GROUP	SECTION 8 GROUP	ALL GROUPS	*N*
FAMILY'S PRIMARY OR SECONDARY REASON FOR SIGNING UP FOR MTO WAS BETTER SCHOOLS (%) [PR]	49.4	52.0	57.6	52.7	5,101

Notes: The numbers represent mean values on baseline characteristics for each treatment group and overall, weighted to reflect randomization ratios and the selection of youth into the interview sample and the two-phase sampling design of the long-term evaluation.
Square brackets indicate the source of the outcome information: PR = parent report. CCD = Common Core of Data. SLAD = School-level assessment data from the National Longitudinal School-Level State Assessment Score Database.
Data source and sample: Parent reported items were reported by the baseline head of household on the Moving to Opportunity Participant Baseline survey (age and gender were verified by the youth as part of the long-term survey). School characteristics come from the above-referenced data sources and are based on parent reports from the baseline survey or from the interim survey (if baseline school is missing in the baseline data).
Measures: The older youth (N = 2,016) and younger youth (N = 3,085) sample sizes are reflected in measures presented above that are also standard covariates in our regression analysis. Missing values for the covariates were imputed by site, treatment group, age, and gender. See Section 1.3 and the technical appendices (forthcoming) for details. Other measures, such as the Head Start measure and the school characteristics, have lower sample sizes because missing values were not imputed.

7.3 EARLIER RESEARCH ON NEIGHBORHOODS AND EDUCATIONAL OUTCOMES INCLUDING SHORT-TERM AND INTERIM MTO IMPACTS

A number of observational (nonexperimental) studies have shown that neighborhood socio-economic status is correlated with the educational outcomes of children, even after statistically adjusting for observable characteristics of the children and their families. Previous research studies have found that the presence of affluent neighborhood residents in particular is positively related to young children's IQ and verbal ability scores (e.g., Brooks-Gunn et al., 1993; Duncan, Brooks-Gunn, and Klebanov, 1994; Klebanov et al., 1997); higher reading recognition (Chase-Lansdale et al., 1997), math achievement (Entwisle, Alexander, and Olson, 1994); and adolescent school completion and self-reported grades (Brooks-Gunn et al., 1993; Connell and Halpern-Felsher, 1997; Dornbusch, Ritter, and Steinberg, 1991; Duncan, 1994).

The most influential "natural experiment" study of the relationship between neighborhood environments and schooling outcomes is the Gautreaux mobility program in Chicago. Starting in the 1970s, under Gautreaux thousands of African-American public housing residents were offered the opportunity to use housing vouchers to relocate to more racially and economically mixed neighborhoods in either the suburbs of Chicago or in other parts of the city itself. Students whose families moved to the suburbs initially had difficulties adapting to the higher academic expectations in suburban schools, and their grades suffered as a result (Rosenbaum, 1995).

However, later findings from Gautreaux showed that children who moved to suburban neighborhoods were more likely to graduate from high school and attend four-year colleges compared with those whose families moved to other parts of the city of Chicago (Rubinowitz and Rosenbaum, 2000; Popkin et al., 2000; DeLuca and Rosenbaum, 2003).

MTO was inspired by Gautreaux and designed as a true randomized experiment to overcome lingering concerns about selection bias that arise with studies of Gautreaux. As discussed in previous chapters, through the time of the interim follow-up (4 to 7 years after baseline), MTO was successful in helping experimental and Section 8 group families move to neighborhoods that are hypothesized to be more educationally "productive" than those serving controls with respect to some of the community characteristics that the previous section hypothesizes may be relevant for educational outcomes (Orr et al., 2003; Kling, Ludwig, and Katz, 2005; Sanbonmatsu et al., 2006; Kling, Liebman, and Katz, 2007). MTO helped families move into neighborhoods that had lower poverty rates and greater presence of college graduates, employed people, and people employed specifically in managerial or professional occupations. MTO adults in the experimental and Section 8 group were also more likely than controls to report feeling safe, and that their neighbors would work together to support shared pro-social norms—what sociologists call "collective efficacy" (Sampson, Raudenbush and Earls, 1997).

However MTO had much more muted impacts on the characteristics of the schools that children attended (Sanbonmatsu et al., 2006; see also Ferryman et al., 2008 and Deluca and Rosenblatt, 2010). Even though children in the experimental group were living in considerably safer and more affluent neighborhoods, some 80 percent were attending schools in the same struggling urban school districts as they had been at baseline. MTO was found to have had some small but positive effects on children's educational experiences based on the characteristics of children's schools, but few were attending schools that could even be considered above average for the states in which they resided.

Taken as a whole, the pattern of impacts on educational outcomes for MTO children found at interim was disappointing (Orr et al., 2003; Sanbonmatsu et al., 2006). Across the five MTO sites, the changes in neighborhood conditions associated with being assigned to the MTO experimental group were not sufficient to generate noteworthy impacts on the education-related behaviors, attitudes, or achievement test scores of MTO children. However, Burdick-Will and coauthors (2011) find a positive impact on reading achievement scores for the combined Baltimore and Chicago demonstration sites.[2]

If the underlying MTO process model is correct, the program's lack of impacts on educational outcomes might be attributed to the relatively small changes observed in the characteristics of the schools attended by the MTO children. MTO's qualitative report (Popkin et al., 2002) also suggested that increased opportunities for school choice, the children's desires to remain with friends, negative experiences in more affluent schools, and student special needs led some experimental group children to remain in the same school or to attend schools in their old neighborhoods. More recent qualitative interviews with Baltimore families found that parents' decisions about schooling often had little to do with academic quality. For many poor families, moving priorities began with proximity to transportation, family

members, and mothers' jobs—with schools sometimes coming after that, if at all (Deluca and Rosenblatt, 2010).

The interim report also observed that it might have been too soon to observe impacts on educational outcomes for these children, given the relatively small changes observed in the mediators. At the time of the interim evaluation some MTO children had been exposed to their new environment for as little as four years and there was some evidence that they were having difficulty adjusting to these new environments. It was hoped that more beneficial impacts would be observed in the long-term follow-up.

---

2 These results are consistent with the positive impact on reading scores for African-Americans in the MTO sample reported by Sanbonmatsu et al. (2006) and driven by results for the Chicago and Baltimore sites.

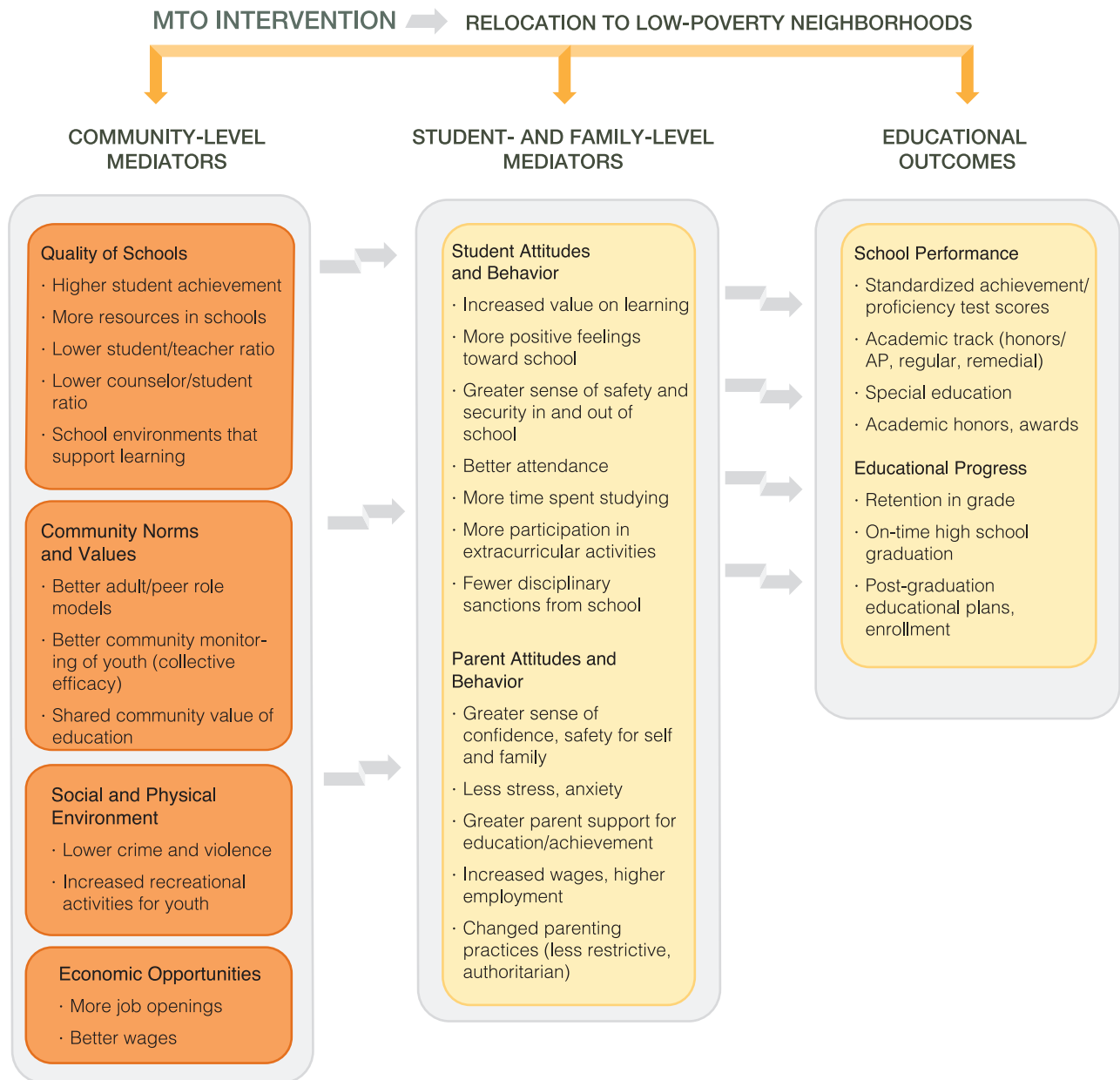**EXHIBIT 7.2. HYPOTHESIZED PATHWAYS THROUGH WHICH MTO MAY IMPACT ACHIEVEMENT AND EDUCATIONAL OUTCOMES**

MTO INTERVENTION → RELOCATION TO LOW-POVERTY NEIGHBORHOODS

COMMUNITY-LEVEL MEDIATORS

Quality of Schools
· Higher student achievement
· More resources in schools
· Lower student/teacher ratio
· Lower counselor/student ratio
· School environments that support learning

Community Norms and Values
· Better adult/peer role models
· Better community monitoring of youth (collective efficacy)
· Shared community value of education

Social and Physical Environment
· Lower crime and violence
· Increased recreational activities for youth

Economic Opportunities
· More job openings
· Better wages

STUDENT- AND FAMILY-LEVEL MEDIATORS

Student Attitudes and Behavior
· Increased value on learning
· More positive feelings toward school
· Greater sense of safety and security in and out of school
· Better attendance
· More time spent studying
· More participation in extracurricular activities
· Fewer disciplinary sanctions from school

Parent Attitudes and Behavior
· Greater sense of confidence, safety for self and family
· Less stress, anxiety
· Greater parent support for education/achievement
· Increased wages, higher employment
· Changed parenting practices (less restrictive, authoritarian)

EDUCATIONAL OUTCOMES

School Performance
· Standardized achievement/ proficiency test scores
· Academic track (honors/ AP, regular, remedial)
· Special education
· Academic honors, awards

Educational Progress
· Retention in grade
· On-time high school graduation
· Post-graduation educational plans, enrollment

Exhibit 7.2 is a modified version of Exhibit 6.1 in the MTO interim impacts evaluation (Orr et al., 2003).

7.4 DATA SOURCES AND MEASURES

A variety of survey-based and administrative data sources were used to measure MTO youth's school environments, experiences in school, educational expectations and educational and achievement outcomes.

SCHOOL MOBILITY AND SCHOOL CHARACTERISTICS

As part of our in-person survey data collection we obtained measures of school-level characteristics and mobility from an in-depth retrospective history of schools that children attended. We asked youth to report all of the schools they attended for each grade from their current or most recent school back through to their grade at baseline (or back through kindergarten for youth who were not yet of elementary school age at baseline). For approximately 60 percent of the youth sample, the school history is constructed by combining youth reports from their most recent school attended to the last school reported by their parents at the interim survey point.[3] One challenge in constructing the school history was to identify all of the specific schools that were mentioned by survey respondents. Most schools were accurately identified by using a school-lookup to identify the school during the interview. For schools that could not be identified during the interview, the respondent was asked for the name and address of the school. The information provided was sometimes, but not always, sufficiently detailed to identify the school. In some cases the respondent did not report the name of the school. Across our sample, we were able to identify the schools for approximately 92 percent of all student-years of elementary and secondary education attended from baseline to interview date.

We use the school history to construct measures of school mobility such as counts of the total number of different schools and school districts attended. Additionally, we used the school history information to match the schools attended by the MTO youth to data from the National Center for Education Statistics (NCES) Common Core of Data (1993-94 to 2009-10 academic years; U.S. Department of Education, 2011a) and Private School Surveys (available years between 1993-94 and 2007-08; U.S. Department of Education, 2011b). This allows us to construct a host of school-level characteristics depicting sociodemographic composition of the student body (racial/ethnic composition, free and reduced price lunch eligibility), school size (the number of students as of October), pupil-teacher ratio, Title I status, and the type of school (magnet or charter).[4]

In addition, we matched the schools to the National Longitudinal School-Level State Assessment Score Database (NLSLSASD) spanning 1999 to 2007 to obtain school-level test score data that we then used to rank the schools within MTO's five main states (NLSLSASD, 2010).[5] We use school-level statistics on the share of students at each school performing at or above proficiency in math and in reading (or language arts scores when reading is not available) to rank schools for a specific grade, academic year, and subject. We then average the reading and math rankings to obtain a combined measure.

We present measures of school characteristics for both the most recent school (with data available) that the youth attended, and for the average across all of the schools attended by the youth from baseline to the interview weighted by the number of years they attended a school. If a youth repeated a grade at a school, we count that school-grade twice. If a youth attended more than one school during the same year, we divide that year equally among the schools. To the extent possible, we link each year of a youth's school history to the closest year of data available for that school and that measure.

ACHIEVEMENT ASSESSMENTS

The youth in-person interviews included a 45-minute achievement assessment in math and reading as designed for the 5th and 8th grade follow-up waves of

---

3 For the long-term survey, these youth were asked to report on their schools and grades going back only to the highest grade reported by their parent at interim.

4 A Title I eligible school is a school designated as being high poverty and eligible to participate in federal Title I programs. Schoolwide Title I programs apply to Title I schools with student bodies that consist of at least 40 percent low-income students. Schools designated by state legislatures as "charter schools" provide free public education under a special charter granted by the legislature. Magnet schools and programs are designed to either attract students of different racial and ethnic backgrounds to reduce racial isolation or to focus on a particular academic or social theme such as science or performing arts.

5 We limit our analyses of test score rankings to schools within the five original MTO demonstration states.

the U.S. Department of Education's Early Childhood Longitudinal Study-Kindergarten Cohort (ECLS-K).[6] Youth ages 10 to 12 were administered the 5th grade test and youth ages 13 to 20 were administered the 8th grade test. The ECLS-K assessments are adaptive—respondents first take a short "routing test" that then directs the interviewer to provide them with test forms of different difficultly levels—to reduce the time required to accurately measure a subject's academic achievement level.[7] Because not all youth were asked every test item, our assessment results were scored for us by the Educational Testing Service (ETS) to provide us with estimates of each youth's underlying academic ability (known as a "theta score" in the testing literature) as estimated from a statistical model based on item response theory.

One concern was the possibility that some of the older youth in our survey sample would find the items on the tests too easy. If these youth correctly answered every item on the test, the assessment would lose its ability to provide information about which youth in the study know more than others, known as a "ceiling effect" in the testing literature. To guard against ceiling effects, we supplemented the ECLS-K 8th grade test with a small set of math and reading items from the U.S. Department of Education's National Educational Longitudinal Survey-1988 (NELS) assessment for high school students. Only about 8 percent of the MTO sample of 13- to 20-year-olds performed well enough to take these additional NELS items in math or reading. Another concern is 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. About 14 percent of

youth ages 13-20 performed below the level of chance on the reading test and about 7 percent on the math test.

The ECLS-K achievement assessments address many of the limitations documented from the administration of the Woodcock Johnson-Revised (WJ-R) assessment at the MTO interim study, which included evidence of "interviewer effects" (where results systematically differed across children according to which interviewer administered the tests) and average scores that did not vary much by age among adolescents. Equally important, the ECLS-K assessments are designed to more closely measure what children learn in school (as opposed to measuring aptitude only), and to be more sensitive to capturing whether MTO moved children into improved schooling and learning environments. The ECLS-K assessments have several other desirable features including appropriate coverage of material that is relevant for the wide dispersion of ages of MTO youth in the long-term MTO study, good discriminating power across a wide range of ability levels. They also have been extensively pre-tested and piloted to ensure that, for example, the test items work equally well for subgroups of youth such as those from racial and ethnic minority groups.

INTERVIEW OBSERVATIONS OF YOUTH DURING ACHIEVEMENT ASSESSMENTS

During the administration of the math and reading achievement assessments, the interviewers documented their perceived observations of youth attentiveness, cooperation and motivation using five predetermined response categories. We constructed a series of binary outcomes contrasting the two most positive responses with the three most negative responses. "Complete and full attention" and "very attentive" were contrasted with "attentive", "difficulty attending," and "unable to attend"; high and very high motivation with average, low, and very low motivation; and "very cooperative" and "cooperative" with "matter of fact", "uncooperative", and "very uncooperative." (We report these interviewer assessments in Supplemental Exhibit 7.7; they do not appear to be affected by the MTO intervention.)

SELF-REPORTED YOUTH OUTCOMES

The MTO youth survey additionally asked youth a series of questions about their school performance and,

---

6 The ECLS-K 5th grade test is administered in the home via paper and pencil and an interviewer-controlled easel (both for MTO and for ECLS-K itself). The ECLS-K 8th grade national test is administered in a group format in school whereas the MTO 8th grade assessment is individually administered to youth in their homes.

7 The ECLS-K 5th test is administered via an easel. Both the 5th and 8th grade tests are two-stage tests. Nationally, the first stage is a short routing test so that the combination of age, grade and performance help determine the difficulty of the second stage of the test (as compared with age or grade alone). This routing test is scored on-site by an assessor and is then used to direct children to the appropriate level for the second stage. The second stage 5th grade test is available in three levels and the second stage 8th grade test is available in two levels, where items in the highest level of the 5th grade test, for example, overlap with some items in the lowest level of the 8th grade test.

as discussed below in the section on mediators, their perceptions and experiences in school and where and how they spent time outside of school.

Schooling Outcomes: Youth reported their overall grades in their last year of school and, as measures of their educational track, whether they had ever been enrolled in an advanced math or Advanced Placement (AP) course or a program for the gifted and talented; whether they had ever been enrolled in any special class or received special help for any emotional, physical or mental condition; and whether they had ever repeated a grade. For older youth (ages 15 to 20), we constructed a measure of being educationally "on track"—currently in school or had received a high school degree or certificate of General Educational Development (GED)—and as a measure of college preparation, we also asked whether youth had taken the SAT or ACT.[8]

PARENT-REPORTED OUTCOMES FOR GROWN CHILDREN

In the MTO adult or household head survey, parents reported on their MTO grown children, who were ages 21 to 30 as of December 31, 2007, and so too old for our long-term follow-up youth survey sample, but who were under age 18 at baseline. Specifically, parents were asked to report whether each of their grown children was currently enrolled in school, had attained a GED or high school diploma, and had ever been enrolled in college.

SELF-REPORTED MEASURES OF CANDIDATE MEDIATORS

School Climate: Youth reported whether they strongly agreed, agreed, disagreed, or strongly disagreed with five statements about the climate of their most recent school:[9] (1) teachers are interested in students; (2) students who study hard are teased; (3) discipline is fair; (4) the youth feels safe at the school; and (5) the youth often feels "put down" by teachers. We calculated a school climate index from these measures as the fraction of normatively

positive responses on the five items: strongly agree/agree responses to the positive items (teachers interested in students, fair discipline, and safety) and disagree/strongly disagree responses to the negative items (teasing and feeling "put down"). These items were drawn from a variety of sources. The teacher interest in students, discipline, and safety items replicate items asked in the interim MTO study and are drawn from the National Longitudinal Survey of Youth-1997 (NLSY97). The teasing item comes from Harvard University professor Ronald Ferguson's Tripod Study, and the "put down" item comes from NELS.

Problem behavior: We constructed tardiness and absenteeism outcomes to indicate whether the youth was late for school once or more per month, whether the youth was absent more than five percent of the school year, and the fraction of the school year that the youth was absent. Youth also reported whether they had been suspended or expelled from school in the past two years. These items all replicate items used at interim.[10]

Educational expectations: Youth are asked to report the highest degree they expect to complete ranging from less than high school graduation to graduate or professional school. We also asked youth a similar question about how far in school they believed their mother wanted them to go (same response options). These items were borrowed from NELS.

School engagement: The school engagement measures focus on homework and reading. We asked youth how much of their assigned homework that they usually complete, with response options ranging from "All plus some extra" to "None" and including a "Never assigned" option. For youth who indicated that homework was assigned, we also asked separate questions about how many hours per week they spent on homework in school and out of school. Additionally, youth reported the number of hours per week they spent doing additional reading on their own outside of school (not in connection with schoolwork).[11]

---

8 Most of these items were also asked at the interim study and are borrowed from (or are original items similar to items from) NLSY97. Exceptions include the repeated grade item, asked at interim and borrowed from the Los Angeles Family and Neighborhood Survey (LA-FANS). The gifted/talented program and special education items are new to the long-term survey and come from NELS.

9 Youth who were enrolled in post-secondary school or who were no longer in school were asked to report on their most recent year in high school.

10 Original source surveys for these adapted items are as follows: tardiness—Survey of Program Dynamics-1998; absenteeism—NLSY97; suspension/expulsion—LAFANS.

11 Original source surveys for these adapted items are as follows: homework completion—Ronald Ferguson's Tripod Study; homework hours—NELS; and additional reading—the 1979 wave of NLSY.

Time use and access to technology: To measure where and how youth spend their time outside of school, youth report on their participation in school sports or school clubs or groups and, if so, whether any leadership positions were held in those school-related activities. We also asked how often youth participate in organized group activity, hang out in their neighborhood or a nearby basketball court, shop at a mall or a store, and hang out at their own house and at someone else's house. We also determined access to technology by asking whether they use a computer in their home and how often they use the internet.

PARENT-REPORTED MEASURES OF CANDIDATE MEDIATORS

Parents were asked to report on the schooling, health, and behavior of one randomly selected youth ages 10 to 17 as of December 2007 in their household. Parents reported whether in the past two years someone from the youth's school had asked someone from the household to "come in and talk about problems [the youth] was having with school work or behavior." To measure their engagement with their child's schooling, we asked parents whether in the past 12 months they or another household adult had attended a school meeting or event (such as a back-to-school night or parent/teacher organization meeting). We also asked whether they had volunteered or worked at the youth's school or been on a school committee or club outside of school. Finally, parents reported whether they were very, somewhat, or not all satisfied with the education their child had received up to that point.[12]

POST-SECONDARY ENROLLMENT FROM THE NATIONAL STUDENT CLEARINGHOUSE

In addition to obtaining self-reported data on college enrollment from youth, we obtained post-secondary enrollment data on MTO youth and grown children from a match through the StudentTracker service of the National Student Clearinghouse (NSC). The NSC is a non-profit organization that works with colleges and universities to provide degree and enrollment

verification. The NSC enrollment database covers over 3,000 colleges and over 90 percent of students in the United States (National Student Clearinghouse, 2011). We focus our analyses on enrollment data for the three-year period from January 2007 to January 2010.[13] The NSC data allowed us to construct measures of whether MTO participants had been enrolled in any post-secondary institution in their database as well as the types of institutions (two- and four-year colleges; private, public, and for-profit colleges) in which they were enrolled and the length of their enrollment. NSC data coverage is generally even across the treatment groups and there is little variation in coverage by randomization site. For interviewed youth and grown children whose parents were interviewed, we also have survey reports of any college attendance and attendance at two- vs. four-year colleges. Comparisons of the NSC data measures and the survey outcome measures appear in Supplemental Exhibits 7.8 and 7.9. The results from the NSC do not differ substantially from the results from self-reporting.

7.5 LONG-TERM IMPACTS ON HYPOTHESIZED MEDIATORS OF EDUCATIONAL EFFECTS

Exhibit 7.3 presents MTO's impacts on the characteristics of the schools attended by MTO youth over the course of the long-term follow-up period. Compared with schools attended by control group youth, MTO experimental and Section 8 youth attended schools that differed across a variety of socioeconomic and demographic characteristics. Youth in the experimental and Section 8 groups attended schools with lower proportions of minority students and students receiving free or reduced-price lunch, and youth in the experimental group attended schools that performed better on state exams. Experimental and Section 8 youth attended schools that were 8 to 11 percent larger on average than those attended by control group students, and for youth in the experimental group, the most recent schools had more pupils per teacher as well. The ratio of pupils to teachers differs from the number of students per classroom because it includes reading specialists and

---

12 The satisfaction item was new to the long-term survey and came from NELS, while the remaining items were originally asked on the MTO baseline survey and were also asked as part of the interim study.

13 NSC data were available back through 2001, but it took until the end of 2006 for the NSC to be near complete (96% of schools had joined the NSC by then), so we have limited our analysis to data from 2007 forward.

other types of teachers. MTO did not affect the type of school youth attended in terms of whether the school was a charter or a magnet.[14]

Youth in the experimental group attended schools that ranked slightly higher on statewide reading and math exams compared with controls. Youth in all three treatment groups disproportionately attended low performing schools, with average school rankings ranging from the 19th to 22nd percentiles across the groups. Only about one in six youth from either the treatment or control groups recently attended a school in the top half of their state's schoolwide test score distribution.

Exhibit 7.4 presents MTO treatment effects on school mobility. At a marginally significant level, experimental group youth may have attended school in a slightly larger number of total different school districts compared with the control group. The total number of schools attended was similar across the MTO treatment groups.

Broad objective measures of school-level student demographics and standardized test score performance provide one picture of school quality. This picture can be complemented by the youth's own perceptions of school quality (Exhibit 7.5). Experimental group youth reported marginally more favorable and hospitable interactions and perceptions of the school environment than the control group, as measured by our index of school climate. More specifically, compared with their control group peers, experimental group youth were less likely to report feeling put down by teachers, and female experimental group youth in particular were more likely to report that teachers took interest in students.

Supplemental Exhibits 7.1 through 7.4 present various other aspects of school participation and related activities that could mediate the effects of MTO on educational outcomes. Youth in the experimental and Section 8 group have similar levels of tardiness, absenteeism, and behavior problems in school as the control group (Supplemental Exhibit 7.1). However, MTO male youth in both the

experimental and Section 8 groups are more likely to be suspended or expelled from school in the past two years compared with controls. MTO experimental group youth—especially males—also are less likely to report that they expect to complete college. These experimental group youth are no more likely to report their mothers want them to complete college than are the control group youth (Supplemental Exhibit 7.2). The MTO treatments had few detectable effects on time spent on homework or reading, or on parent involvement in the youth's school (Supplemental Exhibit 7.3). MTO had little effect on parental satisfaction of their children's education for the experimental group, however parents in the Section 8 group were less satisfied with their children's education.

Youth reports of where their out-of-school time is spent, computer use and access to the internet are generally similar for MTO experimental, Section 8 and control groups, except for some indication of higher rates of home computer use for male youth in the Section 8 group compared with controls (Supplemental Exhibit 7.4). There is a small reduction in MTO experimental and Section 8 youth spending time at someone else's house compared with the control youth.

---

14 Only a small percentage of MTO youth attended private elementary or secondary school; youth in all three random assignment groups spent an average of about 4% of their school years since baseline attending private schools.

EXHIBIT 7.3. CHARACTERISTICS OF SCHOOLS ATTENDED BY YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
RACE/ETHNICITY OF STUDENTS AT SCHOOL						
MINORITY [CCD, PSS]						
Most recent school	0.879	– 0.023*	– 0.047*	– 0.014	– 0.021	5,045
		(0.010)	(0.021)	(0.011)	(0.016)	
Average school	0.904	– 0.037*	– 0.077*	– 0.016~	– 0.023~	5,077
		(0.008)	(0.016)	(0.008)	(0.012)	
BLACK [CCD, PSS]						
Most recent school	0.495	– 0.009	– 0.020	– 0.021~	– 0.031~	4,857
		(0.010)	(0.022)	(0.012)	(0.019)	
Average school	0.532	– 0.022*	– 0.046*	– 0.025*	– 0.037*	5,054
		(0.007)	(0.016)	(0.009)	(0.013)	
HISPANIC [CCD, PSS]						
Most recent school	0.342	– 0.015~	– 0.032~	0.003	0.005	5,058
		(0.009)	(0.019)	(0.010)	(0.014)	
Average school	0.339	– 0.022*	– 0.046*	0.006	0.009	5,077
		(0.007)	(0.015)	(0.007)	(0.011)	
SOCIOECONOMIC STATUS OF STUDENTS AT SCHOOL						
SHARE ELIGIBLE FOR FREE LUNCH [CCD]						
Most recent school	0.598	– 0.025*	– 0.053*	– 0.021~	– 0.031~	4,766
		(0.010)	(0.021)	(0.011)	(0.017)	
Average school	0.701	– 0.048*	– 0.101*	– 0.026*	– 0.039*	5,043
		(0.007)	(0.015)	(0.008)	(0.012)	
SHARE ELIGIBLE FOR FREE OR REDUCED-PRICE LUNCH [CCD]						
Most recent school	0.668	– 0.026*	– 0.054*	– 0.021~	– 0.031~	4,749
		(0.010)	(0.022)	(0.011)	(0.017)	
Average school	0.752	– 0.040*	– 0.083*	– 0.019*	– 0.029*	5,043
		(0.007)	(0.014)	(0.008)	(0.012)	
SHARE SCHOOLS ELIGIBLE FOR TITLE I [CCD]						
Most recent school	0.731	– 0.069*	– 0.145*	– 0.052*	– 0.078*	5,036
		(0.017)	(0.036)	(0.018)	(0.027)	
Average school	0.774	– 0.047*	– 0.099*	– 0.020~	– 0.029~	5,077
		(0.010)	(0.022)	(0.011)	(0.017)	

EXHIBIT 7.3. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SCHOOL SIZE						
NUMBER OF STUDENTS [CCD, PSS]						
Most recent school	1235.1	139.4*	290.6*	104.5*	156.4*	5,058
		(38.4)	(80.0)	(41.2)	(61.7)	
Average school	927.1	25.6~	53.4~	41.8*	62.5*	5,077
		(14.9)	(31.1)	(17.5)	(26.1)	
PUPIL–TEACHER RATIO [CCD, PSS]						
Most recent school	16.463	0.377*	0.784*	0.230	0.344	4,997
		(0.170)	(0.354)	(0.191)	(0.286)	
Average school	17.834	− 0.103	− 0.214	− 0.061	− 0.091	5,076
		(0.103)	(0.215)	(0.113)	(0.170)	
SCHOOL TYPE						
CHARTER SCHOOL [CCD]						
Most recent school	0.061	− 0.009	− 0.019	− 0.013	− 0.019	5,038
		(0.010)	(0.020)	(0.010)	(0.015)	
Average school	0.025	− 0.006~	− 0.013~	− 0.005	− 0.008	5,077
		(0.004)	(0.008)	(0.004)	(0.006)	
MAGNET SCHOOL [CCD]						
Most recent school	0.163	0.011	0.022	− 0.004	− 0.005	5,020
		(0.015)	(0.031)	(0.016)	(0.024)	
Average school	0.161	− 0.013	− 0.028	− 0.016	− 0.025	5,077
		(0.010)	(0.020)	(0.010)	(0.015)	
SCHOOL-LEVEL RANKING ON STATE EXAM						
PERCENTILE RANKING [SLAD]						
(ONLY THROUGH 8TH GRADE FOR NEW YORK AND MARYLAND)						
Most recent school	24.848	1.204	2.523	0.178	0.265	4,884
		(1.064)	(2.229)	(1.089)	(1.619)	
Average school	18.684	3.070*	6.430*	1.218~	1.810~	4,884
		(0.651)	(1.364)	(0.661)	(0.983)	
SHARE SCHOOLS RANKING ABOVE 50TH PERCENTILE [SLAD]						
Most recent school	0.169	0.012	0.025	− 0.014	− 0.020	4,884
		(0.017)	(0.035)	(0.017)	(0.025)	
Average school	0.104	0.019*	0.040*	− 0.003	− 0.004	4,884
		(0.009)	(0.019)	(0.009)	(0.014)	

EXHIBIT 7.3. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SHARE SCHOOLS RANKING ABOVE 75TH PERCENTILE [SLAD]						
Most recent school	0.065	0.016	0.033	0.006	0.009	4,884
		(0.011)	(0.023)	(0.011)	(0.017)	
Average school	0.029	0.014*	0.030*	0.007	0.010	4,884
		(0.005)	(0.011)	(0.005)	(0.007)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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). Average school characteristics are weighted by the amount of time youth spent at each school.
Square brackets indicate the source of the outcome information: CCD = Common Core of Data, PSS = Private School Universe Survey, SLAD = School-level assessment data from the National Longitudinal School-Level State Assessment Score Database.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: In some cases, youth school histories combine self-reports from the long-term survey with parent reports from the interim survey. Long-term survey interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: The number of students measure is based on enrollment as of October. A Title I eligible school is a school designated as being high poverty and eligible to participate in federal Title I programs. Schoolwide Title I programs apply to Title I schools with student bodies that consist of at least 40 percent low-income students. Schools designated by state legislatures as "charter schools" provide free public education under a special charter granted by the legislature. Magnet schools and programs are designed to either attract students of different racial and ethnic backgrounds to reduce racial isolation or to focus on a particular academic or social theme such as science or performing arts.

EXHIBIT 7.4. SCHOOL MOBILITY AMONG YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
Number of schools attended [SR, PR]	4.022	0.072	0.150	0.070	0.105	5,078
		(0.062)	(0.128)	(0.070)	(0.104)	
Number of public school districts attended [SR, PR]	1.540	0.071~	0.147~	0.055	0.082	5,078
		(0.039)	(0.082)	(0.042)	(0.062)	
Most recent school in 5 main districts [SR, PR]	0.640	− 0.031	− 0.064	− 0.018	− 0.027	5,078
		(0.021)	(0.045)	(0.024)	(0.036)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: In some cases, youth school histories combine self-reports from the long-term survey with parent reports from the interim survey. Long-term survey interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: The 5 main districts are Baltimore City Public Schools, Boston Public Schools, Chicago Public Schools, Los Angeles Unified School District, and the New York City Department of Education.

EXHIBIT 7.5. PERCEPTIONS OF SCHOOL CLIMATE AMONG YOUTH AGES 10–17						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
SCHOOL CLIMATE INDEX [SR]						
All	0.797	0.020~	0.043~	− 0.002	− 0.003	3,328
		(0.011)	(0.023)	(0.012)	(0.017)	
Female	0.786	0.025~	0.052~	0.006	0.010	1,694
		(0.015)	(0.032)	(0.016)	(0.025)	
Male	0.807	0.016	0.034	− 0.011	− 0.015	1,634
		(0.014)	(0.031)	(0.016)	(0.022)	
STUDENTS GET TEASED IF THEY STUDY HARD [SR]						
All	0.294	− 0.016	− 0.034	− 0.001	− 0.002	3,323
		(0.021)	(0.045)	(0.023)	(0.033)	
Female	0.296	− 0.011	− 0.023	− 0.048	− 0.072	1,692
		(0.030)	(0.063)	(0.031)	(0.047)	
Male	0.292	− 0.022	− 0.047	0.047	0.065	1,631
		(0.030)	(0.064)	(0.034)	(0.047)	
DISCIPLINE IN SCHOOL IS FAIR [SR]						
All	0.758	0.017	0.037	− 0.032	− 0.046	3,316
		(0.020)	(0.043)	(0.023)	(0.034)	
Female	0.740	0.027	0.055	− 0.019	− 0.028	1,685
		(0.028)	(0.058)	(0.032)	(0.049)	
Male	0.775	0.008	0.018	− 0.046	− 0.063	1,631
		(0.028)	(0.061)	(0.032)	(0.044)	
OFTEN FEELS PUT DOWN BY TEACHERS [SR]						
All	0.129	− 0.031*	− 0.065*	− 0.005	− 0.007	3,327
		(0.015)	(0.031)	(0.017)	(0.025)	
Female	0.124	− 0.033	− 0.067	0.005	0.008	1,693
		(0.021)	(0.044)	(0.024)	(0.036)	
Male	0.133	− 0.029	− 0.062	− 0.015	− 0.021	1,634
		(0.020)	(0.043)	(0.024)	(0.033)	
FEELS SAFE IN SCHOOL [SR]						
All	0.831	− 0.001	− 0.003	− 0.001	− 0.001	3,325
		(0.017)	(0.037)	(0.020)	(0.028)	
Female	0.808	0.007	0.015	0.013	0.020	1,694
		(0.026)	(0.053)	(0.028)	(0.043)	
Male	0.853	− 0.010	− 0.021	− 0.015	− 0.020	1,631
		(0.023)	(0.049)	(0.025)	(0.035)	
TEACHERS INTERESTED IN STUDENTS [SR]						
All	0.816	0.040*	0.084*	0.015	0.022	3,325
		(0.017)	(0.036)	(0.019)	(0.027)	

EXHIBIT 7.5. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	_N_
TEACHERS INTERESTED IN STUDENTS [SR] (CONTINUED)						
Female	0.801	0.051*	0.105*	– 0.004	– 0.006	1,692
		(0.025)	(0.051)	(0.028)	(0.043)	
Male	0.831	0.029	0.062	0.036	0.049	1,633
		(0.023)	(0.050)	(0.025)	(0.034)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 17 as of December 31, 2007.
Measures: The school climate index is the fraction of positive responses on the five individual school climate items presented in this exhibit. 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.

7.6 LONG-TERM IMPACTS ON ACHIEVEMENT AND EDUCATION OUTCOMES

Exhibit 7.6 shows that MTO had no detectable effects on math or reading achievement test scores among youth aged 13 to 20 at the end of 2007.[15] Recall that we have converted our achievement test score results into "Z-scores," which come from subtracting the control group's average test score from each child's individual test score result, and 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.

The null result on achievement test scores holds even for the subset of children who were less than 6 years old at the time of study enrollment—those who had been hypothesized to benefit the most from moves to lower poverty neighborhoods (see Supplemental Exhibit 7.5 for subgroup analyses by baseline age).[16] In Supplemental Exhibit 7.6, we see some signs of a positive effect on test

scores (at least for reading) for the Section 8 group in the Chicago site and a negative effect in New York City but overall few signs of any consistent site differences in program effects—unlike with the interim follow-up data, where both Baltimore and Chicago seemed to show consistent large, positive effects on achievement test scores (Burdick-Will et al., 2011).

MTO also has few detectable effects on youth self-reports of their own schooling performance, as shown in Exhibit 7.7. Experimental and control group youth were similar in terms of reporting they were educationally on track; however, male youth in the Section 8 group appear less likely to be on track than their male counterparts in the control group. Experimental youth appear less likely to have a diploma or to get Bs or higher in school compared with control group youth (both of these differences are only marginally significant with p<.10), and experimental youth were also less likely to take advanced coursework as compared with the control group. And Section 8 youth, especially males, are less likely to take advanced coursework and the SAT or ACT. However, female youth in the Section 8 group are substantially less likely to have repeated a grade than females in the control group.

Approximately one-quarter of youth ages 15 to 20 in the control group were enrolled in some type of post-secondary institution since 2007; and, this is similar

15 Similarly, we do not detect any significant effects on reading or math for the smaller sample of youth ages 10-12 who took the ECLS 5th grade test.

16 The standard errors on these estimates are quite large. A 95 percent confidence interval allows us to rule out a beneficial (or detrimental) impact any larger than about .08 to .09 standard deviations for the either the experimental or Section 8 groups. Analyses of reading and math achievement measured as proficiency probabilities at varying cut-points also show no statistically detectable effect.

across the MTO treatment groups. MTO generally had few detectable effects on post-secondary school enrollment and attendance for youth in our long-term follow up (Exhibit 7.8). Male youth in the Section 8 group appear to be less likely to attend college than those in control group.

Exhibit 7.9 shows that female grown children in the section 8 group were somewhat more likely to graduate from high school than the control group. But female grown children in both the experimental and Section 8 groups were less likely than controls to enroll in college.

EXHIBIT 7.6. ACHIEVEMENT ASSESSMENT RESULTS FOR YOUTH AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
READING ASSESSMENT SCORE [ECLS-K]						
All	0.000	0.003	0.006	0.040	0.058	4,432
		(0.041)	(0.085)	(0.044)	(0.064)	
Female	0.051	− 0.020	− 0.040	0.054	0.081	2,286
		(0.055)	(0.110)	(0.061)	(0.093)	
Male	− 0.050	0.027	0.058	0.025	0.035	2,146
		(0.056)	(0.118)	(0.058)	(0.081)	
MATH ASSESSMENT SCORE [ECLS-K]						
All	0.000	− 0.025	− 0.052	0.000	0.000	4,420
		(0.044)	(0.090)	(0.048)	(0.069)	
Female	− 0.004	− 0.034	− 0.069	− 0.033	− 0.050	2,280
		(0.055)	(0.112)	(0.061)	(0.093)	
Male	0.004	− 0.016	− 0.034	0.034	0.047	2,140
		(0.060)	(0.128)	(0.067)	(0.093)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: ECLS-K = achievement assessment from the Early Childhood Longitudinal Study-Kindergarten cohort study.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: 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.

EXHIBIT 7.7. SCHOOLING OUTCOMES FOR YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EDUCATIONALLY ON-TRACK [SR]						
All (ages 15–20)	0.814	– 0.014	– 0.028	– 0.029	– 0.044	3,614
		(0.018)	(0.037)	(0.021)	(0.031)	
Female	0.827	– 0.007	– 0.015	0.008	0.012	1,842
		(0.023)	(0.046)	(0.026)	(0.040)	
Male	0.801	– 0.019	– 0.041	– 0.066*	– 0.096*	1,772
		(0.026)	(0.055)	(0.029)	(0.042)	
HIGHEST K-12 GRADE ENROLLED [SR]						
All (ages 10–20)	10.482	– 0.053	– 0.109	– 0.006	– 0.009	5,087
		(0.039)	(0.081)	(0.042)	(0.064)	
Female	10.533	– 0.091	– 0.184	0.018	0.028	2,592
		(0.059)	(0.118)	(0.064)	(0.101)	
Male	10.435	– 0.012	– 0.027	– 0.033	– 0.047	2,495
		(0.048)	(0.103)	(0.053)	(0.076)	
HIGH SCHOOL DIPLOMA [SR]						
All (ages 19–20)	0.622	– 0.073~	– 0.141~	– 0.056	– 0.092	1,125
		(0.038)	(0.073)	(0.042)	(0.068)	
Female	0.708	– 0.087~	– 0.169~	– 0.054	– 0.089	5,76
		(0.048)	(0.094)	(0.054)	(0.088)	
Male	0.536	– 0.058	– 0.110	– 0.058	– 0.094	549
		(0.058)	(0.109)	(0.062)	(0.102)	
MOSTLY BS OR HIGHER [SR]						
All (ages 10–20)	0.438	– 0.035~	– 0.073~	0.002	0.002	5,066
		(0.019)	(0.039)	(0.021)	(0.031)	
Female	0.502	– 0.041	– 0.082	– 0.010	– 0.015	2,582
		(0.027)	(0.054)	(0.029)	(0.045)	
Male	0.379	– 0.030	– 0.064	0.013	0.018	2,484
		(0.026)	(0.055)	(0.029)	(0.042)	
ADVANCED COURSEWORK [SR]						
All (ages 10–20)	0.889	– 0.027*	– 0.056*	– 0.041*	– 0.062*	5,092
		(0.012)	(0.025)	(0.014)	(0.021)	
Female	0.887	– 0.028~	– 0.056~	– 0.034~	– 0.053	2,596
		(0.016)	(0.033)	(0.019)	(0.029)	
Male	0.891	– 0.026	– 0.055	– 0.049*	– 0.070*	2,496
		(0.017)	(0.036)	(0.019)	(0.028)	
EVER RECEIVED SPECIAL EDUCATION SERVICES [SR]						
All (ages 10–20)	0.153	0.007	0.015	– 0.006	– 0.009	5,088
		(0.014)	(0.028)	(0.015)	(0.023)	
Female	0.115	0.028~	0.057~	0.013	0.020	2,592
		(0.017)	(0.034)	(0.019)	(0.030)	

	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EVER RECEIVED SPECIAL EDUCATION SERVICES [SR] (CONTINUED)						
Male	0.189	– 0.014	– 0.031	– 0.025	– 0.036	2,496
		(0.020)	(0.043)	(0.022)	(0.031)	
EVER REPEATED A GRADE [SR]						
All (ages 10–20)	0.377	0.000	0.000	– 0.048*	– 0.072*	5,096
		(0.018)	(0.038)	(0.020)	(0.030)	
Female	0.349	– 0.029	– 0.059	– 0.085*	– 0.133*	2,598
		(0.025)	(0.050)	(0.026)	(0.041)	
Male	0.405	0.029	0.063	– 0.011	– 0.016	2,498
		(0.026)	(0.056)	(0.028)	(0.041)	
TOOK SAT/ACT [SR]						
All (ages 15–20)	0.518	– 0.031	– 0.065	– 0.040~	– 0.060~	3,599
		(0.022)	(0.046)	(0.024)	(0.036)	
Female	0.569	– 0.049	– 0.098	– 0.024	– 0.037	1,833
		(0.030)	(0.061)	(0.034)	(0.052)	
Male	0.470	– 0.013	– 0.028	– 0.056~	– 0.082~	1,766
		(0.032)	(0.069)	(0.033)	(0.048)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Youth long-term survey. All interviewed youth.

Measures: On-track youth are those who were currently in school or received a high school diploma or GED. The highest grade enrolled measure was top-coded at 12th grade. The Mostly Bs or Higher measure reflects the overall grades that the youth received in their most recent school year (youth in post-secondary school reported on their most recent year of high school). Advanced coursework that the youth reported ever having taken an advanced math or AP class or having been enrolled in a program for the gifted and talented. The special education services measure is based on a report of whether the youth had ever been enrolled in any special class or received special help for any emotional, physical or mental condition. Grade repetition is the proportion of youth who reported that they had ever repeated a grade, including kindergarten.

EXHIBIT 7.8. POST-SECONDARY OUTCOMES FROM NATIONAL STUDENT CLEARINGHOUSE DATA FOR YOUTH AGES 15–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SINCE 2007, ATTENDED…						
ANY COLLEGE [NSC]						
All	0.262	– 0.014 (0.016)	– 0.029 (0.033)	– 0.021 (0.017)	– 0.033 (0.025)	4,717
Female	0.305	– 0.011 (0.023)	– 0.023 (0.048)	0.000 (0.025)	0.001 (0.039)	2,300
Male	0.222	– 0.016 (0.021)	– 0.033 (0.044)	– 0.042* (0.021)	– 0.064* (0.032)	2,417
TWO-YEAR COLLEGE [NSC]						
All	0.184	– 0.015 (0.014)	– 0.032 (0.029)	– 0.015 (0.015)	– 0.023 (0.022)	4,717
Female	0.205	– 0.017 (0.021)	– 0.036 (0.042)	0.011 (0.022)	0.017 (0.034)	2,300
Male	0.165	– 0.013 (0.018)	– 0.027 (0.039)	– 0.041* (0.019)	– 0.061* (0.028)	2,417
FOUR-YEAR COLLEGE [NSC]						
All	0.105	0.004 (0.011)	0.009 (0.024)	– 0.004 (0.012)	– 0.007 (0.018)	4,717
Female	0.130	0.015 (0.018)	0.031 (0.037)	0.005 (0.019)	0.008 (0.029)	2,300
Male	0.083	– 0.006 (0.013)	– 0.013 (0.029)	– 0.013 (0.014)	– 0.020 (0.021)	2,417
PRIVATE COLLEGE [NSC]						
All	0.048	0.000 (0.008)	– 0.001 (0.016)	0.003 (0.008)	0.005 (0.013)	4,717
Female	0.060	0.000 (0.012)	– 0.001 (0.025)	0.012 (0.014)	0.018 (0.021)	2,300
Male	0.036	0.000 (0.009)	0.000 (0.019)	– 0.005 (0.009)	– 0.008 (0.014)	2,417
PUBLIC COLLEGE [NSC]						
All	0.225	– 0.013 (0.015)	– 0.028 (0.032)	– 0.022 (0.016)	– 0.033 (0.024)	4,717
Female	0.261	– 0.013 (0.022)	– 0.026 (0.046)	– 0.004 (0.024)	– 0.006 (0.037)	2,300
Male	0.192	– 0.014 (0.020)	– 0.029 (0.042)	– 0.040* (0.020)	– 0.059* (0.030)	2,417
FOR-PROFIT COLLEGE [IES]						
All	0.011	0.004 (0.004)	0.008 (0.009)	0.006 (0.005)	0.009 (0.007)	4,717

EXHIBIT 7.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SINCE 2007, ATTENDED… (CONTINUED)						
FOR-PROFIT COLLEGE [IES] (CONTINUED)						
Female	0.014	0.003	0.006	0.003	0.004	2,300
		(0.007)	(0.014)	(0.007)	(0.011)	
Male	0.008	0.005	0.010	0.009	0.014	2,417
		(0.005)	(0.010)	(0.006)	(0.009)	
SINCE 2007, NUMBER OF MONTHS ENROLLED IN….						
TWO-YEAR COLLEGES [NSC]						
All	1.267	0.103	0.216	− 0.066	− 0.100	4,717
		(0.142)	(0.299)	(0.137)	(0.208)	
Female	1.374	0.246	0.509	0.185	0.284	2,300
		(0.218)	(0.451)	(0.213)	(0.326)	
Male	1.169	− 0.031	− 0.066	− 0.304~	− 0.457~	2,417
		(0.183)	(0.392)	(0.173)	(0.260)	
FOUR-YEAR COLLEGES [NSC]						
All	1.182	0.007	0.014	− 0.235	− 0.357	4,717
		(0.158)	(0.333)	(0.152)	(0.230)	
Female	1.564	− 0.019	− 0.039	− 0.268	− 0.411	2,300
		(0.258)	(0.532)	(0.252)	(0.385)	
Male	0.833	0.031	0.066	− 0.204	− 0.306	2,417
		(0.176)	(0.378)	(0.170)	(0.255)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: NSC = National Student Clearinghouse. IES = Institute for Education Sciences.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: The sample for the NSC and IES measures is all core household members ages 15 to 20 as of December 31, 2007.
Measures: Number of days enrolled in college was calculated according to enrollment begin date and enrollment end date reported by NSC, and the number of days was then divided by 30 to create number of months enrolled in school.

EXHIBIT 7.9. SCHOOLING OUTCOMES FOR GROWN CHILDREN AGES 21–30						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
CURRENT ENROLLMENT AND SECONDARY SCHOOLING OUTCOMES						
CURRENTLY ENROLLED IN SCHOOL [PR]						
All	0.158	– 0.007 (0.016)	– 0.016 (0.036)	– 0.001 (0.023)	– 0.002 (0.042)	3,089
Female	0.205	– 0.015 (0.025)	– 0.033 (0.054)	0.006 (0.036)	0.010 (0.062)	1,527
Male	0.110	0.000 (0.020)	0.000 (0.047)	– 0.007 (0.025)	– 0.013 (0.047)	1,562
HAS HIGH SCHOOL DIPLOMA [PR]						
All	0.554	– 0.013 (0.024)	– 0.029 (0.054)	0.007 (0.031)	0.013 (0.055)	3,088
Female	0.624	– 0.004 (0.031)	– 0.008 (0.068)	0.002 (0.042)	0.003 (0.072)	1,530
Male	0.482	– 0.022 (0.032)	– 0.052 (0.076)	0.011 (0.039)	0.022 (0.074)	1,558
HAS HIGH SCHOOL DIPLOMA OR GED [PR]						
All	0.707	– 0.016 (0.022)	– 0.036 (0.050)	0.055* (0.027)	0.099* (0.049)	3,088
Female	0.751	– 0.001 (0.028)	– 0.003 (0.061)	0.069~ (0.036)	0.118~ (0.061)	1,530
Male	0.662	– 0.030 (0.031)	– 0.071 (0.073)	0.042 (0.038)	0.078 (0.071)	1,558
POST-SECONDARY SCHOOLING OUTCOMES						
SINCE 2007, ATTENDED…						
ANY COLLEGE [NSC]						
All	0.196	– 0.015 (0.014)	– 0.035 (0.034)	– 0.011 (0.016)	– 0.019 (0.027)	4,639
Female	0.282	– 0.046* (0.023)	– 0.106* (0.054)	– 0.052* (0.025)	– 0.089* (0.042)	2,275
Male	0.115	0.014 (0.017)	0.034 (0.040)	0.028 (0.019)	0.048 (0.033)	2,364
TWO-YEAR COLLEGE [NSC]						
All	0.135	– 0.018 (0.012)	– 0.042 (0.029)	– 0.013 (0.014)	– 0.022 (0.024)	4,639

EXHIBIT 7.9. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
POST-SECONDARY SCHOOLING OUTCOMES (CONTINUED)						
SINCE 2007, ATTENDED… (CONTINUED)						
TWO-YEAR COLLEGE [NSC] (CONTINUED)						
Female	0.198	– 0.040*	– 0.094*	– 0.044*	– 0.076*	2,275
		(0.020)	(0.047)	(0.022)	(0.037)	
Male	0.076	0.004	0.008	0.017	0.030	2,364
		(0.014)	(0.033)	(0.015)	(0.027)	
FOUR-YEAR COLLEGE [NSC]						
All	0.083	– 0.003	– 0.007	0.006	0.010	4,639
		(0.010)	(0.023)	(0.011)	(0.019)	
Female	0.121	– 0.020	– 0.047	– 0.010	– 0.017	2,275
		(0.016)	(0.037)	(0.018)	(0.031)	
Male	0.047	0.013	0.032	0.020	0.035	2,364
		(0.011)	(0.027)	(0.013)	(0.023)	
PRIVATE COLLEGE [NSC]						
All	0.053	0.001	0.002	– 0.006	– 0.010	4,639
		(0.008)	(0.019)	(0.009)	(0.015)	
Female	0.074	– 0.008	– 0.018	– 0.021	– 0.035	2,275
		(0.013)	(0.030)	(0.014)	(0.024)	
Male	0.033	0.009	0.022	0.008	0.014	2,364
		(0.010)	(0.023)	(0.011)	(0.019)	
PUBLIC COLLEGE [NSC]						
All	0.153	– 0.010	– 0.025	– 0.004	– 0.006	4,639
		(0.013)	(0.031)	(0.014)	(0.025)	
Female	0.226	– 0.035~	– 0.082~	– 0.033	– 0.057	2,275
		(0.021)	(0.049)	(0.023)	(0.039)	
Male	0.085	0.013	0.032	0.024	0.043	2,364
		(0.015)	(0.036)	(0.016)	(0.029)	
FOR-PROFIT COLLEGE [IES]						
All	0.031	– 0.002	– 0.005	– 0.001	– 0.002	4,639
		(0.006)	(0.015)	(0.007)	(0.013)	
Female	0.039	– 0.005	– 0.012	– 0.005	– 0.008	2,275
		(0.010)	(0.023)	(0.011)	(0.019)	
Male	0.023	0.001	0.003	0.002	0.004	2,364
		(0.008)	(0.019)	(0.009)	(0.016)	

EXHIBIT 7.9. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
POST-SECONDARY SCHOOLING OUTCOMES (CONTINUED)						
SINCE 2007, NUMBER OF MONTHS ENROLLED IN…						
TWO-YEAR COLLEGES [NSC]						
All	1.052	− 0.196	− 0.462	− 0.198	− 0.344	4,639
		(0.128)	(0.302)	(0.129)	(0.224)	
Female	1.535	− 0.275	− 0.639	− 0.396~	− 0.679~	2,275
		(0.221)	(0.515)	(0.219)	(0.375)	
Male	0.592	− 0.122	− 0.293	− 0.012	− 0.022	2,364
		(0.128)	(0.306)	(0.137)	(0.240)	
FOUR-YEAR COLLEGES [NSC]						
All	0.875	0.043	0.100	0.079	0.137	4,639
		(0.133)	(0.314)	(0.146)	(0.253)	
Female	1.275	− 0.108	− 0.252	− 0.023	− 0.039	2,275
		(0.226)	(0.525)	(0.244)	(0.419)	
Male	0.495	0.188	0.450	0.177	0.310	2,364
		(0.148)	(0.354)	(0.166)	(0.290)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report. NSC = National Student Clearinghouse. IES = Institute for Education Sciences.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Parent report measures come from the adult long-term adult survey and are presented for grown children (under age 18 at baseline and ages 21 to 30 as of December 31, 2007) whose parents were interviewed as part of the long-term survey. The sample for the NSC and IES measures is all core household members who are grown children, regardless of whether their parent was interviewed.
Measures: Number of days enrolled in college was calculated according to enrollment begin date and enrollment end date reported by NSC, and the number of days was then divided by 30 to create number of months enrolled in school.

7.7 INTERPRETATION OF EDUCATIONAL RESULTS

The long-term follow-up data examined here found no detectable impacts on achievement test scores in reading or math measured 10 to 15 years after baseline. We also found few statistically significant impacts on other educational outcomes for youth. There is some indication that females in the Section 8 group might be less likely than those in the control group to repeat a grade, but on several other measures such as high school graduation, grades, advanced course taking, and taking the SAT or ACT, there were statistically significant or marginally significant declines in outcomes for youth in one or both of the treatment groups compared with controls. Given the large number of impact estimates examined in this chapter, and the fact that the sign of the impacts are somewhat mixed, the few statistically significant effects presented in this chapter may simply arise from chance.

We also found few statistically significant effects on educational outcomes even for those children who were very young (preschool age) at baseline. This is perhaps surprising because Chapter 2 showed that MTO generated particularly large differences across randomized groups in average neighborhood conditions during the first few years after random assignment—exactly when children who were of preschool age at baseline would have been in what researchers believe to be a particularly "plastic" stage of human development.

These results should not be interpreted as implying that neighborhoods never matter for academic achievement, since school quality may be one especially important mediator through which neighborhoods influence learning and MTO wound up generating very modest differences across randomized groups in the average characteristics of the schools children attended. For example all children across all three MTO groups attended schools that had overwhelmingly majority-minority student bodies, in which about two-thirds were eligible for free or reduced-price lunch and that ranked on average near the 20th to 25th percentile relative to other schools based on the achievement test performance of their students. Part of the issue could be that most children in our study sample stayed within the large urban school districts that served the five MTO demonstration cities. While many of these cities were engaging in urban school reform during our study period, which might have narrowed some of the difference in school quality found between very poor and less poor neighborhoods, the school systems as a whole were still not performing terribly well compared with national norms. It is certainly possible that a mobility intervention that achieved larger impacts on school quality—for example, a program that helped more families move to affluent suburbs—could have more pronounced impacts on educational outcomes. MTO is informative instead about the impact on educational outcomes that neighborhood change can have without much change in school quality.

SUPPLEMENTAL EXHIBIT 7.1. ABSENTEEISM, TARDINESS, OR PROBLEMS AT SCHOOL FOR YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
LATE FOR SCHOOL ONCE OR MORE PER MONTH [SR]						
ALL (AGES 10–20)	0.626	0.000	0.001	− 0.010	− 0.015	5,085
		(0.020)	(0.041)	(0.022)	(0.032)	
Female	0.637	− 0.021	− 0.042	0.000	0.000	2,592
		(0.027)	(0.054)	(0.028)	(0.044)	
Male	0.616	0.022	0.048	− 0.022	− 0.031	2,493
		(0.026)	(0.057)	(0.029)	(0.042)	
ABSENT MORE THAN 5% OF THE SCHOOL YEAR [SR]						
ALL (AGES 10–20)	0.392	− 0.020	− 0.041	0.019	0.028	4,951
		(0.019)	(0.039)	(0.021)	(0.032)	
Female	0.382	− 0.007	− 0.015	0.026	0.041	2,522
		(0.026)	(0.051)	(0.030)	(0.046)	
Male	0.402	− 0.032	− 0.069	0.012	0.017	2,429
		(0.026)	(0.056)	(0.030)	(0.043)	
FRACTION OF SCHOOL YEAR ABSENT FROM SCHOOL [SR]						
ALL (AGES 10–20)	0.060	0.000	0.001	0.001	0.002	4,951
		(0.003)	(0.007)	(0.003)	(0.005)	
Female	0.059	− 0.003	− 0.005	0.001	0.001	2,522
		(0.004)	(0.008)	(0.005)	(0.007)	
Male	0.060	0.003	0.007	0.002	0.002	2,429
		(0.005)	(0.012)	(0.005)	(0.007)	

SUPPLEMENTAL EXHIBIT 7.1. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	N
SCHOOL ASKED SOMEONE TO COME IN TO DISCUSS YOUTH'S SCHOOLWORK OR BEHAVIOR PROBLEMS IN PAST 2 YEARS [PR]						
ALL (AGES 10–17)	0.367	− 0.034	− 0.072	− 0.037	− 0.056	1,963
		(0.030)	(0.063)	(0.038)	(0.057)	
Female	0.309	− 0.036	− 0.078	− 0.014	− 0.022	998
		(0.039)	(0.084)	(0.050)	(0.078)	
Male	0.419	− 0.030	− 0.063	− 0.058	− 0.083	965
		(0.044)	(0.092)	(0.053)	(0.076)	
SUSPENDED OR EXPELLED FROM SCHOOL IN PAST TWO YEARS [SR]						
ALL (AGES 10–17)	0.308	0.021	0.045	0.029	0.042	3,322
		(0.022)	(0.046)	(0.024)	(0.034)	
Female	0.288	− 0.035	− 0.073	− 0.012	− 0.018	1,692
		(0.028)	(0.058)	(0.032)	(0.048)	
Male	0.327	0.078*	0.170*	0.069*	0.095*	1,630
		(0.031)	(0.068)	(0.034)	(0.047)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, PR = parent report.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Self-reported items come from the youth long-term survey, while the parent report measure comes from the adult long-term survey. The sample for the parent report measure is all core household members ages 10 to 17 as of December 31, 2007. The sample for the suspension/expulsion measure is limited to interviewed youth ages 10 to 17 as of December 31, 2007. All other measures use the full interviewed youth sample (ages 10 to 20 as of December 31, 2007).

SUPPLEMENTAL EXHIBIT 7.2. FUTURE EXPECTATIONS OF YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
YOUTH EXPECTS TO AT LEAST…						
COMPLETE TWO YEARS OF COLLEGE [SR]						
All	0.696	– 0.048*	– 0.100*	– 0.025	– 0.037	5,078
		(0.018)	(0.038)	(0.020)	(0.030)	
Female	0.746	– 0.031	– 0.062	– 0.020	– 0.032	2,591
		(0.024)	(0.048)	(0.026)	(0.041)	
Male	0.647	– 0.066*	– 0.143*	– 0.028	– 0.040	2,487
		(0.027)	(0.058)	(0.029)	(0.041)	
FINISH COLLEGE [SR]						
All	0.559	– 0.035~	– 0.074~	– 0.029	– 0.043	5,078
		(0.019)	(0.040)	(0.021)	(0.031)	
Female	0.632	– 0.033	– 0.066	– 0.015	– 0.023	2,591
		(0.026)	(0.052)	(0.028)	(0.045)	
Male	0.490	– 0.038	– 0.082	– 0.043	– 0.061	2,487
		(0.027)	(0.059)	(0.030)	(0.042)	
YOUTH THINKS MOTHER WANTS HIM/HER TO AT LEAST…						
COMPLETE TWO YEARS OF COLLEGE [SR]						
All	0.870	– 0.009	– 0.018	– 0.002	– 0.003	5,039
		(0.013)	(0.026)	(0.014)	(0.021)	
Female	0.897	– 0.008	– 0.016	– 0.014	– 0.022	2,569
		(0.016)	(0.032)	(0.018)	(0.028)	
Male	0.845	– 0.010	– 0.021	0.010	0.014	2,470
		(0.019)	(0.041)	(0.020)	(0.029)	
FINISH COLLEGE [SR]						
All	0.820	– 0.006	– 0.012	0.006	0.009	5,039
		(0.015)	(0.032)	(0.016)	(0.024)	
Female	0.854	– 0.003	– 0.006	– 0.016	– 0.025	2,569
		(0.018)	(0.037)	(0.020)	(0.031)	
Male	0.788	– 0.008	– 0.018	0.029	0.041	2,470
		(0.023)	(0.050)	(0.024)	(0.034)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: Completing two years of college includes a two-year degree. Finishing college indicates the completion of a four- or five-year degree. Youth who reported thinking that they will or that their mother wants them to attain education beyond completing two years of college or finishing college are included with the "complete two years of college" and "finish college" groups, respectively.

SUPPLEMENTAL EXHIBIT 7.3. SCHOOL ENGAGEMENT AND PARENTAL INVOLVEMENT IN SCHOOL						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
SCHOOL ENGAGEMENT						
ALWAYS COMPLETES HOMEWORK [SR]						
All (ages 10–20)	0.478	– 0.005	– 0.010	– 0.014	– 0.020	5,095
		(0.019)	(0.040)	(0.021)	(0.031)	
Female	0.536	0.018	0.036	0.010	0.015	2,596
		(0.026)	(0.052)	(0.028)	(0.044)	
Male	0.424	– 0.027	– 0.058	– 0.036	– 0.052	2,499
		(0.027)	(0.058)	(0.029)	(0.041)	
HOURS PER WEEK SPENT ON HOMEWORK [SR]						
All (ages 10–20)	5.294	0.067	0.140	– 0.166	– 0.248	4,762
		(0.204)	(0.424)	(0.220)	(0.329)	
Female	5.800	– 0.158	– 0.317	– 0.032	– 0.050	2,377
		(0.306)	(0.612)	(0.333)	(0.528)	
Male	4.834	0.291	0.629	– 0.307	– 0.434	2,385
		(0.275)	(0.595)	(0.290)	(0.410)	
HOURS PER WEEK READING [SR]						
All (ages 10–20)	3.234	– 0.027	– 0.056	0.034	0.051	5,093
		(0.199)	(0.415)	(0.233)	(0.348)	
Female	3.915	0.190	0.384	0.209	0.328	2,595
		(0.310)	(0.625)	(0.346)	(0.542)	
Male	2.595	– 0.244	– 0.526	– 0.136	– 0.193	2,498
		(0.243)	(0.524)	(0.298)	(0.426)	
PARENT INVOLVEMENT IN SCHOOL						
ADULT ATTENDED AN EVENT OR A MEETING AT YOUTH'S SCHOOL IN PAST YEAR [PR]						
All (ages 10–17)	0.706	0.026	0.056	0.008	0.012	1,969
		(0.028)	(0.058)	(0.037)	(0.055)	
Female	0.696	0.017	0.036	0.002	0.004	1,003
		(0.039)	(0.082)	(0.051)	(0.079)	
Male	0.715	0.036	0.076	0.013	0.019	9,66
		(0.038)	(0.081)	(0.050)	(0.072)	
ADULT VOLUNTEERED AT YOUTH'S SCHOOL IN PAST YEAR [PR]						
All (ages 10–17)	0.166	– 0.001	– 0.002	0.002	0.003	1,970
		(0.023)	(0.049)	(0.027)	(0.041)	
Female	0.178	– 0.013	– 0.028	– 0.001	– 0.002	1,004
		(0.034)	(0.073)	(0.040)	(0.062)	
Male	0.155	0.012	0.025	0.005	0.008	966
		(0.031)	(0.064)	(0.036)	(0.052)	

SUPPLEMENTAL EXHIBIT 7.3. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
PARENT INVOLVEMENT IN SCHOOL (CONTINUED)						
ADULT IS SOMEWHAT OR VERY SATISFIED (VERSUS NOT AT ALL SATISFIED) WITH YOUTH'S EDUCATION [PR]						
All (ages 10–17)	0.920	− 0.015	− 0.031	− 0.054*	− 0.080*	1,966
		(0.018)	(0.038)	(0.025)	(0.038)	
Female	0.926	0.008	0.018	− 0.044	− 0.068	1,001
		(0.022)	(0.047)	(0.032)	(0.050)	
Male	0.915	− 0.038	− 0.080	− 0.063~	− 0.091~	965
		(0.029)	(0.061)	(0.038)	(0.054)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report, PR = parent report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Self-reported items come from the youth long-term survey, while the parent report measures comes from the long-term adult survey. The sample for the self-reported measures is interviewed youth ages 10 to 20 as of December 31, 2007. The sample for the parent report measures is all core household members ages 10 to 17 as of December 31, 2007.
Measures: The sample size for the weekly homework hours measure is lower than other self-reported measures in this exhibit due to a survey instrument skip error that was corrected in the midst of the interview fielding period. Imputing missing values by randomization site and treatment group and by site, group, and using responses from youth who answered other homework questions similarly to those affected by the skip error yields qualitatively similar results. The weekly reading hours measure refers to reading outside of school that is not in connection with schoolwork.

SUPPLEMENTAL EXHIBIT 7.4. TIME USE AND ACCESS TO TECHNOLOGY AMONG YOUTH AGES 10–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
TIME USE: EXTRACURRICULAR ACTIVITY						
PARTICIPATES IN SCHOOL SPORTS [SR]						
All	0.445	− 0.016	− 0.033	− 0.001	− 0.002	5,088
		(0.019)	(0.039)	(0.021)	(0.031)	
Female	0.374	− 0.034	− 0.068	− 0.015	− 0.024	2,592
		(0.025)	(0.051)	(0.028)	(0.043)	
Male	0.511	0.002	0.004	0.012	0.018	2,496
		(0.028)	(0.059)	(0.030)	(0.043)	
PARTICIPATES IN SCHOOL CLUBS/ GROUPS [SR]						
All	0.278	0.004	0.008	− 0.004	− 0.005	5,048
		(0.017)	(0.035)	(0.019)	(0.028)	
Female	0.321	0.015	0.030	− 0.008	− 0.012	2,573
		(0.024)	(0.049)	(0.026)	(0.041)	
Male	0.239	− 0.007	− 0.015	0.001	0.002	2,475
		(0.023)	(0.049)	(0.025)	(0.036)	

SUPPLEMENTAL EXHIBIT 7.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
TIME USE: EXTRACURRICULAR ACTIVITY (CONTINUED)						
LEADERSHIP POSITION IN SCHOOL SPORTS OR CLUBS/GROUPS [SR]						
All	0.361	− 0.006 (0.018)	− 0.013 (0.038)	0.015 (0.020)	0.022 (0.030)	5,092
Female	0.351	− 0.014 (0.025)	− 0.028 (0.051)	− 0.024 (0.028)	− 0.038 (0.044)	2,596
Male	0.370	0.000 (0.026)	0.001 (0.055)	0.054~ (0.029)	0.077~ (0.042)	2,496
PARTICIPATES IN ORGANIZED ACTIVITY ONCE OR MORE PER WEEK [SR]						
All	0.506	− 0.024 (0.019)	− 0.050 (0.039)	− 0.006 (0.020)	− 0.009 (0.030)	5,093
Female	0.390	− 0.038 (0.025)	− 0.077 (0.050)	− 0.014 (0.027)	− 0.022 (0.042)	2,596
Male	0.614	− 0.010 (0.026)	− 0.021 (0.057)	0.001 (0.029)	0.002 (0.041)	2,497
TIME USE: INFORMAL ACTIVITY						
HANGS OUT IN NEIGHBORHOOD OR AT BASKETBALL COURT ONCE OR MORE PER WEEK [SR]						
All	0.450	− 0.008 (0.019)	− 0.017 (0.039)	− 0.018 (0.020)	− 0.026 (0.030)	5,095
Female	0.327	− 0.017 (0.023)	− 0.034 (0.047)	− 0.038 (0.025)	− 0.060 (0.039)	2,596
Male	0.566	0.000 (0.027)	0.001 (0.059)	0.003 (0.030)	0.004 (0.042)	2,499
SHOPS AT MALL OR STORE ONCE OR MORE PER WEEK [SR]						
All	0.476	− 0.001 (0.019)	− 0.003 (0.040)	0.002 (0.021)	0.003 (0.031)	5,092
Female	0.515	− 0.009 (0.027)	− 0.018 (0.054)	0.014 (0.029)	0.022 (0.046)	2,596
Male	0.439	0.007 (0.027)	0.015 (0.057)	− 0.011 (0.029)	− 0.016 (0.042)	2,496
HANGS OUT AT SOMEONE ELSE'S HOUSE ONCE OR MORE PER WEEK [SR]						
All	0.561	− 0.037~ (0.019)	− 0.077~ (0.039)	− 0.042* (0.020)	− 0.062* (0.030)	5,071

SUPPLEMENTAL EXHIBIT 7.4. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
HANGS OUT AT SOMEONE ELSE'S HOUSE ONCE OR MORE PER WEEK [SR] (CONTINUED)						
Female	0.512	− 0.031	− 0.062	− 0.030	− 0.047	2,586
		(0.027)	(0.054)	(0.028)	(0.044)	
Male	0.606	− 0.042	− 0.092	− 0.053~	− 0.076~	2,485
		(0.026)	(0.056)	(0.029)	(0.041)	
NEVER HANGS OUT AT HOME [SR]						
All	0.077	− 0.004	− 0.008	− 0.007	− 0.010	5,095
		(0.010)	(0.020)	(0.010)	(0.015)	
Female	0.075	− 0.008	− 0.016	0.000	0.000	2,596
		(0.014)	(0.028)	(0.015)	(0.024)	
Male	0.079	0.000	0.001	− 0.014	− 0.020	2,499
		(0.014)	(0.031)	(0.015)	(0.021)	
ACCESS TO TECHNOLOGY						
USES COMPUTER AT HOME [SR]						
All	0.704	− 0.009	− 0.019	0.049*	0.073*	5,095
		(0.020)	(0.041)	(0.020)	(0.030)	
Female	0.720	− 0.027	− 0.055	0.029	0.045	2,596
		(0.024)	(0.048)	(0.026)	(0.041)	
Male	0.688	0.009	0.019	0.068*	0.097*	2,499
		(0.026)	(0.056)	(0.027)	(0.039)	
USES INTERNET AT LEAST 4 HOURS PER WEEK [SR]						
All	0.555	0.001	0.001	− 0.014	− 0.021	3,949
		(0.021)	(0.043)	(0.024)	(0.035)	
Female	0.571	− 0.020	− 0.039	− 0.017	− 0.027	2,028
		(0.029)	(0.055)	(0.031)	(0.049)	
Male	0.540	0.022	0.047	− 0.012	− 0.017	1,921
		(0.030)	(0.064)	(0.033)	(0.047)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR = self-report.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 10 to 20 as of December 31, 2007.
Measures: The sample size for the weekly hours of internet use measure is lower than other self-reported measures in this exhibit due to a survey instrument skip error that was corrected in the midst of the interview fielding period. Imputing missing values by randomization site and treatment group yields qualitatively similar results.

SUPPLEMENTAL EXHIBIT 7.5. ACHIEVEMENT ASSESSMENT RESULTS BY BASELINE AGE AMONG YOUTH AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
BASELINE AGES 0–5						
READING ASSESSMENT SCORE [ECLS-K]						
All	– 0.092	0.026	0.057	0.080	0.108	2,542
		(0.054)	(0.115)	(0.056)	(0.076)	
Female	– 0.031	0.021	0.045	0.087	0.122	1,309
		(0.074)	(0.156)	(0.081)	(0.113)	
Male	– 0.152	0.032	0.069	0.072	0.094	1,233
		(0.072)	(0.158)	(0.073)	(0.095)	
MATH ASSESSMENT SCORE [ECLS-K]						
All	0.001	– 0.057	– 0.121	– 0.060	– 0.080	2,535
		(0.058)	(0.123)	(0.060)	(0.081)	
Female	0.019	– 0.067	– 0.141	– 0.066	– 0.092	1,306
		(0.077)	(0.162)	(0.079)	(0.111)	
Male	– 0.017	– 0.046	– 0.101	– 0.054	– 0.069	1,229
		(0.078)	(0.171)	(0.086)	(0.112)	
BASELINE AGES 6–11						
READING ASSESSMENT SCORE [ECLS-K]						
All	0.119	– 0.026	– 0.050	– 0.008	– 0.013	1,890
		(0.058)	(0.113)	(0.065)	(0.105)	
Female	0.160	– 0.061	– 0.118	0.017	0.029	977
		(0.076)	(0.147)	(0.092)	(0.155)	
Male	0.081	0.014	0.028	– 0.036	– 0.055	913
		(0.083)	(0.167)	(0.090)	(0.138)	
MATH ASSESSMENT SCORE [ECLS-K]						
All	– 0.001	0.017	0.034	0.081	0.129	1,885
		(0.060)	(0.119)	(0.070)	(0.112)	
Female	– 0.035	0.014	0.026	0.024	0.042	974
		(0.072)	(0.140)	(0.089)	(0.152)	
Male	0.032	0.019	0.038	0.138	0.209	911
		(0.090)	(0.182)	(0.103)	(0.155)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: ECLS-K = achievement assessment from the Early Childhood Longitudinal Study-Kindergarten cohort study.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: 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.

SUPPLEMENTAL EXHIBIT 7.6. ACHIEVEMENT ASSESSMENT RESULTS BY SITE AMONG YOUTH AGES 13–20						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
READING ASSESSMENT SCORE [ECLS-K]						
All	0.000	0.003	0.006	0.040	0.058	4,432
		(0.041)	(0.085)	(0.044)	(0.064)	
Baltimore	− 0.020	− 0.025	− 0.043	− 0.071	− 0.093	589
		(0.108)	(0.183)	(0.112)	(0.147)	
Boston	0.107	− 0.081	− 0.206	0.128	0.225	878
		(0.099)	(0.253)	(0.095)	(0.167)	
Chicago	− 0.281	0.074	0.256	0.237*	0.330*	974
		(0.088)	(0.302)	(0.099)	(0.138)	
Los Angeles	− 0.057	0.090	0.136	0.050	0.061	1,069
		(0.078)	(0.119)	(0.088)	(0.107)	
New York City	0.266	− 0.074	− 0.147	− 0.188*	− 0.335*	922
		(0.085)	(0.170)	(0.096)	(0.170)	
MATH ASSESSMENT SCORE [ECLS-K]						
All	0.000	− 0.025	− 0.052	0.000	0.000	4,420
		(0.044)	(0.090)	(0.048)	(0.069)	
Baltimore	− 0.084	− 0.164	− 0.276	− 0.111	− 0.144	591
		(0.105)	(0.177)	(0.116)	(0.151)	
Boston	0.204	− 0.126	− 0.321	0.040	0.070	875
		(0.114)	(0.291)	(0.108)	(0.187)	
Chicago	− 0.314	0.028	0.095	0.137	0.191	973
		(0.101)	(0.349)	(0.125)	(0.174)	
Los Angeles	− 0.061	0.080	0.122	0.044	0.053	1,063
		(0.076)	(0.116)	(0.088)	(0.107)	
New York City	0.262	− 0.021	− 0.042	− 0.166~	− 0.296~	918
		(0.084)	(0.169)	(0.088)	(0.157)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: ECLS-K = achievement assessment from the Early Childhood Longitudinal Study-Kindergarten cohort study.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: 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.

SUPPLEMENTAL EXHIBIT 7.7. INTERVIEWER OBSERVATIONS OF YOUTH AGES 13–20 DURING ACHIEVEMENT ASSESSMENT						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
HIGHLY OR VERY HIGHLY MOTIVATED DURING ASSESSMENT [OBS]						
All	0.840	– 0.019	– 0.039	0.019	0.028	4,582
		(0.015)	(0.032)	(0.015)	(0.022)	
Female	0.851	– 0.008	– 0.017	0.019	0.030	2,335
		(0.019)	(0.039)	(0.020)	(0.031)	
Male	0.830	– 0.029	– 0.063	0.019	0.027	2,247
		(0.022)	(0.047)	(0.022)	(0.031)	
COOPERATIVE OR VERY COOPERATIVE DURING ASSESSMENT [OBS]						
All	0.914	– 0.001	– 0.003	0.008	0.012	4,580
		(0.011)	(0.022)	(0.012)	(0.018)	
Female	0.921	0.005	0.010	0.021	0.033	2,335
		(0.014)	(0.028)	(0.014)	(0.022)	
Male	0.908	– 0.008	– 0.017	– 0.006	– 0.008	2,245
		(0.016)	(0.035)	(0.019)	(0.026)	
VERY OR FULLY ATTENTIVE DURING ASSESSMENT [OBS]						
All	0.811	0.019	0.039	0.041*	0.060*	4,578
		(0.016)	(0.033)	(0.016)	(0.024)	
Female	0.826	0.024	0.048	0.031	0.047	2,334
		(0.020)	(0.040)	(0.022)	(0.033)	
Male	0.797	0.014	0.030	0.051*	0.072*	2,244
		(0.022)	(0.048)	(0.024)	(0.033)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: OBS = interviewer observations.
Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.
Data source and sample: Youth long-term survey. Interviewed youth ages 13 to 20 as of December 31, 2007.
Measures: For interviewer observations of motivation, high and very high motivation were two of five choices (the other three were average, low, and very low). For cooperation, cooperative and very cooperative were two of five choices (the other three were matter of fact, uncooperative, and very uncooperative). For attention, very attentive and complete and full attention were two of five choices (the other three were attentive, difficulty attending, and unable to attend).

SUPPLEMENTAL EXHIBIT 7.8. POST-SECONDARY SCHOOLING OUTCOMES OF YOUTH AGES 15–20, SELF-REPORT VERSUS ADMINISTRATIVE DATA						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
ANY COLLEGE						
EVER ATTENDED ANY COLLEGE [SR]						
All (ages 15–20)	0.243	0.011	0.022	– 0.009	– 0.014	3,610
		(0.019)	(0.040)	(0.021)	(0.032)	
Female	0.271	0.023	0.047	0.011	0.017	1,838
		(0.026)	(0.053)	(0.031)	(0.048)	
Male	0.218	– 0.002	– 0.004	– 0.029	– 0.042	1,772
		(0.026)	(0.056)	(0.027)	(0.039)	
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)	
EVER ATTENDED ANY COLLEGE [SR] OR ATTENDED ANY COLLEGE SINCE 2007 [NSC], OVERLAPPING SAMPLE						
All	0.355	– 0.022	– 0.044	– 0.019	– 0.028	3,611
		(0.019)	(0.038)	(0.021)	(0.031)	
Female	0.408	– 0.025	– 0.051	– 0.001	– 0.002	1,841
		(0.027)	(0.054)	(0.031)	(0.047)	
Male	0.302	– 0.018	– 0.037	– 0.037	– 0.053	1,770
		(0.026)	(0.053)	(0.027)	(0.039)	
FOUR-YEAR COLLEGE						
EVER ATTENDED FOUR-YEAR COLLEGE [SR]						
All (ages 15–20)	0.111	0.003	0.006	– 0.023	– 0.035	3,610
		(0.014)	(0.030)	(0.016)	(0.023)	
Female	0.130	0.012	0.024	– 0.017	– 0.026	1,838
		(0.021)	(0.042)	(0.024)	(0.037)	
Male	0.094	– 0.006	– 0.013	– 0.029	– 0.042	1,772
		(0.019)	(0.040)	(0.020)	(0.029)	
ATTENDED FOUR-YEAR COLLEGE SINCE 2007 [NSC]						
All	0.105	0.004	0.009	– 0.004	– 0.007	4,717
		(0.011)	(0.024)	(0.012)	(0.018)	
Female	0.130	0.015	0.031	0.005	0.008	2,300
		(0.018)	(0.037)	(0.019)	(0.029)	

SUPPLEMENTAL EXHIBIT 7.8. (CONTINUED)						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
FOUR-YEAR COLLEGE (CONTINUED)						
ATTENDED FOUR-YEAR COLLEGE SINCE 2007 [NSC] (CONTINUED)						
Male	0.083	– 0.006	– 0.013	– 0.013	– 0.020	2,417
		(0.013)	(0.029)	(0.014)	(0.021)	
EVER ATTENDED FOUR-YEAR COLLEGE [SR] OR ATTENDED FOUR-YEAR COLLEGE SINCE 2007 [NSC], OVERLAPPING SAMPLE						
All	0.148	– 0.001	– 0.002	– 0.013	– 0.020	3,611
		(0.015)	(0.031)	(0.017)	(0.025)	
Female	0.181	0.002	0.003	– 0.012	– 0.018	1,841
		(0.022)	(0.045)	(0.025)	(0.039)	
Male	0.115	– 0.004	– 0.008	– 0.015	– 0.021	1,770
		(0.019)	(0.040)	(0.020)	(0.029)	

Notes: * = p < 0.05, ~ = p < 0.10 on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: SR= self-report, NSC = National Student Clearinghouse.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Self-reported measures come from the youth long-term survey and are presented for interviewed youth ages 15 to 20 as of December 31, 2007. The sample for the NSC and IES measures is all core household members ages 15 to 20 as of December 31, 2007, regardless of whether they were interviewed.

Measures: Number of days enrolled in college was calculated according to enrollment begin date and enrollment end date reported by NSC, and the number of days was then divided by 30 to create number of months enrolled in school.

SUPPLEMENTAL EXHIBIT 7.9. POST-SECONDARY SCHOOLING OUTCOMES OF GROWN CHILDREN AGES 21–30, SELF-REPORT VERSUS ADMINISTRATIVE DATA						
	CONTROL MEAN	EXPERIMENTAL VERSUS CONTROL		SECTION 8 VERSUS CONTROL		
OUTCOME		ITT	TOT	ITT	TOT	*N*
EVER ENROLLED IN COLLEGE [PR]						
All	0.399	− 0.039~	− 0.089~	− 0.020	− 0.035	3,081
		(0.022)	(0.051)	(0.029)	(0.052)	
Female	0.484	− 0.034	− 0.075	− 0.016	− 0.027	1,524
		(0.031)	(0.068)	(0.041)	(0.071)	
Male	0.311	− 0.044	− 0.104	− 0.024	− 0.045	1,557
		(0.030)	(0.072)	(0.036)	(0.067)	
ATTENDED ANY COLLEGE SINCE 2007 [NSC]						
All	0.196	− 0.015	− 0.035	− 0.011	− 0.019	4,639
		(0.014)	(0.034)	(0.016)	(0.027)	
Female	0.282	− 0.046*	− 0.106*	− 0.052*	− 0.089*	2,275
		(0.023)	(0.054)	(0.025)	(0.042)	
Male	0.115	0.014	0.034	0.028	0.048	2,364
		(0.017)	(0.040)	(0.019)	(0.033)	
EVER ENROLLED IN COLLEGE [PR] OR ATTENDED ANY COLLEGE SINCE 2007 [NSC], OVERLAPPING SAMPLE						
All	0.415	− 0.033	− 0.076	− 0.006	-0.010	3,216
		(0.021)	(0.047)	(0.028)	(0.049)	
Female	0.522	− 0.043	-0.095	− 0.028	-0.048	1,570
		(0.029)	(0.065)	(0.039)	(0.066)	
Male	0.312	-0.024	− 0.056	0.015	0.028	1,646
		(0.028)	(0.065)	(0.035)	(0.065)	

Notes: * = $p < 0.05$, ~ = $p < 0.10$ on two-tailed t-test. Robust standard errors shown in parentheses. ITT = Intention-to-Treat or estimated impact of being offered an MTO housing voucher; TOT = Treatment-on-the-Treated or estimated impact of moving using an MTO housing voucher. The control mean is unadjusted. Unless otherwise indicated, the control mean and impacts 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: PR = parent report, NSC = National Student Clearinghouse.

Model: Experimental and Section 8 impacts 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. Youth and grown children impacts by gender were estimated as an interaction with treatment status. See Section 1.3 and the technical appendices (forthcoming) for details.

Data source and sample: Parent report measures come from the adult long-term survey and are presented for grown children (under age 18 at baseline and ages 21 to 30 as of December 31, 2007) whose parents were interviewed as part of the long-term survey. The sample for the NSC and IES measures is all core household members who are grown children, regardless of whether their parent was interviewed.

Measures: Number of days enrolled in college was calculated according to enrollment begin date and enrollment end date reported by NSC, and the number of days was then divided by 30 to create number of months enrolled in school.

CHAPTER 8

SUMMARY AND IMPLICATIONS

In this chapter, we summarize the pattern of MTO impacts 10 to 15 years after random assignment and discuss their implications both for social science and for public policies designed to improve the well-being of low-income families.

MTO provided very disadvantaged families living in public housing developments in high-poverty neighborhoods a chance to move to a better life. We find that the offer of an MTO housing voucher, which enables families to move to lower-poverty neighborhoods, generated sustained reductions over the next 10 to 15 years in neighborhood (census tract) poverty rates for families in both treatment groups (Section 8 and the experimental group) relative to families in the control group. The differences in neighborhood conditions between the MTO treatment (experimental and Section 8) groups and the control group narrowed over time, in large part because of residential mobility and ongoing declines in the average poverty rates in the census tracts of families in the control group. MTO moves also generated sustained improvements in housing conditions and in other neighborhood attributes beyond economic segregation, such as safety; the latter is particularly important given that concern for safety was the most important reason families signed up for MTO.

Neighborhood improvements induced by MTO moves boosted mental and physical health much more than labor market outcomes, educational outcomes, and for the most part more than for risky and criminal behavior as well. Moreover, the health effects in MTO seem more pronounced for adult women than for youth. The MTO study sample was very disadvantaged at baseline, and so our results may not be fully applicable to more representative samples of families. But our results are certainly consistent with the idea that some of the variation in health outcomes across neighborhoods in current-day America is caused by neighborhood

conditions themselves. On the other hand, the cause of low earnings and high rates of unemployment, criminal involvement, and high school dropout found in many disadvantaged urban neighborhoods may rest more with individual barriers to success in school, the labor market and other pro-social settings than to purely neighborhood-level sources of disadvantage, at least within the range of neighborhood variation that mobility programs like MTO are able to modify. Accordingly, interventions that directly and effectively address individual-level barriers remain essential components in strategies to improve the long-term life chances of low-income families in addition to any housing mobility and neighborhood-based policies.

8.1 SUMMARY OF IMPACT ESTIMATES

Chapters 2 through 7 of this report presented impact estimates on the following outcomes for adults and youth:

- Residential mobility, housing conditions, neighborhood environments, and social networks;
- Physical health;
- Mental health;
- Employment, earnings, income, receipt of social assistance, and savings and assets;
- Risky and criminal behavior; and
- Educational outcomes.

A Technical Appendix will provide additional technical details about our data collection and estimation procedures, as well as the results of sensitivity analyses.

8.2 MOBILITY, HOUSING, NEIGHBORHOODS, AND SOCIAL NETWORKS

MTO provided help to families in the experimental group to move out of public housing into private-market housing located in low-poverty census tracts (those with 1990 poverty rates below 10 percent).

To understand the consequences of such moves for the subsequent socioeconomic and health outcomes of these families, we compared their experiences with those of the families in the control group. The MTO demonstration also allowed us to analyze the impacts of moving using a standard Section 8 voucher through analogous comparisons of the Section 8 group and the control group.

Among those assigned to the experimental group in the long-term survey sample, 47 percent moved with an MTO housing voucher (the "compliers"), while 63 percent of families in the Section 8 group moved with an MTO housing voucher. Readers might be surprised that so few families took advantage of what would initially seem like such an appealing opportunity to move out of public housing, and conclude that there must be something wrong with how MTO was implemented. But the voucher use rate in MTO is in line with what other studies have found. The voucher "take-up" or "compliance" rate in the Experimental Housing Allowance Program was approximately 65 percent (Leger and Kennedy, 1990), and it was approximately 20 percent in the Gautreaux mobility program in Chicago (Rubinowitz and Rosenbaum, 2000, p. 67).

Assignment to either the experimental or Section 8 groups increased the number of moves families made compared with controls, but a perhaps surprising finding is that mobility rates for the control group were lower than might be expected on the basis of previous studies of low-income households who were not receiving government housing assistance. Over the 10 to 15-year study period, the average control group family moved about twice compared with about three total moves for families who relocated as part of the MTO experimental or Section 8 groups.

MTO moves clearly improved the quality of the housing units in which families were living. Experimental group adults were about 5 percentage points more likely than controls to rate their housing at the time of the long-term survey as good or excellent (the effects of treatment on the treated [TOT] was approximately 11 percentage points), a sizable change compared with the control group prevalence rate of 57 percent. MTO movers in

both the experimental and Section 8 groups were also less likely than controls to report specific housing problems, such as vermin, broken windows or missing screens, plumbing that is not working, or peeling paint and plaster.

MTO moves did not seem to affect overall housing costs for families, although, as discussed below, the confidence interval for this estimate was fairly wide. Compared with control group families, those in the experimental group were less likely to have problems paying their rent on time, but they were more likely to have trouble paying utilities. Being in the Section 8 treatment group increased the likelihood of renting and reduced the likelihood of homeownership; the experimental treatment effects were qualitatively similar but not statistically significant.

Among families who moved as part of the experimental group, MTO was clearly successful in helping them experience substantially different neighborhood environments compared with controls, at least in the short term. The results presented in Exhibit 1.5 of Chapter 1 showed that at baseline, the average MTO family was living in a census tract in which 53 percent of all residents were poor. Among those in the experimental and Section 8 groups who moved with an MTO voucher (the "compliers"), the average tract poverty rates decreased to 11 and 29 percent respectively after the initial move through the MTO program.

Over time, these poverty differences narrow substantially. At the time of the long-term follow-up surveys, the difference in average census-tract poverty rates between experimental group compliers and control group families had declined from around 35 or 40 percentage points to about 10 percentage points. Overall the duration-weighted average census-tract poverty rate for the control group over the entire 10- to 15-year study period was about 40 percent. This compares with 20 percent for experimental group compliers and 29 percent for Section 8 group compliers.

The convergence in neighborhood poverty rates over time occurred in part because some experimental group compliers moved after their first year in a low-poverty

area to neighborhoods with somewhat higher poverty rates. A majority of the convergence since interim, however, occurred because of declines over time in the control group's average census-tract poverty rate, which is itself due mostly to the relocation of control group families over time into lower-poverty areas (rather than from control families living in neighborhoods that were gentrifying around them).

A different approach to thinking about the size of the MTO impact on neighborhood poverty is to ask how far MTO shifted families within the national distribution of neighborhood poverty rates. For example, Quigley and Raphael (2008) note that at the time of the interim MTO study, the impact for all experimental group families (regardless of whether they moved in conjunction with the program) amounted to a shift from the 96th percentile of the nationwide census tract poverty distribution to the 88th percentile. Of course, the effect of MTO on those who actually move via the program will be larger. But perhaps more important, and less obvious, is the fact that there are just not that many census tracts in the United States with extremely high poverty rates of the sort in which MTO families are living (as Figure 4 in Quigley and Raphael, 2008, p. 22, makes clear). This means very large changes in the absolute tract poverty level can lead to modest changes in a family's rank in the nationwide tract poverty distribution.

An alternative way to think about the relative magnitudes of MTO's impacts on tract poverty rates within the nationwide tract-poverty distribution, which is less sensitive to the fact that there are relatively few areas with extremely high poverty rates, is in terms of standard deviations within the national Census 2000 distribution for tract poverty rates. Experimental group moves generate decreases in tract poverty rates equal to nearly 3 standard deviations measured one year after random assignment, 0.8 standard deviations measured 10 to 12 years after random assignment, and 1.5 standard deviations when averaged over the entire post-study period (results from analyses not shown in tables). Not many policy interventions induce changes of this size in the social environments of families.

The presumption in MTO was that the census tract poverty rate is in part an indicator of the constellation of living conditions that may be important to the well-being of low-income families. Consistent with this presumption, we found that MTO also generated large, persistent changes in a range of other neighborhood attributes, including exposure to more affluent neighbors, less physical and social disorder, and perceptions of greater safety. Despite concerns about the risk of social isolation for MTO families in lower-poverty areas, moves increased the likelihood that families were friends with someone who had a college degree.

Although MTO moves also led to statistically significant declines in neighborhood racial composition, the experimental group compliers were still living in census tracts in which the large majority of all residents were members of racial and ethnic minority groups. This is one important way in which the MTO "treatment" is different from that of the widely cited Gautreaux mobility program. The latter program was more explicitly focused on moves to reduce residential racial segregation (Rubinowitz and Rosenbaum, 2000). Another difference is that Gautreaux was explicitly designed to help families move into affluent suburban areas, which MTO achieved at a much lower rate.

8.3 PHYSICAL HEALTH

A large theoretical and empirical literature dating back to the 1700s has hypothesized that neighborhood environments may influence physical health outcomes through exposure to environmental risk factors, social norms about health-related behaviors, and access to health care providers. The interim MTO study found that, four to seven years after random assignment, MTO moves reduced the likelihood that adults were obese (as defined by a body mass index [BMI] of 30 kg/m2 or higher). MTO had no detectable impacts on most other measures of physical health collected as part of the interim study. Given the potential importance of physical health impacts for both MTO participants themselves and society as a whole, the long-term study included a detailed battery of health outcomes, including directly measured height and weight (rather than self-reported, as in the interim study), blood pressure, and blood samples that were assayed for "biomeasures" that can be used to diagnose disease or predict future risk.

The results showed that 10 to 15 years after random assignment, there was no longer a statistically significant impact on obesity (BMI ≥ 30 kg/m2), perhaps because such a large share of the sample became obese during that interval (58 percent of control group families). However, assignment to either the experimental or Section 8 groups reduced the chances of more extreme obesity (BMI ≥ 35 kg/m2) by around 5 percentage points compared with a control group prevalence of around 35 percent, and reduced the chances of BMI ≥ 40 kg/m2 by around 3 percentage points (although not statistically significant for the Section 8 group) compared with a control group prevalence of about 18 percent.

Alongside these sizable differences in extreme obesity, MTO also generates declines in diabetes prevalence of up to 6 percentage points for the experimental group using our preferred measures from blood assays of glycosylated hemoglobin (HbA1c), effects that were somewhat smaller if we rely instead on self-reports of diabetes status or treatment. These impacts are sizable compared with the control group prevalence rate of 16 to 20 percent. We also found that experimental group moves may have reduced the likelihood of having high-risk levels (>4 mg/L) of high-sensitivity C-reactive protein (a measure of inflammation), which have been shown to be predictive of cardiovascular disease risk.

There were few impacts on most other physical health measures for adults, with the exception of self-reported functional limitations (e.g., difficulty climbing stairs or carrying groceries). MTO had few significant impacts on the physical health outcomes of youth in the participating families. We discuss below possible ways in which these health impacts may have arisen.

8.4 MENTAL HEALTH

Given the importance of safety in motivating families to sign up for the demonstration as well as MTO's impacts on neighborhood safety, mental health might be expected to be one of the outcome domains most improved by MTO.

The long-term follow-up found sustained MTO impacts on a widely used index of psychological distress (Kessler's "K6" index). The long-term data showed that K6 scores were about 0.1 standard deviations lower for adults assigned to the experimental group than to the control group. This is about the same size as found in the interim MTO study (Kling, Liebman, Katz, 2007, Table 3, p. 92). Given the importance of mental health for the well-being of MTO program participants, the long-term surveys included more detailed mental health measures. We found that those in the experimental group had a 3 percentage point reduction and those in the Section 8 group a 5 percentage point reduction (marginally significant) in lifetime prevalence of major depression among adults compared with a control group prevalence of 20 percent. Although MTO had no statistically significant effects on other specific disorders, almost all of the impact estimates are in the direction of improved mental health. One exception is that MTO moves may have increased rates of substance abuse or dependence.

Female youth (ages 15–20 at the time of the long-term surveys) in the experimental group had improved mental health (as measured by the mental health index) compared to controls. The changes were as large in magnitude as adult improvements. For female youth ages 13–20 in the experimental group, MTO also reduced serious behavioral or emotional problems, as measured by the commonly used "strengths and difficulties questionnaire" (SDQ); reduced psychological distress; and appears to have reduced the prevalence of panic attacks (past year). Male youth in the Section 8 group may have experienced an increased prevalence of post-traumatic stress disorder relative to controls. This general pattern of generally beneficial impacts on female youth and some detrimental impacts on male youth echoes the pattern found in the interim report (Kling, Ludwig and Katz 2005, Kling, Liebman and Katz, 2007).

8.5 ECONOMIC SELF-SUFFICIENCY

The MTO program began in the mid-1990s, a time when the economy was booming and welfare reform was increasing the number of single mothers who were working in the formal labor market. Employment rates of mothers in both treatment groups and the control group increased sharply as well from the mid-1990s to 2000. The increases, however, were no higher for either the experimental or Section 8 groups than for the control group. In fact, previous studies of MTO found that the

first few years after random assignment, the experimental and Section 8 group adults had somewhat lower average earnings than controls, presumably from the disruptions and transitions associated with moving (Orr et al., 2003).

For the long-term follow-up, we had hypothesized that MTO moves might increase employment rates and earnings more for experimental than control mothers for a variety of reasons. The disruptions associated with moving would presumably have subsided. Additional time in lower-poverty neighborhoods might have increased the chances that movers had made new social connections that could serve as sources of job referrals, or would have internalized or responded to social norms in low-poverty areas that were more supportive of work. The children in these households were mostly adolescents by the long-term survey, thus limiting child-care constraints. We also speculated that the improved mental and physical health outcomes documented in the interim report and in the long-term survey could eventually translate into improved labor market outcomes.

None of these hypotheses were evident in the long-term data. The initial negative effect of MTO moves on labor market outcomes attenuated over time, but there were no statistically significant gains in longer-term employment rates, earnings, or total family income as measured by either survey self-reports or administrative records. We found some signs of negative effects of the Section 8 treatment on self-reported employment rates in our long-term survey data, but we suspect this is likely an artifact of the slight differences in timing between when the surveys were carried out for the Section 8 versus control groups during the Great Recession in 2008–2009.

For the youth in MTO households, moves had no detectable effects on idleness rates but seem to have had, if anything, adverse effects on employment rates and earnings. These youth were age 15–20 at the time of the long-term survey. For youth who were teenagers at the time of the interim study and were grown children at the long-term study, males in the experimental group were more likely than controls to be employed, according to the proxy reports by their parents. However, state unemployment insurance (UI) data showed they were less likely to be employed. MTO may have increased

employment rates in jobs that are not covered by the UI system, but it is also possible these are chance findings.

8.6 RISKY AND CRIMINAL BEHAVIOR

We had anticipated that MTO might have even more pronounced impacts on the risky and criminal behavior of those who were adolescents (age 15–20) at the time of the long-term follow-up (10–15 years after random assignment) compared to those who were age 15–20 at the interim study (4–7 years after baseline). The interim study found declines in violent-crime arrests for both male and female youth who moved through MTO relative to controls, but for most other forms of risky behavior found that male youth did worse as a result of moving through MTO while female youth did better. The effects of MTO might have been even stronger for those who were teenagers at the time of the long-term study (compared to those who were teens at interim) because they would have been exposed for a longer period of time to less dangerous and economically distressed neighborhood environments. Moreover those who were teens at the time of the long-term follow-up would have lived in less disadvantaged communities early in childhood – a life stage in which cognitive, socio-emotional and behavioral skills have been hypothesized by many social scientists to be particularly amenable to social policy intervention.

However the estimated effects of MTO on risky and criminal behavior of those who were teens at the long-term follow-up were more muted compared to what the effects on teens were at the time of the interim study. We find no statistically significant effects of MTO on violent crime arrests in the long term data. The long term data provide a few indications of the same type of gender difference in youth responses to MTO moves as were found in the interim data, with male youth who moved through MTO engaging in relatively more of some risky behaviors (smoking) compared to controls and female youth who moved through MTO experiencing declines in some risky behaviors (drinking) compared to controls.

The fact that the effects of MTO on risky and criminal behavior are generally more muted in the long-term data compared to the interim data suggests that contemporaneous neighborhood environments

may matter more for this outcome domain than does accumulated exposure to less distressed areas. The fact that we see signs of a gender difference in responses to residential mobility in a sample of teens in the long term data that is mostly non-overlapping with the youth who were studied in the interim follow-up, and also in an entirely different set of public housing families examined in a Chicago study by Jacob, Ludwig and Miller (2011), suggests that the gender difference in MTO impacts documented at interim may not be just a statistical artifact unique to that particular study and sample.

The one outcome for which we do see at least some hints of more pronounced impacts in the long-term data than in the interim data is with declining arrest rates for drug distribution among the MTO treatment groups compared to controls. The results are large and statistically significant for male youth in the experimental group, somewhat sizable for both the experimental and Section 8 effects for male grown children (but only significant for the Section 8 group), and for adults is sensitive to whether we focus on the extensive versus intensive margin of involvement in drug selling. The stronger results for adolescents than for older people is consistent with the widely-documented "age-crime curve" showing that aggregate arrest rates for most crimes peak during late adolescence or early adulthood. Evidence of impacts on all age groups for drug distribution, even if the results are somewhat mixed for the older cohorts, would itself seem to provide some support for the idea that this might be a real effect and not a chance finding.

8.7 EDUCATIONAL OUTCOMES

As with the interim study, MTO had few statistically significant effects on educational outcomes in the long-term follow up. Disappointingly, the null results also extended to children who had not yet enrolled in school at baseline, who would have experienced particularly large MTO-induced changes in neighborhood environments during a life stage in which cognitive, social-emotional and behavioral skills are hypothesized by many social scientists to be unusually "plastic" and susceptible to intervention.

Compared to the control group, children assigned to the experimental and Section 8 groups had similar scores on

reading and math achievement tests, grades in school, and rates of grade retention. However, Section 8 males were less likely to be educationally on track and to have attended college than control group males. There was no evidence that impacts on educational outcomes varied systematically with a child's age at time of random assignment.

These findings do not mean that neighborhood environments never matter for educational outcomes. The MTO mobility intervention generated more pronounced impacts on neighborhood conditions than on school conditions. As with the interim follow-up, the long term data show that MTO had modest and mixed impacts on school quality. Children assigned to the treatment groups rather than to the control group were in schools that had slightly lower proportions of low-income and minority students, and those assigned to the experimental group were in schools that had slightly higher test scores, but were also larger in terms of the total size of the student body. A majority of MTO children were still attending majority-minority, overwhelmingly low-income public schools located in the districts serving the five original MTO cities.

The potential contingent nature of neighborhood effects on educational outcomes also comes from considering the pattern of MTO impacts across cities and over time from the interim to the long-term follow-up. Data from the interim study showed some signs of MTO impacts on achievement test scores for children in the Baltimore and Chicago sites, the cities where baseline levels of both concentrated neighborhood disadvantage and neighborhood crime and violence were more extreme than in the other three MTO cities (Boston, Los Angeles, and New York City). That this might be a real effect rather than a spurious finding is suggested by evidence of neighborhood effects on test scores drawn from two entirely different Chicago study samples (see Burdick-Will et al., 2011). Over time from the interim to the long-term study in MTO, the neighborhood conditions in which Baltimore and Chicago families lived became more similar to what we see in the other three cities, and any indication of cross-site differences in MTO impacts on test scores attenuates. This pattern seems consistent with our results for the risky and criminal

behavior chapter in suggesting that contemporaneous neighborhood conditions may be more important than accumulated exposure in affecting outcomes in these two domains.

8.8 CAN THESE MTO RESULTS BE TRUSTED?

Although we have sought to maximize the scientific quality of our long-term MTO follow-up, it remains possible that we have missed some important MTO impacts.

One potential problem with any study such as ours is sample attrition. If, for example, the most successful MTO families could not be tracked or refused to be interviewed, then our estimates likely understate MTO's benefits. We do not believe systematic differences across randomized MTO groups in attrition from the study sample are a major concern. Thanks to the efforts of the University of Michigan's Survey Research Center (SRC), the effective response rates for the long-term MTO follow-up were 90 percent for adults and 89 percent for youth. Moreover, the response rates were quite similar across randomized groups. High response rates such as these limit the degree to which any differences between respondents and nonrespondents can affect our estimates. Moreover, the characteristics (measured when the study began) of those who responded to the long-term survey appear to be quite similar across randomized groups.

Another potential threat comes from survey misreporting. Of particular concern is the possibility that living in a more versus less disadvantaged neighborhood could affect survey misreporting patterns, if, for example, people were more embarrassed to admit being unemployed in more affluent areas where relatively more people are working. We have tried to limit this problem by using multiple sources of data. For example, we directly measured health outcomes whenever possible (such as height, weight, blood pressure, and even using blood spots to measure health outcomes like diabetes) rather than relying only on self-reports. We also used administrative government records along with survey measures of labor market outcomes. The impacts as measured across these various sources of data are generally consistent.

A third threat to the MTO study is the possibility of "interference" between program participants, called the "stable unit treatment value assumption" (SUTVA) (Rubin, 1980; see also Sobel [2006] for a discussion within the context of MTO). In MTO, this problem could arise if the neighbors of MTO participants were offered MTO housing vouchers, or if there were other MTO participants living in their destination neighborhoods. The basic concern is that MTO participation by other people in the community could modify the impacts of MTO participation on a given family, so that the effects of MTO moves might depend on the share of other families in the community that also participate in the program. If this were the case, then our estimates would be relevant only for other mobility programs with similar types of interactions among residents and compliance rates.

Although this assumption cannot be tested directly, some suggestive evidence argues against its practical importance in the MTO context. First, most families that signed up for MTO were fairly socially isolated at baseline: fully 41 percent of household heads reported that they had no friends in the neighborhood, and 65 percent reported that they had no family in the neighborhood. Given that around one-quarter of eligible families signed up for MTO, and some public housing residents would not have been eligible (for example, because they did not have children), social interactions among MTO families may have been limited. Second, the MTO intervention deliberately aimed to avoid concentrating MTO families in new neighborhoods (as can be seen in exhibits C2.2–C2.6 in Orr et al., 2003). In future work, we plan to explore this issue more by taking advantage of exact address information that included apartment numbers for families at baseline. These addresses will allow us to examine whether MTO's impacts vary according to how many other families in nearby units were also offered MTO vouchers.

Perhaps the most serious scientific challenge with this study concerns its statistical power, which, of course, is an issue with almost any feasible social experiment. All of the statistical tests presented in this report examine whether the 95 percent confidence intervals for our estimate of MTO's impact on some outcome include zero

or not. When our confidence interval includes zero, we cannot reject the null hypothesis that there is no effect, but that does not mean the effect is actually zero. There are many estimated impacts that are not statistically significant and where we can be confident that MTO does not have large effects. However, we sometimes cannot rule out moderately-sized impacts. The size of the effect that we can rule out varies across outcomes depending on the statistical properties of the outcome variable itself. For example, for employment rates as measured by administrative unemployment insurance (UI) records (Exhibit 5.5), our 95 percent confidence interval ranges from –10 percent of the control mean employment rate to +10 percent of the control mean. The 95 percent confidence interval of our estimated effect of MTO on housing costs ranged from around –10 percent up to +20 percent of the control mean. It is important for readers to keep in mind that an effect that is "not statistically significant" does not mean its effect is necessarily zero.

8.9 IMPLICATIONS FOR SOCIAL SCIENCE AND PUBLIC POLICY

In this section, we address the following social science and policy questions:

- What are the mechanisms through which MTO improves long-run health outcomes, and perhaps some measures of risky and criminal behavior as well?
- Why does MTO not have more pronounced effects on labor market or educational outcomes?
- What do the MTO results imply for the scientific study of "neighborhood effects"?
- What do the MTO results imply for housing policy in particular and social policy in general?

WHAT ARE THE MECHANISMS THROUGH WHICH MTO IMPROVES HEALTH OUTCOMES AND PERHAPS RISKY AND CRIMINAL BEHAVIOR AS WELL?

MTO's evaluation design provides a strong platform for estimating the causal effects of moving as part of either the experimental or Section 8 groups. Determining why MTO affects outcomes is more challenging because assignment to one of the treatment groups changed

such a wide range of housing and neighborhood attributes for families. One useful step for assessing the potential mechanisms of action behind MTO's impacts is to examine those outcomes where MTO did generate statistically significant impacts, and consider which potentially relevant measures of candidate mediating mechanisms were or were not also changed by MTO. Unchanged measures of a candidate mediating mechanism would indicate that this causal pathway was unlikely to be the active ingredient in changing outcomes.

For example, one striking feature of the results is that MTO seemed to have improved many aspects of health without generating detectable changes in many of the most obvious mediating pathways. For adults the MTO experimental treatment reduced diabetes, extreme obesity prevalence, and the risk of having elevated rates of C-reactive protein (a predictor of cardiovascular disease risk). Yet the experimental treatment did not have detectable impacts on diet (recognizing that our measures are limited for those health behaviors), or on the likelihood of having health insurance, or on other measures of access to medical care, and had at best mixed impacts on our measures of physical exercise.

It is plausible that MTO improved health by reducing psychological and social stress. The baseline MTO surveys make clear that concerns about safety were the most common motivation for signing up for MTO and moving to a new neighborhood. Our follow-up surveys show that MTO moves succeeded in improving perceptions of neighborhood safety. Medical research has shown that stress may affect metabolism and other physiological systems (Kuo et al., 2008; Merkin et al., 2009), even independent of the effects of stress on diet, such as the consumption of "comfort foods" that are high in fat and sugar. This stress pathway would also be consistent with our findings of beneficial MTO impacts on a measure of general psychological distress (as measured by the "K6" index) and on the prevalence of major depression.

Learning more about the mediating mechanisms of impacts on health is an important goal for future research. One way to push the experimental data is to

examine whether the patterns of MTO health impacts are relatively larger in those sites where assignment to the treatment group generates relatively larger impacts on health or related outcomes, which is the quasi-experimental approach applied by Kling, Liebman and Katz (2007) using the interim data.

We also see some hints in the long-term follow-up data that the MTO intervention may have reduced drug selling among male youth, male grown children, and perhaps adults (household heads) as well. Drug selling—at least outdoor drug selling of the sort that is most vulnerable to law enforcement discovery—may be even more geographically concentrated across neighborhoods than other types of crime in part because of differences in the capacity of community residents to control public spaces through informal social control or by marshaling public sector resources. In addition, selling regularly from the same location—usually in economically disadvantaged areas—is a way to help solve the market coordination problem of buyers and sellers finding one another, given that formal advertising is not possible in this market (see for example Cook et al., 2007). Why we should see more pronounced signs of MTO effects on drug selling in the long-term data compared to the interim data is not entirely clear, but could be related to the large increase in marijuana use among young people that occurred over the MTO study period.

The long-term follow-up of MTO participants also produced some muted echoes of the same gender difference in how youth respond to mobility interventions as was found in the interim study (see Kling, Ludwig, and Katz, 2005; Kling, Liebman, and Katz, 2007). The survey and administrative data collected for the interim study, together with subsequent qualitative work, raised several hypotheses for this gender difference. For example, previous work raised the possibility that male youth might spend more of their time hanging out in public spaces than do female youth, which could put them at elevated risk for getting into trouble or into conflict with community residents. Male youth who move through MTO might also have more difficulty than do female youth in navigating their new neighborhood environments, making friends, or staying connected to father figures (Clampet-Lundquist

et al., 2011). Compared to females, male youth may also experience a larger decline in their relative academic standing as a result of moving through MTO, and have more of a comparative advantage in anti-social behavior as a means of securing status or resources (Kling, Ludwig, and Katz, 2005). While the long-term MTO surveys were designed to help test most of these candidate hypotheses, we have not yet found the "smoking gun" to definitively explain why male and female youth seem to respond differently to residential mobility interventions like MTO.

WHY DID MTO FAIL TO INFLUENCE LABOR MARKET AND EDUCATIONAL OUTCOMES?

Given the large body of previous (nonexperimental) research suggesting that neighborhood environments might play a role in affecting labor market and educational outcomes, it is puzzling that our long-term follow-up found few detectable beneficial impacts. One possible reason is that MTO seems to have had only modest impacts on some of the mechanisms through which neighborhood environments are thought to affect these outcome domains.

For example, much of the previous research on labor market outcomes has focused on a "mismatch" in geographic proximity to jobs among low-income individuals. This theory argues that one reason employment rates are so low for many urban minority workers is because racial discrimination prevents them from relocating to suburbs and other locations where job opportunities are becoming more plentiful. Yet MTO moves did not seem to have very large impacts on these mediating mechanisms. Even at the interim study, there was little impact of moves on local job availability as measured by employment growth by residential ZIP code (Kling, Liebman and Katz, 2007, p. 99). From a social science perspective, one implication is that the MTO experiment provides a weak test of the spatial mismatch hypothesis (Kain, 1968). Yet the fact that MTO provides a weak test of the spatial mismatch idea is itself informative. Because MTO is at least as intensive as any residential mobility program that could be implemented at large scale, it suggests that mobility programs might not be the most promising way to overcome any spatial

mismatch problems and improve access to jobs for disadvantaged urban workers.

MTO did seem to have some effect on social connections to people who could be good sources of job referrals, but the effect of those connections on labor market outcomes is unclear. Chapter 2 showed that moving via the experimental group increased the likelihood of having at least one close friend who graduated from college by nearly 15 percentage points (nearly one-third of the control group mean of 53 percent), and increased the likelihood of having at least one close friend who worked full-time by 7 percentage points (around 10 percent of the control mean of 74 percent; p < .10). Yet in the interim survey data, only a very modest share of MTO participants reported that they found their jobs "through someone living in their neighborhood such as a friend, relative or acquaintance" (Kling, Liebman, and Katz, 2007). Some of the qualitative work with MTO families also suggests that neighbors may not have had information about the types of jobs that would be most relevant for MTO program participants (Turney et al., 2006).

With respect to educational outcomes, many parents at least 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. Yet MTO had more modest impacts on school quality compared to the intervention's impacts on other neighborhood social conditions, consistent with the trend of many urban school systems to provide families with more "public school choice" that enables them to attend schools outside their local neighborhoods (see for example Cullen, Jacob, and Levitt, 2006). Children assigned to the MTO treatment groups wound up attending 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 to the student bodies of the control group's schools. But children in the MTO treatment groups still wound up attending schools that were very low performing and served overwhelmingly poor and majority-minority student populations.

Another question is why the mental and physical health impacts in both the interim and long-term follow-ups did not translate into improved labor market outcomes, given the connection between health and employment, or in children's schooling outcomes, given that improved mental health for parents should presumably translate into more developmentally productive parenting practices. The answer may be that while MTO created changes in mental and physical health that are large enough to be important for policy, they are not large enough by themselves to lead to impacts on educational or labor market outcomes that are sizable enough to be detected in the MTO data.[1]

In sum, the challenges that MTO participants face in schools and in the labor market do not appear to have been caused by the kinds of neighborhood-level risk factors that a typical MTO move could alleviate. Perhaps these problems arise from individual- or family-level risk factors, or perhaps they are caused by neighborhood conditions that MTO did not address (for example, concentrations of minority neighbors or school quality). At any rate, these findings suggest that housing mobility programs should be supplemented by more direct individual- and family-level interventions to generate substantial improvements in schooling, work and earnings.

WHAT DO THE RESULTS IMPLY ABOUT THE EXISTENCE OF NEIGHBORHOOD EFFECTS?

Because MTO is the first true randomized experiment that has been carried out to test the effects of residential mobility on low-income families, the study is often looked to as a key source of information about the effects of neighborhood environments on families ("neighborhood effects"). It is important to keep in

---

1 For example, Ettner, Frank, and Kessler (1997) estimated that mental health disorders reduced labor force participation rates by between 14 and 34 percentage points for adult women. The effect of moving via the experimental group on the lifetime prevalence of any mental health disorder is around 5 percentage points, which is not quite statistically significant (p value of .22). But even if we were confident that this were a "real" 5 percentage point impact, not a statistical fluke, a 5 percentage point change in prevalence of mental health disorders would imply an MTO impact on labor force participation rates of no more than about 2 percentage points (5 percent times 34 percent). Our estimates for MTO's impacts on labor force participation would not enable us to rule out an impact of about this size (see Exhibit 5.7).

mind that the MTO "treatment" combines the effects of moving per se, changes in housing quality, and neighborhood conditions, which in principle make it difficult to disentangle the effects of these different changes. In practice, we suspect that the effects of moving per se may not be such an important determinant of differences in outcomes across MTO groups since the effects of the extra moves that treatment-group families make are likely to dissipate over time. Moreover, the control group itself on average moved twice over this time period as well, so most families in MTO in all three groups have experienced the disruptions of moving several times during the study period.

If we do not believe that differences in move rates across MTO groups are a key determinant of differences in outcomes, to what degree are the impacts that we find owing to changes in housing conditions versus neighborhood conditions? Some suggestive evidence that our results might be driven by changes in neighborhoods rather than housing quality comes from the analysis by Kling, Liebman and Katz (2007) of the interim data. They find that the pattern across MTO demonstration sites and randomized treatment groups in effects on adult mental health is strongly related to the pattern across sites and groups in changes in tract poverty rates specifically, much more so than to the site-group pattern in MTO impacts on the likelihood of changing housing units with a housing voucher.

Also relevant in thinking about what we can learn about neighborhood effects from MTO is that the study enrolled a particularly disadvantaged group of public housing families who volunteered to participate in this program. If the effects of MTO-like moves are different for different types of families, then MTO's impacts may not tell us what would happen if we changed the neighborhoods of other, more representative samples of American families. The possibility that MTO-like moves might have distinctive effects on different types of families also implies that MTO does not necessarily inform us about the effects of involuntary moves across neighborhoods, as occurs when local housing authorities demolish housing projects for example. If families sign up for MTO because they believe they have relatively more to gain from moving, then MTO findings would overstate the effects of an involuntary mobility program.

Finally, some social scientists have reacted with surprise to the fact that MTO has not had more pronounced impacts on outcomes like employment rates or earnings. They have argued that MTO is not a fair test of the neighborhood effects hypothesis because the demonstration did not generate changes in neighborhood conditions that are "large enough" (Clampet-Lundquist and Massey, 2008). Of course, with any social experiment it is always the case that a more intensive intervention can be imagined. MTO does not have anything to say about, for example, the effects of moving a family from the distressed, crime-ridden South Side Chicago neighborhood of Washington Park into a condominium in Chicago's Gold Coast or a house in the upscale Chicago suburb of Oak Park. But the range of neighborhood variation induced by MTO is about as large as what we could possibly imagine any feasible housing policy achieving. In fact, it is hard to imagine an elected official in contemporary America ever pushing to implement anything on a large scale that was even remotely like the experimental treatment, which required families to move at least initially into a low-poverty census tract. Therefore, we think the proper interpretation of the neighborhood effects question that can be answered by MTO is: What are the effects on the outcomes of this very disadvantaged inner-city sample of public housing families from helping them make moves that change neighborhood conditions by about as much as what we could imagine any actual policy ever achieving in the real world?[2]

WHAT DO THE RESULTS IMPLY FOR HOUSING POLICY IN PARTICULAR AND SOCIAL POLICY GENERALLY?

MTO tells us most about the effects of a modestly scaled, voluntary housing-mobility intervention on the outcomes of low-income families. MTO offered housing vouchers to no more than a few thousand families in five of the nation's largest cities, and so was unlikely to

---

2 It is true that the Gautreaux mobility program in Chicago generated even more dramatic changes in neighborhood conditions than the MTO results (see Rubinowitz and Rosenbaum, 2000). Similarly large changes in neighborhood conditions can be found in a recent Baltimore mobility program as well (see DeLuca and Rosenblatt, undated). But, crucially, both of these mobility programs were initiated at the instigation of judges, rather than elected policymakers. Gautreaux and the Baltimore mobility program also differ from MTO in many ways with respect to their target populations, which is a separate point from the political feasibility for elected officials to push for Gautreaux-like mobility programs.

have had any important effect on the way the overall private housing market operated in these cities. A larger mobility program might have different impacts on families from those estimated in MTO if efforts to move relatively more low-income families cause changes in the overall private housing market and the options available to families, what sociologists often call "system-level effects" and economists call "general-equilibrium effects." A larger mobility program could, for example, increase rents in the private housing market, at least in the short term, or lead to the re-concentration of former public housing families in new neighborhoods.

Some additional calculations show that an MTO-type, small-scale mobility intervention for disadvantaged families may well generate more benefits than costs. Our rough estimate is that the costs of the extra housing assistance and counseling provided to the MTO experimental treatment group might add up to around $800 per treatment-group "complier" per year, as averaged over the 10 to 15 year follow-up study period.[3] On the benefit side of the ledger, it seems possible that MTO's impacts on diabetes and depression alone could be enough to approximately equal the tangible costs of the intervention.[4] Whether MTO passes a benefit-cost test then would depend on whether any intangible costs of the program, in the form of negative spillovers of MTO moves on residents of origin or destination

---

3 The cost of the housing vouchers themselves in the short term are negligible, or even negative, since the alternative is public housing initially for families. Many housing economists believe that public housing might be even more expensive than housing vouchers (Olsen, 2003). Exhibit 2.4 shows that the effect of experimental moves on the likelihood of receiving any housing assistance 10–15 years after baseline was +.05 (not statistically significant). If the average housing subsidy cost for families is around $8,000 per year, then the increased persistence on housing subsidies might cost $400 per year. The other tangible financial cost would be the additional counseling services provided to families, which averaged approximately $4,500 per family (Goering et al., 1999, Table 4, which we assume is 1995 dollars). If we amortize this over 12 years of post-study data, on average, this would be approximately $400 per year per experimental group mover.

4 In Chapter 3, we showed that moving via the experimental group reduced the prevalence of diabetes by nearly 11 percentage points. Previous research suggests that diabetes increases annual medical expenditures by around 240 percent per person, equal to at least $4,500 per year in 2011 dollars (Trogdon and Hyland, 2008). Therefore, an 11 percentage point decline in diabetes prevalence would generate benefits of approximately $500 per year per MTO adult assigned to the experimental group. In Chapter 4, we found that the effect on depression prevalence during the past year is 4.3 percentage points, which is not quite statistically significant. A review by Berto et al. (2000) suggests the total costs of depression in the United States could be on the order of $110 billion per year. If 7 percent of American adults have clinical depression in a given year, this implies $7,500 per case in social costs, so that the estimated experimental TOT effect on depression implies benefits of around $340.

neighborhoods, are outweighed by the intangible benefits to MTO families themselves of improved quality of life from better housing and neighborhood conditions.

The MTO findings suggest that housing mobility programs alone are unlikely to be a panacea for the schooling problems and labor market difficulties faced by disadvantaged families living in public housing projects and other high-poverty, inner-city neighborhoods. Policies to increase skills and directly address other individual barriers to work remain essential if we are to improve the long-term life chances and economic self-sufficiency of disadvantaged families living in high-poverty areas. Several recent random-assignment evaluations indicate that high-quality education, training, and employment services programs can improve schooling outcomes and produce sustained earnings increases for inner-city, disadvantaged youth and adults. 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). The Jobs-Plus demonstration produced sustained (seven-year) earnings gains for adult, nondisabled public housing residents through employment and training services, changes in rent rules to increase work incentives, and neighbor-to-neighbor outreach centering on work (Riccio, 2010). Sectoral employment programs—industry-specific training programs to prepare underskilled workers for skilled positions and connect them with employers—have produced substantial earnings gains for disadvantaged adults in large U.S. cities (Maguire et al., 2010). And high schools organized as Career Academies integrating academic and technical curricula and work-based learning opportunities with local employers produce sizable long-term (11-year) earnings improvements for youth in low-income urban settings (Kemple, 2008).

The MTO results point to the possible value in paying increased attention to community-level interventions for improving health outcomes of disadvantaged families, such as those in MTO, who live in some of our most distressed inner-city areas. In principle, the

effects of a community-level intervention that changed neighborhood environments without moving families could be different from the effects of an MTO-like mobility program that moves families into different types of neighborhoods. This type of difference in the effects of a mobility intervention versus community-level intervention could arise if, for example, moving disrupts important social ties and social support that would otherwise help families take full advantage of changes in neighborhood environments. But as a general rule, poor families in the United States tend to move often.[5] In practice, the effects of an intervention that changed all severely distressed urban neighborhoods in some way might wind up being little different from interventions that helped very disadvantaged families, such as those in MTO, move out of such neighborhoods.

Perhaps the more difficult challenge for drawing inferences about community-level interventions from MTO is the difficulty of determining which aspects of neighborhood environments should be the highest priority targets for such programs. The MTO findings do not suggest that access to health care or opportunities, or neighborhood racial segregation, are the key ways in which community environments influence health outcomes like diabetes, extreme obesity, psychological distress and depression. More likely intervention priorities are neighborhood socioeconomic composition, informal social control as suggested by the "collective efficacy" theory of Sampson, Raudenbush, and Earls (1997), and, perhaps most important, the neighborhood attribute that the MTO families themselves have cared the most about since the beginning—safety.

5 For example, data from the Current Population Survey show that between 2002 and 2003, 14 percent of people changed residences; the figure was 18 percent for African-Americans and Hispanics, and 23 to 24 percent for minorities with incomes below the poverty line (Schachter, 2004).

REFERENCES

Adams, J., and M. White. 2006. Is the disease risk associated with good self-reported health constant across the socio-economic spectrum? *Public Health* 120: 70-75.

Almond, Douglas, and Janet Currie. 2010. Human capital development before age 5. NBER Working Paper no. 15827. Cambridge, MA: National Bureau of Economic Research.

Altman, David G. 1995. Strategies for community health intervention: promises, paradoxes, pitfalls. *Psychosomatic Medicine* 57: 226-33.

American Diabetes Association. 2010. Standards of medical care in diabetes – 2010. *Diabetes Care* 33(1): S11-61.

American Psychiatric Association. 1994. *Diagnostic and statistical manual of mental disorders* (4th ed.). Washington, DC: Author.

Anda, Robert F., Vincent J. Felitti, J. Douglas Bremner, John D. Walker, Charles Whitfield, Bruce D. Perry, Shanta R. Dube, and Wayne H. Giles. 2006. The enduring effects of abuse and related adverse experiences in childhood: A convergence of evidence from neurobiology and epidemiology. *European Archives of Psychiatry and Clinical Neuroscience* 256: 174-86.

Ander, Roseanna, Philip J. Cook, Jonathan Guryan, Jens Ludwig, Harold Pollack, and Laurence Steinberg. 2010. "Preventing Youth Violence." Chicago: University of Chicago Crime Lab.

Anderson, Elijah. 1999. *Code of the Streets: Decency, Violence, and the Moral Life of the Inner City.* New York: W.W. Norton & Company.

Anderson, Robin J. 2011. Dynamics of economic well-being: Poverty, 2004-2006. Current Population Report P70-123. Washington, DC: U.S. Census Bureau.

Anderson Robert N., and Betty L. Smith. Smith. 2003. Deaths: Leading causes for 2001. *National Vital Statistics Reports* 52(9).

Aneshensel, Carol S., and Clea A. Sucoff. 1996. The neighborhood context of adolescent mental health. *Journal of Health and Social Behavior* 37(4): 293-310.

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. 1996. Identification of causal effects using instrumental variables. *Journal of the American Statistical Association* 91(434): 444-55.

Angrist, Joshua D., and Jorn-Steffen Pischke. 2009. *Mostly harmless econometrics.* Princeton, NJ: Princeton University Press.

Anil, Bulent, David L. Sjoquist, and Sally Wallace. 2010. The effect of a program-based housing move on employment: Hope VI in Atlanta. *Southern Economic Journal* 77(1): 138-60.

Arima, Hisatomi, Michiaki Kubo, Koji Yonemoto, Yasufumi Doi, Toshiharu Ninomiya, Yumihiro Tanizaki, Jun Hata, Kiyoshi Matsumura, Mitsuo Lida, and Yutaka Kiyohara. 2008. High-sensitivity C-reactive protein and coronary heart disease in a general population of Japanese: The Hisayama study. *Arteriosclerosis, Thrombosis, and Vascular Biology* 28(7): 1385-91.

Aud, Susan, William Hussar, Grace Kena, Kevin Bianco, Lauren Frohlich, Jana Kemp, Kim Tahan, Thomas Nachazel, and Gretchen Hannes. 2011. "The Condition of Education 2011." U.S Department of Education, National Center for Education Statistics.

Bagley, Christopher, Solomon Jacobson, and Clare Palmer. 1973. Social structure and the ecological distribution of mental illness, suicide, and delinquency. *Psychological Medicine* 3: 177-87.

Baltimore City Public Schools. 2011. "Blueprint for Progress: Report on Results 2009-2010." Accessed August 15, 2011. http://www.bcps.org/SYSTEM/reports/ResultsReport2009-2010.pdf.

Banerjee, Abhijit, and Sendhil Mullainathan. 2010. The shape of temptation: Implications for the lives of the poor. Working paper, Harvard University.

Bayer, Patrick, Steve Ross, and Giorgio Topa. 2008. Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy* 116(6): 1150-96.

Beaghan, Michael, and Sharon Stern. 2009. Usability of the American Community Survey Estimates of the Group Quarters Population for Substate Geographies. Washington, DC: U.S. Census Bureau.

Beatty, Alexandra. 2010. "Committee on the Impact of Mobility and Change on the Lives of Young Children, Schools, and Neighborhoods." Prepared for the National Research Council and Institute of Medicine. The National Academies Press, Washington, D.C.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76(2): 169-217.

Becker, Gary S., and Kevin M. Murphy. 2000. *Social economics: Market behavior in a social environment.* Cambridge, MA: Belknap Press.

Bell, Janice, Jeffrey S. Wilson, and Gilbert C. Liu. 2008. Neighborhood greenness and 2-year changes in body mass index of children and youth. *American Journal of Preventative Medicine* 35(6): 547-53.

Bellair, Paul E. 2000. "Informal Surveillance and Street Crime: A Complex Relationship." *Criminology* 38: 137-170.

Berkman, Lisa F., and Lester Breslow. 1983. *Health and ways of living.* New York: Oxford University Press.

Berkman, Lisa F., and Thomas Glass. 2000. Social integration, social networks, social support and health. In *Social Epidemiology*. 137-173. New York: Oxford University Press.

Berto, Patrizia, Daniele D'Ilario, Pierfrancesco Ruffo, Roberto Di Virgilio, and Fortunata Rizzo. 2000. Depression: Cost-of-illness studies in the international literature, a review. *Journal of Mental Health Policy and Economics* 3: 3-10.

Beyers, Jennifer M., Rolf Loeber, Per-Olof H. Wikstrom, Magda Stouthamer-Loeber. 2001. "What predicts adolescent violence in better-off neighborhoods?" *Journal of Abnormal Child Psychology.* 29(5): 369-81.

Bickel, Gary, Mark Nord, Cristofer Price, William Hamilton, and John Cook. 2000. *Guide to Measuring Household Food Security, Revised 2000.* Alexandria, VA: U.S. Department of Agriculture, Food and Nutrition Service.

Bird, Chloe E., Teresa Seeman, José J. Escarce, Ricardo Basurto-Dávila, Brian K. Finch, Tamara Dubowitz, Melonie Heron, et al. 2010. Neighbourhood socioeconomic status and biological 'wear and tear' in a nationally representative sample of US adults. *Journal of Epidemiology and Community Health* 64(10): 860-65.

Bishaw, Alemayehu, and Suzanne Macartney. 2010. Poverty: 2008 and 2009. American Community Survey Brief ACSBR/09-1. Washington, DC: U.S. Census Bureau.

Bishaw, Alemayehu, and Trudi J. Renwick. 2011. Poverty: 2007 and 2008 American Community Surveys. American Community Survey Report ACSBR/08-1. Washington, DC: U.S. Census Bureau.

Blair Lewis, Lavonna, David C. Sloane, Lori Miller Nascimento, Allison L. Diamant, Joyce Jones Guinyard, Antronette K. Yancey, and Gwendolyn Flynn. 2005. African Americans' access to healthy food options in South Los Angeles restaurants. *American Journal of Public Health* 95(4): 668-73.

Blake, Kevin S., Rebecca L. Kellerson, and Aleksandra Simic. 2007. *Measuring overcrowding in housing.* Washington, DC: U.S. Department of Housing and Urban Development.

Blank, Rebecca. 2002. Evaluating welfare reform in the United States. *Journal of Economic Literature* 40(4): 1105-66.

Block, Jeanne H. 1983. Differential premises arising from differential socialization of the sexes: Some conjectures. *Child Development* 54: 1335-54.

Bloom, Howard. 1984. Accounting for no-shows in experimental evaluation designs. *Evaluation Review* 8 (April): 225-46.

Bloom, Howard S., Saskia Levy Thompson, and Rebecca Unterman. 2010. *Transforming the High School Experience: How New York City's New Small Schools are Boosting Student Achievement and Graduation Rates.* Manpower Demonstration Research Corporation, June. http://www.mdrc.org/publications/560/overview.html.

Bloomberg, Gordon R., and Edith Chen. 2005. The relationship of psychologic stress with childhood asthma. *Immunology and Allergy Clinics of North America* 25(1): 83-105.

Blumstein, Alfred. 2000. "Disaggregating the violence trends." In *The Crime Drop in America*, edited by Alfred Blumstein and Joel Wallman, 13-44. NY: Cambridge University Press.

———. 1995. "Youth Violence, Guns, and the Illicit-Drug Industry." *Journal of Criminal Law and Criminology* 86: 10-36.

Bombard, Jennifer, Patricia Dietz, Christine Galavotti, Lucinda England, Van T. Tong, Donald K. Hayes, and Brian Morrow. 2010. Chronic diseases and related risk factors among low-income mothers. *Maternal and Child Health Journal* December 14.

Borjas, George J. 1995. Ethnicity, neighborhoods and human-capital externalities. *American Economic Review* 85: 365-90.

Borman, Geoffrey D., Robert E. Slavin, Alan Cheung, Anne Chamberlain, Nancy Madden, and Bette Chambers. 2007. Final reading outcomes of the national randomized field trial of Success For All. *American Educational Research Journal*. 44(3): 701-731.

Bottcher, Jean. 2001. Social practices of gender: How gender relates to delinquency in the everyday lives of high-risk youths. *Criminology* 39: 893-931.

Bound, John, Charles C. Brown, and Nancy Mathiowetz. 2001. Measurement error in survey data. In *Handbook of econometrics*, ed. E.E. Learner and J.J. Heckman. 3705-3843. New York: North Holland.

Briggs, Xavier de Souza. 2008. More pluribus, less unum? The changing geography of race and opportunity. In *Neighbourhood Renewal and Housing Markets: Community Engagement in the US & UK*, ed. H. Beider. Oxford, UK: Blackwell.

_____. 1998.Brown kids in white suburbs: Housing mobility and the many facets of social capital. *Housing Policy Debate* 9(1): 177-221.

Briggs, Xavier de Souza, Susan J. Popkin, and John Goering. 2010. *Moving to Opportunity: The story of an American experiment to fight ghetto poverty.* New York: Oxford University Press.

Brock, W. A., and S. N. Dulauf. 2001. "Interactions-Based Models." In *Handbook of Econometrics*, Vol. 5, edited by J. J. Heckman and E. Leamer, 3297–3380. Amsterdam: North-Holland.

Brooks-Gunn, Jeanne, Greg J. Duncan, Pamela K. Klebanov, and Naomi Sealand. 1993."Do Neighborhoods Influence Child and Adolescent Development?" *American Journal of Sociology* 99: 353-95.

Browning, Christopher R., and Kathleen A. Cagney. 2002. Neighborhood structural disadvantage, collective efficacy, and self-rated physical health in an urban setting. *Journal of Health and Social Behavior* 43(4): 383-99.

———. 2003. Moving beyond poverty: Neighborhood structure, social processes, and health. *Journal of Health and Social Behavior* 44(4): 552-71.

Bryant-Stephens, Tyra. 2009. Asthma disparities in urban environments. *Journal of Allergy and Clinical Immunology* 123(6): 1199-206.

Burdick-Will, Julia, Jens Ludwig, Stephen Raudenbush, Robert Sampson, Lisa Sanbonmatsu, and Patrick Sharkey. 2011. "Converging Evidence for Neighborhood Effects on Children's Test Scores: An Experimental, Quasi-Experimental and Observational Comparison." *Social Inequality and Educational Disadvantage*. Greg Duncan and Richard Murnane, eds. New York: Russell Sage Foundation.

Bureau of Justice Statistics. 2011. *Sourcebook of Criminal Justice Statistics*, Table 4.7.2009. Accessed September 1. http://www.albany.edu/sourcebook/pdf/t472009.pdf.

Carneiro, Pedro, and James J. Heckman. 2003. Human capital policy. In *Inequality in America: What role for human capital policies?* ed. by James J. Heckman and Alan B. Krueger. 77-239. Cambridge, MA: MIT Press.

Carr, James H., and Jenny Schuetz. 2001. *Financial services in distressed communities: Issues and answers.* Washington, DC: Fannie Mae Foundation.

Case, A., and L. Katz. 1991. "The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths." National Bureau of Economic Research Working Paper No. 3705.

Caskey, John. 1997. Lower-income Americans, higher cost financial services. University of Wisconsin–Madison, School of Business: Filene Research Institute, Center for Credit Union Research.

Casserly, Michael. 2002. "Beating the Odds II: A City-by-City Analysis of Student Performance and Achievement Gaps on State Assessments, Spring 2001 Results." Prepared for the Council of the Great City Schools. Washington, D.C.

Center on Budget and Policy Priorities. 2011. *State-by-state fact sheets: Federal rental assistance*. Washington, DC: author.

Chamberlain, Anne, Cecelia Daniels, Nancy A. Madden, and Robert E. Slavin. 2007. A randomized evaluation of the Success for All middle school reading program. *Middle Grades Research Journal* (MGRJ). 2(1): 1-21.

Chapman , Chris, Jennifer Laird, and Angelina Kewal-Ramani. 2010. "Trends in High School Dropout and Completion Rates in the United States: 1972–2008." Prepared for the National Center for Education Statistics, Washington, D.C.

Chase-Lansdale, P. Lindsay, Rachel A. Gordon, Jeanne Brooks-Gunn, and Pamela K. Klebanov. 1997. "Neighborhood and familial influences on the intellectual and behavioral competence of preschool and early school-age children." In *Neighborhood poverty: Context and consequences for development*, eds. Jeanne Brooks-Gunn, Greg J. Duncan, and J. Lawrence Aber. 79-118. New York: Russell Sage Foundation.

Chetty, Raj, John Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. 2010. How does your kindergarten classroom affect your earnings? Evidence from Project STAR. NBER Working Paper no. 1638. Washington, DC: National Bureau of Economic Research.

Chobanian, Aram V., George L. Bakris, Henry R. Black, William C. Cushman, Lee A. Green, Joseph L. Izzo, Jr., Daniel W. Jones, et al., and the National High Blood Pressure Education Program Coordinating Committee. 2003. Seventh report of the Joint National Committee on Prevention, Detection, Evaluation, and Treatment of High Blood Pressure. *Hypertension* 42(6): 1206-52.

Christakis, Nicholas, and James Fowler. 2007. The spread of obesity in a large social network over 32 years. *New England Journal of Medicine* 357:370-79.

Cicala, Steven, Roland Fryer, and Jörg Spenkuch. 2011. "A Roy Model of Social Interactions." Harvard University, February.

Clampet-Lundquist, Susan. 2007. No more 'Bois ball: The impact of relocation from public housing on adolescents. *Journal of Adolescent Research* 22(3): 298-323.

Clampet-Lundquist, Susan, Kathryn Edin, Jeffrey R. Kling, and Greg J. Duncan. 2006. Moving at-risk kids to better neighborhoods: Why girls fare better than boys. Working Paper 888. Princeton, NJ: Princeton University, Industrial Relations Section.

———. 2011. Moving teenagers out of high-risk neighborhoods: How girls fare better than boys. *American Journal of Sociology* 116: 1154-89.

Clampet-Lundquist, Susan, and Douglas S. Massey. 2008. Neighborhood effects on economic self-sufficiency: A reconsideration of the Moving to Opportunity experiment. *American Journal of Sociology.* 114(1): 107-43.

Cohen, Mark A. 2005. *The Costs of Crime and Justice*. New York: Routledge.

Cohen, Sheldon. 2004. Social relationships and health. *American Psychologist* 59(8): 676-84.

Cole, Tim J., Mary C. Bellizzi, Katherine M. Flegal, and William H. Dietz. 2000. Establishing a standard definition for child overweight and obesity worldwide: International survey. *British Medical Journal* 320:1240.

Coleman, James S. 1988. Social capital in the creation of human capital. *American Journal of Sociology* 94: S95-S120.

Coleman, John C., and Leo B. Hendry. 1999. *The nature of adolescence* (3rd ed.). London: Routledge.

Comarow, Avery. 2010. Best Hospitals 2010-11: The Honor Roll. U.S. News & World Report. Accessed February 11, 2001. http://health.usnews.com/health-news/best-hospitals/articles/2010/07/14/best-hospitals-2010-11-the-honor-roll

Comey, Jennifer, Xavier De Souza Briggs, and Gretchen Weismann. 2008. *Struggling to stay out of high-poverty neighborhoods: Lessons from the Moving to Opportunity experiment*. Washington, DC: Urban Institute.

Connell, James P. and Bonnie L. Halpern-Felsher. 1997. "How neighborhoods affect educational outcomes in middle childhood and adolescence: conceptual issues and an empirical example." In *Neighborhood poverty: Context and consequences for development,* eds. Jeanne Brooks-Gunn, Greg J. Duncan, and J. Lawrence Aber. 174-99. New York: Russell Sage Foundation.

Cook, Philip J., and Kristen Goss. 1996. A selective review of the social-contagion literature. Working Paper. Sanford Institute of Public Policy, Duke University.

Cook, Philip J., and John H. Laub. 2002. After the epidemic: recent trends in youth violence in the United States. In Michael Tonry, ed., *Crime and justice: a review of research.* Pp. 117-153. Chicago: University of Chicago Press.

———. 1998. "The Unprecedented Epidemic in Youth Violence." In *Youth Violence*, edited by Michael Tonry and Mark H. Moore, 101-138. University of Chicago Press.

Cook, Philip J., Jens Ludwig, Sudhir A. Venkatesh, and Anthony A. Braga. 2007. Underground gun markets. *Economic Journal*. 117: F558-588.

Cook, Philip J., and Jens Ludwig. 2000. *Gun Violence: The Real Costs.* New York: Oxford University Press.

Corcoran, Mary, Roger Gordon, Deborah Laren, and Gary Solon. 1992. The association between men's economic status and their family and community origins. *Journal of Human Resources* 27:575–601.

Corcoran, Mary, and Colleen Heflin. 2003. Barriers to work among recipients of housing assistance. *Cityscape* 6(2): 73-87.

Cove, Elizabeth, Xavier De Souza Briggs, Margery Austin Turner, and Cynthia Duarte. 2008. *Can escaping from poor neighborhoods increase employment and earnings?* Washington, DC: The Urban Institute.

Cowie, Catherine C., Keith F. Rust, Danita D. Byrd-Holt, Mark S. Eberhardt, Katherine M. Flegal, Michael M. Engelgau, Sharon H. Saydah, Desmond E. Williams, Linda S. Geiss, and Edward W. Gregg. 2006. Prevalence of diabetes and impaired fasting glucose in adults in the U.S. population. *Diabetes Care* 29:1263–68.

Crain, Ellen F., Michelle Walter, George T. O'Connor, Herman Mitchell, Rebecca S. Gruchalla, Meyer Kattan, George S. Malindzak, et al. 2002. Home and allergic characteristics of children with asthma in seven U.S. urban communities and design of an environmental intervention: The Inner-city asthma study. *Environmental Health Perspectives* 110(9): 939-45.

Crane, Jonathan. 1991. The epidemic theory of ghettos and neighborhood effects on dropping out and teenage childbearing. *American Journal of Sociology* 96(5): 1226-59.

Crowder, Kyle, and Scott J. South. 2005. Race, class, and changing patterns of migration between poor and nonpoor neighborhoods. *American Journal of Sociology,* 110(6): 1715-76.

Cubbin, Catherine, and Marilyn A. Winkleby. 2005. Protective and harmful effects of neighborhood-level deprivation on individual health knowledge, behavior changes, and risk of coronary heart disease. *American Journal of Epidemiology* 162: 559-68.

Cullen, Julie Berry, Brian A. Jacob and Steven D. Levitt. 2006. The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica*. 74(5): 1191-1230.

Currie, Janet, and Duncan Thomas. 1995. Does Head Start make a difference? *American Economic Review* 80: 341-364.

Currie, Janet, and Reed Walker. 2011. Traffic congestion and infant health: Evidence from E-Z pass. *American Economic Journal: Applied Economics* 3(1): 65-90.

Curtis, Lori J., Martin D. Dooley, and Shelley A. Phipps. 2004. Child well-being and neighbourhood quality: Evidence from the Canadian National Longitudinal Survey of Children and Youth. *Social Science and Medicine* 58(10): 1917-27.

Darling-Hammond, Linda. 1996. "Restructuring Schools for High Performance." In *Rewards and Reform: Creating Educational Incentives that Work*. Susan H. Fuhrman and Jennifer A. O'Day, eds. 144-194. New York: Jossey-Bass.

Darmon, Nicole, and Adam Drewnoski. 2008. Does social class predict diet quality? *American Journal of Clinical Nutrition* 87(5): 1107-17.

Deater-Deckard, Kirby. 2001. Annotation: Recent research examining the role of peer relationships in the development of psychopathology. *Journal of Child Psychology and Psychiatry* 42: 565-79.

Delpierre, C., V. Lauwers-Cances, G.D. Datta, T. Lang, and L. Berkman. 2009. Using self-rated health for analysing social inequalities in health: A risk for underestimating the gap between socioeconomic groups? *Journal of Epidemiology and Community Health* 63(6): 426-32.

Deluca, Stefanie, Greg J. Duncan, Micere Keels, and Ruby Mendenhall. 2010. Gautreaux mothers and their children: An update. *Housing Policy Debate* 20(1): 7-25.

DeLuca, Stefanie and James E. Rosenbaum. 2003. "Do Blacks Prefer Integrated Neighborhoods? Testing Survey Opinions with Quasi-Experimental Residential Mobility Data." *Housing Policy Debate* 14: 305-346.

———. 2002. "If Low-Income Blacks Are Given a Chance to Live in White Neighborhoods, Will They Stay? Examining Mobility Patterns with Quasi-Experimental Data." Working Paper WP-02-28. Evanston, IL: Institute for Policy Research, Northwestern University.

DeLuca, Stefanie and Peter Rosenblatt. 2010. "Does Moving To Better Neighborhoods Lead to Better Schooling Opportunities? Parental School Choice in an Experimental Housing Voucher Program." *Teachers College Record.* 112(5): 1443–1491.

———. 2009. Leaving the Ghetto: Residential Mobility and Opportunity in Baltimore. Paper presented at the annual meeting of the American Sociological Association Annual Meeting, San Francisco, CA, Aug 08.

Deming, David. 2009. Early childhood intervention and life-cycle skill development: Evidence from Head Start. *American Economic Journal: Applied Economics.* 1(3): 111-34.

Denavas-Walt, Carmen, Bernadette Proctor, and Jessica Smith. 2010. Income, poverty, and health insurance coverage in the United States: 2009. U.S. Census Bureau, Current Population Reports: Consumer Income, P60-238. Washington, DC: Government Printing Office.

Department of Data Management (DDM). 2006. "Iowa Tests of Basic Skills Citywide Results Over Time--1997 to 2005." Prepared for the Office of Research, Evaluation, and Accountability. Chicago Public Schools.

Depres, Jean-Pierre, and Isabelle Lemieux. 2006. Abdominal obesity and metabolic syndrome. *Nature* 444(14): 881-87.

Devine, Deborah J., Robert W. Gray, Lester Rubin, and Lydia B. Taghavi. 2003. Housing choice voucher location patterns: Implications for participant and neighborhood welfare. Washington, DC: U.S. Department of Housing and Urban Development.

Dietz, Robert D. 2002. The estimation of neighborhood effects in the social sciences. *Social Science Research* 31: 539-75.

Diez-Roux, Ana V., and Christina Mair. 2010. Neighborhoods and health. *Annals of the New York Academy of the Sciences* 1186: 125-45.

Diez-Roux, Ana V., Sharon Stein Merkin, Donna Arnett, Lloyd Chambless, Mark Massing, F. Javier Nieto, Paul Sorlie, Moyses Szklo, Herman A. Tyroler, and Robert L. Watson. 2001. Neighborhood of residence and incidence of coronary heart disease. *New England Journal of Medicine* 345(2): 99-106.

Dodge, Kenneth A. 2009. Community intervention and public policy in the prevention of antisocial behavior. *Journal of Child Psychology and Psychiatry* 50: 194-200.

Dornbusch, Sanford M., Philip L. Ritter, and Laurence Steinberg. 1991. "Community influences on the relations of family statuses to adolescent school performance: Differences between African Americans and Non-Hispanic Whites." *American Journal of Education* 38: 543-67.

Dowd, James J., Rose P. Sisson, and Dennis M. Kern. 1981. Socialization to violence among the aged. *Journal of Gerontology* 36: 350-61.

Dowd Jennifer B., and Anna Zajacova. 2010. Does self-rated health mean the same thing across socioeconomic groups? Evidence from biomarker data. *Annals of Epidemiology* 20(10): 743-49.

Dubowitz, Tamara, Melonie Heron, Chloe E. Bird, Nicole Lurie, Brian K. Finch, Ricardo Basurto-Dávila, Lauren Hale, and José J. Escarce. 2008. Neighborhood socioeconomic status and fruit and vegetable intake among whites, blacks, and Mexican Americans in the United States. *American Journal of Clinical Nutrition* 87(6): 1883-91.

Duncan, Greg J. 1994. "Families and Neighbors as Sources of Disadvantage in the Schooling Decisions of Black and White Adolescents." *American Journal of Education* 103(1): 20-53.

Duncan, Greg J., Jeanne Brooks-Gunn, and Pamela K. Klebanov. 1994. Economic deprivation and early childhood development. *Child Development* 65(2): 296-318.

Duncan, Greg J. and Eric Petersen. 2001.The Long and Short of Asking Questions about Income, Wealth and Labor Supply. *Social Science Research,* 30(2): 248-263.

Durlauf, Steven N. 2004. Neighborhood effects. In *Handbook of regional and urban economics,* ed. J.V. Henderson and J.V. Thisse.2173-2242. Amsterdam: North Holland.

Ellen, Ingrid D., and Margery A. Turner. 1997. Does neighborhood matter? *Housing Policy Debate* 8(4): 833-66.

Entwisle, Doris R., Karl L. Alexander, and Linda Steffel Olson. 1994. "The gender gap in math: Its possible origins in neighborhood effects." *American Sociological Review* 59: 822-38.

Esbensen, F., and D. Huizinga. 1990. "Community Structure and Drug Use: From a Social Disorganization Perspective." *Justice Quarterly* 7(4): 691-709.

Ettner, Susan L., Richard G. Frank and Ronald C. Kessler. 1997. The impact of psychiatric disorders on labor market outcomes. *Industrial and Labor Relations Review* 51(1): 64-81.

Famularo, Richard, Terence Fenton, Robert Kinscherff, and Marilyn Augustyn. 1996. Psychiatric comorbidity in childhood post traumatic stress disorder. *Child Abuse and Neglect* 20: 953-61.

Farrington, David. 2002. "Key Results from the First Forty Years of the Cambridge Study in Delinquency Development." In *Taking Stock of Delinquency: An Overview of Findings from Contemporary Longitudinal Studies,* edited by T. Thornberry and M. Krohn. New York: Kluwer/Plenum.

Feins, Judith D., and Mark D. Shroder. 2005. Moving to opportunity: The demonstration's design and its effects on mobility. *Urban Studies* 42(8): 1275-99.

Ferguson, Ronald. 2010. Tripod Student Perceptions Survey (Version 6). Tripod Project for School Improvement. Available at http://tripodproject.org.

———. 2001. "A Diagnostic Analysis of Black-White GPA Disparities in Shaker Heights, Ohio." In *Brookings Papers on Education Policy,* edited by Diane Ravitch, 347-414. Washington, DC: Brookings Institution Press.

Ferryman, Kadija, Xavier De Souza Briggs, Susan J. Popkin, and Maria Rendon. 2008. *Do better neighborhoods for MTO families mean better schools?* Washington, DC: Urban Institute.

Fields, Jason, Kristin Smith, Loretta E. Bass, and Terry Lugaila. (2001). A Child's Day: Home, School and Play (Selected Indicators of Child Well-Being). CENSUS-P70-68. Washington D.C.: Bureau of the Census (DOC).

Filippelli, Gabriel M., and Mark A.S. Laidlaw. 2010. The elephant in the playground: Confronting lead-contaminated soils as an important source of lead burdens to urban populations. *Perspectives in Biology and Medicine* 53(1): 31-45.

Fischer, Will, and Barbara Sard. 2008. *Preserving safe, high quality public housing should be a priority of federal housing policy.* Washington, DC: Center on Budget and Policy Priorities.

Fitzpatrick, Kevin, and Mark Lagory. 2000. *Unhealthy places: The ecology of risk in the urban landscape.* New York: Routledge.

Ford, Earl S., Ali H. Mokdad, and Wayne H. Giles, 2003. Trends in waist circumference among U.S. adults. *Obesity Research* 11:1223–31.

Fortson, Jane G., and Lisa Sanbonmatsu. 2010. Child health and neighborhood conditions: Evidence from a randomized housing voucher experiment. *Journal of Human Resources* 45(4): 840-64.

Franco, Manuel, Ana V. Diez-Roux, Thomas A. Glass, Benjamin Caballero, and Frederick L. Brancati. 2008. Neighborhood characteristics and availability of healthy foods in Baltimore. *American Journal of Preventive Medicine* 35(6): 561-67.

Furukawa, T.A., R.C. Kessler, T. Slade, and G. Andrews. 2003. The performance of the K6 and K10 screening scales for psychological distress in the Australian National Survey of Mental Health and Well-Being. *Psychological Medicine* 33: 357-62.

Garces, Eliana, Duncan Thomas, and Janet Currie. 2002. Longer term effects of Head Start. American Economic Review 92: 999-1012.

General Accounting Office. 2001. Federal housing assistance programs: Costs and housing characteristics. Report GAO-01-901R. Washington, DC: GAO.

Gennetian, Lisa, Sendhil Mullainathan, and Eldar Shafir (forthcoming).Economic instability, mental resources and poverty traps. In The *Behavioral Foundations of Policy,* ed. E. Shafir. Princeton and New York: Princeton University and Russell Sage Foundation.

Gidlow, Christopher, Thomas Cochrane, Rachel C. Davey, Graham Smith, and Jon Fairburn. 2010. Relative importance of physical and social aspects of perceived neighborhood environment for self-reported health. *Preventive Medicine* 51(2): 157-63.

Gifford-Smith, Mary, Kenneth A. Dodge, Thomas J. Dishion, and Joan McCord. 2005. Peer influence in children and adolescents: Crossing the bridge from developmental to intervention science. *Journal of Abnormal Child Psychology* 33: 255-65.

Glaeser, Edward L., and José A. Scheinkman. 1999. Measuring social interactions. Unpublished paper, Harvard University.

Goering, John, and Judith D. Feins, eds. 2003. *Choosing a better life? Evaluating the Moving to Opportunity social experiment.* Washington DC: Urban Institute.

Goering, John, Judith D. Feins, and Todd M. Richardson. 2003. What have we learned about housing mobility and poverty deconcentration? In *Choosing a better life? Evaluating the Moving to Opportunity social experiment,* ed. John Goering and Judith D. Feins. 3-36. Washington, DC: Urban Institute.

———. 2002. A cross-site analysis of initial Moving to Opportunity demonstration results. *Journal of Housing Research* 13(1). Available at www.fanniemaefoundation.org/programs/jhr/pdf/jhr_1301_goering.pdf.

Goering, John, Joan Kraft, Judith Feins, Debra McInnis, Mary Joel Holin, and Huda Elhassan. 1999. *Moving to Opportunity for fair housing demonstration program: Current status and initial findings.* Washington, DC: U.S. Department of Housing and Urban Development.

Goldstein, Sara, Pamela Davis-Kean, and Jacquelynne Eccles. 2005. Parents, peers, and problem behavior: A longitudinal investigation of the impact of relationship perceptions and characteristics on the development of adolescent problem behaviors. *Developmental Psychology* 41(1): 401-13.

Gonzalez, Nancy, and Kenneth A. Dodge. 2010. "Family and Peer Influences on Adolescent Behavior and Risk-Taking."

Goodman, Anna, and Robert Goodman. 2009. Strengths and difficulties questionnaire as a dimensional measure of child mental health. *Journal of the American Academy of Child and Adolescent Psychiatry* 48: 400-03.

Gould, Eric D., Victor Lavy, and M. Daniele Paserman 2009. Sixty years after the magic carpet ride: The long-run effect of the early childhood environment on social and economic outcomes. NBER Working Paper no. 14884. Cambridge: National Bureau of Economic Research.

Green, Jennifer G., Katie A. McLaughlin, Patricia A. Berglund, Michael J. Gruber, Nancy A. Sampson, Alan M. Zaslavsky, and Ronald C. Kessler. 2010. Childhood adversities and adult psychiatric disorders in the national comorbidity survey replication I: associations with first onset of DSM-IV disorders. *Archives of General Psychiatry* 67: 113-23.

Groves, Betsy M., Barry Zuckerman, Steven Marans, and Donald J. Cohen. 1993. Silent victims: Children who witness violence. *Journal of the American Medical Association* 269: 262-64.

Groves, Robert M., Floyd J. Fowler, Mick P. Couper, James M. Lepkowski, Eleanor Singer, and Roger Tourangeau. 2004. *Survey Methodology.* New York: John Wiley & Sons.

Guttmacher Institute. 2010. U.S. Teenage Pregnancies, Births, and Abortions: National and State Trends and Trends by Race and Ethnicity. www.guttmacher.org (accessed August 28, 2011).

Hanratty, Maria H., Sara A. McLanahan, and Becky Pettit. 2003. Los Angeles site findings. In *Choosing a better life? Evaluating the Moving to Opportunity social experiment,* ed. John Goering and Judith Feins. 245-74. Washington, DC: Urban Institute.

———. 1998. The impact of the Los Angeles Moving to Opportunity program on residential mobility, neighborhood characteristics, and early child and parent outcomes. Working Paper Number 98-18. Los Angeles: Bendheim-Thoman Center for Research on Child Wellbeing.

Harcourt, Bernard, and Jens Ludwig. 2006. Broken windows: New evidence from New York City and a 5-city social experiment. *University of Chicago Law Review.*

Harkness, Joseph, and Sandra J. Newman. 2005. Housing affordability and children's wellbeing: Evidence from the National Survey of America's Families. *Housing Policy Debate* 16(2): 223-55.

Haro, Josep M., Saena Arbabzadeh-Bouchez, Traolach S. Brugha, Giovanni De Girolamo, Margaret E. Guyer, Robert Jin, Jean Pierre Lepine, Fausto Mazzi, Blanca Reneses, Gemma Vilagut, Nancy A. Sampson, and Ronald C. Kessler. 2006. Concordance of the Composite International Diagnostic Interview Version 3.0 (CIDI 3.0) with standardized clinical assessments in the WHO World Mental Health Surveys. *International Journal of Methods in Psychiatric Research* 15(4): 167-80.

Hirschfield, Alex, and Kate J. Bowers. 1997. "The effect of social cohesion on levels of recorded crime in disadvantaged areas." *Urban Studies.* 34(8): 1275–95.

Hirschi, Travis, and Michael R. Gottfredson. 1983. "Age and the Explanation of Crime." *American Journal of Sociology* 89(3): 552-584.

Hogan, Dennis P., Nan Marie Astone, and Evelyn M. Kitagawa. 1985. "Social and Environmental Factors Influencing Contraceptive Use Among Black Adolescents." *Family Planning Perspectives.* 17(4): 165-169.

Hogan, Dennis P., and Evelyn M. Kitagawa. 1985. "The Impact of Social Status, Family Structure, and Neighborhood on the Fertility of Black Adolescents." *American Journal of Sociology.* 90(4): 825-855.

Huizinga, David, Terence Thornberry, Kelly Knight, and Peter Lovegrove. 2007. *Disproportionate Minority Contact in the Juvenile Justice System: A Study of Differential Minority Arrest/Referral to Court in Three Cities.* Washington, DC: National Criminal Justice. Available at https://www.ncjrs.gov/pdffiles1/ojjdp/grants/219743.pdf.

Hunt, D. Bradford. 2009. *Blueprint for disaster: The unraveling of Chicago public housing.* Chicago: University of Chicago Press.

Huston, Sara L., Kelly R. Evenson, and Philip Bors. 2003. Neighborhood environment, access to places for activity, and leisure-time physical activity in a diverse North Carolina population. *American Journal of Health Promotion* 18: 58-69.

Idler, Ellen, and Yael Benyamini. 1997. Self-rated health and mortality: A review of twenty-seven community studies. *Journal of Health and Social Behavior* 38(1): 21-37.

Idler, Ellen, and Stanislav V. Kasl. 1995. Self-ratings of health: Do they also predict changes in functionality? *Journal of Gerontology: Social Sciences* 50(6): S344-53.

Ihlanfeldt, Keith, and David Sjoquist. 1998. The spatial mismatch hypothesis: A review of recent studies and their implications for welfare reform. *Housing Policy Debate* 9(4): 849-92.

Inagami, Sanae, Deborah Cohen, Brian Karl Finch, and Steven Asch. 2006. You are where you shop: Grocery store locations, weight, and neighborhoods. *American Journal of Preventive Medicine* 31(1): 10–17.

Ingels, Steven J., Ben Dalton, Tommy E. Holder, Jr., Erich Lauff, Laura J. Burns, Laura LoGerfo. 2011. "The High School Longitudinal Study of 2009 (HSLS:09) A First Look at Fall 2009 Ninth-Graders." Prepared for the National Center for Education Statistics.

Ingels, Steven J., Kathryn L. Dowd, John D. Baldridge, James L. Stipe, Virginia H. Bartot, and Martin R. Frankel. 1994. *National education longitudinal study of 1998 second follow-up: student component data file user's manual*. Washington, DC: U.S. Department of Education.

Institute for Social Research. 2008. *Health and retirement study: Physical measures and biomarkers* 2008. Ann Arbor: University of Michigan. Available at http://hrsonline.isr.umich.edu/modules/meta/2008/core/qnaire/online/2008PhysicalMeasuresBiomarkers.pdf.

Ioannides, Yannis M., and Linda Datcher Loury. 2004. Job information networks, neighborhood effects, and inequality. *Journal of Economic Literature* 42:1056-93.

Jacob, Brian. 2004. Public housing, housing vouchers and student achievement: Evidence from public housing demolitions in Chicago. *American Economic Review* 94(1): 233-58.

———. 2003. "Getting Inside Accountability: Lessons from Chicago." In William G. Gale and Janet Rothenberg Pack, eds., *Brookings-Wharton Papers on Urban Affairs 2003*. Washington, D.C.: The Brookings Institution, pp. 41-70.

Jacob, Brian A., and Jens Ludwig. 2011. The effects of housing assistance on labor supply: Evidence from a voucher lottery. *American Economic Review*.

Jacob, Brian A., Jens Ludwig, and Douglas L. Miller. 2011. "The effects of housing and neighborhood conditions on child mortality." National Bureau of Economic Research Working Paper No. 17369.

Jargowsky, Paul A. 1997. *Poverty and place: Ghettos, barrios, and the American city*. New York: Russell Sage Foundation.

Jencks, Christopher, and Susan E. Mayer. 1990. The social consequences of growing up in a poor neighborhood. *In Inner-city poverty in the United States,* ed. L.E. Lynn, Jr., and M.G.H. McGeary. 111-86. Washington, DC: National Academy Press.

Jencks, Christopher and Meredith Phillips, eds. 1998. *The Black-White Test Score Gap*. Washington, D.C.: The Brookings Institution.

Johnston, L. D., P.M. O'Malley, J.G. Bachman, and J.E. Schulenberg. 2011. *Monitoring the Future national results on adolescent drug use: Overview of key findings, 2010*. Ann Arbor: Institute for Social Research, University of Michigan.

Joint National Committee on Prevention, Detection, Evaluation, and Treatment of High Blood Pressure. 1997. The sixth report of the Joint National Committee on Prevention, Detection, Evaluation, and Treatment of High Blood Pressure. *Archives of Internal Medicine* 157(21): 2413-46.

Jones, LeAlan, and Loyd Newman. 1998. *Our America: Life and death on the south side of Chicago*. New York: Washington Square.

Kain, John F. 1968. Housing segregation, Negro employment, and metropolitan decentralization. *Quarterly Journal of Economics* 82(2): 175-97.

Kan, Haidong, Gerardo Heiss, Kathryn M. Rose, Eric A. Whitsel, Fred Lurmann, and Stephanie J. London. 2008. Prospective analysis of traffic exposure as a risk factor for incident coronary heart disease: The Atherosclerosis Risk in Communities (ARIC) Study. *Environmental Health Perspectives* 116(11): 1463-68.

Katz, Lawrence F., Jeffrey R. Kling, and Jeffrey B. Liebman. 2001. Moving to Opportunity in Boston: Early results of a randomized mobility experiment. *Quarterly Journal of Economics* 116: 607-54.

Kawachi, Ichiro, and Lisa F. Berkman, eds. 2003a. . *Neighborhoods and health*. New York: Oxford University Press.

———. 2003b. Introduction, In *Neighborhoods and health,* ed. Ichiro Kawachi and Lisa F. Berkman. Pp. 1-19. New York: Oxford University Press.

Keels, Micere. 2008. Residential attainment of now-adult Gautreaux Children: Do they gain, hold or lose ground in neighborhood ethnic and economic segregation? *Housing Studies* 23(4): 541-64.

Keels, Micere, Stefanie DeLuca, Greg J. Duncan, Ruby Mendenhall, and James Rosenbaum 2005. Fifteen years later: Can residential mobility programs provide a long-term escape from neighborhood segregation, crime, and poverty? *Demography* 42(1): 51-73.

Kemple, James J. 2008. *Career Academies: Long-term impacts on labor market outcomes, educational attainment, and transitions to adulthood.* New York: MDRC.

Kessler, Ronald, Shelli Avenevoli, E. Jane Costello, Jennifer Green, Michael Gruber, Steven Heeringa, Kathleen Merikangas, Beth-Ellen Pennell, Nancy Sampson, and Alan Zaslavsky. 2009. National Comorbidity Survey Replication Adolescent Supplement (NCS-A): II. Overview and design. *Journal of the American Academy of Child and Adolescent Psychiatry* 48(4): 380-85.

Kessler, Ronald C., Peggy R. Barker, Lisa J. Colpe, Joan F. Epstein, Joseph C. Gfroerer, Eva Hiripi, Mary J. Howes, Sharon-Lise T. Normand, Ronald W. Manderscheid, Ellen E. Walters, and Alan M. Zaslavsky. 2003. Screening for serious mental illness in the general population. *Archives of General Psychiatry* 60: 184-89.

Kessler, R.C., W.T. Chiu, O. Demler, K.R. Merikangas, and E.E. Walters. 2005. Prevalence, severity, and comorbidity of 12-month DSM-IV disorders in the National Comorbidity Survey Replication. *Archives of General Psychiatry* 62:617-27.

Kessler, Ronald C., Katherine A. McGonagle, Shanyang Zhao, Christopher B. Nelson, Michael Hughes, Suzann Eshleman, Hans-Ulrich Wittchen, and Kenneth S. Kendler. 1994. Lifetime and 12-month prevalence of DSM-III-R psychiatric disorders in the United States: Results from the National Comorbidity Survey. *Archives of General Psychiatry* 51: 8-19.

Kessler, R.C., and T.B. Üstün, eds. 2008. *The WHO world mental health surveys: Global perspectives on the epidemiology of mental disorders.* New York: Cambridge University Press.

_____. 2004. The World mental health (WMH) survey initiative version of the World Health Organization (WHO) composite international diagnostic interview (CIDI). *International Journal of Methods in Psychiatric Research* 13: 93-121.

Khadduri, Jill, Mark Shroder, and Barry Steffen. 1998. Welfare reform and HUD-assisted housing: Measuring the extent of needs and opportunities. Paper presented at Conference on Managing Affordable Housing Under Welfare Reform: Reconciling Competing Demands. Washington, DC, June 26.

Kirby, James B., and Kaneda, Toshiko 2005. Neighborhood socioeconomic disadvantage and access to health care. *Journal of Health and Social Behavior* 46(1): 15-31.

Klebanov, Pamela K., Jeanne Brooks-Gunn, P. Lindsay Chase-Lansdale, and Rachel A. Gordon. 1997. "Are neighborhood effects on young children mediated by features of the home environment?" In *Neighborhood poverty: Context and consequences for development,* eds. Jeanne Brooks-Gunn, Greg J. Duncan, and J. Lawrence Aber. 119-145. New York: Russell Sage Foundation.

Kleiman, Mark A. R. 1993. "Enforcement swamping: A positive-feedback mechanism in rates of illicit activity." *Mathematical Computational Modeling* 17(2): 65-75.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. Experimental analysis of neighborhood effects. *Econometrica* 75(1): 83-119.

———.2005. "Bullets don't got no name: Consequences of fear in the ghetto." In *Discovering successful pathways in children's development: Mixed methods in the study of childhood and family life,* ed. Thomas S. Weisner. 243-281. Chicago: University of Chicago Press.

Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment. *Quarterly Journal of Economics* 120(1): 87-130.

Kloos, Bret, Seema Shah. (2009). A social ecological approach to investigating relationships between housing and adaptive functioning for persons with serious mental illness. *American Journal of Community Psychology* 44: 316-26.

Knudsen, Eric I., James J. Heckman, Judy Cameron, and Jack P. Shonkoff. 2006. Building America's future workforce: Economic, neurobiological and behavioral perspectives on investment in human skill development. *Proceedings of the National Academy of Sciences.* 103(27): 10155-10162.

Kooklan, Paria. 2008. The Section 8 Housing Choice Voucher Program: Federal Rules and Local Policies Affecting Individuals with Criminal Histories. Los Angeles, CA: Los Angeles Homeless Services Authority. http://www.lahsa.org/docs/policyandplanning/section8.pdf.

Kornitzer, M., Dramaix, M., and G. De Backer. 1999. Epidemiology of risk factors for hypertension: Implications for prevention and therapy. *Drugs* 57(5): 695-712.

Kotlowitz, Alex. 1992. *There are no children here.* New York: Anchor.

Krause, Neal. 1993. Neighborhood deterioration and social isolation in later life. *International Journal of Aging and Human Development* 36: 9-38.

Kuo, Lydia E., Magdalena Czarnecka, Joanna B. Kitlinska, Jason U. Tilan, Richard Kvetnanský, and Zofia Zukowska. 2008. Chronic stress, combined with a high-fat/high-sugar diet, shifts sympathetic signaling toward neuropeptide Y and leads to obesity and the metabolic syndrome. *Annals of the New York Academy of Sciences* 1148: 232-37.

LaGrange, Teresa C., and Robert A. Silverman. 1999. Low self-control and opportunity: Testing the general theory of crime as an explanation for the gender differences in delinquency. *Criminology* 37: 41-72.

Lara, Marielena L., Christina Gamboa, M. Iya Kahramanian, Leo S. Morales, and David E. Hayes Bautista. 2005. Acculturation and Latino health in the United States: A review of the literature and its sociopolitical context. *Annual Review of Public Health* 46: 367-97

Lawler, Edward J., and Shayne R. Thye. 1999. Bringing emotions into social exchange theory. *Annual Review of Sociology* 25: 217-44.

Lee, Crystal, Rachel R. Huxley, Rachel P. Wildman, and Mark Woodward. 2008. Indices of abdominal obesity are better discriminators of cardiovascular risk factors than BMI: A meta-analysis. *Journal of Clinical Epidemiology* 61:646-53.

Lee, Rebecca E., and Catherine Cubbin. 2002. Neighborhood context and youth cardiovascular health behaviors. *American Journal of Public Health* 92(3): 428-36.

Leger, Mireille L. and Stephen D. Kennedy (1990) *Final comprehensive report of the freestanding housing voucher demonstration.* Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research.

Leventhal, Tama, and Jeanne Brooks-Gunn. 2004. A randomized study of neighborhood effects on low-income children's educational outcomes. *Developmental Psychology* 40(4): 488-507.

———. 2003. The early impacts of Moving to Opportunity on children and youth in New York City. In *Choosing a better life: Evaluating the Moving to Opportunity social experiment,* ed. John Goering and Judith Feins. Washington, DC: Urban Institute Press.

———.2000. The neighborhoods they live in: The effects of neighborhood residence of child and adolescent outcomes. *Psychological Bulletin* 126: 309-37.

Levitt, Steven D. 2004. "Understanding Why Crime Fell in the 1990s: Four Factors That Explain the Decline and Six That Do Not." *Journal of Economic Perspectives* 18(1): 163-190.

Little, Michelle and Laurence Steinberg. 2006. "Psychosocial correlates of adolescent drug dealing in the inner city." *Journal of Research in Crime and Delinquency* 43(4): 357-386.

Loewenstein, George, and Ted O'Donoghue. 2004. Animal spirits: Affective and deliberative processes in human behavior. Working Paper. Carnegie Mellon University, Department of Social and Decision Sciences.

Lovasi, Gina S., Kathryn M. Neckerman, James W. Quinn, Christopher C. Weiss, and Andrew Rundle. 2009. Effect of individual or neighborhood disadvantage on the association between neighborhood walkability and body mass index. *American Journal of Public Health* 99: 279-84.

Ludwig, Jens. 2006. "The Costs of Crime." Testimony to the U.S. Senate Judiciary Committee. September 19, 2006.

Ludwig, Jens, Greg J. Duncan, and Paul Hirschfield. 2001. Urban poverty and juvenile crime: Evidence from a randomized housing-mobility experiment. *Quarterly Journal of Economics* 116(2): 655-80.

Ludwig, Jens, Greg Duncan, and Joshua Pinkston. 2005. Housing mobility programs and economic self-sufficiency: Evidence from a randomized experiment. *Journal of Public Economics* 89(1): 131-56.

Ludwig, Jens, and Jeffrey R. Kling. 2007. Is crime contagious? *Journal of Law and Economics* 50: 491-518.

Ludwig, Jens, Helen Ladd, and Greg J. Duncan. 2001. The effects of urban poverty on educational outcomes: Evidence from a randomized experiment. In *Urban poverty and educational outcomes,* ed. William Gale and Janet Rothenberg Pack. 147-201. Washington, DC: Brookings Institution.

Ludwig, Jens, Jeffrey B. Liebman, Jeffrey R. Kling, Greg J. Duncan, Lawrence F. Katz, Ronald C. Kessler, and Lisa Sanbonmatsu. 2008. What can we learn about neighborhood effects from the Moving to Opportunity experiment? *American Journal of Sociology* 114(1): 144-88.

Ludwig, Jens, and Douglas L. Miller. 2007. Does Head Start improve children's life chances? Evidence from a regression discontinuity approach. *Quarterly Journal of Economics*. 122(1): 159-208.

Lumeng, Julie C., Danielle Appugliese, Howard J. Cabral, Robert H. Bradley, and Barry Zuckerman. 2006. Neighborhood safety and overweight status in children. *Archives of Pediatrics and Adolescent Medicine* 160(1): 25-31.

Luttmer, Erzo F.P. 2005. Neighbors as negatives: Relative earnings and well-being. *Quarterly Journal of Economics* 120(3): 963-1002.

Macintyre, Sally, and Anne Ellaway. 2003. Neighborhoods and health: An overview. In *Neighborhoods and Health,* ed. I. Kawachi and L. Berkman. Pp. 20-42. New York: Oxford University Press.

Maguire, Sheila, Joshua Freely, Carol Clymer, Maureen Conway, and Deena Schwartz. 2010. *Tuning in to local labor markets: Findings from the sectoral employment impact study*. New York: Public/Private Ventures.

Mango, Eiji and Albert Park. Forthcoming. Relative deprivation and health: Which reference groups matter? *Journal of Human Resources*.

Maguin, Eugene, and Rolf Loeber. 1996. "Academic performance and delinquency." *Crime and Justice.* 20: 145-264.

Manski, Charles F. 2000. Economic analysis of social interactions. *Journal of Economic Perspectives* 14: 115-36.

Maurin, Eric, and Julie Moschion. 2009. The social multiplier and labor market participation of mothers. *American Economic Journal: Applied Economics* 1(1): 251-72.

McDade, Thomas W. 2003. Life history theory and the immune system: Steps toward a human ecological immunology. *Yearbook of Physical Anthropology* 46:100-25.

McDade, Thomas W., Joy F. Stallings, Adrian Angold, E. Jane Costello, Mary Burleson, John T. Cacioppo, Ronald Glaser, and Carol M. Worthman. 2000. Epstein-Barr virus antibodies in whole blood spots: A minimally invasive method for assessing an aspect of cell-mediated immunity. *Psychosomatic Medicine* 62:560-67.

McLoyd, Vonnie C. and Constance A. Flanagan, eds. 1990. *Economic Stress: Effects on Family Life and Child Development*. New York: Jossey-Bass.

McPherson, Miller, Lynn Smith-Lovin, and James M. Cook (2001) Birds of a feather: Homophily in social networks. *Annual Review of Sociology*. 27: 415-444.

Mendenhall, Ruby, Greg Duncan, and Stefanie DeLuca. 2006. Neighborhood resources, racial segregation and economic mobility: Results from the Gautreaux program. *Social Science Research* 35(4): 892-923.

Merkin, Sharon S., Ricardo Basurto-Davila, Arun Karlamangla, Chloe Bird, Nicole Lurie, Jose Escarce, and Teresa Seeman. 2009. Neighborhoods and cumulative biological risk profiles by race/ethnicity in a national sample of us adults: NHANES III. *Annals of Epidemiology* 19(3): 194-201.

Merry, Sally, Heather McDowell, Sarah Hetrick, Juillet Bir, and N. Muller. 2004. Psychological and/or educational interventions for the prevention of depression in children and adolescents. Cochrane Database of Systematic Reviews CD003380.

Mills, Gregory, Daniel Gubits, Larry Orr, David Long, Judie Feins, Bulbul Kaul, Michelle Wood, and Amy Jones. 2006. *Effects of housing vouchers on welfare families.* Washington, DC: U.S. Department of Housing and Urban Development, Office of Public Development and Research.

Moffitt, Terrie E. 1993. "Adolescent-limited and life-course-persistent antisocial behavior: A developmental taxonomy." *Psychological Review* 100(4): 674-701.

Moffitt, Robert A. 2001. Policy interventions, low-level equilibria, and social interactions. In *Social dynamics,* ed. Steven Durlauf and Peyton Young. Cambridge, MA: MIT Press.

Moore, Kristin A., Sharon McGrode., Elizabeth C. Hair, and Marjorie L. Gunnoe. 1999. *NLSY97 codebook supplement main file round 1, appendix 9, family process and adolescent outcome measures.* Washington, DC: U.S. Department of Labor.

Morenoff, Jeffrey D. , Robert J. Sampson, and Stephen W. Raudenbush. 2001. "Neighborhood Inequality, Collective Efficacy, and the Spatial Dynamics of Urban Violence." *Criminology* 39(3): 517-59.

Morland, Kimberly, Steve Wing, Ana Diez Roux, and Charles Poole. 2002. Neighborhood characteristics associated with the location of food stores and food service places. *American Journal of Preventive Medicine* 22: 23-29.

Mujahid, Mahasin S., Ana Diez Roux, Mingwu Shen, Deepthiman Gowda, Brisa Sanchez, Steven Shea, David Jacobs, Jr., and Sharon Jackson. 2008. Relationship between neighborhood environments and obesity in the multi-ethnic study of atherosclerosis. *American Journal of Epidemiology*. 167 (11): 1349-1357.

Multisite Violence Prevention Project. 2004. The multisite violence prevention project: Background and overview. *American Journal of Preventive Medicine* 26: 3-11.

Murnane, Richard J. 2007. "Improving the Education of Children Living in Poverty." *The Future of Children,* 17(2), pp. 161-182.

National Institutes of Health, National Heart, Lung and Blood Institute, and Obesity Education Initiative. 2004. *Seventh report of the Joint National Committee on Prevention, Detection, Evaluation, and Treatment of High Blood Pressure.* Washington, DC: National Institutes of Health.

———. 1998. *Clinical guidelines on the identification, evaluation, and treatment of overweight and obesity in adults.* Washington, DC: National Institutes of Health.

National Longitudinal School-Level State Assessment Score Database (NLSLASD) 2010. Retrieved Jan 13, 2011 from http://www.schooldata.org.

National Student Clearinghouse (NSC). 2011. http://www.studentclearinghouse.org/about/aboutus.htm. Accessed on August 22, 2011.

Neal, Derek A. 2004. The relationship between marriage market prospects and never-married motherhood. *Journal of Human Resources* 39(4): 938-57.

Newman, Sandra J., ed. 1999. *The home front: Implications of welfare reform for housing policy.* Washington, DC: Urban Institute.

Newman, Sandra J., and Ann B. Shnare. 1997. "... and a suitable living environment": The failure of housing programs to deliver on neighborhood quality. *Housing Policy Debate* 8(4): 703-41.

Office of Research, Assessment and Evaluation (ORAE). 2010a. "Boston Public Schools: 4-Year Graduation Rate." Accessed August 15, 2011. http://www.bostonpublicschools.org/files/Cohort%202010%204-Y%20Graduation%20Rate%20Report%5B1%5D.pdf.

———. 2010b. "Massachusetts Comprehensive Assessment System Results of Spring 2010." Accessed August 15, 2011. http://www.bostonpublicschools.org/files/2010_mcas_final.pdf.

Offord, David R. 2000. Selection of levels of prevention. *Addictive Behaviors* 25: 833-42.

Olsen, Edgar O. 2003. Housing programs for low-income households. In *Means-tested transfer programs in the United States,* ed. Robert Moffitt. Chicago: University of Chicago Press.

———. 2000. The cost-effectiveness of alternative methods of delivering housing subsidies. Working paper. Charlottesville: University of Virginia, Department of Economics.

Oreopoulos, Philip. 2003. The long-run consequences of living in a poor neighborhood. *Quarterly Journal of Economics* 118(4): 1533-75.

Orr, Larry, Judith D. Feins, Robin Jacob, Erik Beecroft, Lisa Sanbonmatsu, Lawrence Katz, Jeffrey B. Liebman, and Jeffrey R. Kling. 2003. *Moving to Opportunity interim impacts evaluation.* Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research.

Page, Marianne, and Gary Solon. 2003. Correlations between brothers and neighboring boys in their adult earnings: The importance of being urban. *Journal of Labor Economics* 21(4): 831-55.

Patterson, Rhiannon, Michelle Wood, Ken Lam, Satyendra Patrabansh, Gregory Mills, Steven Sullivan, Hiwotte Amare, and Lily Zandniapour. 2004. *Evaluation of the welfare to work voucher program: Report to Congress.* Washington, DC: U.S. Department of Housing and Urban Development.

Pavetti, LaDonna, and Dottie Rosenbaum. 2010. *Creating a safety net that works when the economy doesn't: The role of the food stamp and TANF programs.* Washington, DC: Center on Budget and Policy Priorities.

Pearson, Thomas A., George A. Mensah, R. Wayne Alexander, Jeffrey L. Anderson, Richard O. Cannon III, Michael Criqui, Yazid Y. Fadl, et al. 2003. Markers of inflammation and cardiovascular disease: Application to clinical and public health practice: A statement for healthcare professionals from the Centers for Disease Control and Prevention and the American Heart Association. *Circulation* 107: 499-511.

Peterson, Ruth D., Lauren J. Krivo, and Mark A. Harris. 2000. Disadvantage and neighborhood violent crime: Do local institutions matter? *Journal of Research in Crime and Delinquency.* 37(1): 31-63.

Pickett, Kate E., and Michelle Pearl. 2001. Multilevel analyses of neighborhood socioeconomic context and health outcomes: A critical review. *Journal of Epidemiology and Community Health* 55(2): 111-22.

Piquero, Alex R., David P. Farrington, and Alfred Blumstein. 2003. "The Criminal Career Paradigm." *Crime and Justice* 30: 359-506.

Ponce, Nancy A., Katherine J. Hoggatt, Michelle Wilhelm, and Beate Ritz. 2005. Preterm birth: The interaction of traffic-related air pollution with economic hardship in Los Angeles neighborhoods. *American Journal of Epidemiology* 162(2): 140-48.

Popkin, Sue, Larry F. Buron, Diane K. Levy, and Mary K. Cunningham. 2000. "The Gautreaux Legacy: What Might Mixed- Income and Dispersal Strategies Mean for the Poorest Public Housing Tenants?" *Housing Policy Debate* 11(4): pp. 9-11.

Popkin, Susan J., Laura E. Harris and Mary K. Cunningham. 2002. Families in Transition: A Qualitative Analysis of the MTO Experience. Washington, DC: The Urban Institute. http://www.huduser.org/publications/pdf/mtoqualf.pdf.

Popkin, Sue, Bruce Katz, Mary Cunningham, Karen Brown, Jeremy Gustafson, and Margery Turner. 2004. *A decade of HOPE VI: Research findings and policy challenges.* Washington, DC: Urban Institute.

Popkin, Susan J., Tama Leventhal, and Gretchen Weismann. 2008. *Girls in the 'hood: The importance of feeling safe.* Washington, DC: Urban Institute.

Popkin, Susan J., Tama Leventhal, and Gretchen Weismann. 2010. Girls in the 'hood: How safety affects the life chances of low-income girls. *Urban Affairs Review.*

Quigley, John M. 2000. A decent home: Housing policy in perspective. In *Brookings-Wharton Papers on Urban Affairs,* ed. William G. Gale and Janet Rothenberg Pack. Pp. 53-100. Washington, DC: Brookings Institution.

Quigley, John M., and Steven Raphael. 2008. Neighborhoods, economic self-sufficiency, and the MTO Program. *Brookings-Wharton Papers on Urban Affairs.* Washington, DC: Brookings.

Quinlan, Kyran P. 1996. Injury control in practice. *Archives of Pediatrics Adolescent Medicine* 150: 954-57.

Raphael, Steven, and Jens Ludwig. 2003. "Do prison sentence enhancements reduce gun crime? The case of Project Exile." In *Evaluating Gun Policy,* edited by Jens Ludwig and Philip J. Cook, 251-286. Washington, DC: Brookings Institution Press.

Raudenbush, Stephen W., and Robert J. Sampson. 1999. 'Ecometrics': Toward a science of assessing ecological settings, with application to the systematic social observation of neighborhoods. *Sociological Methodology* 29: 1-41.

Raudsepp, Lennart, and Roomet Viira. 2000. Sociocultural correlates of physical activity in adolescents. *Pediatric Exercise Science* 12(1): 51-60.

Research and Policy Support Group (RPSG). 2010. "NYC 2010 Mathematics and English Language Arts Citywide Test Results." Accessed August 15, 2011. http://schools.nyc.gov/accountability/ Reports/Data/TestResults/2010_MATH_ELA_ NYC_FULL%20DECK.pdf.

Rezaeian, Mohsen, Graham Dunn, Selwyn St. Leger, and Louis Appleby. 2005. The ecological association between suicide rates and indices of deprivation in English local authorities. *Social Psychiatry and Psychiatric Epidemiology* 40: 785-91.

Rhine, Sherrie L.W., Maude Toussaint-Comeau, Jeanne M. Hogarth, and William H. Greene. 2001. The role of alternate financial service providers in serving LMI neighborhoods. In *Changing Financial Markets and Community Development,* A Federal Reserve System Community Affairs Research Conference, Proceedings of a Conference held in Washington, D.C., April 5-6, 2001: 59-80. Chicago, IL: Federal Reserve Bank of Chicago.

Riccio, James A. 2010. Sustained earnings gains for residents in a public housing jobs program: Seven-year findings from the jobs-plus demonstration. Policy Brief. New York: MDRC.

Ridker, Paul M., Julie E. Buring, Nancy R. Cook, and Nadar Rifai. 2003. C-reactive protein, the metabolic syndrome, and risk of incident cardiovascular events. *Circulation* 107:391-97.

Ridker, Paul M., Nadar Rifai, Lynda Rose, Julie E. Buring, and Nancy R. Cook. 2002. Comparison of C-reactive protein and low-density lipoprotein cholesterol levels in the prediction of first cardiovascular events. *New England Journal of Medicine* 347(20): 1557-65.

Rivkin, Steven G., Eric A. Hanushek, and John F. Kain. 2005. Teachers, schools, and academic achievement. *Econometrica* 73(2): 417-58.

Roderick, Melissa, Anthony S. Bryk, Brian A. Jacob, John Q. Easton, and Elaine Allensworth. 1999. Ending Social Promotion: Results from the First Two Years. Chicago: Consortium on Chicago School Research. December 1999.

Rosenbaum, James. 1995. Changing the geography of opportunity by expanding residential choice: Lessons from Gautreaux. *Housing Policy Debate* 6(1): 6231-69.

Rosenbaum, James E. and Stefanie DeLuca. 2008. "Does Changing Neighborhoods Change Lives? The Chicago Gautreaux Housing Program." In David Grusky (Ed.), *Social Stratification: Race, Class and Gender in Sociological Perspective.* Westview Press. pp. 393-399.

Rosenbaum, Emily, and Laura E. Harris. 2003. Short-term impacts of moving for children: Evidence from the Chicago MTO program. In *Choosing a better life: Evaluating the moving to opportunity social experiment,* ed. William Gale and Janet Rothenberg Pack. Washington, DC: Urban Institute.

Rosenbaum, Emily, Laura Harris, and Nancy A. Denton. 1999. New places, new faces: An analysis of neighborhoods and social ties among MTO movers in Chicago. Unpublished manuscript. Available at http://www.nber.org/mtopublic/chicago/mto_chi1.pdf

Rosenbaum, James, and Susan Popkin. 1991. Employment and earnings of low-income blacks who move to middle-class suburbs. In *The urban underclass,* ed. Christopher Jencks and Paul Peterson. pp. 342-256. Washington, DC: Brookings Institution.

Ross, Catherine E., and John Mirowsky. 2001. Neighborhood disadvantage, disorder, and health. *Journal of Health and Social Behavior* 42(3): 258-76.

Rountree, Pamela Wilcox, and Barbara D. Warner. 1999. "Social ties and crime: Is the relationship gendered?" *Criminology.* 37(4): 789-813.

Rubin, Donald B. 1980. Comment on 'Randomization analysis of experimental data in the Fisher randomization test' (by D. Basu). *Journal of the American Statistical Association* 75: 591-593

Rubinowitz, Leonard S., and James E. Rosenbaum. 2000. *Crossing the Class and Color Lines: From Public Housing to White Suburbia.* Chicago: University of Chicago Press.

Saelens, Brian E., James F. Sallis, Jennifer B. Black, and Diana Chen. 2003. Neighborhood-based differences in physical activity: An environment scale evaluation. *American Journal of Public Health* 93(9): 1552-58.

Saguaro Seminar, Kennedy School of Government, Harvard University. 2002. *The social capital benchmark survey (data codebook).* Horsham, PA: TNS Intersearch [producer]. Storrs. CT: Roper Center for Public Opinion Research [distributor].

Sallis, James F., Judith J. Prochaska, and Wendell C. Taylor. 2000. A review of correlates of physical activity of children and adolescents. *Medicine and Science in Sports and Exercise* 32(5): 963-75.

Sallis, James, Brian Saelens, Lawrence Frank, Terry L. Conway, Donald J. Slymen, Kelli L. Cain, James E. Chapman, and Jacqueline Kerr. 2009. Neighborhood built environment and income: Examining multiple health outcomes. *Social Science and Medicine* 68:1285-93.

Sampson, Robert J. 2008. Moving to inequality: Neighborhood effects and experiments meet social structure. *American Journal of Sociology* 114(1): 189-231.

Sampson, Robert J., and Dawn Jeglum Bartusch. 1998. Legal cynicism and (subcultural?) tolerance of deviance: The neighborhood context of racial differences. *Law and Society Review* 32: 777-804.

Sampson, Robert J., Jeffrey D. Morenoff, and Thomas Gannon-Rowley. 2002. Assessing 'neighborhood effects': Social processes and new directions in research. *Annual Review of Sociology* 28: 443-78.

Sampson, Robert J., Jeffrey D. Morenoff, and Stephen Raudenbush. 2005. "Social Anatomy of Racial and Ethnic Disparities in Violence." *American Journal of Public Health* 95: 224-232.

Sampson, R.J., and S.W. Raudenbush. 1999. "Systematic social observation of public spaces: a new look at disorder in urban neighborhoods." *American Journal of Sociology* 105(3): 603-651.

Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls. 1997. Neighborhoods and violent crime: A multilevel study of collective efficacy. *Science* 277: 918-924.

Sanbonmatsu, Lisa, Jeffrey R. Kling, Greg J. Duncan, and Jeanne Brooks-Gunn. 2006. Neighborhoods and academic achievement: Results from the Moving to Opportunity experiment. *Journal of Human Resources* 41(4): 649-91.

Schachter, Jason P. 2004. *Geographical mobility: 2002 to 2003. Population characteristics.* Current Population Report P20-549. Washington, DC: U.S. Bureau of the Census.

Scharfstein, Joshua, and Megan Sandel. 1998. *Not safe at home: How America's housing crisis threatens the health of its children.* Boston: Boston Medical Center Children's Hospital and the Doc4Kids Project.

Schelling, Thomas C. 1978. *Micromotives and macrobehavior.* New York: Norton.

———. 1971. Dynamic models of segregation. *Journal of Mathematical Sociology* 1: 143-86.

Schill, Michael, and Susan Wachter. 1995. The spatial bias of federal housing law and policy: Concentrated poverty in urban America. *University of Pennsylvania Law Review* 143(5): 1285-1342.

Schneider, Harald, Heide Glaesmer, Jens Klotsche, Steffen Bohler, Hendrik Lehnert, Andreas Zeiher, Winfried Marz, David Pittrow, Gunter K. Stalla, and Hans-Ulrich Wittchen. 2006. Accuracy of anthropometric indicators of obesity to predict cardiovascular risk. *Journal of Clinical Endocrinology and Metabolism* 92(2): 589-94.

School Information Branch Planning, Assessment, and Research (SIBPAR). No Date. "District Accountability Report Card 2006-2007." Prepared for the Los Angeles Unified School District. Accessed August 15, 2011. www.lausd.k12.ca.us/ lausd/offices/bulletins/ldstldtz.html.

Schott, Liz. 2009. *An introduction to TANF.* Washington, DC: Center for Budget and Policy Priorities.

Shannon, Lyle. 1982. "Assessing the Relationship of Adult Criminal Careers to Juvenile Careers: A Summary." Washington, DC: U.S. Department of Justice.

Sharkey, Patrick. 2010. The acute effect of local homicide on children's cognitive performance. *Proceedings of the National Academy of Sciences.* 107(26): 11733-38.

————. 2008. The intergenerational transmission of context. *American Journal of Sociology* 113(4): 931-69.

————. 2006. "Navigating dangerous streets: The sources and consequences of street efficacy." *American Sociological Review* 71: 826-846.

Sherman, Lawrence. 2002. Fair and effective policing. In *Crime: Public policies for crime control,* ed. James Q. Wilson and Joan Petersilla. Pp. 383-412. Oakland, CA: ICS Press.

Shonkoff, Jack P., and Deborah A. Phillips. 2000. *From neurons to neighborhoods: The science of early childhood development.* Washington, DC: National Academy Press.

Shroder, Mark. 2002a. Locational constraint, housing counseling, and successful lease-up in a randomized housing voucher experiment. *Journal of Urban Economics* 51(2): 315-38.

————. 2002b. Does housing assistance perversely affect self-sufficiency? A review essay. *Journal of Housing Economics* 11(4): 381-417.

Shroder, Mark, and Arthur Reiger. 2000. Vouchers versus production revisited. *Journal of Housing Research* 11(1): 91-107.

Silver, Eric, Edward P. Mulvey, and Jeffrey W. Swanson. 2002. Neighborhood structural characteristics and mental disorder: Faris and Dunham revisited. *Social Science & Medicine* 55(8): 1457-70.

Simons, L.G., R.L. Simons, R.D. Conger, and G.H. Brody. 2004 "Collective socialization and child conduct problems: A multilevel analysis with an African American sample." *Youth and Society* 35(3): 267-292.

Simpson, Gloria A., Barbara Bloom, Robin A. Cohen, Stephen Blumberg, and Karen H. Bourdon. 2005. U.S. children with emotional and behavioral difficulties: Data from the 2001, 2002, and 2003 national health interview surveys. *Advance Data from Vital and Health Statistics* 360: 1-13.

Skogan, Wesley G. 1990. *Disorder and decline: Crime and the spiral of decay in American neighborhoods.* Berkley and Los Angeles, CA: University of California Press.

Sobel, Michael E. 2006. What do randomized studies of housing mobility demonstrate? Causal inference in the face of interference. *Journal of the American Statistical Association.* 101(476): 1398-1407.

Srinivasan, Sathanur R., Ren Wang, Wei Chen, Christine Y. Wei, Jihua Xu, and Gerald S. Berenson. 2009. Utility of waist-to-height ratio in detecting central obesity and related adverse cardiovascular risk profile among normal weight younger adults (from the Bogalusa Heart Study). *American Journal of Cardiology* 104(5): 721-24.

Steinberg, Laurence, Ilana Blatt-Eisengart, and Elizabeth Cauffman. 2006. "Patterns of competence and adjustment among adolescents from authoritative, authoritarian, indulgent, and neglectful homes: A replication in a sample of serious juvenile offenders." *Journal of Research on Adolescence* 16(1): 47-58.

Subramanian S.V., J. T. Chen, D. H. Rehkopf, P. D. Waterman, and N. Krieger. 2006. Comparing individual- and area-based socioeconomic measures for the surveillance of health disparities: A multilevel analysis of Massachusetts births 1989-1991. *American Journal of Epidemiology* 164(9): 823-34.

Substance Abuse and Mental Health Services Administration. 2010. Results from the 2009 national survey on drug use and health. Volume 1: Summary of national findings. NSDUH Series H-38A, HHS publication no. SMA 10-4856. Rockville, MD: Office of Applied Studies.

Swanson, Christopher. 2004. "Who Graduates? Who Doesn't? A Statistical Portrait of Public High School Graduation, Class of 2001." Prepared for the Urban Institute's Education Policy Center. Urban Institute: Washington, DC.

Swanson, Christopher. 2009. *Cities in Crisis 2009: Closing the Graduation Gap – Educational and Economic Conditions in America's Largest Cities.* Educational Projects in Education, Bethesda, MD. www.edweek.org/co/copies.

Temkin, Kenneth, and Noah Sawyer. 2004. *Analysis of alternative financial service providers.* Washington, DC: Fannie Mae Foundation.

Thompson, Estina E., and Neal Krause. 2000. Living alone and neighborhood characteristics as predictors of social support later in life. *The Journals of Gerontology Series B: Psychological Sciences and Social Sciences* 55: S245-53.

Thornberry, Terence P., David Huizinga, and Rolf Loeber. 2004. "The causes and correlates studies: Findings and policy implications." *Juvenile Justice* 9: 3-19.

Tillman, Robert. 1987. "The Size of the 'Criminal Population': The Prevalence and Incidence of Adult Arrest." *Criminology* 25(3): 561- 579.

Torres, Susan, and Caryl Nowson. 2007. Relationship between stress, eating behavior, and obesity. *Nutrition* 23: 887-94.

Tracy, Paul, Marvin Wolfgang, and Robert Figlio. 1985. "Delinquency in Two Birth Cohorts: Executive Summary." Rockville, MD: Office of Juvenile Justice and Delinquency Prevention.

Trogdon Justin G., and Thomas Hylands. 2008. Nationally representative medical costs of diabetes by time since diagnosis. *Diabetes Care* 31(12): 2307-2311.

Turner, Margery, and Kingsley G. Thomas. 2008. Federal Programs for Addressing Low-Income Housing Needs. Washington, DC: Urban Institute.

Turney, Kristin, Susan Clampet-Lundquist, Kathryn Edin, Jeffrey R. Kling, and Greg J. Duncan. 2006. Neighborhood effects on barriers to employment: Results from a randomized housing mobility experiment in Baltimore. Brookings-Wharton Papers on Urban Affairs. Washington, DC: Brookings.

U.S. Census Bureau. 2004. Census Special Reports, AHS/R/04-1, Housing Data Between the Censuses: The American Housing Survey. Washington, DC.

U.S. Department of Housing and Urban Development (HUD). 2011. Affordable Housing needs 2009: Report to Congress. Washington DC: HUD.

———. 2003. Choice voucher location patterns: Implications for participant and neighborhood welfare. Washington, DC: HUD, Office of Policy Development and Research.

———. 2000. Economic cost analysis of different forms of assisted housing. Issue Brief 11. Washington, DC: HUD, Office of Policy Development and Research.

———. 1997. *Characteristics of HUD-assisted renters and their units in 1993.* Washington, DC: HUD.

U.S. Department of Education. 2011a. National Center for Education Statistics. Common Core of Data (CCD). Public Elementary/Secondary School Universe Survey. 1993-2010. Retrieved May 18, 2011 from http://nces.ed.gov/ccd.

———. 2011b. National Center for Education Statistics. Private School Universe Survey (PSS). 1993–2010. Retrieved May 27, 2011, from http://nces.ed.gov/surveys/pss/.

van den Berg, Agnes E., Jolanda. Maas, Robert A. Verheij, and Peter P. Groenewegen. 2010. Green space as a buffer between stressful life events and health. *Social Science and Medicine* 70: 1203-10.

van der Heide, S., H.F. Kauffman, A.E.J. Dubois and J.G.R. de Monchy. 1997. Allergen-avoidance measures in homes of house-dust-mite-allergic asthmatic patients: Effects of acaricides and mattress encasings. *Allergy* 52(9): 921-27.

van Kamp, Irene, Jeanne van Loon, Mariel Droomers, and Augustinus de Hollander. 2004. Residential environment and health: A review of methodological and conceptual issues. *Reviews on Environmental Health* 19: 381-401.

Vermilyea, Todd, and James A. Wilcox. 2002. Who is unbanked and why: Results from a large, new survey of low-and-moderate income adults. Chicago: Federal Reserve Bank of Chicago, Conference on Bank Structure and Competition.

Votruba, Mark E., and Jeffrey R. Kling. 2009. Effects of neighborhood characteristics on the mortality of black male youth: Evidence from Gautreaux. *Social Science and Medicine* 68(5): 814-23.

Waitzman, Norman J., and Ken R. Smith. 1998. Phantom of the area: Poverty-area residence and mortality in the United States. *American Journal of Public Health* 88: 973-76.

Warner, Barbara D., and Pamela Wilcox Rountree. 1997. "Local Social Ties in a Community and Crime Model: Questioning the Systematic Nature of Informal Social Control." *Social Problems* 44(4): 520-536.

Watson, Tara, 2009. Inequality and the measurement of residential segregation by income. *Review of Income and Wealth* 55: 820-844.

Weaver, Ian C. G. 2009. Shaping adult phenotypes through early life environments. *Birth Defects Research Part C: Embryo Today* 87: 314-26.

Weinberg, Bruce, Patricia B. Reagan, and Jeffrey J. Yankow. 2004. Do neighborhoods affect work behavior? Evidence from the NLSY79. *Journal of Labor Economics* 22(4): 891-924.

Whitley, Elise, David Gunnell, Daniel Dorling, and George D. Smith. 1999. Ecological study of social fragmentation, poverty, and suicide. *British Medical Journal* 319: 1034-37.

WHO World Mental Health Survey Consortium. 2004. Prevalence, severity, and unmet need for treatment of mental disorders in the World Health Organization world mental health surveys. *Journal of the American Medical Association* 291(21): 2581-90.

Widom, Cathy S. 1989. "Child Abuse, Neglect, and Violent Criminal Behavior." *Criminology* 27(2): 251-271.

Wiener, Joshua M., Raymond J. Hanley, Robert Clark, and Joan F. Van Nostrand. (1990). Measuring the activities of daily living—Comparisons across national surveys. *Journal of Gerontology* 45(6): S229-37.

Wildman, Rachel P. Paul Muntner, Kristi Reynolds, Aileen P. McGinn, Swapnil Rajpathak, Judith Wylie-Rosett, and MaryFran R. Sowers. 2008. The obese without cardiometabolic risk factor clustering and the normal weight with cardiometabolic risk factor clustering: Prevalence and correlates of 2 phenotypes among the US population. NHANES 1999-2004. *Archives of Internal Medicine* 168: 1617-24.

Wilson, William Julius. 1996. *When Work Disappears: The World of the New Urban Poor.* New York: Alfred A. Knopf.

———. 1987. *The truly disadvantaged: The inner city, the underclass, and public policy.* Chicago: University of Chicago Press.

Wright, Rosalind J. 1998. Review of psychosocial stress and asthma: An integrated biopsychosocial approach. *Thorax* 53: 1066-74.

Wikström, Per-Olof., and Rolf Loeber. 2000. "Do disadvantaged neighborhoods cause well-adjusted children to become adolescent delinquents? A study of male juvenile serious offending, risk and protective factors, and neighborhood context." *Criminology.* 38: 1109-1142.

Wolfgang, Marvin E. 1983. "Delinquency in two birth cohorts." *American Behavioral Scientist* 27: 75-86.

Wolfgang, Marvin E., Robert M. Figlio, and Thorsten Sellin. 1972. *Delinquency in a Birth Cohort.* Chicago: University of Chicago Press.

Zapata, B. Cecilia, Annabella Rebolledo, Eduardo Atalah, Beth Newman, and Mary-Claire King. 1992. The influence of social and political violence on the risk of pregnancy complications. *American Journal of Public Health* 82(5): 685.

Zaslow, M.J., and C.D. Hayes. 1986. Sex differences in children's response to psychosocial stress: Toward a cross-context analysis. In *Advances in Developmental Psychology,* vol. 4, ed. M. Lamb, A. Brown, and B. Rogoff. Hillsdale, NJ: Lawrence Erlbaum.

Zenk, Shannon N., Amy J. Schulz, Barbara A. Israel, Sherman A. James, Shuming Bao, and Mark L. Wilson. 2005. Neighborhood racial composition, neighborhood poverty, and the spatial accessibility of supermarkets in metropolitan Detroit. *American Journal of Public Health* 95(4): 660-67.

Zimmerman, Gregory M. and Steven F. Messner. 2011. "Neighborhood context and nonlinear peer effects on adolescent violent crime." *Criminology* 49(3): 873-904.

November 2011