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Moving to OPPORTUNITY *for* *Fair Housing Demonstration Program*

Interim Impacts Evaluation

Moving to Opportunity
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Prepared for:
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Table of Contents

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Executive Summary

In 2000, 3.5 million poor people across the United States lived in neighborhoods with poverty concentrations in excess of 40 percent. A growing social science literature suggests that such concentration has a variety of detrimental effects on the residents of these areas in terms of both their current well-being and their future opportunities. The harmful effects of high-poverty areas are thought to be especially severe for children whose behavior and prospects may be particularly susceptible to a number of neighborhood characteristics, such as peer group influences, school quality, and the availability of supervised after school activities.

Less has been written about whether and how other neighborhood environments exert positive influences on behavior and life changes. Ellen and Turner (1997) summarize the literature in this area, citing various theories about the mechanisms by which middle-class (often predominantly white) neighborhoods shape or reshape the lives of their residents.

This study reports interim results from a major federal initiative to explore whether living in better neighborhoods can improve the lives of low-income parents and children. That initiative is the Moving to Opportunity for Fair Housing Demonstration, originally mandated by Congress and carried out by the U.S. Department of Housing and Urban Development (HUD).

Introduction to Moving to Opportunity

Moving to Opportunity (MTO) was designed to answer questions about what happens when very poor families have the chance to move out of subsidized housing in the poorest neighborhoods of five very large American cities. MTO was a demonstration program: its unique approach combined tenant-based housing vouchers (from the Section 8 program[1]) with location restrictions and housing counseling. MTO was also a randomized social experiment, carefully designed and rigorously implemented to test the effects of this approach on participating families.

Between 1994 and 1998, the housing authorities in five demonstration sites—Baltimore, Boston, Chicago, Los Angeles, and New York—worked in partnership with local nonprofit counseling organizations to recruit about 4,600 very low-income families for MTO. The families, all of whom lived in public housing or private assisted housing projects in the poorest parts of these cities, responded to outreach that offered them a chance to move with housing vouchers from their current homes and neighborhoods. Exhibit ES.1 summarizes key facts about demonstration implementation.

The demonstration sites shared some characteristics, including the presence of large, distressed public housing developments in concentrated poverty neighborhoods (where more than 40 percent of the population lived below the poverty line). The cities differed in other ways: in the racial and ethnic

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[1] In 1999 the Section 8 program was renamed the Housing Choice Voucher Program. In this report we will continue to refer to the program as *Section 8*, because the rules of the demonstration were set under that program.

composition of their eligible populations and in the nature of their housing markets. Despite these differences, the demonstration was implemented with considerable uniformity, particularly with respect to recruitment, informed consent of participants, issuance of vouchers, and the rules governing their use. Through joint training, central oversight, and regular monitoring and data collection, HUD made sure that the procedures developed for MTO were carefully followed.

EXHIBIT ES.1

Moving to Opportunity Implementation—Basic Facts

- **Origin—**The MTO demonstration was funded by Congress, with $70 million in Section 8 rental assistance for fiscal year 1992 (carried over to FY93), with additional vouchers allocated by participating housing authorities and with additional funds from the local housing authorities and nonprofit counseling agencies.
- **Sites—**Baltimore, Boston, Chicago, Los Angeles, and New York City.
- **Family eligibility—**Families had to live in public housing or private assisted housing in areas of the central cities with very high poverty rates (40 percent or more), have very low incomes, and have children under 18 years old.
- **Program size—**Among those who applied for the program between June 1994 and July 1998, 4,608 families were found to be eligible. Of those, 3,169 families were offered vouchers and 1,676 were able to find a unit and successfully move.
- **Continuous tracking—**HUD has been working to keep in touch with the MTO families since they joined. In 2002 researchers contacted almost 8,900 adults and children for this study. Taking into account a subsample of hard-to-find families, the effective response rate for the interim evaluation is 89 percent.

A key reason for developing special procedures and making sure they were uniformly implemented was that MTO was a randomized social experiment as well as a demonstration program. The critical feature of MTO's research design was random assignment of the families who joined the demonstration (with their informed consent). Each family was randomly assigned to one of three groups:

- The **experimental group** was offered housing vouchers that could only be used in low-poverty neighborhoods (where less than 10 percent of the population was poor). Local counseling agencies helped the experimental group members to find and lease units in qualifying neighborhoods.
- The **Section 8 group** was offered vouchers according to the regular rules and services of the Section 8 program at that time, with no geographical restriction and no special assistance.
- Finally, **control group** members were not offered vouchers but continued to live in public housing or receive other project-based housing assistance.

To use their vouchers, families assigned to the experimental group had to move to low-poverty areas. Those in the Section 8 group could use their vouchers to move to neighborhoods of their own choosing. Both groups were required to make these moves within a limited amount of time. In order to retain their vouchers, experimental families were required to stay in low-poverty areas for one year, after which they could move without locational constraints.

Exhibit ES.2 summarizes the key features of MTO's research design. Random assignment makes the three groups of participating families statistically the same, so that any later significant differences (differences greater than chance would produce) in the neighborhoods, housing, employment, or other aspects of the experimental group's lives in comparison with the control group can be attributed to the MTO intervention. Of course, such differences should only be attributed to MTO if there are social scientific hypotheses suggesting that changing location can influence these outcomes. And in fact, a considerable theoretical foundation does exist for the MTO experiment (as described below).

EXHIBIT ES.2

MTO Experimental Design—Basic Facts

- **Research objective—**to test the long-term effects on adult and child well-being when families move from public or project-based assisted housing in very poor areas to private-market rental housing in areas with much lower poverty rates.

- **Experimental design—**random assignment of the families who joined the program to one of three groups:

  – an **experimental group,** which received Section 8 vouchers useable only in low-poverty areas (census tracts with less than 10 percent of the population below the poverty line in 1990), along with counseling and assistance in finding a private rental unit.
  – a **Section 8 group,** which received regular vouchers (geographically unrestricted) and whatever briefing and assistance the local Section 8 program regularly provided.
  – a **control group,** which received no vouchers but continued receiving project-based assistance.

- **Longitudinal study—**By following the families over a period of about 10 years, collecting data on various aspects of the adults' and children's lives, and comparing the experiences of each treatment group to that of the control group, the experiment would permit answers to these vital questions:

  – What are the impacts of joining the MTO demonstration on household location and on the housing and neighborhood conditions of the participants?
  – What are the impacts of moving to a low-poverty neighborhood on the employment, income, education, health, and social well-being of family members?

MTO eligibility was targeted to residents of project-based subsidized housing in neighborhoods with poverty rates of 40 percent or more. The mean poverty rate of baseline locations was, in fact, much higher at 56 percent. And a substantial proportion of MTO families were living in severely distressed public housing when they joined, including a number of the earliest developments to be demolished under the HOPE VI program.

After random assignment, members of the experimental group received their geographically restricted vouchers and worked with the local nonprofit counseling agencies to prepare for and conduct their housing searches in low-poverty areas. Just under half of the experimental group families moved to low-poverty areas with MTO vouchers. Families in the Section 8 group received their regular vouchers and housing authority briefings and assistance and then searched for housing on their own. Just over 60 percent of this group was able to use the MTO vouchers, which required moving to other housing but without the restriction to low-poverty areas. After random assignment, members of the control group continued to live in their project-based subsidized housing in these areas of great poverty. The nonmovers in both the experimental and Section 8 groups also initially remained in their baseline public or assisted housing units.

Despite its unique aspects, the MTO experiment can tell us a great deal about HUD's main current housing programs. While not representative of public housing nationwide, the conditions of distress and concentrated poverty where the families were living when they joined MTO were not uncommon in big city public housing across the country. By offering tenant-based subsidies (vouchers) to such families, MTO provides a test of what difference it might make to switch very low-income families from place-based to mobile subsidies. At the present time, these are the major forms of low-income rental assistance with about 1.1 million families and individuals living in public housing, 1.5 million households in privately owned assisted projects, and 1.8 million households using vouchers. By constraining the experimental group to move to low-poverty communities, MTO was testing whether vouchers can be a vehicle for substantial changes in neighborhood environment. If the long-term results of MTO research show significant improvements in the well-being and life chances of experimental group members, we will have learned that housing vouchers can provide access to meaningful opportunities for poor families.

Of course, policies designed to move low-income families from public housing in high-poverty areas to private housing in low-poverty areas can take forms other than the location-restricted vouchers used in MTO. Mobility counseling or other supports for moving to low-poverty areas could be incorporated into the regular voucher program. HUD could create goals and performance incentives for program administrators to encourage moves to opportunity areas, and both assisted and affordable housing in low-poverty areas can be created or preserved through decisions with respect to state agency refinancing policies, allocations of low-income housing tax credits, use of HOME funds, public housing authority (PHA) project basing of vouchers, and other existing housing programs and policies.

Context of MTO

Policy and social science background

Recent interest in geographic location and mobility as important factors shaping the futures of low-income families began with the results of the Gautreaux Program, a federal court-ordered racial desegregation program in Chicago. Under the name of tenant activist Dorothy Gautreaux, applicants and residents of Chicago public housing brought a class-action housing segregation lawsuit against HUD and the Chicago Housing Authority (CHA) in 1966. The courts ordered HUD and CHA to remedy the extreme racial segregation they had imposed on public housing applicants and residents by providing (among other remedies) a housing mobility option throughout the Chicago region for about 7,100 black families.

This option became known as the Gautreaux Program, which took shape in the late 1970s. Participating families were helped to move out of racially isolated areas through the (then new) tenant-based Section 8 program. Families chosen for the Gautreaux program received Section 8 certificates[2] that required them to move either to predominantly white or racially mixed neighborhoods. They also received assistance from housing counselors to make these moves.

Beginning in the late 1980s, research on the Gautreaux Program suggested that, over time, the moves to less segregated suburban locations were associated with measurable improvements in the lives of participating adults and children. Researchers found that suburban movers were more likely to have been employed than city movers. Positive changes were also reported for small samples of children who had been living in less segregated neighborhoods. Although they had initially experienced declines in school performance, in the long run (7 to 10 years) such children were less likely to drop out of school and were more likely to take college-track classes than their peers in a comparison group who moved to city neighborhoods, which were both poorer and more racially segregated than the suburban locations. After graduating from high school, the Gautreaux children were also more likely than their city peers to attend a 4-year college or become employed full-time.

At roughly the same time, several influential studies were drawing attention to the increasing concentration of poverty and the harm done to residents of high-poverty areas, in terms of both their current well-being and their future opportunities. The Gautreaux research excited great interest in both social scientific and policy circles because it seemed to suggest that there were remedies to the damaging effects of life in concentrated poverty neighborhoods. Yet the Gautreaux findings were limited by the fact that the causal link between the new residential locations and the improvements was not certain: The observed differences might reflect differences between the kinds of people who moved to the suburbs through Gautreaux and those who moved within the city rather than reflecting the effects of the different residential locations. Because this was a nonexperimental comparison of families who moved to different types of neighborhoods, there was a serious risk of selection bias in drawing conclusions from such a comparison.

MTO was designed to be the experiment that directly and rigorously tests whether moves to low-poverty areas can bring about positive changes in the lives of poor families. Because families in MTO were randomly assigned, the three groups started out comparable by definition. And as long as comparisons made thereafter are based on the three groups as a whole (all their members, not just movers), the risk of selection bias is eliminated.

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[2] The form of the voucher program current at that time.

Prior studies of MTO

Research on MTO began while the operational phase of the demonstration was still under way. HUD issued a first report to Congress once all the sites had begun enrolling and counseling families. Observations and analyses of the counseling delivered to experimental group families through MTO were documented about midway through the operations period. When enrollment and lease-up ended in 1999, HUD reported initial findings about the participating families and the program moves made by experimental and Section 8 group families.

In 1997 HUD's Office of Policy Development and Research conducted an MTO grant competition and ultimately made eight small awards to teams of researchers with varied topics and approaches. Each team was given access to the MTO participants in one of the five sites for purposes of assessing different aspects of the families' early experiences there. The small grant research results suggested that the demonstration might well be having—at least in the short term—impacts on such dimensions as health, safety, delinquency patterns, and educational outcomes. The early studies did not find any employment or other economic effects.

An important contribution of this research was to suggest the appropriate breadth of a full-scale evaluation. But because the timing of program entry extended from 1994 to 1998 and because each study was done in a single site, the small grant research needed to be followed by more comprehensive and uniform research when more time had elapsed for the families in the program. It was clear that the MTO design and sample could be used to learn about a wide range of topics. It was equally clear that many questions remained to be answered.

The Interim Evaluation

The present study—the MTO interim evaluation—was designed to examine MTO's impacts at about the midpoint of the 10-year research period originally mandated by Congress. A final impact evaluation will be conducted approximately a decade after the end of program operations. This interim research does not utilize the entire MTO program population because the families that joined MTO in 1998 (and in some cases did not move until early 1999) had less than 4 years exposure to the program after random assignment. The final evaluation will include the entire set of families in MTO.

The interim evaluation has two major components, one using qualitative methods and the other using quantitative methods, to assess MTO's effects in six study domains:

1. Mobility, housing, and neighborhood
2. Adult and child physical and mental health.
3. Child educational achievement.
4. Youth delinquency and risky behavior.
5. Adult and youth employment and earnings.
6. Household income and public assistance receipt.

The main goals of the qualitative research were to help enrich our understanding of how neighborhood affects families, to help illuminate the mechanisms that underlie such effects, and to assist in the interpretation of the quantitative findings from the analysis of the survey and administrative data.

The central quantitative objective is to estimate the impacts of the housing vouchers received by the experimental and Section 8 groups—after 4 to 7 years—on a wide range of outcomes across the six domains. MTO's random assignment design ensures that the measured differences can be attributed to the demonstration intervention and not to differences in the families' characteristics or motivation.

However, it is certainly too soon to conclude that the absence of significant differences in one or more domains means MTO had no impact. In its timing, this study is directed at relatively short-term or midterm effects, those most immediately associated with changes in residential location. The final evaluation (after 10 years) may show that the midterm effects have (or have not) endured. And it may detect additional effects that took longer to appear.

The questions addressed in this interim evaluation are of great importance. To what extent are the adverse outcomes associated with living in very poor neighborhoods the products of the neighborhoods rather than of the characteristics of those living there? If the adverse outcomes are products of the neighborhoods, to what extent do opportunity moves to areas with minimal poverty offer a means of ameliorating them? If public housing residents are given unrestricted tenant-based housing assistance, do they make locational choices that afford them access to some or all of the same life improvements as opportunity moves?

But MTO can teach us even more. They also offer a perspective on the importance of creating or preserving assisted housing in low-poverty locations. This latter point is relevant to quite a number of current housing policy issues and initiatives affecting new and existing private project-based assisted housing: mark to market, mark-up-to-market, state agency refinancing policies, allocation of Low Income Housing Tax Credits, use of HOME funds, and PHA project basing of vouchers. The MTO research results addressing these important questions could help inform social policy in the United States for years to come.

Study Findings on MTO Mobility

The move out of public housing into a low-poverty neighborhood is intended to expose the experimental group to an environment that might improve life chances. The move to private market housing—whatever the neighborhood—is intended to expose the Section 8 group to an environment that might also alter future paths, as compared to the lives of those who remain, at least initially, in project-based public or assisted housing in high-poverty areas.

Families in all three groups may have moved since random assignment. These moves could result from changes in peoples' lives related to MTO—such as increased employment and earnings—and they could in turn affect the outcomes in other areas such as education or housing assistance. Thus, it was important to examine both initial and subsequent moves as they relate to the outcomes of interest to this study.

In this section, we first present estimated impacts for the entire experimental group or Section 8 group randomly assigned, including those who did not lease up, and then show the corresponding findings for the families who did move with program vouchers. The former estimates show the effect of the demonstration on the entire group offered vouchers, the latter on those who actually experienced a program-induced change in residential location.

We found that MTO had substantial, positive effects on the mobility of families in the experimental and Section 8 groups and on the characteristics of the neighborhoods in which they lived. Almost half of the families assigned to the experimental group leased up with program vouchers, as did more than three-fifths of the families in the Section 8 group (Exhibit 2.1). To use the voucher, experimental group families were required to move to census tracts with poverty rates below 10 percent in the 1990 Census. Because many moved to neighborhoods where the poverty rate was increasing between 1990 and 2000, we estimate that only about half of their destinations had poverty rates below 10 percent at the time of the move, although virtually all had rates below 20 percent (Exhibit 2.3). Among the Section 8 group, who could use the voucher anywhere they could find housing that met Section 8 quality standards (with a rent they could afford and a willing landlord), fewer than 30 percent of those who moved with program vouchers moved to census tracts with poverty rates below 20 percent, although the overwhelming majority moved to neighborhoods with lower poverty rates than the areas where they had lived in public housing.

As noted earlier, the experimental families were only constrained to live in low-poverty areas for one year. By the time of the interim evaluation, these differentials in poverty rates had narrowed somewhat, in part because of subsequent moves by the experimental families and in part because of changes over time in neighborhood poverty rates, but they had by no means disappeared. Among those who moved with program vouchers, 60 percent of the experimental group families were still in census tracts with poverty rates below 20 percent, while 30 percent of the Section 8 families were in such tracts (Exhibit 2.5). The treatment-control differentials had narrowed as well, in part as a result of changes in the poverty rates of the neighborhoods where treatment group families resided but also because over two-thirds of the control group families had moved (either on their own or in connection with public housing demolition or redevelopment—e.g., as part of the HOPE VI program). By the time of the interim evaluation, about 17 percent of the control group families lived in census tracts with poverty rates below 20 percent, and just over half lived in tracts with rates below 40 percent.

It is noteworthy that even those families who moved to low-poverty areas did not necessarily move to predominantly white or racially integrated areas. Among families in the Section 8 group, at the time of the interim evaluation over three quarters of both those who moved with program vouchers and those who did not were living in census tracts that were over 80 percent minority, about the same proportion as among control group families (Exhibit 2.6). Among experimental families, 60 percent of those who moved with program vouchers were in heavily minority areas. For minority families in the experimental group who moved with program vouchers, the experiment reduced the average percent minority in their neighborhood by less than 10 percentage points. There was no significant effect on this measure for Section 8 families (Exhibit 2.8).

These mobility patterns resulted in a number of significant improvements in the environment in which experimental group families lived and lesser improvements for Section 8 group families. At the time of the interim evaluation, experimental group families who moved with program vouchers lived in neighborhoods with higher proportions of adults employed, substantially higher proportions of two-parent families and high school graduates, and nearly twice the rate of homeownership as in the neighborhoods where they would have lived absent the demonstration, as evidenced by where the controls lived (Exhibit 2.10). Section 8 group families who moved with program vouchers also saw significant gains in these neighborhood attributes, but those gains were generally only about half as large as those experienced by experimental group families.

These changes in the neighborhood environment substantially increased the chances that adults in experimental group families would have college educated friends or friends earning $30,000 or more (Exhibit 2.10). There was no significant effect on these outcomes for adults in Section 8 families, who lived in somewhat higher poverty areas than the families in the experimental group.

Evidence about Short to Mid-Term Effects of MTO

Among the expected impacts of the MTO demonstration, some might occur in the short term (1 to 3 years), others in the middle term (perhaps 5 to 6 years), while still others would not be expected to occur until more time had passed for the people in the program. We expected short- to midterm effects on housing, neighborhood, safety, health, and delinquency (based on the earlier MTO research).

Improved housing, neighborhood conditions, and safety

The families who moved with program vouchers markedly improved their neighborhood conditions, reporting large reductions in the presence of litter, trash, graffiti, abandoned buildings, people "hanging around," and public drinking, relative to the control group (Exhibit 3.5). They also reported that they had less difficulty getting police to respond to their calls. The proportion of families who expressed satisfaction with their current neighborhoods was much higher in both treatment groups than in the control group. On every one of these measures, the proportion of the experimental group reporting improved conditions was about 10 percentage points larger among the Section 8 group.

Perhaps most notable from the perspective of the families themselves is the fact that they were successful in achieving the goal that loomed largest in their motivation to move out of their old neighborhoods: improvements in safety. The adults reported substantial increases in their perception of safety in and around their homes and large reductions in the likelihood of observing or being victims of crime (Exhibit 3.5). These gains were greater for the experimental group families, but they were still substantial for those in the Section 8 group who moved with program vouchers.

MTO substantially improved the quality of housing occupied by the families who moved with program vouchers. A markedly higher proportion of adults voiced satisfaction with their housing at the time of the interim evaluation, compared to the control group—21 percent more for the experimental group adults and 12 percent more for the Section 8 group adults (Exhibit 3.5). MTO also increased somewhat the proportion of families receiving housing subsidies, while substantially reducing the fraction living in public housing (Exhibit 3.4). However, some of this effect was probably due to the impacts of HOPE VI and Vacancy Consolidation on a number of the developments where the control group lived during the period since random assignment.

In sum, the MTO demonstration succeeded in substantially improving the housing and residential environments of the families who moved with program vouchers on a wide range of measures. While these improvements were greater for the experimental group, who were constrained to move to low-poverty areas at least initially, the Section 8 group also experienced sizeable improvements in housing and neighborhood environment relative to the control group.

Improvements in adult and child health

Urban residents of high-poverty neighborhoods are likely to have high incidences of health problems. The high rates of activity limitations, asthma, high blood pressure, obesity, psychological distress, depression, and anxiety observed in the control group at the time of the interim evaluation bear out this expectation (Exhibit 4.2).

Estimation of MTO's impacts on these outcomes and on measures of smoking, drinking, and general physical health revealed one significant impact on adults' physical health: a large reduction in the incidence of obesity among both experimental and Section 8 families (Exhibit 4.2). There were also improvements in mental health among adults in the experimental group families: a reduction in psychological distress, a reduction in depression (statistically significant on one measure of depression though not on the other), and an increase in feelings of calm and peacefulness. There were no significant mental health improvements among those in the Section 8 group and there were no significant effects on the other adult health measures in the interim evaluation among those in either the experimental or Section 8 group.

Among children, the significant effects of MTO on health were confined to mental health measures—a moderately large reduction in psychological distress for girls in the experimental group; a substantial decrease in the incidence of depression among girls in the Section 8 group; and very large reductions in the incidence of generalized anxiety disorder among girls in both treatment groups (Exhibit 4.5). These findings of significant impacts on measures of mental health, for both adults and children, are consistent with the improvements in the families' perceptions of personal safety discussed above.

Mixed effects on youth delinquency and risky behavior

At baseline, when the children who were ages 15 to 19 at the time of the interim evaluation were ages 8 to 15, significant proportions had already exhibited problem behavior or been suspended from school. By the time of the interim evaluation, among youth in this age range, 24 percent of the girls and 39 percent of the boys in the control group had been arrested—half of them for violent crimes (Exhibit 5.3).

In the interim evaluation, survey data from parents and from the youth themselves were used to measure a number of delinquent, risky, and problem behaviors. The youth were also asked whether they had ever been arrested. In addition, administrative data from the criminal justice system were used to measure the number of arrests for specific crimes.

For both boys and girls in the experimental and Section 8 groups, there were no significant effects on either an index of 15 problem behaviors reported by parents or on a narrower index of self-reported delinquent behaviors related to criminal behavior (Exhibit 5.2). However, there were significant increases in self-reported behavior problems among boys ages 12 to 19, in both treatment groups.

Participation in MTO resulted in a large reduction in the proportion of girls ages 15 to 19 in the Section 8 group who had ever been arrested for violent crimes (Exhibit 5.3). This effect contributed to a significant reduction in the frequency of arrests for violent crimes for all youth (Exhibit 5.4). There were no effects on the incidence of arrests for other crimes for girls. The only effects on arrests for boys were very substantial increases in the proportion ever arrested and the frequency of arrests for property crimes in the experimental group (Exhibits 5.3 and 5.4). This increase in arrests might reflect more stringent policing in new locations, rather than (or in addition to) more criminal behavior.

For girls ages 15 to 19 in the experimental group, but not for those in the Section 8 group, there were reductions in risky behavior, concentrated in marijuana use and smoking. Among boys in this age range in both treatment groups there were significant increases in smoking, but not in other types of risky behavior (Exhibit 5.5).

This pattern of gender differences in effects—positive for girls and negative for boys—suggests that boys and girls react differently to the disruption of moving and the challenge of integrating into a new environment. However, the available results do not allow us to say specifically why this is the case. To the extent that this difference reflects a response to the transition from a high-poverty environment to a lower poverty environment, one might expect this pattern to be different in the longer term for youths who have completed that transition or who have grown up in the new environment.

Evidence about Longer Term Effects of MTO

In the hypotheses generated about MTO effects, it was expected that impacts in several important domains would take longer than 4 to 7 years to become evident. These domains were education, employment, and economic self-sufficiency (an end to public assistance receipt). The Gautreaux research suggested that children moving to schools with very different characteristics might show achievement losses in the short run even though in the longer run they would catch up with their new schoolmates. Evidence is lacking about the time path of neighborhood effects on changes in economic self-sufficiency due to the absence of prior long-term research.

It is important to recognize that the control group—the benchmark against which we measure the effects of MTO—has not been static in the period since random assignment. For example, between 1995 and 2001 the employment rate among sample adults more than doubled, from 24 to 51 percent, and welfare receipt rates declined by more than half. Many control families moved out of public housing; as a result, at the time of the interim evaluation the average poverty rate in the neighborhoods where controls lived was 15 percentage points lower than it had been at baseline. In part, these improvements in the lives of controls represent natural turnover in welfare caseloads and the labor market. In addition, powerful external forces were at work during this period. The welfare system changes implemented in the mid-1990s (the shift from Aids to Families with Dependent Children (AFDC) to Temporary Assistances for Needy Families (TANF) and the advent of time limits) had substantial effects on nearly all low-income families. And the sustained economic boom of the 1990s offered increased opportunities to MTO families regardless of their group assignment. By improving conditions for control group members, these powerful external forces could make it less likely that MTO would show significant impacts on employment and earnings relative to the control group.

Small impacts on children's education

For the interim evaluation, education research focused on children ages 5 to 19 at the time the data were collected. We interviewed parents about the school-related attitudes, behaviors, and performance of all children in the sample. We interviewed children ages 8 to 19 about their own views and experiences in school. We also administered four different achievement tests from the Woodcock-Johnson Battery-Revised to sample children and collected data from published sources about the schools the children attended over the period since random assignment.

MTO had significant but small effects on the characteristics of the schools sample children attended (Exhibit 6.3). Experimental group children attended schools with somewhat lower percentages of poor and minority children and of students with limited English proficiency than they would have in the absence of the demonstration. The schools attended by experimental group children were ranked marginally higher on state exams than the schools attended by control students, but were less likely to be magnet schools. All of these differences were relatively small. For example, the schools attended by those who moved with program vouchers were only at about the 25th percentile on state exams, as compared with the 17th percentile for the schools attended by controls at the time of the interim evaluation. MTO had no significant effect on the teacher-pupil ratio.

Among the children in the Section 8 group, participation in MTO reduced the schools' percentages of minority and poor (exhibit 6.3). There were no other significant effects on the schools attended by children in the Section 8 group at the time of the interim evaluation, although the average ranking of schools attended by children in that group over the course of the followup period was slightly higher than that of the schools attended by control children. All of these effects were smaller than those on the schools of experimental group children.

These relatively modest impacts on school characteristics reflect the fact that, at the time of the interim evaluation, nearly three quarters of the children in families in the experimental group who leased up with program vouchers were attending schools in the same school district they were in at baseline. This may be because, as suggested in the MTO qualitative analysis, some children did not change schools when their families moved or because the families did not move very far. In particular, many families remained within the same big city school districts where they lived at baseline.

Not surprisingly, given the small impact on school characteristics, the demonstration had virtually no significant effects on any of the measures of educational performance analyzed, for either the experimental group or the Section 8 group (Exhibits 6.5–6.7). Of the 58 outcomes analyzed, there were significant impacts on only two: the Woodcock-Johnson calculation score for all children in the Section 8 group and the broad math score for children ages 8 to 11in the Section 8 group.

Impacts on economic well-being

Data on employment, earnings, household income, and public assistance were obtained from both administrative records and the interim survey. Administrative data provided a continuous history of employment, earnings, and AFDC/TANF and food stamp benefits from random assignment through the time of the interim evaluation. Survey data provided measures of employment, earnings, unearned income, receipt of SSI and Medicaid, and food security in 2001.

No effects on employment or earnings

At baseline, only about a quarter of all MTO adults were working. This proportion more than doubled over the followup period for both treatment and control group members. But the only statistically significant treatment-control difference in any of the measures of adult employment or earnings analyzed was a slight reduction in the employment rate in the first two years after random assignment among adults in the experimental group (Exhibits 7.3–7.4).

Although there were no statistically significant impacts on the employment or earnings of youth, either overall or by gender (Exhibit 7.4), there was a large reduction in the proportion of female youth working and not in school, with a concomitant (though not statistically significant) increase in the proportion attending school (Exhibit D7.1). Consistent with these findings, girls in the treatment groups perceived their chances of going to college and getting a well paying, stable job as much higher than their control counterparts (Exhibit E6.4). These findings are also consistent with the positive effects on girls' mental health and criminal behavior reported above.

No impacts on receipt of public assistance

At the time they were randomly assigned, the MTO adult sample members had very high rates of public assistance receipt and average incomes well below the poverty line. About three-fourths of the sample members were receiving AFDC at baseline, and four out of five were receiving food stamps (Exhibit 8.2). Further, nearly all sample adults (94 percent) had received AFDC at some point.

Average income was about $9,300 at baseline, well below the poverty line for a family of three. Median income was still lower, approximately $7,800. These results show that sample members were quite disadvantaged when they entered the MTO demonstration.

Four to seven years later, the AFDC/TANF receipt rates had fallen by half across the entire sample. Less than 30 percent were receiving welfare benefits, although 46 percent were still receiving food stamps. Forty-five percent of the sample adults were working and off TANF in 2001. These figures did not differ among the randomly assigned groups. The only significant impacts of MTO on receipt of transfer payments were small increases in the receipt and amount of AFDC/TANF and/or food stamp benefits during portions of the followup period for each group (exhibits 8.4-8.7).

At the time of the interim evaluation survey, average household income was about $15,500. Two-thirds of the sample had incomes below the poverty level, and half of these households had incomes below 50 percent of the poverty level. Some 11 percent of the sample households had experienced food insecurity with hunger in the previous 6 months. Participation in MTO did not affect incomes or food security, as there were no significant differences in these outcome measures between either of the treatment groups and the control group (Exhibit 8.8).

Hypotheses about long-term effects

There are a number of reasons to expect that observing the MTO population over a longer period of time may reveal significant program impacts in domains with no midterm effects. For example, the Gautreaux Program research suggested that children would need a prolonged period in better schools before making up prior deficits and moving ahead. Rosenbaum (1991) found that 1 to 6 years after their families moved to the suburbs many children "were still struggling to catch up, and it was not clear if they would succeed." But 7 years later, he found substantial, statistically significant impacts on eight of nine education- and employment-related outcomes for the same children.

There are strong theoretical reasons why it may take many years for the full effects of neighborhood to manifest themselves. Developmental outcomes such as educational performance almost certainly reflect the cumulative experience of the child from an early age. Children who spend their first ten years in an environment that does not facilitate educational achievement may never fully overcome that disadvantage, even if they then move to an environment that supports educational achievement. On the other hand, if a safer neighborhood and exposure to more educated adults affects long-term educational outcomes, we may yet see some educational effects.

In the interim evaluation, the youth sample is composed of children who moved out of public housing at ages 5 to 15. In the final evaluation, the youth sample will have left public housing at ages birth to 10. Those youth will have spent a much larger proportion of their formative years outside the concentrated poverty of public housing. Therefore, they may show much greater gains in educational achievement and other developmental outcomes.

It is also true that the move from high-poverty areas to lower poverty neighborhoods is likely to be disruptive and require some adjustment period, during which positive behavioral effects may not appear and, in fact, negative effects may be observed. If these effects indicate that the first 4 to 7 years after random assignment has been an adjustment period for these youth, we may observe different impacts in the longer term, once that transition is complete.

We cannot, of course, predict the impacts that will be observed 5 years after our data were collected. We can, however, examine the interim findings for evidence that impacts are related to time since random assignment. The most direct evidence on this question is provided by the time path of impacts on those outcomes for which we have longitudinal data over the entire followup period—the employment, earnings, and public assistance outcomes measured with administrative data. Examining the impacts in years 1 to 2 and years 3 to 4 after random assignment for each of the main outcomes measured with these data (Exhibit G.6), we found at least modest evidence of increasingly favorable effects over time.

Major Conclusions

Assessment of results

A summary assessment of the findings presented in this report and the impact estimates described above suggests that:

- The findings do provide convincing evidence that MTO had real effects on the lives of participating families in the domain of housing conditions and assistance and on the characteristics of the schools attended by their children;

- There is no convincing evidence of effects on educational performance; employment and earnings; or household income, food security, and self-sufficiency.

- The statistically significant impact estimates are uniformly large enough to be relevant for policy. Many are, in fact, quite large.

- Given the size of the interim evaluation sample and the leaseup rates in the two treatment groups, the impact estimates are sufficiently imprecise that some true impacts that are large enough to be relevant for policy may not have been detected as statistically significant.

- Although MTO induced substantial differences in the proportion of time spent in low-poverty areas by the three assignment groups, it was not a pure test of the effects of living in low-poverty areas compared to living in public housing in high-poverty areas, even for the families in the experimental group who moved with program vouchers. Extrapolating the effect of living continuously in low-poverty areas might show them to be more substantial than those observed in the demonstration. However, our ability to measure those effects quantitatively is limited.

- There is at least modest evidence that the impacts of the demonstration are becoming more favorable over time, at least for public assistance, which was the only outcome for which we were able to estimates effects over time. If this holds for other outcomes, we might expect more and larger impacts in the final evaluation, 10 years after random assignment.

Policy Implications of the Interim Evaluation Results

The interim findings allow us to address three fundamental questions related to policy with respect to low-income families in public housing:

- What social benefits and costs accrue as a result of moving low-income families out of public housing projects in high-poverty areas into private housing, and how do those benefits differ between policies that restrict such moves to low-poverty areas and those that do not?

- How effective is policy likely to be in changing the environment of low-income families?

- What do the interim results have to say about alternative approaches to improving the lives of low-income families?

The social benefits and costs of moving low-income families out of public housing in distressed neighborhoods into private housing

Although we have not attempted to conduct a formal cost-benefit analysis, the interim evaluation results provide relatively clear evidence of the main social benefits and costs of MTO. From the families' perspectives, the principal benefit of the move was a substantial improvement in housing and neighborhood conditions. Families who moved with program vouchers largely achieved the single objective that loomed largest for them at baseline: living in a home and neighborhood where they and their children could feel and be safe from crime and violence. On a list of observable characteristics, their homes and neighborhoods were substantially more desirable than those where control group members lived. These benefits accrued to families in both the experimental group and the Section 8 group, although the improvements tended to be roughly twice as large for experimental group families, who were required to move to low-poverty areas, at least initially.

Perhaps not surprisingly, these improvements in living environment led to significant gains in mental health among adults in the experimental group. The levels of psychological distress and depression were substantially reduced in this group. In addition, adults in both the experimental and Section 8 groups experienced substantial reductions in obesity for reasons we do not yet understand.

Among the children in these families, girls appear to have benefited from the move in several ways. They experienced improved psychological well-being, reporting lower rates of psychological distress, depression, and generalized anxiety disorder, and improved perceptions of their likelihood of going to college and getting a well paid, stable job as an adult. These girls' behaviors changed as well, with a smaller proportion working instead of attending school. They were less likely to engage in risky behavior or to use marijuana. Finally, both these girls and society as a whole benefited from a reduced number of arrests for violent crimes.

The principal social costs that must be offset against these benefits are the costs of the MTO mobility counseling, any increased costs due to the greater likelihood of receiving housing assistance among those who leased up with program vouchers, and an increase in the rate of behavior problems, smoking, and arrests for property crimes among boys ages 15 to 19.

We cannot place values on these social costs and benefits. Policymakers will have to decide whether the gains of this kind of policy outweigh the costs. But the interim evaluation has demonstrated that there are substantial social benefits as well as some costs associated with facilitating the movement of public housing residents who desire to move to low-poverty areas.

How effective is policy likely to be in changing the environment of low-income families?

One of the clearest messages of the interim evaluation results is that policy can influence, but it cannot dictate, the residential location of low-income families. As noted above, the demonstration reduced the proportion of the followup period that families who moved with program vouchers spent in areas of concentrated poverty by 47 percentage points in the experimental group and 35 percentage points in the Section 8 group (exhibit 2.9). It increased the proportion of time spent in areas with poverty rates below 20 percent by 53 percentage points among families in the experimental group.

Another lesson of the MTO demonstration is that the poverty rate, while important, may be an overly simplistic way to characterize neighborhoods. Residential environments are multidimensional, and no single measure will capture all the attributes that are important to the life chances of low-income families. Thus, for example, the fact that a majority of the program movers in the experimental group moved to areas with low, but rising, poverty rates may have had an important effect on their subsequent outcomes. Similarly, even in the experimental group, a large proportion of those who moved with program vouchers stayed within the city rather than moving to suburban areas. This meant that their children attended schools in the same school systems as control group children, which almost certainly limited the improvement in school quality they experienced as compared with (for example) a move to the suburbs. Moreover, the low-income areas to which families in the experimental group moved were still heavily minority. To the extent that racial integration or diversity has a positive influence on any of the outcomes analyzed here, that influence was largely absent in this demonstration. These considerations suggest that policy makers seeking to improve the environment of poor families may want to consider other characterizations of neighborhood than that provided by the poverty rate alone.

When thinking about the implications of these results for policy, it is also important to recognize that all of the impacts presented here are measured relative to a control group receiving some mix of existing housing subsidies. Some control families eventually received regular Section 8 vouchers, some continued to benefit from public housing subsidies, and some left housing assistance altogether. Indeed, some control group members were unable to remain in public housing because their units were demolished under HOPE VI or other revitalization efforts. We did not attempt to eliminate the influence of these changes in control circumstances, including the receipt of Section 8 vouchers, from the estimates. Rather, we view the results as measures of the incremental effects of offering vouchers, with or without locational restrictions, to residents of public housing in areas of concentrated poverty during the particular period encompassed by the study. These findings answer this question: How much better off are the recipients of the demonstration vouchers than families who started out in the same situation and who received no help from the demonstration? This means that the estimates from this study are not applicable to all types of policy. For example, for a policy that replaces public housing with vouchers, the appropriate control benchmark would probably be continued residence in public housing. That is not what was tested here—indeed, it probably cannot be tested—and the results of the present test probably understate the effects that would be expected from such a policy.

What do the interim results have to say about alternative approaches to improving the lives of low-income families?

The most fundamental question addressed by MTO is this: To what extent are the problems encountered by public housing residents the result of the high concentration of poor families in those developments and the surrounding neighborhoods, and to what extent are they caused by attributes of the families themselves? To the extent that these problems are environmental, the appropriate policy response is to foster dispersion of these families to more positive environments. To the extent that these problems reflect family characteristics—e.g., lack of education, limited work experience, or membership in a group that faces discrimination—the appropriate policy response is to address these characteristics directly.

By the final evaluation, the effects of environment will have had more time to manifest themselves. At this point, however, we can say that some of the problems of public housing residents do appear to be environmental. These include the housing and neighborhood quality deficiencies and psychological and behavioral problems on which MTO had significant effects. It remains to be seen whether the problems of physical health, educational performance and attainment, employment, earnings, and welfare dependence that characterize public housing residents are amenable to housing policies designed to change their residential environment. At this point, there is no evidence that they are. If that finding is confirmed in the final evaluation, that would suggest that policies designed to deal directly with these specific problems—educational improvements, employment and training, or welfare-to-work policies—will be more effective solutions.

Chapter One
The Moving to Opportunity Interim Evaluation

In 2000, 3.5 million poor people across the United States lived in high-poverty neighborhoods (census tracts).[3] A growing literature suggests that such concentration has a variety of detrimental effects on the residents of these areas in terms of both their current well-being and future opportunities.[4] The deleterious effects of high-poverty areas are thought to be especially severe for children whose behavior and prospects may be particularly susceptible to a number of neighborhood characteristics such as peer group influences, school quality, and the availability of supervised afterschool activities. Less has been written about whether and how other neighborhood environments exert positive influences on behavior and life changes. Ellen and Turner (1997) summarize the literature in this area, citing various theories about the mechanisms by which middle-class (often predominantly white) neighborhoods shape or reshape the lives of their residents.

Until recently such effects could only be studied by comparing the behavior and life outcomes of low-income residents of high-poverty areas with those of poor families in low-poverty neighborhoods. Such comparisons potentially confused the effects of neighborhood with the effects of the characteristics of families who lived in those two types of residential areas. The Moving to Opportunity (MTO) demonstration was designed to support direct analysis of neighborhood impacts by employing an experimental design (random assignment) to provide the first opportunity to measure the effects of neighborhood without these confounding factors.

1.1 The Moving to Opportunity Demonstration

The MTO demonstration was conducted in five cities—Baltimore, Boston, Chicago, Los Angeles, and New York—between 1994 and 1998. To be eligible for MTO families had to have children under 18 and be living in public housing developments or other 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 public housing authorities (PHAs) in each city conducted outreach to all eligible households and all those interested were given the opportunity to apply for this special program. Interested households were placed on the MTO waiting lists of the local PHAs in the five demonstration sites.

Applicants were drawn from the waiting lists for intake and given an explanation of the MTO demonstration. If they were still interested, they signed the Enrollment Agreement and completed the Participant Baseline Survey. Then they went through the process of eligibility determination for

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3 See Jargowsky (2003). In total, there were 7.8 million persons living in high-poverty census tracts in 2000. These figures represent a reduction in poverty concentration, in contrast to the increases from 1970 to 1990 (see Jargowsky 1997).

4 See, for example, Wilson (1987, 1996); Jencks and Mayer (1990); and Brooks-Gunn, et al. (1993).

Section 8.[5] Eligible applicant families—4,608 in all—were randomly assigned to one of three groups. The experimental group received rental assistance vouchers that could be used 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 families in the experimental group to help them locate and lease suitable housing in a low-poverty area. This counseling was intended to help the experimental group families meet the locational constraint within the time limit for leasing up.[6] The Section 8 group received regular Section 8 vouchers, which could be used anywhere. These families also had a time limit and they did not receive any mobility counseling. The control group received no vouchers, but continued to be eligible for project-based assistance. Families in the experimental group and the Section 8 group who failed to lease up with demonstration vouchers were also eligible to continue to receive project-based assistance.[7]

The Department of Housing and Urban Development (HUD) sponsored the MTO demonstration to ascertain the effects of improved neighborhood environments—low-poverty areas—on various aspects of the lives of low-income families. We discuss below how the design of the MTO demonstration and its evaluation will help us measure these effects. This interim evaluation measures demonstration effects 4 to 7 years after program entry. A final impact evaluation will be conducted approximately 10 years after the end of program operations.

HUD specified six key subject domains of possible social and economic impact for investigation in this interim evaluation: mobility and housing; adults' and children's health; delinquent or criminal behavior of juveniles and adults; children's educational achievement; employment history, earnings, and benefits; and income and public assistance. Each of these domains is covered in this report.

HUD has used this interim evaluation to establish a framework for the final evaluation of MTO's impacts by:

- Combining quantitative and qualitative methods, drawing on the strengths of each to tell the full story.

- Defining a set of measures for each impact area that are appropriate for investigating impacts at the interim point (4 to 7 years after random assignment) and are also appropriate to the final evaluation after 10 years.

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5 It is estimated that just over one-fourth of the eligible families in targeted public housing developments in Baltimore, Boston, Los Angeles, and New York City enrolled in MTO. See Goering et al. (1999), Table 5, p. 32.

6 Program rules set the maximum search period at 90 days. While the experimental group generally had more time to lease up than the Section 8 group, the time was still limited. Also, the counseling agencies could require that some of the extra time be spent on preparation for housing search.

7 At the time of MTO demonstration operations, the Section 8 program was still issuing both certificates and vouchers. Subsequently HUD converted all certificates to vouchers. In 1999 Section 8 was renamed the Housing Choice Voucher Program and changed in a number of ways. In this report, to simplify the text, the term *voucher* will be used to refer to all the tenant-based resources issued through MTO. However, we will continue to refer to the program as *Section 8*, because the rules of the demonstration were set under the tenant-based version of that program.

- Contributing to our state of knowledge about the mechanisms by which the neighborhood environment affects the future of resident adults and children.

The questions addressed in this interim evaluation are of great importance. To what extent are the adverse outcomes associated with living in very poor neighborhoods the products of the neighborhoods rather than of the characteristics of those living there? If the adverse outcomes are products of the neighborhoods, to what extent do opportunity moves to areas with minimal poverty offer a means of ameliorating them? If public housing residents are given tenant-based housing assistance, do they make locational choices that afford them access to some or all of the same life improvements as opportunity moves? The research results addressing these questions could shape social policy in the United States for years to come.

1.2 Previous Studies of Mobility Programs and the Effects of Neighborhood

Recent interest in geographic location and mobility as important factors shaping the futures of low-income families began with the results of the Gautreaux Program, a federal court-ordered racial desegregation program in Chicago. Under the name of tenant activist Dorothy Gautreaux, applicants and residents of Chicago public housing brought a class-action housing segregation lawsuit against HUD and the Chicago Housing Authority (CHA) in 1966 (Davis 1993; Rubinowitz and Rosenbaum 2000). After years of litigation the courts ordered HUD and the CHA to remedy the extreme racial segregation that they had imposed on public housing applicants and residents. Starting in the late 1970s these agencies had to provide, among other remedies, a housing mobility option throughout the Chicago region for approximately 7,100 Black families.

The Gautreaux Program took shape as a result of the court's ruling. Participating families were helped to move out of racially isolated areas through the (then new) tenant-based Section 8 program. Families chosen for the Gautreaux program received Section 8 certificates that required them to move to either predominantly white or racially mixed neighborhoods. They also received assistance from housing counselors to make these moves.

Beginning in the late 1980s research on the Gautreaux Program suggested that the moves to less segregated suburban locations were associated with measurable improvements in the lives of participating adults and children. Popkin, Rosenbaum, and Meaden (1993) found that participants who had moved to white suburban areas were significantly more likely to report having had a job since they moved than participants who moved to neighborhoods in the city.[8]

Positive changes were also reported for small samples of children who had been living in less segregated neighborhoods for periods of 7 to 10 years. Such children were less likely to drop out of

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[8] These findings were based on a survey only of participants who remained in their suburban communities, excluding others who moved back to the city (or elsewhere) and still others in the program who never moved to the suburbs at all.

school and were more likely to take college-track classes than their peers in a comparison group who moved within the city of Chicago rather than to suburban areas. The city neighborhoods were both poorer and more racially segregated than the suburban locations. After graduating from high school, the Gautreaux children were also more likely than their city peers to attend a 4-year college or become employed full-time (Rubinowitz and Rosenbaum 2000).

Although the results from the research on Gautreaux Program participants were encouraging, they did show that such moves could create initial setbacks. Children who moved to white suburban communities initially experienced declines in school performance, and children of suburban movers appeared to be somewhat more likely to be placed in special education (at an average of 2 years postmove for those so placed). However, after 9 years these children did better on a number of measures than those whose families moved within the city (Kaufman and Rosenbaum 1992, Rubinowitz and Rosenbaum 2000).

In assessing both the positive and negative findings of the Gautreaux study, it is important to bear in mind that this was a nonexperimental comparison of different families who moved to different types of neighborhoods. There is a serious risk of selection bias in such a comparison—that is, the observed differences may reflect differences between the kinds of people who moved to the suburbs and those who moved within the city rather than the effects of these different residential locations. It was precisely this danger that led HUD to sponsor the current experimental study to investigate these effects. The Gautreaux findings for children were also based on a very small sample (69 households).

Other recent research has focused on possible theoretical causes for both positive and negative effects of neighborhoods (Manski 1993, 2000; Galster and Killen 1995; Galster, Quercia, and Cortes 2000; Leventhal and Brooks-Gunn 2001a). The core question is whether there are clear, independent effects of neighborhood and, if so, whether they are favorable or unfavorable from the standpoint of the family. Only recently has there been evidence and discussion about how neighborhood environments may exert positive influences on behavior and life chances (Brooks-Gunn, Duncan, and Aber 1997; Sampson, Morenoff, and Gannon-Rowley 2002).

Galster and Killen (1995) noted the complexity of the causal influences linking metropolitan and neighborhood-based opportunities and pointed out the dynamic nature of opportunities and the critical issue of residents' willingness and ability to take advantage of contextually positioned resources. Ellen and Turner's (1997) summary of literature in this area suggests various mechanisms by which middle-class (often predominantly white) neighborhoods shape or reshape the lives of their residents. The effects of neighborhood appear to be more pronounced for children rather than for adults, with Leventhal and Brooks-Gunn (2001a) offering evidence that neighborhood influences on achievement measures such as IQ are most important at ages below 5 years and less important at later ages.

Other research has looked qualitatively at the issue of how neighborhood environments shape residents' lives. Patillo-McCoy (1999) explores the influences on teens growing up in a moderate-income African American neighborhood in Chicago, looking at both the positive aspects of the community and the ways in which proximity to poorer neighborhoods poses risks for youth. Bourgois (1995) uses his portrait of drug dealers in New York to show how in many troubled neighborhoods a different set of social rules lead youth to become involved in deviant behavior. Two qualitative studies of Chicago's public housing (Popkin et al. 2000; Venkatesh 2000) describe how residents in public housing in Chicago cope with the extreme dangers of their environment and the key role that gangs play in the community. Other ethnographic researchers have documented the importance of social networks for low-income families, focusing on systems of mutual help that allow families to cope with extreme poverty and manage to support their families (Stack 1974; Edin and Lein 1997). However, these studies have also documented the ways in which these relationships may undermine an individual's attempts to get ahead.

It is important to bear in mind that the empirical basis for all of these studies was naturally occurring correlations between outcomes and neighborhood characteristics. These correlations may reflect differences in the individual characteristics (e.g., motivation, ability, etc.) of the low-income families who live in different areas, rather than the effects of those areas. The only way to ensure that this is not the case is to offer a randomly selected group of low-income families the opportunity to move from high-poverty areas to lower poverty areas, as was done in MTO.

1.3 Previous Research on the MTO Demonstration

Research on MTO began while the operational phase of the demonstration was still underway. HUD issued a first report to Congress once all the sites had begun enrolling and counseling families (U.S. Department of Housing and Urban Development, 1996). Observations and analyses of the counseling delivered to experimental group families through MTO were documented about midway through the operations period (Feins, McInnis, and Popkin 1997). At the end of enrollment and leaseup in 1999, HUD reported initial findings about the participating families and the program moves made by experimental and Section 8 group families (Goering et al. 1999).

In 1997 HUD's Office of Policy Development and Research conducted a grant competition and ultimately made eight small grant awards to teams of researchers with varied topics and approaches. Each team was given access to the MTO participants in one of the five sites[9] for purposes of assessing different aspects of the families' experiences. A substantial number of analyses and articles resulted from these grants.[10] Researchers taking advantage of the experimental design of MTO to eliminate selection bias found distinct improvements in neighborhood and school environments:

- Significantly lower crime rates in the neighborhoods of the experimental and Section 8 groups in Los Angeles compared with the control group's neighborhoods (Hanratty, McLanahan, and Pettit 2001).

- Major gains in safety for program movers in both the experimental and Section 8 groups in New York, with significantly greater increases in neighborhood quality and satisfaction for the experimental group than for the Section 8 group (Leventhal and Brooks-Gunn 2001b).

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9 Access was also provided to data collected at baseline about these participants.

10 For a volume containing the major pieces of work from the HUD-sponsored grants research, see Goering and Feins (2003).

- Significant and positive differences in average test scores for schools attended by the Boston experimental group children in 1997 compared with the schools of the control group children (Katz, Kling, and Liebman 2001).

- Positive differences in the resources and characteristics of schools attended by Baltimore children in the experimental and Section 8 groups compared with the schools of children in the control group (Ludwig, Ladd, and Duncan 2001).

- A lower rate of decline in test scores for younger children in the Baltimore MTO experimental and Section 8 comparison groups as they grew older compared with children in the control group, suggesting that the move might have helped to prevent the kinds of dramatic decline in test scores often found in inner-city schools (Ludwig, Ladd, and Duncan 2001b).

- A slightly greater likelihood that Baltimore teens in the experimental and Section 8 groups would experience grade retention or be suspended or expelled compared with teens in the control group (Ludwig, Ladd, and Duncan 2001b).

- Significantly fewer behavior problems among boys in the Boston experimental and Section 8 groups compared with boys in the control group (Katz, Kling, and Liebman 2001).

- Significant improvements in child health (reductions in the percentage experiencing asthma attacks or accidents requiring medical attention) for the Boston experimental and Section 8 group children compared with the control group children (Katz, Kling, and Liebman 2001).

- Reduced numbers of arrests per 100 juveniles ages 11 to 16 in the Baltimore experimental and Section 8 groups compared with control group juveniles (Ludwig, Duncan, and Hirschfield 2001).

At the same time, there were areas of research in which little or no effect was found:

- Adults in the experimental and Section 8 groups in Boston showed no differences from control group adults in key economic outcomes—the percentage with employment earnings and the percentage receiving public assistance (Katz, Kling, and Liebman 2001).

- Adults in the experimental and Section 8 groups in Los Angeles worked significantly more hours per week than the control group adults but did not have higher earnings or income (Hanratty, McLanahan, and Pettit 2001).

- There were no significant differences in any of the other sites on welfare receipt, employment, or hours worked.

These early MTO research results suggested that the demonstration might well be having some notable effects on participants. But because the timing of program entry extended from 1994 to 1998 and because each study was done in a single site, with relatively small sample sizes, the small grant research needed to be followed by more comprehensive and uniform research when more time had elapsed for the families in the program. It was clear that the MTO design and sample could be used to learn about a wide range of topics.[11] It was equally clear that many questions remained to be answered.

1.4 Research Questions Addressed by the Interim Impact Evaluation

The MTO demonstration was intended both to provide information about the effects of neighborhood on families and to test possible programs to induce changes in where low-income families live. The programmatic interventions tested were vouchers restricted to low-income areas (the experimental group) and regular housing vouchers (the Section 8 group). In each of the six outcome domains of this study (housing, health, delinquency and risky behavior, education, employment and earnings, and income and public assistance), we analyzed the impacts of the special (location-restricted) vouchers and regular Section 8 vouchers on the members of the households that joined MTO, with a particular emphasis on the household heads and their school-age children.

For each of these two treatment groups, we produced estimates corresponding to two different questions: the effect of the intervention on the average level of the outcome on the entire treatment group, including those who failed to lease up with the MTO voucher,[12] and the effect on only those who leased up.[13] The former estimate, known in statistical terms as the "intent to treat" (ITT) effect, measures the degree to which, on average, the intervention affected all individuals who were eligible to receive it (assigned to the experimental or Section 8 group) whether they leased up or not. Obviously, the size of this estimate will vary with the proportion of families who received the intervention (in this case, with the proportion who leased up). The effect on only those who lease up is known in statistics as the effect of the "treatment on the treated" (TOT).

All estimates are measured relative to the control group members, who received no vouchers but were eligible to remain in public housing. To improve the precision of the estimates, we used regression analysis to control for any chance differences between the treatment and control groups on a number of characteristics measured at baseline (when families joined MTO). For a complete description of the estimation procedures, see appendix B.

If one is interested in the effectiveness of a program like MTO in improving the situation of the entire class of families to whom it is offered, the ITT estimates are the appropriate set of results to examine. The overall effectiveness of such a policy depends both on the proportion of families who use the

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[11] Later sections of this chapter and appendix A contain further information on the sample and on the data collected for the study.

[12] Leasing up means that the family finds a housing unit that passes the program's quality standards, has a willing landlord, and has rents affordable to the family under program rules. A lease is then signed that obligates the administering agency to pay the voucher amount toward the rent and obligates the tenant to pay the remainder. Someone who rents a housing unit with the help of the voucher assistance is said to lease up. If these conditions are not met, the voucher expires and the family has failed to lease up.

[13] See appendix B for a detailed description of how these estimates were generated.

voucher and its effects on those who do. This point is particularly salient in comparing the ITT impacts on the experimental group with the ITT impacts on the Section 8 group. Because of the restriction of the experimental group's vouchers to low-poverty areas, a smaller proportion of them leased up than in the Section 8 group. This lower success rate will offset to some extent the presumably greater effects of a lower poverty environment on the members of the experimental group who did lease up.

If, instead, one is interested in the effects of neighborhood on family outcomes, one should consult the TOT results. They reflect the difference in outcomes between similar families in different residential environments.[14] However, the TOT estimates are non-experimental, while the ITT estimates reflect the full power of the experimental design. For a complete description of the estimation procedures, see appendix B.

In both cases, it is important to note that effects are not measured relative to living in public housing. They are measured relative to the outcomes of a set of families that started out living in public housing. As we will see in the next chapter, many of the control group families subsequently left public housing and moved to the same kind of neighborhoods as some of the treatment group families. Some even received vouchers through the regular Section 8 program.[15] The estimates presented here represent the incremental effects of demonstration vouchers relative to what happened to the controls—and, therefore, what would have happened to the treatment group families in the absence of the MTO demonstration.

It is also important to bear in mind that even the experimental group families that leased up did not spend the entire followup period in low-poverty areas. As we will see in the next chapter, some made a second or third move to relatively higher poverty areas than those they originally chose to meet the voucher's location constraint. And in some cases, even if the family stayed in the same neighborhood, its poverty level increased over the course of the followup period due to other changes in population and incomes.[16] It is true, however, as we will show in the next chapter, that both the experimental group and the Section 8 group spent significantly more time in low-poverty areas than the control families. Thus the experimental contrasts on which the impact estimates are based *do* reflect the effect of a lower poverty residential environment. They also reflect lower rates of residence in public housing.

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[14] These differences in residential environment may include differences in the dwelling unit as well as differences in the neighborhood. For example, dwelling units in lower poverty areas may have fewer health hazards. Because we believe that neighborhood effects will generally predominate, however, we use *neighborhood* as a shorthand term for the entire set of environmental factors.

[15] The implications of this fact for interpreting program impacts are addressed in chapter 9.

[16] The MTO families constituted extremely small proportions of the population of the destination census tracts, so they had no noticeable effect on these changes. Experimental group movers constituted at most 2.3 per 1,000 households in their new neighborhoods. See Goering et al. (1999) p. 42.

Understanding the Impact Estimates

Throughout this report the exhibits that present the results of statistical tests for program impacts take a standard format. Each exhibit lists the outcome measures being analyzed in the left-most column. Various hypotheses predicted that these outcomes would be affected by the MTO experimental treatment (moving to a low-poverty area).

The next column, Control Mean, gives the average value for the control group on each outcome measure. If the averages for the experimental and Section 8 groups are not different from the control mean, then there is no MTO impact on this measure at this point in time.

The remaining four columns give the estimated impacts. The first pair shows the estimated ITT (intent-to-treat) and TOT (treatment-on-treated) effects for the experimental group; the second pair shows the estimates for the Section 8 group. The ITT columns show the estimated impacts on the assigned group as a whole, including both families who leased up and families who never rented with a voucher obtained through MTO. The TOT columns show the estimated impacts on the program movers—the sample members who actually moved with program vouchers.

The TOT estimates are non-experimental, in the sense that they are not directly observed for whole randomly assigned groups. They are based on the assumption that the program had no impact on the nonprogram mover families. The TOT estimates are calculated by dividing the ITT estimates for each group by the group's leaseup rate. Because only 47 percent of the experimental group and 62 percent of the Section 8 group leased up, this makes the TOT estimates substantially larger than the ITT estimates. However, since the standard errors for the TOT estimates are adjusted in the same way, TOT impacts are statistically significant only if the corresponding experimental ITT estimates are significant.

The control means indicate what the outcomes for the experimental and Section 8 groups as a whole would have been without MTO. However, they may not represent the no-treatment outcomes for program movers (those who leased up), because these families differ from nonprogram movers in various ways.

The error of estimate for each estimated impact is shown below it, in parentheses. If an estimated impact is statistically unlikely to have occurred by chance (is more than 1.96 times its standard error), it is marked with an *.

Finally, it should be noted that although the ITT estimates for the experimental and Section 8 groups are directly comparable because these two groups were randomly assigned from the same pool, the TOT estimates for the two groups are not directly comparable. That is because different proportions of the two groups leased up. As expected, the leaseup rate was higher for families assigned to the Section 8 group than for families assigned to the experimental group. Differences between the average effects on participants for the two groups may arise because of differences in effects for the families that leased up under both groups or because effects were different for the additional families that leased up in the Section 8 group.

1.5 Using the Experimental Design to Estimate Impacts

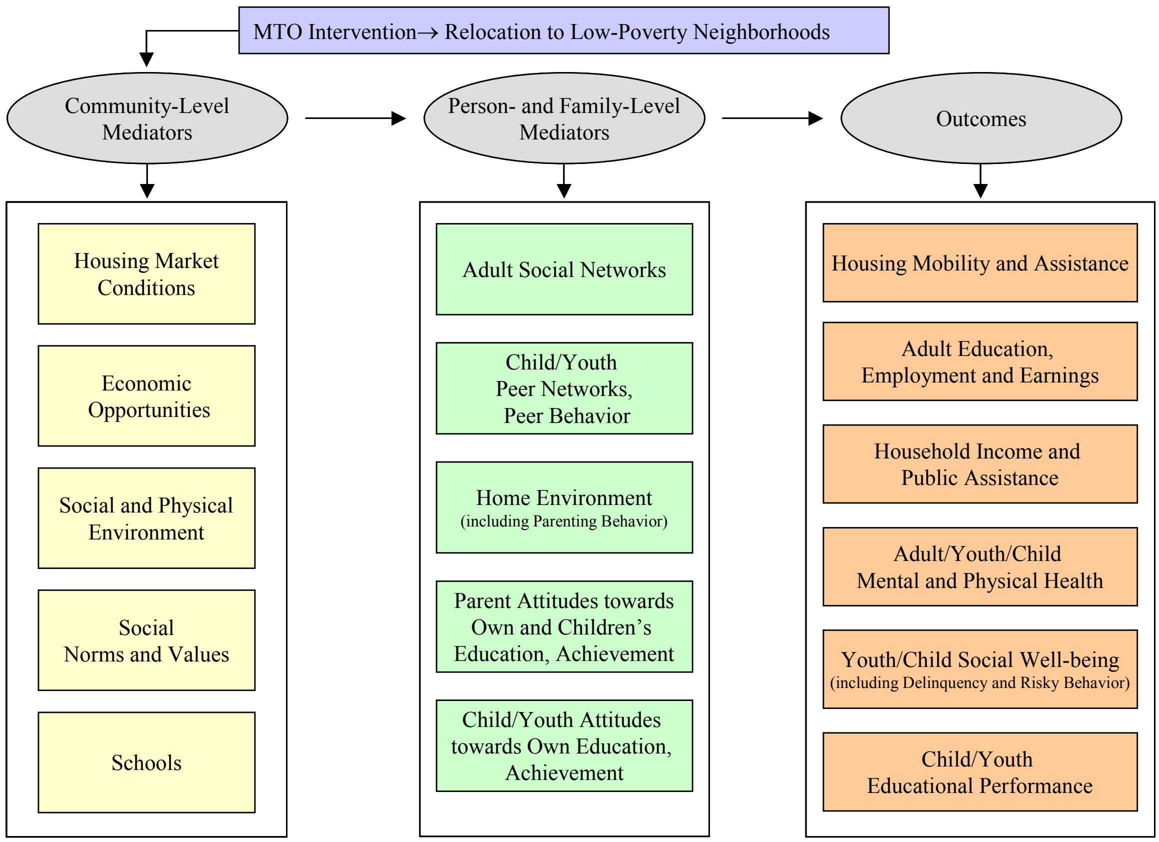
This study was based on a set of hypotheses from the social science literature about the effects of a change in neighborhood environment on adult and child wellbeing. Distilling the literature allowed us to identify a set of hypothetical pathways by which relocation to low-poverty neighborhoods could affect a variety of outcomes. Exhibit 1.1 is a generalized model showing these pathways, with the experimental intervention at the top and the six outcome domains at the right. The hypothesized influence of neighborhood or community is mediated by a series of factors that bear a logical relationship to each other. Relocation brings about changes in community-level factors, which have consequences for person- and family-level mediators. In turn, these affect the outcomes of interest in this study.

Analytic framework

This study's impact analysis has been structured to shed light not only on the ultimate impacts of moving out of public housing but also on the causal mechanisms through which those effects occur. In each domain we have not only specified the outcomes of interest but also described alternative pathways through which impacts on those outcomes might occur and the mediating factors along those pathways. The qualitative research carried out early in this project, along with the prior research cited above, played a key role in helping to define the mediating factors. For example, the extended interviews with adults and youth suggested new hypotheses about how changing neighborhoods affected families.

Estimation of impacts on the mediating factors (as well as on final outcomes) helps to distinguish the causal mechanisms responsible for the estimated impacts. For example, it might be hypothesized that families who moved to low-poverty neighborhoods would experience higher employment rates, because the move to a lower stress environment improved their mental health (e.g., reduces the incidence of depression) and so increased their employability. Direct estimation of impacts on measures of mental health would help to determine whether this potential mediating factor could have contributed to any observed impacts on employment and earnings.

EXHIBIT 1.1
HYPOTHESIZED PATHWAYS OF MTO IMPACTS



Note that both qualitative exploration and quantitative examination of the mediating factors can suggest causal mechanisms and can rule out certain theories if, for example, there are no impacts on the mediating factors involved in those theories. But they cannot conclusively prove causality. Continuing the example, a finding that the experimental vouchers both reduced the incidence of depression and increased employment rates would be consistent with the theory that moving out of the unsafe, high-stress public housing environment improved family members' mental health, making them more employable and/or more willing to search for work. But that finding would also be consistent with the interpretation that family members' mental health improved because they worked more. Thus when there are significant effects on the hypothesized mediating factors, inferences about causality will necessarily be judgmental. However, when there are no significant impacts on the mediating factors, the interpretation will be clearer. If, for example, we find that employment increased but mental health did not improve we can rule out the theory that employment increased because of improvements in mental health.[17]

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[17] We cannot, of course, definitively rule out an impact on any mediator or outcome. We can only say that if there was an impact it must be less than the minimum effect detectable with this sample.

The most fundamental mediating factor we are studying is, of course, the poverty level of the neighborhood. We will examine the degree to which MTO affected several alternative measures of this basic environmental factor in chapter 2. Other mediating factors examined vary across the outcome domains. At the community level, however, they can be grouped into three broad categories: community norms and values, the social and physical environment, and economic opportunities.

Individuals who move to a new community are likely to be affected by the norms and values of that community through peer pressure and community expectations. We would expect these effects to be stronger the more the individual interacts with members of the new community. We would also expect such effects to be stronger if the norms and values of the new community are substantially different from those of the individual's old community.

The social and physical environment in the community may affect a number of outcomes. For example, the incidence of crime and violence in the community may be a potentially important mediating factor, affecting not only the families' sense of security and well-being but also the likelihood that they themselves would become involved in illegal activities. The social resources of the community, including school quality, recreational facilities, public and private social services, and healthcare facilities, will facilitate or limit certain behaviors and outcomes. The physical environment, including safety hazards, air quality, and presence of allergens, may have important effects on family health.

Economic opportunities in the local community may influence family members' employment and earnings directly and a number of other outcomes indirectly. For example, if family members obtain jobs with better health insurance coverage, they may have better access to medical care and, as a result, improved health. Better economic opportunities may also provide constructive alternatives to crime and delinquency.

1.6 Sample and Data Collection for the Interim Evaluation

Sample definition and description

The sample used in the interim evaluation includes all 4,248 families randomly assigned in the MTO demonstration through December 31, 1997.[18] This is not the entire MTO population: family intake continued in one site (Los Angeles) through July 1998, and leaseups occurred there until March 1999. However, the sample for the present study was restricted in order to assure that at least 4 years had passed since random assignment for all its members. The allocation of this sample among the

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18 The full MTO population consists of 4,608 families. The 4,248 families in the interim evaluation sample represent 92.2 percent of the full population. This study's sample includes all of the families in four of the five sites (Baltimore, Boston, Chicago, and New York). Exhibit C1.1 in appendix C compares the sample to the entire MTO population.

An additional 356 families were randomly assigned after January 1, 1998. These families were not included in the sample for this analysis because the increase in sample size they would have provided was deemed insufficient to justify shortening the followup to include them.

treatment groups, by site and overall, is shown in exhibit 1.2. The number of families in each site range from 636 families in Baltimore to 1,081 in New York City.[19]

EXHIBIT 1.2
ALLOCATION OF THE INTERIM EVALUATION SAMPLE FAMILIES BY SITE AND TREATMENT GROUP

	Experimental Group	Section 8 Group	Control Group	Total
Baltimore	252	187	197	636
Boston	366	267	326	959
Chicago	460	202	232	894
Los Angeles	250	168	260	678
New York City	401	385	295	1,081
All Sites	1,729	1,209	1,310	4,248

Source: MTO data system
Sample: All families randomly assigned through December 31, 1997.

Although MTO enrollment took place by family, the interim evaluation focuses on individual members of these families and their experiences. It was designed to answer questions midway through the 10-year followup period about one adult and up to two children in each of the families in the sample. The children were sampled randomly from among all age-eligible children (ages 5 to 19) in each family.[20] Exhibit 1.3 shows the sample allocation by treatment group and site for the sampled adults and children. On average, the sample included 2.6 members per family, including 1.6 children.

19 A somewhat larger number of families were assigned to the experimental group than to the Section 8 group to achieve the desired sample sizes despite a likely lower leaseup rate in the experimental group. Assignment rates within sites were further adjusted to compensate for differences between expected and actual leaseup rates. (The sample weights used in the quantitative analyses adjusted for differences among sites and over time in the rate of random assignment. See the discussion in appendix B.)

20 See appendix B for details of sample selection.

EXHIBIT 1.3
ALLOCATION OF THE INTERIM EVALUATION SAMPLE MEMBERS BY SITE AND TREATMENT GROUP

	Experimental Group	Section 8 Group	Control Group	Total
Baltimore				
Adults	252	187	197	636
Children	361	289	303	953
Boston				
Adults	366	267	326	959
Children	555	408	509	1,472
Chicago				
Adults	460	202	232	894
Children	764	332	373	1,469
Los Angeles				
Adults	250	168	260	678
Children	420	272	429	1,121
New York City				
Adults	401	385	295	1,081
Children	591	606	471	1,668
All Sites				
All	4,420	3,116	3,395	10,931
Adults	1,729	1,209	1,310	4,248
Children	2,691	1,907	2,085	6,683

Source: MTO data system
Sample: All families randomly assigned through December 31, 1997.

Exhibit 1.4 shows the allocation of the child sample by age among the treatment groups. A child's age for data collection purposes was uniformly measured as of May 31, 2001.[21] Different information was collected about different age groups. For this analysis, the key age groups for the sampled children were ages 5 to 7, 8 to 11, and 12 to 19. These age groups were set to differentiate among children by developmental stage and by hypothesized differences in neighborhood influence. However, other age breaks were used in some analyses as the text and tables will indicate.[22]

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21 Since the field data collection continued through September 2002, this means that—at the moment they were interviewed or tested— some children were more than a year older than their age as defined for sampling.

22 Although their ages were similar at the time of the interim evaluation, the children varied considerably in the length of their exposure to the MTO treatment. Children 5 to 7 at the time of the study were from birth

EXHIBIT 1.4
ALLOCATION OF THE INTERIM EVALUATION CHILD SAMPLE BY AGE AND TREATMENT GROUP

	Experimental Group	Section 8 Group	Control Group	Total
Ages 5 to 7	371	264	309	944
Ages 8 to 11	885	638	679	2,202
Ages 12 to 19	1,435	1,005	1,097	3,537
All Children	2,691	1,907	2,085	6,683

Source: MTO data system.
Sample: All families randomly assigned through December 31, 1997.

Socioeconomic and Demographic Characteristics of the Sample at Baseline

As with the MTO program population as a whole, the racial composition of the interim evaluation sample (exhibit 1.5) is heavily African American. Two-thirds of the overall sample is African American, and one-third is Hispanic. By site (exhibit C1.2), Baltimore and Chicago have almost entirely African American samples while the other three sites have substantial proportions of Hispanic families (40 to 50 percent). Only the Boston program enrolled a significant number of nonHispanic white families. Women headed most MTO households at baseline, although in Los Angeles a substantial minority of households had male heads and two parents present. The median number of children was three.

Recruited from public housing residents, the MTO program families had average cash incomes of about $9,300 when they entered the program and about 60 percent depended on public assistance, Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF) as their primary income source. While some were employed, most were not. Approximately 70 percent were not working when they joined MTO. About 40 percent of the household heads were not high school graduates and did not have a GED, although some (about 16 percent) were then in school.

to age 4 at baseline. The 8 to 11-year-olds were ages 1 to 8 at baseline. And the youth (ages 12 to 19) ranged in age from 5 to 16 at baseline.

Exhibit 1.5
Demographic and Socioeconomic Characteristics of MTO Families at Baseline by Random Assignment Group

	Experimental Group	Section 8 Group	Control Group	All Groups
Race/Ethnicity of Head of Household [a]				
African American non-Hispanic	62.4%	61.9%	63.5%	62.6%
Hispanic	30.3%	30.9%	29.9%	30.4%
White non-Hispanic	3.1%	2.7%	2.6%	2.9%
American Indian non-Hispanic	0.5%	0.3%	0.3%	0.4%
Asian/Pacific Islander non-Hispanic	1.9%	2.3%	1.3%	1.8%
Other non-Hispanic	1.8%	2.0%	2.5%	2.1%
Sex of Head of Household				
Male	8.5%	8.7%	8.0%	8.4%
Female	91.5%	91.3%	92.0%	91.6%
Head of Household's Marital Status				
Never married	61.7%	61.8%	63.2%	62.2%
Married	11.7%	11.5%	10.5%	11.3%
Divorced	9.7%	9.4%	9.3%	9.5%
Widowed or separated	16.9%	17.3%	17.0%	17.1%
Median Number of Children	3	3	3	3
Average Total Household Income	$9,385	$9,189	$9,337	$9,314
Percent with AFDC as Primary Income Source	61.1%	62.2%	61.5%	61.6%
Head of Household Currently in School?				
Yes	16.0%	16.8%	15.8%	16.2%
No	84.0%	83.2%	84.2%	83.8%
Head of Household a Graduate?				
High school	42.2%	40.6%	38.5%	40.6%
GED	17.8%	20.0%	22.0%	19.7%
Neither	40.0%	39.4%	39.5%	39.7%
Head of Household Currently Working?				
Full-time	16.1%	16.0%	16.3%	16.1%
Part-time	12.8%	11.1%	10.3%	11.6%
Not working	71.1%	72.6%	73.4%	72.2%
Working for benefits	0.0%	0.2%	0.1%	0.1%

Source: MTO Participant Baseline Survey

Sample: Adults from families randomly assigned through December 31, 1997.

Notes: The respondent to the Baseline Survey was usually the same person as the sample adult for the interim evaluation. Household income was defined following the rules for Section 8 eligibility. Percentages may not add to 100 because of rounding. Data are weighted as described in appendix B.

a) Respondent self-reports. A number of African American respondents skipped the ethnicity question and are not included in the distribution reported. Many Hispanics used the *Other* category for the race question.

In exhibit 1.5, the figures for each of the randomly assigned groups are very similar on all these characteristics. In fact, statistical tests show that the MTO random assignment process worked. The experimental, Section 8, and control groups are the same (any differences being no greater than chance would produce).

Data on these families' background conditions and experiences (exhibit C1.3) offer insights into their motivations for joining a mobility program. At least three-fourths of the respondents in every group indicated that getting away from drugs and gangs was the first or second most important reason for wanting to move. A high proportion of respondents were dissatisfied with their current neighborhoods, and high rates of victimization were reported on a range of crimes.[23] About half indicated they wanted to relocate to areas with better schools for their children. But not all the motivation to move at baseline was neighborhood related. Nearly half the sample mentioned getting better housing as the first or second reason for wanting to move.

Data collection for the interim evaluation

Extensive qualitative and quantitative data were collected in 2001 and 2002 for the sample. There were three main components of the data collection:

- Individual data on sample members were collected through in-person interviews with adults and children ages 8 to 19 from the families in MTO and through educational achievement tests for children ages 5 to 19.

- Administrative and published data were collected about the employment and public assistance outcomes for the sample and about the families' residential locations and the schools the children have attended.

- A qualitative study involved extended interviews with parents and teenagers in MTO families (a small subset of the sample), focusing on their views about the effects of neighborhood on the behavior and experiences of family members. The data collection for this component was described fully in Popkin, Harris, and Cunningham, 2001.

Participant data collection for this study

Data about the MTO sample members in the interim evaluation sample were collected between January and September 2002 through interviews with the sample members and through direct measurement and educational testing. Exhibit 1.6 summarizes the topics about which data were collected, by method, according to the age of sample members.

The three surveys—household, youth, and child—were administered largely in person by trained interviewers, using Computer-Assisted Personal Interviewing (CAPI) on laptop computers. The surveys for all three samples were administered primarily in the respondents' homes, with the session

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[23] The victimization rates reported in the Participant Baseline Survey were about four times higher than those reported in a 1994 national survey of residents of public housing family developments.

scheduled at the respondents' convenience.[24] Field interviewers also recorded their observations of the home and neighborhood environments.

EXHIBIT 1.6
CONTENT OF PARTICIPANT DATA COLLECTED FOR THE MTO INTERIM EVALUATION

	Adult[1]	Youth 12-19	Children 5-11[3]
Survey Contents	Housing and neighborhood Education and training Employment and earnings Income and public assistance Outlook and social networks Health Household composition Child education[2] Child health[2] Child behavior[2] Child time use[2] MTO experience	Education Employment and earnings Risky behavior Health Neighborhood and social networks Emotions Time use Future plans	Education Neighborhood, danger, and risk Health Behavior and family dynamics
Direct Measurement	Blood pressure	None	Height and weight
Educational Testing	None	Woodcock-Johnson Revised—selected tests	Woodcock-Johnson Revised—selected tests
Interviewer Observation	Interior conditions Exterior conditions Neighborhood conditions	Interaction between adult and youth	Interaction between adult and child

Notes:
1 Adults were selected for interviewing in the following order of precedence: female head of family intending to move through MTO; female spouse of family intending to move through MTO; wife of baseline head, if a member of the family intending to move through MTO; non-female (male or unknown gender) head of family intending to move through MTO.
2 The adult respondent was asked questions about each sampled child in the household, up to two.
3 Surveys were administered only to sampled children ages 8 to 11. Direct measurement and educational testing were carried out for sampled children ages 5 to 11.

Data were collected from sample members in two phases: the full sample phase (in which all 10,931 sample cases were worked) and the subsample phase (in which additional efforts were made to complete data collection with a subsample of full phase nonrespondents). An intensive data collection effort involving more than 100 interviewers achieved high response rates for both adults and children. When the responses for the full sample are combined with the weighted responses for the subsample

24 A small number of surveys with adult and youth respondents were administered by telephone.

of hard-to-find households, the effective response rate for the interim evaluation was 90 percent for the adults and 88 percent for the children. Appendix A of this report provides further details about the data sources, methods, sample sizes, and other features of the study's participant data collection.

Collection of administrative and published data for this study

The MTO Interim Evaluation drew upon several administrative databases for measuring both outcomes and mediating factors. A number of sources of published data were also used. There were five main categories of administrative and published data collected for this study:

1. State administrative data on earnings from covered employment in unemployment insurance programs and on benefits provided through TANF and food stamps programs.

2. Arrest and disposition data from local police agencies and courts.

3. Data from HUD administrative systems on participants in the public housing and Section 8 programs, on Section 8 Fair Market Rents, and on PHA expenditures for public housing operations.

4. Data on the schools attended by sample children (and their school districts) from state and local sources and the National Center for Educational Statistics Common Core of Data.

5. Published data from the U.S. Census of 1990 and 2000 at the census tract and block group levels.

The specific data sets in each of these categories are described in appendix A, which provides information on their time coverage and sources.

1.7 Overview of this Report

The balance of this report is organized into eight chapters with a number of appendices supporting them. Chapter 2 provides essential background information to the impact analyses across all the study's domains. It describes the sample's leaseup and mobility patterns after random assignment and estimates the program's impacts on mobility. Understanding these patterns is essential to interpreting the analytic results presented in chapters 3 through 8.

The quantitative analyses of MTO's impacts in each of the six study domains are found in chapters 3 through 8. Each of these chapters provides a review of the hypotheses about the potential effects of moves to low-poverty neighborhoods on outcomes and mediating factors in the domain analyzed, and then shows the results of testing those hypotheses against the quantitative data collected for this interim evaluation.

Chapter 9 synthesizes the results of the analyses across all the domains, in combination with the earlier qualitative study results. It addresses the policy implications of these interim evaluation findings as a whole. What have we learned about the impact of moves to low-poverty areas, at the 5-

year mark? What still remains to be investigated? Why might results differ at the end of the 10-year study period?

The report's appendices (referenced throughout the text) provide important details on the interim evaluation's data sources and data collection methods (appendix A) and estimation methods (appendix B). Appendices C through E contain supplementary tables for chapters 2 through 8. Appendix F provides additional analytic results for variants on the sample used in this study. Appendix G contains supporting materials for chapter 9.

Chapter Two
Geographic Mobility in the MTO Interim Evaluation Sample

This chapter presents an analysis of the interim evaluation sample's geographic mobility. It gives a picture of the sample members' old and current neighborhoods and assesses MTO's impacts on residential location.

Summary

The MTO intervention had statistically significant and moderately sizable effects on the sample members' choice of neighborhoods in 2002 and on the amount of time since random assignment spent in census tracts with lower poverty levels. While experimental group members did not (on average) spend very much time in low-poverty areas—and while many of the areas they first moved to were marked by increasing poverty over the decade between 1990 and 2000—there were still substantial and positive differences in neighborhood characteristics for the experimental group at the time of this study relative to the control group. There were also significant but smaller positive differences for the Section 8 group relative to the control group. These analyses provide critical context for understanding and interpreting the quantitative findings on program effects in housing, health, delinquency, education, employment and earnings, and income and receipt of public assistance. Those findings are presented in chapters 3 through 8.

2.1 Hypotheses about Mobility in MTO

All the hypothesized impacts of participation in MTO across the six study domains depend on residential mobility, the characteristics of neighborhoods to which sample members move, and the length of time they remain there. For the experimental group, the move out of public housing to a low-poverty neighborhood is meant to set the stage for exposure to the influences of an environment that might improve life chances. For the Section 8 group, the move out of public housing to private-market housing, whatever the neighborhood, sets the stage for exposure to differences in environment that might also alter future paths as compared with those remaining in project-based public or assisted housing.

Hypotheses about mobility shaped the MTO demonstration as well as the design of this interim evaluation. The initial hypotheses concerned what the families would do after random assignment:

- It was expected that the experimental group families would have difficulty using their vouchers due to the challenges of finding a unit in a low-poverty location. Mobility counseling was provided to help meet those challenges.

- Section 8 group families were expected to succeed in using their vouchers at about the same rate as local voucher holders generally and at a higher rate than the experimental group families.

- Control group families were expected largely to remain in their project-based subsidized housing.

Other hypotheses concerned what the sample members might do subsequent to those immediate results of the random assignment and voucher offers. Among the hypotheses about later mobility were these:

- After the initial period in their low-poverty locations (a year was required), experimental group families might choose to move again. Now unconstrained as to location, their choices would be broader. The experience of safer neighborhoods with better schools and more opportunities could lead these families to stay in lower poverty neighborhoods even if they moved from their original units. On the other hand, those experiencing isolation from friends, families, church, or other support networks might decide to move to higher poverty neighborhoods to be closer to their networks.

- Sample members in the Section 8 group would be expected to remain primarily in medium-poverty areas, based on earlier analyses.[25]

- Factors such as rent increases, unit conversions, building sales, or other features of the private market could lead to greater mobility for both treatment groups offered vouchers than for the control group remaining in project-based housing.

For families in the experimental and Section 8 groups, subsequent moves could result from changes in peoples' lives related to MTO—such as increased employment and earnings—and they could in turn affect the outcomes in other areas such as education or housing assistance. Thus it is important to examine both initial and subsequent moves as they relate to the outcomes of interest to this study.

Moves by the control group are also of keen interest to this study. Changes in income or family composition might have led sample members to leave public housing on their own. Control group families could have applied on their own for Section 8. Since the mid-1990s changes to public housing—such as the expansion of the HOPE VI program for severely distressed developments—have probably brought more mobility to public housing residents than would have been observed at an earlier period. Some private project-based developments have also undergone change, notably conversion to tenant-based assistance (vouchers).[26] We are interested here in all kinds of mobility so that we can detect and understand any impacts of MTO on movement patterns. The impacts we measure are those beyond what was happening to control group members.

The importance of mobility to MTO hypotheses overall

If the hypotheses about initial and followup mobility in each randomly assigned group proved true, positive effects on the treatment group sample members would be anticipated both in the short and the long run. Shorter term positive effects would include improved safety, better housing, and better

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[25] See Feins (2003) and Katz, Kling, and Liebman (1999a), both analyzing MTO data from 1997.

[26] As noted in chapter 1, the estimates presented in this report represent the incremental effects of MTO demonstration vouchers relative to what happened to the controls. Chapter 9 provides some additional information on this issue.

neighborhood conditions. Longer term positive effects could include greater educational attainment, higher rates of employment, and higher earnings and incomes. If the mobility outcome is that the experimental group tends to stay in private-market housing in lower poverty neighborhoods, while the control group tends to stay in public housing in higher poverty areas, then we can definitively test both the shorter term and longer term hypothesized impacts. The contrast between Section 8 group mobility outcomes and a fairly stationary control group would reflect the impact of moving away from project-based housing.

However, if the mobility effect is that both treatment groups—or even all three, including the control group families—move away from initial locations to neighborhoods with the same or similar average poverty levels (and other characteristics) over time, the hypotheses about MTO's impacts might predict instead that shorter term effects (e.g., on safety) would diminish over time and that longer term impacts might not ever be observed from the MTO demonstration.

2.2 Mobility Data Sources and Measures

Data sources

Using a combination of existing information from the 1997 and 2000 canvasses of the MTO sample, other tracking efforts, and interim household survey data, we assembled basic longitudinal information about the mobility history of each adult or child in the sample:

- The sequence (chain) of all residential moves for each sample member from the point of random assignment to the time of the interim data collection,
- The location of each confirmed dwelling in the chain of moves,
- The duration of stay in each confirmed dwelling in the chain of moves.

We used Census data from 1990 and 2000 to explore the nature of the surrounding neighborhood for each dwelling in the chain of moves.

The qualitative research identified factors leading to subsequent moves and explored in depth why some individuals and families who had initially moved to low-poverty areas later moved to higher poverty neighborhoods. The portions of the household and youth survey instruments that dealt with subsequent moves were developed using the evidence gathered through the qualitative field work on the most salient aspects of neighborhood and the ways that neighborhood differences were experienced by sample members.

Discussions of mobility during the qualitative interviews also offered the opportunity to ask sample members about their adjustment to the private housing market, the challenges of switching neighborhoods and schools, and how the respondents assessed each home and living environment encountered along the chain of moves. Obstacles to adjustment were identified and the adult survey instrument contained questions directed at determining how common these obstacles were across the interim evaluation sample.

Mobility measures

The key measures of residential mobility outcomes—also used as mediating factors in the impact analysis for other domains—included:

- The length of time at each location,
- The neighborhood characteristics of each location (via geocoding addresses to link with census variables),
- The respondent's interaction with the community,
- The respondent's assessment of the neighborhood,
- The respondent's reasons for leaving/staying in the community, including landlord willingness to continue leasing the unit to the respondent,
- The proportion of families moving from low-poverty to higher poverty locations.

One important use of these measures was for characterizing the areas where sample members lived when they enrolled in MTO and where they were living at the time of the interim evaluation. For all known addresses, links to 1990 and 2000 Census data at the tract and block group levels allowed construction of indicators about the local areas. The indicators were used to examine how the current locations of the experimental and Section 8 groups compared with those of the control group.

A second important use of these measures was to construct variables representing the length of exposure (in months) to specific kinds of environments—e.g., to low-poverty areas. Such exposure measures are a way to sum up a series of locations as they might be relevant to MTO outcomes. Which of these is more relevant depends to some extent on the outcome being considered. Neighborhood effects on the incidence of recent asthma attacks, for example, would be expected more in relation to current location than to neighborhood history. But neighborhood effects on school quality and educational achievement might be expected to result from the entire sequence of locations and schools after random assignment.

2.3 Baseline Conditions and Initial Leaseups

Neighborhood conditions at baseline

The MTO demonstration recruited families from public or assisted housing from among the poorest census tracts of five central cities. Using 1990 and 2000 Census data, we can characterize the families' locations at the time they joined MTO. These areas exhibited the effects of concentrated poverty:

- More than half their populations (on average) were living in poverty.

- Nearly three-fourths of the families in these neighborhoods were headed by a single, female parent.
- More than 30 percent of all residents were high school dropouts.
- Unemployment was over 25 percent and labor force participation was low for both men (55 percent) and women (38 percent).
- More than 40 percent of the families had no member working.

As described briefly in chapter 1 (and shown in exhibit C1.3), those who joined MTO reported very high levels of dissatisfaction with their neighborhoods. The threatening presence of drug and gang activity and high rates of crime victimization were powerful motivators for these adults who longed to raise their children in safer environments. To some of them, moving to safety might have been just as appropriate a name as moving to opportunity for this demonstration. They hoped to obtain vouchers through MTO that would enable them to move out and away from the threats and hazards of their baseline developments.

Leaseup rates and the determinants of leaseup success

All of the families randomly assigned in MTO to the experimental group and the Section 8 group were offered housing vouchers. Not every family randomly assigned to receive a voucher was able to find a unit to which they wanted to move that met the Section 8 Housing Quality Standards, with a landlord who would accept the family and a voucher and would sign a lease.[27] As shown in exhibit 2.1, some 47 percent of the families assigned to the experimental group in the interim evaluation sample moved under the program, while 62 percent of the families assigned to the Section 8 group participated.[28] The sample thus contains 1,566 program movers out of a total of 4,248 families.[29]

Lease-up rates ranged from 47 to 79 percent for the Section 8 group across the five sites, due to housing market differences and other factors. Lease-up rates for the experimental group (ranging from 33 to 67 percent) were also affected by differences in the counseling provided through MTO. The nonprofit counseling organizations varied in the breadth and intensity of their services, in the extent to which they included non-housing as well as housing-related assistance, and in the degree to

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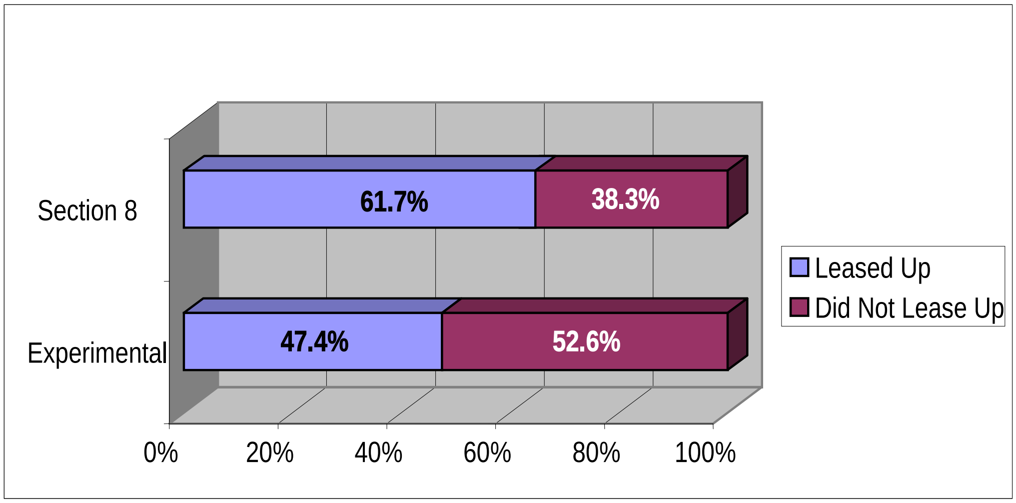
27 This process is known in program terms as leasing up. That not all voucher holders lease up is true of the national Housing Choice Voucher program as well. Only a portion of those offered vouchers are able to use them. The most recent study of participation rates in the program showed that 69 percent of families and individuals receiving vouchers in 2000 from large metropolitan housing authorities succeeded in using them (Finkel and Buron 2001, p. i.). That rate was higher in 1993 (just prior to the start of MTO), measuring 81 percent at that time.

28 MTO did not offer or give those assigned to the control group tenant-based subsidies with which to make such moves.

29 These leaseup rates mirror closely the leaseup rates for the MTO population overall (47 percent for the experimental group and 60 percent for the Section 8 group). That population includes additional families enrolled in 1998 in Los Angeles.

which they acted as advocates for their clients.[30] Changes in counseling personnel during MTO operations—and changes of the agencies filling this role in two sites (Chicago and Los Angeles)—may also have had some effect on client success.

EXHIBIT 2.1
MTO PROGRAM LEASEUP RATES BY GROUP



Source: MTO tracking logs.
Note: Weighted data.

Two prior research efforts examined the factors affecting leaseups in the MTO program. Feins, McInnis, and Popkin (1997) tested whether counseling affected the leaseup rate for families in the experimental group. They showed that client characteristics, market factors, and counseling utilization (the degree to which participants used services offered by the nonprofit agencies) were associated with the chance that experimental group families could lease up under the low-poverty location constraint. Families with one child (rather than more than one) were more likely to move as were families with lower incomes. In contrast, Hispanic families and those with higher incomes were less likely to move. Families with the head of the household in school and those dissatisfied with their neighborhoods were more likely to move.

A more recent analysis (Shroder 2002) looked at the program as a whole, examining the factors behind variations in leaseup rates across both the experimental and comparison groups. The author tested the effects of three sets of factors:

- Indicators of the probability of being accepted by a landlord including market factors, demographic characteristics, and personal factors.[31]

[30] The MTO demonstration design allowed for a considerable range of counseling practices. See Feins, McInnis, and Popkin (1997).

- Indicators of the net benefit of changing units including preferences and attitudes expressed at baseline in survey data.

- Indicators of the costs of housing search, including wage and income characteristics of the sample member, access to transportation, and features of the counseling treatment offered participants in the experimental group at the sample member's site.

Factors from each of the three sets were associated with leasing up. For example, looser metropolitan housing markets increased the probability of leasing up while larger family size reduced it. The adult's positive attitude (as expressed in answers to the baseline survey) was a significant predictor of success, as was greater dissatisfaction with the sample member's baseline location. For the experimental group—which faced a locational constraint (the requirement to move to a low-poverty area) but which could take advantage of counseling services from a local nonprofit organization to assist in the process—on average the effect of the constraint outweighed the effect of the counseling. The locational constraint reduced the leaseup rate for that group (compared with the Section 8 group), although counseling utilization was positively associated with leaseup rates (partially helped overcome the constraint).

The leaseups that occurred through the MTO program were by no means the only moves sample members made after random assignment, as will be seen in the next section. Control group families could move on their own. Families that did not succeed in leasing up could move on their own. Experimental group families that leased up could move again after a year, without the location constraint. But the experimental treatment was intended to alter the pattern of moves that might ordinarily occur, and the quantitative analyses conducted for this study address whether that purpose was achieved.

2.4 Sample Mobility in the Followup Period

In the 4 to 7 years after random assignment, the interim evaluation sample was quite mobile.[32] Exhibit 2.2 divides the adult sample members into those who leased up (moved from their baseline locations with the vouchers MTO provided) and those who did not lease up. The latter group is divided between those who moved subsequently (left their baseline homes without the help of MTO) and those who stayed (were still in their baseline locations in 2002).

Exhibit 2.2 shows that 30 percent of the families assigned to the control group were still at their baseline addresses in 2002. The high percentages of movers in the control group (69.6 percent) and

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[31] Factors influencing whether the owner of the unit is likely to agree to lease to a family holding a housing voucher might include the local vacancy rate (i.e., whether there are many other potential tenants for the unit), the size and composition of the household, their race or ethnicity, and the household head's self-presentation in seeking to rent (reflecting her confidence in her ability to find a unit but also some otherwise unmeasured attributes). See Shroder (2002) for details.

[32] See exhibit C2.1 in appendix C for a map of the sample adults' locations across the United States in 2002.

among the experimental and Section 8 group families who did not move with the MTO vouchers (35 percent and 22.7 percent, respectively) result in part from major changes in their public housing environments due to federal and local efforts to deal with distressed public housing. These efforts included the HOPE VI program (which provided federal funds to demolish many units and to rebuild some), vacancy consolidation (demolition) efforts, and local comprehensive modernization projects. At baseline, 22 percent of the control group members in the sample lived in developments that were (or were scheduled to be) affected by these programs during the period in which the MTO program was recruiting and enrolling families.

EXHIBIT 2.2
GEOGRAPHIC MOBILITY IN THE INTERIM EVALUATION ADULT SAMPLE BY TREATMENT GROUP

	Experimental Group	Section 8 Group	Control Group	Total
Leased Up	47.4%	61.7%	N/A	36.9%
Did Not Lease Up				
Moved	35.0%	22.7%	69.6%	42.2%
Stayed	17.5%	15.7%	30.4%	21.0%
All Adults	100%	100%	100%	100%

Sources: MTO data system, adult survey
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Data are weighted as described in appendix B. Numbers may not sum to 100 due to rounding. Mobility patterns in this exhibit are for the full period since random assignment.

Thus some of the families in all three groups who did not lease up through MTO but did move subsequently may have been required to move around by the housing authorities. Those in the treatment groups who did not move through MTO might well have been affected by the same public housing changes as the control group. In fact, another 3 percent of the experimental group members lived in the same block group in 2002 as at random assignment, as did another 2 percent of the Section 8 group. These adults could be considered to have stayed, too. [33]

Some 16 to 17 percent of the experimental and Section 8 groups were still in their baseline locations by 2002 when this study's data were collected. Total mobility rates for these two groups were thus

[33] According to the Census Bureau a block group is a cluster of census blocks generally containing 600 to 3,000 people, with an optimum size of 1,500 people. By contrast, a census tract generally has 1,500 to 8,000 people, with an optimum size of 4,000 people. In urban areas block groups might typically combine 3 to 4 city blocks. Although the block group is useful for establishing tightly clustered locations, in this report we primarily use census tract-level data since (a) that was the basis for the MTO experiment, (b) the boundaries of tracts generally change less over time than those of block groups, and (c) the standard errors for block groups tend to be quite large because of their relatively small sample sizes and the Census Bureau's data masking methodology.

roughly equal. The initial leaseup rate for the Section 8 group was larger than the initial leaseup rate for the experimental group, but a larger proportion of the experimental group subsequently moved on their own.

Program moves—experimental group

For the members of the experimental group who leased up through MTO, the initial moves to low-poverty areas were both satisfying and challenging. Respondents in the qualitative study sample frequently commented on the improvement in safety as the most important aspect of their moves. However, some households in low-poverty neighborhoods experienced a loss of access to convenient transportation, free recreational activities, health care, shopping, and church that those in more central locations enjoy. MTO families moved from large public housing developments to a variety of housing types including single-family homes, duplexes, townhouses, and apartment complexes. Experimental group movers were more likely than Section 8 movers to live in single-family homes or townhouses.[34] Most of the experimental group respondents in the qualitative sample rented from small landlords rather than large management companies. To a great extent, their perceptions of their housing depended on the quality of their relationships with the buildings' owners.

To use their MTO vouchers the families in the experimental group were required to move to census tracts with less than 10 percent poverty according to the 1990 Census. Exhibit 2.3 shows the poverty rate of the neighborhoods to which experimental group families moved with their MTO vouchers. In the first panel we see the poverty rates measured in 1990 Census data—the same data used to identify low-poverty areas when the demonstration was operating. The first panel shows that there was substantial compliance with the locational constraint, with 89 percent of the experimental group making program moves to areas with less than 10 percent poverty, and 94 percent moving to areas with less than 11 percent poverty.[35]

Now that Census 2000 data are available, what do they tell us about the initial destinations of experimental group movers? The second panel of exhibit 2.3 shows the poverty rates for the same locations as the first panel, but with poverty measured in April 2000 rather than April 1990. These figures are quite different from the previous ones. They show that only about 40 percent of the experimental group's program move locations were still low-poverty areas in 2000, although 90 percent were still in areas of less than 20 percent poverty.

We can also use the 1990 and 2000 poverty rates to estimate what the actual poverty rates may have been in the census tracts to which experimental group families moved at the time they leased up. These estimates are shown in the third panel of exhibit 2.3. These estimates are based on the change in poverty rates in the destination census tracts over the decade from 1990 to 2000. As the comparison of the exhibit's first two panels indicated, a considerable proportion of the experimental group's destination tracts increased in poverty population during the decade. As a result, just half of the moves were to areas estimated to really have poverty rates below 10 percent at the time of the

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[34] This information comes from the Neighborhood Assessments that the qualitative interviewers completed.

[35] A considerable number of leaseups occurred in census tracts with poverty rates up to 10.9 percent. HUD also granted a small number of waivers in special circumstances for leaseups in higher poverty locations.

move and another third were to areas of 10 to 15 percent poverty at the time. All told, 97 percent were to areas with less than 20 percent poverty. The remaining 3 percent of experimental group program moves were to census tracts with still higher poverty rates.

EXHIBIT 2.3
NEIGHBORHOOD POVERTY RATE AT TIME OF FIRST LEASEUP

	LT 10%	10–15%	15–20%	20–30%	30–40%	40%+	Mean
Poverty Rate in 1990 Census							
Experimental Group							
Program movers (n=813)	89.0	7.6	1.4	1.4	0.5	0.2	7.5%
Section 8 Group							
Program movers (n=735)	10.7	12.9	14.1	24.1	21.1	17.2	26.9%
Poverty Rate in 2000 Census							
Experimental Group							
Program movers (n=815)	38.9	33.2	17.9	8.8	0.5	0.6	12.4%
Section 8 Group							
Program movers (n=737)	5.9	7.6	15.0	28.8	24.0	18.7	28.4%
Estimated Poverty Rate at Time of Move[a]							
Experimental Group							
Program movers (n=813)	50.7	33.8	12.2	2.3	0.6	0.4	10.8%
Section 8 Group							
Program movers (n=735)	6.9	7.9	14.9	29.3	21.2	19.9	27.8%

Source: MTO data system, 1990 and 2000 census tract-level data.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Data are weighted as described in appendix B.
(a) Estimates were made using a simple linear interpolation over the decade between the 1990 census and 2000 census. For example, if the 1990 poverty rate in the destination census tract was 8 percent but in the 2000 census it was 12 percent, over the decade the rate was assumed to change by .4 percent per year. For a leaseup in 1995 in this tract, the estimated poverty rate at that time would be about 10 percent. (The formula used the actual date of the program move and estimated the poverty rate based on days elapsed from April 1, 1990.)

The dynamic underlying this finding is the demographic and socioeconomic change in these local areas during the 1990s. Students of the geography of poverty have noted that the 1990s saw a reduction in poverty concentration in many U.S. cities (Jargowsky 2003; Kingsley and Pettit 2003). The sharp decline in the proportion of poor people living in high-poverty census tracts has garnered the most attention. But at the same time, poverty spread into the middle ranges. Balancing the 5 percent reduction in the concentrated poverty tracts were increases of 5 percent in the tracts with 10 to 30 percent poverty rates (Kingsley and Pettit 2003, p. 3). Most salient to MTO, the share of the metropolitan poor living in census tracts with poverty rates of 10 to 20 percent increased by 2 percent over the decade.

To explore the dynamic further, we can categorize the neighborhoods to which experimental group families actually moved by the changes in poverty rates during the decade from 1990 to 2000. We have broken the distribution into three categories—decreasing poverty rates, stable poverty rates, and increasing poverty rates. Stable areas are defined as census tracts with changes of no more than 5 percent in either direction during the decade.

Exhibit 2.4 shows that just over half of the locations chosen by experimental group families were characterized by decreasing or stable poverty rates, but the other half had increasing poverty rates through the 1990s. We can speculate on the reasons for this pattern, which is quite distinct from the pattern for the Section 8 group (described below). But the implication for the MTO experiment is to raise questions about some of the neighborhoods to which the experimental group families moved. It seems likely that neighborhoods with increasing poverty rates are neighborhoods beginning to decline. Moves to low-poverty neighborhoods in decline may not provide the opportunity-rich environments hypothesized to improve the lives and well-being of the movers.

The likely reason for this pattern is that experimental group families found it easier to rent units in neighborhoods experiencing some decline in prosperity. These might be areas seeing reduced demand for their rental stock, with softening rents. Landlords might therefore be more willing to rent to families on Section 8 or families from public housing than in the past. However, the arrival of experimental group families alone (or of the few Section 8 group families making low-poverty program moves) did not play a role in the decline of these areas, because the numbers of MTO families moving to any single neighborhood were too small to be influencing tract-level changes. At the most, experimental group families accounted for 2.3 per 1,000 households in the low-poverty destination tracts (Goering et al., 1999, p.42).

EXHIBIT 2.4
CHANGING NEIGHBORHOOD AT TIME OF FIRST LEASEUP?[1]

	Decreasing Poverty Rate, 1990–2000	Stable Poverty Rate, 1990–2000	Increasing Poverty Rate, 1990–2000	Total
Experimental Group				
Program movers (n=813)	1.2%	54.3%	44.6%	100%
Section 8 Group				
Program movers (n=735)	18.0%	46.3%	35.7%	100%

Source: MTO data system, 1990 and 2000 census tract-level data.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Data are weighted as described in appendix B.
1 Stable poverty rates are defined as tract-level poverty rates changing less than 5 percent in either direction between 1990 and 2000. Decreasing poverty rates are tract-level rates that fell 5 or more percentage points in the decade, while increasing poverty rates are tract-level rates that rose 5 or more percentage points in the decade.

The dynamics of the neighborhood changes differed among the five MTO sites (see the maps in appendix exhibits C2.2 to C2.6 for reference). In Boston and Chicago, about 30 percent of the experimental group program movers chose locations in census tracts that increased in poverty from 1990 to 2000. In Baltimore, 41 percent of the experimental group program movers did the same. Just over half the Los Angeles experimental group lease-ups were in destination tracts marked by rising poverty, but LA was the only MTO site where the MSA-wide poverty concentration increased, cutting against the national pattern (Jargowsky 2003, pp. 11, 14-17). The site with the highest proportion of experimental group movers (60 percent) leasing up in tracts with rising poverty was New York. By the time of the 2000 Census, only one-third of the leaseup locations in that site had poverty rates under 10 percent.[36]

Examining the locations of the low-poverty tracts where MTO families lived in 2002, it is also notable that more than half of these tracts were outside the central cities of the five metropolitan areas. About 58 percent of the low-poverty tracts were in suburban areas, compared to 27 percent of the tracts with poverty rates in the 10 to 15 percent range, 18 percent of the tracts with poverty rates in the 15 to 20 percent range, and less than 10 percent of the tracts with poverty rates of 20 percent or more.

Program moves—Section 8 group

When they were issued their vouchers through MTO the families assigned to the Section 8 group encountered the same Section 8 rules and procedures and faced the same range of choices as any other person seeking to lease up for the first time with tenant-based housing assistance. Because most of them were public housing residents, they could not lease in place—that is, they could not use their vouchers for their current apartments. This often makes it easier to become a Section 8 participant.[37] But neither was there a constraint related to the characteristics of the neighborhoods they could consider, as there was for the experimental group.

Most of the 60 percent of Section 8 group adults who did move with the MTO voucher leased up in areas with poverty rates of 20 percent or more (exhibit 2.3). Indeed, almost one-fifth of them leased up in concentrated poverty areas (40 percent plus). Only a small share of the Section 8 group (about 10 percent) leased in census tracts with poverty rates below 10 percent. Comparing the experimental and Section 8 groups in exhibit 2.3 makes it clear that there was a substantial difference in the initial locations chosen by the two groups.

From the vantage point offered by 2000 Census tract-level data, almost half of the neighborhoods chosen by the experimental group program movers were increasing in poverty during the decade. The picture is very different regarding the neighborhoods to which the Section 8 group families made program moves (exhibit 2.4). Almost 20 percent of the families moved to neighborhoods characterized by decreases in poverty after 1990 and about 45 percent moved to areas with stable poverty rates. Because the areas chosen had much higher poverty rates to begin with, this does not

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[36] Most of the remainder (58 percent) were in areas with poverty rates of 10 to 19 percent. About 9 percent were in areas with poverty rates of 20 percent or more.

[37] Some families joined MTO from project-based assisted housing and under some circumstances it was possible for them to lease in place. At least one family did so. Over the long term leasing in place has been a major source of units for Section 8 participants nationwide. But Finkel and Buron (2001) report in their recent Section 8 success study that only 21 percent of successful Section 8 enrollees leased in place in 2000 compared with 37 percent in 1993.

mean that the Section 8 group chose more promising communities, only that these areas started from a different level and were on a different trend line.

Control group mobility

The mobility of the control group tells us what mobility patterns the experimental and Section 8 group families would have followed without MTO. The control group families started out in high-poverty census tracts (40 percent or more persons living in poverty according to the 1990 census).[38] Exhibit 2.5 shows that by 2002, only 48 percent of the control group continued to live in concentrated poverty areas. This decline is largely due to control group members moving; two-thirds moved between random assignment and 2002. Of the control movers, in 2002 66 percent lived in areas with somewhat lower poverty rates than at baseline, and 24 percent lived in areas with less than 20 percent poverty. Note in exhibit 2.5 the similarity in patterns for the stayers in all three groups and for the nonprogram movers in all three groups.

The stayers in the control group were still concentrated in high-poverty neighborhoods in 2002, but 40 percent of them were in census tracts that had decreased in poverty over the previous decade. The same was true for a smaller share of the control group movers (26 percent). Overall, 44 percent of the control group adults were living in areas with stable poverty rates from 1990 to 2000.

Experimental group mobility—subsequent moves

MTO demonstration rules required that experimental group families sign 1-year leases for the units they rented with the program's vouchers. After that year, there was no further locational constraint and the families could stay or move as they wished within the rules of the Section 8 program. By the time of the interim evaluation, one-third of the experimental group program movers remained in the same neighborhood (census tract) as their initial move, although perhaps not at the same address. A full two-thirds of these movers had moved outside these tracts.

After leasing up, 66 percent of the adults in experimental group program mover families made one or more additional moves. The survey data show that when sample members began looking for housing to make a subsequent move, over two-thirds reported searching in the same or similar neighborhoods to that of their leaseup address. However, we know from the mobility histories that by 2002 many had ended up changing locations. And for the experimental group, this meant moving to areas more like the ones where Section 8 families and control group movers lived.[39]

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38 There was an exception made to this 40 percent requirement in Boston. HUD agreed to include in MTO three public housing developments in areas below the 40 percent threshold because of high local crime rates and other conditions consistent with MTO targeting.

39 Exhibits C2.2 through C2.6 in appendix C are maps showing the 2002 locations of program movers in the experimental and Section 8 groups.

EXHIBIT 2.5
CENSUS 2000 POVERTY RATE OF CURRENT RESIDENTIAL LOCATION[1]
(PERCENT DISTRIBUTIONS–WEIGHTED DATA)

	Under 10%	10–15%	15–20%	20–30%	30–40%	40%+	Mean
Control Group							
Stayed in place (n=343)	0	0	0	3.4	17.2	79.4	51.1%
Moved (n=793)	5.4	8.1	10.0	20.0	22.7	33.8	33.6%
Total control group (n=1136)	***3.8***	***5.7***	***7.0***	***15.0***	***21.1***	***47.6***	***38.9%***
Experimental Group							
Did not lease up	***2.7***	***4.3***	***6.5***	***13.1***	***26.4***	***47.0***	***39.6%***
Stayed (n=267)	0	0	0	3.1	24.7	72.1	49.1%
Moved (n=518)	4.1	6.5	9.8	18.3	27.3	34.1	34.6%
Leased up	***25.3***	***19.1***	***15.7***	***18.5***	***11.7***	***9.7***	***20.0%***
Did not move again (n=245)	38.4	33.1	14.4	12.9	0.6	0.6	12.6%
Moved again (n=456)	18.2	11.6	16.4	21.5	17.7	14.6	24.0%
Total experimental group (n=1,485)	***13.3***	***11.3***	***10.8***	***15.7***	***19.5***	***29.4***	***30.4%***
Section 8 Group							
Did not lease up	***2.8***	***7.2***	***6.3***	***11.6***	***23.7***	***48.4***	***38.3%***
Stayed (n=166)	0	0	0	3.1	26.3	70.6	46.8%
Moved (n=242)	4.7	12.1	10.6	17.5	21.9	33.2	32.5%
Leased up	***6.4***	***9.2***	***15.0***	***26.3***	***23.2***	***20.0***	***28.6%***
Did not move again (n=215)	5.1	6.5	15.6	27.5	22.6	22.8	29.1%
Moved again (n=426)	7.0	10.6	14.6	25.7	23.5	18.6	28.4%
Total Section 8 group (n=1,050)	***5.0***	***8.4***	***11.6***	***20.6***	***23.4***	***31.1***	***32.4%***

Source: MTO data system
Sample: Adults from families randomly assigned through December 31, 1997.
Note: Data are weighted as described in appendix B.
[1] Measured at the census tract level.

These second movers tended to move to higher poverty neighborhoods than those that stayed but lower poverty areas than those who did not lease up (exhibit 2.5). It is also worth noting that two-thirds of the families that did not lease up under the program did eventually move on their own. These families, too, tended to move to neighborhoods with moderate to high poverty rates.

The main reasons experimental group movers moved again after their initial leaseup were because of leasing problems (22 percent) and conflicts with their landlords (20 percent). Getting a bigger or better apartment was the second most common reason for moving (18 percent). Safety was a small but not insignificant reason (9 percent) for second moves by experimental group program mover families. Although costs may have been a factor in disputes with landlords, higher rents (1 percent) and utilities (1.3 percent) were not generally reasons the experimental group program movers gave for leaving the homes to which they moved through MTO.

A number of experimental group respondents to the qualitative interviews reported that the MTO move was the first time they had ever rented from an individual landlord. Some had very good relationships with their landlords and were pleased with the way they maintained their units. Others had landlords whom they felt were hostile, unresponsive about maintenance problems, and raised their rents. Some owners sold their buildings with little warning.

The qualitative interviews suggested that those who had good relationships with landlords were more likely to stay in their new communities, while those who were less satisfied were more likely to make subsequent moves.[40] An experimental group mover in New York talked at length about how happy she was with her apartment and her landlord. He owned a dry cleaning business in the first floor of the building and was friendly with both her and her daughter. The mover raved about how well he maintained the building:

> *My landlord, anything that breaks, he up here the next day. He's very helpful. … He keeps up with everything. The exterminator comes in once a month. He's fabulous. He's very helpful.*

Other respondents complained about problems with their landlords. For example, one woman in the experimental group in New York reported a range of maintenance issues with her private market unit, including rats and rodents, problems getting exterior lights repaired, and paper-thin doors. She said her landlord was unresponsive about making repairs:

> *Before the tenant upstairs moved up there, we had rats and rodents a couple of months. Three or four months 'fore he send the exterminator. … The landlord is the pits! … He's one step from being a slumlord. Nothing gets fixed. Nothing. He does absolutely nothing.*

Indeed, landlord problems frequently were the factor that prompted a subsequent move. In the survey data, landlord problems were the second largest category (20 percent) of reasons given for moves away from the initial housing in low-poverty neighborhoods.

Section 8 group mobility—subsequent moves

Like the sample members assigned to the experimental group, many families in the Section 8 group who had moved through MTO moved again in the period before the interim evaluation. In fact, two-thirds of the families that leased up in the Section 8 group were living in different locations by the time of the interim evaluation.

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40 The survey data indicating problems with landlords were not collected in a way that allowed us to test these relationships more generally.

Families moved again for a variety of reasons including: problems with their current housing unit or landlord; housing costs; desire to be closer to (or further from) friends and family; lack of adequate public transportation; and distance from shopping, school, or employment. The survey data indicate that program movers' motivations for leaving their initial homes differed somewhat between the experimental and Section 8 groups. For the Section 8 group, safety issues (23 percent) and building issues (13 percent) figured prominently while landlord problems did not.[41] Both groups put getting a bigger or better apartment high on the list of reasons they moved again.

A considerable number of the Section 8 families who failed to lease up during MTO were also able to move from their baseline locations. Exhibit 2.5 showed the poverty characteristics of this group's current locations according to the move patterns of the members. About 5 percent were living in areas with poverty rates below 10 percent when this study's data were collected and a quarter of them were living in areas with poverty rates under 20 percent.

Geographic mobility and neighborhood racial composition

Up to this point, we have examined sample mobility in terms of a single characteristic of residential locations—the poverty rate. Broadening this perspective, exhibit 2.6 provides parallel figures on the racial and ethnic makeup of the census tracts where the sample lived at the time of this study's data collection. (The measure combines racial minorities with Hispanic ethnicity into total percent minority population.) MTO was not designed to address issues of racial or ethnic concentration directly. But its roots in the Gautreaux Program and its parallels with several remedial mobility programs ordered by courts during the 1990s make the racial composition of destination neighborhoods a question of real interest. Also, early MTO research showed some significant treatment effects on the racial composition of the sample's locations in 1997 (Feins 2003).

In 2002 the vast majority of sample members (87 percent) lived in areas of extremely high minority concentration (80 percent or more). Only a handful of the adults (2 percent) lived in areas with less than 20 percent minority population and just 8 percent lived in areas with less than 40 percent minority population. The pattern appeared to differ only slightly by random assignment group. About 10 percent of the experimental group and 6 percent for the Section 8 group lived in areas with less than 40 percent minority population. Thus sample members from all groups were living in highly segregated neighborhoods, although the areas might differ in their poverty rates.

The low-poverty areas initially chosen by experimental group movers in the interim evaluation sample were considerably less segregated, with an average minority population of 51 percent according to the 1990 Census. However, by 2000 these same areas had an average percent minority population of 67 percent.

The neighborhoods of the MTO families who lived in low-poverty census tracts in 2002 were still much less racially concentrated than tracts with higher poverty rates where other MTO families were living. For example, experimental group members' low-poverty tracts averaged 46 percent minority

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[41] Experimental group members cited safety much less frequently but leasing problems more frequently. Some of these problems may have forced the families to move. For example, some respondents indicated that landlords would not renew leases or that Section 8 would not approve the unit on HQS again.

population, and Section 8 group members' low-poverty tracts averaged 53 percent minority population. But as Exhibit 2.5 made clear, only a small proportion of the sample in either group did live in low-poverty areas. The 2002 neighborhoods of the families living in areas with poverty rates of 15 percent or more averaged 75 percent or more minority population.[42]

EXHIBIT 2.6
CENSUS 2000 PERCENT MINORITY OF CURRENT RESIDENTIAL LOCATION[1]
(PERCENT DISTRIBUTIONS)

	Under 20%	20–40%	40–60%	60–80%	Over 80%	Mean
Control Group						
Stayed in place (n=343)	0	2.4	3.1	5.2	89.3	93.8%
Moved (n=793)	2.6	5.3	6.5	10.1	75.4	85.3%
Total (n=1136)	***1.8***	***4.4***	***5.5***	***8.7***	***79.6***	***87.8%***
Experimental Group						
Did not lease up	***1.5***	***3.0***	***4.7***	***6.0***	***84.9***	***90.2%***
Stayed (n=267)	0	1.1	4.2	2.5	92.2	94.2%
Moved (n=518)	2.3	3.9	5.0	7.8	81.1	88.1%
Leased up	***5.7***	***10.5***	***10.9***	***13.5***	***59.4***	***75.4%***
Did not move again (n=245)	7.5	17.0	11.0	19.3	45.2	68.0%
Moved again (n=456)	4.7	7.1	10.8	10.3	67.1	79.4%
Total (n=1,486)	***3.5***	***6.5***	***7.6***	***9.5***	***72.9***	***83.2%***
Section 8 Group						
Did not lease up	***2.4***	***5.4***	***6.1***	***7.4***	***78.7***	***86.7%***
Stayed (n=166)	0	2.7	8.6	2.7	85.9	91.4%
Moved (n=242)	4.0	7.6	4.0	10.6	73.8	83.5%
Leased up	***2.2***	***3.2***	***9.6***	***9.0***	***76.0***	***85.5%***
Did not move again (n=215)	1.4	1.4	8.5	9.9	78.8	87.6%
Moved again (n=426)	2.5	4.2	10.2	8.6	74.6	84.4%
Total (n=1,049)	***2.2***	***4.2***	***8.2***	***8.4***	***77.0***	***86.0%***

Source: MTO data system
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Data are weighted as described in appendix B.
[1] Measured at the census tract level.

[42] This pattern varied little by site. The MTO families in Boston lived in areas averaging 66 percent minority for the experimental group and 72 percent minority for the Section 8 and control groups. In all the other sites, the average percent minority in the neighborhoods of all three groups was over 80 percent.

Geographic mobility and other neighborhood characteristics

Exhibit 2.7 provides a summary view of other area characteristics for the baseline locations of all the adults in the interim evaluation sample and for the control group's locations at the time of this study. The exhibit's upper panel presents a number of concentrated poverty indicators while the lower panel presents several opportunity indicators—the types of neighborhood characteristics hypothesized to benefit participants who moved to low-poverty areas. The exhibit first shows how the sample's origin (baseline) locations looked on these indicators measured in 1990 Census data. Then it shows the same characteristics of the same addresses measured in 2000 Census data. Finally, the exhibit shows the characteristics of the control group's current residential locations (also measured in 2000 Census data).

The origin locations for all sample members were clearly in neighborhoods with multiple problems—high poverty and dependence on public assistance, many high school dropouts, high unemployment. The 1990 Census data show that three-fourths of all households in these areas consisted of female-headed families with children and that 42 percent of the families had no working members. Only half of the families had wage or salary income and less than one-fifth of the adult population had any education beyond high school.

The 1990s saw two major interventions to change these neighborhoods and the lives of their residents: the major public housing initiatives described earlier in this chapter and the major changes in the federal welfare laws with the end of Aid to Families with Dependent Children (AFDC) and the advent of the Temporary Assistance for Needy Families (TANF) program. The decade was also a period of sustained economic growth. By the mid-1990s when MTO was recruiting volunteer families, some of the indicators shown in exhibit 2.7 had probably begun to change, but we have no small-area data to examine from that period.

By April 2000 when the data for the next census were gathered, a number of the indicators for the baseline neighborhoods showed improvement (column 2). The rate of public assistance receipt had fallen to fewer than a quarter of the population, less than one-third of the families had no working members, 60 percent of the families had wage or salary income, and one-fourth of the adult population reported education beyond high school. On the other hand, half the populations of these areas were still living below the poverty line and the local unemployment rate averaged 25 percent.

The differences between the first two columns of exhibit 2.7 are all due to changes in the decade from 1990 to 2000, with the locations held constant. But the last column—showing the same indicators for the 2002 locations of control group adults—reveals that the contrasts are much greater when the control group's mobility is taken into account. These differences (between columns 1 and 3) are the joint result of movement by control group members to different neighborhoods and changes in the neighborhoods themselves.

EXHIBIT 2.7
MEAN CHARACTERISTICS OF ORIGIN LOCATIONS AND CONTROL GROUP 2002 LOCATIONS
(PERCENT DISTRIBUTIONS)

Tract Characteristic	All Groups Combined	All Groups Combined	Control Group
	Origin Locations 1990 Census	Origin Locations 2000 Census	2002 Locations 2000 Census
Concentrated Poverty Indicators for the Sample's Residential Locations			
Persons in poverty	56.0	49.1	38.9
Households receiving public assistance income	45.9	23.0	17.6
Female-headed families with own Children	72.8	64.5	56.8
High school dropouts	32.8	30.2	25.8
Unemployment rate	26.7	24.5	19.0
Labor force participation			
Males	54.9	49.9	54.6
Females	38.2	43.5	47.2
Families with no workers	42.0	29.9	24.2
Opportunity Indicators for the Sample's Residential Locations			
Persons with incomes twice the poverty level	23.7	27.2	37.3
Households with wage or salary income	51.8	60.1	66.4
Persons with education beyond HS	19.3	25.3	30.7
Some college	10.1	13.0	15.7
College graduate	9.1	12.3	15.0
16 to 19-year-olds in school	67.2	72.7	74.8
Owner-occupied housing	7.6	9.8	22.9
Racial and Ethnic Composition of Population			
Black	65.4	57.2	55.6
Hispanic	26.6	32.1	28.9
Minority	90.7	91.3	87.8

Sources: MTO data system, U.S. Census
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Data are weighted as described in appendix B. Results for origin addresses are measured first in 1990 Census tract-level data and then in 2000 Census tract-level data. Results for control group's 2002 locations are measured in 2000 Census tract-level data.

The neighborhoods where control group members lived at the time of this study had average poverty rates just under 40 percent (the widely used threshold for high-poverty areas and MTO's targeting criterion for recruiting families into the demonstration). Compared with the baseline neighborhoods, the areas were characterized by lower unemployment, higher labor force participation, and fewer families with no workers. Two-thirds of all households in these areas had wage or salary income and 30 percent of the adults had more than a high school education. Perhaps most striking, the areas were marked by much higher rates of owner-occupied housing (23 percent compared to 10 percent), an indicator often taken as a sign of residents having a greater stake in their areas and being more likely to maintain or improve conditions there.

2.5 Geographic Mobility Impacts

Thus it appears that even without the MTO intervention the residential locations of the participants, as measured by where the control group families were living in 2002, would have been considerably better than they were when these families joined MTO. Even so, the mobility patterns described above for the two treatment groups differ in a statistically significant way from the mobility patterns of the control group. By measuring the impacts of the two treatments (experimental and Section 8) on locations and exposure periods, we can ascertain to what extent the premise of MTO—moving families from concentrated poverty areas to low-poverty areas—was actually met.[43]

Early in this chapter, we discussed a number of hypotheses about the mobility of the MTO sample members. There were reasons to expect the treatment groups to show greater mobility, after their initial moves than the control group. However, changes in public housing during and after the MTO program were an important stimulus to nonMTO mobility. More than two-thirds of the control group adults have moved since they joined MTO, and so have two-thirds of the Section 8 group members and 60 percent of the experimental group members who did not lease up with the MTO vouchers.

We also described hypotheses about why experimental group families might stay in (or leave) low-poverty neighborhoods. As earlier tables have shown, the high mobility since random assignment has led experimental group movers to move toward higher poverty locations while control group families moved toward lower poverty areas. However, exhibit 2.8 shows that even with these moves, families that moved to low-poverty areas are still in considerably lower poverty neighborhoods than control families, with Section 8 group families falling in between.[44]

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[43] Exhibits C2.7 through C2.11 show the 2002 locations of sample members from all three groups (for the original MTO metropolitan areas).

[44] Section 1.4 and appendix B provide assistance in interpreting exhibit 2.8 and the other exhibits with tests of outcomes in the remainder of the report.

Exhibit 2.8 shows the effects of the experimental and Section 8 treatments on the sample adults' current choices of locations, in terms of two characteristics: the poverty rate and the percent minority. The first panel shows the effects on poverty rates for the whole sample and for specific subgroups within it.[45] The average poverty rate (percent poverty population) in current neighborhoods for the control group—combining all racial and ethnic groups—was 38.7 percent. There are statistically significant effects for both the experimental and Section 8 groups. Being assigned to the experimental group alters the families' neighborhood choices and (as a result) lowers the average poverty rate by 8 points, while for the Section 8 group the intent-to-treat effect is 6.5 points. The treatment-on-treated effects, which estimate effects only for those in the treatment groups who actually moved through MTO, are larger.[46] They indicate that the low-poverty location constraint led the experimental group families who leased up to live in neighborhoods with poverty rates an estimated 17 points lower than they would have without their involvement in MTO. The effect for those leasing up in the Section 8 group was an estimated 11 point reduction in the poverty rates of their chosen neighborhoods. The participants' race and ethnicity make some difference in the effects concerning the poverty rate of the current location, with slightly smaller effects for Hispanic sample members.

Thus with respect to neighborhood poverty rates, the MTO treatments do affect locational choices, resulting in significantly reduced poverty rates of areas chosen by both experimental and Section 8 group members compared to those occupied by control group families. The experimental group counterfactual mean of 37.2 percent poverty is reduced to 20 percent. The Section 8 group counterfactual mean of 38.9 percent is reduced to 28.6 percent. The magnitude of these reductions is notable, particularly the fact that experimental group movers on average face local poverty rates just half that of the concentrated poverty threshold level of 40 percent. The reduction is also notable because it is measured relative to control group locations, many of which underwent major changes through HOPE VI and other programs to remove or redevelop severely distressed public housing.

The lower panel of exhibit 2.8 tests for differences relative to the control group in the racial and ethnic composition (percent minority) of the sample members' current residential locations. Only the experimental group showed a significant impact on this outcome, with reductions in percent minority population 4 percent (ITT) and 10 percent (TOT) relative to the neighborhoods where control group adults were living. However, the control mean for percent minority population is very high. On average the control group adults were living in areas of about 88 percent minority population at the time of the interim evaluation. Reductions of 4 or even 10 percentage points make little difference to these figures.

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[45] All subgroups in the impact analyses are defined by baseline characteristics, so that they are exogenous to the treatment and can be tested within the experimental design.

[46] They are calculated by dividing through the coefficients by the (fractional) share of leaseups in each group. Thus the TOT effects will always be larger than the ITT effects and so will their standard errors (by the same proportion). The adjustment does not, therefore, affect statistical significance. See appendix B for further information on this "Bloom adjustment."

EXHIBIT 2.8
MTO MOBILITY OUTCOMES, PART 1

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Average 2000 Census Poverty Rate of Current Location (n=3670)					
All racial/ethnic groups	0.386	-0.080* (0.008)	-0.172* (0.017)	-0.062* (0.008)	-0.103* (0.014)
African Americans	0.401	-0.084* (0.010)	-0.178* (0.021)	-0.069* (0.011)	-0.108* (0.017)
Hispanics	0.365	-0.067* (0.013)	-0.141* (0.027)	-0.050* (0.013)	-0.089* (0.023)
All minorities	0.389	-0.079* (0.008)	-0.172* (0.017)	-0.063* (0.008)	-0.103* (0.014)
Lived in early HOPE VI development at baseline	0.379	-0.054* (0.013)	-0.127* (0.031)	-0.049* (0.013)	-0.076* (0.021)
Average 2000 Census Percent Minority of Current Location (n=3670)					
All racial/ethnic groups	0.876	-0.045* (0.008)	-0.096* (0.017)	-0.013 (0.009)	-0.022 (0.014)
African Americans	0.912	-0.048* (0.011)	-0.103 * (0.023)	-0.025 (0.013)	-0.039 (0.020)
Hispanics	0.845	-0.019 (0.022)	-0.040 (0.045)	0.009 (0.021)	0.017 (0.037)
All minorities	0.883	-0.036* (0.010)	-0.078* (0.021)	-0.013 (0.011)	-0.021 (0.017)
Lived in early HOPE VI development at baseline	0.875	-0.029 (0.015)	-0.068 (0.035)	-0.012 (0.016)	-0.018 (0.024)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Household survey, other locating data, 2000 Census tract-level data. See appendix A for details.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: Notes: a) ITT = Intent-to-Treat; TOT = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

MTO was created to test more rigorously the impacts attributed to the Gautreaux program in Chicago. As discussed in chapter 1, the Gautreaux Program was a racial desegregation initiative designed to give families in minority-concentrated public housing an opportunity to live in less segregated neighborhoods. When MTO was created it was theorized that requiring families to move to low-poverty neighborhoods would also result in desegregation (hence the full program name of MTO was the Moving to Opportunity for Fair Housing Demonstration Program). At the time there were many more low-poverty white neighborhoods than low-poverty minority neighborhoods in the five metropolitan areas. Exhibit 2.8 shows to what extent that expectation proved accurate. The table shows that the Section 8 group families, without the low-poverty requirement, have not moved to neighborhoods with lower concentrations of minorities. Experimental group families have moved to neighborhoods with lower minority concentrations, but the effect is small relative to the level of racial isolation characterizing the locations where the control group families live.

Exhibit 2.9 shows the results of testing for MTO's impacts on length of exposure to neighborhoods with different poverty levels. By separating the locations according to ranges of poverty rates (10-point intervals between zero and 40 percent), we can examine in what ranges each treatment had significant impacts and whether the effects of assignment to the experimental group were different from the effects of assignment to the Section 8 group.

The estimates in exhibit 2.9 show that assignment to the experimental group materially reduces the amount of time sample members spent in concentrated poverty areas and increases the time spent in areas with poverty rates of less than 20 percent. The TOT effect is an average reduction relative to the control group in the time spent in concentrated poverty neighborhoods of almost 3 years (32 months) or 47 percent of the time between random assignment and followup, with a corresponding increase in the time spent in areas with poverty rates of less than 20 percent. Assignment to the Section 8 group also produces material reductions in the time spent in concentrated-poverty neighborhoods compared to the control group. This reduction is smaller than the experimental reduction (2 years or 35 percent of the time since random assignment for those leasing up in the Section 8 group). In addition, assignment to the Section 8 group tends to shift people to somewhat poorer neighborhoods than assignment to the experimental group. It primarily increases time in neighborhoods with poverty rates of 10 to 30 percent, whereas the experimental treatment increases are concentrated in neighborhoods with poverty rates below 20 percent.

We saw in exhibit 2.7 that the places where control group members were living at the time of this evaluation appeared more favorable (relative to baseline locations) on a set of concentrated poverty indicators and opportunity indicators related to employment, income, family composition, education, and the like. Exhibit 2.10 is designed to illustrate the context of the MTO treatment—to show the differences mobility made in these types of indicators for the experimental and Section 8 group families relative to the control group. There are often substantial and statistically significant impacts on associated neighborhood characteristics for those leasing up in the experimental group, with particularly large impacts on the percent of their neighbors who are homeowners and the percent with incomes greater than twice the poverty line. While noticeably smaller, the impacts for those leasing up in the Section 8 group are still statistically significant. Having higher-income and more educated neighbors does appear to translate into having more friends with higher incomes and more education. Experimental group families making program moves showed significant increases in the proportion reporting having college-educated friends or friends who earn more than $30,000 a year. The estimated effects for Section 8 group families making program moves were smaller and not statistically significant. Neither group showed any effect on access to transportation, compared to the control group.

EXHIBIT 2.9
MTO MOBILITY OUTCOMES, PART 2

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Number of Months Since Random Assignment Living in Areas with Poverty Rates (n=4248):					
Below 10 percent	2.8	9.3* (0.6)	19.6* (1.3)	1.2* (0.5)	2.0* (0.8)
At least 10 percent but less than 20 percent	6.4	8.5* (0.7)	17.9* (1.5)	5.8* (0.8)	9.4* (1.3)
At least 20 percent but less than 30 percent	7.1	0.4 (0.6)	0.9 (1.2)	6.6* (0.8)	10.7* (1.3)
At least 30 percent but less than 40 percent	14.7	-2.8* (0.8)	-6.0* (1.7)	1.6 (1.0)	2.6 (1.6)
40 percent or above	41.0	-15.4* (1.0)	-32.4* (2.1)	-15.2* (1.1)	-24.6* (1.8)
Proportion of Months Since Random Assignment Living in Areas with Poverty Rates (n=4248):					
Below 10 percent	0.039	0.128* (0.011)	0.270* (0.023)	0.020* (0.009)	0.032* (0.014)
At least 10 percent but less than 20 percent	0.085	0.123* (0.010)	0.259* (0.021)	0.084* (0.011)	0.136* (0.017)
At least 20 percent but less than 30 percent	0.094	0.008 (0.008)	0.017 (0.017)	0.089* (0.011)	0.144* (0.017)
At least 30 percent but less than 40 percent	0.195	-0.038* (0.014)	-0.081* (0.029)	0.033* (0.015)	0.053* (0.025)
40 percent or above	0.583	-0.224* (0.014)	-0.472* (0.030)	-0.216* (0.015)	-0.351* (0.025)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Household survey, other locating data, 2000 Census tract-level data. See appendix A for details.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Poverty rates have been interpolated to reflect the part of the decade 1990 to 2000 when the sample lived in these locations.

EXHIBIT 2.10
CONTEXT OF THE MTO TREATMENT

Context	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Characteristics of the Current Neighborhood (2000 Census)					
Share of adults employed (n=3669)	0.810	0.035* (0.004)	0.075* (0.008)	0.032* (0.004)	0.052* (0.006)
Share of two-parent families (n=3670)	0.385	0.067* (0.007)	0.142* (0.014)	0.047* (0.007)	0.076* (0.012)
Share of owner-occupied housing units (n=3670)	0.230	0.095* (0.009)	0.201* (0.019)	0.062* (0.009)	0.101* (0.015)
Share of persons with incomes twice the poverty level (n=3670)	0.374	0.103* (0.008)	0.218* (0.016)	0.061* (0.008)	0.100* (0.013)
Share of persons with education beyond HS (n=3670)	0.307	0.060* (0.007)	0.128* (0.014)	0.039* (0.007)	0.064* (0.011)
Share of persons with college degree (n=3670)	0.151	0.038* (0.005)	0.080* (0.010)	0.020* (0.005)	0.032* (0.008)
Accessibility of Transportation [SR] (n=3515)					
Share of adults with working car or less than 15 minutes to public transport	0.948	0.005 (0.010)	0.011 (0.022)	0.008 (0.011)	0.014 (0.018)
Adult Friendship [SR]					
Share with 3+ close friends (n=3517)	0.349	0.016 (0.021)	0.034 (0.044)	0.008 (0.023)	0.014 (0.039)
Share with college-educated friends (n=3416)	0.410	0.066* (0.022)	0.140* (0.046)	0.044 (0.024)	0.073 (0.041)
Share with friends earning more than $30,000 (n=3036)	0.424	0.052* (0.023)	0.112* (0.048)	0.003 (0.026)	0.005 (0.042)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey, 2000 Census
Samples: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

2.6 Interpretation of Results

The results presented in this chapter show that MTO had substantial, favorable impacts on the geographic mobility of families in the experimental and Section 8 groups and on the characteristics of the neighborhoods they chose, relative to the areas chosen by the control group families. These impacts began with families' program moves. Almost half of the families assigned to the experimental group leased up with program vouchers, as did three-fifths of the families in the Section 8 group. Because many experimental group adults moved to neighborhoods where the poverty rate was increasing between 1990 and 2000, we estimate that only about half of their destinations had poverty rates below 10 percent at the time of the move, although virtually all had rates below 20 percent. Among the Section 8 group, less than 30 percent of those who moved with program vouchers moved to census tracts with poverty rates below 20 percent, even though the overwhelming majority moved to neighborhoods with lower poverty rates than the areas where they had lived in public housing. While 70 percent of the control group families had moved away from their baseline locations, half were still living in concentrated poverty areas.

Thus the MTO treatment led to significant differences in where families moved with the program vouchers. By the time of the interim evaluation these differences had narrowed somewhat because of subsequent moves and changes over the period in neighborhood poverty rates, but they had not disappeared. In 2002 the TOT reduction measured 17 points in the poverty rates of current residential locations for those who leased up in the experimental group. The TOT estimate was a 10- point reduction for those who leased up from the Section 8 group. Looking at the entire period after random assignment, there were very substantial reductions in the months and proportion of time that program movers spent living in concentrated-poverty neighborhoods relative to controls: reductions of 32 months (47 percent) for those leasing up in the experimental group and 24 months (35 percent) in the time program movers from the Section 8 group spent in high-poverty areas.

Even families who moved to low-poverty areas did not necessarily move to predominantly white or racially integrated areas. At the time of the interim evaluation, more than three quarters of the Section 8 group families—both those who moved with program vouchers and those who did not—were living in census tracts that were over 80 percent minority, about the same proportion as among control group families. Among experimental families, 60 percent of those who moved with program vouchers were also living in heavily minority areas (see exhibit 2.6). At the time of this study, families in the experimental group who moved with program vouchers lived in areas where the average percent minority was an estimated 10 percentage points lower; there was no significant effect on this measure for Section 8 families.

These mobility patterns of the experimental group families (as shaped by the MTO intervention) placed them in significantly better environments, compared to the living environments of the control group. There were some lesser improvements for Section 8 group families. At the time of the interim evaluation, experimental group families who moved with program vouchers lived in neighborhoods with higher proportions of employed adults, substantially higher proportions of two-parent families and high school graduates, and nearly twice the rate of homeownership as in the neighborhoods where the controls lived (and where they would have lived absent the demonstration). Living in these better neighborhood environments substantially increased the chances that adults in experimental group families would have college-educated friends or friends earning $30,000 or more.

Section 8 group families who moved with program vouchers also saw significant gains in the same neighborhood attributes relative to the control group, but those gains were generally only about half as large as those experienced by experimental group families. There was no significant effect on having college-educated or high-earnings friends for adults in Section 8 families.

In sum, the period from random assignment to the interim evaluation data collection saw substantial MTO impacts on mobility and on the locations chosen by experimental group members. Significant neighborhood differences remained at the time of the interim evaluation relative to the control group's locations, although they were smaller than the initial differences caused by the low-poverty leaseup constraint. Across the whole sample, this interval of 4 to 7 years saw other location changes as some control group members and nonprogram movers exited public housing and moved into the private market. Thus even with these significant impacts, the complex mobility patterns of the MTO sample make it difficult to predict what the program's impacts might be in other areas, such as housing, health, education, or employment. The next chapters address these topics.

Chapter Three
Impacts on Housing, Neighborhoods, and Safety

This chapter discusses the reasons why joining MTO would be expected to affect the housing and neighborhood conditions of the sample members assigned to the experimental or Section 8 groups. It describes the initial housing and neighborhood conditions and the safety issues that played a major role in the decision to join MTO. Then it presents the interim findings on housing and neighborhood impacts using respondent self-reports from the interim evaluation survey and administrative data from HUD.

Summary

We found substantial program effects on a wide variety of measures related to housing, neighborhood conditions, and safety. These effects were significant for the experimental group and the Section 8 group each taken as a whole (intent-to-treat effects). The estimated effects for program movers were often much larger for the experimental group than for the Section 8 group. Families assigned to the experimental and Section 8 groups were more likely to be receiving housing assistance 4 to 7 years after random assignment than members of the control group. On specific measures of housing quality (problems with vermin and with paint or wallpaper within apartments), neighborhood quality (litter and trash in the area, public drinking), and neighborhood safety (residents witnessing drug transactions, feelings of safety at night), the positive effects for experimental group program movers were particularly large relative to the control mean. On one measure—the percent of survey respondents reporting problems with the police not responding to calls in the area—the estimated effect for experimental group families that leased up reduced the control mean nearly to zero. Taken together, MTO's effects in this domain showed clear housing, neighborhood, and safety improvements relative to controls, and they were of great importance to participants whose primary motivation for joining MTO (in many cases) was improved safety.

3.1 Hypotheses about Housing, Neighborhood, and Safety in MTO

There are several hypotheses about MTO's potential impacts on housing and neighborhood. The hypotheses about housing impacts focus on housing assistance, housing status (tenure and security), and housing conditions. Those about neighborhood impacts focus on improvements in the physical conditions and safety of the local areas to which the experimental group families moved.

Hypothesized housing impacts

Housing status. Housing status outcomes consist of housing tenure, housing costs, and housing insecurity. Tenure refers to whether the sample member occupies housing as a renter, as an owner, is living doubled up with others, or is homeless. All MTO sample members were renters when they joined the demonstration.[47] Because tenure and income are closely related (lower income families are more likely to be renters) sample members would be expected to remain renters for some time after random assignment. But some may have become homeowners. And it is also possible that some sample members will be found to be living with friends or relatives due to the loss of their own housing. Some may even be living in a homeless shelter or on the street.

It is difficult to say whether these outcomes are expected for members of one assignment group more than another. It is possible that the experimental group members—because they received counseling for moves out of public housing and into low-poverty areas—would be better prepared to assume the responsibilities of private tenancy and so be less likely to lose their Section 8 housing. On the other hand, if rents in low-poverty areas rise more than in other areas, experimental group families in such areas might encounter increasing rent burdens and be at greater risk of losing their housing than the families in the Section 8 group that moved to other kinds of neighborhoods or the controls who at least initially were insulated from market pressures. Thus there were no clear ex-ante hypotheses about shorter run MTO outcomes on housing tenure, costs, or insecurity.

However, the qualitative data collected in the first phase of this study did suggest that program movers in both treatment groups might experience difficulty meeting utility costs and that rising rent and utility costs could make it difficult for families to continue to afford housing in the private market, particularly in better neighborhoods where housing costs were often higher and might increase faster.

In the longer run, it was hypothesized that members of the experimental group—taking advantage of the opportunities provided by low-poverty neighborhoods—would be more likely to move toward economic self-sufficiency than control group members. One aspect of self-sufficiency is homeownership. If living in low-poverty areas brings about increased employment and income, buying a home may become attainable for experimental group families especially with the variety of special programs to help low- and moderate-income families achieve homeownership. It is expected that some sample members will become homeowners by the time of the final impact evaluation. This may even be observed for a few at the point of the interim evaluation.

Housing assistance. Housing assistance refers to the receipt of rental subsidies: whether or not the sample member is getting help paying rent through one of the federal housing programs. All those who joined the MTO demonstration were already receiving housing assistance because they lived in subsidized housing—either in public housing or in private, assisted housing developments. These types of housing have supply-side (project-based) subsidies, which are reflected in low rents for the units.

The MTO demonstration offered those assigned to the experimental and Section 8 groups an opportunity to change the form of their housing subsidy from project-based to tenant-based so they could take the subsidy with them and use it wherever they chose to live. Leasing up in either group changed the form of assistance being received, reducing the proportion living in public or assisted housing.

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47 Some were lease holders, while others were living (with their children) as part of extended or multigenerational families.

But in the short run, and even in the range of 4 to 7 years after random assignment, it was not hypothesized that MTO participants would leave housing assistance altogether. There is little prior research on the subject of transitions off housing assistance either for public housing residents or for recipients of tenant-based rental subsidies. HUD can now measure the average length of tenure in the public housing and Section 8 programs, but there are no followup data on where people go when they leave or why they do so. We are not aware of any recent analyses that shed light on expectations for this demonstration about the duration of housing assistance or the timing of exits.

While the expected effect is unclear, the major factors with potential to change housing assistance status are worth mentioning. First, leasing up in the Section 8 or the experimental group requires families to rent in the private market. If families that lease up are later unable to renew their leases or cannot find other units to rent with their vouchers, they can lose assistance. And without the subsidy to help pay rent they may lose their housing and be forced to double up with friends or become homeless.

Second, having a housing voucher gives experimental and Section 8 group families that leased up much more choice about their housing and neighborhoods. This may make families whose earnings improve over time more likely to continue using the voucher than to stay in public housing, especially distressed public housing in concentrated-poverty areas. Third, if the changes in neighborhood engendered by the program moves of experimental group families lead to substantial increases in income, some families may have their assistance payments reduced to minimal levels or become ineligible for continued assistance. Finally, the counseling associated with the experimental treatment—and the possibility of different rent increases in different neighborhoods—could lead to differences between experimental group and Section 8 group sample members in their ability to remain in the program.

Housing conditions. Because the families that moved as a result of joining MTO were using the vouchers MTO provided, the rental housing they leased had to meet Section 8's Housing Quality Standards (HQS), a set of requirements about the safety and habitability of the dwellings. These standards are enforced through inspections by the local housing authorities, which can require that a landlord make repairs or improvements to meet the standards before a voucher holder can occupy the unit.[48] Although most housing authorities also inspect the public housing units in their stock, MTO participants were recruited from some of the worst big-city public housing sites in the country (as chapter 2 noted). Therefore, the housing conditions of the control group were unlikely to measure up to the Section 8 housing quality standards.

While HQS establishes the minimum unit quality, these standards are not the only reason why improvements in housing conditions were hypothesized for the experimental group. Low-poverty areas tend to offer newer housing with more amenities, which could exceed both the program standards and the quality of rental housing available in other areas. Movers to low-poverty areas would be expected to rent housing units in better condition, with fewer maintenance problems, than

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48 Of course, the landlord must be willing to do so. In most jurisdictions owners of rental property may choose whether or not to accept voucher holders as tenants. Some landlords do not want to deal with the paperwork and inspection requirements of the program, although the standard lease has been changed in recent years to make the voucher program more landlord-friendly.

the rental housing available in poorer neighborhoods. Experimental group program movers to such areas could be expected to obtain better housing than the control group—and perhaps better than the Section 8 group's housing—as long as such housing was available within the Section 8 rent limits (called Fair Market Rents). On the other hand, experimental group program movers could be faced with tradeoffs between housing quality and neighborhood quality. If rents were generally higher or increased more rapidly in low-poverty areas, they might have to accept a unit of lesser quality at an affordable rent in order to remain in a better neighborhood.

Hypothesized neighborhood and safety impacts

Changing experimental group families' residential neighborhoods from high-poverty to low-poverty is the primary MTO intervention. Chapter 2 showed that assignment to the experimental group in MTO did, in fact, affect the locations where sample members were living at the time of the interim evaluation. Experimental group sample members were living in areas with poverty rates significantly lower than the places control group members were living. Further, the reductions in neighborhood poverty for experimental group program movers (and the improvements in other neighborhood indicators) were substantially and significantly greater than those for the leased-up families in the Section 8 group.

But moves to low-poverty areas would be expected to change many other aspects of the neighborhood environment, not just the proportion of the residents living in poverty. Movers to low-poverty areas would be expected to find better maintained and safer neighborhoods. The adverse environment of concentrated poverty areas—the physical decay, social disorder, and danger characteristics of these areas—would be left behind.

Most of all, moves to low-poverty areas were hypothesized to improve safety. It was expected that those living in low-poverty areas would not need to be alert for gunfire, would not need to be concerned about the hazards posed by abandoned buildings or empty lots, would not need to worry about getting children safely home from school, and would not need to protect their children by keeping them from playing outdoors. Indeed, these kinds of changes in neighborhood (and their expected effects on the physical and mental well-being of the sample members) were hypothesized to be the source of immediate improvements in the lives of the experimental group families. The qualitative data collected early in this evaluation supported these hypotheses about neighborhood impacts.

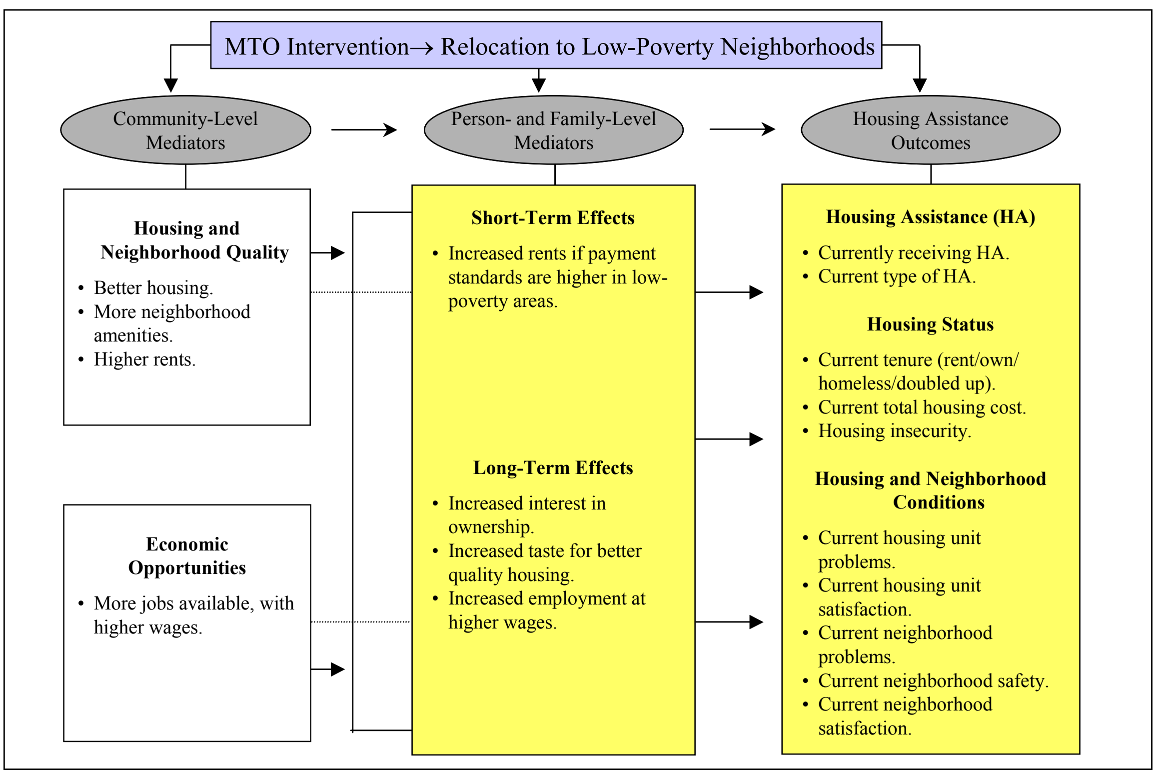
The main outcomes of interest in the areas of housing and neighborhoods are shown in exhibit 3.1, in the right-most box. The outcomes are grouped into three sets—housing assistance, housing status, and housing and neighborhood conditions. The MTO intervention, which brought about the program moves by experimental group families into low-poverty neighborhoods, is shown at the top. Below and to the left, the diagram suggests that the effects of low-poverty moves may be mediated by various community level and person- or family-level factors.

As chapter 1 explained, this study's impact analysis was structured not only to assess MTO's impacts across the six domains but also to allow some investigation of causal mechanisms. The general model (see exhibit 1.1) showed the wide range of mediating factors that were hypothesized to play a role in shaping the outcomes for families moving to low-poverty areas. These included community-level mediators (such as economic opportunities and schools) as well as person- and family-level factors (such as adult social networks and parental attitudes).

Exhibit 3.1 shows the mediators expected to affect housing and neighborhood outcomes. The key mediators for the housing and neighborhood impacts explored in this chapter are community-level factors, which would be expected to differ due to the moves made by the experimental and Section 8 groups with vouchers received through MTO. The sample's moves were analyzed in chapter 2 and shown there to have significantly affected where sample members had lived after random assignment and where they were living at the time of the interim evaluation. Thus it is already clear that neighborhood conditions and economic opportunities (as measured by the characteristics shown in exhibit 2.10) did change for the families as a result of their moves to different neighborhoods due to MTO.

The other mediators that could play a role in MTO impacts on housing and neighborhood conditions are person- and family-level factors such as the employment and earnings of participants and tastes for better quality housing or homeownership. Effects on various indicators of the sample's well-being will be tested in later chapters.[49]

EXHIBIT 3.1
HOUSING AND NEIGHBORHOOD OUTCOMES AND MEDIATING FACTORS



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49 See chapter 7 (on employment and earnings) and chapter 8 (on household income and receipt of public assistance).

3.2 Data Sources and Measures

Data sources

For this study there are two main sources of data on housing and neighborhood conditions and safety. For adult sample members who completed the interim evaluation survey there are a variety of survey responses about current conditions including some items directly comparable to baseline measures.

There are questions about the respondent's sense of safety and any recent experience with crime victimization. There are also observational data on the housing units and neighborhoods reported by the field interviewers who visited the sample members during 2002.

In addition, administrative data from HUD systems have been matched to identifiers of the sample to determine whether and when sample members were receiving HUD housing assistance and collect information on assistance type and amounts. We used data from the Multifamily Tenant Characteristics System (MTCS), the HUD system that collects information from the public housing authorities (PHAs) about public housing residents and about participants in the tenant-based voucher program. We also got data from the Tenant Rental Assistance Certification System (TRACS), a similar system for households assisted in project-based Section 8 developments and other assisted projects.

The data from the two HUD systems span the period 1995 through 2001. Over this period, there was a substantial increase in reporting to MTCS, with most housing authorities submitting data by 2001. However, it still cannot be assumed that these administrative data are complete.

Measures

Three sets of outcome measures were used in the analysis of housing, neighborhood, and safety impacts. The housing assistance measures addressed whether the sample member was still receiving a housing subsidy and, if so, in what form—whether living in public housing, living in a private assisted development,[50] or receiving tenant-based assistance through the voucher program.

For housing status, we tested MTO's effects on current housing tenure, monthly housing costs paid by sample members, and housing cost burden. In addition, we explored whether experimental group or Section 8 group members were experiencing more housing insecurity than those in the control group. By insecurity we mean various difficulties sample members could be having in regard to their housing. Such difficulties could be financial (problems paying rent or utilities) or nonfinancial (being homeless or doubled up in someone else's housing unit, encountering discrimination in housing search, or having landlord problems). We built two indexes (as shown in exhibit 3.2) reflecting the severity of problems with ability to pay for housing and then tested them for program impacts.

[50] This is another form of project-based assistance, different from public housing.

Third, we analyzed impact measures concerning housing and neighborhood conditions and safety. We tested for differences in the incidence of housing problems reported by the survey respondents. We examined respondent reports on several types of neighborhood problems as well as their answers to questions on the safety of their current locations and any recent experience of victimization. Taken together, this set of housing and neighborhood measures gave a broad picture of the sample members' current living situations.

EXHIBIT 3.2
HOUSING INSECURITY MEASURES

Measure	Values	Components	Weights
Index of rent or mortgage problems	Minimum=0 Maximum=2	Respondent reports being 15 or more days late in paying rent or mortgage at least once in the past 12 months.	1
		Respondent reports receiving an eviction or foreclosure threat due to nonpayment.	1
Index of utility payment problems	Minimum=0 Maximum=2	Respondent reports being 15 or more days late in paying utilities at least once in the past 12 months and/or being charged a late fee and/or receiving a shutoff.	1
		Respondent reports either having services shut off or moving out (even for a little while) because utilities were shut off.	1
Index of combined problems in ability to pay for housing	Minimum=0 Maximum=4	Index of rent or mortgage problems	2
		Index of utility payment problems	2
Homeless, doubled up, or evicted	Indicator (0,1)	Respondent reports being homeless, doubled up, or evicted during last 12 months.	Any/all of these conditions=1
Faced discrimination or bias in housing search	Indicator (0,1)	Respondent reports being turned down for housing based on race, ethnicity, gender, family status, disability, source of income, or bias against public housing residents.	Any/all of these conditions=1
Had problems with recent landlord	Indicator (0,1)	Respondent reports leaving housing unit because of issues with landlord.	Any/all of these conditions=1

3.3 Baseline Housing and Neighborhood Status of MTO Participants and Control Group Context

Baseline conditions for the MTO sample

The MTO demonstration recruited families from each participating city's poorest neighborhoods. By targeting program eligibility to census tracts with poverty rates of 40 percent or more (as required by program rules), the local housing authorities also focused MTO recruitment on some of their worst public housing developments—worst in physical condition, worst in the incidence of crime and violence, and worst as places to live. These developments were located in very troubled neighborhoods with the multiple problems typical of concentrated poverty areas.

As chapter 2 noted, during the same time period as MTO major efforts were made to improve or demolish the most distressed public housing developments around the country. The HOPE VI program (along with vacancy consolidation and comprehensive modernization) affected developments from which MTO families were recruited in four of the five MTO sites (all sites but New York). About one-fifth of the interim evaluation sample (21 percent) lived at baseline in public housing developments affected by early HOPE VI and related programs.[51] The proportion of MTO sample members in developments affected by these initiatives varied from 16 percent of the Los Angeles families in this study to 44 percent of the Baltimore families in this study.

That one-fifth of the interim evaluation sample came from the earliest public housing sites to receive major funding for distressed conditions indicates how bad the living conditions at baseline were for many sample members. The prospect of disruption and demolition, when residents learned about the plans for their developments, may have been a factor encouraging residents to join MTO and it may have affected the effort those in the experimental and Section 8 groups made to lease up with their MTO vouchers. In addition, HOPE VI and vacancy consolidation provided some residents in affected developments with an opportunity to obtain Section 8 vouchers. In Baltimore this occurred almost simultaneously with the start of MTO, while in Boston, Chicago, and Los Angeles it occurred 1 to 2 years afterward (but when MTO was still recruiting families). We estimate that about 14 percent of the control group obtained vouchers outside of MTO. In addition, about seven percent of the experimental group and two percent of the Section 8 group obtained vouchers outside MTO.[52]

Baseline neighborhood conditions. In the context of the mobility analysis, chapter 2 described the neighborhoods where the sample members were living at baseline. These areas of concentrated poverty were marked by low labor force participation, high proportions of single-parent families, low educational attainment, and high rates of welfare receipt (see exhibit 2.7). For the families who joined MTO, the dangers of their housing developments were a primary motivation for trying to move (see exhibit C1.3). More than 80 percent put getting away from drugs and gangs as key reasons for wanting to move. Fewer than 10 percent wanted to move within the same neighborhood.

When asked about their neighborhoods in the baseline survey, a high proportion of the respondents reported big problems on several indicators: presence of abandoned buildings (38 percent), presence of litter or trash in the streets (53 percent), presence of graffiti (63 percent), and presence of drug dealers (87 percent). Only one-third felt safe or very safe on the street during the day and just 12 percent felt safe or very safe on the street at night. Most striking, only four percent felt very safe when home alone at night.

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[51] By early HOPE VI sites we mean sites designated for HOPE VI implementation during MTO operations, which ended intake in mid-1998. HOPE VI implementation grants have continued to be made by HUD through Federal fiscal year 2002.

[52] These were nonprogram movers from the experimental and Section 8 groups.

Baseline housing situation. A factor common to all the families joining the MTO demonstration was the receipt of housing assistance—either as residents of public housing or as residents of project-based assisted housing. About 90 percent of the interim evaluation sample lived in public housing at baseline; the remaining 10 percent (some 444 of these 4,248 families) came from project-based, assisted developments in the private market.[53]

Of the large share from public housing, nearly all came from distressed public housing, much of which would meet the formal definition of substandard housing (Fitzpatrick and LaGory 2000). Residents of distressed public housing were exposed to a range of hazards, including lead paint, asbestos, cockroach and rodent infestations, exposed electrical wiring and pipes, broken plumbing, unscreened windows, unlit halls and stairwells, and broken elevators (Scharfstein and Sandel 1998; National Commission on Severely Distressed and Troubled Public Housing 1992). Conditions in high-rise developments were particularly bad; in some developments, it was common for young children to play in front of unprotected windows or for asthmatic mothers and children to have to climb many flights of stairs every day.

The families enrolling in MTO also expressed marked dissatisfaction with their housing (see exhibit C1.3). Only one-fourth of the study sample rated the condition of their current units at the time as excellent or good and the same proportion rated their current housing as poor. More than one-third of the baseline respondents reported big problems with rats or mice in their apartments and with peeling paint or wallpaper. About 20 percent of the sample members reported big problems with plumbing or heat that did not work, broken or missing locks, and broken windows or windows without screens. Nearly half the sample identified getting a bigger or better apartment as the first or second most important reason for wanting to move (the next most frequent response after getting away from drugs and gangs).

At baseline it was quite common for MTO applicants to come from multigenerational families living together in public housing. For the younger generations of these families—the daughters raising their own children while living with their mothers (and sometimes their sisters and sisters' children, too)—MTO offered the chance to obtain their own apartments and their own housing subsidies.

Because all the MTO families were already living in subsidized housing at baseline, they were paying rent in proportion to their incomes. Families recruited from public housing were paying rents limited to 30 percent of adjusted income. The federal housing subsidy was provided through funds paid to the housing authorities to make up the difference between the family's contribution and the cost of maintaining and operating the housing.[54] Families living in private, assisted developments at baseline

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[53] 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.) Here and elsewhere they are called private, assisted housing.

[54] Although PHAs have some other income sources (e.g., from commercial space in their properties) and also receive funds for modernization, the Federal operating subsidy is by far the largest supplement to rental income.

also paid reduced rents, with rent levels calculated in a manner similar to that for public housing residents. The private owners had received subsidies to build or rehabilitate the housing in exchange for renting to low- and moderate-income tenants for a certain period of time.

Current housing situation and neighborhood conditions for the control group

In the 4 to 7 years after random assignment, there were changes in the housing and neighborhood conditions of the control group members. By the time of the interim evaluation there had been substantial mobility (as chapter 2 described). Approximately two-thirds of the control group adults were living in a different location than their baseline address. And some of those in the same locations had changed apartments as a result of public housing renovation or demolition.

Neighborhood conditions. Although a substantial proportion had moved from their baseline locations, many adults in the control group still reported dissatisfaction with their current neighborhoods (see exhibit C3.1 in appendix C). While 75 percent said they felt safe during the day only 55 percent reported feeling safe at night. Two-thirds of the control group respondents indicated there were problems with trash or litter in their neighborhoods and over half said there were problems with public drinking. Two-thirds reported a problem with people hanging out in the area, and about one-third said police did not come when called. Twenty-one percent reported that they or someone else in their household had been victimized in the past 6 months.[55] Overall, one-third said they were very or somewhat dissatisfied with the area where they currently lived. Still, this proportion was considerably reduced from baseline (see for comparison exhibit C1.3).

The field staff collecting the survey data for this study usually interviewed sample members in their homes. While there the interviewers recorded observations on the condition of the building exteriors and the surrounding block.[56] Twenty-nine percent of the control group families' buildings had metal bars on windows above the basement level. The majority of the buildings in which the control group members were living were observed to be in fair condition (52 percent), but 16 percent were rated as in poor condition or badly deteriorated. Other residential structures on the same block received the same mix of ratings. One-third of the streets had major or minor accumulations of trash.

The sociodemographics of the neighborhoods where control group members were living at the time of this study were described in chapter 2, in terms of 2000 Census indicators (see exhibit 2.7). The neighborhoods were quite poor—39 percent of the population lived in poverty on average—but less than one-fifth of the families living there were receiving public assistance, and three-fourths of the families had at least one member in the labor force. The mobility of the control group members in the period after random assignment, combined with other, large-scale changes (HOPE VI and public

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[55] Specific questions were asked about: having a purse, wallet, or jewelry stolen; being threatened with a knife or gun; being beaten or assaulted; being shot or stabbed; or having an attempted or actual house-breaking. Responses on each item are shown in exhibit C3.1. Their joint occurrence for the control group was 21 percent.

[56] The field staff who conducted interviews with sample members were trained to record observations of the housing and neighborhood where the respondents lived. However, these were not full housing or neighborhood inspections. See appendix A for details.

housing modernization, welfare system changes, and the flourishing economy of the 1990s) had apparently put them in areas with more favorable characteristics than their baseline neighborhoods.

Current housing situation of control group members. At the time of this study's data collection, most of the adults in the control group were still living in rental housing (89 percent), either unsubsidized or subsidized (including public housing). About four percent were doubled up (living with family or friends, some paying rent and others not). There were a few adults who were living in shelters or group quarters, and a few were incarcerated. About five percent had become homeowners (see exhibit C3.2).

Current housing assistance receipt in the control group. According to both survey responses and HUD administrative data, about one-third of the control group adults were not receiving housing assistance at the time of the interim evaluation data collection (see exhibit C3.3).[57] Of those with assistance, administrative data indicate that the largest share was living in public housing (44 percent of the whole group). Another 10 percent were living in private, assisted housing. And 12 percent of the control group had vouchers, according to the administrative data.[58] These tenant-based vouchers did not come from MTO, of course. Some control group members may have received vouchers by being on the regular Section 8 waiting list, while others probably received vouchers through the relocation efforts of the local housing authorities when HOPE VI or vacancy consolidation efforts began in the origin public housing developments.

Housing quality and condition. At the time of the interim evaluation, according to field observations, 36 percent of the control group adults were living in detached or attached single-family housing, about 30 percent in lowrise multifamily housing, and about 24 percent in highrise buildings (see exhibit C3.4). Five percent of the control group's housing was rated by the observers as badly deteriorated and another 11 percent was considered to be in poor condition. About 14 percent of the control group families' dwellings were observed to have open cracks or holes in the walls.

The adults in the control group expressed general dissatisfaction with the condition of their housing. More than 50 percent reported problems with rats, mice, or roaches in their units, the same proportion reported problems with peeling paint or wallpaper, and 40 percent indicated there were problems with

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[57] Some control group adults were homeowners, homeless, or doubled up when the data were collected for this study. Others were simply unassisted renters no longer in subsidized housing. Although this might seem surprising, it has been noted elsewhere that a substantial number of public housing residents have left or been "lost" to housing assistance during the process of HOPE VI implementation in housing authorities across the country. Kingsley, Johnson, and Pettit give an estimate of 20 percent for those who moved to other types of HUD assisted projects or no longer receive HUD assistance (2001, p. 3). Others suggest that the "lost" alone may be as many as 20 percent of the original residents (National Housing Law Project 2002).

[58] These figures are from administrative data. The corresponding survey results for the whole group are: public housing 45 percent; private, assisted housing 2 percent; vouchers 21 percent; unknown assistance type 2 percent. It appears that the wording of a survey question (about project-based Section 8 housing) led to respondents or interviewers confusing vouchers and project-based assistance. The number with vouchers was overreported as a result, and the number in private, assisted housing was underreported. We think the administrative data are more accurate on this subsidy program item.

heating or plumbing.[59] Nearly half of the control group respondents reported their housing to only be in fair or poor condition. Although these self-reports indicate a higher level of problems than the field observations (and a greater incidence of problems than at baseline), the sample members were certainly more familiar with the condition of their entire housing units and with the functioning of their basic systems.

Overall, then, at the time of the interim evaluation the control group adults viewed their housing and neighborhoods with considerable dissatisfaction, noting a variety of problems that signified neglect and that in some cases made them feel unsafe. Whether the housing situations or neighborhood conditions were significantly better for the experimental group or the Section 8 group is the question addressed in the next section.

3.4 Impacts on Housing, Neighborhoods, and Safety

This section presents the results of testing for experimental effects on the key housing, neighborhood, and safety outcomes identified above. (Section 2.5 provided a boxed explanation of how to interpret the impact estimate tables.)

Current housing status

All of the sample members from the experimental and Section 8 groups who made program moves were shifting from project-based to tenant-based housing assistance. The simplicity of paying 30 percent of income for rent (adjusted for utilities) was replaced by the complexity of rent payments that not only took income into account but also reflected the landlord's asking price and the local housing authority's payment standard[60] and utility allowances.[61]

Exhibit 3.3 shows MTO's effects on various housing status outcomes. Sample members in the control group were paying an average of $412 per month in total housing costs (rent or mortgage payment plus utilities). The amount was no different for the experimental and Section 8 groups, nor did a sample member's employment or welfare receipt at baseline make a significant difference in program effects on total housing cost.

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[59] Exhibit C3.3 shows the responses to each item separately, while responses on combinations of items are discussed here.

[60] Housing vouchers are designed to offer a shopping incentive to apartment seekers by letting them keep the difference if they find a unit with rent below the payment standard. Voucher holders are also allowed to lease units with rents above the payment standard, but they pay the difference. These features must be understood enough to be part of the prospective renter's calculus for determining what is affordable with the voucher. And they are considerably more complex than the way the rent and utility calculations for public housing are done.

[61] Utility allowances are the amounts the participant is assumed to incur for the utilities paid out of pocket, based on the size of the unit. They are set by the agency administering the voucher. The subsidy payment is calculated to cover the difference between 30 percent of income and the sum of contract rent plus utility allowance.

EXHIBIT 3.3
KEY MTO HOUSING STATUS OUTCOMES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Current Total Monthly Housing Cost[1] (n=3386)					
All	$412	$25 (20)	$53 (42)	$6 (18)	$10 (31)
Employed at baseline	$562	-$47 (52)	-$98 (109)	-$60 (51)	-$97 (81)
Receiving cash assistance at baseline	$372	$22 (19)	$45 (39)	$7 (16)	$11 (26)
Current Total Housing Cost Burden (n=3113)					
All (mean burden)	0.312	0.016 (0.010)	0.035 (0.021)	0.011 (0.009)	.018 (.015)
Employed at baseline	0.307	0.016 (0.021)	0.034 (0.044)	0.004 (0.020)	.006 (.032)
Receiving cash assistance at baseline	0.319	0.003 (0.011)	0.006 (0.023)	0.004 (0.011)	.007 (.017)
Housing Insecurity Indexes (Ability To Pay during Last 12 Months)					
Rent or mortgage problems[2] (n=3502)	0.293	-0.042 (0.026)	-0.090 (0.055)	-0.046 (0.029)	-0.077 (0.048)
Utility payment problems[3] (n=3509)	0.270	0.105* (0.024)	0.223* (0.052)	0.072* (0.025)	0.120* (0.041)
Combined problems with ability to pay for housing[4] (n=3497)	0.565	0.061 (0.038)	0.129 (0.080)	0.026 (0.041)	0.043 (0.069)
Other Housing Insecurity Indicators during Last 12 Months					
Homeless, doubled up, or evicted (n=3521)	0.080	0.014 (0.013)	0.030 (0.028)	0.019 (0.016)	0.032 (0.026)
Reported facing discrimination or bias in housing search (n=3520)	0.030	0.037* (0.009)	0.080* (0.019)	0.064* (0.012)	0.107* (0.020)
Reported problems with recent landlord (n=3526)	0.036	0.006 (0.008)	0.012 (0.016)	0.013 (0.009)	0.022 (0.014)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey data. See appendix A for details.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Due to the statistical software used to generate the impact estimates the numbers of cases used are slightly smaller than the numbers in the corresponding appendix C tables. This accounts for the small differences in control means.
[1] Excludes cases with missing rent or mortgage payment information.
[2] Items included are: Respondent reports being 15 or more days late in paying rent or mortgage at least once in the past 12 months, Respondent reports receiving an eviction or foreclosure threat due to nonpayment. Maximum value of index is 2.
[3] Items included are: Respondent reports being 15 or more days late in paying utilities at least once in the past 12 months and/or being charged a late fee and/or receiving a shutoff, Respondent reports either having services shut off or moving out (even for a little while) because utilities were shut off. Maximum value of index is 2.
[4] Items included are utility problems, rent or mortgage problems. Maximum value of index is 4.

The second panel of exhibit 3.3 shows the results of testing for MTO's impacts on housing cost burden (the ratio of total housing costs to income). Burden for the treatment groups did not differ significantly from burden for the control group, nor were there significant subgroup differences on this measure.

The third panel of exhibit 3.3 reports the results of testing for differences in three housing insecurity indexes (as defined in exhibit 3.2). The control group mean for the index of rent or mortgage payment problems in the past 12 months was .29, and this value did not differ significantly for the experimental or Section 8 groups. For the index of utility payment problems, the control mean was .27, but it was .34 for the Section 8 group as a whole and .38 for the experimental group as a whole. Both were significantly higher than the control group. Among those who leased up in the experimental group, the estimated impact on the incidence of utility payment problems was 22 points (nearly double the control value), while for the Section 8 group the estimated TOT effect was 12 points.

These data are consistent with the qualitative report findings (Popkin, Harris, and Cunningham 2001) of difficulty adapting to paying utilities separate from rent.[62] Rising rent and utility costs made it difficult for families to continue to afford housing in the private market, according to the qualitative interviews. Heating bills can be particularly high especially for those who moved to single-family homes, duplexes, or townhouses. In the qualitative interviews respondents described problems with regard to utility payments. A Section 8 comparison group mover from New York described how she and her family conserved electricity during the day and heat during the night, in an effort to lower her bills.

> *When I took the papers and everything for the apartment, they was like, you have to pay for your heat. How are you going to do this? And my attitude was, the Lord will provide. They didn't want to hear that. They wanted to know how you're going to pay these bills. I'm like, the Lord will provide. But then, like I said, I just had to come to terms with it being either/or. You can't have both. During the day, I really don't use no lights because of the daylight, but during the night, I may like turn them on, like if they're taking a shower or what have you, just to warm it up enough to get in and out of the shower, and turn it off and all go to bed.*

Conserving electricity was not enough to make the bills affordable. Several respondents described how they juggled bills to make ends meet. Deborah went on to say:

> *It was like taking from Peter to pay Paul. You know, it was like – eenie, meenie, miney, moe. I'll pay ConEd this week, rent this one. I had to work it out like that.*

Returning to the impact estimates, the final panel of exhibit 3.3 reports survey responses indicating nonfinancial aspects of housing insecurity over the past 12 months. Some eight percent of the sample had been homeless, had lived doubled up with another family, or had been evicted during the last year

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62 In older public housing many or all utilities are provided by the housing authority and are not paid separately from the monthly rent amount.

(evictions for reasons other than non-payment of rent). This did not differ by assigned group nor did the incidence of reported problems with landlords.

Adult survey respondents were asked about their experiences since random assignment searching for a house or apartment to rent. Three percent of the control group reported encountering bias or discrimination in housing search. But the incidence of such reports was significantly higher for the experimental group and higher still for the Section 8 group. Experimental group program movers reportedly experienced about double the incidence of bias or discrimination as control group members. For the Section 8 program movers, the estimated effect was even greater, more than three times the control mean.[63]

We turn now to the second group of housing outcome measures, which concern receipt of housing subsidy. Data from two different sources (the interim survey and HUD administrative records) were used to test for MTO's impact on the receipt of housing assistance. The first panel of exhibit 3.4 shows that, based on self-reports in the survey, the rates of housing subsidy receipt by experimental and Section 8 group members were significantly higher than the rate for the control group (71 percent). For the treatment groups as a whole, the assistance rates were 7.7 and 7.9 percentage points higher, respectively. The estimates for program movers in these groups show the impacts to be 16 and 13 percentage points higher, respectively. Those employed at baseline had a lower mean rate of assistance but similar effects for both treatment groups. Sample members receiving cash assistance at baseline had a slightly higher control mean rate of assistance, but the pattern of significant impacts for both groups was the same.

The second panel of exhibit 3.4 shows the impact estimates on housing subsidy receipt measured using administrative data.[64] The control mean from these data is somewhat lower than for the self-reported measure, at 66 percent. There is a significant effect only for the Section 8 group. Rates of housing assistance receipt were somewhat lower for those employed at baseline and somewhat higher for recipients of welfare at baseline, again only for the Section 8 group.

As exhibit 3.4 shows, very few movers in either experimental group of the Section 8 group had returned to public housing at the time of the interim evaluation.[65] One panel shows results using survey responses on this topic; the other shows results with administrative data from HUD. In both sets of data about 44 percent of the control group families were public housing residents at the time of

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63 Survey respondents were asked about the most recent time since random assignment that they had searched for housing and been told a unit was not available to them. We examined the pattern of response by group and by type of housing assistance. In all groups tenant-based voucher holders were the most likely to report encountering bias or discrimination while public housing residents were the least likely. However, we do not have the necessary data to adjust these figures for differences in search activity.

64 The rate of agreement between the survey and administrative data on receipt of housing assistance for the adult sample was 78 percent.

65 This outcome measure is not a simple reflection of the fact that leasing up in the experimental or Section 8 groups meant shifting to tenant-based assistance. Additional variation was introduced by the high level of control group mobility and the sample members' access to vouchers outside of MTO in conjunction with HOPE VI and other public housing development changes.

EXHIBIT 3.4
KEY MTO HOUSING ASSISTANCE OUTCOMES

Outcome	Control Mean	Experimental Vs. Control ITT[a]	Experimental Vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Living in Household Currently Receiving Housing Assistance [Survey] (n=3525)					
All	0.706	0.077*	0.164*	0.079*	0.133*
		(0.019)	(0.041)	(0.021)	(0.035)
Employed at baseline	0.631	0.111*	0.227*	0.117*	0.187*
		(0.042)	(0.086)	(0.047)	(0.074)
Receiving cash assistance at baseline	0.716	0.083*	0.171*	0.089*	0.142*
		(0.022)	(0.045)	(0.024)	(0.038)
Living in Household Currently Receiving Housing Assistance [Admin] (n=4248)					
All	0.657	0.032	0.067	0.100*	0.162*
		(0.018)	(0.037)	(0.019)	(0.030)
Employed at baseline	0.615	0.059	0.124	0.111*	0.175*
		(0.035)	(0.074)	(0.038)	(0.060)
Receiving cash assistance at baseline	0.678	0.039	0.078	0.098*	0.153*
		(0.020)	(0.040)	(0.021)	(0.033)
Currently Living in Public Housing [Survey] (n=3525)					
All	0.454	-0.197*	-0.419*	-0.232*	-0.388*
		(0.020)	(0.044)	(0.021)	(0.035)
Employed at baseline	0.378	-0.166*	-0.338*	-0.184*	-0.293*
		(0.039)	(0.080)	(0.040)	(0.064)
Receiving cash assistance at baseline	0.468	-0.216*	-0.445*	-0.251*	-0.403*
		(0.023)	(0.048)	(0.024)	(0.039)
Currently Living in Public Housing [Admin] (n=4248)					
All	0.435	-0.184*	-0.388*	-0.226*	-0.366*
		(0.018)	(0.037)	(0.018)	(0.030)
Employed at baseline	0.397	-0.161*	-0.338*	-0.234*	-0.367*
		(0.034)	(0.072)	(0.035)	(0.055)
Receiving cash assistance at baseline	0.445	-0.200*	-0.402*	-0.241*	-0.375*
		(0.020)	(0.041)	(0.021)	(0.033)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey, HUD administrative data. See appendix A for details.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Due to the statistical software used to generate the impact estimates the numbers of cases used are slightly smaller than the numbers in the corresponding appendix C tables. This accounts for the small differences in control means.

this study, and there were strong and significant reductions to this proportion for those assigned to a group receiving vouchers through MTO. According to the administrative data, members of the experimental group were 18 percentage points less likely to live in public housing, and those in the Section 8 group were 23 percentage points less likely. Estimated effects on experimental group families that leased up suggest a rate of residence in public housing below 5 percent. For Section 8 program movers, the estimated effect reduced public housing residency to under 8 percent. It is clear that few of those who were enabled to move through the MTO demonstration returned to public housing during the 4 to 7 years since moving.

Housing and neighborhood quality and satisfaction

The third set of housing outcomes addresses the areas of housing and neighborhood quality and satisfaction. The impact estimates for these measures, which use self-reports from the adult survey data, are shown in exhibit 3.5. There were significant, positive effects for both the experimental and Section 8 groups on all of the neighborhood safety measures, on most of the housing quality measures, and on every outcome related to neighborhood conditions and quality, but the effects were often substantially greater for the experimental group. For example, about half the control group adults reported problems with peeling paint or plaster, but these kinds of problems only affected about 21 percent of those who leased up in the experimental group and about 38 percent of the Section 8 program movers. The experimental group families had significant reductions (relative to controls) in the incidence of problems with rats, mice, and roaches in their dwellings; Section 8 families did not differ from control group families on this measure. Consistent with these findings, while 52 percent of the control group adults rated their current housing as excellent or good, this proportion was 7 to 10 points higher for those assigned to the treatment groups, 12 points higher for Section 8 group program movers, and 21 points higher for experimental group program movers.

A similar pattern is shown for the neighborhood safety and condition measures in exhibit 3.5. Some 21 percent of control group adults reported that they or a family member had been a crime victim in the past 6 months. Significantly smaller proportions of the experimental and Section 8 groups reported crime victimization (4 percentage points less for all the experimental group adults, 5 percentage points less for all the Section 8 group adults). Three-fourths of the control group adults reported feeling safe in their current neighborhoods during the day. At night, however, only about half of them (55 percent) reported feeling safe where they were living at the time of this study. The estimated effect of a Section 8 group program move was to raise this by 16 points; the estimated effect of an experimental group program move was to raise it by 30 points. While nearly half the adults in the control group (45 percent) reported seeing drug transactions in the neighborhood in the past month, this was true for one-third of the experimental and Section 8 group members. For those in the experimental group who leased up, the estimated effect brought this down to only 20 percent seeing such transactions.

EXHIBIT 3.5
MTO HOUSING AND NEIGHBORHOOD CONDITIONS AND SAFETY OUTCOMES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Measures of Current Housing Quality					
Share reporting problem with vermin (n=3524)	0.528	-0.047* (0.022)	-0.100* (0.046)	-0.007 (0.024)	-0.012 (0.039)
Share reporting problem with heating/ plumbing (n=3514)	0.393	-0.038 (0.021)	-0.082 (0.046)	-0.006 (0.023)	-0.010 (0.039)
Share reporting problem with peeling paint/plaster (n=3525)	0.492	-0.087* (0.022)	-0.186* (0.047)	-0.064* (0.024)	-0.108* (0.040)
Share rating current housing as excellent or good (n=3525)	0.520	0.099* (0.022)	0.210* (0.046)	0.071* (0.024)	0.119* (0.040)
Measures of Current Neighborhood Safety					
Share feeling safe during the day (n=3514)	0.750	0.093* (0.018)	0.198* (0.039)	0.096* (0.019)	0.161* (0.032)
Share feeling safe at night (n= 3482)	0.549	0.142* (0.022)	0.303* (0.046)	0.093* (0.024)	0.156* (0.040)
Share saw drugs past 30 days (n=3480)	0.445	-0.117* (0.022)	-0.248* (0.046)	-0.103* (0.024)	-0.171* (0.039)
Share any household member crime victim in last 6 months (n= 3499)	0.209	-0.040* (0.017)	-0.085* (0.036)	-0.053* (0.018)	-0.089* (0.030)
Measures of Current Neighborhood Quality					
Share reporting litter/trash/graffiti/ abandoned buildings (n=3502)	0.704	-0.111* (0.021)	-0.236* (0.046)	-0.076* (0.024)	-0.127* (0.040)
Share reporting public drinking/groups of people hanging out (n=3489)	0.695	-0.170* (0.022)	-0.360* (0.046)	-0.099* (0.024)	-0.166* (.040)
Share reporting police not responding (n=3286)	0.337	-0.128* (0.020)	-0.266* (0.042)	-0.092* (0.022)	-0.157* (0.038)
Share very satisfied or satisfied with current neighborhood (n=3524)	0.475	0.138* (0.022)	0.293* (0.047)	0.108* (0.024)	0.180* (0.040)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult Survey
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Due to the statistical software used to generate the impact estimates the numbers of cases used are slightly smaller than the numbers in the corresponding appendix C tables. This accounts for the small differences in control means.

These findings are consistent with those of the qualitative report (Popkin, Harris, and Cunningham 2001), which highlighted the dramatic improvements in neighborhood safety respondents described, in contrast to their previous neighborhoods in public housing. In addition to noting reductions in graffiti and trash, respondents in the experimental group described their new neighborhoods as quiet and peaceful. Respondents in the comparison group did not speak as much about quiet but still acknowledged the reduced visibility of drugs and violence. This point is particularly salient considering the ways that some of the respondents described their subsequent moves from their initial low-poverty areas to neighborhoods with higher poverty levels: Both adults and children described feeling vividly the loss of their sense of safety.

Measures of neighborhood quality and satisfaction (found in the last panel of exhibit 3.5) also showed effects that were significant for the experimental and Section 8 groups, but the impact estimates were about double in size for the experimental group program movers relative to the Section 8 group program movers. Overall, the control group had about a 50-50 chance of saying they were very or somewhat satisfied with their neighborhoods. But this chance was increased by over 10 points for the treatment groups taken as a whole, and the treatment-on-treated impact for the experimental group raised this chance by nearly 30 points. Thus the members of the experimental group, even after subsequent moves, continued to reap the benefits of moves to better neighborhoods.

In the qualitative interviews, some teenagers recalled and talked clearly about the differences between their former developments and their current neighborhoods. For example, Jordan, a 16-year-old boy from Baltimore living in a low-poverty neighborhood, said:

> *Yes, it's definitely better here [low-poverty neighborhood] than at Murphy Homes. Some examples, not too much drug activity over here, cops patrol here every, they patrol here mostly all day. There's not too many people out vandalizing things. We've got good places here that's not touched with any graffiti.*

The Section 8 comparison group program movers in the qualitative sample also mentioned safety as the most valuable aspect of their current neighborhoods. Some still complained about problems with drugs and crime, describing their neighborhoods as safe during the day but unsafe at night. Others talked about problems like graffiti or a few bad teenagers. But because the public housing developments they came from were so dangerous the reductions in violent crimes felt like a substantial improvement to most movers.

Nicolasa, a Section 8 group respondent from Boston, was living in a neighborhood with a 32 percent poverty rate (a relatively high rate). Yet she talked about how she enjoyed her neighbors and the comparative quiet and safety of her neighborhood.

> *I'm happy here. I've been here for 3 years. I'm not thinking of moving. If I do move, it'll be because I found something even better. But for now, I'm not thinking of moving. I get along with the people in the house, and, more importantly, the street is quiet, especially for the kids. If I want to go downstairs, walk around with them or sit outside, I can do that and feel safe.*

3.5 Interpretation of Results

We found substantial, positive program effects on a wide variety of measures related to housing, neighborhood, and safety. These effects were significant for the experimental and the Section 8 groups taken as a whole (intent-to-treat effects), and the estimated effects for program movers were often much larger for the experimental group than for the Section 8 group. Examples include the effects on crime victimization, on measures of housing quality (problems with vermin and with paint or plaster within apartments), on measures of neighborhood quality (litter and trash in the area, public drinking) and on measures of neighborhood safety (residents witnessing drug transactions, feelings of safety at night).

MTO program movers did experience some adverse effects compared to the control group in two areas. The proportion encountering problems paying utility bills was significantly higher, particularly for the experimental group. And more of the experimental and Section 8 group members reported facing bias or discrimination in housing search. Experimental group program movers reportedly experienced about double the incidence of bias or discrimination as control group members, while the estimated effect for Section 8 program movers was over three times the control mean. We speculate that this significant impact is due to the greater likelihood that the treatment group members rent in the private market, while a far greater share of the control group adults are occupying public housing.

Yet the positives come through clearly in the survey responses of experimental group and Section 8 group adults. They rate their current housing and neighborhoods far more highly than the controls. Taken together, these findings suggest that at the mid-term of the 10-year research period the MTO program had produced notable housing and neighborhood improvements for the participants, compared with the housing, neighborhood and safety conditions faced by members of the control group. Despite substantial control group mobility and despite some convergence of neighborhood characteristics for the experimental and Section 8 groups, estimated effects for experimental group program movers were consistently larger (even twice as large) as the effects for the Section 8 group movers.

Chapter Four
Impacts on Adults' and Children's Health

This chapter reviews the reasons why MTO participation might be expected to affect the health of the adults and children in the sample and then presents the interim findings on health impacts. Most of the results for adults and youth ages 12 to 19 are based on self-reported health indicators from the interim surveys. The results for children under age 12 are based on reports from their parents or primary caregivers. The findings on adult hypertension come from direct measurements obtained at the time of the survey.

Summary

MTO reduced the incidence of obesity and psychological distress, and increased feelings of calm and peacefulness among sample adults in the experimental group but not in the Section 8 group. MTO also appears to have reduced the incidence of depression among the experimental group, although this reduction was statistically significant for only one of the two measures of depression. MTO did not have a statistically significant impact on general health, activities of daily living, asthma, or blood pressure. Nor were there statistically significant results on smoking or alcohol consumption.

For children, there were no statistically significant overall effects on general health, asthma, obesity, or injuries, although there was a statistically significant increase in injuries among male youth ages 12 to 19. In terms of mental health youth (ages 12 to 19) in the Section 8 group showed a significant decrease in generalized anxiety. For psychological distress and depression there were no statistically significant overall impacts for either the experimental or Section 8 groups. There was, however, evidence of mental health improvements among girls in both the experimental and Section 8 groups that deserves further study.

4.1 Hypotheses about Adult and Child Health in MTO

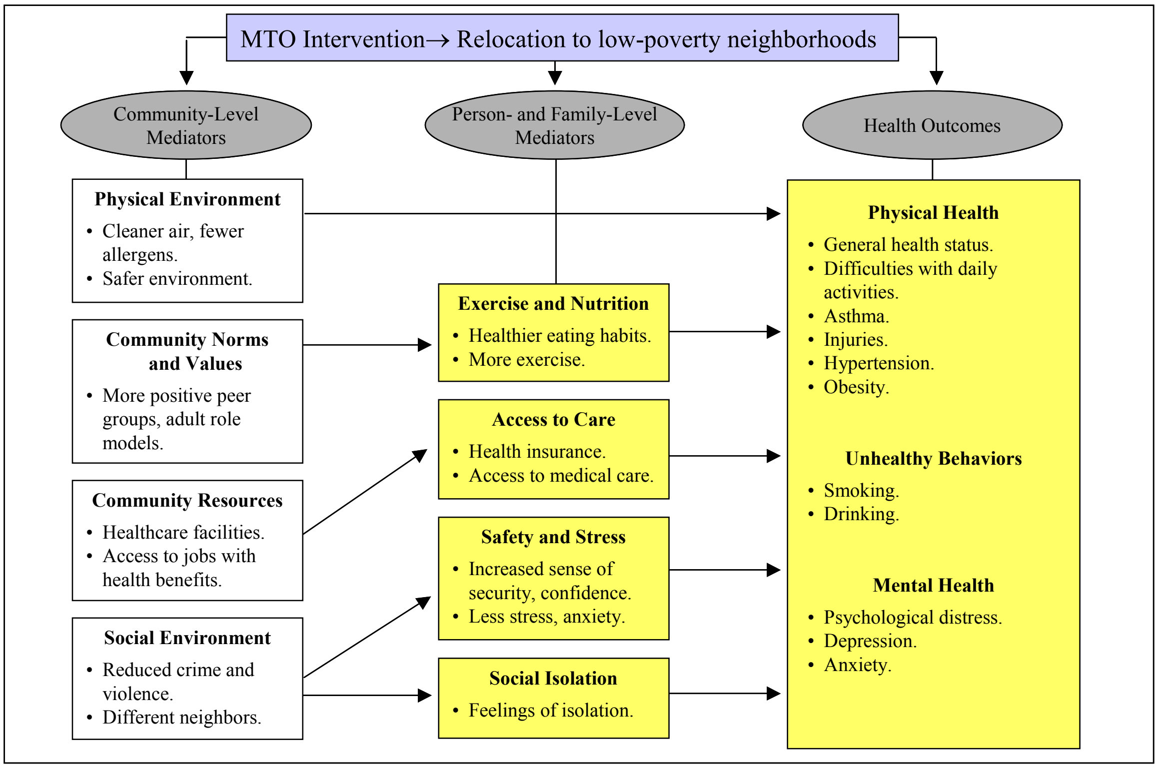
Conceptual framework

A substantial literature in epidemiology suggests that living in a high-poverty urban setting is associated with a wide range of adverse health outcomes for both adults and children. If these associations reflect causal relationships then the MTO demonstration would be expected to improve health outcomes. In particular, relocation to a low-poverty neighborhood could affect health status through various mechanisms described below (and in exhibit 4.1).

Changes in the physical environment. Children in urban areas are more likely to suffer from asthma (Weiss et al., 1992), possibly due to poor air quality (Thurston, 1997; Mortimer et al. 2002, Ostro et al. 2001) and exposure to allergens from cockroaches, mites, and cats (Gelber et al. 1993). Accidents are the leading cause of death among children ages 1 to 14 and urban children have higher rates of injuries and accidents possibly due to unsafe playgrounds and other features of the environment

(Scharfstein and Sandal, 1998; Quinlan, 1996). This evidence suggests that encouraging people to move to lower poverty neighborhoods might lead to reductions in asthma and injuries.

EXHIBIT 4.1
HEALTH OUTCOMES AND MEDIATING FACTORS



Changes in the social environment. Preliminary short-term evidence from the MTO sites demonstrated that MTO reduced criminal victimization and exposure to violence among members of both treatment groups (Katz, Kling, and Liebman 2001), thereby reducing stress and increasing participants' feelings of tranquility (Katz, Kling, Liebman 2003).

Exposure to violence can have long-term behavioral and psychological consequences for both children and adults (Groves et al. 1993; Famularo et al. 1996; Zapata et al. 1992). Stress is also hypothesized to be a trigger for asthma (Wright 1998) and possibly a risk factor for hypertension (Black et al. 1997; Kornitzer et al. 1999). If the MTO treatment reduces exposure to crime and violence, it could improve overall well-being and reduce psychological distress, depression, anxiety, and hypertension (Ross and Mirowsky, 2001, Silver et al. 2002, Aneshensel and Sucoff, 1996). Reduced exposure to violence could also directly reduce injuries from assaults. It is also possible that moves to lower poverty neighborhoods could have adverse effects on mental health by leaving MTO family members socially or culturally isolated.

Changes in community norms and values. Moves to lower poverty neighborhoods could change the characteristics of sample members' peers. Peers in the low-poverty neighborhoods may have healthier eating and exercise habits (Lee and Cubbin, 2002) and may be less likely to engage in unhealthy habits such as smoking (Berkman and Breslow 1983). Peer influences have been documented for these behaviors (Sallis et al. 2000; Raudsepp and Viira 2000). Therefore, MTO could lead to reduced obesity through healthier diet and exercise and to reductions in unhealthy behaviors.

Changes in community resources. Higher income neighborhoods could have medical professionals who are accustomed to providing more resource-intensive healthcare than is provided in lower income neighborhoods. If this is the case, then members of the MTO treatment groups might have higher quality care available to them after their moves. It is not clear whether low-income residents of high-income neighborhoods would be able to access this higher quality care, especially if language or cultural barriers are a factor. Moreover, since many academic medical centers are located in urban areas it is possible that moves to low-poverty neighborhoods would reduce access to high-quality care. It is also possible, that higher income communities offer more job opportunities that include health benefits. In this case treatment group members could be more likely to have health insurance and to receive preventive care.

Earlier research

There is a moderate-sized nonexperimental literature that suggests the possibility of a relationship between neighborhood of residence and health status. Recent contributions include Waitzman and Smith (1998) who find that people living in federally designated poverty areas have higher rates of mortality even after controlling for individual characteristics; Silver et al. (2002) who find that neighborhood disadvantage is associated with higher rates of major depression and substance abuse disorder; Ross and Mirowsky (2001) who find that living in a disadvantaged neighborhood is associated with lower levels of self-reported health and physical functioning; and Browning and Cagney (2002) who find that individuals residing in neighborhoods with greater collective efficacy report better overall health. Diez Roux (2001), Kawachi and Berkman (2003), and Macintyre and Ellaway (2003) provide useful reviews of this literature.

Preliminary short-term evidence from the MTO sites showed that adults in both treatment groups had improvements in general health status and were more likely to feel calm and peaceful (Katz, Kling, and Liebman 2001). Children in the experimental group experienced reductions in asthma, injuries, and fearfulness relative to the control group (Katz, Kling, and Liebman 2001; Leventhal & Brooks-Gunn 2003). Younger children in the Section 8 group also experienced reductions in feeling unhappy, sad, or depressed (Leventhal & Brooks-Gunn 2003).

4.2 Data Sources and Measures

Data sources

All of the health outcomes described here except high blood pressure were measured with data from the interim surveys. The data on adult and youth (ages 12 to 19) health outcomes are self-reported. The data on child (ages 5 to 11) health outcomes are based on parental or primary care-giver reports.

Adult blood pressure was measured directly at the time of survey administration using readings from an automated sphygmomanometer (blood pressure monitor).

Measures

All of the survey questions are ones that have been used before in large national surveys. Many are those used by the National Health Interview Survey, including: general health, asthma, height and weight, smoking, and psychological distress. Several of the measures require additional explanation.

Limited activities. Reports whether the sample adult had difficulty with two activities of daily living (ADLs): lifting and carrying groceries and climbing stairs. We estimate the fraction of the sample who reported being limited a little or a lot on at least one of the two activities. These were chosen from the larger universe of ADLs as the ones most likely to be relevant in a sample in which most adults were fairly young. Measures of ADLs are used by a number of national surveys and are important for measuring an individual's functional status and quality of life (Wiener et al., 1990).

High blood pressure currently. This is defined as an adult with a systolic blood pressure of 140 or higher or a diastolic blood pressure of 90 or higher. This definition is standard (Black et al. 1997).

Obese currently. Defined as the percentage of the sample with a body mass index (weight in kilograms divided by height in meters squared) of 30 or higher. This criterion is consistent with the National Health Interview Study definition of obese (Pleis & Cole, 2002).[66]

Moderate or heavy drinker during past year. Defined as an adult who drank an average of at least three drinks per week during the past 12 months.

Psychological distress index. Reports the fraction of six mental health outcomes that the adult sample member reported feeling at least some of the time during the past 30 days. For adults the six items are feeling: so sad nothing could cheer you up, nervous, restless or fidgety, hopeless, everything was an effort, or worthless. For youth, the question so sad nothing could cheer you up is replaced by so depressed nothing could cheer you up to allow us to compare results with nationally representative surveys administered to youth.

Depressed during the past year (adult). Estimates the fraction of the sample that experienced an episode of major depression at some point during the past year using the (CIDI-SF) Major Depressive Episode scale. The CIDI-SF, the development of which is described by Kessler et al. (1998), yields a symptom count for depression. A count of 3 or more means that it is likely that a respondent with this profile would meet full diagnostic criteria for major depressive episode if given the complete Composite International Diagnostic Interview (CIDI), which is the instrument used to generate population estimates of psychiatric conditions in the U.S. (Kessler et al. 1994). We used version 1.0 of the CIDI-SF. This version contained an error in the skip pattern for major depression which makes it impossible to classify certain boundary cases as "depressed" or "not depressed." We therefore

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[66] The obesity measures reported in this chapter are based upon self-reported height and weight. For young children we also collected direct measurements. However, we are still assessing the quality of those data.

present two sets of results, one set in which the boundary cases are classified as "depressed" and a second set in which the boundary cases are classified as "not depressed."[67]

Anxiety during the past year (adult). Constraints on interview length prevented us from administering a full screen for Generalized Anxiety Disorder (GAD). However, we did include a short two-question sequence asking whether the sample adult had experienced either "a period lasting one month or longer when most of the time he or she felt worried, tense or anxious," or "a time when he or she worried a lot more than most people would in his or her situation."

Calm and peaceful most of the time in the past month (adult). We included one question on a positive aspect of mental health: feeling calm and peaceful. We report whether the sample adult felt calm and peaceful at least some of the time during the past 30 days.

Depression and generalized anxiety disorder during lifetime (youth). For both of these outcomes, we used scales developed for use by the National Comorbidity Survey Replication: Adolescent Supplement (NCSR-AS).[68]

4.3 Context and Baseline Status of the Sample

No data on current health status were collected in the baseline survey administered when families joined MTO. It was not until qualitative research with MTO families conducted as part of HUD's small grant program suggested that the evaluation might have large impacts on the health of participating families that health became a focus of the evaluation (Kling, Liebman, and Katz, 2001). This qualitative research suggested that fear of random violence was the prime motivation for trying to move, that MTO moves led to reductions in stress levels among both children and adults, and that children were less likely to be injured in their new neighborhoods.

There is some evidence in the baseline data of environmental conditions that might produce health problems. For example, 35 percent of MTO families reported that their apartments had big problems with rats or mice suggesting that environmental triggers of asthma may have been quite prevalent. Similarly, 24 percent reported a family member having been beaten or assaulted in the past 6 months, a rate that was high even when compared to other samples of public housing residents (Katz, Kling, and Liebman 2001). More generally, as urban residents of high-poverty neighborhoods MTO sample members were likely at baseline to have had high rates of obesity, hypertension, substance abuse, asthma, depression, and exposure to violence. Data collected in the first few years after random assignment from members of the control group confirmed that, absent the treatment, the overall health of MTO sample members was generally worse than demographically similar members of the U.S. population (Katz, Kling, and Liebman 2001).

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[67] For a discussion of methods for handling the CIDI-SF v1.0 skip pattern errors, see the "CIDI-SF Memo: Edits" (December 2002) which is available through the CIDI-SF website (http://www.who.int/msa/cidi/CIDI-SFeditsmemo.pdf).

[68] For a description of the NCSR-AS see www.hcp.med.harvard.edu/ncs/.

4.4 Mediators for Health Impacts in MTO

This section reviews effects on the mediating factors most directly relevant to the hypotheses described in section 4.1. The tables containing these results are found in appendix E and in chapters 3 and 7.

Home physical environment

Chapter 3 shows that experimental group adults were less likely to report problems with vermin than were control group adults and that members of both treatment groups reported improvements in housing quality (exhibit 3.4). Thus exposure to some environmental triggers of asthma may have been reduced by MTO. However, exhibit E4.1 shows that both treatment groups had higher percentages of housing with wall-to-wall carpets than the control group did. Carpeting can harbor dust mites and its removal or treatment is often recommended as an asthma remediation strategy (Crain et al. 2002, van der Heide et al., 1997).

Exercise and nutrition

We analyzed several measures of exercise and nutrition (exhibit E4.2). Among young children ages 8 to 11 there were no overall statistically significant improvements in aerobic exercise. However, boys in the Section 8 group experienced a statistically significant decline in aerobic exercise. For youth (ages 12 to 19) there was an increase in aerobic exercise in the experimental group, but no statistically significant change in the amount of light physical activity. Nor was there a change in the fraction of days during the past week in which children (ages 8 to 19) ate fruits or vegetables. Among adults there was an increase in moderate physical activity among members of the Section 8 group, but no statistically significant change for the experimental group. There was an increase in the frequency of adult fruit and vegetable consumption in the experimental group, but no statistically significant change in the Section 8 group.

Access to healthcare

We analyzed several measures of access to healthcare, including whether family members have health insurance, did not get medical care when they needed it, have a usual place to receive medical care, or have spoken to a health professional in the past 6 months (exhibit E4.3). There were no statistically significant impacts of the MTO demonstration on access to health care for either treatment group. In addition, exhibit 7.4 shows that there was no impact on the probability that an adult sample member is currently employed at a job offering health insurance.

Social isolation

We analyzed a variety of measures of social networks, friendships, and social capital (exhibits E5.2 and E5.3). There were no statistically significant impacts on adult friendships or social networks. There were increases in both treatment groups in the fraction of adults who said that their neighbors would do something about kids spray-painting graffiti or skipping school and hanging out on a street corner. Among children there was little impact of the intervention on the number of friends. The only statistically significant result was a 2.4 percentage point increase in the fraction of experimental group children with at least one close friend above a control mean of 91.4 percent. There were, however, large reductions in contacts with friends from the old neighborhood for children in both treatment groups.

Safety and exposure to violence

The MTO intervention appears to have produced large improvements in safety and exposure to violence. Exhibit 3.4 shows that adults in both treatment groups were more likely to feel safe both during the day and at night. They were also less likely to see someone dealing drugs or to report any household member being victimized by crime in the past 6 months. Children in the experimental group were less likely to say that there were gangs in their neighborhood, that they had seen people selling or using illegal drugs in their neighborhood, or that they had heard gunshots in their neighborhood (exhibit E5.5). Reductions for the Section 8 group were of a similar magnitude for seeing people with drugs and hearing gunshots, but only the result for gunshots was statistically significant. Among children there were no statistically significant effects for seeing a person shot or stabbed, having a gun or knife pulled on them, or having been cut, shot, or stabbed.

4.5 Interim Impacts on Health

Effects on adults' health

Preliminary research on MTO in the Boston site (Katz, Kling, and Liebman, 2001) suggested that MTO caused large improvements in general health as measured by the standard question "In general is your health excellent, very good, good, fair, or poor?" Preliminary studies of Boston and New York (Katz, Kling, and Liebman, 2001, Leventhal and Brooks-Gunn 2000) showed improvements in adult mental health. An important question for the interim evaluation was therefore whether the improvements in general health were attributable solely to improvements in mental health or whether MTO also caused improvements in physical health.

General health. The first set of results in exhibit 4.2 shows effects on adult physical health. General health was measured by the question "In general is your health excellent, very good, good, fair, or poor?" The percentage of treatment group adults who reported good or better health was not statistically different than the percentage for the control group.

Physical health. To isolate the effects on physical aspects of health, the interim evaluation collected data on whether sample adults had difficulty with two activities of daily living (ADLs): lifting and carrying groceries and climbing stairs. These were chosen from the larger universe of ADLs as the ones most likely to be relevant in a sample in which most adults were fairly young. Exhibit 4.2 shows that there was no discernable effect of MTO on the percentage of the sample who reported being limited a little or a lot in at least one of the two activities. Forty-four percent of control group members reported being limited, as did 42 percent of experimental and 41 percent of Section 8 group members.

The interim evaluation also collected data for three health conditions that are particularly prevalent in inner-city neighborhoods: asthma, high blood pressure, and obesity. Twenty-one percent of control group sample adults reported having had an asthma or wheezing attack during the past year. Roughly 20 percent of experimental and Section 8 sample adults had experienced such attacks. Thus there was no statistically significant impact of the program on asthma.

This result is interesting because, as noted above in the section on mediators, MTO does appear to have reduced exposure to two triggers of asthma: stress-producing violence and rats, mice, and cockroaches. However, MTO also caused the percentage of sample adults living in housing with wall-to-wall carpeting to increase. It is possible that the detrimental effects of the increase in carpeting offset the benefits of reduced exposure to the other two asthma triggers.

While blood pressure has a significant genetic component, it is also a function of stress, weight, and activity patterns such as exercise and diet, all of which could be affected by living in a new neighborhood (Kornitzer et al. 1999). Increasing evidence suggests that variations in the magnitude and timing of blood pressure response to stress are associated with heightened risk of developing hypertension and accelerated arteriosclerosis (Kamarck et al. 1997; Everson et al. 1997; Lynch et al. 1998). There is also considerable evidence of links between distressed psychosocial states and heightened blood pressure (Everson, Kaplan & Salonen, 1997; Everson et al. 1998). These are all possible pathways through which MTO could, in theory, affect blood pressure. In addition, by affecting access to healthcare, MTO could influence whether people take their blood pressure medicine. Exhibit 4.2 shows, however, that MTO did not have a statistically significant impact on the share of sample adults with high blood pressure (defined as an adult with a systolic blood pressure of 140 or higher or a diastolic blood pressure of 90 or higher). In the control group 30 percent of sample adults had high blood pressure, whereas 32 percent of adults in the experimental and Section 8 groups had high blood pressure.

EXHIBIT 4.2
ADULT PHYSICAL AND MENTAL HEALTH OUTCOMES

Outcome	Control Mean	Experimental Vs. Control ITT[a]	Experimental Vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
General Health [SR] (n=3523)					
Good or better health currently	0.669	-0.016 (0.020)	-0.033 (0.042)	-0.007 (0.022)	-0.013 (0.036)
Physical Health					
Activities limited a little or a lot currently [SR] (n=3517)	0.436	-0.018 (0.021)	-0.037 (0.045)	-0.023 (0.023)	-0.039 (0.038)
Asthma or wheezing attack during past year [SR] (n=3522)	0.214	-0.014 (0.018)	-0.031 (0.037)	-0.010 (0.019)	-0.016 (0.032)
High blood pressure currently [M] (n=3230)	0.301	0.022 (0.021)	0.049 (0.045)	0.019 (0.023)	0.032 (0.039)
Obese currently [SR] (n=3405)	0.471	-0.051* (0.022)	-0.108* (0.047)	-0.047 (0.025)	-0.079 (0.042)
Unhealthy Behaviors [SR]					
Smoker currently (n=3499)	0.290	0.010 (0.020)	0.021 (0.042)	0.003 (0.022)	0.006 (0.036)
Moderate or heavy drinker during past year (n=3477)	0.094	-0.008 (0.013)	-0.017 (0.027)	0.003 (0.014)	0.004 (0.023)
Mental Health [SR]					
Psychological distress index for past month (n=3521)	0.329	-0.034* (0.015)	-0.073* (0.032)	-0.012 (0.016)	-0.020 (0.028)
Depressed during past year, including boundary cases (n=3520)	0.219	-0.036* (0.018)	-0.078* (0.039)	-0.019 (0.020)	-0.032 (0.034)
Depressed during past year, excluding boundary cases (n=3520)	0.180	-0.026 (0.017)	-0.056 (0.036)	-0.010 (0.019)	-0.016 (0.032)
Anxiety during past year (n=3473)	0.393	-0.028 (0.022)	-0.060 (0.047)	-0.011 (0.024)	-0.019 (0.040)
Calm and peaceful most of the time or more often in the past month (n=3520)	0.466	0.061* (0.022)	0.129* (0.047)	0.016 (0.024)	0.027 (0.041)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Adult survey
Sample: Adults from families randomly assigned by December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b)Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

MTO does appear to have significantly reduced the incidence of obesity (measured as the percentage of the sample with a body mass index of 30 or more) in the experimental group.[69] Measured as the impact on those who leased up, obesity was reduced by 11 percentage points in the experimental group.

Given that obesity was the only one of six adult physical health items that showed any program impact, it is worth asking whether this is likely to be a true impact of the program or simply the result of sampling variation. Some corroborating evidence comes from questions about adult diet. As discussed above, exhibit E4.2 provides some evidence that eating habits changed in response to the MTO treatment. There was a 3 percentage point increase for adults in the experimental group in the fraction of days in the last week that they ate some fruits or vegetables. Regarding adult exercise, the Section 8 group experienced a statistically significant increase in moderate physical activity. The increase in exercise for the experimental group was smaller and not statistically significant.

Mental health. The interim evaluation collected data on two detailed measures of adult mental health: a psychological distress index and a measure of depression. The psychological distress index gives the fraction of six mental health outcomes that the adult sample member reported feeling at least some of the time during the past 30 days. The depression measure estimates the fraction of the sample that experienced an episode of major depression at some point during the past year using the CIDI-SF Major Depressive Episode scale.

As exhibit 4.2 shows, the control group had a mean level of about 33 percent on the psychological distress index and between 18 and 22 percent, depending on which measure is used, had been depressed in the last year. MTO reduced psychological distress among experimental group sample adults by about 3.5 percentage points. The effect on those who leased up was a 7 percentage point reduction in psychological distress. This experimental group impact is of substantial magnitude, reducing the incidence of psychological distress by over one-fifth. The point estimates of the impact on depression for the experimental group are of a similar magnitude to that for psychological distress, though only the depression measure that includes the boundary cases is statistically significant (the p-value on the more restrictive measure is .12). The estimates for the Section 8 group were not statistically distinguishable from zero for either psychological distress or depression.[70]

In addition to the detailed measures, we also included two mental health outcomes measured using only one or two questions: generalized anxiety disorder and feeling calm and peaceful. Generalized anxiety disorder (GAD) is the second most prevalent psychological disorder. Although constraints on interview length prevented us from administering a full screen for GAD, we did include a short two-question sequence asking whether the sample adult had experienced either “a period lasting one

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69 The p-value of the experimental effect on obesity is .021. The p-value of the Section 8 effect on obesity is .057.

70 We explored the sensitivity of our results to excluding data from one interviewer whose data appear to have been of questionable quality. In the 112 adult interviews conducted by this interviewer, 36 percent of the adults were recorded as having volunteered that they were on medication for depression, and scored as depressed. Among the 3408 other interviews, one percent were recorded as on medication for depression. Excluding the interviews conducted by this interviewer reduces the estimated prevalence of depression, but has little impact on the between-group differences.

month or longer when most of the time he or she felt worried, tense or anxious" or "a time when he or she worried a lot more than most people would in his or her situation." The results for anxiety could not be statistically distinguished from zero. On the second measure, feeling calm and peaceful, the experimental group showed a statistically significant increase. The results for the Section 8 group were not statistically significant.

Unhealthy behaviors. The interim evaluation collected data on two unhealthy behaviors: smoking and drinking. There are several mechanisms through which moves to low-poverty neighborhoods could influence these behaviors. Depression and stress are correlated with both tobacco and alcohol use and exposure to tobacco and alcohol advertising, particularly advertising targeted at minority groups, could decline.

In our analysis, a person is considered a smoker if he or she smoked at least one cigarette in the past 30 days. Exhibit 4.2 shows that MTO had no impact on smoking behavior. Sample adults in all three program groups had smoking rates of between 29 and 30 percent. Exhibit 4.2 also reports the percentage of sample adults who were moderate or heavy drinkers during the past year. There was no effect of MTO on this drinking behavior. Between 9 and 10 percent of sample adults in all three groups were moderate or heavy drinkers.

Effects on children's health

Physical health. The physical health measures and the mechanisms by which MTO might affect children's health generally parallel those for adults. Exhibits 4.3 and 4.4 show the physical health outcomes for children ages 5 to 11 and ages 12 to 19, respectively. We collected data on general health using the same question as that used for adults. For children and youth we estimate the impact of having very good or better health. There were no statistically significant program effects on general health for either treatment group. We also collected data on whether the children or youth had had an asthma or wheezing attack during the past year. Again there were no statistically significant impacts. We also collected data on obesity (measured by whether the youth's body mass index placed them in the 95th percentile or greater for his or her age and sex). In contrast with the adults, there was no effect of MTO on youth obesity.

In addition, the interim evaluation collected data on one health outcome that was not collected for adults: whether the child or youth had had an accident or injury requiring medical attention in the prior year. Accidents and injuries are an important factor in overall health for children and youth. Low-poverty neighborhoods may be safer in some respects (e.g., better housing, less exposure to violence, safer playgrounds), but they may encourage more exercise and outdoor play. Thus, the causes of accidents and injuries may change as a result of the MTO treatment. Exhibits 4.3 and 4.4 show that there was no discernable impact of the MTO treatment on accidents and injuries for either treatment group overall for either young children or for youth.[71] However, boys ages 12 to 19 in the Section 8 group experienced a statistically significant increase in injuries (exhibit 4.4).

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[71] In results not shown, we have examined a measure of injuries that excludes sports-related injuries. We similarly find no overall impact of MTO on injuries using this alternative measure.

The lack of positive impacts on asthma and child injuries is inconsistent with the earlier evidence from the Boston site. We have confirmed that this is not simply the result of cross-site variation. Using the interim evaluation data and restricting the sample to the same households studied in the earlier work similarly results in no impact of MTO on asthma or child injuries. Therefore, the initial effects appear to have been short-term effects that did not persist to the time of the interim evaluation.

Mental health. Three mental health measures were obtained for youth ages 12 to 19. First, we used a six-item scale of psychological distress during the past month. Second, we used a lifetime depression scale that is meant to produce estimates of major depressive episodes. This scale was developed for use by the National Comorbidity Survey Replication: Adolescent Supplement (NCSR-AS). Finally, we use the NCSR-AS lifetime generalized anxiety disorder scale.

Exhibit 4.5 shows that the results across the three mental health outcomes are similar. There is little overall effect on youth as a whole except for a decline in generalized anxiety for the Section 8 group. None of the other aggregate impact measures were statistically significant. However, the zero overall impact estimates on these measures appear to mask substantial differences between boys and girls. For all three measures, the point estimates for girls in both treatment groups suggest substantively large improvements in mental health and four of the six estimates are statistically significant. In contrast, the point estimates for boys generally indicate small declines in mental health, though none of them are statistically significant.[72]

[72] The one exception is the result for generalized anxiety disorder where both boys and girls in the Section 8 group experienced improvements.

EXHIBIT 4.3
CHILD PHYSICAL HEALTH OUTCOMES, AGES 5 TO 11

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child's Health is Very Good or Better Currently [PR] (n=2525)					
All children (ages 5 to 11)	0.707	-0.005	-0.010	-0.011	-0.017
		(0.026)	(0.055)	(0.029)	(0.044)
Girls	0.752	-0.001	-0.002	0.008	0.012
		(0.033)	(0.067)	(0.038)	(0.058)
Boys	0.660	-0.009	-0.020	-0.031	-0.047
		(0.038)	(0.086)	(0.042)	(0.063)
Had Asthma or Wheezing Attack During Past Year [PR] (n=2516)					
All children (ages 5 to 11)	0.150	-0.009	-0.018	0.015	0.023
		(0.018)	(0.038)	(0.021)	(0.032)
Girls	0.119	-0.008	-0.017	-0.004	-0.006
		(0.022)	(0.045)	(0.026)	(0.039)
Boys	0.182	-0.009	-0.020	0.035	0.053
		(0.027)	(0.061)	(0.034)	(0.051)
Had Injury Requiring Medical Attention During Past Year [PR] (n=2521)					
All children (ages 5 to 11)	0.074	-0.006	-0.013	0.003	0.005
		(0.014)	(0.029)	(0.015)	(0.024)
Girls	0.052	0.001	0.003	0.003	0.005
		(0.016)	(0.033)	(0.019)	(0.029)
Boys	0.097	-0.013	-0.030	0.004	0.006
		(0.022)	(0.048)	(0.025)	(0.037)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Adult survey
Sample: Children. All child ages as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 4.4
CHILD PHYSICAL HEALTH OUTCOMES, AGES 12 TO 19

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child's Health is Very Good or Better Currently [SR] (n=2822)					
All children (ages 12 to 19)	0.680	0.001 (0.024)	0.001 (0.054)	-0.006 (0.027)	-0.010 (0.048)
Girls	0.635	0.030 (0.034)	0.065 (0.075)	0.008 (0.038)	0.013 (0.062)
Boys	0.725	-0.029 (0.034)	-0.066 (0.076)	-0.020 (0.036)	-0.036 (0.067)
Had Asthma or Wheezing Attack During Past Year [SR] (n=2812)					
All children (ages 12 to 19)	0.163	0.029 (0.019)	0.065 (0.042)	0.012 (0.021)	0.022 (0.036)
Girls	0.192	0.034 (0.029)	0.073 (0.063)	-0.011 (0.031)	-0.018 (0.050)
Boys	0.134	0.025 (0.025)	0.055 (0.055)	0.035 (0.028)	0.065 (0.052)
Had Injury Requiring Medical Attention During Past Year [SR] (n=2817)					
All children (ages 12 to 19)	0.122	0.004 (0.016)	0.010 (0.035)	0.030 (0.019)	0.053 (0.034)
Girls	0.108	-0.021 (0.020)	-0.047 (0.043)	-0.010 (0.023)	-0.016 (0.037)
Boys	0.136	0.030 (0.025)	0.068 (0.056)	0.070* (0.031)	0.130* (0.058)
Body Mass Index in the 95th Percentile or Greater Currently [SR] (n=2676)					
All children (ages 12 to 19)	0.165	0.018 (0.021)	0.041 (0.048)	0.001 (0.023)	0.002 (0.040)
Girls	0.164	0.013 (0.027)	0.029 (0.060)	-0.002 (0.031)	-0.004 (0.050)
Boys	0.166	0.024 (0.032)	0.053 (0.071)	0.005 (0.034)	0.009 (0.063)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Youth survey
Sample: Youth. All ages as of May 31, 2001.
Notes: a) a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b)Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 4.5
YOUTH MENTAL HEALTH OUTCOMES, AGES 12 TO 19

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Psychological Distress Index for Past Month [SR] (n=2803)					
All (ages 12 to 19)	0.256	-0.006	-0.013	-0.006	-0.010
		(0.015)	(0.032)	(0.016)	(0.028)
Girls	0.304	-0.045*	-0.098*	-0.043	-0.072
		(0.020)	(0.043)	(0.023)	(0.037)
Boys	0.208	0.034	0.077	0.031	0.057
		(0.020)	(0.045)	(0.022)	(0.040)
Depression During Lifetime [SR] (n=2709)					
All (ages 12 to 19)	0.065	-0.007	-0.015	-0.014	-0.024
		(0.013)	(0.028)	(0.013)	(0.022)
Girls	0.102	-0.025	-0.054	-0.041*	-0.067*
		(0.020)	(0.044)	(0.021)	(0.033)
Boys	0.028	0.011	0.025	0.013	0.024
		(0.015)	(0.034)	(0.016)	(0.029)
Generalized Anxiety Disorder During Lifetime [SR] (n=2652)					
All (ages 12 to 19)	0.067	-0.016	-0.035	-0.035*	-0.061*
		(0.013)	(0.028)	(0.013)	(0.022)
Girls	0.091	-0.042*	-0.092*	-0.047*	-0.078*
		(0.018)	(0.040)	(0.019)	(0.031)
Boys	0.042	0.012	0.026	-0.023	-0.042
		(0.018)	(0.040)	(0.016)	(0.029)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Youth survey
Sample: Youth. Ages as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b)Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

4.6 Interpretation of Results

The results of the interim evaluation confirm one of the important findings of the earlier preliminary MTO research. Moves from high-poverty public housing to low-poverty neighborhoods appear to have had important mental health benefits for sample adults, most likely due to reduced exposure to crime and violence and therefore an improved sense of safety. The interim evaluation suggests a second important health benefit from the MTO demonstration that had not been identified before: lower rates of obesity. Given that obesity is associated with several serious health conditions including diabetes, gallstones, hypertension, heart disease, stroke, osteoarthritis, and certain types of cancers (Fields et al. 2001; Must et al. 1999; Wellman and Friedberg 2002), this is a potentially important finding.

An interesting question is whether the mental health and obesity results are related. For example, could the mental health improvements be the mediating factor that led to the lower rates of obesity? While several studies have documented an association between obesity and depression, the pathways and mechanisms behind this association have not been determined (Carpenter et al. 2000). Some theories suggest obesity as a cause of depression, others posit depression as a cause of obesity, and still others suggest that a third factor could be responsible for both.

There are two puzzles in the child health results. The first is that the positive impacts of MTO on asthma and child injuries that were documented for the Boston site in early research were not confirmed in the interim evaluation. We currently have no good explanation for why the initial findings on asthma and injuries did not persist.[73] The second puzzle is the difference in mental health results between girls and boys. Some possible explanations for differential impacts on boys and girls are discussed at the end of chapter 5.

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73 The failure to confirm the early Boston results on asthma and injuries is not simply the result of differences across sites. Even when the sample is restricted to the same 540 families studied in the Boston pilot study, we find no effects in the interim evaluation.

Chapter Five
Impacts on Delinquency and Risky Behavior Among Youth

This chapter discusses reasons why MTO participation might be expected to affect the behavior of youth (ages 12 to 19) in the sample and then presents the interim findings on their delinquency and risk-taking behavior. To study delinquency we examined behavior problems at home and school, gun and gang involvement, property crimes and violent behavior, assaults, and arrests. To study risky behavior we looked at substance use and sexual activity. Such behavior may impose costs on society in several ways and by means of different mechanisms—through its implications for the youth involved, through its possible damaging effects on the social environment, and (in the case of crimes and violent behavior) through its direct effects on victims.

Summary

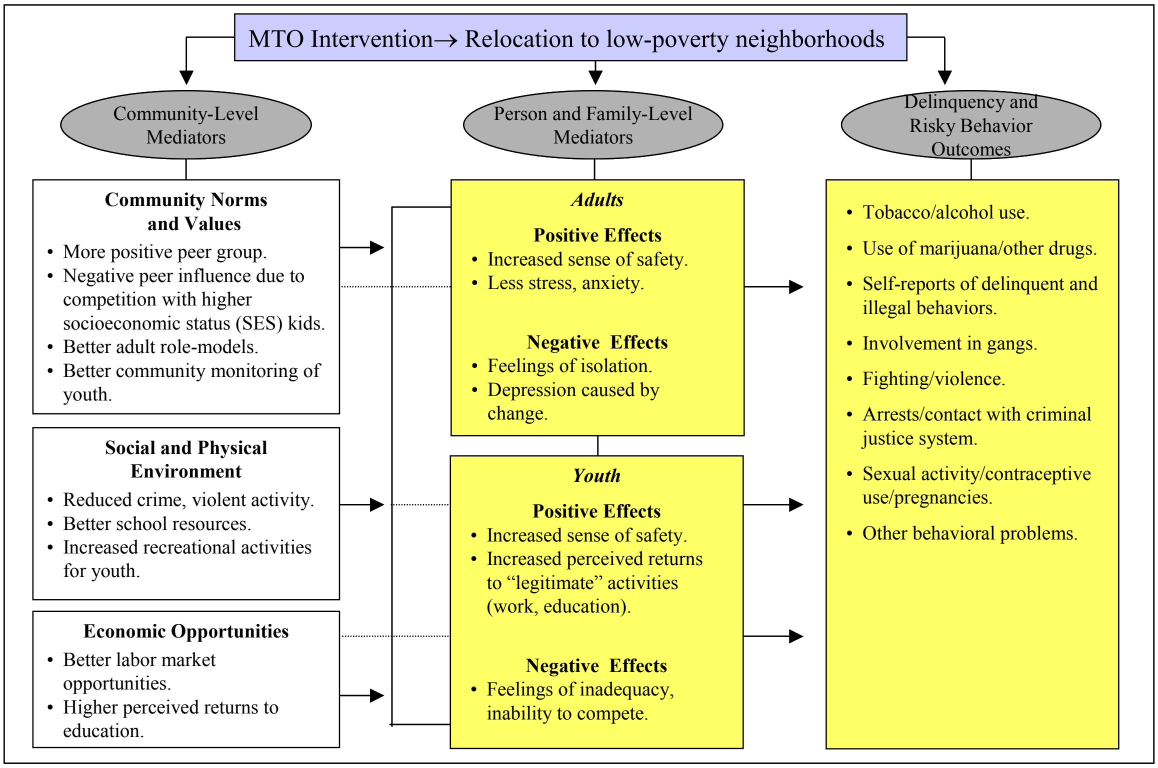
The pattern of results differs by treatment group and gender. For experimental group girls, there were significant effects of reduced risky behavior (of which marijuana use and smoking were significant components) compared to the control group. For Section 8 group girls, there were significant effects of a smaller fraction arrested (concentrated in violent crime arrests). For both experimental and Section 8 group boys, there were significant effects of more self-reported behavior problems and more smoking than in the control group. For experimental group boys, there were also significant effects of more property crime arrests than in the control group. Pooling boys and girls together, there was a significant effect of fewer violent crime arrests in the experimental group than in the control group during the first four years after random assignment. For girls and boys in both the experimental and Section 8 groups, there were no significant effects on other measures of behavior problems, delinquency, arrest, alcohol use, or sexual activity.

5.1 Hypotheses about Youth Delinquency and Risky Behavior in MTO

Conceptual framework

MTO may have important effects on behavior problems for youth and, to a lesser extent, adults. An extensive literature (summarized in Brock and Durlauf, 1999) posits theories that neighborhoods may affect social pathologies such as delinquency, substance use, and early childbearing. Jencks and Mayer (1990) highlight five types of models that have been proposed to describe the mechanisms through which neighborhoods may exert influence (see exhibit 5.1). We discuss each in turn.

EXHIBIT 5.1
DELINQUENCY AND RISKY BEHAVIOR OUTCOMES AND MEDIATING FACTORS



Peer influences through contagion effects. This model would predict that youth in the MTO treatment groups will display lower levels of those delinquent and risky behaviors for which high socioeconomic status (SES) youth have a lower prevalence than low-SES youth. This prediction follows from epidemic or contagion models which emphasize the power of peers to influence one another's behavior and assume "like begets like." These models imply that if children grow up in a neighborhood where their peers are more likely to commit crimes or drink too much, they are more likely to do these things themselves.

Relative deprivation or competition effects. Models of relative deprivation suggest that well-off neighbors may provoke resentment among those from poorer backgrounds so that poor youth are more likely to develop or fall into a deviant subculture when living in low-poverty neighborhoods. Academic competition with more affluent and better-prepared peers may cause some youth to work harder, but it may lead others to become frustrated and more likely to drop out of the competition and to engage in deviant behaviors. These models suggest youth in the experimental group may show higher levels of delinquent behaviors than youth in the control group.

Neighborhood adult influences. Collective socialization models posit that adults in a neighborhood may influence young people who are not their children. More affluent adults may act as role models who demonstrate that success is possible if you work hard and play by the rules. High-SES adults may act as enforcers who help maintain public order. In this model, youth in the MTO treatment groups may have lower social pathologies than control group members since treatment group members who lease up through MTO end up in neighborhoods containing a larger proportion of high-SES adults. Among the treatment groups, those who lease up in the experimental group, who must move to low-poverty areas, may show lower rates of antisocial behavior than those in the Section 8 group, who may move into higher poverty areas.

Community resources. More affluent neighborhoods are likely to offer better labor market opportunities for youth, greater school resources, and possibly a larger range of positive recreational and extracurricular activities. Enhanced community resources may increase the perceived returns to legitimate work, educational investments, and clean recreational activities relative to illegal activities and other delinquent behaviors. On the other hand, more affluent communities may also present more lucrative opportunities for theft. As a result this theory predicts that compared to the controls, the experimental group (and possibly the Section 8 group) may commit property crimes at a higher or lower rate but should be expected to have lower delinquency rates and higher rates of involvement in positive activities (such as work and schooling).

Neighborhood safety influence. Greater neighborhood safety (lower crime and violence rates) reduces the need to join gangs for protection and may thereby reduce delinquent behavior and increase positive activities for the MTO treatment groups.

Earlier research

The nonexperimental empirical literature reveals mixed results on the importance of these theoretical neighborhood mechanisms in affecting delinquency and risky behaviors. Case and Katz (1991) found a strong relationship between one's own illegal drug use and that of one's peers, and also some relationship between own and peer 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 of 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. Sampson, Raudenbush, and Earls (1997) argue that collective efficacy (social cohesion among neighbors combined with willingness to intervene on behalf of the common good) is linked to lower levels of violence within neighborhoods.

Preliminary short-term evidence from the MTO sites showed that boys in both the experimental and Section 8 groups exhibited fewer behavior problems (disobedience, bullying, depression) than those in the control group in Boston (Katz, Kling, and Liebman, 2001) and in New York (Leventhal and Brooks-Gunn 2003). Evidence from MTO in Baltimore suggested that the experimental group males had fewer arrests for violent crimes but a short-term increase in arrests for property crime (Ludwig, Duncan, and Hirschfield 2001) relative to the control group.

5.2 Data Sources and Measures

Data sources

All of the delinquency and risky behavior outcomes described here, except arrests and contacts with the criminal justice system, were measured with data from the interim surveys with youth or their parents. For data on arrests and contacts with the criminal justice system, we obtained administrative data from California, Illinois, Maryland, Massachusetts, and New York.[74] Mediating factors of time use, social interactions, church attendance, and the behaviors and attitudes of neighborhood and school peers were measured with interim survey data. Neighborhood characteristics were derived from census data.

Measures

Most of the relevant survey questions came directly from the National Longitudinal Survey of Youth (NLSY97). 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 delinquency and behavior problems (see Moore et al. 1999). To aggregate information on specific outcomes in order to examine broader patterns, we used several indices that represent the fraction of the behaviors engaged in by the youth.

- Abbreviated behavior problems index: fraction of 11 behavior problems reported by sample adult and youth, to be often or sometimes true of a youth: 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 9 delinquent behaviors that the youth reported 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, selling drugs, and being arrested.

- 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. The main analyses of arrests also focused on having ever been arrested. This reference period has the advantage of encompassing events throughout the entire period since random assignment. Most youth

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74 Arrest histories for adults and juveniles came from state criminal justice agencies in California, Illinois, Maryland, and Massachusetts. For New York, state-level criminal justice data were used to capture arrests anywhere in the state for ages 16 and up (as well as any arrests before age 16 that were prosecuted in adult rather than juvenile courts). For additional information about arrests before age 16, we obtained juvenile records from the New York City Department of Probation.

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. The use of "ever" as the reference period was also used as an approximation to the concept of "in the (4 to 7) years since random assignment" for asking questions of youth during the survey data collection, where it had the advantage of simplicity and of eliminating error due to erroneous recall of whether the event occurred before or after random assignment. For consistency, in analyses of administrative data, the same concept of "ever arrested" was used, and pre-random assignment arrests were explicitly controlled for in the analysis. Other reference periods were also explored in the analysis.

5.3 Context and Baseline Status of the Sample

The MTO program was initiated in 1994 near the end of a period in which youth violence had increased dramatically. For example, from 1985 to 1993 the rate at which teenagers were arrested for murder more than doubled. Most of this increase was driven by gun homicides, committed disproportionately by and against minority youth (Cook and Laub, 1998, 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 rates at which teens were arrested for both violent and property offenses crested nationwide during the mid-1990's and have since declined by approximately one-third (Cook and Laub, 2002). The source of this decline in youth crime remains unclear, although candidate explanations include changes in the nature of crack use or distribution, increases in the nation's prison population, changing demographics, and a booming economy (Blumstein and Wallman, 2000, Cook and Laub, 2002, Donohue and Levitt, 2001). Whatever the cause, the arrest rates for MTO teens generally mirror the pattern found in national data. For example, the number of arrests of 16-year-olds declined by around one-third from 1997 to 2001.[75] The level of arrests is substantially higher in the MTO population than in a national sample, consistent with the disadvantaged family and community circumstances in which most MTO youth grew up.[76]

This chapter focuses mainly on youth ages 12 to 19 on May 31, 2001, who entered the MTO demonstration 4 to 7 years earlier. Thus, the ages of this group at baseline ranged from 5 to 16. At that time, parents of all children ages 6 to 17 were asked a series of questions about the children's behavior. These measures show large baseline differences between girls and boys. Two are most

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[75] For this calculation the sample is defined as being youth in any of the three MTO groups who were age 16 for at least one-quarter of each calendar year.

[76] Since categories used in national data differ somewhat from the juvenile arrest data available for MTO participants, we have compared arrests for robbery where the definition is the most consistent. Robbery arrest rates for MTO youth ages 15 to 19 are roughly five times higher than the national average reported by Zimring (1998).

relevant for this chapter: problem behavior (going to a special class or school or having received special help in school during the past 2 years for behavioral or emotional problems) and suspension (suspended or expelled from school during the past 2 years). At baseline, 6 percent of the girls in the sample had exhibited problem behavior and 7 percent had been suspended. Fourteen percent of the boys had exhibited problem behavior and 15 percent had been suspended.

5.4 Effects on Mediators for Youth Delinquency and Risky Behavior in MTO

This section reviews effects on the mediating factors most directly relevant to the hypotheses described in Section 5.2. The tables containing these results are found in appendix E.

Peer influences

We analyzed whether the program changed the characteristics of sample members' peers. In the qualitative research that preceded the interim evaluation survey, youth respondents spoke about a difference in peer influences. A 15-year-old boy from a Los Angeles Section 8 family that had leased up said (referring to the public housing development where he had lived):

> *At Nickerson, it was cool. But now when I go to visit over there, I probably would have ended up in a gang or smoking or something. So I'm glad that I moved from over there.*

There were no significant effects for either the experimental or Section 8 group on having friends involved in school activities or on friends carrying weapons to school (exhibit E5.1). There were significant effects for both the experimental and Section 8 groups relative to the control group of having more friends who use drugs, largely concentrated among boys (exhibit E5.1), and for the experimental group of having a higher fraction with at least one close friend (exhibit E5.2). Note that while there was some visiting of baseline neighborhoods by those whose families leased up through MTO and moved out of the origin neighborhood, the fraction either living in or making visits to the baseline neighborhood was much higher in the control group than in the experimental or Section 8 groups. The exposure of the control group youth to the baseline neighborhoods was much greater than the experimental or Section 8 groups (exhibit E5.2).

Characteristics of families in the neighborhood

We also analyzed census data about nearby residents (discussed in depth in chapter 2), which is relevant for both the relative deprivation and neighborhood adult influence hypotheses. The youth interviewed for the qualitative study were very aware of the differences between the families in their new neighborhoods and the neighbors in their old ones. Speaking about the public housing development from which his experimental group family had moved, a 16-year-old Baltimore youth described the contrast:

> *I'd say it's overall better than back in Murphy Homes. People at Murphy Homes would rather steal a car than to buy one. Out here, everyone's just working, has a job.*

Briefly, assignment to the experimental and Section 8 groups had positive effects on the fraction of families in the neighborhood with two parents, with an employed adult, and with incomes above the poverty line. The interim survey also collected reports from sample adults about neighborhood collective efficacy, which is among the measures of adult characteristics most specifically related to delinquency and risky behavior. There were significant effects for both the experimental and Section 8 groups of greater perception that neighbors would likely do something about kids doing graffiti and about kids skipping school in the neighborhood (exhibit E5.3).

Community resources

We analyzed employment possibilities and participation in institutions outside school. There were positive effects for the experimental and Section 8 groups on local area unemployment rates (exhibit D2.1). Contrary to a hypothesis that participation in community activities would decrease for experimental and Section 8 youth, there was no impact on attendance at church youth activities. For the most part, there were no effects on participation in structured or supervised activities after school—with the exception of experimental group girls, who were more likely to be in a structured activity at 5:30 p.m. in places such as school, church, or a community center (exhibit E5.4).

Neighborhood safety

We analyzed a variety of measures of exposure to violence (exhibit E5.5). Youth interviewed for the qualitative research early in this study spoke at length about safety. A 14-year-old girl from a Chicago experimental group family that had leased up was reflective about the trade-off:

> *Like, OK, you can wake up every day and we're not worried about seeing anybody getting shot and no gang members, nothing like that and it's quiet and it's cool and calm up here. In the city there's a lot of activities that's going on that's negative. Here there's a lot of positive. Yeah, the only thing is, it's like too quiet out here. Um, it's boring but it's good that I'm safe, rather be bored than unsafe.*

For measures that specifically asked about the neighborhood, such as hearing gunshots in the neighborhood at least once in the past week, there were significant improvements for both the experimental and Section 8 groups relative to the control group. There was also a significantly lower fraction in the experimental group in the proportion of youth seeing people using or selling drugs in the neighborhood than in the control group. For measures of overall exposure to violence not specific to the neighborhood—including prevalence of gangs, witnessing of stabbings or shootings, or having a knife or gun pulled on the youth—there were no significant effects for either group (with the exception of a significant decrease in gang prevalence for the experimental group).

5.5 Interim Impacts on Youth Delinquency and Risky Behavior

This section discusses the main results for delinquency and risky behavior outcomes. It begins by examining behavior problems and delinquent acts that are prevalent among all youths in the sample ages 12 to 19. The most serious of these acts may result in arrests. For this outcome, we focused on the age range in which arrests are most prevalent, 15 to 19, since including ages for which arrests are uncommon dampens the power of our analyses. Finally, we examined program effects on risky behavior (substance use and sexual activity), which are also most prevalent among ages 15 to 19.

Behavior problems and delinquency

As described above in the section on measures, sample adults reported the fraction of 11 behavior problems that their child might have. There were no significant effects on behavior problems as reported by parents for boys or girls in either the experimental or the Section 8 group (exhibit 5.2).

These results contrast with the earlier findings at the Boston and New York sites after 2 years, discussed above, that behavior problems for boys were significantly less prevalent in the experimental and Section 8 groups than the control group.[77] In fact, in the present analysis, there were significantly more self-reported behavior problems for boys ages 12 to 19 in the experimental and Section 8 groups than in the control group.[78]

We also examined impacts on the outcomes shown in exhibit 5.2, with separate estimates by both gender and baseline problem behavior status (not shown in the exhibits). For the experimental group, effects of more behavior problems relative to the control group do appear to be concentrated among those who had problem behaviors at baseline, particularly among boys.[79]

While the behavior problems index covers a broad array of troubles that many children have, the delinquency index focuses more narrowly on issues that tend to be related to illegal activity. For this outcome, there were no significant effects for either group or gender.

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[77] Analysis of results specifically for Boston and for New York, using the same age groups and measures (when available) as in previous research, did not show statistically significant effects on behavior problems 4 to 7 years after random assignment. Analysis based on the same children in Boston having data from both 1997 and 2002 showed a significant effect of more behavior problems in the experimental group relative to the control group in 2002.

[78] Although not the age group focused on in this chapter, boys ages 5 to 11 also had a significantly higher fraction of behavior problems in the experimental group than in the control group (exhibit D5.1) according to parental reports.

[79] In addition to baseline behavior problems and gender, we examined interactions with mother's education and child gender, with family size and child gender, with race and child gender, and with younger age at time of random assignment and child gender—but found no consistent pattern of results for behavior problems or for other delinquency outcomes.

EXHIBIT 5.2
BEHAVIOR PROBLEMS & DELINQUENCY OUTCOMES, YOUTH AGES 12 TO 19

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Abbreviated Behavior Problems Index [PR] (n=2770)					
All youth (ages 12 to 19)	0.256	0.019 (0.014)	0.042 (0.032)	0.004 (0.015)	0.006 (0.026)
Female	0.230	0.005 (0.019)	0.011 (0.041)	-0.002 (0.021)	-0.003 (0.034)
Male	0.282	0.032 (0.020)	0.074 (0.046)	0.009 (0.021)	0.016 (0.038)
Abbreviated Behavior Problems Index [SR] (n=2810)					
All youth (ages 12 to 19)	0.343	0.036* (0.014)	0.080* (0.032)	0.023 (0.016)	0.039 (0.028)
Female	0.352	-0.002 (0.019)	-0.004 (0.042)	-0.007 (0.021)	-0.012 (0.035)
Male	0.336	0.075* (0.020)	0.169* (0.045)	0.052* (0.022)	0.095* (0.040)
Delinquency Index [SR] (n=2819)					
All youth (ages 12 to 19)	0.089	0.004 (0.008)	0.008 (0.017)	0.007 (0.009)	0.012 (0.016)
Female	0.061	0.001 (0.008)	0.001 (0.018)	0.001 (0.010)	0.001 (0.016)
Male	0.118	0.006 (0.013)	0.015 (0.029)	0.013 (0.015)	0.024 (0.027)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult and Youth surveys.
Sample: Children ages 12 to 19 as of May 31, 2001, from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

Arrests

The peak age of arrest in the MTO population is 18, and the prevalence of arrest at age 18 is more than three times higher than at age 13. To focus on the prime offending ages where the statistical power of our analysis is greatest, our analysis focused on ages 15 to 19.[80] Exhibit 5.3 shows results from both the interim survey and administrative records on whether youth ages 15 to 19 in 2001 had ever been arrested. For all types of crimes taken together, there was no significant effect on the fraction of either boys or girls ever arrested in the self-reported survey data. In the administrative data there was no significant effect for boys, but there was a significant effect of a smaller proportion arrested among girls in the Section 8 group than the control group.

Regarding other results in exhibit 5.3, there was a large and significant effect of a smaller proportion of girls ever arrested for violent crimes (which were largely assaults) in the Section 8 group. There was also a significantly greater proportion of boys who had ever been arrested for property crimes in the experimental group than in the control group. Other effects for violent and property crime arrests were insignificant, and there were no significant effects for other (non-violent non-property) crimes in any group.

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[80] Although not the focus of this chapter, we also analyzed arrest outcomes of sample adults. As seen in exhibit D5.1, for adults there are no statistically significant differences across groups in the likelihood of ever having been arrested for any offense, for property crimes, or for violent crimes (p-value .054). There was a significant effect of a smaller proportion arrested for other offenses (nonviolent nonproperty, including drugs) in the experimental group relative to the control group. The sample adults are mainly women, and these results parallel the evidence of beneficial program effects for older female youth.

EXHIBIT 5.3
ARREST OUTCOMES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Ever Arrested [SR] (n=1574)					
All youth (ages 15 to 19)	0.224	-0.004	-0.008	0.001	0.002
		(0.028)	(0.064)	(0.032)	(0.057)
Female	0.140	-0.022	-0.048	-0.023	-0.039
		(0.032)	(0.070)	(0.035)	(0.059)
Male	0.311	0.016	0.039	0.027	0.054
		(0.044)	(0.109)	(0.052)	(0.103)
Ever Arrested [ADMIN] (n=2646)					
All youth (Ages 15-19)	0.313	0.018	0.043	-0.020	-0.036
		(0.022)	(0.050)	(0.024)	(0.042)
Female	0.238	-0.022	-0.051	-0.063*	-0.112*
		(0.028)	(0.065)	(0.029)	(0.052)
Male	0.388	0.058	0.135	0.021	0.038
		(0.031)	(0.072)	(0.035)	(0.061)
Ever Arrested for Violent Crime [ADMIN] (n=2646)					
All youth (Ages 15-19)	0.174	-0.010	-0.024	-0.035	-0.063
		(0.017)	(0.040)	(0.020)	(0.035)
Female	0.144	-0.040	-0.093	-0.088*	-0.155*
		(0.022)	(0.051)	(0.022)	(0.039)
Male	0.204	0.019	0.045	0.015	0.026
		(0.026)	(0.060)	(0.030)	(0.053)
Ever Arrested for Property Crime [ADMIN] (n=2646)					
All youth (Ages 15-19)	0.118	0.022	0.051	0.014	0.026
		(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)
Ever Arrested for Non-Violent, Non-Property Crime [ADMIN] (n=2646)					
All youth (Ages 15-19)	0.168	0.003	0.007	0.0004	0.001
		(0.017)	(0.039)	(0.019)	(0.033)
Female	0.097	-0.034	-0.080	-0.019	-0.033
		(0.018)	(0.041)	(0.020)	(0.035)
Male	0.240	0.040	0.094	0.019	0.034
		(0.027)	(0.064)	(0.030)	(0.053)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Youth survey data and individual criminal justice system arrest data.
Sample: Children ages 15 to 19 as of May 31, 2001, from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

Exhibit 5.4 shows results for the proportion of calendar quarters with at least one arrest during the first four years after random assignment, for the cohort of youth who were ages 15 to 19 four years after random assignment. (That is, they were ages 11 to 15 at baseline.) This measure complements the measure of ever arrested in exhibit 5.3 by capturing the frequency of arrest. This is also essentially the same measure analyzed in previous research on MTO's Baltimore site by Ludwig et al. (2001). For all types of crimes pooled together, there were no significant effects on the proportion of calendar quarters with an arrest during the first four years after random assignment.

We found an effect of significantly less frequent violent crime arrests in the experimental group relative to the control group when pooling boys and girls together, although results separately for boys (p-value .07) and for girls were not significant. Further analysis (not shown in the exhibits) of the smaller groups of these youth for whom we have data at least 6 years after random assignment showed similar effects in years 1-4 but no significant effects for either gender on violent crime in year 6; although imprecise, these results may suggest that the relatively lower rate of violent crime arrests for the experimental group did not persist. The effect on Section 8 girls using the measure of proportion of quarters arrests goes in the same direction as the results for fraction ever arrested, but it was not significant.[81]

The results showed significant effects of more frequent property crime arrests for boys in the experimental group than in the control group during the first four years after random assignment. Further analysis (also not shown in the exhibits) of the smaller groups of these youth for which we have data at least 6 years after random assignment showed similar effects in years 1-4 but no significant effects for either gender or group on property crime in year 6; again although imprecise, these results may suggest that the relatively higher rate of property crime arrests for experimental group boys did not persist.

Previous research on MTO's Baltimore site (Ludwig et al. 2001) found that the most significant effect was a smaller proportion arrested for violent crime (concentrated among boys) in the experimental group, and also found some effects of increased property crime arrests for experimental group boys relative to the control group during the period shortly after random assignment. The analysis of data for all five sites generally echoes this previous research, with less frequent violent crime arrests and more frequent property crime arrests in the experimental group than in the control group. However, in the five-site analysis, the higher property crime arrest rate for the experimental group relative to the control group was not particularly concentrated 1 to 2 years after random assignment (not shown in the exhibits).

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[81] When the analyses in exhibits 5.3 and 5.4 use the same sample (restricted to youth ages 15 to 19 on May 31, 2001 and ages 15 to 19 four years after random assignment) then there is a significant effect of fewer violent crime arrests for Section 8 group girls than for the control group using both measures.

EXHIBIT 5.4
Fraction of Calendar Quarters Youth Had Any Arrest 1-4 years Since Random Assignment for Ages 15 to 19 Four Years After Random Assignment

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
All Crime 1st Through 4th Years After Random Assignment [ADMIN] (n=2532)					
All youth	0.0364	0.0006	0.0013	0.0007	0.0012
		(0.0036)	(0.0087)	(0.0040)	(0.0072)
Female	0.0191	-0.0036	-0.0085	-0.0003	-0.0005
		(0.0035)	(0.0085)	(0.0042)	(0.0076)
Male	0.0533	0.0048	0.0115	0.0019	0.0034
		(0.0061)	(0.0146)	(0.0063)	(0.0115)
Violent Crime 1st Through 4th Years After Random Assignment [ADMIN] (n=2532)					
All youth	0.0138	-0.0038*	-0.0091*	-0.0010	-0.0019
		(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)
Property Crime 1st Through 4th Years After Random Assignment [ADMIN] (n=2532)					
All youth	0.0098	0.0021	0.0050	0.0007	0.0012
		(0.0016)	(0.0039)	(0.0017)	(0.0031)
Female	0.0055	-0.0011	-0.0027	0.0016	0.0028
		(0.0015)	(0.0036)	(0.0021)	(0.0038)
Male	0.0139	0.0054*	0.0130*	0.0001	0.0002
		(0.0027)	(0.0064)	(0.0025)	(0.0045)
Non-Violent, Non-Property Crime 1st Through 4th Years After Random Assignment [ADMIN] (n=2532)					
All youth	0.0140	0.0017	0.0042	0.0015	0.0027
		(0.0022)	(0.0052)	(0.0022)	(0.0041)
Female	0.0051	-0.0011	-0.0025	0.0007	0.0013
		(0.0020)	(0.0048)	(0.0019)	(0.0031)
Male	0.0227	0.0046	0.0111	0.0025	0.0045
		(0.0038)	(0.0090)	(0.0039)	(0.0072)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Individual criminal justice system arrest data.
Sample: Children ages 15 to 19 four years after random assignment.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

The overall pattern of results by crime type described above was also generally shown in analysis on the outcome of number of arrests (not shown in the exhibits)—complementing that shown on the prevalence of arrest. There were no significant effects on ever being arrested for other (nonviolent, nonproperty) crimes. Focusing on arrests in year 2001 for youth ages 15 to 19 as of May 31, 2001 (not shown in the exhibits), there were no significant effects on self-reported arrests or in administrative records of violent, property, or other arrests for either group, except for a lower fraction of violent crime arrests for girls in the Section 8 group than in the control group.

Risky behavior

Results in exhibit 5.5 show that a significantly smaller fraction of the experimental group girls than control group girls engaged in four risky behaviors. The control group mean was about .43, indicating that the typical youth had engaged in almost one-half of the four risky behaviors. The estimated magnitude of the effect for girls in families who leased up in the experimental group was -.16.

Among the components of the risky behavior index, also shown in exhibit 5.5, there were significant effects of less smoking and marijuana use for girls in the experimental group than in the control group. There were significant effects of more smoking for boys in both the experimental and Section 8 groups than in the control group. The effects on smoking for boys were very large in magnitude, with an estimated effect for boys who leased up (in either the experimental or Section 8 group) of over .30 in the proportion who ever smoked, with a control mean of .26. The effects on use of marijuana, on smoking, and on alcohol use in the past month (not shown in the exhibits) had a general pattern of less use for girls and more use for boys in the experimental and Section 8 groups relative to the control group, with most of these estimates being statistically significant for both genders. The gender pattern of effects on sexual activity is more mixed, and the estimates were not statistically significant.

EXHIBIT 5.5
RISKY BEHAVIOR AND DRUG OUTCOMES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Risky Behavior Index [SR] (n=1588)					
All youth (ages 15 to 19)	0.439	-0.008 (0.023)	-0.019 (0.053)	0.016 (0.025)	0.030 (0.047)
Female	0.431	-0.072* (0.031)	-0.156* (0.067)	-0.016 (0.034)	-0.027 (0.057)
Male	0.447	0.057 (0.030)	0.142 (0.075)	0.050 (0.034)	0.100 (0.070)
Ever Used Marijuana [SR] (n=1569)					
All youth (ages 15 to 19)	0.344	-0.031 (0.034)	-0.071 (0.079)	-0.020 (0.038)	-0.036 (0.069)
Female	0.342	-0.129* (0.044)	-0.276* (0.095)	-0.079 (0.050)	-0.132 (0.085)
Male	0.348	0.068 (0.049)	0.169 (0.122)	0.042 (0.052)	0.083 (0.104)
Ever Had Alcoholic Drink [SR] (n=1582)					
All youth (ages 15 to 19)	0.421	-0.024 (0.035)	-0.054 (0.081)	-0.005 (0.038)	-0.008 (0.070)
Female	0.410	-0.072 (0.049)	-0.155 (0.107)	-0.003 (0.052)	-0.005 (0.088)
Male	0.432	0.025 (0.049)	0.062 (0.121)	-0.007 (0.054)	-0.013 (0.109)
Ever Smoked [SR] (n=1583)					
All youth (ages 15 to 19)	0.290	0.019 (0.032)	0.044 (0.074)	0.067 (0.035)	0.122 (0.065)
Female	0.314	-0.085* (0.043)	-0.184* (0.093)	-0.015 (0.046)	-0.024 (0.077)
Male	0.264	0.125* (0.045)	0.314* (0.113)	0.152* (0.051)	0.305* (0.102)
Ever Had Sex [SR] (n=1548)					
All youth (ages 15 to 19)	0.717	-0.009 (0.029)	-0.021 (0.068)	0.014 (0.032)	0.025 (0.058)
Female	0.667	-0.006 (0.041)	-0.013 (0.090)	0.033 (0.043)	0.055 (0.073)
Male	0.769	-0.013 (0.040)	-0.032 (0.099)	-0.007 (0.046)	-0.014 (0.091)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Youth survey.
Sample: Children ages 15 to 19 as of May 31, 2001, from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

5.6 Interpretation of Results

Overall, for most measures of environmental context, the experimental and Section 8 groups were at least as well off and often more well off than the control group—with similar effects for boys and girls. The magnitudes of many effects were quite large. For gunshots in the past week, as an example, the control group mean was .12, and the estimated reduction for those who leased up in the Section 8 group was .06. For neighbors doing something about graffiti, the control group mean was .54 and the estimated effect for those who leased up in the experimental group was .24. For all the hypotheses except relative deprivation, improvement in environmental context predicted improvements in individual outcomes.

The deleterious effects on outcomes such as behavior problems and smoking for boys were unexpected. A hypothesis to be investigated further is the relative deprivation hypothesis discussed in section 5.2. However, most aspects of the experimental and Section 8 group effects on social environment—such as the reduction in exposure to violence and greater neighborhood collective efficacy discussed in section 5.3—were predicted to have the same effect on boys and girls. Other mediators that could have had different effects by gender, such as social adjustment, number of friends, and contact with fathers or other adults, did not generally have program effects significantly different from zero for either gender or that differed significantly between boys and girls. Experimental group girls were significantly more likely to be in a structured activity after school, but the effect on boys was not significant. Understanding the differential effects by gender will require further research.

At the beginning of this chapter we noted that delinquency and risky behavior may impose costs on society—through their implications for the youth involved, through possible damaging effects on the social environment, and in the case of crimes and violent behavior, through direct effects on victims. The direct effects on youth themselves—of increases or decreases in substance use or smoking—are relatively straightforward. The net effect on the social environment of increases in smoking for boys and decreases for girls are partially offsetting. The increases in behavior problems and property crime arrests for boys are social costs in that they spill over to others, while reductions in drug use for experimental group girls and in violent crime arrests for some Section 8 group girls are social benefits as well as direct benefits for these youth.

Chapter Six
Impacts on Children's Education

One expectation for the MTO demonstration was that moving children to better neighborhoods would provide access to better educational opportunities in the form of better schools and increased exposure to peers and communities that value academic achievement and would, in turn, lead to improved educational outcomes. This chapter summarizes the reason why MTO participation might be expected to affect the educational outcomes of the children in the interim evaluation sample, describes the educational experiences of the MTO children, and presents the interim findings on educational impacts.

Summary

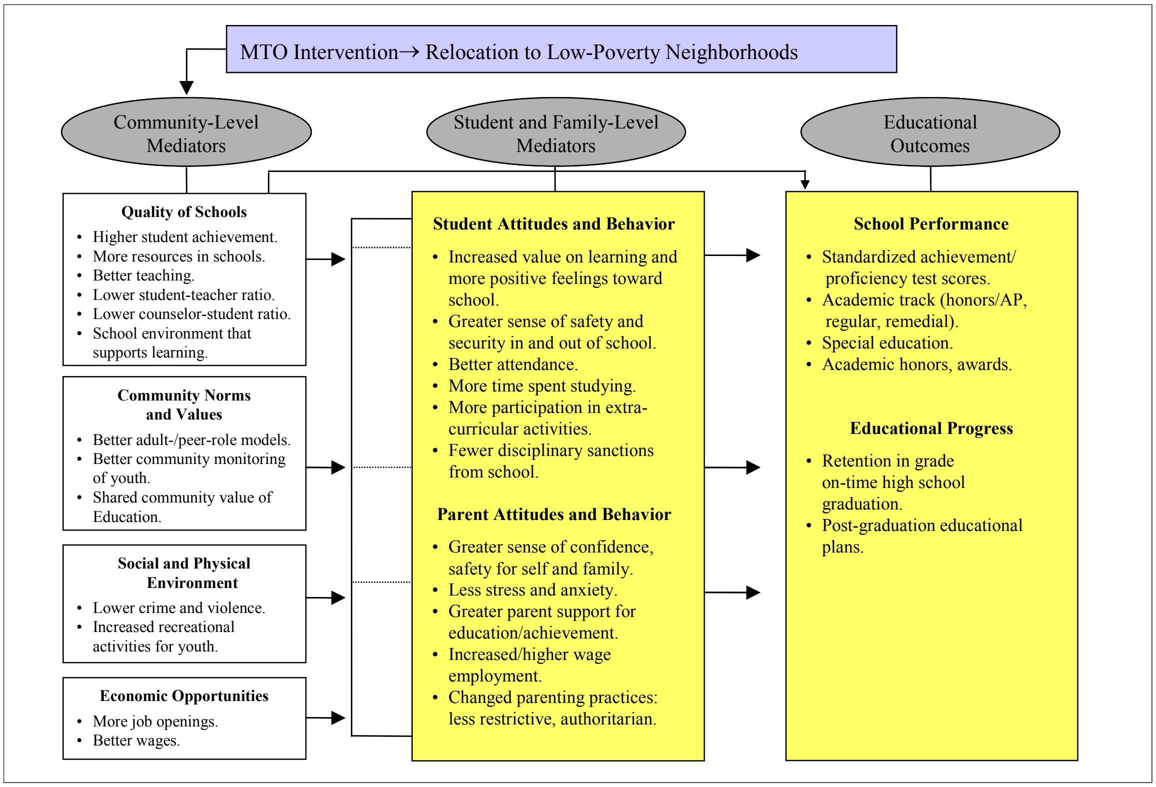
At the time of the interim evaluation, approximately 80 percent of the children in the experimental group were attending schools in the same school district as they had been at baseline. Even so, the evidence suggests that MTO had a small but positive effect on the characteristics of children's schools. There were also significant impacts on some of the other community-level variables hypothesized to mediate educational outcomes. At this point in the demonstration, however, these positive effects on the mediators of educational achievement have not had significant impacts on education-related behaviors or attitudes of MTO children or on their school achievement and educational progress.

6.1 Hypotheses about Education in MTO

There are a number of pathways by which the MTO demonstration might influence educational outcomes for the children of MTO families who move to more affluent neighborhoods. A great deal of research has demonstrated the positive relationship between neighborhood socioeconomic status and educational outcomes. In a series of studies, Brooks-Gunn and colleagues have shown that—controlling for family background characteristics—affluent neighbors are positively related to young children's IQ and verbal ability scores (e.g., Brooks-Gunn et al., 1993; Duncan et al., 1994; Klebanov et al., 1997), higher reading recognition (Chase-Lansdale et al., 1997) and math achievement (Entwistle, Alexander, and Olson, 1994), adolescent school completion, educational attainment, and self-reported grades (Brooks-Gunn et al., 1993; Connell and Halpern-Felsher, 1997; Dornbusch, Ritter, and Steinberg, 1991; Duncan, 1994). Similarly, studies of the Gautreaux housing mobility program in Chicago found lower dropout rates and an increased likelihood of attending college among children whose families relocated from public housing to suburban neighborhoods (Rosenbaum, 1995).

This research has pointed to a number of potential mediators of the relationship between neighborhood and educational outcomes. Exhibit 6.1 presents a conceptualization of these pathways. As shown in the exhibit 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, all four of these community characteristics are hypothesized to affect educational outcomes through changes in the behavior and attitudes of both students and their families.

EXHIBIT 6.1
EDUCATIONAL OUTCOMES AND MEDITATING FACTORS



Community characteristics linked to student educational outcomes

The first and most direct mediator of educational outcomes is school quality. Higher income neighborhoods have been shown to 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 hypothesized to increase students' commitment to academic achievement and to promote behaviors that produce achievement—studying, attendance, and engagement in school. All of the children in the sample for the interim evaluation were of school age during the period leading up to the evaluation, and therefore it is reasonable to expect that differences in school quality could be an important influence on educational outcomes of these students.[82]

Conversely, it is possible that relocating families in more affluent neighborhoods and sending children to new schools could have a negative effect on school achievement for MTO children. This is due to increased competition for grades and academic success in more affluent schools and because MTO students might develop low self-confidence in the midst of more affluent, higher achieving peer group. Indeed, studies of the Gautreaux housing mobility program in Chicago found that students whose families moved to the suburbs initially had difficulties adapting to the higher expectations in the suburban schools and their grades suffered as a result (Rosenbaum, 1995).

A second characteristic associated with the affluence of the community is the extent to which the community socializes its youth towards achievement, learning, and productive social behavior (i.e., whether and how neighborhoods provide individual and communitywide support and motivation for achievement). Both adults and peers in the community play a role in this socialization. The presence or absence of adult role models who provide examples of the advantages of being well-educated and employed is one way in which a community supports the values of educational achievement. More affluent neighborhoods tend to have higher rates of professional and managerial employment, and adults with these jobs act as positive role models for educational completion, academic success, and career-mindedness. In support of this hypothesis, Crane (1991) found that after controlling for the individual background characteristics of youth, a higher percentage of professional/ managerial workers in a neighborhood was associated with lower dropout rates for youth. As shown in exhibit 6.1, it is hypothesized that youth growing up in communities that have adult role models for, and consistent valuing of, education and achievement will themselves have values and expectations more consistent with educational achievement and learning, which will ultimately lead to improved educational outcomes.

Peer groups can also be a factor in either promoting or devaluing academic achievement. Youth in more affluent communities may be more likely to value education and achievement, and they may be more supportive of staying in school, doing well in school, and getting involved in school. If children of MTO families who relocate to more affluent neighborhoods develop new peer groups with values and behaviors more conducive to high educational achievement, then they may adopt more positive attitudes towards school and academic achievement and their own educational performance may be positively affected.

A third community characteristic in the model is the physical environment itself. More affluent communities are likely to be safer, which may have consequences for the attitudes and mental health of both parents and youths. Parents who believe that their children are safe may feel less stress and anxiety, resulting in improved mental health and sense of control over their lives. This improved mental health status may then lead to different parenting behaviors. McLoyd (1990) has posited that restrictive and authoritarian patterns of parenting, which are more often exhibited by low-income parents and which have been shown to be associated with poorer educational outcomes for children,

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82 It is possible that some children, even if their families moved, did not change schools. Certainly the qualitative data point to this. Other children will have changed schools more than once. School continuity—or lack of continuity—may well have its own effects.

are themselves linked to the mental health stresses associated with poverty. In addition to the effect of safe communities on parents and parenting behavior, children who feel safe in their physical surroundings may be more likely to flourish academically and personally.

A fourth characteristic of more affluent communities is increased economic opportunities for the adults in that community. This not only includes more job opportunities, but also better paying jobs. If these opportunities result in improved economic circumstances for MTO families who move out of low-poverty neighborhoods, there will be more resources in the family. Greater family resources can be linked to improved educational outcomes for children in the family, if the resources translate into more materials in the home that support educational achievement (books, creative materials, educational media). Increased economic self-sufficiency can also affect educational outcomes for children if parental employment leads to increased familial support for achievement in general.

Student and parent attitudes and behavior linked to educational outcomes

Exhibit 6.1 shows the links between community characteristics and the behavior and attitudes of students and their parents. These behaviors are in turn linked to student educational outcomes. For students, attitudes in four areas are thought to be related to levels of educational performance. First are students' beliefs about themselves: expectations for their own educational achievement, how much they value education and achievement in life, and their belief in their own abilities and chances for educational success. Second are attitudes about school: how strongly they feel connected to school, whether or not they feel teachers in the school care about them, and how they do in school. Third are their attitudes about their own family: whether and how much they think their parents care about school. Fourth are attitudes about their peer group: how they think their friends feel about school and about doing well in school (Ryan, 2001; Cairns et al., 1989; French et al., 2001; Murdock et al., 2000)

Student behavior is also an important link between the community and educational outcomes. School-related behavior includes the amount of time spent studying, rigor of course work taken, and engagement in school—both participation in school activities such as sports and clubs and evidence of attendance or disciplinary problems.

Parental attitudes that have been thought to mediate student achievement include educational expectations, emphasis on educational achievement, and support for the school. Parental behavior includes active involvement in the school (teacher conferences, parent-teacher organizations, etc.), support for homework completion, and parenting practices that encourage students to think about and evaluate their own actions and consequences.

Student educational outcomes

Student educational outcomes are the final outcomes of interest. As shown in exhibit 6.1, the two major components of educational outcomes are academic achievement, as measured by standardized test scores; and overall progress through school—promotion or retention in grade, ontime high school graduation, and post-high school graduation educational plans.

6.2 Data Sources and Measures

To understand the mechanisms through which residential location affected educational performance, we estimated impacts on a variety of community and school characteristics as well as student and parent attitudes and behaviors. Community-level data on crime and economic health were drawn from published statistics at the state level and from Census 2000 data. Data on the socialization patterns in the community (adult and peer) were obtained through interviews with the adults in the MTO families. Data on school characteristics were obtained from multiple sources, including parent and youth reports and administrative data from both state and national records. The administrative sources included (a) school-level administrative data from each of the five MTO states (Illinois, New York, California, Massachusetts, and Maryland) collected from state Web sites and state departments of education; (b) the National School-Level State Assessment Score Database for student test scores; (c) the National Center for Education Statistics (NCES) Public School Elementary and Secondary School Universe Survey Data Annual Files (the Common Core of Data (CCD))[83]; and (d) the 1999-2000 Private School Survey conducted by NCES.[84] The schools attended by the MTO children from baseline until the time of the interim data collection were identified from detailed school histories collected from parents in the adult survey.

Finally, parent and child attitudes and behaviors were measured through interviews with parents and all children age 8 to 19 in the sample. In addition, for all children ages 5 to 7 we collected data on student behavior and attitudes through interviews with parents (see appendix A).

The data used to measure the impact of MTO on educational outcomes came from two primary sources. Survey data were collected from children, youth, and adults (appendix A). From surveys we obtained information about grades, coursework taken, grade retention, high school completion, and college attendance. All children ages 5 to 19 were administered four achievement subtests from the Woodcock-Johnson Battery-Revised (WJ-R). The WJ-R is a set of individually administered tests for measuring cognitive ability and achievement. The wide age range and breadth of coverage of the tests make it possible to use the same set of tests with children of all age levels. The WJ-R tests were standardized on a nationally representative group of 6,359 subjects, ages 24 months to 95 years. The tests are recognized as the premier battery for measuring both the cognitive abilities and school achievement of school-aged children and young adults.

The WJ-R subtests administered included letter-word identification, passage comprehension, math calculation, and applied problems. Each subtest produces a total score. Two composites scores can also be computed: broad reading (which is an average of the child's letter-word identification and passage comprehension scores) and broad math (which is an average of the math calculation and applied problems scores). In addition, one child under the age of 11 from each family was administered the concept formation subtest, which measures a child's reasoning ability.

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[83] Eight years of CCD data were collected starting with 1993.

[84] This file includes names, addresses, enrollment, and other descriptive data for 29,845 private schools in the 50 states and the District of Columbia.

To provide a context for understanding the impact estimates obtained from the Woodcock- Johnson, exhibit 6.2 shows some descriptive statistics for the MTO children in the control group. In this analysis the WJ-R test scores are presented as scale scores centered on 500. The exhibit shows the average scale score by age in the nationally representative norming sample and the corresponding average scale score for children in the MTO control group. The exhibit also shows the average age equivalent score for each age group in the MTO control group (in terms of years above and below the national average).[85] Surprisingly, the youngest children in the MTO sample were performing better than average at the time of the interim evaluation.[86] However, as the age of the children increased their relative performance decreased substantially.

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85 An age equivalent scores indicates the age level at which the average score is equal to the subject's score. For example, an age equivalent score of eight indicates that the child scored as well as the average 8-year-old on the test.

86 The scores on the Woodcock-Johnson for the youngest MTO children (5 to 11 years) are higher than was expected based on the demographic characteristics of the children. On most other measures at-risk children consistently scores substantially below the national average. For example, in the national evaluation of the Comprehensive Child Development Program, on the PPVT and the K-ABC, 5-year-old children had an average score more than a standard deviation below the national mean. In the Head Start FACES study, on the PPVT, preschool children (who had not yet experienced Head Start) scored nearly a standard deviation below the mean.

To investigate the reason for the high WJ-R scores in the MTO sample, we examined WJ-R scores from earlier research on similar samples of at-risk children. First, we looked at scores from previous studies of the New York City MTO site. In the New York sample, scores on the WJ-R subtests were also high relative to scores on the PPVT. In other studies of low income children, results on the Woodcock-Johnson were variable. In the Head Start FACES study, children scored two-thirds of a standard deviation below the mean on the WJ-R at pre-test. On the other hand, the low-income child sample from the PSID scored at or above the norm for their age group on the same WJ-R subtests used in the MTO Interim Evaluation.

Careful examination of the test administration and scoring procedures revealed neither evidence of consistent tester bias nor evidence of consistent errors in scoring the WJ-R tests or computing standard scores in the MTO sample. Therefore, we have to conclude either that (a) children do better on tests, like the WJ-R, that test a relatively narrow set of skills than they do on general aptitude tests, or (b) the norming of the WJ-R subtests for the youngest children consistently inflates their scores.

Findings are the same as those shown here if the youngest children (ages 5 to 7) are removed from the sample. If all children under the age of 11 are eliminated from the sample the negative effect on the calculation tests for Section 8 children is no longer statistically significant but there are no other substantive changes in the results.

EXHIBIT 6.2
CONTROL GROUP CHILDREN'S WOODCOCK JOHNSON BROAD READING SCORES

Age of Child	Nationally Representative Sample Scale Score	MTO Control Children Scale Score	MTO Children Age Equivalent Score (Years Above/Below National Average)
Five (n=35)	418	433	0.35
Six (n=76)	452	449	0.05
Seven (n=123)	475	472	0.15
Eight (n=114)	490	484	-0.11
Nine (n=126)	498	491	-0.30
Ten (n=134)	506	494	-0.89
Eleven (n=144)	511	505	-0.58
Twelve (n=128)	514	507	-1.25
Thirteen (n=115)	520	507	-2.31
Fourteen (n=103)	523	509	-3.11
Fifteen (n=99)	526	511	-3.86
Sixteen (n=92)	529	512	-4.57
Seventeen (n=76)	532	513	-5.90
Eighteen (n=63)	532	516	-6.31
Nineteen (n=71)	534	517	-7.79

Source: Woodcock Johnson Battery-Revised tests.
Sample: Control group children ages 5 to 19 as of May 31, 2001.

6.3 Baseline Education Experiences and Control Group Context

Before turning to a discussion of the results it is important to understand both where our sample of children began and how the context of urban education has changed over the demonstration period.

Baseline education experiences of the sample

There has been growing concern about the plight of children in urban schools in recent years. Previous research has shown that 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 (NCES, 1999; Casserly, 2002; Council of the Great City Schools, 1999). These poor educational outcomes have been linked to both low-quality schools and home environments that do not support educational achievement.

While we do not know baseline test scores of the MTO children, we do know a number of things about their educational experiences before participation in the MTO demonstration began. Approximately twenty-seven percent of the original sample who were ages 6 to 17 years old at baseline had learning or behavioral problems at baseline, including having been expelled from school. The families of 26 percent of these children indicated that during the 2 years prior to random assignment, someone from their child's school had asked them to discuss behavior or other problems the child was having at school. Approximately 16 percent of the children who were ages 6 to 17 at baseline had been enrolled in a gifted class or school at the time of random assignment. These numbers are somewhat higher than national averages both in terms of special education and gifted classes. Nationwide, a little more than 13 percent of the school-age population receives special education services (NCES, 2001), 10 percent of children ages 12 to 17 have ever been suspended from school, and 10 percent are enrolled in gifted classes (Fields et al., 2001).

However, the MTO children attended schools characterized by both low achievement and high poverty. At baseline MTO children attended schools in which 78 percent of the students were eligible for free lunch and which ranked, on average, at the 15th percentile on state assessments. Fifty-three percent of the MTO families indicated that the primary or secondary reason they wanted to move was to send their children to better schools.

We also know something about the educational environment in the homes of the children at baseline. Only 60 percent of the sample adults had completed a high school degree or GED at the time of random assignment. Fewer than 27 percent of the sample adults who had a child under the age of 5 at the time of random assignment reported that the child was read to more than once a day.

Thus when the MTO demonstration began, the opportunity was ripe to improve the educational experiences of these children and, as a result, their educational performance.

Control group context

To understand the impact of MTO on experimental and Section 8 group members, it is important to understand whether and how the characteristics of schools attended by the children in the control group changed over the time. Although we hypothesized that the educational experience of children in the experimental and Section 8 groups would change as a result of MTO, the educational experiences of the control group children may also have changed over this period of time. There are two reasons for this.

First, the MTO demonstration was implemented at a time of increased emphasis on urban school reform. Over the past decade, in every one of the MTO sites, there have been high-profile initiatives aimed at improving the educational outcomes for students in city schools. As a result, while experimental and Section 8 children might, because they were able to move, attend schools in lower poverty neighborhoods, the differences between the schools in the more affluent communities and those in the original, higher poverty neighborhoods, might be attenuated by urban school reform. For example, in most Chicago public schools, the number of children performing at grade level in reading and mathematics on the Iowa Test of Basic Skills has increased substantially over the past 10 years and these increases have been most dramatic in the lowest performing schools in the city (Roderick, et al., 1999). The performance of Chicago public school students has increased relative to other schools in the state (Jacob, 2002). Similarly, over the past several years, Boston's state assessment results have improved across all grades and subject areas. Boston's gains exceeded statewide gains in every grade and subject, except grade 8 in reading and science where Boston and state gains were equal. Dropout rates have also fallen considerably in Boston over the past 10 years. The Los Angeles, New York, and Baltimore public schools have shown similar improvements with the number of students scoring at the proficient level on state assessments improving substantially in recent years.

Second, control group families were not precluded from moving, and as earlier chapters have shown, many of the control group families had left public housing and experienced a substantial increase in income in the years between random assignment and the interim evaluation. So control group children may have moved to schools with characteristics similar to those attended by students whose family moved to a more affluent neighborhood as part of MTO.

For the children in the control group, the average performance level of the schools attended improved from baseline to the current time. At baseline the control group children attended schools that ranked around the 13th percentile on state assessments. They are currently attending schools that rank around the 17th percentile. Control group children are also more likely to attend a magnet school now than they were at baseline. The schools attended by the control group now have fewer students eligible for free lunch (66 percent currently versus 80 percent at baseline) and fewer limited English proficient students. All this suggests that urban schools may have been changing or control group families may have been seeking new schools for their children during the time since random assignment. As a consequence there are likely to be fewer differences in the school experiences of the MTO and the control children and less reason to expect differential student achievement.

6.4 Impacts on Hypothesized Mediators of Educational Effects in MTO

As outlined above we hypothesized that a variety of factors would have an influence on the educational outcomes of MTO children, including the quality of the schools they attended and the characteristics of the community in which they live. In turn we hypothesized that these factors would have an effect on the education-related behaviors and attitudes of both parents and children. We explore the evidence for each of these mediators of educational outcomes below.

School characteristics

The most direct mediator of educational outcomes is the quality of the schools attended by MTO children. If children in the experimental and Section 8 groups attended higher quality schools, as defined by higher student achievement, better resources, lower student-teacher ratios, and more stable academic environments, then we hypothesized that it would lead to improved educational outcomes for MTO children. Improvements in student achievement could result directly from improved instruction in these schools or because better schools could promote more positive attitudes among parents and children about school and educational achievement, which would lead in turn to more productive school-related behavior (e.g., better attendance, more time studying).

The evidence suggests that MTO has had a small but positive impact on children's school experiences based on the characteristics of the schools the children attended. At the time of the interim survey the experimental group children were attending schools with fewer students eligible for free lunch, fewer minority students, and fewer limited English proficient students than were control group children (exhibit 6.3). Experimental group children were also attending schools that had higher performance overall. The effect on the entire experimental group was to increase the percentile rank of the school

EXHIBIT 6.3
CHARACTERISTICS OF THE SCHOOLS ATTENDED BY MTO CHILDREN

	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Percent Free Lunch [ADMIN] (n=3562)					
Current school	0.657	-0.066* (0.010)	-0.130* (0.020)	-0.026* (0.011)	-0.044* (0.018)
Average school	0.721	-0.067* (0.009)	-0.133* (0.018)	-0.034* (0.010)	-0.059* (0.017)
Percent White [ADMIN] (n=4875)					
Current school	0.106	0.040* (0.009)	0.089* (0.021)	0.029* (0.014)	0.046* (0.022)
Average school	0.086	0.047* (0.007)	0.101* (0.016)	0.029* (0.010)	0.046* (0.016)
Pupil-Teacher Ratio [ADMIN] (n=4876)					
Current school	15.038	0.181 (0.225)	0.395 (0.498)	-0.176 (0.220)	-0.285 (0.355)
Average school	14.609	-0.038 (0.188)	-0.083 (0.409)	-0.269 (0.208)	-0.435 (0.337)
Percent Limited English Proficient [ADMIN] (n=4019)					
Current school	0.168	-0.027* (0.006)	-0.058* (0.013)	-0.005 (0.007)	-0.009 (0.011)
Average school	0.181	-0.030* (0.006)	-0.063* (0.013)	-0.008 (0.007)	-0.013 (0.011)
Magnet School [ADMIN] (n=3945)					
Current school	0.249	-0.051* (0.019)	-0.113* (0.041)	-0.029 (0.023)	-0.043 (0.034)
Average school	0.182	-0.035* (0.015)	-0.077* (0.034)	-0.026 (0.018)	-0.039 (0.027)
Percentile Rank on State Exam [ADMIN] (n=3935)					
Current school	0.167	0.038* (0.008)	0.085* (0.018)	0.017 (0.008)	0.026 (0.013)
Average school	0.144	0.041* (0.006)	0.091* (0.014)	0.017* (0.007)	0.027* (0.010)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: U.S. Department of Education, Common Core of Data 1993 to 2001, National School-Level State Assessment Score Database, 2000 to 2001
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

currently attended by four percentage points (from the 17th percentile to the 21st percentile), while the effect on those who leased up was to increase it by eight percentage points. Experimental group children were also less likely than control group families to be attending a magnet school. Because they were given the opportunity to move, experimental group families may not have felt the need to seek out alternative schooling options like magnet schools for their children. The trends for the Section 8 group were similar to the experimental group, although generally the impacts were smaller and in many cases not statistically significant.

Some of the families who moved to low-poverty neighborhoods subsequently moved back to neighborhoods with higher levels of poverty (see chapter 2) and there is also some evidence from the qualitative evaluation that students may have switched schools frequently (Popkin et al., 2001). To account for these factors, the exhibit also displays a weighted average of the characteristics of the schools attended by the children since the time of random assignment. The weighted averages may provide a better estimate of children's school environments over the course of the entire demonstration than a simple description of the child's current school. These exposure measures cover anywhere from 1 to 7 years of schooling, depending on the age of the child at the time of random assignment. On average they represent approximately 5.5 years of schooling.

As seen in exhibit 6.3, there was little difference between the estimates obtained when the weighted average of school characteristics was used in the impact analyses although the estimated effects for the average school were slightly larger.

In summary, MTO had a modest impact on children's educational experiences based on the characteristics of the schools attended by the experimental group children. The children in the experimental group were attending somewhat higher performing schools with fewer poor and minority students than the control group children. However the differences may not be educationally significant. Moving from a school ranked at the 17th percentile in the state to one ranked at the 21st or even 25th percentile is not a substantial improvement. To determine how many children were attending substantially higher performing schools at the time of the interim evaluation, we explored the percentage of children in the experimental group who attended schools that ranked above the 50th percentile in their state and those who attended schools that ranked above the 75th percentile. Only two percent of the experimental group children attended schools that performed at or above the 75th percentile. Less than 10 percent of the experimental group children attended schools ranked at even the 50th percentile or higher. This suggests that most experimental group children were not attending substantially higher performing schools as a result of MTO. These findings differ greatly from those of the Gautreaux Program in Chicago where 88 percent of the sample who moved to suburban schools attended schools with average ACT scores at the national average or above (Rosenbaum, 1995).

This suggests that a further exploration of the pattern of school moves among the MTO children is warranted. The qualitative report on the MTO demonstration (Popkin et al. 2001), suggests that one reason MTO may have had a limited impact on children's school environments is that a number of experimental group children did not attend their local neighborhood school, even if their families had relocated to and remained in low-poverty areas. The report states, "…MTO children's educational experiences since program assignment are more complex than anticipated, because many children attend schools outside their immediate communities…Families cited many different reasons for placing children in out-of-area schools, including ties to friends or previous communities, school quality, and children's special needs." In Chicago, several families who had moved chose to place or keep their children in schools near their former public housing developments. This was possible, the report suggested, because "Many urban school systems now allow children to apply citywide for special programs or schools and even offer charter schools as alternatives. School choice at the high school level appears to be particularly common, with families in Chicago, Boston, and New York all reporting that their children could apply to attend different high schools throughout the city."

Using detailed school history information collected about MTO children from their parents, we can examine whether many children switched schools when their families moved or returned to schools near public housing shortly after moving to a lower poverty area. While the proportion of children currently attending the same school as they were at the time of random assignment could be explored, because children naturally change schools as they get older, and move from elementary school to middle school and from middle school to high school, these numbers would significantly underestimate the number of children who were attending the same school they would have been attending had their families not been give the opportunity to move.

As an alternative we explored the proportion of experimental group children who are attending school in the same ZIP Code area as the school they were attending at baseline. Because ZIP Codes cover a relatively small geographic area (generally not more than a few miles), this may provide a good estimate of the number of children who were attending schools near their old public housing developments at the time of the interim evaluation. Approximately 20 percent of the experimental group children were attending schools in the same ZIP Code area as at baseline compared with 27 percent in the control group. Among those families who leased up, the number was 16 percent. These numbers probably undercount the number of experimental group children attending schools near their original public housing developments because many elementary schools in urban areas feed into high schools with different ZIP Codes. A child may have remained in the elementary school near their development and may have attended the same high school as they would have had they remained in public housing, but those schools may have had different ZIP Codes. While such an analysis is only suggestive it seems likely that at least some experimental group children remained in or returned to schools in their old neighborhoods after random assignment.[87]

The qualitative report suggests a number of reasons why families chose to place their children in out-of-area schools. Some parents cited negative experiences with the schools in their new community. Many did not feel their children were safe in the new schools and others were concerned with the educational quality of the schools, noting that their children's grades had suffered when they moved to the new school. The qualitative report also suggests that some children were attending out-of-area

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[87] Obtaining a more exact estimate of the number of children still attending schools close to their public housing developments would require obtaining additional data or more in-depth analysis. One avenue that might be pursued would be to obtain information from school districts about the neighborhood school associated with a residential location, although increased opportunities for school choice might complicate such analysis. Alternatively, using geocoded data, the distance between the residential location and the school location of both experimental and control group children could be compared, although such an analysis would still not identify the actual number of experimental or Section 8 children attending schools near their original public housing developments.

schools because they were attending schools for children with special needs. Finally, the qualitative findings noted parental reluctance to make their children change schools multiple times.

Quantitative analyses support some of these findings. For example, it seems possible that some of the negative experiences children had in the schools in low-poverty areas were due to the difficulties children faced when transitioning to schools in which few students were of their same racial or ethnic background. Of the experimental group children attending schools in a different ZIP Code area than they were at baseline, 36 percent were attending a school where the dominant race or ethnicity was different than their own.[88] Children in the both the experimental and Section 8 groups also changed schools more frequently than did the control group. At the time of the interim evaluation MTO control group children had attended an average of 2.3 schools and experimental and Section 8 group children had attended an average of 2.4 schools, a statistically significant but small difference. However, it does not appear that a large proportion of children were attending schools for students with special needs. The data indicate that less than 3 percent of the total MTO sample was attending a special needs school at the time of the interim evaluation.

Another possibility is simply that the MTO children did not move far enough from their baseline neighborhood to make a substantial difference in the type of school they attended. To determine whether MTO children were actually attending schools in substantially different neighborhoods than at baseline, we explored the proportion of children in the experimental group who were attending schools in the same district as they were at baseline. In every site with the exception of Los Angeles, this is essentially an urban-suburban comparison. At the time of the interim evaluation, approximately 80 percent of the experimental group members were attending schools in the same district as they had attended at baseline. Among those whose families leased up, almost 70 percent of the children were attending schools in the same district as at baseline. In general, the schools in the new districts appear to be of higher quality than the schools in the urban districts. Of the experimental group children attending schools in a different district than at baseline, 20 percent were in schools that were performing above the 50th percentile on state assessments compared with 8 percent of the children who remained in the urban districts.

Exhibit 6.4 shows the percentage of children attending schools in a different district than at baseline for each of the five MTO sites by random assignment group. There is some variation by site, with more experimental children in Baltimore, Boston, and Los Angeles attending schools in a different district than in either Chicago or New York.[89]

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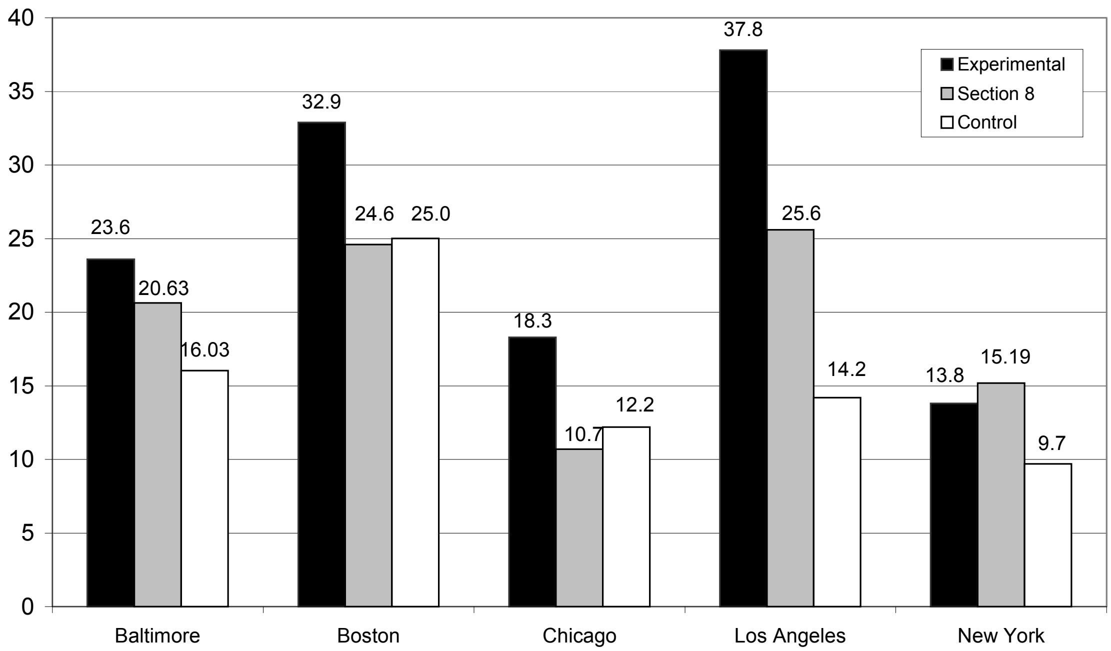
[88] Dominant is defined as greater than 50 percent. A Hispanic student attending a school that was more than 50 percent black would be considered to be attending a school with a different racial/ethnic composition than his or her own.

[89] The Los Angeles public schools are located in several different districts, which explains the higher number of experimental children attending schools in different districts than baseline in this site.

Exhibit 6.4
Percentage of Children Attending Schools in a Different District than at Baseline



Other community-level mediators

Although school characteristics were thought to be extremely important, other community-level factors were also hypothesized to affect educational outcomes, including community norms and values, the social and physical environment, and economic opportunities. The evidence on these mediating factors was mixed but generally positive. Impact estimates for the mediators discussed below that are not found in earlier chapters of the report are provided in appendix E (exhibits E6.1 to E6.7).

The strongest impact of MTO was on the social and physical environment in which the children lived. It increased the sense of safety reported by adults and (as detailed in chapter 3) significantly improved families housing and neighborhood quality. MTO also reduced youths' reported frequency of seeing people selling and using drugs and of hearing gunshots in the neighborhood. This suggests that as a result of MTO children were living in safer, more secure environments.

There was also some evidence that MTO had a positive impact on the community norms and values to which children were exposed by increasing the number of positive adult role models with whom the children in the experimental group had contact. Based on parent report, MTO increased the likelihood that sample adults in the experimental group had at least one friend who was a college graduate or earned more than $30,000 a year. MTO also increased the likelihood of adults reporting that their neighbors would intervene if they saw children doing graffiti, skipping school, or hanging out, suggesting that children experienced increased monitoring from adults in the community. However, as already shown in chapter 5, MTO's impact on peer role models was less clear. MTO had no effect on the likelihood that a child had friends who were involved in school activities. It also had no effect on the likelihood that a child had a friend who carried a weapon, and it increased the likelihood that a child had friends who took drugs. Although the demonstration may have been successful in moving families to neighborhoods with norms and values that encouraged and supported educational achievement, it does not appear to have substantially altered the behavior or values of the children's peer networks, the individuals with whom children have the most contact.

Finally, there is some evidence that MTO had a positive impact on the economic opportunities for adults. Both the experimental and Section 8 groups lived in neighborhoods with a lower poverty rate, and where there was a higher percentage of adults in the neighborhood who were currently employed (see chapter 2). However, although families moved to neighborhoods with greater economic opportunities, MTO had no effect on the annual earnings or employment levels of the experimental or Section 8 adults relative to the control group, so the circumstances of the adults with whom the children live and with whom they have the most contact were not impacted.

Student- and family-level mediators

Despite the changes observed in the community-level mediators, few impacts of MTO were found on the student- or family-level mediators hypothesized to affect educational outcomes.

Although there was evidence that MTO did impact parent attitudes, these changes did not translate into changes in parenting behavior. Experimental and Section 8 adults reported a greater sense of well-being, stronger feelings of safety, and lower levels of stress and anxiety relative to the control group (see chapters 3 and 4). However, there was no effect on the level of parental monitoring of children as reported by the parents (e.g., whether the adult knew the child's friends or teachers, or who the child was with when he or she was not at home) or on the degree of parental warmth exhibited to the child by the sample adult at the time of the home visit. MTO also did not increase the frequency with which the sample adult reported attending school events, general school meetings, or volunteering at the child's school.

Similarly, although MTO children were attending schools with different characteristics, MTO had no impact on children's reports of the overall school climate. MTO had no effect on how safe children felt at school, how interested they felt teachers at their schools were in their school performance, the extent to which behavior problems interfered with learning, and the amount of cheating at their school. Results also suggest that MTO did not impact the educational resources available to students in school (e.g., there was no effect on the number of children who indicated they had their own math book to bring home from school).

Finally, MTO did not have any impact on student school-related behaviors. There was no effect of MTO on the number of students who reported having been tardy for school more than once a month, on the number of students whose parents had been called by the school about problems with the child's behavior or schoolwork, or on the number of children who had been suspended or expelled from school in the past 2 years. MTO also had no impact on the number of hours children spent doing homework, watching television or reading for pleasure, or on the degree to which children reported working hard in school or paying attention in class.

6.5 Impacts on Hypothesized Outcomes

To assess the degree to which MTO affected children's school performance and educational progress, we estimated the impacts of MTO on academic achievement, as measured by standardized test scores and on overall progress through school—including promotion or retention in grade, coursework taken, high school graduation, and post-high school graduation educational plans.[90]

Because there were few impacts on the hypothesized mediators of educational outcomes, it is not surprising that few impacts were found on the outcomes themselves. Exhibit 6.5 shows impact estimates for the Woodcock-Johnson Achievement tests scores. MTO had no significantly positive impacts on scores from any of the five subtests or on the composite reading or mathematics scores. There was a negative effect on the calculation subtest for children in the Section 8 group. Exhibit 6.6 shows these same results broken down by gender, age, mother's education, and whether or not the child had any behavior problems at baseline. There were no statistically significant positive effects on the Woodcock-Johnson for any of these subgroups. The 8- to 11-year-olds in the Section 8 group scored significantly lower on the broad math test. The sample size and precision of the Woodcock-Johnson estimates were such that we would have been very likely to detected an impact as small as 6 or 7 points on any one of these tests, which is equivalent to answering approximately two or three more question correctly. The standard deviations on the WJ-R subtests were around twenty-five points, so we would have been able to detect an increase of a little over a quarter of a standard deviation on most subtests.

Exhibit 6.7 shows impact estimates for the survey measures. MTO also had no positive significant impacts on any of the measures of educational progress obtained from the surveys. However, there are some findings worth noting here. Although it was hoped that the MTO demonstration would lead to positive educational outcomes for children, it was also hypothesized that MTO could have a detrimental effect on children's self-esteem and self-confidence if children who moved to more affluent communities felt isolated because there were few student of the same background or if they felt inadequate compared to higher performing peers. A negative self-image might lead to negative educational outcomes such as lower grades. It was also possible that poor and minority children who moved to more affluent schools might be more likely to be placed in special education or retained in grade.

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[90] When data were available, impacts on educational outcomes were estimated for all children age 5 to 19 in the interim evaluation sample. However, in many instances survey questions were only relevant to a smaller age range and were only asked of a subsample of the population. In these instances the exhibits indicate the subsample used to obtain the estimates.

EXHIBIT 6.5
EDUCATION OUTCOMES: WOODCOCK-JOHNSON ACHIEVEMENT TESTS

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Woodcock-Johnson Scores [ADMIN]					
Broad reading (ages 5 to 19) (n= 5169)	497.31	0.92 (0.93)	2.04 (2.06)	0.45 (1.06)	0.74 (1.73)
Broad math (ages 5 to 19) (n= 5187)	501.23	0.22 (0.78)	0.49 (1.75)	-1.07 (0.85)	-1.74 (1.39)
Letter/word identification (ages 5 to 19) (n= 5229)	498.86	0.58 (1.20)	1.27 (2.66)	0.23 (1.33)	0.38 (2.17)
Passage comp (ages 5 to 19) (n= 5192)	495.71	0.89 (0.84)	1.96 (1.85)	0.54 (0.95)	0.89 (1.56)
Applied problems (ages 5 to 19) (n= 5202)	499.11	1.25 (0.90)	2.79 (2.00)	-0.39 (1.03)	-0.63 (1.69)
Calculation (ages 5 to 19) (n= 5239)	503.35	-0.91 (0.82)	-2.01 (1.81)	-1.77* (0.86)	-2.89* (1.40)
Concept form (ages 5 to 11) (n= 1764)	485.55	0.42 (1.42)	0.93 (3.10)	-0.80 (1.70)	-1.22 (2.59)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Woodcock Johnson-Revised tests.
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Analyses take into account correlation within families.
e) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).
f) Standard deviations for each of the subtest for the MTO sample were as follows: Broad Reading: 29, Broad Math: 25, Letter/word Identification: 37, Passage Comprehension: 25, Applied Problems: 26, Calculation: 28, Concept Formation: 23.

EXHIBIT 6.6
EDUCATION OUTCOMES: WJ ACHIEVEMENT TESTS FOR SUBGROUPS, AGES 5-19

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Broad Reading [ADMIN] (n= 5164)					
Female (ages 5 to 19)	499.69	1.84 (1.14)	3.96 (2.45)	-0.19 (1.24)	-0.30 (1.99)
Male (ages 5 to 19)	494.94	-0.03 (1.38)	-0.06 (3.14)	1.13 (1.59)	1.89 (2.66)
Ages 5 to 7	456.48	2.57 (2.51)	5.08 (4.96)	3.72 (2.88)	5.72 (4.43)
Ages 8 to 11	492.32	-.719 (1.47)	-1.61 (3.28)	-1.49 (1.61)	-2.24 (2.41)
Ages 12 to 19	511.02	1.50 (1.26)	3.38 (2.84)	0.88 (1.48)	1.56 (2.62)
Baseline behavior problems[1]	503.64	2.17 (2.07)	4.95 (4.73)	0.86 (2.21)	1.71 (4.39)
Mother HS grad/GED	500.07	-1.13 (1.55)	-2.84 (3.60)	1.04 (1.87)	1.69 (3.03)
Broad Math [ADMIN] (n= 5182)					
Female (ages 5 to 19)	502.30	0.81 (1.06)	1.73 (2.26)	-1.21 (1.09)	-1.92 (1.73)
Male (ages 5 to 19)	500.18	-0.36 (1.05)	-0.82 (2.42)	-0.91 (1.17)	-1.52 (1.95)
Age 5 to 7	460.44	2.01 (1.97)	4.08 (3.99)	1.72 (1.95)	2.62 (2.96)
Age 8 to 11	498.33	-1.74 (1.11)	-3.90 (2.48)	-4.59* (1.24)	-6.82* (1.84)
Age 12 to 19	513.94	0.99 (1.14)	2.24 (2.57)	0.50 (1.21)	0.88 (2.15)
Baseline behavior problems	508.93	0.06 (1.78)	0.15 (4.10)	-1.77 (1.97)	-3.51 (3.90)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Woodcock Johnson-Revised tests.
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Analyses take into account correlation within families.
e) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

[1] Combines special education for behavior problems, suspended or expelled and someone asked to talk about child's problems at school or behavior in last 2 years.

EXHIBIT 6.7
EDUCATION OUTCOMES: SCHOOLING

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
School Outcomes [SR, PR]					
Mostly Bs or higher[1] (ages 12 to 19) (n=2753)	0.393	-0.043 (0.026)	-0.096 (0.058)	-0.050 (0.028)	-0.087 (0.048)
Advanced coursework[2] (ages 5 to 19) (n=5457)	0.200	-0.002 (0.016)	-0.004 (0.036)	0.010 (0.017)	0.016 (0.028)
Special ed. recipient[3] (ages 5 to 17) (n=4731)	0.245	0.024 (0.017)	0.051 (0.036)	0.013 (0.018)	0.020 (0.029)
Educationally on track[4] (ages 15 to 19) (n=1550)	0.741	0.029 (0.028)	0.064 (0.062)	0.036 (0.031)	0.068 (0.058)
Ever repeated a grade[5] (ages 5 to 19) (n= 5354)	0.221	0.026 (0.016)	0.058 (0.035)	-0.020 (0.017)	-0.032 (0.028)
Took SAT/ACT (ages 15 to 19) (n=1562)	0.342	-0.032 (0.032)	-0.073 (0.074)	0.022 (0.035)	0.041 (0.063)
Attended College [SR] (n=2819)					
Some (ages 12 to 19)[6]	0.043	-0.003 (0.010)	-0.007 (0.021)	0.005 (0.010)	0.009 (0.018)
4 years[7]	0.017	-0.005 (0.006)	-0.011 (0.013)	-0.001 (0.007)	-0.001 (0.012)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child survey, Youth survey, POCY survey. See appendix A for details.
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Analyses take into account correlation within families.
e) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

[1] Overall grades child [12-19] received last year were high (mostly Bs or higher) as self reported.
[2] Youth self-reported taking an advanced math class, AP class, or being enrolled in a gifted class or school in the past 2 years.
[3] The proportion of children whose parent reported they had received special education services for learning and/or behavior/emotional problems in the last two years. Results were the same for “ever” received special education services.
[4] Youth is currently in school or received a HS diploma or GED.
[5] The proportion of children whose parent reported that they had ever repeated a grade, including kindergarten. Results were the same for repeated excluding kindergarten.
[6] Currently enrolled in a two or four year college. Results were the same for “ever” attended.
[7] Currently enrolled in a four year college.

Although not statistically significant, the coefficients on some of the survey measures were negative, suggesting that the MTO children may have had some difficulty adjusting to new educational environments. This may be another reason why few achievement effects were found.

Analyses were conducted to determine if there were MTO impacts on subgroups. Each of the educational outcomes was estimated separately for girls versus boys, for children of different ages, for children whose mothers had different levels of education, for families who indicated that a primary reason for moving was to attend better schools, and for children with and without baseline school behavior problems (see appendix D, exhibits D6.1 to D6.3 for the results of these analyses). MTO had a negative effect on the self-reported grades of the boys in the sample. The effect of MTO was to reduce the proportion of boys in the experimental group who reported they received mostly Bs or higher by 10 percentage points; among those who leased-up, the reduction was 21 percentage points. This finding is consistent with the results presented in chapter 5, which suggested that MTO may have had some detrimental effects for boys. In addition, the subgroup estimates showed that experimental group children age 8 to11 were more likely to have repeated a grade than control group children, while children 12-19 in the Section 8 group were less likely to have repeated a grade. Children in the experimental group whose families indicated that a primary reason for moving was so their children could attend better schools, had lower self-reported grades than similar families in the control group. These findings may reflect differences in the schools attended by children in these two groups. As can be seen in exhibit D6.3 children whose families were motivated to move because they wanted better schools for their children were attending schools with fewer students eligible for free or reduced price lunch. There were no other statistically significant differences for any of the subgroups.

While there were few statistically significant impacts among children of different ages on any of the outcomes explored, it is possible that age at random assignment is more important in predicting outcomes than the child's age at the time of the interim evaluation. It seems plausible that children who moved when they were younger might have more positive outcomes than those who were older and had already spent substantial time in low poverty schools and neighborhoods. Due to variations in the time of random assignment, the ages of the children at baseline varied widely even in the relatively small age ranges we used to conduct the subgroup analyses. Children who were between 5 and 7 years old when they were interviewed were between –1 and 4 years old at baseline. Those who were between 8 and 11 years old at the time of the evaluation were 1 to 8 years old at baseline, and those aged 12 to 19 where between 5 and 16 years old at baseline. To explore whether age at random assignment had an effect on outcomes for the MTO children we conducted subgroup analyses for children who were 5 years old or younger at random assignment, for children between 6 and 11 years old at random assignment and for those who were 12 or older (see appendix D, exhibit D6.4). Experimental group children who were 12 or older at the time of random assignment were more likely to have repeated a grade than children of the same age in the control group. There were no other statistically significant differences among children randomly assigned at different ages. It should be noted that these results are not experimental, since the child's age at random assignment is related to the time the family was randomly assignment. As discussed in chapter 9, there were large differences between families randomly assigned early and those randomly assigned later in the demonstration.

Finally, previous research conducted in the Boston and Baltimore MTO sites found that the quality of schools improved significantly in both sites. In Baltimore findings suggested that MTO had had a positive impact on the test scores of the experimental group children (Katz, Kling, and Liebman, 2001; Ludwig, Ladd, and Duncan, 2001). In addition, there was some variation by site in the number of children attending schools in different districts than at baseline at the time of the interim evaluation. Given these findings, we thought it was important to do an impact analysis by site on the achievement score data. The findings are generally consistent with previous research (see Appendix D, exhibit D6.5). There is a strong and significant experimental group impact in Baltimore on the WJ-R reading exam. There is also a strong and significant impact in reading in Chicago. There were no statistically significant results in the other three sites and no significant impacts in math. These findings suggest that further analyses by site, which explore differences in school quality, might be useful.

Interpretation of results

We hypothesized that there were four important community characteristics associated with the level of affluence of a neighborhood that could impact educational outcomes: 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. These factors were hypothesized to influence the education-related attitudes of both parents and children, which would in turn influence educational achievement and educational progress.

The findings suggest that MTO had a small but positive effect on children's educational experiences based on the characteristics of children's schools, although at the time of the interim evaluation, approximately 80 percent of the children in the experimental group were attending schools in the same district as they had been at baseline. There were also significant impacts on some of the other community-level variables hypothesized to mediate educational outcomes. At this point in the demonstration, however, these positive effects on the mediators of educational achievement have not had significant impacts on education-related behaviors or attitudes of MTO children or positive and significant impacts on any of the measures of school achievement or educational progress.

If the underlying model is correct, the lack of impacts might be attributed to the relatively small changes observed in the characteristics of the schools attended by the MTO children. At the time of the interim survey, experimental children were attending only slightly higher performing schools than the control group and were relatively similar in composition to the schools they were attending at baseline. Only 20 percent of the children in the experimental group were attending schools in a different district than at baseline. The qualitative report (Popkin et al., 2001) 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.

It is also possible that it is simply too soon to observe impacts on educational outcomes for these children, especially 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 4 years and there is some evidence that they were having difficulty adjusting to these new environments. The Gautreaux Program in Chicago found that it took time for children to feel comfortable in their new environments and positive results were not observed for some children until 6 years after they had moved (Rosenbaum, 1995).

Chapter Seven
Impacts on Employment and Earnings

This chapter summarizes the reasons why MTO participation might be expected to affect the employment and earnings of adults and older youth in the interim evaluation sample. It then presents the interim findings on employment and earnings impacts, using respondent self-reports from the interim evaluation survey and administrative data from state unemployment insurance (UI) wage records.

Summary

Although the experimental and Section 8 group families tended to move to neighborhoods with more favorable employment opportunities and norms more supportive of work than those where controls lived, we found virtually no significant effects on the employment or earnings of either adults or youth in either of these groups. The only significant effect on earnings or employment was a short-term negative effect on employment for the experimental group relative to the control group, an effect that dissipates over time. The most encouraging finding in this analysis was a relatively large (22 percentage point) increase in the proportion of female youth attending school as a major activity in the experimental group among families who leased up, and a concomitant 16 percentage point reduction in idleness (neither employed nor enrolled in school) among female youth in this same group. No corresponding effects were found for boys.

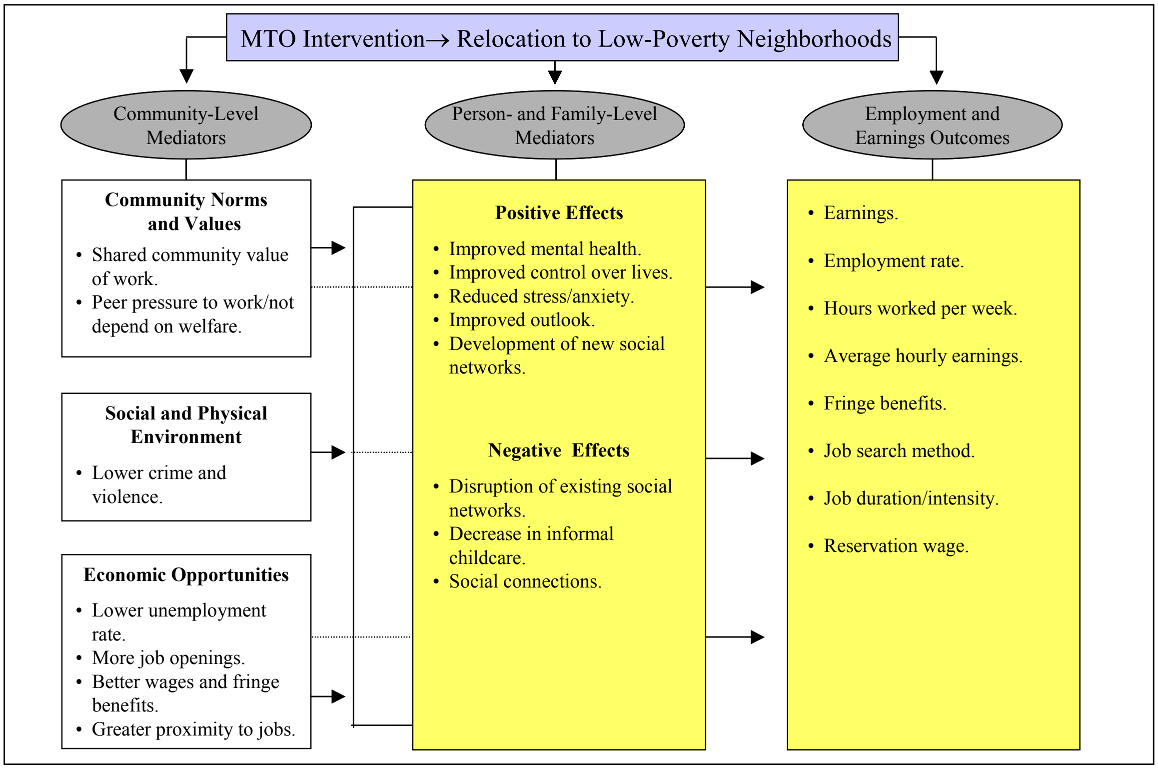
7.1 Hypotheses about Employment and Earnings in MTO

A primary motivation for the MTO demonstration is to measure the impacts of neighborhood on the employment and earnings of low-income families. Residential mobility might affect employment and earnings through any or all of the following causal mechanisms (see exhibit 7.1):

- Low-poverty areas are likely to have lower unemployment rates and faster job growth than higher poverty areas. This may result in higher employment and earnings for MTO movers and may lead to better jobs in terms of wages and fringe benefits.

- The ability to locate near potential sources of employment rather than being tied to the location of a public housing project may reduce job search costs and (once employed) commuting costs. This may lead to increased employment and earnings and reduced reservation wages. If relocation leads to a broader range of employment opportunities, it may also tend to increase wages and fringe benefits.

EXHIBIT 7.1
EMPLOYMENT AND EARNINGS OUTCOMES AND MEDIATING FACTORS



- Relocation to a safer neighborhood may lead to reduced stress and anxiety, and more generally, improved mental health and sense of control over their lives. This may result in more active job search and, therefore, increased employment and earnings. It may also reduce reservation wages. If so, it may lead to lower wage rates and fringe benefits.

- Community norms in low-poverty areas are likely to be more supportive of work and less accepting of welfare than those in public housing projects. To the extent that sample members feel pressure to work rather than collect welfare, this might be expected to increase job search, employment, and earnings, and may reduce reservation wages.

- Relocation may disrupt pre-existing social support networks that are important sources of informal childcare and labor market information and connections. Relocation may also disrupt pre-existing employment relationships. In the short run, this could lead to reduced employment and earnings and increased reservation wages, although we would expect these effects to be reduced over time as new social networks are established.

- Relocation may result in improved physical health, either through a reduction in environmental hazards or through better healthcare. Improved health represents an increase in human capital through lower rates of absenteeism and other channels that could result in improved job prospects.

The existing evidence concerning the effects of residential mobility programs on the employment of adults is somewhat mixed. Rosenbaum and Popkin's (1991) analysis of a survey of female household heads in the Gautreaux Program in Chicago found substantially higher employment rates (14 percentage points) for those who moved to the suburbs than for those who moved to other parts of the central city. But analyses of the early labor market impacts of MTO using administrative data found no significant effects in the first few years after randomization in either the Boston (Katz, Kling, and Liebman, 2001) or Baltimore sites (Ludwig, Duncan, and Pinkston, 2000). Earlier household survey data for the Boston site also indicated little evidence of early employment effects. A household survey of early enrollees in the Los Angeles site found no significant 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, 1998).

7.2 Data Sources and Measures

Respondent self-reports from the interim evaluation survey and administrative data from state unemployment insurance (UI) records were used to assess the impact of MTO participation on employment and earnings of sample adults and of older youth (those 14 to 19 years old as of May 31, 2001). Sample adults in the interim evaluation survey were asked a series of questions on their current employment status, hours of work, earnings from their current main job, earnings from all jobs for calendar year 2001, employee benefits, job search behavior, informal work, and the duration and characteristics of their current main job. Employment for sample adults at the survey date was measured using the standard employment status questions from the current population survey that are the basis for official Bureau of Labor Statistics estimates of employment, unemployment, and labor force participation. Older youth were also asked a battery of standard employment status questions and a further series of questions on earnings, informal employment arrangements, and enrollment status. Further questions from the interim evaluation survey and census data on the characteristics of current neighborhoods provide information to examine mediating factors for employment and to test alternative hypotheses about the impacts of MTO on labor market outcomes.

Administrative data on UI records of the quarterly earnings of sample adults and youth were collected and processed from the five MTO states (California, Illinois, Maryland, Massachusetts and New York). The available administrative data for each state provided earnings information for at least calendar years 1995 to 2001. Administrative earnings information is typically complete for the first 16 quarters after random assignment. States used social security numbers (SSNs) to match MTO sample members to the UI records.

Four of the states provided individual-level earnings information on each MTO sample member who matched to the UI records. However, the fifth state (Massachusetts) could only provide the data aggregated across groups consisting of at least 10 MTO individuals. For methodological consistency, the UI data from all of the states were aggregated into cells of at least 10 individuals and then analyzed at the cell level. The same algorithm was used to construct the cells for all five states. The algorithm maintained the distinctions between sites, randomly assigned groups, and randomization periods. The cells were constructed to be as small as possible (but with at least 10) and to be as homogeneous as possible on selected characteristics. For the adults, the selected characteristics were randomization quarter, baseline education, and baseline work status. For the youth, the selected characteristics were age group and gender. The cell-level analyses of mean outcomes control for these mean characteristics. We have compared cell-level analyses of treatment effects for the four states with individual-level data; in all cases, the results are very similar using the two approaches.

The administrative data analyses allow us to examine the evolution of the employment and earnings impacts of MTO with time in the program (time since random assignment). The disadvantages of the administrative data are: the failure to include informal and uncovered employment, and potential errors in SSNs and in the process of matching respondents to the state UI records.

Traditional measures of employment, labor force participation, and earnings can be used to 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 adult labor market outcomes in the long run. Idleness (being out of work and not enrolled in school) was also examined as possibly a more accurate indicator of poor youth labor market outcomes.

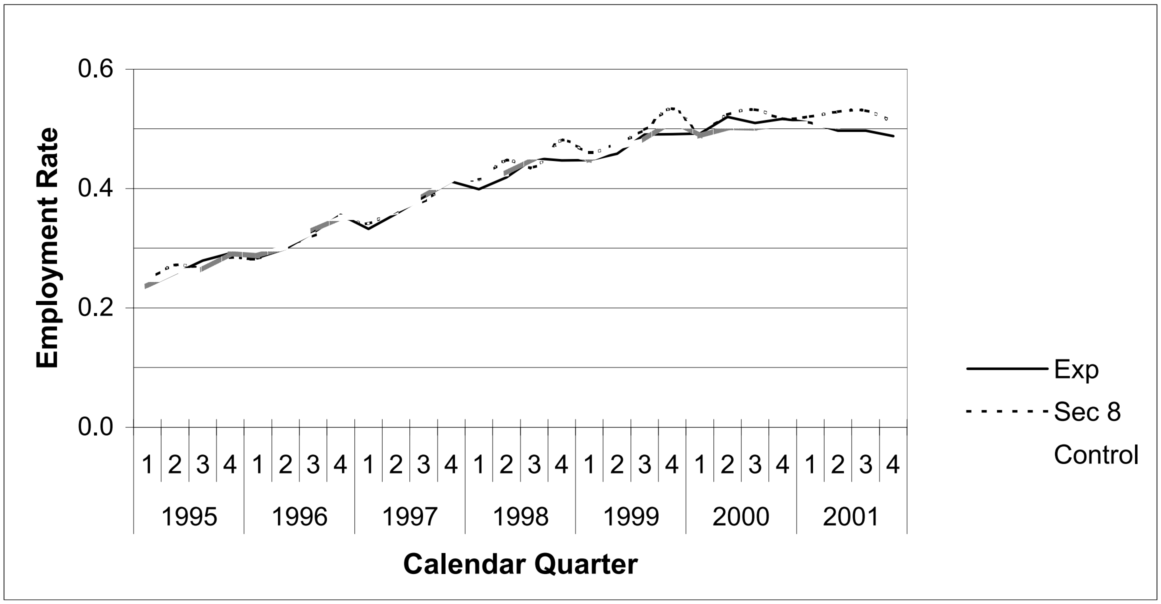
7.3 Context and Baseline Employment Status of the Sample

According to the baseline survey the employment rate of the sample adults was only around 26 percent (25 percent for the control group) at the time of random assignment. This low employment rate is not surprising because the sample consisted largely of female household heads with limited education who at baseline were Aid to Families with Dependent Children (AFDC)/ Temporary Assistance for Needy Families (TANF) recipients living in public housing projects in high-poverty, inner-city neighborhoods.

However, the labor market conditions and tax-transfer system incentives and constraints facing the sample members have changed dramatically since the mid-1990s. The national unemployment rate declined from 5.6 percent at the start of the MTO demonstration in the last quarter of 1994 to 4.0 percent in 2000, before rising back to 5.8 percent in 2002. The employment and earnings improvements of the late 1990s were particularly great for disadvantaged workers and particularly strong in some large U.S. cities, including Boston. Changes in the welfare system began with state welfare waivers in the early 1990s, followed by federal legislation for welfare reform (implemented by states starting in late 1996). These changes, combined with the expansion of the earned income tax credit from 1993 to 1996, served to increase greatly the financial and social incentives for female household heads to move off public assistance and into employment. National data indicate large declines in the welfare rolls and large increases in the employment rates and labor market earnings of single female household heads from the mid-1990s to 2000 (Blank 2002). Thus the changing economic and policy environment would lead us to expect substantial increases in the labor market attachment of the sample adults at least through 2000, even in the absence of any MTO impacts.

In fact, the employment rate of the sample adults has increased greatly over the past 7 years. The sharply rising employment rate of sample adults in all three treatment groups from 1995 to 2000 is illustrated in exhibit 7.2, using data from state UI records for sample adults from all five MTO sites. For sample adults in the control group, the employment rate (share with positive quarterly earnings) measured from administrative data more than doubled from 23.6 percent in the first quarter of 1995 to 50.9 percent in the first quarter of 2001. The employment gains slowed with the onset of the economic downturn in 2001 and employment rates for sample adults declined slightly from late 2000 to late 2001. Nevertheless, although aggregate labor market conditions significantly weakened in 2001 and 2002, the (self-reported) employment rate of sample adults in the control group still more than doubled from 25 percent at baseline to 52 percent at the time of the interim evaluation survey in 2002.

EXHIBIT 7.2
ADULT EMPLOYMENT BY CALENDAR QUARTER AND TREATMENT GROUP (ADMINISTRATIVE DATA)



Sources: State administrative unemployment insurance (UI) records from California, Illinois, Maryland, Massachusetts, and New York.
Sample: Adults from families randomly assigned through December 31, 1997.

7.4 Impacts on Hypothesized Mediators

The mobility experiences of the experimental and Section 8 groups provide further context for assessing the alternative mechanisms through which one might observe impacts of MTO on the labor market outcomes of sample adults. The experimental and Section 8 groups on average have moved into neighborhoods that appear to have more favorable employment opportunities and norms more supportive of work than the neighborhoods of the control group. The experimental and Section 8 groups reside in neighborhoods with substantially lower poverty rates, higher adult employment rates, and higher shares of two-parent families than the control group (exhibits 2.8 and 2.10). The MTO treatment does not appear to have increased social isolation. And sample adults in the experimental group are significantly more likely to have friends who are college educated and who earn more than $30,000 (exhibit 2.14). Improvements in neighborhood safety and modest evidence of improvements in some indicators of mental health for sample adults in the experimental group were also hypothesized to facilitate labor market participation and success (chapters 3 and 4). But moves to new neighborhoods do not seem to have improved the perceived access to transportation of those in the experimental and Section 8 groups (exhibit 2.14).

The qualitative research that preceded the interim evaluation survey identified two main barriers to adult employment. Health problems posed the greatest challenge to a number of qualitative sample respondents; again, the improvements on mental health indicators for adults in the experimental group might suggest a reduction in this impediment to working. The other barrier was lack of available or affordable childcare. For example, a Section 8 group respondent noted:

> *…There isn't anyone to take care of the little one because he has asthma. I have my mother, but she's very busy and takes care of other children. (3A349)*

Reported improvements in safety (chapter 3) and increases in potential adult role models with stable jobs in the experimental and Section 8 groups' new neighborhoods provide a potentially favorable context for improving the labor market opportunities and attitudes of youth. Gender differences for youth in some mental health outcomes (chapter 4) and delinquency behaviors (chapter 5) imply likely more positive impacts of MTO on the labor market experiences of female youth than male youth.

7.5 Interim Employment and Earnings Impacts on Adults

Exhibit 7.3 shows estimates of the employment and earnings impacts of MTO on sample adults using respondent self-reports from the interim evaluation survey. There were no significant effects on the employment rates of adults in either the experimental or Section 8 groups at the time of the survey, or on the likelihood of being employed full-time (for 35 or more hours) at that time. There are also no statistically significant impacts of MTO on self-reported earnings (as measured by either annual earnings across all jobs in 2001 or current weekly earnings at the main job) or on the likelihood of employment in a job with weekly earnings above the poverty line. MTO does appear to have a marginally statistically significant positive impact on the likelihood of labor force participation (being employed or actively searching for work) of 3.8 percentage points (p=.058) for the experimental group and of 4.1 percentage points (p=.066) for the Section 8 group (appendix exhibit D7.1a).

The estimated impacts of MTO on adult employment and earnings in the first 4 years after random assignment using state administrative UI records are displayed in the top two panels of exhibit 7.4. The time patterns of employment outcomes with time from random assignment by treatment group are illustrated in more detail in exhibit 7.5. The overall message from the state UI data is similar to the basic results from the interim evaluation survey data. There are no statistically significant impacts of the experimental or Section 8 treatments on overall cumulative adult employment or on cumulative earnings for the first 4 years after random assignment. But the results from the UI records do hint at some differences in the impacts of MTO on employment with exposure to the treatment (time since random assignment). 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 2 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.

EXHIBIT 7.3
IMPACTS ON ADULT EMPLOYMENT AND EARNINGS, SURVEY DATA

Outcome	Control Mean	Experimental Vs. Control ITT[a]	Experimental Vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Employment [SR]					
Currently employed (n=3517)	0.522	0.014 (0.021)	0.030 (0.044)	0.026 (0.023)	0.044 (0.039)
Currently employed at a job offering health insurance (n=3483)	0.296	0.023 (0.019)	0.050 (0.041)	0.004 (0.021)	0.007 (0.035)
Currently employed full-time (35 or more hours at all jobs) (n=3488)	0.394	-0.001 (0.021)	-0.002 (0.044)	0.001 (0.023)	0.001 (0.038)
Currently employed at a job with weekly earnings above poverty (n=3311)	0.329	-0.008 (0.020)	-0.017 (0.043)	0.016 (0.022)	0.026 (0.037)
Adult Earnings [SR]					
Annual individual earnings in 2001 (n=3313)	$8,899	137 (449)	292 (957)	47 (495)	79 (829)
Current weekly earnings at main job (n=3311)	$182	-1 (9)	-3 (20)	-3 (10)	-4 (17)
Employed over one year at current main job (n=3475)	0.362	0.029 (0.021)	0.062 (0.045)	0.030 (0.022)	0.049 (0.037)

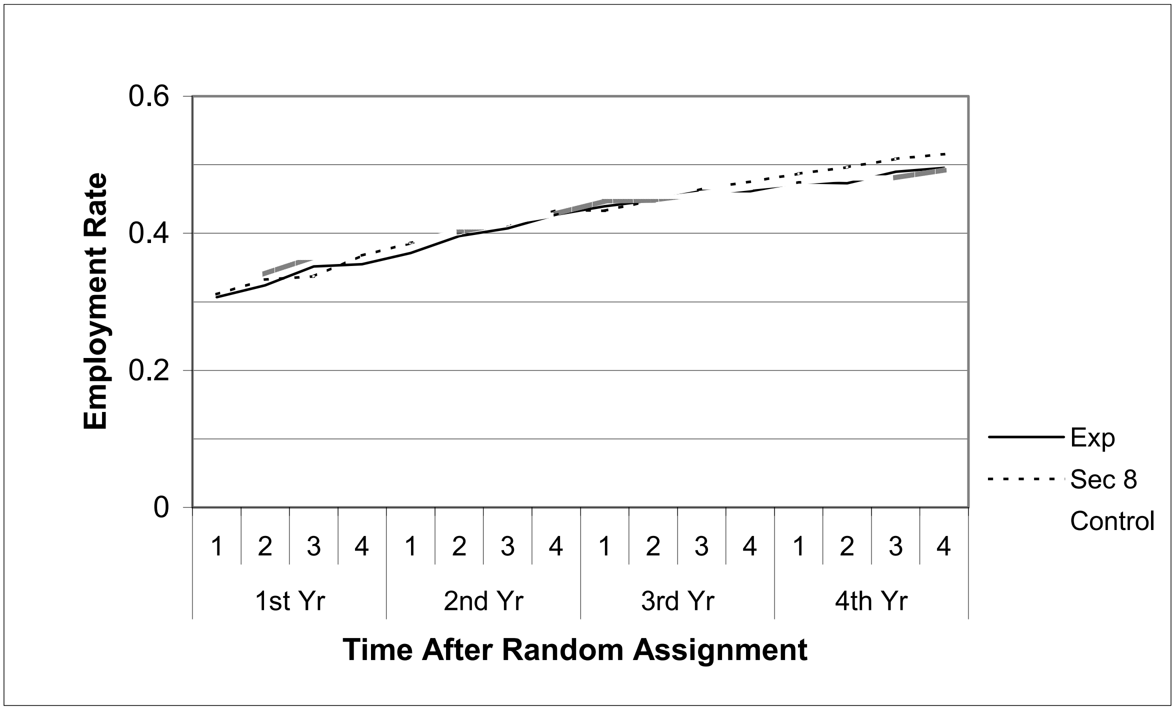
* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 7.4
IMPACTS ON EMPLOYMENT AND EARNINGS, ADMINISTRATIVE DATA

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Employment [ADMIN] (n=4070)					
Fraction of quarters employed, 1st and 2nd years after RA	0.386	-0.025* (0.012)	-0.053* (0.026)	-0.011 (0.013)	-0.017 (0.022)
Fraction of quarters employed, 3rd and 4th years after RA	0.473	-0.009 (0.014)	-0.019 (0.030)	0.008 (0.015)	0.013 (0.025)
Annualized fraction of quarters employed, 1st through 4th years after RA	0.430	-0.017 (0.012)	-0.036 (0.025)	-0.001 (0.013)	-0.002 (0.021)
Adult Earnings [ADMIN] (n=4070)					
Earnings, 1st and 2nd years after RA	$4835	-260 (245)	-552 (520)	-154 (282)	-251 (459)
Earnings, 3rd and 4th years after RA	$6859	-170 (310)	-360 (658)	45 (337)	73 (549)
Annualized earnings, 1st through 4th years after RA	$5847	-215 (254)	-456 (539)	-55 (288)	-89 (470)
Youth Employment: Fractions of Quarters Employed in 2001 [ADMIN] (n=2619)					
All youth (ages 14-19)	0.222	0.006 (0.014)	0.014 (0.031)	0.018 (0.015)	0.029 (0.025)
Girls	0.246	0.013 (0.020)	0.029 (0.044)	0.030 (0.025)	0.048 (0.039)
Boys	0.200	0.001 (0.019)	0.001 (0.044)	0.006 (0.021)	0.011 (0.036)
Youth Earnings During 2001 [ADMIN] (n=2619)					
All youth (ages 14-19)	$1366	77 (140)	173 (315)	170 (133)	277 (216)
Girls	$1369	172 (205)	385 (460)	260 (205)	410 (322)
Boys	$1361	-10 (165)	-23 (372)	87 (173)	146 (290)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Data sources: State administrative Unemployment Insurance (UI) records from Maryland, Massachusetts, Illinois, California and New York.
Samples: Adults and youth in families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Mean cell outcomes were regressed controlling for mean covariates. For adults, the covariates included site, quarter of randomization, baseline work status and baseline education. For youth, the covariates included site, age and gender. Standard errors were adjusted to account for the actual number of individuals. This method produces cell mean outcome estimates that correspond exactly to those using microdata outcomes and mean covariates.
e) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 7.5
ADULT EMPLOYMENT BY QUARTER SINCE RANDOM ASSIGNMENT AND TREATMENT GROUP (ADMINISTRATIVE DATA)



Sources: State administrative unemployment insurance (UI) records from California, Illinois, Maryland, Massachusetts, and New York.
Sample: Adults from families randomly assigned through December 31, 1997.

A similar pattern of improvement over time is observed in the comparison of the Section 8 group and the control group. The time pattern of the findings from the UI data suggests some short-run disruption effects of moving on employment outcomes in the first 2 years after random assignment, effects that dissipate over time. Modest improvements in labor market opportunities offset these disruptions for both the Experimental and Section 8 groups by 4 to 5 years after random assignment.[91] Again, however, the estimated employment impacts by the end of the observed period in the administrative data are not statistically different from zero.

As shown in exhibit 7.3, there were no significant impacts on the proportion of either the experimental group or the Section 8 group employed at the time of the survey, employed full-time or in jobs offering health insurance, or working at a job with weekly earnings above the poverty line.

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[91] Unreported results for the sub-sample of cases with administrative data available for the full first five years after random assignment indicate an improvement in the employment of the Experimental group relative to the controls of .026 (p=.074) from -.019 (se=.013) in the first two years after random assignment to .007 (se=.016) for the fourth and fifth years after random assignment. The analogous improvement for the Section 8 group is a statistically significant .037 (p=.016) from -.017 (se=.014) to .020 (se=.018).

MTO does appear to have increased the likelihood that members of the experimental group were employed in a white collar job (managerial, professional, or technical occupation) by 3.3 percentage points (p=.063, appendix exhibit D7.3). But there were no significant effects on annual earnings in 2001 or weekly earnings at the time of the survey.

7.6 Interim Employment and Earnings Impacts on Youth

Exhibit 7.6 summarizes the estimates of the effects of MTO on the labor force status and earnings of older youth aged 15 to 19 (as of May 31, 2001), based on interim evaluation survey data.[92] Some 27.5 percent of the youth in the control group were employed at the survey date and almost the exact same share were idle (neither in school nor working). MTO had no statistically significant effects on the youth employment rate, the idleness rate, or weekly earnings at the time of the survey. The state UI data similarly show no statistically significant overall effects of MTO on the employment or earnings of youth in calendar year 2001 (exhibit 7.4). There was, however, a statistically significant increase for experimental group youth in schooling as their major activity (being enrolled in school but not employed) of 6.2 percentage points (appendix exhibit D7.1b).

The differences in MTO effects on the mental health and risky behavior outcomes of boys and girls, documented in earlier chapters, motivated us to examine differences in MTO impacts on their labor force behavior. MTO does not have statistically significant effects on the employment rates of boys or girls in either survey or administrative UI data. But MTO does appear to have possible differential effects on the time allocated toward schooling and work of boys and girls. MTO substantially reduced the idleness rate of girls by 7.3 percentage points for the entire experimental group (p=.074) and by 15.7 percentage points for those in families who leased up (Exhibit 7.6). The idleness rate for male youth appears to be unaffected.

A more detailed decomposition of employment and enrollment status for youth (shown in appendix exhibit D7.1b) indicates that the decline in idleness for female youth is driven by a large and statistically significant increase in time allocation to schooling as the major activity, with a 10.2 percentage point increase(p=.020) in the share enrolled in school and not employed among all female youth in the experimental group (and 22.0 percentage point increase for those who leased up). The overall school enrollment rate for female youth in the experimental group increased by 7.2 percentage points (p=.096). The reductions in idleness and increases in time allocation to schooling of female youth in the experimental group are consistent with positive MTO impacts for this group on perceptions of their likelihood of going to college and getting a well-paid, stable job as an adult (appendix exhibit E6.4). Male youth in the experimental group did not experience similar effects on future expectations and if anything have lower (but statistically insignificant) perceptions of their likelihood of future educational and employment success.

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[92] Since few 14-year-olds participate in the formal labor market, we focus the analysis of youth employment outcomes using the interim evaluation survey data on those aged 15 to 19. The analysis of the UI administrative data includes youth aged 14 to 19 because the cell data from Massachusetts do not allow us to separate out the 14-year-olds.

For youth in Section 8 families, the only statistically significant effect was a 6.4 percent (p = .042) reduction in the share of girls employed and not in school. This decline in employment as the major activity for Section 8 girls was not associated with a significant increase in idleness because it was offset by a rise in schooling as a major activity.

EXHIBIT 7.6
IMPACTS ON YOUTH EMPLOYMENT AND EARNINGS, SURVEY DATA

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Youth Employed [SR] (n=1581)					
All youth (Ages 15-19)	0.275	-0.028	-0.065	-0.021	-0.038
		(0.030)	(0.070)	(0.033)	(0.060)
Girls	0.320	-0.026	-0.057	-0.060	-0.101
		(0.046)	(0.098)	(0.047)	(0.078)
Boys	0.229	-0.029	-0.073	0.021	0.042
		(0.039)	(0.098)	(0.044)	(0.090)
Youth Current Weekly Earnings at Main Job [SR] (n=1531)					
All youth (Ages 15-19)	$55	-5	-12	-4	-7
		(8)	(18)	(9)	(17)
Girls	$59	1	3	-14	-23
		(12)	(26)	(12)	(20)
Boys	$50	-12	-29	6	13
		(10)	(24)	(13)	(27)
Youth Idle (neither employed nor in school)[SR] (n=1587)					
All youth (Ages 15-19)	0.277	-0.033	-0.077	0.003	0.006
		(0.029)	(0.067)	(0.032)	(0.058)
Girls	0.266	-0.073	-0.157	0.012	0.021
		(0.041)	(0.088)	(0.043)	(0.071)
Boys	0.288	0.006	0.015	-0.006	-0.012
		(0.040)	(0.100)	(0.044)	(0.090)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Youth survey.
Sample: Youth from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

7.7 Interpretation of Results

This chapter examined the effects of MTO on the labor market outcomes of sample adults and older youth using respondent self-reports from the interim evaluation survey and administrative data from state UI records. Although MTO increased the likelihood of families living in neighborhoods that appear to have better employment opportunities and norms towards work, both data sources indicate no significant overall impacts of MTO on the employment rates or earnings of adults and of older youth.

The most encouraging finding here is the reduction in the share of female youth who were idle (neither currently employed nor enrolled in school), raising the share of full-time students (enrolled in school and not employed) by a very substantial 22 percentage points among girls in families who leased up. This impact has the potential for longer term positive effects for these girls.

Chapter Eight
Impacts on Income and Receipt of Public Assistance

This chapter presents interim findings on MTO's impacts on household income, public assistance receipt, and poverty status. We begin by discussing the relationship between MTO participation and household income and receipt of public assistance. We then describe the data sources and measures used. The next section provides context, by discussing the baseline status of sample members with respect to income and public assistance receipt, and by presenting impacts on mediators—that is, intermediate outcomes that might help explain MTO's impacts on income and public assistance. Then we present impacts on the outcomes of interest. The final section discusses the extent to which the results are consistent with expectations, and the implications of the findings.

Summary

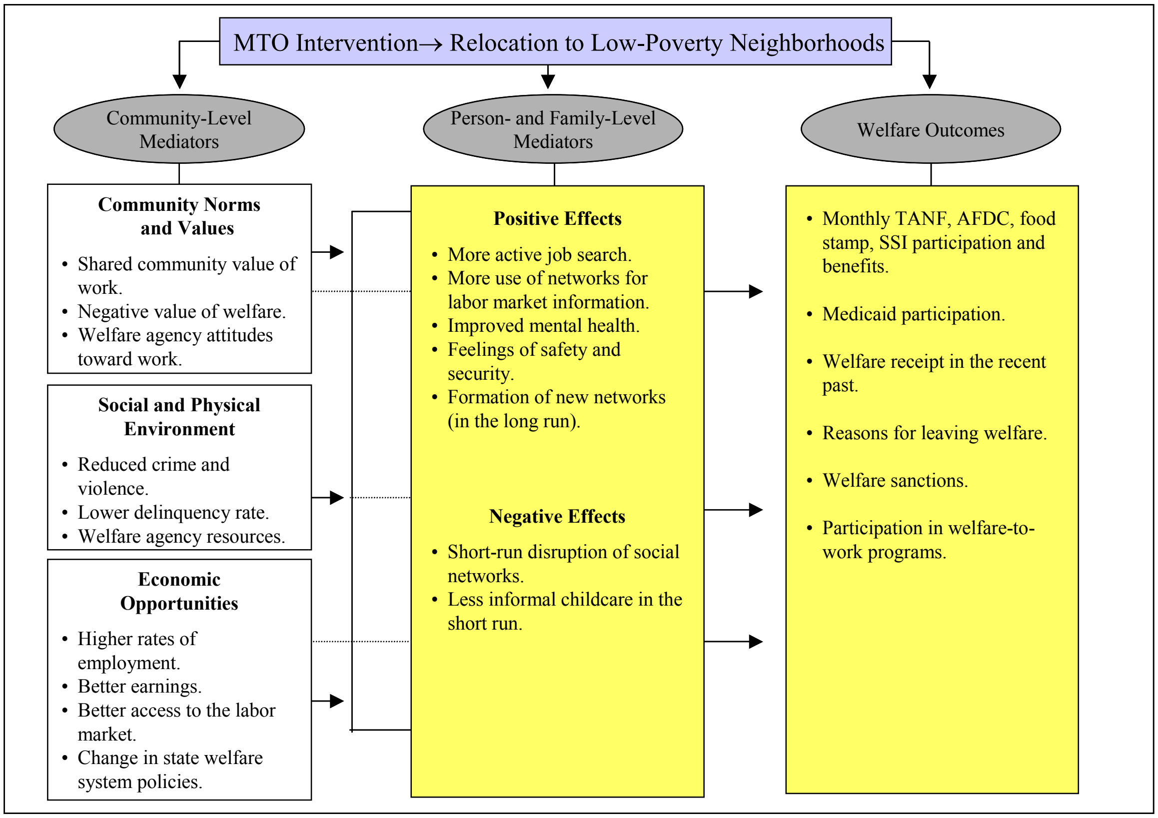
Consistent with the findings of no effects on employment and earnings in the previous chapter, we find no evidence that MTO reduced public assistance receipt or increased average household income, income relative to poverty, or food security. There is also no evidence that any of the subgroups examined experienced reductions in welfare benefits relative to controls. Most of the estimated impacts were small in size (much less than 10 percent of the control group average), so that even if the estimated impacts had been statistically significant they might not be important in a policy sense. The few statistically significant estimated impacts indicated increases in welfare receipt.

8.1 Hypotheses about MTO's Impacts on Public Assistance Receipt and Income

The impacts of MTO on welfare receipt are likely to be the mirror image of the impacts on employment and earnings—to the extent that MTO increases employment, it can be expected to reduce participation in such income-tested programs as Aid to Families with Dependent Children (AFDC), Temporary Assistance to Needy Families (TANF), food stamps, Supplemental Security Income (SSI), and Medicaid (see Exhibit 8.1). As we saw in Chapter 7, however, there had been no significant impacts on employment and earnings by the time of the interim evaluation. Therefore, it seems unlikely that we will see effects on receipt of public assistance.

The likely effects of MTO on household income depend, in turn, primarily on the effects on earnings and public assistance, because these are the main components of income for most sample members. If the demonstration interventions did not affect receipt of public assistance, then, given the lack of effect on employment and earnings we would expect little or no effect on household income.

EXHIBIT 8.1
WELFARE AND OTHER TRANSFER PROGRAMS OUTCOMES AND MEDIATING FACTORS



Previous analyses of data for MTO enrollees in Boston and Baltimore provided mixed evidence about MTO's impacts on welfare receipt. In Baltimore Ludwig et al. (2000) found that MTO reduced welfare receipt for the experimental group by an average of 6 percentage points (equal to about 15 percent of the average receipt rate for the control group) over a 3-year followup period, but there was no evidence of an impact on welfare receipt for the Section 8 group. In Boston Katz, Kling, and Liebman (2001) found no evidence of reductions in welfare receipt for either the experimental or Section 8 group over a 2-year followup period.

8.2 Data Sources and Measures

This chapter assesses the impact of MTO on public assistance receipt and income based on respondent self-reports from the interim survey and administrative records from state TANF agencies. Sample adults in the interim survey were asked about current receipt of TANF, food stamps, SSI, and Medicaid, and also about their total household income in 2001 (the calendar year preceding the survey). The survey provided two measures of income: one based on a single question about the total combined income of all members of the household in 2001, and the other constructed from a series of questions about specific sources of income for each member of the household (i.e., earnings, self-employment income, government benefits, and other sources). This chapter presents results for the first measure of income; results for the second measure are presented in an appendix. Data from the interim survey were also used to estimate impacts on food security and a measure of self-sufficiency (the proportion of sample adults who were working and off TANF at the time of the survey).

Using administrative records, this chapter presents estimated impacts on the proportion of sample members receiving benefits and the amount of benefits received from both AFDC/TANF and food stamps. State welfare agencies in all five sites provided these data, matched to sample members by social security numbers. Because the data received from several states did not cover the entire followup period, we present results for sample members from all five sites only in the fifth year after random assignment.[93] In addition, we present results on AFDC/TANF receipt and benefit amounts for 5 years after random assignment for sample members in three sites, and results on food stamp receipt and benefit amount for sample members in two sites.[94] In both cases, the results are based on the subsample for whom data were available for the entire 5-year period. All benefit amounts are measured in 2001 dollars.

Additional questions from the interim survey and census data on the characteristics of current neighborhoods were examined as mediating factors to help explain the impact results for public assistance receipt and income and to assess the hypotheses discussed in Section 8.1. Many of the same mediating factors that are likely to affect labor market outcomes may also affect public assistance receipt and income. These include labor market opportunities and access to jobs, community norms with respect to employment and receipt of welfare, sense of physical safety in neighborhood, and any effects on physical and mental health. In addition, income and public assistance outcomes are likely to be mediated by the families' social networks. Impacts on these mediating variables were presented in previous chapters.[95]

8.3 Baseline Income and Public Assistance Status and Control Group Context

This section provides context for the impact results in the next section by presenting baseline characteristics for the full sample, showing the time path of welfare receipt for the control group, and summarizing impacts on hypothesized mediating outcomes.

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93 Several sites provided little or no data in the years immediately following random assignment. By followup year 5, we had data on a large portion of our sample from all five sites. Even the year 5 data, however, is available for only a subset of our observations: We have TANF data for 2,984 of the 4,248 sample adults, and we had food stamp data for 2,710.

94 Five years of AFDC/TANF data are available for New York, Chicago, and Boston. Five years of food stamp data are available for Chicago and Boston only. Los Angeles and Illinois did not provide 5 years of administrative data for any sample members for either program.

95 Chapter 7 discussed impacts on employment-related mediators, chapter 2 presented effects on community norms, chapter 3 covered impacts on sample members' sense of safety and neighborhood conditions, and chapter 4 presented effects on physical and mental health.

Baseline characteristics

A snapshot of the sample at the time of random assignment provides a useful context for understanding the impact results presented later in this chapter. This section addresses the baseline characteristics most relevant to the impacts shown in this chapter. Previous chapters provide results for a range of other baseline measures. Exhibit 8.2 shows characteristics separately for the experimental, Section 8, and control groups, although random assignment guarantees that only chance differences exist at baseline.

At the time they were randomly assigned, the MTO adult sample members had very high rates of public assistance receipt, low rates of employment, and average incomes well below the poverty line. Approximately three out of four sample members were receiving AFDC at baseline, and four out of five were receiving food stamps. Further, nearly all sample adults (93 percent) had received AFDC at some point. Consistent with these high rates of welfare receipt, only one of four sample members was working at baseline. Average income was about $9,300, well below the poverty line for a family of three. Median income was still lower, approximately $7,700. These results show that sample members were quite disadvantaged when they entered the MTO demonstration.

EXHIBIT 8.2
SELECTED BASELINE CHARACTERISTICS: WORK, PUBLIC ASSISTANCE, AND INCOME

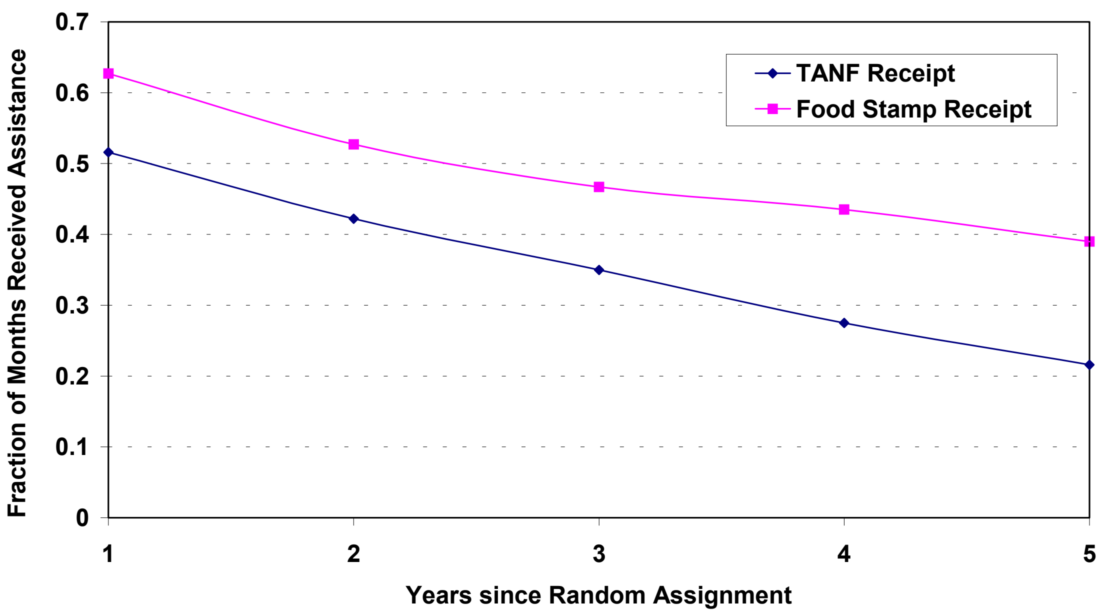
Characteristic	Experimental	Section 8	Control
Employment Status at Baseline			
Working	25.7	25.6	24.2
Not working, but previously worked	54.1	57.3	57.2
Never worked for pay	20.2	17.1	18.6
Public Assistance Receipt at Baseline			
Receiving AFDC	74.4	75.3	74.5
Receiving food stamps	80.3	81.0	80.1
Ever received AFDC	93.5	92.6	92.1
Working and not receiving AFDC	16.6	15.9	15.9
Receiving WIC	34.5	35.6	34.8
Receiving SSI	18.3	17.3	17.6
Receiving SSDI	9.6	8.0	8.4
Household Income at Baseline			
Average income	$9,385	$9,189	$9,337
Median income	$8,064	$7,536	$7,824

Source: Participant baseline survey.
Sample: Adults
Notes: Data are weighted as described in appendix B.

Receipt of public assistance over time for the control group

Impacts are measured as the average outcomes for the treatment groups (experimental or Section 8) relative to the outcomes for the control group. The control group's experience over time therefore represents the standard against which outcomes for the treatment groups are evaluated. In the three sites for which we have AFDC/TANF data for the entire followup period, the administrative data show high initial rates of receipt (consistent with the baseline survey), and a steady decline in receipt over time. Over the followup period, AFDC/TANF receipt rates fell by more than half. This pattern of declining receipt is typical for a cohort of individuals who were all initially receiving benefits. Food stamp receipt exhibits a similar pattern (shown for two sites), except that initial receipt rates were higher and the decline was more gradual (exhibit 8.3). In order for MTO to reduce welfare receipt, it would be necessary for the receipt rates of the treatment groups not only to fall over time but to fall by more than the rate for the control group.

Exhibit 8.3
Receipt of Public Assistance Over Time for the Control Group



Note: TANF receipt rates based on data from New York, Chicago, and Boston.
Food stamp receipt rates based on data from Chicago and Boston.

8.4 Impacts On Hypothesized Mediators

The hypotheses discussed earlier in this chapter suggest that MTO might affect welfare receipt and income by inducing experimental and Section 8 group members to move to neighborhoods that have better employment opportunities, are safer, and have norms more supportive of work and less accepting of welfare. In fact, MTO has produced such effects. On average over the followup period, the experimental and Section 8 groups moved into neighborhoods with substantially lower poverty rates, higher adult employment rates, and a higher proportion of two-parent families than the control group (exhibit 2.10). MTO-induced mobility also led to improvements in neighborhood safety (exhibit 3.3) and in some indicators of mental health (exhibit 4.1). The reduction in neighborhood poverty, however, did not produce significant impacts on employment and earnings, which are the most direct mediators of public assistance receipt and income. Further, the demonstration did not significantly strengthen social networks or improve adult access to transportation (which was reportedly very high for all).

8.5 Interim Impacts on Public Assistance Receipt and Income

This section summarizes impact results for public assistance receipt, household income, food security, and a measure of self-sufficiency.

Impacts on public assistance receipt

We find no evidence of MTO impacts on public assistance receipt over the followup period through the interim data collection. AFDC/TANF receipt rates at the time of the survey were two to four percentage points lower for the experimental and Section 8 groups compared to the control group, but this difference was not statistically significant (exhibit 8.4, top panel). Estimated impacts on current receipt of food stamps, SSI, and Medicaid were also not significant, and they varied in sign. Five years after random assignment, estimated impacts on welfare benefits were similarly not significant (exhibit 8.4, bottom panel). Estimated impacts on TANF and food stamp receipt and payments were of mixed sign and small in size for both the experimental and Section 8 groups.

Impacts on AFDC/TANF receipt over time, for the three states where we have 5 years of followup data, do not demonstrate a clear trend, although there is some evidence MTO increased AFDC/TANF receipt and the amount of benefits received in the experimental group during the first 4 years after random assignment (exhibit 8.5). A similar pattern is found for receipt of food stamp benefits (exhibit 8.6). For the same subsample of persons with 5 years of followup data, we see a different trend in impacts for the Section 8 group. For this group there were insignificant impacts in years 1 and 2; however, in later years, receipt rates for AFDC/TANF and food stamps were significantly higher for the Section 8 group than for the control group (exhibits 8.5 and 8.6).[96] Note, however, that the

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[96] Specifically, for the Section 8 group compared to the control group, AFDC/TANF receipt rates were higher in years 3 and 4 combined and in year 5. Food stamp receipt rates were higher in years 3 and 4, and total food stamp benefits received were higher in year 5.

significant impacts on public assistance in the Section 8 group were not found in the five-site sample in year 5.

Subgroup impacts on total AFDC/TANF plus food stamp payments were also not significant (exhibit 8.7). Impacts were estimated for subgroups defined by race and ethnicity and by several measures of barriers to employment—welfare history, education, employment status at baseline, and access to a car. The results provide no evidence that MTO had different welfare receipt impacts on sample adults who varied according to these barriers or by race and ethnicity.

Impacts on household income

Consistent with the general lack of impacts on earnings and public assistance, MTO produced no significant impacts on household income. Total household income for the control group in 2001 was approximately $15,500 per household, and estimated impacts for the experimental and Section 8 groups was $500 or less.[97] Results for income of the sample adult alone, the sample adult and spouse combined, and the distribution of income relative to the poverty line also revealed no evidence of impact (exhibit 8.8).

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[97] Results for the other measure of household income presented in appendix exhibit D8.1 also show no impacts on income. We also examined impacts on different locations on the distribution of both the self-reported and constructed total household income measures, using quantile regressions to estimate impacts at the 25th percentile, the median, and the 75th percentile of each distribution. These results, shown in appendix exhibit D8.2, also showed no evidence of impacts.

EXHIBIT 8.4
HOUSEHOLD RECEIPT OF PUBLIC ASSISTANCE – ALL SITES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Proportion Receiving Benefits at Time of Survey [SR]					
AFDC/TANF (n=3509)	0.286	-0.023 (0.019)	-0.049 (0.040)	-0.035 (0.021)	-0.058 (0.034)
Food stamps (n=3514)	0.460	-0.011 (0.021)	-0.023 (0.044)	0.002 (0.023)	0.003 (0.038)
SSI (n=3511)	0.228	0.020 (0.016)	0.043 (0.035)	0.025 (0.019)	0.041 (0.031)
Medicaid (n=3468)	0.561	-0.030 (0.021)	-0.064 (0.045)	-0.042 (0.022)	-0.071 (0.038)
AFDC/TANF [ADMIN]					
Fraction of months Sample adult received AFDC/TANF, year 5 (n=2984)	0.255	-0.006 (0.016)	-0.013 (0.035)	0.028 (0.019)	0.044 (0.030)
Total AFDC/TANF payments received by sample adult, year 5 (n=2984)	$1,264	-$33 (93)	-$71 (205)	$49 (102)	$77 (161)
Fraction of months any sample adult received food stamps, year 5 (n=2710)	0.435	0.012 (0.019)	0.026 (0.041)	0.034 (0.021)	0.052 (0.032)
Total food stamp payments received by sample adult, year 5 (n=2710)	$1,249	-$22 (65)	-$48 (139)	$117 (76)	$178 (116)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey, administrative data from state welfare agencies. See appendix A for details.
Sample: Adults from families randomly assigned by December 31, 1997. Administrative outcomes: sample adults (and their households), data from the 49th to the 60th month after random assignment, all sites.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b)Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 8.5
RECEIPT OF AFDC/TANF: TRENDS OVER TIME – THREE SITES WITH LONGITUDINAL DATA

Outcome	Control Mean	Experimental Vs. Control ITT[a]	Experimental Vs. Control TOT[a]	Section 8 Vs. Control ITT[a]	Section 8 Vs. Control TOT[a]
AFDC/TANF [ADMIN]					
Fraction of months sample adult received AFDC/TANF, year 5 (n=1962)	0.216	0.009 (0.019)	0.023 (0.050)	0.048* (0.024)	0.084* (0.042)
Total AFDC/TANF payments to sample adult, year 5 (n=1962)	$1,037	$2 (107)	$6 (280)	$37 (121)	$64 (210)
Fraction of months sample adult received AFDC/TANF, years 3 and 4 (n=2934)	0.313	0.035* (0.016)	0.084* (0.039)	0.044* (0.018)	0.081* (0.033)
Total AFDC/TANF payments to sample adult, years 3 and 4 (n=2934)	$3,144	$184 (191)	$443 (460)	$74 (206)	$137 (378)
Fraction of months sample adult received AFDC/TANF, years 1 and 2 (n=2632)	0.469	0.036* (0.017)	0.086* (0.041)	0.027 (0.019)	0.048 (0.034)
Total AFDC/TANF payments to sample adult, years 1 and 2 (n=2632)	$4,522	$485* (201)	$1,160* (481)	$249 (217)	$440 (384)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Administrative data from state welfare agencies. See appendix A for details.
Sample: Sample adults from three sites: New York, Chicago, and Boston.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b)Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 8.6
RECEIPT OF FOOD STAMPS: TRENDS OVER TIME – TWO SITES WITH LONGITUDINAL DATA

Outcome	Control Mean	Experimental Vs. Control ITT[a]	Experimental Vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Food Stamps [ADMIN]					
Fraction of months sample adult received food stamps, year 5 (n=1423)	0.390	0.034 (0.025)	0.090 (0.066)	0.052 (0.029)	0.082 (0.046)
Total food stamps payments to sample adult, year 5 (n=1423)	$1,056	$95 (81)	$253 (214)	$213* (101)	$334* (158)
Fraction of months sample adult received food stamps, years 3 and 4 (n=1853)	0.451	0.032 (0.020)	0.083 (0.052)	0.054* (0.022)	0.092* (0.037)
Total food stamps payments to sample adult, years 3 and 4 (n=1853)	$2,491	$206 (140)	$532 (362)	$271 (158)	$460 (268)
Fraction of months sample adult received food stamps, years 1 and 2 (n=1853)	0.577	0.037* (0.017)	0.096* (0.044)	0.030 (0.020)	0.051 (0.034)
Total food stamps payments to sample adult, years 1 and 2 (n=1853)	$3,043	$297* (118)	$768* (306)	$194 (139)	$329 (235)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Administrative data from state welfare agencies. See appendix A for details.
Sample: Sample adults. Data from two sites: Chicago and Boston.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 8.7
SUBGROUP IMPACTS ON TOTAL AFDC/TANF PLUS FOOD STAMP PAYMENTS, YEAR 5

Subgroup	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Race/Ethnicity [ADMIN]					
Black (n=2925)	$1,590	-$26	-$56	$20	$29
		(109)	(237)	(124)	(182)
Non-Black (n=2925)	$1,513	-$37	-$80	-$161	-$301
		(201)	(437)	(210)	(393)
Hispanic (n=2952)	$1,648	-$4	-$9	-$353	-$649
		(228)	(484)	(231)	(426)
Non-Hispanic (n=2952)	$1,534	-$33	-$73	$50	$75
		(105)	(230)	(117)	(177)
Barriers to Employment at Baseline [ADMIN] (n= 2984)					
Receiving AFDC/TANF at random assignment	$1,944	-$96	-$199	-$30	-$46
		(120)	(249)	(131)	(199)
Had no high school diploma	$1,781	-$67	-$148	-$130	-$213
		(128)	(282)	(139)	(229)
Not working at random assignment	$1,816	-$73	-$159	-$51	-$80
		(117)	(256)	(129)	(204)
Did not have car that ran at random assignment	$1,634	-$41	-$92	-$100	-$159
		(103)	(233)	(115)	(182)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Household survey, other locating data, 2000 Census tract-level data. See appendix A for details.
Sample: Adults from families randomly assigned by December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT 8.8
IMPACTS ON INCOME, FOOD SECURITY, AND SELF-SUFFICIENCY

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Total Income in 2001 [SR]					
Total income of sample adult (n=3365)	$11,890	$442 (427)	$938 (906)	$101 (482)	$169 (811)
Total income of sample adult and current spouse (n=3261)	$13,166	$322 (600)	$683 (1,273)	-$436 (665)	-$729 (1,111)
Household total income (n=3211)	$15,536	$239 (571)	$505 (1,205)	-$162 (636)	-$271 (1,062)
Poverty Status [SR] **(n= 3526)**					
Percent of households <50% of poverty line in 2001	0.347	-0.016 (0.020)	-0.035 (0.043)	0.017 (0.023)	0.028 (0.038)
Percent of households 50 to 99% of poverty line in 2001	0.326	0.018 (0.021)	0.039 (0.044)	0.006 (0.023)	0.010 (0.038)
Percent of households 100 to 149% of poverty line in 2001	0.170	-0.005 (0.017)	-0.011 (0.036)	-0.026 (0.017)	-0.044 (0.028)
Percent of households >150% of poverty line in 2001	0.157	0.003 (0.015)	0.007 (0.032)	0.004 (0.017)	0.006 (0.028)
Food Security [SR] **(n=3519)**					
Percent of households food insecure with hunger	0.111	-0.022 (0.013)	-0.046 (0.028)	-0.003 (0.015)	-0.004 (0.025)
Self-Sufficiency [SR] **(n= 3472)**					
Percent of sample adults working and off TANF in 2001	0.452	0.018 (0.020)	0.038 (0.043)	0.019 (0.023)	0.032 (0.038)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Adult survey. See appendix A for details.
Sample: Adults from families randomly assigned by December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for more details.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

Impacts on other outcomes

MTO also had no effects on measures of food security and self-sufficiency (exhibit 8.8, bottom panel). The proportion of households who experienced hunger[98] in the year prior to the survey was 20 percent lower in the experimental group than the control group (8.8 compared to 11.0 percent), but the difference was not significant. For the Section 8 group the point estimate was very close to zero. Almost one-half of the control group was working and not receiving TANF at the time of the survey, about two percentage points less than the estimates for the experimental and Section 8 groups.

8.6 Interpretation of Results

MTO has so far not caused any significant reductions in public assistance receipt or increases in household income for either the experimental group or the Section 8 group. If anything, it may have increased welfare receipt in the first 4 years after random assignment. The absence of impacts is consistent across outcome measures and subgroups, and it is also consistent with the lack of impacts on employment and earnings shown in chapter 7.

These results may seem somewhat surprising, given the differences that MTO produced in the neighborhoods of the experimental and Section 8 groups. Neighborhoods with lower poverty rates and higher adult employment rates (neighborhoods that were perceived as safer) apparently had little influence on employment or welfare receipt for sample members and therefore not on income either.

One interpretation of these findings is that place alone is not enough to produce substantial changes in employment and welfare receipt, or that the sample's exposure to different places was not sufficient to affect these changes. It is possible that combining better neighborhoods with other policies (such as employment assistance or training, financial work incentives, affordable high-quality child care) would have larger effects. It is also possible that larger changes in family environment (through a greater proportion of moves to low-poverty neighborhoods) would produce increases in employment and reductions in welfare receipt.[99] Or participants may need more time to adjust to new neighborhoods and take advantage of greater opportunities. Additional analysis and longer followup may shed further light on the results.

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[98] The interim survey measured hunger using a six-item scale developed by Abt Associates for the U.S. Department of Agriculture. For a detailed explanation see Bickel et al. (2000).

[99] Higher leaseup rates would not be expected to produce larger TOT impacts but could improve the precision of the impact estimates and thereby increase the likelihood of detecting small to moderate effects.

Chapter Nine
Summary and Implications of the Estimated Impacts of MTO

This chapter summarizes the impact estimates presented in the previous chapters, assesses the size and statistical significance of those estimates, as well as the likelihood that further impacts will emerge before the final evaluation 10 years after random assignment, and discusses the implications of the findings for policy.

Summary of Impact Estimates

In chapters 2 through 8, we presented estimates of the impact of MTO on the mobility of participating families and the characteristics of the neighborhoods in which they lived, as well as on outcomes in six aspects of their lives:

- Housing conditions and receipt of housing assistance.
- Adults' and children's mental and physical health.
- Delinquency and risky behavior among youth.
- The schools attended by children in the sample and their educational achievement.
- Employment and earnings of adults and youth.
- Household income and receipt of public assistance.

In this section, we review the impacts in each of these areas.

Impacts on residential location, housing quality, and receipt of housing subsidies

MTO had substantial, positive effects on the mobility of families in the treatment groups and on the characteristics of the neighborhoods in which they lived. Almost half of the families assigned to the experimental group were able to use their program vouchers, as were over three-fifths of the families in the Section 8 group (exhibit 2.1). In order to use the voucher, experimental group families were required to move to census tracts with poverty rates below 10 percent in the 1990 Census. Because many moved to neighborhoods where the poverty rate was increasing between 1990 and 2000, we estimate that only about half of their destinations had poverty rates below 10 percent at the time of the move, although virtually all had rates below 20 percent (exhibit 2.3). Among the Section 8 group, who could use the voucher anywhere they could find housing that met Section 8 quality standards, less than 30 percent of those who leased housing units with program vouchers moved to census tracts with poverty rates below 20 percent, although the overwhelming majority moved to neighborhoods with lower poverty rates than those where they had lived in public housing.

As noted earlier, the experimental families were only constrained to live in low-poverty areas for one year. By the time of the interim evaluation, these differentials in poverty rates had narrowed somewhat, in part because of subsequent moves by the experimental families and in part because of changes over time in neighborhood poverty rates, but they had not disappeared entirely. Among those who moved with program vouchers, 60 percent of the experimental group families were still in census tracts with poverty rates below 20 percent, while 30 percent of the Section 8 families were in such tracts (exhibit 2.5). The treatment-control differentials had narrowed as well, partly as a result of changes in the poverty rates of the neighborhoods where treatment group families resided, but also because over two-thirds of the control families had moved to housing in different locations—some to private housing, with or without Section 8 subsidies, and some to other public housing units.[100] By the time of the interim evaluation, about 17 percent of the control group families lived in census tracts with poverty rates below 20 percent and just over half lived in tracts with rates below 40 percent.

It is noteworthy, however, that even those treatment group families who moved to low-poverty areas did not necessarily move to predominantly white or racially integrated areas. Among families in the Section 8 group, at the time of the interim evaluation over three quarters of both those who moved with program vouchers and those who did not were living in census tracts that were over 80 percent minority, about the same proportion as among control families (exhibit 2.6). Among experimental group families, 60 percent of those who moved with program vouchers were in heavily minority areas. For minority families in the experimental group who moved with program vouchers, the experiment reduced the average percent minority in their neighborhood by less than 10 percentage points. There was no significant effect on this measure for the Section 8 group (exhibit 2.8).

These mobility patterns resulted in a number of significant improvements in the environment in which experimental group families lived and lesser improvements for Section 8 group families. Relative to the control group, MTO reduced the proportion of the followup period that such families spent in areas of concentrated poverty by 47 percentage points in the experimental group and 35 percentage points in the Section 8 group (exhibit 2.9). It increased the proportion of time spent in areas with poverty rates below 20 percent by 53 percentage points among families in the experimental group. Section 8 families were more likely to locate in moderate-poverty areas; the demonstration increased the proportion of time these families spent in areas with poverty rates below 30 percent by 31 percentage points.

At the time of the interim evaluation, experimental group families who moved with program vouchers lived in neighborhoods with higher adult employment rates, a substantially higher proportion of two-parent families and high school graduates, and nearly twice as many homeowners as in the neighborhoods they would have lived in absent the demonstration (exhibit 2.10). Section 8 group families who moved with program vouchers also saw significant improvements in these neighborhood attributes, relative to the control group, although those gains were generally only about half as large as those experienced by the experimental group families.

These environmental changes substantially increased the chances that adults in experimental group families would have college-educated friends or friends earning $30,000 or more (exhibit 2.10).

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[100] It should be noted that some of these moves may have been associated with the redevelopment of public housing projects as part of the HOPE VI program.

There was no significant effect on these outcomes for adults in the Section 8 group who lived in somewhat higher poverty areas than the families in the experimental group.

The families who moved with program vouchers also markedly improved the quality of their physical and social environments, reporting large reductions in the presence of litter, trash, graffiti, abandoned buildings, people hanging around, and public drinking, relative to the control group (exhibit 3.5). They also reported that they had less difficulty getting police to respond to their calls. The proportion of families who expressed satisfaction with their current neighborhoods was much higher in both treatment groups than in the control group. On every one of these measures, the proportion of the experimental group reporting improved conditions was about 10 percentage points larger among the Section 8 group.

Perhaps most notable from the perspective of the families themselves is the fact that they were successful in achieving the goal that loomed largest in their motivation to move out of their old neighborhoods—substantial increases in their perceived safety in and around their homes and large reductions in the likelihood of observing or being victims of crime (exhibit 3.5). These gains were greater for the experimental group families, but they were still substantial for those in the Section 8 group who moved with program vouchers.

As with the effects on neighborhood, MTO substantially improved the quality of housing occupied by the families who moved with program vouchers. A markedly higher proportion of families in both treatment groups voiced satisfaction with their housing at the time of the interim evaluation than in the control group (exhibit 3.5). It also substantially reduced the fraction living in public housing, while increasing somewhat the proportion of families receiving housing subsidies. It should be noted that, even without an MTO voucher, over two-thirds of the families in the control group left their original public housing project during the followup period, either through their own efforts or because their units were renovated or demolished by Federal and local programs. Even so, at the time of the interim evaluation, controls lived in substantially less desirable housing treatment group members, both in terms of measurable characteristics of the housing and in terms of the families' satisfaction with their housing.

In sum, the demonstration succeeded in substantially improving the housing and residential environment of the families who moved with program vouchers on a wide range of measures. While these improvements were greater for the experimental group, who were constrained to move to low-poverty areas, at least initially, the Section 8 group also experienced sizeable environmental improvements, relative to the control group.

Impacts on adults' and children's health

While health data were not collected at baseline, it seems reasonable to suppose that participating families were subject to a wide range of health conditions and risks. At baseline, over one-third of the families reported serious problems with rats or mice and a quarter said that a family member had been beaten or assaulted in the prior 6 months. More generally, urban residents of high-poverty neighborhoods are likely to have high rates of obesity, hypertension, substance abuse, asthma, depression, and exposure to violence. The high rates of activity limitations, asthma, high blood pressure, obesity, psychological distress, depression, and anxiety observed in the control group at the time of the interim evaluation bear out these expectations (exhibit 4.2).

Estimation of impacts on these outcomes and on measures of smoking, drinking, and general physical health revealed one significant impact on adults' physical health: a large reduction in the incidence of obesity, among adults in the experimental group (exhibit 4.2). There was also a substantial reduction in psychological distress among adults in the experimental group families, but not among those in the Section 8 group. There were no significant effects on the other measures of adult health measured in the interim evaluation.

Among children, the significant effects of MTO on health were confined to mental health measures—a moderately large reduction in psychological distress for girls in the experimental group; a substantial decrease in the incidence of depression among girls in the Section 8 group; and very large reductions in the incidence of generalized anxiety disorder among girls in both treatment groups (Exhibit 4.5).

These findings of significant impacts on measures of mental health, especially for the experimental group, are consistent with the improvements in the families' perceptions of personal safety discussed above.

Impacts on delinquency and risky behavior among youth

Children who were ages 15 to 19 at the time of the interim evaluation were between the ages of 8 and 15 at baseline. Even so, significant proportions had already exhibited problem behavior or been suspended from school. By the time of the interim evaluation, among youth this age, 24 percent of the girls and 39 percent of the boys in the control group had been arrested—over half of them for violent crimes (exhibit 5.3).

In the interim evaluation, survey data from parents and from the youth themselves were used to measure a number of delinquent, risky, and problem behaviors. The youth were asked whether they had ever been arrested. In addition, administrative data from the criminal justice system were used to measure the number of arrests for specific crimes.

For boys and girls ages 12 to 19 in both treatment groups, there were no significant effects on either an index of 15 problem behaviors reported by parents or on a narrower index of self-reported delinquent behaviors related to criminal behavior (exhibit 5.2). However, there were significant increases in self-reported behavior problems among boys ages 12 to 19, in both treatment groups.

Participation in MTO resulted in a large reduction in the proportion of girls ages 15 to 19 in the Section 8 group who had ever been arrested for violent crimes (Exhibit 5.3). This effect contributed to a significant reduction in the frequency of arrests for violent crimes for all youth (Exhibit 5.4). There were no effects on the incidence of arrests for other crimes for girls. The only effects on arrests for boys were very substantial increases in the proportion ever arrested and the frequency of arrests for property crimes in the experimental group (Exhibits 5.3 and 5.4). This increase in arrests might reflect more stringent policing in new locations, rather than (or in addition to) more criminal behavior.

For girls ages 15 to 19 in the experimental group, but not for those in the Section 8 group, there were reductions in risky behavior, concentrated in marijuana use and smoking. Among boys in this age range in both treatment groups there were significant increases in smoking, but not in other types of risky behavior (exhibit 5.5).

This pattern of gender differences in effects—positive for girls and negative for boys—suggests that boys and girls react differently to the disruption of moving and the challenge of integrating into a new environment. However, the available results do not allow us to say specifically why this is the case. To the extent that this difference reflects a response to the transition from a high-poverty environment to a lower poverty environment, one might expect this pattern to be different in the longer term for youths who have completed that transition or who have grown up in the new environment.

Impacts on children's education

For the interim evaluation, education research focused on children ages 5 to 19 at the time the data were collected. We interviewed parents about the school-related attitudes, behaviors, and performance of all children in the sample. We interviewed children ages 8 to 19 about their own views and experiences in school. We also administered four achievement different tests from the Woodcock -Johnson Battery-Revised to all sample children ages 5 to 19 and collected data from published sources about the schools the children attended.

MTO had significant but small effects on the characteristics of the schools sample children attended (exhibit 6.3). Experimental group children attended schools with somewhat lower percentages of poor and minority children and of students with limited English proficiency than they would have in the absence of the demonstration. The schools attended by experimental group children were ranked marginally higher on state exams than the schools attended by control students but were less likely to be magnet schools. All of these differences were relatively small. For example, the schools attended by those who moved with program vouchers were only at about the 25th percentile on state exams, as compared with the 17th percentile for the schools attended by controls at the time of the interim evaluation. MTO had no significant effect on the student-teacher ratio.

Among the children in the Section 8 group, participation in MTO reduced the schools' percentages of minority and poor (exhibit 6.3). There were no other significant effects on the schools attended by children in the Section 8 group at the time of the interim evaluation, although the average ranking of schools attended by children in that group over the course of the followup period was slightly higher than that of the school attended by control children. All of these effects were smaller than those on the schools of experimental group children.

These relatively modest impacts on school characteristics reflect the fact that, at the time of the interim evaluation, nearly three quarters of the children in families in the experimental group who leased up with program vouchers were attending schools in the same school district as at baseline. This may be because, as suggested in the MTO qualitative analysis, some children did not change schools when their families moved or because the families did not move out of the city. Since most of the cities in the study have a single citywide school district, families who moved within the city remained in the same school district.

Not surprisingly, given the small impact on school characteristics, the demonstration had virtually no significant effects on any of the measures of educational performance analyzed for either the experimental group or the Section 8 group (exhibits 6.5 to 6.7). Of the 58 outcomes analyzed, there were significant impacts on only two: small reductions in scores on the Woodcock-Johnson calculation score for all children in the Section 8 group and the broad math score for children ages 8 to 11in the Section 8 group.

Impacts on employment, earnings, household income, and receipt of public assistance

Data on employment, earnings, household income, and public assistance were obtained from a combination of administrative records and the interim survey. Administrative data provided a continuous history of employment, earnings, and Aid to Families with Dependent Children (AFDC)/Temporary Assistance for Needy Families (TANF) and food stamp benefits from random assignment through the time of the interim evaluation. Survey data provided measures of employment, earnings, and unearned income in 2001, and food security and receipt of SSI and Medicaid at the time of the interview.

At baseline, approximately a quarter of the sample adults were working. This proportion more than doubled over the followup period for both treatment and control group members. But the only statistically significant treatment-control difference in any of the measures of adult employment or earnings analyzed was a slight reduction in the employment rate in the first two years after random assignment among adults in the experimental group (Exhibits 7.3–7.4).

Although there were no statistically significant impacts on the employment or earnings of youth, either overall or by gender (exhibit 7.4), there was a large reduction in the proportion of female youth working and not in school , with a concomitant (though not statistically significant) increase in the proportion attending school (exhibit D7.1). Consistent with these findings, girls in the treatment groups perceived their chances of going to college and getting a well-paying, stable job as much higher than their control counterparts (exhibit E6.4). These findings are also consistent with the positive effects on girls' mental health and criminal behavior reported above.

At the time they were randomly assigned, the MTO adult sample members had very high rates of public assistance receipt and average incomes well below the poverty line. Approximately three-fourths of the sample members were receiving AFDC at baseline, and four out of five were receiving food stamps (exhibit 8.2).). Further, nearly all sample adults (94 percent) had received AFDC at some point.

Average income was about $9,300 at baseline, well below the poverty line for a family of three. Median income was still lower, approximately $7,800. These results show that sample members were quite disadvantaged when they entered the MTO demonstration.

Four to seven years later, the AFDC/TANF receipt rates had fallen by half across the entire sample. Less than 30 percent were receiving welfare benefits, although 46 percent were still receiving food stamps. Forty-five percent of the sample adults were working and off TANF in 2001. These figures did not differ among the randomly assigned groups. The only significant impacts of MTO on receipt of transfer payments were small increases in the receipt and amount of AFDC/TANF and/or food stamps benefits during portions of the followup period for each group (exhibits 8.4 to 8.7).

At the time of the interim evaluation survey, average household income was about $15,500. Two-thirds of the sample had incomes below the poverty level and one-third of these households had incomes below 50 percent of the poverty level. Eleven percent of the sample households had experienced food insecurity with hunger in the prior 6 months. Participation in MTO did not affect incomes or food security in either of the treatment groups (exhibit 8.8).

Assessing the Impact Estimates

In Appendix G we examine several questions related to the size and statistical significance of the estimated impacts of MTO, and the likelihood that more and larger impacts will be observed in the future:

- Do the findings provide evidence of real effects on family outcomes?
- Were the estimated effects of MTO large enough to be relevant for policy?
- Might the study have missed some effects that were large enough to be relevant for policy?
- How different would the results have been, if the families who moved with program vouchers had stayed in low-poverty areas longer?
- Can larger effects be expected in the longer term?

In this section, we summarize the conclusions of those analyses.

Do the findings provide evidence of real effects on family outcomes?

As indicated in the previous section, a number of the estimated effects of MTO were statistically significant. For an individual estimate, statistical significance at the .05 level means that the chance of obtaining an estimate that large or larger by chance alone is less than 5 in 100. This is generally regarded as a low enough risk of a false positive to be disregarded and to treat the estimate as convincing evidence of a real effect. But when large numbers of estimates are derived, the likelihood that some of them will exceed the .05 significance threshold by chance alone is substantially higher. If, for example, we derived 100 estimates, we would expect 5 of them to be significant by chance alone. The question therefore arises: given the number of estimates presented here, how much credence should be placed in those that were statistically significant?

Unfortunately, there is no simple answer to this question. For any given number of estimates, the number that would be expected to be significant by chance alone is easily calculated. But, as with any expected value, the actual number of false positives in any given sample can be greater or less than the expected value. If the impacts are correlated across outcomes, the actual number of false positives can be *substantially* greater or less than the expected number. Further, the number of statistically significant estimates presented in the report is affected by the apparent significance of the summary measure for a domain, such as when we present the components of the risky behavior index and not the delinquency index in Chapter 5, because the former index has a significant treatment effect and we explore its components in greater detail. Nevertheless, we believe that there is some information to be gained by examining the numbers and patterns of statistically significant estimates across the domains and subdomains analysed.

In several domains, for example, the number of statistically significant estimates is actually less than would be expected by chance alone. These include the employment and earnings domain (one significant estimate out of 46), the subdomain of educational performance (two significant estimates out of 58), and the subdomain of household income, food security, and self-sufficiency (zero significant estimates out of 18).[101] In these cases, it is clear that the interim evaluation provides little or no evidence of real effects of MTO on the outcomes of interest. Conversely, there are some domains and subdomains where the number of significant estimates substantially exceeds the number that would be expected by chance alone. These include the housing conditions and housing assistance domain (17 significant outcomes out of 24) and the subdomain of school characteristics (16 significant estimates out of 24). It seems unlikely that this many estimates would be significant by chance alone.[102]

The situation is less clear in the remaining domains—health; delinquency, crime, and risky behavior; and public assistance. In these domains, about 15 percent of the estimates were statistically significant, as compared with the 5 percent that would be expected by chance alone. This could simply reflect sampling variability within this particular sample. We leave it to the reader to assess the validity of these estimates.

Were the estimated effects of MTO large enough to be relevant for policy?

As detailed in the previous section, MTO had statistically significant effects on a number of outcomes. Even if we accept these estimates as evidence of real effects of MTO, that does not necessarily mean that they are large enough to be of practical significance. To assess the importance of these effects for policy, in appendix G we compare these estimated impacts with the mean outcomes that would have been experienced in the absence of the demonstration by families who leased up with program vouchers, which we term the counterfactual (exhibit G.1).[103] This comparison provides a measure of the relative size of the change in these families' lives caused by MTO.

Although the answer to this question is necessarily judgmental, we conclude that virtually all of the statistically significant impacts of MTO were substantial enough to be important for policy. Some are, in fact, quite large, especially in the experimental group. For example, among adults in the experimental group who moved with program vouchers, MTO increased the proportion who felt safe at night by two-thirds and the fraction who rated their housing good or excellent by 40 percent, while reducing the proportion who saw drugs being sold in their neighborhood by nearly 60 percent. The impacts on these measures in the Section 8 group were only about half as large, but still substantial.

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101 These counts, and those given below, are for the ITT estimates presented in the text.

102 It can be shown, for example, that the probability of obtaining 16 significant estimates out of 24, as we did in the school characteristics subdomain, when there are no true impacts on these outcomes, is less than .075, regardless of the correlation among the impacts.

103 Note that the counterfactual is different from the control group mean. It is our best estimate of the mean outcome that would have been experienced in the absence of the intervention by the subgroup of treatment group members who leased up with program vouchers. It is computed by subtracting the estimated impact on this group (i.e., the TOT impact) from their actual mean outcome.

The 11 percentage point reduction in obesity among adults in the experimental group represents more than a 20 percent reduction in the obesity rate for this group. Similarly, MTO reduced the psychological distress index by 20 percent among adults.

The effects on youth mental health were similarly large. MTO reduced the rate of generalized anxiety disorder by over two thirds—relative to what it would have been in the absence of the demonstration—for girls who moved with program vouchers in both the experimental group and the Section 8 group and for youth overall in the Section 8 group.

In the delinquency and risky behavior domain, both the favorable effects for girls and the unfavorable effects for boys were also quite substantial, relative to the counterfactual.[104] MTO reduced the rate of arrests for violent crimes by two-thirds among girls in the Section 8 group who moved with program vouchers, and reduced marijuana use and smoking by about half in the experimental group. Among boys who moved with program vouchers in the experimental group, MTO increased the behavior problem index by two-thirds, tripled the rate of arrests for property crimes, and quadrupled the incidence of smoking. Among boys in the Section 8 group, the demonstration raised the behavior problem index by a third, nearly doubled the arrest rate for property crimes, and tripled the proportion who smoked.

Might the study have missed some effects that were large enough to be relevant for policy?

The ability of this evaluation to detect the effects of MTO was limited by the size of the demonstration sample and the proportions of the experimental and Section 8 groups who leased up with program vouchers. In comparing the minimum effects detectable with this sample to the counterfactual for families who leased up with program vouchers (exhibit G.2), we found that the MTO impact estimates are sufficiently imprecise that we may have missed some impacts that are large enough to be relevant for policy, but not large enough to pass the test of statistical significance.

To further investigate this possibility, we examined the 95 percent confidence intervals around the estimated impacts—i.e., the range within which the estimated impact can be expected to fall 95 percent of the time, in repeated sampling (exhibit G.4). The more imprecise the estimate, the wider the confidence interval will be and the greater the chance that an impact that is relevant for policy will not be detected as statistically significant. Again we found that for about two-thirds of the outcomes for which insignificant impact estimates were obtained, the estimates were sufficiently imprecise that there is a non-negligible chance that the true impact was large enough to be relevant for policy.

How different would the results have been if the families who moved with program vouchers had stayed in low-poverty areas longer?

The vouchers issued to families in the experimental group were only valid in census tracts with poverty rates below 10 percent in 1990, but these families were only required to stay in such areas for

[104] As explained earlier, as used here, the counterfactual is our best estimate of the outcome that would have been experienced in the absence of the intervention by the subgroup of treatment group members who leased up with program vouchers. It is computed by subtracting the estimated impact on this group (the TOT impact) from their actual mean outcome.

one year to keep the voucher, and many of them moved elsewhere after the first year. Many moved initially to areas where the poverty rates rose between 1990 and 2000, so that even if they stayed in their initial locations, they were not necessarily in a low-poverty area at the time of the interim evaluation. And, of course, families in the Section 8 group were not constrained in where they could use the voucher.

As a result, even those families in the experimental group who moved with program vouchers spent, on average, only about 20 percent of the followup period in areas with poverty levels below 10 percent, and only about 60 percent of the followup period in areas with poverty rates below 20 percent (exhibit 2.5). At the same time, many families in the control group left public housing and moved to areas with lower poverty rates. On average, control families spent about 11 percent of the followup period in areas with poverty rates below 20 percent (exhibit 2.5).

These are substantial differences in the proportion of time spent in low-poverty areas, and one might reasonably expect them to result in a number of positive effects for the families who moved with program vouchers. But the MTO demonstration was not a pure test of the effects of living in low-poverty areas versus living in public housing in high-poverty areas, even for families in the experimental group. Therefore, we cannot infer those effects from these results with any confidence. However, we can obtain some suggestive evidence on this question by comparing the results for the experimental group and the Section 8 group. In appendix G, we used the estimated impacts for these two groups, and the proportions of the followup period that they lived in low-poverty areas, to extrapolate the effects of MTO to a hypothetical group living continuously in low-poverty areas (exhibit G.5). Using several different extrapolation methods, we found that the impacts of living continuously in low-poverty areas might be much more substantial than those observed in the demonstration. However, the results were quite sensitive to the specific form of the extrapolation and for a few outcomes, this method predicted smaller effects for a group that spent the entire followup period in low-poverty areas.

Can larger effects be expected in the longer term?

This is an interim evaluation. A final evaluation of the MTO demonstration is planned in roughly 5 years—9 to 12 years after random assignment. One potential reason why impacts were not observed for some outcomes is that those impacts have not yet had time to develop. If that is the case, we might expect the final evaluation to find more and larger impacts.

The existing literature provides little guidance on this question because few studies follow their samples for more than 5years. The most relevant precedent is Rosenbaum (1991), who compared families in the Gautreaux program who moved to the Chicago suburbs with those who moved to other housing within the city of Chicago. That study found that, 1 to 6 years after the move, many children in the families who moved to the suburbs "were still struggling to catch up, and it was not clear if they would succeed." Seven years later Rosenbaum found substantial, statistically significant impacts on eight of nine education- and employment-related outcomes for the same children. Largely on the basis of these findings, MTO was designed to have a 10-year followup.

There are strong theoretical reasons why it may take many years for the full effects of neighborhood to manifest themselves. Developmental outcomes like educational performance almost certainly reflect the cumulative experience of the child from an early age. Children who spend their first 10 years in an environment that does not facilitate educational achievement may never fully overcome that disadvantage, even if they then move to an environment that supports educational achievement.[105] In the interim evaluation, the youth sample is composed of children who moved out of public housing at ages 5 to 15. In the final evaluation the youth sample will have left public housing at birth to age 10. These youth will have spent a much larger proportion of their formative years outside the concentrated poverty of public housing. Therefore, they may show much greater gains in educational achievement and other developmental outcomes.

It is also true that the move from high-poverty areas to lower poverty neighborhoods is likely to be disruptive and require some adjustment period during which positive behavioral effects may not appear and, in fact, negative effects may be observed. There is some evidence of such transitional effects in the negative behavioral effects observed for male youth in the interim evaluation. If these effects indicate that the first 4 to 7 years after random assignment has been an adjustment period for these youth, we may observe different impacts in the longer term once that transition is complete.

We cannot, of course, predict the impacts that will be observed 5 years after our data were collected. We can, however, examine the interim findings for evidence that impacts are related to time since random assignment. We can do that in several ways.

The most direct evidence on this question is provided by the time path of impacts on those outcomes for which we have longitudinal data over the entire followup period—the employment, earnings, and public assistance outcomes measured with administrative data. Examining the impacts in years 1 and 2 and years 3 and 4 after random assignment for each of the main outcomes measured with these data (exhibit G.6), we found at least modest support for the hypothesis of more favorable effects over time.

For outcomes measured in the survey, we have no way to estimate impacts at different points in time after random assignment for a given sample. We considered comparing the impacts for those who were randomly assigned early, and therefore have been exposed to the treatment longer, with those who were randomly assigned later. If there were no systematic differences between these two assignment cohorts unrelated to duration of exposure to the intervention, this comparison would measure the difference in impacts resulting from a difference in exposure to the treatment of roughly 19 months. As a check for such differences between the early and late random assignment cohorts, we examined the estimated impacts within each of the two cohorts on outcomes measured with administrative data in the fifth year after random assignment (Exhibit G.7). If the two cohorts are similar, they should show similar impacts in a given year after random assignment. In fact, there were large differences in the impact estimates between the two cohorts. We take these results to indicate that we cannot interpret differences in impacts between the two cohorts as indicative of the effect of length of exposure to the treatment.[106]

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[105] In “The Culture of Poverty”, Oscar Lewis argued that, “By the time slum children are six or seven, they have usually absorbed the basic values and attitudes of their subculture and are not psychologically geared to take full advantage of changing conditions or increased opportunities that may occur in their lifetime” (Lewis, 1968).

[106] In appendix G, we discuss some reasons why families assigned early in the demonstration might differ from those assigned later.

Summary assessment of impact estimates

Overall, the assessment of the impact estimates presented in this section suggests that:

- The findings do provide convincing evidence that MTO had real effects on the lives of participating families in the domain of housing conditions and assistance and on the characteristics of the schools attended by their children;

- There is no convincing evidence of effects on educational performance; employment and earnings; or household income, food security, and self-sufficiency.

- The statistically significant impact estimates are uniformly large enough to be relevant for policy. In fact, many are quite large.

- Given the size of the interim evaluation sample and the leaseup rates in the two treatment groups, the impact estimates are sufficiently imprecise that some true impacts that are large enough to be relevant for policy may not have been detected as statistically significant.

- Although MTO induced substantial differences in the proportion of time spent in low-poverty areas by the three assignment groups, it was not a pure test of the effects of living in low-poverty areas versus living in public housing in high-poverty areas, even for the families in the experimental group who moved with program vouchers. Extrapolating the effects of living continuously in low-poverty areas might be more substantial than those observed in the demonstration. However, our ability to measure those effects quantitatively is limited.

- There is at least modest evidence that the impacts of the demonstration are becoming more favorable over time, at least for public assistance, which was the only outcome for which we were able to estimates effects over time. If this holds for other outcomes, we might expect more and larger impacts in the final evaluation 10 years after random assignment.

Implications of the Interim Evaluation Results for Policy

The interim findings allow us to address three fundamental questions related to policy with respect to low-income families in public housing:

- What social benefits and costs accrue as a result of moving low-income families out of public housing projects in high-poverty areas into private housing, and how do those benefits differ between policies that restrict such moves to low-poverty areas and those that do not?

- How effective is policy likely to be in changing the environment of low-income families?

- What do the interim results have to say about alternative approaches to improving the lives of low-income families?

The social benefits and costs of moving low-income families out of public housing in distressed neighborhoods into private housing

While we have not attempted to conduct a formal benefit-cost analysis, the interim evaluation results provide relatively clear evidence of the main social benefits and costs of MTO. From the families' perspectives, the principal benefit of the move was a substantial improvement in housing and neighborhood conditions. Families who moved with program vouchers largely achieved the single objective that loomed largest for them at baseline: living in a home and neighborhood where they and their children could feel and be safe from crime and violence. They not only reported feeling safer but were also less likely to be victims of crimes. On a whole list of observable characteristics —from plumbing problems and peeling paint or wallpaper to litter, graffiti, and public drinking in the streets—their homes and neighborhoods were substantially more desirable than those where controls lived. These benefits accrued to families in both the experimental group and the Section 8 group, although the improvements tended to be roughly twice as large for experimental group families, who were required to move to low-poverty areas, at least initially.

Perhaps not surprisingly, these improvements in living environment led to significant gains in mental health among adults in the experimental group. The level of psychological distress was substantially reduced in this group. In addition, adults in both the experimental and Section 8 groups experienced substantial reductions in obesity for reasons we do not yet understand.

Among the children in these families, girls appear to have benefited from the move in several ways. They experienced improved psychological well-being, reporting lower rates of psychological distress, depression, and generalized anxiety disorder, and improved perceptions of their likelihood of going to college and getting a well paid, stable job as an adult. These girls' behaviors changed as well, with a smaller proportion working instead of attending school. They were less likely to engage in risky behavior or to use marijuana. Finally, both these girls and society as a whole benefited from a reduced number of arrests for violent crimes.

The principal social costs that must be offset against these benefits are the costs of the MTO mobility counseling, any increased costs due to the greater likelihood of receiving housing assistance among those who leased up with program vouchers, and an increase in the rate of behavior problems, smoking, and arrests for property crimes among boys ages 15 to 19.

We cannot place values on these social costs and benefits. Policymakers will have to decide whether the gains of this kind of policy outweigh the costs. But the interim evaluation has demonstrated that there are substantial social benefits as well as some costs associated with facilitating the movement of public housing residents who desire to move to low-poverty areas.

How effective is policy likely to be in changing the environment of low-income families?

One of the clearest messages of the interim evaluation results is that housing-related policy can influence, but it cannot dictate, the residential location of low-income families. As noted above, the demonstration reduced by half the proportion of the followup period spent in areas of concentrated poverty by families in the experimental group who moved with program vouchers. Among Section 8 families who moved with programs vouchers, the proportion of time spent in areas of concentrated poverty was cut by about a third (Exhibit 2.9). MTO increased the proportion of time spent in areas with poverty rates below 20 percent by 53 percentage points among families in the experimental group.

Of course, policies designed to move low-income families from public housing in high-poverty areas to private housing in low-poverty areas need not take the form of location-restricted vouchers like those used in MTO. One might, for example, incorporate mobility counseling or other supports for moving to low-poverty areas into the regular voucher program. One could also create goals and performance incentives for program administrators to encourage moves to opportunity areas, and one can create or preserve both assisted and affordable housing in low-poverty area through decisions with respect to state agency refinancing policies, allocations of Low Income Housing Tax Credit (LIHTC), use of HOME funds, public housing authority (PHA) project-basing of vouchers, and other existing housing programs and policies. These policies seem likely to have effects on the residential location of low-income families that would fall somewhere between those of the MTO experimental and Section 8 groups. Somewhat stronger effects on residential location might be achieved by coupling locationally restricted vouchers with ongoing supports and/or counseling for families that move to low-poverty areas.

One of the lessons of the MTO demonstration is that the poverty rate, while important, may be an overly simplistic way to characterize neighborhoods. Residential environments are multidimensional, and no one measure will capture all the attributes that are important to the life chances of low-income families. Thus the fact that a majority of the program movers in the experimental group moved to areas with low, but rising, poverty rates may have had an important effect on their subsequent outcomes. Similarly, even in the experimental group, a large proportion of those who moved with program vouchers stayed within the city; this meant that their children attended schools in the same school system as control children. This almost certainly limited the improvement in school quality they experienced as compared with a move to the suburbs. Moreover, the low-income areas to which families in the experimental group moved were still heavily minority. To the extent that racial integration has a positive influence on any of the outcomes analyzed here, that influence was largely absent in this demonstration. These considerations suggest that policymakers seeking to improve the environment of poor families may want to consider other characterizations of neighborhood than that provided by the poverty rate alone.

When thinking about the implications of these results for policy, it is important to recognize that all of the impacts presented here are measured relative to a control group receiving some mix of existing housing subsidies. Some control families eventually received regular Section 8 vouchers, some continued to live in public housing, and some left housing assistance altogether. Indeed, some control group members were unable to remain in public housing because their units were demolished under HOPE VI or other revitalization programs. We did not attempt to eliminate the influence of these changes in control circumstances—including the receipt of Section 8 vouchers—from the estimates. Rather we view the results as measures of the incremental effects of offering vouchers, with or without locational restrictions, to residents of public housing in areas of concentrated poverty, during the particular period encompassed by the study. These findings answer this question: How much better off are the recipients of the demonstration vouchers than families who started out in the same situation and who received no help from the demonstration? This means that the estimates from this study are not applicable to all types of policy. For example, for a policy that replaces public housing with vouchers, the appropriate control benchmark would probably be continued residence in public housing. That is not what was tested here—indeed it probably cannot be tested—and the results of the present test probably understate the effects that would be expected from such a policy.

What do the interim results have to say about alternative approaches to improving the lives of low-income families?

The most fundamental question addressed by MTO is, to what extent are the problems encountered by public housing residents the result of the high concentration of poor families in those developments and the surrounding neighborhoods and to what extent are they caused by attributes of the families themselves? To the extent that these problems are environmental, the appropriate policy response is to foster dispersion of these families to more positive environments. To the extent that these problems reflect family characteristics—such as lack of education, limited work experience, or membership in a group that faces discrimination—the appropriate policy response is to address these characteristics directly.

By the final evaluation, the effects of environment will have had more time to manifest themselves. At this point we can say that some of the problems of public housing residents do appear to be environmental. These include the housing and neighborhood quality deficiencies and psychological and behavioral problems on which MTO had significant effects.

It remains to be seen whether the problems of physical health, educational performance and attainment, employment, earnings, and welfare dependence that characterize public housing residents are amenable to housing policies designed to change their residential environment. At this point, there is no evidence that they are. If that finding is confirmed in the final evaluation, that would suggest that policies designed to deal directly with these specific problems—educational improvements, employment and training, or welfare-to-work policies—will be more effective solutions.

Issues for Further Research

The interim evaluation has addressed a wide range of questions about the impacts of the MTO demonstration. In the course of this analysis, other questions have been raised, and some questions were deemed outside the scope of this analysis. Some of these outstanding questions will be addressed in the long-term analysis, to be conducted approximately 9-12 years after random assignment. In the meantime, HUD plans to make the data used in the interim evaluation available to other researchers, through an application process that will allow controlled access to the data, to begin to address these questions. Among the issues that might be fruitfully addressed in such analyses are:

- What are the mechanisms underlying the significant impacts found in the interim analysis?
- Were impacts larger for families who stayed longer in low-poverty areas? For families who moved to non-minority areas or areas where the poverty rate was decreasing?
- Why were impacts on delinquency, criminal activity, and risky behavior consistently beneficial for girls and adverse for boys?

- What are the linkages among residential location, schools, school performance, and behavior for girls and boys?

- Are child outcomes (health, delinquency, school performance) better for children where adults experienced improved physical or mental health?

- Why are the impacts on public assistance outcomes substantially different for families who were randomly assigned early in each site than for those assigned later?

Answers to many of these questions will require nonexperimental methods. It will be important that the researchers conducting those analyses be sensitive to the risk that the results will be distorted by selection bias, which was, of course, the problem that initially prompted the MTO demonstration.

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Appendix A

Data Collection Sources and Methods

Appendix A
Data Collection Sources and Methods

The MTO Interim Evaluation collected data from a myriad of sources to cover all of the study's domains and to produce a rich data set on the status of the sample in 2001–2002. The study employed two modes of participant data collection—quantitative and qualitative. Data about sample members were also collected from administrative agencies. In addition, information was drawn from published data sets, such as the U.S. Census Bureau, the National Center for Education Statistics Common Core of Data on schools, and HUD data on Section 8/Housing Choice Voucher Fair Market Rents. Also data were drawn from the history of address data stored in the MTO data system since the demonstration started in 1994. This appendix presents a summary of the interim evaluation data and a description of the process of implementing each aspect of the data collection.

The data collected for this evaluation came from three source types: primary data collection by Abt Associates, administrative data collected from HUD, and administrative data collected from other sources. The specific types of data discussed in this appendix include the following:

- Data collected on participants using primary data collection methods
  - Qualitative data.
    - Indepth interviews with adults.
    - Indepth interviews with youth.
    - Neighborhood observations.
  - Quantitative survey data.
    - Interim survey of households.
    - Interim survey of youth.
    - Interim survey of children.
    - Achievement testing of youth.
    - Achievement testing of children.
    - Direct measurement of height and weight for children.
    - Household and neighborhood observations.
  - Location data (address histories).
- Administrative data collected from HUD.
  - Multifamily Tenant Characteristics System (MTCS) data.
  - Tenant Rental Assistance Certification System (TRACS) data.
  - Fair Market Rent (FMR) data.
  - Public housing authority (PHA) Expenditure data.
- Administrative data collected from other secondary data sources.
  - Social Security Number Verification through the Social Security Administration.
  - State Temporary Assistance to Needy Families (TANF) and food stamp (FS) data.

- State Unemployment Insurance (UI) data.
- School data from the National Center for Education Statistics (NCES).
- School data from State, county and city boards of education.
- Census data.
- Adult and juvenile arrest and criminal disposition data.

Qualitative Participant Data Collection

The main purpose of the qualitative interviews was to explore the question of how neighborhood mechanisms may influence outcomes for participants. Qualitative interviews were later integrated with survey data to help enrich our understanding of the experiences of MTO families since their initial moves from public housing.

The qualitative data collection had three components:

- Indepth interviews with adults.
- Indepth interviews with youth.
- Neighborhood observations.

Qualitative interviews were completed with a total of 58 adults and 39 youth. The indepth interviews with adults took 1 to 2 hours, and those with youth lasted approximately 45 minutes. To provide a complete picture of the families' experiences, we sampled both movers and nonmovers. Movers were also sampled based on neighborhood poverty rates. The qualitative data collection took place between February and March 2001.

For more information on the qualitative study data collection, refer to *Families in Transition: A Qualitative Analysis of the MTO Experience* (Popkin et al., December 2001).

Quantitative Participant Data Collection

For this evaluation, Abt Associates interviewed 8,870 respondents. Of these, 3,526 were adult heads of household and the remainder, children. Quantitative data were collected for up to three sample members per household, the adult respondent and up to two sampled children ages 5 to 19. A child's age for data collection purposes was uniformly measured as of May 31, 2001.[1] The child sampling is described in appendix B.

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[1] Since the field data collection continued through September 2002, this means that at the moment they were interviewed or tested some children were more than a year older than their age as defined for sampling.

Participant data collection involved seven main components:

- Interim survey of households.
- Interim survey of youth.
- Interim survey of children.
- Achievement testing of youth.
- Achievement testing of children.
- Direct measurement of height and weight for children.
- Interviewer observations of the household and neighborhood.

Exhibit A.1 shows the contents of the quantitative participant data collection components. Exhibit A.2 shows the sample definitions and sizes and summarizes the survey strategy for each sample. Further details are provided in the text below.

EXHIBIT A.1
CONTENT OF PARTICIPANT DATA COLLECTED FOR THE MTO INTERIM EVALUATION

	Adult[a]	Youth 12-19	Children 5-11[c]
Survey Topics	Housing and neighborhood Education and training Employment and earnings Income and public assistance Outlook and social networks Health Household composition Child education[b] Child health[b] Child behavior[b] Child time use[b] MTO experience	Education Employment and earnings Risky behavior Health Neighborhood and social networks Emotions Time use Future plans	Education Neighborhood, danger, and risk Health Behavior and family dynamics
Direct Measurement	Blood pressure	None	Height and weight
Educational Testing	None	Woodcock-Johnson Revised selected tests	Woodcock-Johnson Revised—selected tests

Notes:
a) Adults were selected for interviewing in the following order of precedence: female head of family intending to move through MTO; wife of core head intending to move through MTO; wife of baseline head, if a member of the family intended to move through MTO; and nonfemale (male or unknown gender) head of family intending to move through MTO.
b) The adult respondent was asked questions about each sampled child in the household, up to two.
c) Surveys were administered only to sampled children ages 8 to 11. Direct measurement and educational testing were carried out for sampled children ages 5 to 11.

EXHIBIT A.2
PARTICIPANT DATA COLLECTION FEATURES

	Household Head	Youth	Children 5-11
Sample Size:	One adult person per family enrolled in MTO through 12/31/1997. N=4,248 adults	Sampled children ages 12 to 19 in households enrolled in MTO through 12/31/1997 N=3,537 youth	Sampled children ages 5 to 11 in households enrolled in MTO through 12/31/1997 N=3,146[a]children
Weighted Response Rate:[b]	89.6%	89.0%	87.4%
Field Period:	December 2001–June 2002 for the main sample; July–September 2002 for subsample	December 2001–June 2002 for the main sample; July–September 2002 for subsample.	December 2001–June 2002 for the main sample; July–September 2002 for subsample.
Mode of Data Collection:	Inperson with some telephone	Inperson with some telephone	Inperson with some telephone[c]

Notes:
a) Of the 3,146 children ages 5 to 11, a total of 2,202 were between 8 and 11 and the remainder were between 5 and 7.
b) To increase the response from hard-to-find cases and reduce nonresponse bias, data collection continued on a subsample of cases. These cases were then weighted when calculating the final response rates.
c) Only children ages 10 and 11 were allowed to complete an interview by telephone.

Field interviewers from Abt Associates interviewed one adult per household for the interim survey of households. In most instances, the adult interviewed was the head of the household that joined MTO and also the primary caregiver for the children of interest to the study.[2] The interim survey of youth was carried out with sampled children ages 12 to 19 in each household. The interim survey of children was administered to sampled children aged 8 to 11. The next two components involved direct administration of standardized achievement tests to all sampled children ages 5 to 11 and sampled youth aged 12 to 19. The final component required interviewers to weigh and measure children ages 5 to 11 following specific measurement procedures.

Data collection components

The interim survey of households. The interim survey of households consisted of a 65-minute interview with one adult per core MTO household. This adult was usually the female head of the MTO core family, as defined by the applicant during the Section 8 eligibility determination process.

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[2] Of the 4,248 households in the interim evaluation sample, two parents were present at the time of MTO enrollment in 108 households. In these instances, we interviewed the mother (unless she was deceased). Typically, the mother had completed the baseline survey and was more likely to be the primary caregiver for the children.

The respondent was asked questions about a wide range of topics across the study domains: housing and mobility, employment and earnings, income and public assistance, health, and risky behavior. When the adult respondent was the primary caretaker of children in the household, she or he was asked a series of questions about the health, education, and social behavior of the sampled children. Additional survey modules covered household composition (similar to the annual MTO canvass) and reactions to the MTO experience.

The interim survey of youth. The interim survey of youth was administered to sample children between the ages of 12 and 19. The youth survey averaged about 30 minutes in length. It covered topics including attitudes toward school, ties to the neighborhood, involvement in afterschool and community activities, health, and risky behavior. Because the surveys were administered using laptop computers, youth were offered the opportunity to self-administer sections of the survey that involved sensitive topics. A small number of youth took advantage of this option.

The interim survey of children. The interim survey of children was administered to sampled children ages 8 to 11. The child survey took 15 to 20 minutes to complete and focused on school, health, friends, the neighborhood, and family support.

Educational achievement testing for children and youth. Sampled children ages 5 to 19 were asked to complete a series of educational achievement tests, part of the Woodcock-Johnson Revised (WJ-R) battery. The testing was expected to average 30 minutes in length for those under 8 and 45 minutes in length for those 8 to 19. Late in the data collection period, interviewers were asked to record the start and end times for each of the individual tests. The average time spent on the WJ-R testing for youth and for children ages 5 to 11 was about 90 minutes. In both groups, each of the math tests took at least three minutes longer than the language tests.

Direct measurement of blood pressure, height, and weight. In conjunction with the survey data collection, field interviewers took direct measurements of blood pressure for adult respondents and of height and weight for child respondents (ages 5 to 11). These measurements were taken in the home, using appropriate and up-to-date equipment, by interviewers trained in these procedures.

Housing and neighborhood observations. The final components of the household-level data collection were interviewer observations of the household and neighborhood. The household observation form had several questions about the interaction between the head of household and the selected sample children. It is important to note that this aspect of the data collection could only be done if the sample child was present at the time of the household respondent's interview. There were also a few questions about the interviewers perception of how well the respondent understood the questions.

Upon leaving the household, the interviewer was instructed to complete the neighborhood observation form. This form collected information about the neighborhood condition—physical appearance, land use, and the overall climate. If interviewers felt that a neighborhood was unsafe they were not required to visit the neighborhood just to complete the observation form.

Participant data collection procedures

Abt Associates designed data collection procedures to coordinate the various parts of this effort. The three surveys—household, youth, and child—were administered in person by trained interviewers using the Bellview Computer-Assisted Personal Interviewing (CAPI) system on a laptop computer. The surveys for all three samples were administered primarily in the respondent's homes, with the session scheduled at the respondent's convenience. Conducting most of the youth interviews in the home was a change from the original data collection protocol, which had called for the majority of the youth interviews to be done in teen centers. These teen center interviews were planned to take place in community facilities where small groups of teens would be invited to come and complete the interviews and testing sessions one-on-one with an interviewer. It was thought that the youth would be more likely to attend interview sessions with peers and would feel more comfortable responding to questions of a sensitive nature outside their homes. In addition, this approach would be more cost-effective than inhome interviews, because many could be done simultaneously.

A pilot test of the teen center approach was conducted in Boston at the end of November 2001 to assess whether to collect the youth data in this manner. Field management reported that the teen centers for the pilot took hours to arrange and staff. Also, after the first three pilot sessions the numbers of broken appointments increased, so the sessions produced almost no completed interviews. Therefore, the decision was made to switch to inhome interviewing.

Data collection implementation. To conduct the extensive participant data collection effort, Abt Associates held a 2-week training in Golden, Colorado, for 5 field managers and more than 100 interviewers between October 22 and November 2, 2001. The five field managers (one for each of the original MTO sites) reported to the national field manager, who supervised this project closely. The training covered all aspects of the data collection, including:

- Administration of all survey instruments.
- Completion of the interviewer observation forms.
- Conducting educational testing with the WJ-R battery.
- Taking direct measurements of height, weight, and blood pressure.
- Use of the CAPI software and the laptop computers.
- Transmission of electronic data to Abt Associates.

CAPI testing and revisions delayed the field period start until the end of December 2001. Data were collected from sample members in two phases: the main sample phase, in which all 10,932 sample cases were worked, and the subsample phase, in which a subset of the remaining cases was worked. The full sample was worked from late December 2001 through June 2002. At that time, it was decided to concentrate the remaining data collection resources on a 3-in-10 subsample to maximize the final response rates and reduce nonresponse bias. Subsample data collection began in early July and continued through mid-September 2002.[3] Exhibit A.3 shows the field period by site for the main and subsamples.

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[3] For more information about the subsample, refer to appendix B.

EXHIBIT A.3
FIELD PERIOD BY MTO SITE

	Full Sample		Subsample (and Remote Cases)	
	Start	**End**	**Start**	**End**
Baltimore	Late December 2002	June 22, 2002	June 29, 2002	September 15, 2002
Boston	Late December 2002	July 6, 2002	July 13, 2002	September 15, 2002
Chicago	Late December 2002	July 6, 2002	July 13, 2002	September 15, 2002
Los Angeles	Late December 2002	June 22, 2002	June 29, 2002	September 15, 2002
New York	Late December 2002	July 6, 2002	July 13, 2002	September 15, 2002

Maximizing Response Rates. To maximize response rates, Abt Associates used several different approaches. First, in sites with lower response rates, Abt sent a team of expert interviewers for a week to conduct a blitz. This allowed the field manager to review carefully the outstanding sample, target particular areas that had clusters of sample, and then send a team out to interview them. Sample shuffling was also very helpful, particularly in converting refusals. Often, field managers found that assigning cases to a different interviewer resulted in a completed interview.

Remote cases (those not within 50 miles of the 5 MTO cities) were only worked during the subsample period. But beginning in mid-April 2002 (about 6 weeks before the subsample work began) interviewers were authorized to conduct adult and youth interviews over the phone, especially for cases that would require travel. This allowed them to obtain at least a partial complete, although the educational testing, direct measurement, and interviewer observations could not be carried out. During the subsample period, interviewers traveled to all out-of-state cases that were in the subsample. Cases were completed in 29 states, Puerto Rico, and the Dominican Republic.

To show our appreciation for the time they spent to complete the data collection, all respondents were given an incentive payment. Adults who completed the household interview were given a $50 money order. Younger children (ages 5 to 11) were given a small gift, and the parent/guardian was given a $25 money order for each child between the ages of 5 and 11 who completed the survey and achievement testing. The older children who completed the youth survey (those between the ages of 12 and 19) were given a $50 money order for completing their surveys and testing. During the subsample period, interviewers were authorized to offer double incentives for those respondents who had refused to participate. This increased incentive was very helpful in converting refusals into completed interviews.

The overall response rate for the household survey (after weighting the subsample data to represent all the cases eligible for subsampling) was 89.6 percent. The response rates for each aspect of the participant data collection are shown in exhibit A.4.

EXHIBIT A.4
WEIGHTED COMBINED RESPONSE RATES FOR KEY INSTRUMENTS

	Total Sample	Number of Completed Cases By Sample:			Effective Response Rate (RR)[a]	RR Adjusted For Dead Resp.[b]	RR Adjusted For Dead/Incap. Resp.[c]
		MAIN	SUB	SUM			
Survey	4248	3398	128	3526	89.6%	91.1%	91.2%
Household Observations	4248	3344	119	3463	87.6%	89.1%	not applicable
Neighborhood Observations	4248	3281	121	3402	86.4%	87.8%	not applicable
Child Survey	2202	1701	79	1780	88.8%	88.9%	89.3%
Child WJ-R	2202	1683	87	1770	88.5%	88.6%	89.0%
Youth Survey	3537	2693	136	2829	89.0%	89.3%	89.6%
Youth WJ-R	3537	2624	135	2759	87.4%	87.6%	88.0%
Young Child H/W	944	665	30	695	80.1%	80.5%	80.6%
Young Child WJR	944	704	31	735	84.4%	84.9%	85.0%
All WJ-R	6683	5011	253	5264	87.3%	87.5%	87.9%
All Child and Youth Surveys	5739	4394	215	4609	88.9%	89.1%	89.5%

Notes:

a) Response rates are measured as the number of completes divided by the total sample count. The effective response rate (RR) is calculated from the main sample response rate (MRR) and the subsample response rate (SRR). The formula is:

$$RR = MRR + SRR*(1-MRR)$$

b) Deceased sample members are removed from the total sample count in this adjustment.

c) Deceased and incapacitated sample members are removed from the total sample count in this adjustment.

Data processing and delivery. The data collection ended on September 15, 2002 in all sites. The remainder of September was spent reconciling samples, ensuring that all supplemental forms were entered, all respondent information booklets were returned, and all addresses were updated. The months of October and most of November were spent cleaning the survey data. The child survey and height/weight data were the first of the data sets delivered to analysts in mid- to late October. The final data set, containing household data, was delivered to analysts on December 5, 2002.

Administrative Data Collection

The MTO Interim Evaluation drew upon several administrative databases both for measuring outcomes and mediating factors. Exhibit A.5 summarizes the full set of administrative data sources. Each of the sources used is described below, after a discussion of matching methods for these data.

Social Security Number verification

Social Security Numbers (SSNs) have been collected from MTO sample members since the time they joined MTO. From time to time since random assignment, in conjunction with passive tracking of sample location, new numbers and other new identifiers have been gathered from various sources. As a result, some of the MTO sample members have multiple SSNs, shared or reversed SSNs, and/or alias (alternative) names in the MTO data system.

Such situations can make determining matches to administrative data more difficult. Both to facilitate the matching process and ensure the reliability of the matches, Abt Associates Inc. and HUD worked out an agreement with the Social Security Administration to verify social security numbers of MTO sample members through their Employment Verification Service (EVS). The EVS results and information in the MTO data system were used together to select the best SSN for each sample member from among those collected prior to random assignment. These identifiers were then used to determine correct administrative data matches.

State administrative data collection

TANF and UI data. Abt staff, with assistance from HUD and colleagues at NBER worked with state agencies on data agreements for participant-level data from three programs: TANF, food stamps, and UI data. Progress was very slow. In some sites the process began in spring 2001 but data were not received until 2002. However, by March 2003 staff had received data from all sites.

In Massachusetts (where confidentiality protections are so strict that no nongovernmental organization can gain data access) Abt could not obtain direct access to the Massachusetts UI data. We were able to coordinate this request through HUD, since the Massachusetts Department of Revenue agency could provide data to other governmental organizations.

EXHIBIT A.5
ADMINISTRATIVE DATA SOURCES FOR OUTCOME AND MEDIATING FACTOR MEASURES

Domain	Outcomes	Data Sources
Employment and earnings	Quarterly employment and earnings	State UI wage records
Delinquency and risky behavior	Arrests and court dispositions	State agencies that maintain data on criminal records
Welfare and other transfer programs	Monthly TANF, food stamp, and SSI benefits and exits from cash assistance, date of TANF time limit, TANF sanctions, and participation in welfare-to-work activities	State welfare agency records
Housing assistance	Amount of housing assistance Receipt of housing assistance	Multifamily Tenant Characteristics System (HUD) Tenant Rental Assistance Certification System (HUD)
Domain	**Mediating Factors**	**Administrative Data Sources**
Education	School quality School resources Crime rates for local area Unemployment rate School attendance Grade completion	U.S. Department of Education Common Core of Data on schools FBI, local police departments, Census 2000, Bureau of Labor Statistics (BLS) Local school district Web sites and published data
Employment and earnings	Crime rates for local area Unemployment rate	FBI, local police departments BLS
Delinquency and risky behavior	School resources School quality SES level	U.S. Department of Education Common Core of Data on schools
Welfare and other transfer programs	Unemployment rate Receipt of public assistance in the local area Crime rate in the local area	BLS Census 2000 FBI, local police departments
Housing assistance	FMRs for local area by housing unit size	HUD

The original plan was to collect TANF, food stamp and UI data as far back as January 1994 (before random assignments in MTO began). However, two agencies could only go as far back as 2000. The California TANF agency could go back only to October 2000. In New York, staff could get FS data only as far back as October 1999 and TANF data back only to December 1996. All other sites provided data as far back as the beginning of 1995 (and most as far back as January 1994).

Exhibit A.6 summarizes the periods covered by the TANF and UI data used in the analysis.

EXHIBIT A.6
STATUS OF STATE ADMINISTRATIVE DATA COLLECTION

Agency	Period Covered
California UI	January 1994–December 2001
California TANF/FS	October 2000–December 2001
Illinois UI	January 1994–December 2001
Illinois TANF/FS	January 1994–December 2001
Maryland UI	January 1994–December 2001
Maryland TANF/FS	January 1994–December 2001
Massachusetts UI	4th Quarter 1994–4th Quarter 2001
Massachusetts TANF/FS	July 1994–December 2001
New York UI	January 1995–December 2001
New York TANF/FS	December 1996 (TANF), October 1999 (FS)–December 2001

HUD administrative data collection

There were four types of administrative data collected from HUD: MTCS, TRACS, FMR, and operating costs.

MTCS and TRACS data. Two extracts from HUD's MTCS system were collected, one containing data through May 2001 and the other containing data through December 2001. MTCS data served two purposes. They provided updated address data and also yielded data on certain housing outcomes. The address portion of these data is discussed under the Locational Analysis section of this appendix.

In October 2002 staff received additional HUD administrative data. The MTCS files covered 1995 to 2002, with the exception of 1999. The TRACS data covered 1995 to 2001. All of the MTCS and TRACS data were processed using fuzzy matching on SSN, name, and date of birth.

Fair Market Rent data. FMR data was collected from HUD's Web site for use in measuring the rental conditions of the MTO sites and how the rents paid by MTO respondents compare to FMRs published by HUD.

HUD data on PHA expenditures. Staff from Abt Associates Inc. and NBER worked together to collect the PHA operating expenditures data. Requests were made to the PHAs in all five MTO sites to submit their HUD form 52599s. As the forms were received, they were sent to Abt for entry and then readied for analysis.

Other administrative data collection

School data. The administrative database designed to contain measures of school-level and district-level characteristics for use in the educational achievement analysis was created in April 2002. In November 2002 the school data from NCES and from the States and localities were turned into uniform data sets and fully documented as to source.

Achievement test data. The College Board has agreed to provide test score data directly to the National Bureau of Economic Research (NBER) because only NBER analysts are authorized to use these data. Achievement test scores will be aggregated to the school level.

Arrest and disposition data. Adult and juvenile arrest and criminal disposition data were collected for this study under a cooperative agreement between NCOVR and HUD. This effort was carried out by Professor Jens Ludwig, of the Georgetown University Center for Public Policy. The individual offense data could be used only by Georgetown University analysts. Therefore, their analysts coordinated with our team members to test and report outcome measures in the same manner as the rest of the analyses.

Location Data (Address Histories) for the MTO Sample

Data on the current and past residential locations of MTO sample members came from several sources: the MTO tracking database, MTCS and TRACS data, and the interim evaluation field data collection. All address data were processed in a systematic way. Addresses were first standardized into U.S. Post Office standard format and each piece of the address was parsed into its own field. Next all addresses were geocoded by vendor Tele-Atlas, using both 1990 and 2000 census geography.[4] Careful analysis of the geocoding of more than 46,000 MTO addresses was done to ensure that the addresses were geocoding correctly and that differences between 1990 and 2000 geocodes appeared reasonable according to table of relationships maintained by the census bureau.

Then the addresses were posted to the MTO tracking database. (For a description of the data system and the tracking database within it, see Feins et al., 1999.)

Once all new addresses were loaded to the data system, address histories were constructed for each member of the interim evaluation sample. The series of addresses for each sample member was run through a spell generator program, which linked consecutive addresses in the same location into spells with start and end dates and all necessary geocodes. This program compared the dwellings at varying points in time, determined whether an address was the baseline (prerandom assignment) location, the MTO move location, the location at the time of the 1997 or 2000 canvasses, or the residential location at the time of this study's data collection in 2002. Spells containing these

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4 The latitude and longitude of a specific address may differ slightly between 1990 and 2000, due to the advent of digital mapping technology during that decade. In addition, some addresses that had changed (for example, due to renaming of streets) or were no longer valid (due to demolition) were able to be geocoded for 1990 but not directly for 2000.

confirmed addresses were tagged with that information. The address spell data were used for all mobility analyses (see chapter 2) and for any other analysis involving the sample members' baseline or current locations.

Collection of Published Data

Census data

This study used data from the SF3 (census tract- and block-group-level files) from the 1990 and 2000 census. After defining an extensive set of variables to characterize the neighborhood settings of the sample, tract-level variables for the 1990 Census for the whole country were calculated and made ready for linking to MTO locations. In addition, 1990 Census block-group-level variables were prepared for all parts of the five states containing the original MTO sites.

The census bureau released the small-area files for Census 2000 for the five MTO states in August 2002. By the end of September, tract- and block-group-level data were available for the whole country. In August staff began converting the 1990 code to work with the Census 2000 files. This task—and the creation and documentation of the 2000 variables for tracts and block groups—was completed at the end of September with files provided to analysts shortly thereafter.

HUD provided 2000 Census data at the tract level for the remainder of the country in October with block group data for the other States coming shortly after. All the remaining tract- and block-group-level variables were created and delivered to the analysts in November.

Appendix B

Samples and Analysis Methods

Appendix B
Samples and Analysis Methods

This appendix describes the basic analysis strategy for the quantitative experimental component of the Moving to Opportunity (MTO) Interim Evaluation. It is divided into nine sections:

1. Sample Selection
2. Data Sources
3. Estimation Methods
4. Outcomes and Mediators
5. Covariates
6. Weights
7. Missing Data
8. Interpretation and Reporting of Results
9. References

B1. Sample Selection

Two samples were used for the analyses: an adult sample and a child sample. Both samples draw on members of the "core" household: members of the family who planned to move together if awarded a voucher or certificate through the MTO program. For the purposes of the interim evaluation, we considered core household members to be those individuals who a) were identified as part of the core family on the HUD 50058 form or b) were members of fifteen families without core status information AND were identified as core members on the Baseline Survey. Only families who were randomly assigned by December 31, 1997 were included in the samples, to allow for a minimum of approximately four years to have passed since random assignment. In addition to these two analysis samples, we describe here a subsample of hard-to-interview respondents that was used to reduce survey non-response bias.

B1.1 Adult sample

The adult sample consists of one adult from each of 4,248 MTO households.[1] The order of precedence for selecting the sample adult is female core head, wife of core head, female baseline head if she is a core member, wife of baseline head if she is a core member, and male or unknown gender core head. (Deceased sample adults were not replaced.) For over 90% of the cases, the sample adult is the female core head.

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[1] A total of 4252 MTO households were randomly assigned by December 31, 1997. The interim evaluation sample excludes four households whose Sample Adult was a core member of another MTO household randomized earlier. Thus, we included 4248 households in the interim evaluation.

B1.2 Child sample

Core children ages 5 to 19 as of May 31, 2001 were eligible to be part of the Child Sample. For survey purposes, we included up to two children from each core household in the sample. For core households with more than two children ages 5-19, we randomly selected two children. Children remained in the sample even if they no longer lived with the sample adult. The final sample consisted of 6,683 children: 3,146 ages 5 to 11 and 3,537 ages 12 to 19.[2]

We selected up to one *sample* child ages 5 to 11 from each core household for the *Concept Formation* (CF) test. For families with two sample children ages 5 to 11, we randomly selected one child to take the CF test.

B1.3 The 3-in-10 subsample

After completion of the main field period for the followup survey, we implemented a subsampling procedure to reduce non-response bias. Our strategy was to continue to work 3 in 10 of the cases that had not been completed during the main field period. By continuing to work a random subsample of cases, we were able to achieve a higher effective response rate than if we had used the same resources to continue to work the full sample. The higher effective response rate helps reduce the bias due to non-response. The effective response rate is equal to the main sample response rate (MRR) plus the subsample response rate (SRR) multiplied by one minus the main sample response rate: MRR+ SRR*(1 – MRR).

The subsample period began after June 22, 2002 for Los Angeles and Baltimore and after July 6, 2002 for Boston, Chicago and New York. Subsample field work continued until September 15, 2002 across all five sites. We identified subsample cases as those cases that had any major component missing at the end of the main field period and that had a family ID number ending with one of three digits: 2, 5 or 8 for Baltimore and LA and 3, 6, or 9 for Boston, Chicago, and New York. We determined these last digits after the main field period had ended.

The subsample consisted of cases that had not been completed for any reason, including unlocated cases, initial refusals, and out-of-area cases. (Interviewers only traveled to out-of-area cases during the subsample period.) Within the same family and even for the same individual, some components may have been completed during the main field period while other components were either never completed or were not completed until the subsample period. This is particularly true for out-of-area cases. During the main field period, if a family was located out-of-area, the interviewer conducted a telephone interview with the adult and any sample children ages 10 to 19 (as of May 31, 2001).[3] If a family's ID number did not end in one of the digits selected for the subsample, no additional fieldwork was done beyond the telephone interviews. However, if the number ended in a selected digit, an interviewer traveled to the family to administer the WJ-R tests, conduct the Child Survey for

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2 A total of 6,694 children were originally selected for the sample. After the resolution of cases involving individuals who were core members of two different families and after updating the children's date of births using the best available information, the final child sample consisted of 6,683.

3 The management team decided that children under age 10 were too young to be interviewed by phone.

any children ages 5 to 9, weigh children ages 5 to 11, and take the blood pressure of the adult respondent.

B2. Data Sources and Analyses

B2.1 Data sources

Our analyses utilize data from direct survey, proxy report, measurement, testing, interviewer observation, administrative data, and published data sources. Below we briefly describe each of these sources and in exhibit B.1 we summarize the data sources for the adult and child samples.

A) *Direct Survey* –Our direct survey sources include interviews with the sample adult using the Adult Survey and with the sample children using the Child Survey (ages 8 to 11) or the Youth Survey (ages 12-19). Most of the interim surveys were conducted in face-to-face interviews; however, some telephone interviews were conducted for out-of-area cases.

B) *Proxy Report* –We asked the sample adult about the education, health, behavior and time use of the sample children using the Parent-on-Child/Youth (POCY) module of the Adult Survey.

C) *Measurement* – The interviewers measured the height and weight of children ages 5 to 11 and took the blood pressure of the sample adults.

D) *Testing* –Interviewers administered the following Woodcock Johnson Revised (WJ-R) achievement tests to the sample children: Letter-Word Identification, Passage Comprehension, Calculation, and Applied Problems. In addition, they administered the WJ-R *Concept Formation* test to children ages 5 to 11 (up to one child per family).

E) *Interviewer Observation* – The interviewers recorded their observations of the adult respondent's interaction with a sample child ages 5 to 11, the adult respondent, the interior of the home, and the exterior of the home.

F) *Administrative Data* – We collected administrative data from federal, state, and local agencies on an individual's employment and earnings, housing assistance, AFDC/TANF and food stamps assistance, and criminal justice involvement. In addition, we collected information on the characteristics of the schools that MTO children attended.

G) *Published Data* – We constructed aggregate neighborhood characteristics such as unemployment rates using data from the Census Bureau, Department of Housing and Urban Development (HUD), the Federal Bureau of Investigation (FBI), local law enforcement agencies, and other sources.

EXHIBIT B.1
SAMPLES AND DATA SOURCES

	Adult	Child
Direct Survey	Adult Survey	Child Survey (ages 8 to 11) or Youth Survey (ages 12 to 19)
Measurement	Blood pressure	height and weight (ages 5 to 11)
Proxy Report		POCY Module of the *Adult Survey*
Testing		Woodcock-Johnson Revised
Interviewer Observation	Adult-child interaction, adult respondent, interior of home, and exterior of home.	
Administrative Data on Individuals and Schools	Unemployment insurance, AFDC/TANF and food stamps, housing assistance, and criminal justice records.	School and school district characteristics and juvenile criminal records.
Published Data on Neighborhood Characteristics	Area statistics from HUD, the Census Bureau, FBI, and from local police departments.	Area statistics from HUD, the Census Bureau, FBI, and from local police departments.

B2.2 Child-centered and adult-centered analyses

The adult-centered or household-centered analyses use outcomes and mediators constructed from the Adult Survey, interviewer observations, and administrative data. We constructed core household aggregate outcomes using administrative data and constructed current household aggregate outcomes using Section G (Household Composition) of the Adult Survey; these are the only sources of information on all household members.

The child-centered analyses included child outcomes and mediators from the Child Survey, Youth Survey, the Parent on Child/Youth (POCY) module of the adult interview, Woodcock-Johnson tests, and administrative data. Because the POCY module was completed as part of the adult interview, if a sample child no longer lived with the sample adult, the POCY information was therefore reported by someone who did not live with the child. Similarly, for these “child leavers” the mediators contained in the main sections of the Adult Survey, such as community monitoring of youth and neighborhood quality, may not reflect the child’s neighborhood. Thus, for these Adult Survey mediators, we only conducted adult-centered analyses.

B2.3 Household and neighborhood measures

We used three types of household measures in our analyses: current household characteristics; current household aggregates; and core household aggregates. Each is described below.

Current household characteristics. We created household and neighborhood characteristics for an individual's current household using data from the Adult Survey, Youth Survey, Child Survey, interviewer observation, and published statistics such as crime rates. We created Current Household Characteristics for each individual from his or her responses to the survey. For children, we have additional household and neighborhood information reported by the sample adult.

Current household aggregates. We constructed household aggregates such as income for an individual's current household using the Adult Survey and administrative data. For the sample adult, we constructed Current Household Aggregates such as income. For the child sample, we did not have current household income for child leavers and thus we could not construct current household income.

Core household aggregates. We created household aggregates such as earnings for all members of the original core household using the Adult Survey and administrative data. After creating the core household aggregates, using the administrative data and the Adult Survey, this information was then merged by household onto the adult sample and the child sample.

B3. Estimation Methods

B3.1 Comparisons

The MTO experiment was designed to measure the effects of providing housing vouchers or certificates to families in public housing projects. Specifically, the demonstration is designed to estimate the effects of (a) living in private housing of the family's own choosing relative to living in public housing and (b) living in private housing in a low-poverty area relative to living in public housing. To address these questions, families were randomly assigned to the control group, the Section 8 group, or the experimental group.[4] As agreed upon previously, we define treatment compliance as leasing up with a voucher or certificate provided through the MTO program. On average, a family in the treatment group had 4 to 6 months after random assignment to leaseup using a voucher or certificate (for more details see Feins, McInnis, Popkin, 1997).

We performed two basic comparisons:

- Section 8 versus Controls, and
- Experimental group versus Controls.

A third possible comparison would have been to compare the Section 8 and Experimental groups with each other; however, we did not make such a comparison. Such a comparison would not have added any information to the two comparisons we did make and, in fact, would have been less informative than those comparisons, because it would not provide a measure of the net effect of either treatment.

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4 In some previous HUD reports, these groups have been labeled the "MTO Experimental," "Section 8 Comparison," and "In-place Control" groups. We used simpler labels (e.g., "Experimental," "Section 8", and "Control") for the Interim Evaluation for ease of exposition.

It should be noted, however, that the difference in impacts estimates between the Section 8 and experimental groups reflect not only effects on "compliers" but also the differential take-up rates for the two treatment groups (i.e., the composition of compliers may differ between the two groups)**.**

B3.2 Estimation of Intent-To-Treat (ITT) effects

In a randomized experiment, the difference in mean outcomes for the treatment and control groups provide an estimate of the impact of being offered the treatment. This estimate captures the average Intent-to-Treat (ITT) effect across all of the individuals included in the study, regardless of whether or not an individual assigned to the treatment actually complied with the treatment. Using a linear regression model, we can estimate the effect of Intent-to-Treat on outcome (Y) using whether an individual (indexed by *i*) was randomly assigned to the group offered the treatment (Z=1) or to the group not offered the treatment (Z=0):

(1) $Y_i = \alpha + Z_i\pi_{ITT} + \varepsilon_i$

where Z_i indicates assignment status and π_{ITT} (the coefficient on Z_i) captures the ITT effect.

To reduce the residual variation and thereby increase the precision of our estimate, we included in our regression models individual and household characteristics observed prior to random assignment (i.e., baseline characteristics):

(2) $Y_i = \alpha + Z_i\pi_{ITT} + X_i\beta + \varepsilon_i$

where X represents a vector of characteristics for each individual (indexed by *i*), β represents the vector of coefficients for X, and α represents a constant. For all analyses using data pooled across sites, X included fixed-effects or dummy variables for each of the sites (with NY serving as the omitted or reference category). For simplicity, we used a linear regression model for the dichotomous as well as the continuous outcomes. Robust standard errors were necessary due to heteroscedasticity.

In the MTO demonstration, individuals were randomized into two different treatment groups. We could have estimated the effect of these treatments using separate regressions for the Section 8 group (denoted by superscript *s*):

(3) $Y_i = \alpha + Z^s_i\pi^s_{ITT} + X_i\beta + \varepsilon_i$

and for the Experimental group (denoted by superscript *e*):

(4) $Y_i = \alpha + Z^e_i\pi^e_{ITT} + X_i\beta + \varepsilon_i.$

For greater efficiency, however, we used a single regression that included separate dummy variables for assignment to the Section 8 treatment group (Z^s) and to the Experimental treatment group (Z^e) to estimate the two treatment effects:

(5) $Y_i = \alpha + Z^s_i\pi^s_{ITT} + Z^e_i\pi^e_{ITT} + X_i\beta + \varepsilon_i.$

The coefficients π^s_{ITT} and π^e_{ITT} are the estimated impacts of the Section 8 and Experimental treatments, respectively.

B3.3 Estimation of Treatment-on-Treated (TOT) effects

Another estimate of interest is the Treatment-on-Treated (TOT) effect – the effect of actually receiving the treatment. Under the weak assumption that the effect of the treatment occurs entirely through moving using an MTO program voucher or certificate, the TOT is the difference in outcomes between those individuals in the treatment group who complied with treatment by leasing up and those in the control group who would have complied or leased up if offered the treatment.[5] More formally, we can define the "compliers" (C=1) and "never takers" (C=0) as in exhibit B.2 below.

Note that, for simplicity, this table describes the case of a single treatment group.

EXHIBIT B.2
DEFINITIONS OF COMPLIERS AND NEVER TAKERS

	All	**Complier (C=1)**	**Never Taker (C=0)**
Offered the Treatment (Z=1)	"Treatment Group" – offered a voucher. Sample mean or E[Y|Z=1] observed.	"Treatment Compliers" – offered a voucher and used it to move. Sample mean or E[Y|C=1, Z=1] observed.	"Treatment Never Takers" – offered a voucher but did not use it to move. Sample mean or E[Y|C=0, Z=1] observed.
Not Offered the Treatment (Z=0)	"Control Group" –not offered a voucher. Sample mean or E[Y|Z=0] observed.	"Control Compliers" – not offered a voucher but would have used a voucher to move if offered one. Sample mean or E[Y|C=1, Z=0] not observed.	"Control Never Takers" –not offered a voucher and would not have used a voucher to move even if offered one. Sample mean or E[Y|C=0, Z=0] not observed.

Using these definitions the effect of the Treatment-on-Treated can be expressed as the difference in the expected value of outcome Y for "treatment compliers" and "control compliers":

(6) $TOT = E[Y|C=1, Z=1] – E[Y|C=1, Z=0]$.

While we observed who in the treatment group was a complier based on whether they leased up through the program, we did not observe who in the control group would have leased up if they had been offered a voucher. In this analysis, we used an adjustment attributed to Bloom (1984) to infer the TOT impact from the ITT impact.

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[5] The housing search process may affect the non-compliers; however, we would expect these effects to be small in comparison to the main effects on compliers.

Under the assumption that the treatment occurs entirely through using an MTO program voucher to move, the entire ITT effect is due to differences between the compliers in the treatment group and the would-be compliers in the control group since the outcomes for the non-compliers in both groups should be the same.[6]

Algebraically, we know that the effect of the intent-to-treat is the difference in the expected values of the outcomes for the treatment and control group:

(7) $\quad ITT = E[Y|Z=1] - E[Y|Z=0].$

We can express the expected values for each group as the expected value for the compliers and non-compliers weighted by the proportion who comply with treatment or leaseup (ρ) and the proportion who do not ($1 - \rho$):

(8) $\quad E[Y|Z=1] = (\rho)\, E[Y|C=1, Z=1] + (1 - \rho)\, E[Y|C=0, Z=1]$

(9) $\quad E[Y|Z=0] = (\rho)\, E[Y|C=1, Z=0] + (1 - \rho)\, E[Y|C=0, Z=0].$

Substituting back into Equation 7, the ITT effect can be rewritten as:

(10) $\quad ITT = (\rho)\, E[Y|C=1, Z=1] + (1 - \rho)\, E[Y|C=0, Z=1]$

$\quad\quad - (\rho)\, E[Y|C=1, Z=0] - (1 - \rho)\, E[Y|C=0, Z=0].$

Under our assumption that the treatment has no effect on the non-compliers, the expected value of the outcome for treatment non-compliers and control non-compliers will be the same and these two terms will offset each other leaving:

(11) $\quad ITT = (\rho)\, E[Y|C=1, Z=1] - (\rho)\, E[Y|C=1, Z=0].$

Dividing both sides by ρ:

(12) $\quad ITT/\rho = E[Y|C=1, Z=1] - E[Y|C=1, Z=0],$

we see that the ITT effect divided by the proportion of compliers equals the difference in expected outcomes for the treatment compliers and the control compliers. By definition this difference is the effect of the TOT. Thus, we can estimate the TOT by dividing the ITT by the proportion of leaseups:

(13) $\quad TOT = ITT/\rho.$

If we can regard ρ as fixed (i.e., if it does not vary from sample to sample, so that we can assume that the lease-up rate would have been the same in the control group), the standard error of the TOT estimate is:[7]

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[6] This derivation of the TOT estimate is due to Bloom (1984).

[7] While not strictly true, this assumption should be very close to correct in large samples.

(14). $SEE_{TOT} = SEE_{ITT} / \rho$,

i.e., in going from the ITT estimate to the TOT estimate, the standard error of estimate is inflated by exactly the same factor as the impact estimate itself. This means that the t-statistic of the estimate is unchanged by this transformation. The same procedure for calculating the TOT standard error can be used for both simple mean differences and regression adjusted ITT estimates.

Some members of all three groups may obtain Section 8 vouchers on their own initiative rather than through the MTO program. An alternative definition of the treatment for the Section 8 group could have been *any* move through a Section 8 voucher (i.e., not limited to moving through an MTO program voucher). However, we did not report such TOT estimates for several reasons. First, they would have required us to make stronger assumptions than we made above to estimate the TOT effect of leasing up only through the program and we felt these stronger assumptions were implausible.[8]

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[8] The three assumptions that would be necessary for estimating the TOT of any Section 8 voucher are as follows.

i) The MTO program has no effect on families who do not leaseup through any form of Section 8 (this is similar to the assumption in the more standard TOT analysis above).
ii) The MTO program has no effect on families who would obtain Section 8 whether or not they were assigned to the treatment group.
iii) All Control group families who receive Section 8 on their own initiative would have received Section 8 (either from MTO or on their own) if they had been assigned to a treatment group.

The second assumption, that the MTO program has no effect on those who always leaseup using a Section 8 voucher, is presumably only plausible for the Section 8 group since the Experimental group was offered counseling services and offered a voucher restricted to certain neighborhoods. Even for the Section 8 group, the assumption is problematic since families assigned to the treatment group would presumably have leased up over a shorter time period than if they had obtained a Section 8 voucher on their own. Thus, to make Section 8 participation on one's own more comparable to Section 8 participation through the program, we would want to restrict Section 8 participation to a similar time period since randomization. Moreover, once we make the necessary restriction on timing, the number of non-MTO program Section 8 families may be so small that the estimated TOT will not be very different from the program TOT discussed above.

More formally, let λ be the fraction of the treatment group that obtains Section 8 from any source, let υ be the fraction of the control group that obtains Section 8 (on their own).

If we define,

- <u>Compliers</u> - people who receive Section 8 if in the treatment group, but not if in the Control group (C=1);
- <u>Always takers</u> - people who receive Section 8 if in any group (C=2), and
- <u>Never takers</u> - people who do not receive Section 8 in either group (C=0)

then:

(15) $$\begin{aligned} ITT = \; & (\lambda - \upsilon)E[Y|Z=1, C=1] + \upsilon E[Y|Z=1, C=2] + (1-\lambda-\upsilon)E[Y|Z=1, C=0] \\ & - (\lambda - \upsilon)E[Y|Z=0, C=1] - \upsilon E[Y|Z=0, C=2] - (1-\lambda-\upsilon)E[Y|Z=0, C=0]. \end{aligned}$$

Second, defining the treatment as use of any voucher would require us to adjust away the effects of nondemonstration vouchers in the control group; we believe that the appropriate counterfactual for the two treatment groups is a control group that receives nondemonstration vouchers. Estimates based on this counterfactual measure the effects of replacing the current mix of housing subsidies for public housing residents with a program of locationally restricted vouchers (in the experimental group) or unrestricted vouchers (in the Section 8 group). Alternatively, if one views the MTO impacts as the effects of changing the neighborhoods in which the families live, treatment is more appropriately defined as the difference in neighborhood characteristics between the treatment and control group, however that difference is achieved.

B3.4 Variation in random assignment ratios

To produce unbiased estimates of treatment effects, we needed to take into account the variation in random assignment (RA) ratios. At the start of the experiment, RA ratios were set to produce equal numbers of leased-up families in the Section 8 and Experimental groups (based on expected leaseup rates). In the midst of the experiment, these RA ratios were adjusted to accommodate higher than anticipated leaseup rates for MTO families in comparison to the rates for the Section 8 group. On average, RA ratios were adjusted twice at each site (see appendix A of Goering et al., 1999). Within each of the 15 resulting site-ratio periods applicants were randomly assigned to the control and treatment groups. However, across the entire experiment, the change in random assignment ratios produces systematic differences in timing and site location between the control group and each of the treatment groups. Thus, a simple comparison of means across the pooled sample that did not take into account the changes in the ratios could have produced biased estimates by confounding the treatment effect with site and period.

The approach we used to remove the systematic relationship between site-time period and assignment status was to assign weights to each observation. Using weights to adjust for the ratio changes offered the advantage of producing a single estimate of the treatment effect and allowing for comparisons of means, as well as simplifying the estimation procedure.

The weights selected had to eliminate the relationship of assignment to both site and time period. A number of different weights could have been used to accomplish this. We created weights that preserved the overall proportions of Control, Section 8, and Experimental families in the sample. Section 6.1 of this memo describes the construction of these weights.

B3.5 Effects for subgroups determined prior to randomization

Estimating impacts for different subgroups of participants helps us better understand who may benefit the most from MTO and on which types of outcomes. For example, in the early impact evaluations youth seemed to be particularly impacted by the treatment. In dividing the sample into subgroups, it was important to rely upon characteristics determined prior to randomization. Below are some of the categories for which we estimated subgroup impacts:

The three assumptions above allow us to drop the second, third, fifth, and sixth terms. We can therefore identify the impact of Section 8 for the compliers as ITT/(λ - υ). Note that this simply scales the ITT number by a bigger multiple than in the standard TOT estimate.

- race/ethnicity,
- welfare status,
- work status,
- gender of child,
- age group of child,
- child behavior problems,
- age of sample adult, and
- family size.

Subgroup estimates for exhaustive, mutually exclusive subgroups were derived by interacting the treatment dummy with dummies for each subgroup (e.g., Worked at Baseline, Did Not Work at Baseline). We adopt this approach, rather than running separate regressions for each subgroup, because of its computational simplicity and the fact that it allows straightforward joint tests for the statistical significance of the differences in impacts among subgroups.

In most cases, the impact coefficients for the different subgroups were of interest in their own right. For example, it was of interest to see whether moving from public housing to a low-poverty area had a larger effect for whites or non-whites. In the case of the subgroups formed by the sites, however, our interest was simply in the variation of impacts across sites, since HUD's interest was not in these specific sites, but in how neighborhoods affect these outcomes more broadly. Of course, if it had been possible to attribute differences in impacts across sites to differences in site characteristics, that would have been very valuable information. Unfortunately, that was not possible. With only five sites, which differ in innumerable potentially relevant ways, it was simply not possible to disentangle the underlying factors that cause impacts to vary across sites. (This is true for both the quantitative analysis and for any qualitative analysis of the impacts that might be undertaken.) Therefore, we did not report identifiable results by site.

B3.6 Effects for different periods since random assignment

On all survey measures there was considerable variation in the time since random assignment. However, this variation captures both time since randomization and cohort. This makes it impossible to interpret the variation in impacts over time. Therefore, for survey outcomes for which we have only a single point-in-time measure, we simply estimated impacts on that measure, conditional on the distribution of time since random assignment in the analysis sample.

For some outcomes, such as quarterly earnings and public assistance, we had longitudinal administrative data outcomes. For these outcomes, we estimated treatment effects for different periods since random assignment by performing separate regressions for specific periods since random assignment. For example, we treated earnings in the first two years after randomization as the outcome measure in one regression and earnings in the third and fourth years after randomization as the outcome in a separate regression. One advantage of using separate regressions to estimate treatment effects for each period is that it allows the effect of baseline characteristics to differ depending on the period.

Longitudinal measures expressed in monetary terms, such as earnings and income, were converted to constant 2001 dollars using the Bureau of Labor Statistics' Consumer Price Index for All Urban Consumers (CPI-U).

B3.7 Clustering on household

In our child analyses we included up to two individuals from each household. As a result, we could not assume that the residuals were independent across all observations. To address this issue, we adjusted the standard errors by clustering on household.

B3.8 Cell-level analyses

For four of the five states, we were able to obtain individual-level administrative data on earnings and employment. However, for the fifth state (Massachusetts), we were only able to obtain data aggregated across groups of at least 10 MTO participants. For methodological consistency, we constructed cells for all five sites using the same algorithm. The cell-level analyses used the mean outcome for the cell and mean covariates for the cell for a limited set of covariates, and were weighted randomization ratio weight for individuals within that cell multiplied by the number of individuals in the cell. We used only a reduced set of covariates for the cell analyses, because the cells only have substantial variation on certain covariates that were used in the algorithm to split individuals into cell that were as homogeneous as possible. For the adults, the reduced set of covariates consisted of site dummies, baseline work status and education, and dummies for quarter of randomization. For the youth, the reduced set of covariates consisted of site dummies, single-year age dummies, and gender.

B4. Outcomes and Mediators

B4.1 Outcome domains

A large number of different variables in each of the domains were available for analysis. The analysis team for each domain selected the most important outcomes and mediating factors to include in the evaluation. The six domains are:

- Housing conditions and assistance,
- Health,
- Delinquency, crime, and risky behavior,
- Education,
- Employment and earnings,
- Household income and public assistance.

In the MTO Research Design Report (Orr, Feins, and Popkin, 2001), we laid out a number of theories about the pathways through which neighborhood might affect the outcomes of interest in each of the six domains. These theories predicted impacts on a number of "mediating factors." To test these theories, we estimated impacts on each of these mediating factors, using the same models as we used for the outcomes. It must be recognized that these are relatively weak tests, because impacts on a given mediating factor may be consistent with multiple theories of causation. Where the theory predicts an impact on a mediating factor and none is found, however, we can reject the theory.

Because they were measured after random assignment, mediating factors were never used as right-hand-side variables in the impact regressions. We were not trying to "model" the path of effects; we were subjecting the implications of the causal theories to statistical tests.

B4.2 Non-experimental analyses for endogenous subgroups

We observed outcomes such as wages or housing search discrimination only for the subgroups of respondents who worked or moved. Since these subgroups are endogenous to the treatment, we could not identify experimental impacts. For example, although we could have observed differences in wages between the treatment and control groups, these differences would not be experimental estimates of the impact on wages because we could be observing wages for different mixes of respondents from each group. We did, of course, create some descriptive tabulations to help understand the experience of families in these subgroups. Because of the risk of selection bias, however, we did not attempt to estimate impacts for these groups using nonexperimental methods.

It is important to note that for many outcomes we were able to define the outcome so that we did not create endogenous subgroups. For example, we defined earnings to include zero earnings.

B4.3 Categorical measures

For these analyses, we converted categorical outcomes to dichotomous ones. The cut-point for dividing the categories depended on the type of categories, the distribution of the data, and on expository or theoretical considerations as well. In general,

a) we divided non-polarized categorical data using the central cut-point or the point that divided the data as close to 50:50 as possible, and

b) we divided Likert-type scales in a way that maintained the distinction between agree and disagree (except in those instances in which the data were extremely skewed).

In some cases, we selected a cut-point that differed from the above guidelines in order to be consistent with the existing literature or to be internally consistent with the coding of closely related variables.

B4.4 Addresses of participants

We have participant address information from a variety of sources. The quality of this address information depends on the source of the information and the frequency with which it was collected. Address information based on actual contacts with respondents is highly reliable for the point in time at which it was collected; unfortunately, we have had at most four relatively widely spaced contacts with the families – at baseline, at the 1997 and 2000 canvasses, and at the interim followup survey. These data may therefore miss a number of moves that occurred between these contacts. Moreover, even for the addresses they capture, they cannot tell us with any precision the date on which the family moved to that address. In addition, we have information that was gathered more continuously through the tracking process, from change of address forms, credit bureaus, and the Public Housing Authority/Multifamily Tenant Characteristic System Data (PHA/MTCS). The addresses provided by these data are probably of lower reliability than those collected through direct contact with the families, but they are more likely to include a complete set of addresses and, where the addresses were accurate, they allowed us to date the families' moves more precisely.

Therefore, in constructing address histories, we used all available addresses except those that could not be geocoded using standard commercial software for standardizing, parsing, and geocoding addresses.

B4.5 Exposure measures

The MTO intervention is expected to affect sample members' outcomes through exposure to a different environment than the one they would have experienced had they remained (at least initially) in public housing – i.e., attributes of the local community are important intervening factors. But the degree to which the families' environment was actually changed is not self-evident – even for those who moved through the demonstration. Section 8 families tended to stay in high-poverty areas, some MTO families moved back into high-poverty areas after their initial move to a low-poverty area, and some controls moved out of public housing. We needed to measure the treatment-control difference in exposure to important community influences (e.g., poverty level, school quality, community values and mores, safety, etc.) in order to understand the strength of the intervention. Unfortunately, the same factors that made this important made it difficult. We could not simply measure the attributes of the community in which the family lived at followup and assume that that was the environment that had shaped their behavior over the entire followup interval. We had to somehow aggregate the *different* environments the family had experienced since the initial intervention.

We did so by taking the time-weighted average of the different communities in which the family had lived between random assignment and the followup interview. If, for example, a family lived in Community A for the first two years after random assignment and Community B for the next three years, up to the time of the followup interview, we would measure the local crime rate to which it was exposed as a weighted average of the crime rates in Communities A and B, with weights two-fifths and three-fifths, respectively. We would treat the resulting measure as an intervening factor, estimating the impact of the intervention on it to obtain a measure of how much the demonstration changed the crime rate to which the family was exposed, on average, over time.

This measure is admittedly arbitrary. For example, it gives the same weight to community influences experienced early in the followup period as it does to those experienced closer to the followup survey. To the extent that family behavior and outcomes primarily reflect recent experience, this presumably places too much weight on the early experiences. But one could equally well argue that the outcomes reflect the families' cumulative experience, or that they reflect environmental influences with a lag. In the absence of confident knowledge of the functional form of this relationship, we used a simple weighted average.

For all community-level mediators (i.e., small area statistics), we constructed both a current measure and a measure that reflects exposure since randomization.

B5. Covariates

B5.1 Baseline covariates

As mentioned above, we included covariates in our regression models to improve the precision of our estimates. Since individuals were randomly assigned to control and treatment groups, the addition of these covariates does not affect the expected value of the estimate itself. All covariates had to be characteristics that were known (or determined) prior to randomization. In selecting covariates, we considered (a) the importance of the variable in predicting the outcomes of interest, (b) the extent of variation on the variable for the sample, and (c) the completeness of the data.

For relatively small numbers of missing values (less than 5 percent of all observations), we imputed values using the mean for non-missing observations weighted by the weights constructed to equalize RA ratios. We conditioned the imputed means for missing adult or household covariates on the site. For missing child covariates that were applicable to children of that baseline age we imputed the mean conditional on age at baseline and gender (if known) as well as site. To impute the child means we used all non-missing observations of core children who were under age 18 at baseline (including those who were 20 and older as of May 31, 2001). If a child covariate was not applicable to a particular child because of the age of the child at baseline, we assigned the covariate a value of zero. (For families randomized in 1998, means were imputed separately.)

We used the following baseline covariates in all impact analyses:

Baseline characteristics for the sample adult, household or baseline respondent

- Site (dummies; omitted category: NY)
- Current age of sample adult (dummies for ages 19-29, 30-39, 40-49; omitted category: 50+)
- Ethnicity of sample adult (dummy for Hispanic; omitted category: non-Hispanic)
- Race of sample adult (dummies for African-American and Other non-white race; omitted category: white)
- Marital status of sample adult (dummy for never married; omitted category: married, separated, or divorced)
- Sample adult is male (dummy)
- Sample adult was working (dummy)
- Sample adult was under age 18 at birth of first child (dummy)
- Sample adult was enrolled in school (dummy)
- Sample adult was a high school graduate (dummy)
- Sample adult had a GED (dummy)
- Size of core family (dummies for 1-2, 3, 4; omitted category: 5+)
- Core household did not contain any teen children (ages 13 to 17) at baseline (dummy)
- Any householder had been robbed, assaulted, or threatened with a weapon within the six months prior to the survey (dummy)
- Any householder was disabled (dummy)
- Baseline Respondent was receiving AFDC/TANF (dummy)
- Baseline Respondent had a car that runs (dummy)
- Baseline Respondent had moved more than 3 times in past 5 years (dummy)

- Baseline Respondent had no friends in neighborhood (dummy)
- Baseline Respondent had no family in neighborhood (dummy)
- Baseline Respondent had lived in neighborhood for 5 or more years (dummy)
- Baseline Respondent had previously applied for Section 8 voucher or certificate (dummy)
- Baseline Respondent's primary or secondary reason for moving was to get away from drugs and gangs (dummy)
- Baseline Respondent's primary or secondary reason for moving was for better schools for children (dummy)
- Baseline Respondent was very dissatisfied with neighborhood (dummy)
- Baseline Respondent considered streets near home very unsafe at night (dummy)
- Baseline Respondent stopped to chat with a neighbor in the street or hallway at least once a week (dummy)
- Baseline Respondent was very likely to tell neighbor if saw neighbor's child getting into trouble (dummy)
- Baseline Respondent was very sure would be able to find an apartment in a different area of the city) (dummy)

For the analyses using the child data, we also included child characteristics from the baseline survey. The baseline survey had two separate forms for children – one for children ages 0 to 5 and one for ages 6 to 17. Some of the same questions were asked on both forms; however, other questions were specific to one age group or the other.

In the analyses, we included covariates from both child forms along with a dummy variable for age group at baseline (i.e., 6 to 17) to indicate which form was relevant to the child. This strategy allowed us to use covariates from both forms, avoided creating separate impacts for different subsets of children, and was consistent with our general strategy for handling covariate non-response.

We controlled for the following child characteristics, listed below by form.

Baseline child characteristics from both forms and current age

- Child is male (dummy; omitted category: female)
- Child's age (single-year age dummies; age as of May 31, 2001)
- Child's age at Baseline was 6 to 17 (i.e., "6 to 17" form should have been filled out for child)
- Child has problems that require special medicine or equipment (dummy)
- Child has problems that make it hard to get to school or to play active sports or games (dummy)

Baseline child characteristics from the "0 to 5" form

- Child weighed less than 6 pounds at birth (dummy)
- Child was in the hospital before his/her first birthday because the child was sick or injured (dummy)
- Someone in home reads a book or story to the child more than once a day (dummy)

Baseline child characteristics from the "6 to 17" form

- Special class or help for learning problems during the 2 years prior to baseline (dummy)
- Special class or help for behavioral or emotional problems during the 2 years prior to baseline (dummy)
- Suspended or expelled from school during the 2 years prior to baseline (dummy)
- School asked someone to come in and talk about problems child was having with school work or behavior during the 2 years prior to baseline (dummy)
- Went to special class for gifted students or did advanced work in any subjects (dummy)

B5.2 Pre-randomization values of outcomes

For outcomes for which we had both pre and post-randomization information, we controlled for the pre-randomization values of these outcomes in our analyses. Administrative data outcomes such as employment, earnings, welfare receipt, food stamps, and arrests often offer both pre- and post-randomization observations. For these outcomes, we generally included annualized outcomes for one or two years prior to randomization if available.

B5.3 Variation in timing

To produce more precise estimates we also included controls for the timing of random assignment and data collection. For outcomes collected at a single point in time, as in surveys, the time since random assignment will capture both cohort and timing effects. For longitudinal administrative data such as quarterly earnings, we generally controlled for the calendar quarter, as well as the number of quarters since random assignment.

B6. Weights

We weighted the MTO data for all of the interim analyses. We weighted both the administrative and survey data to adjust for the changes in randomization ratios during the study. In addition, we weighted the survey data to account for sampling. We sampled up to two children from each family for the survey component, up to one of the sampled children from each family for the Concept Formation test, and three of every ten non-completes for the subsample of hard-to-interview families. In the sections below, we describe the weights that were needed for each of these adjustments. In the last section, we summarize the weights and describe how we combined them for different analyses.

B6.1 Randomization ratio weights

As we discussed in Section 3.4, we used weights to adjust for randomization ratio changes. We used weights that preserved the overall proportions of Control, Section 8, and Experimental families in the sample.

For each group, site and ratio period, the weight applied was equal to:

(16) $w_{jkh} = (n_j / n) / (n_{jkh} / n_{kh})$

where:

- j indexes the treatment and control groups,
- k indexes the site, and
- h indexes the RA ratio period

such that:

- n_j = total # of observations for a specific group (Control, Section 8, or Experimental),
- n = total # of observations in the entire sample,
- n_{jkh} = # of observations for a specific group, site, and period, and
- n_{kh} = # of observations for a site and period combining all groups.

Exhibit B.3 shows the actual weights for each site-ratio period using this formula. When using these RA ratio weights in conjunction with the child sampling weights, the combined weight is the product of the RA ratio weight and the sampling weight.

EXHIBIT B.3
RANDOM ASSIGNMENT RATIO WEIGHTS

	RA Ratios (Exper'l to Section 8 to Control)	RAR Dummy Variable #	Dates in Effect	Weight[a] for Exper'l Group	Weight[a] for Section 8 Group	Weight[a] for Control Group
Baltimore	8:3:5	1	< 02/01/96	0.82	1.49	1.00
	3:8:5	2	>= 02/01/96	2.11	0.57	1.00
Boston	8:3:5	1	< 03/01/96	0.81	1.52	0.99
	3:6:7	3	>= 03/01/96 & < 07/24/97	2.15	0.76	0.71
	8:5:3	7	>= 07/24/97	0.82	0.90	1.66
Chicago	8:3:5	1	< 11/09/96	0.82	1.50	0.99
	10:3:3	6	>= 11/09/96 & < 11/26/97	0.66	1.49	1.62
	6:7:3	8	>= 11/26/97	1.10	0.65	1.63
Los Angeles	8:3:5	1	< 03/20/96	0.82	1.49	0.99
	4:4:6	4	>= 03/20/96 & < 05/01/98	1.43	1.00	0.72
	3:7:4	11	>= 05/01/98	NA	NA	NA
New York	8:3:5	1	< 07/24/96	0.81	1.49	1.00
	5:7:4	5	>= 07/24/96 & <10/24/97[b]	1.30	0.65	1.22
	3:7:6	9	>=10/23/97[c] & < 12/03/97	1.91	0.64	0.91
	8:4:4	10	>= 12/03/97	0.79	1.17	1.27

a. Weights shown are rounded to two decimal places and apply to cases randomized through 1997.
b. For New York assignments on 10/23/97 the 5:7:4 ratios only applied to sequence numbers in the 2,000 range.
c. For New York assignments on 10/23/97 the 3:7:6 ratios only applied to sequence numbers in the 3,000 range.

Source: Goering et al., 1999.

B6.2 Survey sample selection weights

We had survey data for two samples: the sample adults and sample children. No adjustment for selection of adults into the sample was necessary since we selected one sample adult from each core household. For the children, however, we needed weights to adjust for the fact that we interviewed a random sample of children (up to two per family) rather than all eligible children.

The likelihood of including a child in the sample depended on the number of children ages 5-19 in the core household.[9] Since a maximum of two children were sampled from each family, children in

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[9] We did not have the exact date of birth (DOB) for all children at the time of sample selection. Sample selection was based on an imputed age for children without a DOB. Our final weights reflect any new information on the DOBs of core members (i.e., the number of children ages 5 to 19 in a family sometimes differs from our initial calculation).

families with more than two children had a lower probability of selection. We weighted the data in order to produce average treatment effects across all children. We weighted each observation by the inverse probability of the child being selected into the sample. The probability of a child being selected into the sample was:

(17) Sampling Probability $= \dfrac{\#\,selected}{\#\,eligible}$

where:

$\#\,selected$ = the number of children ages 5-19 in the family who were selected for the interim evaluation,

$\#\,eligible$ = the total number of core children ages 5-19 in the family who were eligible to be selected for the interim evaluation.

The weight is equal to the inverse of the sampling probability such that:

(18) Child Survey Sample Weight $= \dfrac{\#\,eligible}{\#\,selected}$

We calculated age as of May 31, 2001 using updated information on date of birth. Note that since "# selected" and "# eligible" rely on updated date of birth information it is possible to have only one selected child in a family with two or more eligible children (i.e., if it turned out that a selected child was not age 5-19).

No special weighting was necessary to pool children of different ages since selection was not stratified by age. Separate survey instruments were, however, used to interview children ages 8 to 11 and those ages 12 to 19.

B6.3 Concept formation child weights

We needed a special set of weights to analyze the results of the WJ-R Concept Formation (CF) test because we administered the test only to sampled children between the ages of 5 and 11 and we administered the test to no more than one child per family. For families with two sampled children ages 5 to 11, we randomly selected one child for the CF test.

To account for this additional selection step, we multiplied the child survey weights by the inverse probability of selecting a sample child between the ages of 5 and 11 for the CF test. (The selection of a child into the child sample was discussed in the previous section on "Survey Sample Selection Weights.") The probability of a sample child age 5 to 11 being selected for the Concept Formation test depends on whether there was a second sample child in the family and if so, on the age of the other sampled child. If there was only one sample child in the family, the probability of selecting the child (ages 5 to 11) for the CF test is one:

(19) Pr(Selection for the CF Test | Sample Age 5 to 11
and No Other Sample Children) = 1.0

If there was another sampled child, the probability of selection depended on the age of the other child. If the other child was between the ages of 12 and 19, the probability is 1.0. If the other child was also between the ages of 5 and 11, the child had only a 50% chance of being selected for the CF test. Since there were sometimes more than two eligible children in the family, the age of the other child had its own probability distribution. For families with two or more eligible children, the probability of selection for the Concept Formation test can be expressed as follows:

(20) Pr(Selection for the CF Test | Sample Age 5 to 11
and Another Sample Child Present)
= 1.0 * Pr(Age of Other Sampled Child is 12 to 19)
+ 0.5 * Pr(Age of Other Sampled Child is 5 to 11).

The probability of the other child being age 12 or older is the ratio of children ages 12 to 19 in the household (y) divided by the number of children (other than the sampled child) in the household (c – 1): $y/(c-1)$. The probability that the other child was age 5 to 11 is one minus the probability of being 12 to 19: $1 - [y/(c-1)]$. We can substitute these two probabilities back into equation 20:

(21) Pr(Selection for the CF Test | Sample Child Age 5 to 11
and Another Sample Children Present)
$= 1.0 * [y/(c-1)] + 0.5 * [1 - (y/(c-1))]$
$= 0.5 * [1 + (y/(c-1))]$

where:

c = the number of children age 5 to 19 in the core household, and
y = the number of youth age 12 to 19 in the core household.

In summary, the probability of selecting a sample child ages 5 to 11 for the CF test is:

(22) Sampling Probability for the CF Test for sample child ages 5 to 11

$$= \begin{cases} 1 & \text{if } \ c = 1 \\ 0.5 * [1 + (y/(c - 1))] & \text{if } \ c > 1 \end{cases}$$

where c = number of children ages 5-19 and y = the number of youth. The weight adjustment for the Concept Formation test is equal to the inverse of this sampling probability such that:

(23) CF Weight for Sampled Child Ages 5 to 11 $= \begin{cases} 1 & \text{if } \ c = 1 \\ 2 / [1 + (y/(c - 1))] & \text{if } \ c > 1 \end{cases}$

B6.4 The 3-in-10 subsample weights

We also adjusted the survey data to account for selection into the 3-in-10 subsample. After completion of the main field period, we randomly selected 3 in 10 uncompleted cases for further fieldwork. The probability of selecting one of these cases was 3/10. Thus, subsample components were weighted up by the inverse of this probability or by 10/3.

The main sample field period ended on June 22, 2002, for the LA and Baltimore sites and on July 6, 2002, for the Boston, Chicago, and New York sites. We considered a case as "uncompleted" at the end of this period if any of the instruments (2-3 depending on the sample type) had not yet been completed. Exhibit B.4 lists the specific instruments whose subsample eligibility was tracked for each sample type. Uncompleted cases with family id numbers ending in a selected digit (see Section 1.3) were eligible for the subsample.

EXHIBIT B.4
INSTRUMENTS TRACKED FOR EACH SAMPLE TYPE

Sample	Survey/Instrument 1	Instrument 2	Instrument 3
Adult	Adult Survey	Household Observation	Neighborhood Observation
Child 5-7	(not applicable)	Height & Weight	WJ-R
Child 8-11	Child Survey	(not applicable)	WJ-R
Child 12-19	Youth Survey	WJ-R	(not applicable)

We assigned the subsample adjustment weights using two nested criteria:

i) Was the instrument completed during the main field period?

- If yes, no adjustment was necessary and the subsample weight equals one.
- If not, ii) Was the instrument eligible for the subsample?
    - If yes, the weight of this observation was adjusted by a factor of 10/3.
    - If not eligible, this observation was not used in the weighted survey results and the subsample weight was set to zero.

Exhibit B.5 summarizes the relationship between the two criteria and the subsample weight adjustments.

EXHIBIT B.5
SUBSAMPLE WEIGHT ADJUSTMENT DEPENDING ON I) COMPLETION STATUS AT THE CLOSE OF THE MAIN FIELD PERIOD AND II) SUBSAMPLE ELIGIBILITY

	Eligible for the Subsample	Not Eligible for the Subsample
Complete During Main Period	1	1
Not Complete During Main Period	10/3	0

For simplicity, we assigned subsample weights to groups of survey items or measures rather than to each item. In addition to the instruments identified in exhibit B.4, we also needed subsample weights for two additional measures: a) adult blood pressure and b) height and weight of children ages 8-11. Separate weights were needed because the subsample status of these measures did not necessarily align with other instruments. (For example, although the Adult Survey and adult blood pressure measures were usually done at the same time, there were some adults with a telephone survey done during the main field period and a blood pressure taken during the subsample period.) Since subsample status for the two measures was not specifically tracked, we used final disposition information on telephone interviews to help determine subsample eligibility. We also created a separate weight for the POCY module.

In all, we created six subsample weights for the different instruments: survey (Adult, Child, and Youth), POCY, measurement (height and weight or blood pressure), Household Observation, Neighborhood Observation and WJ-R. Exhibit B.6 summarizes the different subsample weights and the criteria used to assign them:

- Sample (column 1) - indicates whether the weight applies to the adult or child sample,
- Instrument (column 2) - indicates the specific instrument,
- Weight Variable (column 3) - indicates the name of the subsample weight variable,
- Complete at end of Study (column 4) - shows the criteria for determining that the instrument was "complete" by the end of the field period,
- Complete at End of Main Sample Period? (column 5) - shows the criteria for determining that the instrument was completed during the main field period, and
- Eligible for the Subsample? (column 6) - shows the criteria for determining that the instrument was eligible to be pursued during the subsample field period. (Note that this is essentially a check of whether the last digit of the FAMID was eligible for the subsample and a double check that the criteria in column 5 are complete.)

Columns 5 and 6 determined the subsample weight:

- If Column 5 (Complete at End of Main Sample Period?) was true, the subsample weight was set to one (i.e., component was part of the main sample).
- If Column 5 was false AND Column 6 (Eligible for the Subsample?) was true, the subsample weight was set to 10/3 (i.e., component was part of the subsample).
- If neither Column 5 nor Column 6 was true, the subsample weight was set to zero (component was not completed during the main period and was not pursued during the subsample because the last digit of the family ID was not eligible).

In theory, we could have identified subsample components by interview dates after June 22, 2002, for LA and Baltimore and after July 6, 2002, for Boston, Chicago, and New York. However, due to data retrievals and postponed appointments, components with later dates are not always part of the subsample.

EXHIBIT B.6
INFORMATION USED TO DETERMINE COMPLETION STATUS AND SUBSAMPLE ELIGIBILITY FOR EACH INSTRUMENT

Sample	Instru-ment	Weight Variable	"Complete" at end of Study	i) Complete at End of Main Field Period? Yes if "Complete" at end of Study AND	ii) Eligible for the Subsample? Yes if last digit of family ID was eligible for subsample AND
Adult	Adult Survey	wt_310svy	FINAL_DISP is Telephone Complete, In-Person Complete, or Breakoff	a) SURVEY_MAIN = 1, or b) SUB_ELIG ~= 1	a) SURVEY_MAIN = 0, or b) FINAL_DISP not telephone, in-person, or breakoff.
Adult	Blood Pressure (BP)	wt_310msr	a) BP measurement, b) Adult Survey completed but unable to measure BP for reason other than "Telephone Interview," or c) "Telephone Complete" but no sample children[10]	a) SURVEY_MAIN = 1 and either: FINAL_DISP not equal to "Telephone Complete" or No Sample Children[11], or b) SUB_ELIG ~= 1	a) FINAL DISP = Deceased, b) SURVEY_MAIN = 0, or c) FINAL_DISP = telephone complete and sample children in the household.
Adult	Household Observati on	wt_310hobs	a)HHOBS_COMPLETED = 1, or b) HH_COMPLETED[12]	FINAL_DISP is not deceased[13] nor Cannot Locate (and inst2_sub ~= 1) AND either: a) INS2_MAIN = 1, b) SURVEY_MAIN = 1, or c) SUB_ELIG ~= 1	a) INS2_MAIN = 0 and SURVEY_MAIN = 0, or b) FINAL DISP = Deceased or Cannot Locate

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[10] If a telephone interview was conducted, subsample attempts to measure blood pressure were only made if there were also sample children in the household (e.g., who needed to be administered the WJ-R).

[11] "No sample children" refers to no sample children in the original sample selection and does not include families in which the children were later determined to be ineligible.

[12] In calculating the subsample weights, we make the simplifying assumption that HHOBS and NHOBS were not pursued during the subsample period unless the Adult Survey had not yet been completed. Although, this is different than the process described by the field, it is consistent with the data and resolves a number of discrepancies.

[13] In some instances, a household and neighborhood observation was completed for a sample child's household even though the Sample Adult was deceased.

Sample	Instru-ment	Weight Variable	"Complete" at end of Study	i) Complete at End of Main Field Period? Yes if "Complete" at end of Study AND	ii) Eligible for the Subsample? Yes if last digit of family ID was eligible for subsample AND
Adult	Neighborhood Observation	wt_310nobs	a)NHOBS_COMPLETED = 1, or b) HH_COMPLETED	FINAL DISP is not Deceased nor Cannot Locate (and inst3_sub ~= 1) AND either: a) INS3_MAIN = 1, b) SURVEY_MAIN = 1, or c) SUB_ELIG ~= 1.	a) INS3_MAIN = 0 and SURVEY_MAIN = 0, or b) FINAL DISP = Deceased or Cannot Locate
Child 5-7	Height &Weight (HW)	wt_310msr	a) height or weight measured, or b) HW attempted but unable to measure for a reason other than "Telephone Complete"	a) INS2_MAIN = 1, or b) SUB_ELIG ~= 1.	a) INS2_Main = 0, or b) FINAL DISP = Deceased
Child 8-11	Height &Weight (HW)	wt_310msr	a) height or weight measured, or b) HW attempted but unable to measure for a reason other than "Telephone Complete"	a) SURVEY_MAIN = 1 and FINAL_DISP is NOT telephone complete, or b) SUB_ELIG ~= 1.	a) SURVEY_MAIN = 0, b) FINAL DISP = Deceased, or c) INS3_Main = 0 and FINAL_DISP is telephone complete
Child 8-11	Child Survey	wt_310svy	FINAL_DISP is Telephone or In-Person Complete	a) SURVEY_MAIN = 1, or b) SUB_ELIG ~= 1.	a) SURVEY_MAIN = 0, or b) FINAL DISP = Deceased or Breakoff
Child 12-19	Youth Survey	wt_310svy	FINAL_DISP is Telephone or In-Person Complete	a) SURVEY_MAIN = 1, or b) SUB_ELIG ~= 1.	a) SURVEY_MAIN = 0, or b) FINAL DISP = Deceased or Breakoff
Child 5-11	WJ-R	wt_310wjr	any WJ-R subtest completed (WJR_COMPLETED = 1)	a) INS3_MAIN = 1, or b) SUB_ELIG ~= 1.	a) INS3_MAIN = 0, b) FINAL DISP = Deceased
Child 12-19	WJ-R	wt_310wjr	any WJ-R subtest completed (WJR_COMPLETED = 1)	a) INS2_MAIN = 1, or b) SUB_ELIG ~= 1.	a) INS2_MAIN = 0, b) FINAL DISP = Deceased
Child 5-19	POCY	wt_310pocy	(same as Adult Survey if sample child present)	(same as Adult Survey if sample child present)	(same as Adult Survey if sample child present)

B6.5 Combined weights

In all, we created ten component weights:

RA Ratio Weight (wt_Raratio) - a family-level weight to adjust for changes in the randomization ratio for families assigned by the end of 1997. This weight is:

= 0 for families randomized after 1997 and for families with no sample adult after resolution of individuals assigned to two core families, and

= w_{jkh} (see equation 16) for all other families randomized by December 31, 1997.

RA Ratio Weight Including Families Randomized in 1998 (wt_Raratio98) - a family-level weight to adjust for changes in the randomization ratio for all families (including those randomized in 1998). This weight is:

= 0 for families with no sample adult after resolution of individuals assigned to two core families, and

= w_{jkh} (see equation 16) for all other families.

Survey Sample Weight (wt_sampsvy) - a person-level weight equal to the inverse probability of selection for the survey or testing. This weight is:

= 0 for individuals who are neither sample adults nor sample children;

= 1 for sample adults; and

= the inverse probability of being selected for sample children.

Concept Formation Weight (wt_Cftest) - a person-level weight to adjust for selection of children for the Concept Formation test. This weight is:

= 0 for all adults and for all children not selected for the Concept Formation test; and

= for children selected for the Concept Formation test, the inverse probability of being selected for the Concept Formation test having already been selected for the child sample.

3-in-10 Survey Subsample Weight (wt_310svy) - a person-level survey weight to adjust for selection into the subsample. This weight is:

= 0 for a) all individuals who are neither sample adults nor sample children ages 8-19, and b) individuals without a completed survey by the end of the main field period and who were not eligible for the subsample;

= 1 for all sample adults and sample children with surveys completed during the main field period;

= 10/3 for sample adults and sample children ages 8-19 without a completed survey by the end of the main field period and who were eligible for the subsample.

3-in-10 POCY Subsample Weight (wt_310pocy) - a person level POCY weight to adjust for selection into the subsample. This weight is equal to:

= 0 for all adults, for children not selected for the sample, and for children who did not have a completed POCY by the end of the main field period and who were not eligible for the subsample;

= 1 for all sample children with a POCY completed during the main field period; and

= 10/3 for all sample children who did not have a POCY completed during the subsample period and whose parent was eligible for the subsample.

3-in-10 Measurement Subsample Weight (wt_310msr) - a person- level measurement weight to adjust for selection into the subsample. This weight is:

= 0 for a) all individuals who are neither sample adults nor sample children ages 5-11, and b) sample adults and sample children ages 5-11 without a completed measure (blood pressure or height and weight, respectively) at the end of the main field period and who were not eligible for the subsample;

= 1 for sample adults and sample children ages 5-11 with a completed measure (BP or height and weight, respectively) from the main field period;

= 10/3 for sample adults and sample children ages 5-11 without a completed measure (blood pressure or height and weight) at the end of the main field period and who were eligible for the subsample.

3-in-10 WJ-R Subsample Weight (wt_310wjr) - a person-component level weight to adjust for selection into the subsample. This weight is:

= 0 for a) all adults, b) all children not selected for the sample, and c) sample children who did not have any WJ-R subtests completed by the end of the regular field period and who were not eligible for the subsample;

= 1 for sample children with at least one WJ-R subtest completed during the main field period;

= 10/3 for sample children who did not have any WJ-R subtests completed by the end of the main field period and who were eligible for the subsample.

3-in-10 Household Observation Subsample Weight (wt_310hobs) - a person-level household observation weight to adjust for selection into the subsample. This weight is:

= 0 for a) all individuals who are not sample adults, and b) sample adults without completed Household Observation data by the end of the main field period and who were not eligible for the subsample;

= 1 for sample adults with Household Observation data completed during the main field period;

= 10/3 for sample adults without Household Observation data by the end of the main field period and who were eligible for the subsample.

3-in-10 Neighborhood Observation Subsample Weight (wt_310nobs) - a person-level neighborhood observation weight to adjust for selection into the subsample. This weight is:

= 0 for a) all individuals who are not sample adults, and b) sample adults without completed Neighborhood Observation data by the end of the main field period and who were not eligible for the subsample;

= 1 for sample adults with Neighborhood Observation data completed during the main field period;

= 10/3 for sample adults without Neighborhood Observation data by the end of the main field period and who were eligible for the subsample.

More than one weight is applicable to each survey item. The final weights used in the analyses of the survey data were the product of these multiple weights. Below are the nine total weights used to analyze the data.

<u>Total Core Weight</u> (wt_Totcore) – a person-level weight that is equal to the randomization ratio weight for all core members randomized through 1997 and is equal to zero for non-core members and members randomized after 1997 (i.e., wt_Raratio*core status through 1997).

<u>Total Core Weight</u> Through 1998 (wt_Totcore98) – a person-level weight that is equal to the randomization ratio weight through 1998 for all core members and equal to zero for non-core members (i.e., wt_Raratio98*core status through 1998).

<u>Total Survey Weight</u> (wt_Totsvy) – a person-level weight for self-reported surveys that is equal to the product of wt_Raratio * wt_Sampsvy * wt_310svy.

<u>Total POCY Weight</u> (wt_Totpocy) - a child-level weight for the parental report portion of the survey that is equal to the product of wt_Raratio * wt_Sampsvy * wt_310pocy.

<u>Total Measurement Weight</u> (wt_Totmsr) – a person-level weight for measurement data (Blood Pressure and Child Height and Weight for ages 5-11) that is equal to the product of wt_Raratio * wt_Sampsvy * wt_310msr.

<u>Total WJ-R Weight</u> (wt_Totwjr) – a child-level weight for WJ-R test data that is equal to the product of wt_Raratio * wt_Sampsvy * wt_310wjr.

<u>Total Household Observation Weight</u> (wt_Tothobs) – an adult-level weight for the adult's Household Observation data that is equal to the product of wt_Raratio * wt_Sampsvy * wt_310hobs.

<u>Total Neighborhood Observation Weight</u> (wt_Totnobs) – an adult-level weight for the adult's Neighborhood Observation data that is equal to the product of wt_Raratio * wt_sampsvy * wt_310nobs.

<u>Total Concept Formation Weight</u> (wt_Totcf) – a child-level weight for the Concept Formation tests that is equal to the product of wt_Raratio* wt_sampsvy * wt_310wjr * wt_cftest.

Exhibit B.7 summarizes which combined weight is appropriate for each level of analysis and data source.

EXHIBIT B.7
COMBINED WEIGHTS FOR DIFFERENT ANALYSIS LEVELS

Analysis Level	Data Source for Outcome or Mediator	Weight
Adult, Child, or All Core Members Randomized By the End of 1997	Administrative Data (TANF, Food Stamps, Earnings, Criminal Justice) and Published Data (Census data linked to an individual's address)	wt_Totcore
Adult, Child, or All Core Members Randomized Through 1998	Administrative Data	wt_Totcore98
Adult (or Current Household of Adult)	Adult Survey Sections A-G* and N (excluding blood pressure)	wt_Totsvy
Adult	Adult blood pressure (F13-F16),	wt_Totmsr
Adult	Household Observation (observation of the adult respondent and the parent-child interaction)	wt_Tothobs
Adult	Neighborhood Observation (observations about the interior and exterior of the home)	wt_Totnobs
Child	Child Survey (except height and weight), Youth Survey (including self-reported height and weight)	wt_Totsvy
Child	POCY (Sections J-M of the Adult Survey), School Characteristics	wt_Totpocy
Child	Child (ages 5-11) height and weight measures	wt_Totmsr
Child	WJ-R test scores (except Concept Formation subtest)	wt_Totwjr
Child	WJ-R Concept Formation Subtest	wt_Totcf

*This includes questions F7 through F9 which refer to one randomly selected child.

B7. Missing Data

We addressed two different types of missing data issues: unit nonresponse and item nonresponse. Below we describe the options we had for handling missing data and the options that were most considering the actual magnitude and nature of the problem.

B7.1 Unit nonresponse

We had no direct survey data on individuals who are deceased, could not be located, or refused to be interviewed. Observed and unobserved differences between these nonrespondents and the respondents could have potentially biased our estimates.

To address unit nonresponse, we used two approaches:

1) For outcomes measured with administrative data (e.g., earnings, welfare benefits), we estimated impacts on both the entire sample and on survey respondents only. The

difference in these two estimates provided a measure of the bias attributable to nonresponse.

2) By comparing the effects estimated using only the main sample with the effects estimated using the main sample plus the subsample, we explored the reduction in nonresponse bias achieved using the subsample.

The results of these analyses are described in appendix F.

B7.2 Item nonresponse

The second type of problem concerned item nonresponse due to a subject not knowing or refusing to answer a particular question. In most cases, where the outcome itself was missing, the observation had to be excluded from the analysis; individuals with unknown values of the outcome can contribute nothing to the impact estimate. Such individuals were treated the same way we treat unit nonrespondents. The only exceptions to this rule were cases where data were missing for a component of family income; such components were imputed using the procedure described below.

The treatment of missing covariates depended in part on their prevalence. If only a small percentage of the sample failed to respond on an item, it is unlikely that the treatment of these missing data would materially affect the impact estimates. Moreover, given the large number of outcomes analyzed, the use of sophisticated imputation techniques would have been extremely costly and time-consuming. Therefore, for covariates with less than 5 percent nonresponse, we replaced missing values with conditional means (e.g., missing child covariates were replaced with mean values conditional on a child's site, single-year age at baseline, and gender). Such an approach yields unbiased estimates if data are missing at random. An alternative, still relatively simple, approach is to include a dummy variable for "missing" along with each covariate. This approach yields unbiased impact estimates even if the data are not missing at random. Adopting this approach for all covariates, however, would have doubled the number of right-hand-side variables. Therefore, we used this strategy only for those covariates where more than 5 percent of the data were missing.

In computing total household income, missing values of income components were imputed as follows. Missing values of TANF benefits, business income, and "other income" were replaced with sample means, by site and treatment group. Missing values of earned income for a sample member were imputed by regressing the known values of earned income on age, site, TANF benefits, business income, "other" income, and the difference between the value of total household income reported by the respondent in question G5 and the sum of other household members' earnings.[14]

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[14] Details of the imputation procedures are available from the authors on request.

B8. Interpretation and Reporting of Results

B8.1 Reporting unadjusted versus regression-adjusted means and impacts

We presented only regression-adjusted means and impacts. While unadjusted means have some intuitive appeal as representing what "really" happened, they are clearly less reliable indicators of impact than the adjusted estimates. To the extent that two estimates differ, we wanted the reader to focus on the more reliable estimate. We reported only the control mean and the impact. These two numbers give the reader a sense of what difference the intervention made (the impact estimate) and how large that difference was relative to what would have happened in the absence of the intervention (the control mean). The treatment group mean adds no independent information.

B8.2 Reporting of results from multiple data sources

Many of the child survey outcomes could have been constructed using either the child's self-report or using the adult's report about the child. The Analysis Team agreed that it was only necessary to report one of the effects in the text (and footnote the other) if the reporting sources were in agreement regarding the treatment effect.

B8.3 Interpretation of multiple tests

In interpreting the results and assessing their joint significance, we considered the number of estimates produced with this unusually rich data set. In estimating effects for different subgroups, we also needed to keep in mind the overall impacts and impacts for all of the subgroups rather than just focusing on subgroups with positive findings. To minimize the risk of Type 1 errors in these analyses, we adopted a significance threshold of 5 percent, rather than the conventional 10 percent. Even with this safeguard, it was important to be alert to the danger of estimates that may have been statistically significant by chance alone. When estimating impacts on a large number of outcomes, for example, we considered whether the number of statistically significant impacts materially exceeded the 5 percent level that would be expected under the null hypothesis of no true effect. See appendix I for a detailed discussion of this issue.

Appendix C

Descriptive Tables and Maps

EXHIBIT C1.1
COMPARISON OF INTERIM EVALUATION SAMPLE WITH FULL MTO POPULATION ON DEMOGRAPHIC AND SOCIOECONOMIC CHARACTERISTICS

	Interim Evaluation Sample[1] (All Groups)	Full MTO Population[2] (All Groups)
Race/Ethnicity of Head of Household		
African-American non-Hispanic	62.6%	62.1%
Hispanic	30.4%	31.3%
White non-Hispanic	2.9%	2.6%
American Indian non-Hispanic	0.4%	0.4%
Asian/Pacific Islander non-Hispanic	1.8%	1.7%
Other non-Hispanic	2.1%	2.0%
Sex of Head of Household		
Male	8.4%	8.8%
Female	91.6%	91.2%
Head of Household's Marital Status		
Never married	62.2%	61.9%
Married	11.3%	11.5%
Widowed	9.5%	9.3%
Divorced	17.1%	17.3%
Median Number of Children	3	3
Average Total Household Income	$9,314	$9,310
Percent with AFDC as Primary Income Source	61.6%	59.1%
Head of Household Currently in School?		
Yes	16.1%	16.2%
No	83.9%	83.8%
Head of Household a Graduate?		
High School	40.6%	40.7%
GED	19.7%	18.6%
Neither	39.7%	40.7%
Head of Household Currently Working?		
Full-time	16.1%	16.5%
Part-time	11.5%	11.4%
Not working	72.2%	72.1%
Working for benefits	0.1%	0.1%

Source: MTO Participant Baseline Survey, initial HUD Form 50058.
Sample: Adults.
Notes: The respondent to the baseline survey was usually the same person as the sample adult for the interim evaluation. Household income was defined following the rules for Section 8 eligibility. Percentages may not add to 100 because of rounding. Data are weighted as described in appendix B.
[1] MTO population randomly assigned through December 1997. N=4248
[2] Full MTO program population (all cases ever randomly assigned). N=4608

Exhibit C1.2
Demographic and Socioeconomic Characteristics of MTO Families By Site Group

	Baltimore	Boston	Chicago	Los Angeles	New York	All Sites
Race/Ethnicity of Head of Household [a]						
African-American non-Hispanic	95.9%	32.8%	98.2%	50.0%	47.8%	62.6%
Hispanic	1.6%	45.2%	0.9%	45.3%	49.2%	30.4%
White non-Hispanic	0.2%	11.0%	0.1%	0.8%	0.8%	2.9%
American Indian non-Hispanic	0.7%	0.7%	0.1%	0.2%	0.1%	0.4%
Asian/Pacific Islander non-Hispanic	0.0%	5.1%	0.0%	3.0%	0.6%	1.8%
Other non-Hispanic	1.6%	5.2%	0.8%	0.7%	1.5%	2.1%
Sex of Head of Household						
Male	2.4%	8.5%	3.9%	20.8%	8.1%	8.4%
Female	97.7%	91.5%	96.1%	79.2%	91.9%	91.6%
Head of Household's Marital Status						
Never married	74.2%	57.9%	73.9%	56.0%	53.6%	62.2%
Married	3.5%	12.4%	6.5%	22.6%	11.4%	11.3%
Divorced	8.8%	11.9%	6.3%	6.0%	12.4%	9.5%
Widowed or separated	13.5%	17.7%	13.4%	15.3%	22.6%	17.1%
Median Number of Children	3	3	3	3	3	3
Average Total Household Income	$6,838	$10,701	$7,980	$9,949	$10,316	$9,314
Percent with AFDC as Primary Income Source	63.2%	50.9%	58.7%	74.1%	64.7%	61.6%
Head of Household Currently in School?						
Yes	16.2%	17.7%	14.6%	14.6%	16.9%	16.1%
No	83.8%	82.3%	85.4%	85.4%	83.1%	83.9%
Head of Household a Graduate?						
High School	42.8%	45.4%	43.7%	37.5%	34.4%	40.6%
GED	14.8%	22.7%	18.4%	7.4%	28.2%	19.7%
Neither	42.4%	31.9%	37.9%	55.1%	37.4%	39.7%
Head of Household Currently Working?						
Full-time	13.9%	20.8%	15.2%	16.5%	13.8%	16.1%
Part-time	9.9%	13.9%	10.7%	11.7%	11.0%	11.5%
Not working	76.3%	65.3%	74.1%	71.8%	74.8%	72.2%
Working for benefits	0.0%	0.0%	0.0%	0.0%	0.4%	0.1%

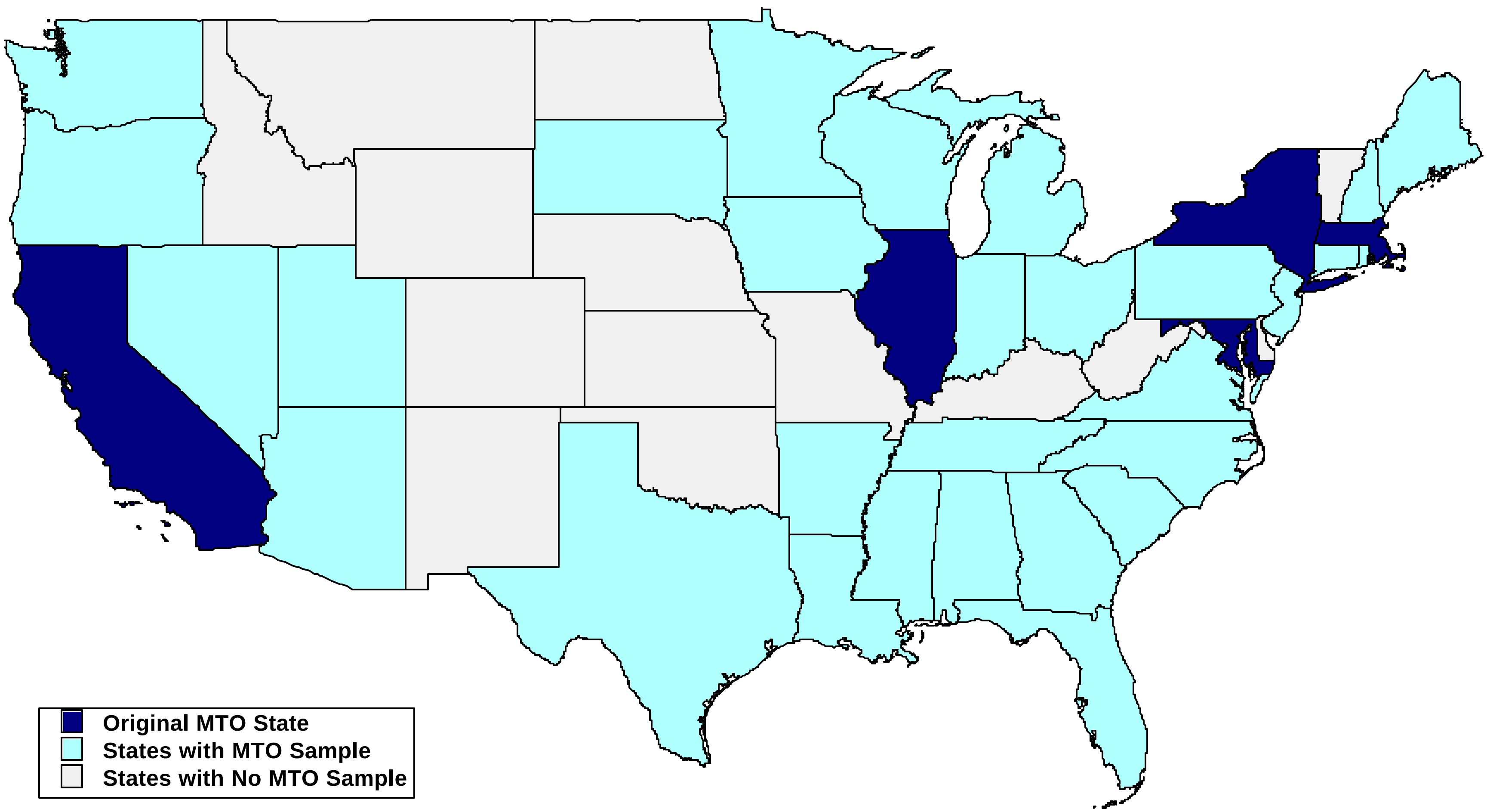
Source: MTO Participant Baseline Survey, initial HUD Form 50058.
Sample: Adults from families randomly assigned through December 1997.
Notes: The respondent to the baseline survey was usually the same person as the sample adult for the interim evaluation. Household income was defined following the rules for Section 8 eligibility. Percentages may not add to 100 because of rounding. Data are weighted as described in appendix B.
(a) Respondent self-reports. A number of African-American respondents skipped the ethnicity question and are not included in the distributions reported. Many Hispanics used the <u>Other</u> category for the race question.

EXHIBIT C1.3
BACKGROUND CONDITIONS AND EXPERIENCES OF MTO FAMILIES BY RANDOM ASSIGNMENT GROUP (PERCENT DISTRIBUTIONS—WEIGHTED DATA)

	Experimental Group	Section 8 Group	Control Group	All Groups
Moved More Than 3 Times in 5 Years?				
Yes	8.6	9.2	10.6	9.4
No	91.4	90.8	89.4	90.6
Ever Lived Outside the [City] Area?				
Yes	30.5	29.2	27.2	29.1%
No	69.5	70.8	72.8	70.9
Most Important Reason for Wanting to Move?				
Get away from drugs, gangs	54.7	50.7	52.5	52.9
Get a bigger/better apartment	22.6	23.3	23.7	23.1
Better schools for my children	16.0	19.5	16.8	17.3
Get a job	1.4	1.0	1.2	1.2
Be near my job	0.4	0.4	0.3	0.3
Be near my family	0.8	0.8	1.0	0.9
Have better transportation	0.2	0.3	0.1	0.2
Other reasons	3.9	4.0	4.4	4.1
Second Most Important Reason for Wanting to Move?				
Get away from drugs, gangs	28.1	30.2	30.3	29.4
Get a bigger/better apartment	26.1	25.5	25.5	25.7
Better schools for my children	32.2	33.0	30.9	32.0
Get a job	4.4	4.6	4.7	4.6
Be near my job	0.7	0.7	1.0	0.8
Be near my family	2.6	1.7	2.2	2.2
Have better transportation	1.1	0.3	1.3	0.9
Other reasons	4.9	4.1	4.1	4.4
Where Want to Move?				
Elsewhere in my neighborhood	6.5	6.6	6.8	6.6
Different neighborhood in city	56.3	59.8	58.8	58.1
Different neighborhood in suburbs	17.3	16.0	15.4	16.3
Different city outside the area	17.5	16.0	16.6	16.8
Other	2.5	1.6	2.5	2.2
Condition of Current House or Apartment?				
Excellent	5.4	4.2	5.3	5.0
Good	22.8	19.9	22.8	22.0
Fair	47.0	49.6	46.1	47.5
Poor	24.8	26.3	25.8	25.5
Satisfaction With Current Neighborhood?				
Very/somewhat satisfied	11.1	19.8	19.7	19.3
Ambivalent	18.6	12.3	12.1	11.8
Very/somewhat dissatisfied	70.3	67.9	68.2	69.0
Experienced in the Past 6 Months?				
Purse/wallet/jewelry snatched	25.3	24.6	23.5	24.6
Threatened with knife or gun	24.5	24.3	24.6	24.4
Beaten or assaulted	23.5	24.7	23.3	23.8
Stabbed or shot	10.9	10.5	11.7	11.0
Break-in (attempted or actual)	26.4	28.6	25.5	26.7

Source: MTO Participant Baseline Survey
Sample: Adults from families randomly assigned through December 1997.
Notes: The respondent to the baseline survey was usually the same person as the sample adult for the interim evaluation. Household income was defined following the rules for Section 8 eligibility. Percentages may not add to 100 because of rounding. Data are weighted as described in appendix B.

Exhibit C2.1
Location of MTO Interim Evaluation Sample in 2002



Source: Adult Survey.
Sample: Adults.
Note: Sample adults also live in Puerto Rico.

Abt Associates Inc.
05/08/2003

EXHIBIT C3.1
CURRENT NEIGHBORHOOD CONDITIONS AND QUALITY FOR THE INTERIM EVALUATION SAMPLE BY RANDOM ASSIGNMENT GROUP (PERCENT DISTRIBUTIONS)

	Experimental Group	Section 8 Group	Control Group	All Groups
Length of Time in Current Neighborhood (Self-Report)				
Less than 6 months	30.9	13.0	8.5	18.9
6–12 months	3.1	26.4	15.7	13.0
1–2 years	18.9	28.8	33.3	26.3
3–4 years	3.1	11.3	19.0	10.6
5–7 years	6.9	0.0	6.8	5.3
Over 7 years	37.1	20.6	16.7	26.1
Seen People Selling Drugs in Your Neighborhood During the Past 30 Days?				
No	67.4	65.5	54.8	63.0
Yes, a couple times a month	3.0	4.4	4.9	3.9
Yes, about once a week	3.9	4.6	5.7	4.7
Yes, almost every day	25.8	25.5	34.7	28.4
Experienced This in The Past Six Months?				
Purse/wallet/jewelry snatched	5.9	4.2	7.2	5.8
Threatened with knife or gun	4.8	4.8	7.4	5.6
Beaten or assaulted	6.5	6.8	9.5	7.5
Stabbed or shot	3.7	2.7	4.4	3.6
Break-in (attempted or actual)	7.5	6.5	7.8	7.3
Feel Safe in Your Neighborhood during the Day?				
Yes	84.6	84.5	74.8	81.6
No	15.4	15.5	25.2	18.4
Feel Safe in Your Neighborhood at Night?				
Yes	69.3	64.2	54.7	63.4
No	30.8	35.9	45.3	36.7
Percent With Specific Neighborhood Problem (Self-Report) [a]				
Litter or trash on the streets or sidewalks	53.1	56.6	63.4	57.2
Graffiti on the walls	34.7	38.5	47.9	39.8
People drinking in public	39.7	45.0	56.3	46.3
Abandoned buildings	27.2	27.5	33.8	29.3
Groups of people hanging out	47.6	51.3	64.5	53.8
Police not coming when called	20.9	24.6	33.7	25.9
Mean Index of Neighborhood Problems [b]	2.2	2.4	3.0	2.5
Current Neighborhood Satisfaction				
Very satisfied	32.4	30.0	19.5	27.8
Somewhat satisfied	29.7	29.1	28.1	29.0
Ambivalent	15.4	18.1	20.4	17.7
Somewhat dissatisfied	9.8	10.0	13.6	11.0
Very dissatisfied	12.7	12.8	18.5	14.5

Source:
Sample: Adults from families randomly assigned through December 1997..
Notes: Data are weighted as described in appendix B.
(a) Percent reporting big problem or small problem with item.
(b) Index is sum of items reported to be big or small problem by the adult respondent. Maximum value 6.

EXHIBIT C3.2
CURRENT HOUSING STATUS OF THE INTERIM EVALUATION SAMPLE
BY RANDOM ASSIGNMENT GROUP

	Experimental Group	Section 8 Group	Control Group	All Groups
Current Housing Tenure				
Rent	90.4%	91.5%	89.3%	90.4%
Own	4.7%	4.6%	5.3%	4.8%
Live with others and pay rent	0.7%	0.3%	0.9%	0.7%
Live with others and pay no rent	3.9%	2.9%	3.9%	3.6%
Other living situations[a]	0.3%	0.7%	0.7%	0.5%
Length of Time in Current Housing Unit				
Under 1 year	17.9%	16.1%	15.3%	16.6%
1–2 years	20.4%	20.0%	21.0%	20.5%
3–4 years	24.7%	25.3%	22.0%	24.1%
5–10 years	26.9%	27.8%	26.7%	27.0%
Over 10 years	10.2%	10.8%	15.0%	11.8%
Number of Moves Since Random Assignment (Self-report)				
None	28.6%	31.1%	34.0%	30.9%
1–3	66.9%	62.5%	61.1%	63.95
4 or more	4.6%	6.4%	4.9%	5.2%
Average Monthly Payment For:				
Rent	$286	$277	$298	$287
Mortgage	$1,491	$996	$898	$1,153
Utilities	$101	$105	$80	$96
Total Monthly Housing Cost [b]				
Mean	$440	$417	$409	$424
Median	$355	$339	$320	$339
Total Monthly Housing Cost Burden [c]				
Mean	0.33	0.32	0.31	0.32
Median	0.30	0.30	0.30	0.30
Those Reporting Encountering Bias or Discrimination In Housing Search	6.7%	9.5%	3.0%	6.4%
Those Reporting Being Homeless or Evicted In The Past Year	9.4%	9.8%	8.1%	9.1%
Those Reporting Recent Landlord Problems	4.2%	4.8%	3.6%	4.2%
Those Experiencing Utility Payment Problems				
Moderate [d]	25.6%	25.5%	20.2%	23.9%
Severe [e]	6.0%	4.3%	3.5%	4.7%
Those Experiencing Payment Problems				
Moderate[f]	12.3%	12.3%	12.2%	12.3%
Severe[g]	6.5%	6.1%	8.6%	7.0%

Source: Adult survey
Sample: Adults from families randomly assigned through December 1997.
Notes: Data are weighted as described in appendix B.
(a) Other living situations include homeless and living in a group shelter; homeless and living on the street; incarcerated; living in a group home, dorm, or barracks; and living in a hospital, nursing home, or special school.
(b) Includes all utility payments, rent (for renters), and payments to principal, interest, taxes, and insurance (for owners).
(c) Ratio of total monthly housing cost to total monthly household income.
(d) Respondent reports up to 3 of the following: 15 or more days late in paying utilities at least once in the past 12 months, charged a late fee, or received a shut-off notice.
(e) Respondent reports all 3 of problems in (d) as well as either having services shut off or moving out (even for a little while) because utilities were shut off.
(f) Respondent reports being 15 or more days late in paying rent or mortgage at least once in the past 12 months.
(g) Respondent reports problem in (f) as well as receiving an eviction or foreclosure threat due to nonpayment.

EXHIBIT C3.3
CURRENT RECEIPT OF HOUSING ASSISTANCE IN THE INTERIM EVALUATION SAMPLE BY RANDOM ASSIGNMENT GROUP[a]
(PERCENT DISTRIBUTIONS)

	Experimental Group	Section 8 Group	Control Group	All Groups
Living in a Household Receiving Housing Assistance (self-report) (n=3525)				
Yes	78.1	78.4	70.8	76.0
No	21.9	21.6	29.2	24.0
Living in a Household Receiving Housing Assistance (Administrative data) (n=4248)				
Yes	68.9	75.7	65.8	69.9
No	31.1	24.3	34.2	30.1
Type of Housing Assistance (self-report) (n=3525)				
Public housing	25.4	22.4	45.4	30.7
Tenant-based vouchers	47.8	51.9	20.6	40.7
Project-based vouchers	2.6	2.7	2.4	2.6
Assisted, type unknown	2.2	1.4	2.3	2.0
No Housing Assistance	21.9	21.6	29.2	24.0
Type of Housing Assistance (Administrative data) (n=4248)				
Public housing	24.9	21.1	43.5	29.6
Tenant-based vouchers	37.8	48.4	12.4	33.0
Project-based vouchers	6.1	6.2	9.9	7.3
No housing assistance	31.1	24.3	34.2	30.1
Main Reason Left Housing Assistance				
Wanted/bought house	19.6	22. 1	17.1	19.5
Problem with PHA	16.4	15.0	7.9	13.3
Income over limits	14.1	14.3	6.0	11.5
Other	4.7	11.2	13.5	9.4
Dangerous neighborhood	9.5	3.1	13.9	9.1
Wanted better neighborhood/unit	5.3	7.7	12.4	8.3
Evicted	6.4	4.1	10.4	7.0
Rent/utilities too high	7.2	4.4	3.7	5.3
Relocated	5.8	4.6	5.3	5.3
Landlord would not take Section 8	5.5	8.8	1.5	5.1
Building no longer inhabitable	2.9	1.5	5.7	3.4
Family reasons	2.5	3.1	2.8	2.8

Sources: Adult survey, HUD MTCS and TRACS
Sample: Adults from families randomly assigned through December 1997.
Notes: Data are weighted as described in appendix B.
(a) Receiving housing assistance includes living in public housing, living in private assisted housing, or having a tenant-based housing voucher.

EXHIBIT C3.4
CURRENT HOUSING CONDITION AND QUALITY OF THE INTERIM EVALUATION SAMPLE BY RANDOM ASSIGNMENT GROUP (PERCENT DISTRIBUTIONS)

	Experimental Group	Section 8 Group	Control Group	All Groups
Respondent's Rating of Current Housing Condition				
Excellent	20.4	19.4	14.7	18.3
Good	42.5	40.1	38.1	40.5
Fair	27.2	28.5	34.8	29.9
Poor	9.9	12.0	12.5	11.3
Number of Rooms in Current Housing Unit				
1–3 rooms	18.1	21.1	22.9	20.4
4 rooms	26.2	25.1	26.2	25.9
5 rooms	29.9	27.3	28.4	28.7
6 or more rooms	25.8	26.5	22.5	25.0
Type of Structure [a]				
Single-family detached	14.5	14.8	11.2	13.6
Single-family attached	23.8	21.4	24.0	23.2
1–3 Story multifamily	34.8	33.1	30.1	32.9
4–6 Story multifamily	9.0	12.0	8.5	9.7
7+ Story multifamily	17.4	18.5	25.7	20.2
Mobile home	0.1	0.1	0.2	0.1
Other	0.4	0.2	0.2	0.3
General Condition of Unit/Building [a]				
Well kept, good repair	38.0	34.8	31.9	35.2
Fair condition	48.4	50.7	52.4	50.2
Poor condition	11.2	12.5	10.5	11.4
Badly deteriorated	2.4	2.1	5.2	3.2
Observed Problems in Unit [a]				
Open cracks or holes in walls	13.1	13.0	13.6	13.2
Peeling paint/plaster	12.2	10.5	12.1	11.7
Moderate clutter	30.7	29.1	30.2	30.1
Severe clutter	10.4	14.1	12.7	12.1
Percent with Specific Housing Problem (Self-Report) [b]				
Walls with peeling paint or broken plaster	40.5	42.7	49.2	43.8
Plumbing that does not work	28.6	31.2	32.4	30.5
Rats or mice	30.3	35.9	35.2	33.4
Cockroaches	32.3	34.0	37.7	34.4
Broken locks or no locks on doors	12.9	15.0	16.6	14.6
Heating system that does not work	16.7	18.3	19.6	18.0
Mean Index of Housing Problems [c]	1.8	2.0	2.1	2.0

Sources: Adult Survey, Interviewer Household and Neighborhood Observations
Sample: Adults from families randomly assigned through December 1997.
Notes: Data are weighted as described in appendix B.
(a) Interviewer observation items. Other items are self-reports by adult sample member.
(b) Percent reporting big problem or small problem with item.
(c) Index is sum of items reported to be big or small problem by the adult respondent. Maximum value is 7.

Appendix D

Detailed Estimation Results – Outcomes by Domain

EXHIBIT D2.1
CONTEXT OF THE MTO TREATMENT

Context	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Characteristics of the Current Neighborhood (Census 2000)					
Percent persons in poverty (n=3675)	38.5	-7.8* (0.8)	-16.7* (1.7)	-6.2* (0.8)	-10.2* (1.4)
Percent households receiving public assistance income (n=3675)	17.4	-4.0* (0.4)	-8.6* (0.9)	-3.4* (0.4)	-5.5* (0.7)
Percent female-headed households with their own children (n=3674)	56.8	-6.7* (0.8)	-14.3* (1.6)	-5.2* (0.8)	-8.5* (1.3)
Percent high school dropouts (n=3675)	25.7	-3.2* (0.5)	-6.9* (1.0)	-2.5* (0.5)	-4.1* (0.8)
Unemployment rate (n=3674)	18.9	-3.4* (0.4)	-7.3* (0.8)	-3.1* (0.4)	-5.1* (0.6)
Labor Force Participation (n=3675)					
Males	54.7	4.0* (0.5)	8.5* (1.0)	2.9* (0.5)	4.7* (0.8)
Females	47.3	3.9* (0.4)	8.4* (0.8)	2.6* (0.4)	4.3* (0.6)
Percent families with no workers (n=3675)	24.1	-4.3* (0.4)	-9.1* (0.9)	-3.6* (0.4)	-5.8* (0.7)
Percent households with wage or salary income (n=3675)	66.6	3.8* (0.5)	8.2* (1.1)	3.5* (0.6)	5.7* (0.9)
Percent of persons with some college completed (n=3675)	15.8	2.0* (0.2)	4.3* (0.5)	1.7* (0.2)	2.7* (0.4)
Percent 16- to 19-year-olds in school (n=3669)	74.7	1.3* (0.5)	2.8* (1.0)	0.9 (0.5)	1.4 (0.8)
Percent owner-occupied housing (n=3675)	23.3	9.3* (0.9)	19.7* (1.9)	6.4* (1.0)	10.4* (1.6)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Census 2000
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

EXHIBIT D5.1
CHILD AND ADULT OUTCOMES

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Abbreviated Behavior Problems Index [PR] (n=2514)					
All children (ages 5 to 11)	0.287	0.008 (0.015)	0.017 (0.031)	0.011 (0.016)	0.017 (0.024)
Female	0.249	-0.026 (0.020)	-0.051 (0.039)	-0.017 (0.022)	-0.026 (0.033)
Male	0.327	0.042* (0.020)	0.095* (0.045)	0.041 (0.023)	0.061 (0.034)
Adult Arrests [ADMIN] (n=4211)					
Ever arrested for any crime	0.317	-0.005 (0.011)	-0.010 (0.024)	-0.011 (0.012)	-0.019 (0.020)
Ever arrested for property crime	0.141	(0.008) (0.007)	0.016 (0.015)	0.003 (0.007)	0.004 (0.012)
Ever arrested for violent crime	0.134	0.014 (0.007)	0.029 (0.015)	0.005 (0.008)	0.009 (0.013)
Ever arrested other crime	0.168	-0.015* (0.008)	-0.032* (0.016)	-0.010 (0.008)	-0.016 (0.013)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey for all five sites and individual criminal justice system arrest data from Baltimore, Boston, Chicago, Los Angeles, and New York.
Sample: Children ages 5 to 11 on May 31, 2001. All sample adults.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data and SR – self-report).

EXHIBIT D6.1
SCHOOLING OUTCOMES BY SUBGROUP

	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Mostly Bs or Higher [SR] (n=2752)					
Female	0.440	0.006	0.013	0.006	0.010
		(0.037)	(0.080)	(0.040)	(0.066)
Male	0.342	-0.093*	-0.213*	-0.106*	-0.194*
		(0.036)	(0.082)	(0.038)	(0.070)
Advanced Coursework [SR] (n=5505)					
Female	0.212	0.004	0.008	0.011	0.018
		(0.022)	(0.047)	(0.025)	(0.039)
Male	0.188	-0.009	-0.015	0.009	0.014
		(0.023)	(0.053)	(0.024)	(0.039)
Special Ed Recipient [PR] (n=4724)					
Female	0.185	0.012	0.024	-0.011	-0.018
		(0.022)	(0.045)	(0.023)	(0.037)
Male	0.304	0.038	0.086	0.038	0.062
		(0.026)	(0.058)	(0.029)	(0.046)
Ages 5 to 7	0.191	0.032	0.064	0.027	0.042
		(0.041)	(0.082)	(0.045	(0.070)
Ages 8 to 11	0.259	0.043	0.095	0.035	0.057
		(0.031)	(0.068)	(0.033)	(0.049)
Ages 12 to 19	0.253	0.015	0.033	-0.011	-0.018
		(0.023)	(0.051)	(0.027)	(0.045)
Educationally on Track [SR] (n=1549)					
Female	0.761	0.068	0.136	0.057	0.098
		(0.038)	(0.076)	(0.042)	(0.073)
Male	0.721	-0.008	-0.019	0.017	0.034
		(0.039)	(0.099)	(0.044)	(0.090)
Ever Repeated a Grade [PR] (n=5375)					
Female	0.199	0.019	0.039	-0.047	-0.076
		(0.023)	(0.048)	(0.024)	(0.038)
Male	0.245	0.033	0.077	0.006	0.011
		(0.023)	(0.048)	(0.024)	(0.038)
Ages 5 to 7	0.121	-0.022	-0.043	-0.029	-0.044
		(0.032)	(0.064)	(0.036)	(0.056)
Ages 8 to 11	0.185	0.072*	0.159*	0.033	0.050
		(0.030)	(0.065)	(0.030)	(0.046)
Ages 12 to 19	0.272	0.016	0.036	-0.053*	-0.092*
		(0.023)	(0.052)	(0.024)	(0.042)
Took SAT/ACT [SR] (n=1561)					
Female	0.386	-0.017	-0.037	-0.021	-0.035
		(0.048)	(0.102)	(0.050)	(0.083)
Male	0.297	-0.046	-0.114	0.068	0.136
		(0.042)	(0.106)	(0.049)	(0.098)

EXHIBIT D6.1 (CONT.)
SCHOOLING OUTCOMES BY SUBGROUP

	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Attends College [SR] (n=2818)					
Female	0.056	-0.005	-0.010	0.010	0.017
		(0.016)	(0.034)	(0.017)	(0.028)
Male	0.030	-0.002	-0.004	0.001	0.001
		(0.011)	(0.024)	(0.012)	(0.022)
Attends 4-Year College [SR] (n=2818)					
Female	0.028	-0.011	-0.024	-0.003	-0.005
		(0.010)	(0.022)	(0.011)	(0.019)
Male	0.007	0.001	0.002	0.002	0.003
		(0.005)	(0.012)	(0.006)	(0.011)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Youth survey, POCY
Sample: All children ages 5-19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT D6.2
EDUCATIONAL OUTCOMES BY MOTIVATION TO MOVE

	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Mostly B's or Higher (ages 12 to 19) [SR] (n=2752)					
Schools primary reason for wanting to move	0.405	-0.081* (0.038)	-0.166* (0.077)	-0.072 (0.039)	-0.127 (0.069)
Schools not a primary reason for wanting to move	0.374	-0.004 (0.036)	-0.009 (0.086)	-0.025 (0.039)	-0.042 (0.066)
Advanced Coursework (ages 5 to 19) [SR] (n=5450)					
Schools primary reason for wanting to move	0.205	-0.008 (0.024)	-0.016 (0.049)	-0.002 (0.025)	-0.003 (0.040)
Schools not a primary reason for wanting to move	0.195	0.005 (0.022)	0.012 (0.052)	0.022 (0.024)	0.037 (0.040)
Special Ed Recipient (ages 5 to 17) [PR] (n=4724)					
Schools primary reason for wanting to move	0.250	0.021 (0.024)	0.043 (0.049)	0.007 (0.026)	0.012 (0.041)
Schools not a primary reason for wanting to move	0.240	0.028 (0.023)	0.065 (0.052)	0.020 (0.025)	0.033 (0.041)
Educationally on Track (ages 15 to 19) [SR] (n=1549)					
Schools primary reason for wanting to move	0.712	0.045 (0.042)	0.097 (0.090)	0.054 (0.046)	0.109 (0.092)
Schools not a primary reason for wanting to move	0.774	0.012 (0.037)	0.027 (0.087)	0.016 (0.040)	0.028 (0.069)
Ever Repeated a Grade (ages 5 to 19) [PR] (n=5347)					
Schools primary reason for wanting to move	0.207	0.030 (0.023)	0.062 (0.048)	0.003 (0.025)	0.005 (0.039)
Schools not a primary reason for wanting to move	0.237	0.023 (0.022)	0.023 (0.051)	-0.046 (0.024)	-0.078 (0.040)
Took SAT/ACT (ages 15 to 19) [SR] (n=1561)					
Schools primary reason for wanting to move	0.353	-0.031 (0.050)	-0.065 (0.099)	0.051 (0.046)	0.098 (0.096)
Schools not a primary reason for wanting to move	0.332	-0.034 (0.044)	-0.084 (0.109)	-0.011 (0.047)	-0.018 (0.082)

EXHIBIT D6.2 (CONT.)
EDUCATIONAL OUTCOMES BY MOTIVATION TO MOVE

	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Attends College (ages 12 to 19) [SR] (n=2818)					
Schools primary reason for wanting to move	0.045	-0.018 (0.013)	-0.037 (0.027)	-0.003 (0.014)	-0.006 (0.025)
Schools not a primary reason for wanting to move	0.042	0.012 (0.015)	0.028 (0.035)	0.014 (0.016)	0.024 (0.027)
Attends 4-Year College (ages 12 to 19) [SR] (n=2818)					
Schools primary reason for wanting to move	0.019	-0.010 0.009	-0.021 0.019	-0.001 0.010	-0.002 0.019
Schools not a primary reason for wanting to move	0.016	0.000 0.007	0.000 0.017	-0.001 0.008	-0.001 0.014
WJ-R Broad Reading (ages 5 to 19) [ADMIN] (n=5164)					
Schools primary reason for wanting to move	498.78	1.026 1.261	2.122 2.609	-0.517 1.530	-0.831 2.460
Schools not a primary reason for wanting to move	495.83	0.846 1.356	1.997 3.203	1.565 1.450	2.626 2.433
WJ-R Broad Reading (ages 5 to 19) [ADMIN] (n=5182)					
Schools primary reason for wanting to move	502.947	0.304 1.109	0.628 2.290	-2.086 1.254	-3.336 2.005
Schools not a primary reason for wanting to move	499.386	0.223 1.069	0.533 2.551	0.123 1.129	0.205 1.882

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child Survey data, POCY, Woodcock Johnson-Revised tests.
Sample: All children ages 5-19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT D6.3
SCHOOL CHARACTERISTICS BY MOTIVATION TO MOVE

	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Percent Free Lunch [SR] (n=3557)					
Schools primary reason for wanting to move	0.653	-0.073* (0.015)	-0.134* (0.026)	-0.027* (0.016)	-0.046* (0.027)
Schools not a primary reason for wanting to move	0.658	-0.060* (0.014)	-0.126* (0.029)	-0.249* (0.015)	-0.042* (0.025)
Percent White [SR] (n=4868)					
Schools primary reason for wanting to move	0.010	0.043* (0.014)	0.089* (0.029)	0.041 (0.023)	0.064 (0.036)
Schools not a primary reason for wanting to move	0.114	0.040* (0.012)	0.092* (0.029)	0.017 (0.015)	0.028 (0.025)
Pupil-Teacher Ratio [PR] (n=4870)					
Schools primary reason for wanting to move	16.638	0.496 (0.303)	1.016 (0.697)	-0.091 (0.341)	-0.142 (0.476)
Schools not a primary reason for wanting to move	13.354	-0.116 (0.297)	-0.268 (0.688)	-0.237 (0.317)	-0.396 (0.529)
Percent Limited English Proficient [SR] (n= 4014)					
Schools primary reason for wanting to move	0.140	-0.024* (0.009)	-0.048* (0.018)	-0.008 (0.009)	-0.013 (0.015)
Schools not a primary reason for wanting to move	0.195	-0.030* (0.009)	-0.068* (0.020)	-0.002 (0.010)	-0.004 (0.018)
Magnet School [PR] (n=3941)					
Schools primary reason for wanting to move	0.280	-0.032 (0.029)	-0.067 0.061	-0.002 0.035	-0.003 0.051
Schools not a primary reason for wanting to move	0.217	-0.070* (0.023)	-0.165* 0.054	-0.058* 0.028	-0.092* 0.044
Percentile Rank on State Exam [ADMIN] (N =3929)					
Schools primary reason for wanting to move	0.163	0.044* (0.011)	0.094* (0.024)	0.023 (0.012)	0.033 (0.017)
Schools not a primary reason for wanting to move	0.170	0.032* (0.011)	0.077* (0.027)	0.010 (0.012)	0.017 (0.021)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: U.S. Department of Education, Common Core of Data 1993 to 2001, National School-Level State Assessment Score Database, 2000 to 2001.
Sample: All children ages 5-19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT D6.4
EDUCATIONAL OUTCOMES BY AGE AT RANDOM ASSIGNMENT

	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Advanced Coursework [SR] (n=5450)					
Less than 5 years old at RA	0.129	-0.013 (0.025)	-0.028 (0.052)	-0.007 (0.027)	-0.010 (0.039)
6-11 years old at RA	0.181	0.023 (0.022)	0.049 (0.046)	0.016 (0.023)	0.026 (0.039)
12 or older at RA	0.370	-0.051 (0.049)	-0.132 (0.126)	0.019 (0.050)	0.037 (0.096)
Special Ed Recipient [PR] (n=4724)					
Less than 5 years old at RA	0.227	0.045 (0.026)	0.094 (0.055)	0.031 (0.029)	0.046 (0.043)
6-11 years old at RA	0.257	0.019 (0.024)	0.041 (0.053)	0.012 (0.027)	0.020 (0.046)
12 or older at RA	0.275	-0.081 (0.066)	-0.227 (0.183)	-0.096 (0.065)	-0.167 (0.114)
Ever repeated a grade [PR] (n=5347)					
Less than 5 years old at RA	0.170	0.021 (0.024)	0.043 (0.049)	-0.016 (0.026)	-0.025 (0.039)
6-11 years old at RA	0.266	0.012 (0.026)	0.025 (0.057)	-0.038 (0.028)	-0.064 (0.048)
12 or older at RA	0.219	0.084* (0.040)	0.213* (0.103)	0.025 (0.041)	0.049 (0.080)
WJ-R Broad Reading [ADMIN] (n=5164)					
Less than 5 years old at RA	478.804	0.902 (1.476)	1.926 (3.151)	0.650 (1.631)	0.950 (2.384)
6-11 years old at RA	507.828	1.118 (1.231)	2.406 (2.650)	0.891 (1.484)	1.528 (2.544)
12 or older at RA	514.463	0.199 (2.651)	0.522 (6.940)	-1.574 (2.523)	-3.115 (4.992)
WJ-R Broad Reading [ADMIN] (n=5182)					
Less than 5 years old at RA	483.846	-0.323 (1.127)	-0.692 (2.413)	-2.170 (1.222)	-3.152 (1.774)
6-11 years old at RA	511.583	0.869 (1.125)	1.884 (2.437)	0.164 (1.254)	0.280 (2.136)
12 or older at RA	516.667	-0.183 (2.199)	-0.479 (5.745)	-1.869 (2.111)	-3.740 (4.223)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child Survey data, POCY, Woodcock Johnson-Revised tests.
Sample: All children ages 5-19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).
e) Results are shown only for those outcomes available for all children age 5-19 at the time of the interim evaluation.

EXHIBIT D6.5
WOODCOCK-JOHNSON SCORES BY SITE

	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
WJ-R Broad Reading [ADMIN] (n=5169)					
New York	501.665	-2.525	-5.357	-6.345*	-15.248*
		(2.092)	(4.437)	(2.030)	(4.878)
Baltimore	494.661	5.421*	10.899*	3.821	5.017
		(2.254)	(4.532)	(2.290)	(3.006)
Boston	499.724	-0.703	-1.797	1.372	2.648
		(1.896)	(4.850)	(2.193)	(4.233)
Chicago	492.288	4.572*	14.395*	3.961	5.854
		(2.108)	(6.635)	(2.668)	(3.943)
Los Angeles	497.746	-1.347	- 2.068	1.055	1.395
		(1.884)	(2.892)	(1.990)	(2.629)
WJ-R Broad Reading [ADMIN] (n=5187)					
New York	503.105	-1.217	-2.577	-4.072*	-9.758*
		(1.846)	(3.910)	(1.828)	(4.380)
Baltimore	499.729	2.316	4.689	0.139	0.182
		(1.521)	(3.080)	(1.672)	(2.178)
Boston	499.437	0.238	0.603	1.804	3.481
		(1.503)	(3.814)	(1.668)	(3.218)
Chicago	501.417	0.878	2.786	-1.231	-1.799
		(1.951)	(6.192)	(2.130)	(3.113)
Los Angeles	501.686	-0.616	-0.953	-1.127	-1.487
		(1.389)	(2.148)	(1.614)	(2.130)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child Survey data, POCY, Woodcock Johnson-Revised tests.
Sample: All children ages 5-19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT D7.1a
IMPACTS ON EMPLOYMENT STATUS, ADULTS & YOUTH

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Employment Status [SR]					
Employed (n=3517)	0.522	0.014	0.030	0.026	0.044
		(0.021)	(0.044)	(0.023)	(0.039)
Unemployed (n=3508)	0.096	0.023	0.049	0.015	0.025
		(0.013)	(0.028)	(0.015)	(0.025)
Not in labor force (n=3508)	0.381	-0.038	-0.080	-0.041	-0.069
		(0.020)	(0.042)	(0.022)	(0.037)
Youth Employed [SR] (n=1581)					
All youth (Ages 15-19)	0.275	-0.028	-0.065	-0.021	-0.038
		(0.030)	(0.070)	(0.033)	(0.060)
Girls	0.320	-0.026	-0.057	-0.060	-0.101
		(0.046)	(0.098)	(0.047)	(0.078)
Boys	0.229	-0.029	-0.073	0.021	0.042
		(0.039)	(0.098)	(0.044)	(0.090)
Youth Employed [SR] (Ages 17-19 Only) (n=912)					
All youth (Ages 17-19)	0.349	-0.025	-0.061	-0.030	-0.054
		(0.043)	(0.103)	(0.047)	(0.087)
Girls	0.387	-0.018	-0.043	-0.085	-0.143
		(0.063)	(0.150)	(0.067)	(0.112)
Boys	0.306	-0.033	-0.079	0.027	0.055
		(0.058)	(0.141)	(0.065)	(0.131)
Youth Unemployed [SR] (Ages 17-19 Only) (n=901)					
All youth (Ages 17-19)	0.267	-0.017	-0.042	-0.001	-0.002
		(0.040)	(0.096)	(0.044)	(0.081)
Girls	0.216	-0.041	-0.099	0.046	0.077
		(0.050)	(0.120)	(0.058)	(0.097)
Boys	0.324	0.008	0.021	-0.050	-0.101
		(0.061)	(0.148)	(0.066)	(0.134)
Youth Not in Labor Force [SR] (Ages 17-19 Only) (n=901)					
All youth (Ages 17-19)	0.383	0.040	0.098	0.027	0.049
		(0.045)	(0.109)	(0.050)	(0.091)
Girls	0.398	0.055	0.131	0.039	0.065
		(0.066)	(0.159)	(0.074)	(0.124)
Boys	0.367	0.024	0.060	0.013	0.027
		(0.060)	(0.147)	(0.069)	(0.138)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult and Youth surveys
Sample: Adults and children ages 15 to 19 as of May 31, 2001
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT D7.1b
IMPACTS ON EMPLOYMENT STATUS, YOUTH AGES15-19

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Youth Employment Only: Not Enrolled in School [SR] (n=1587)					
All youth (Ages 15-19)	0.133	-0.013 (0.023)	-0.030 (0.053)	-0.038 (0.024)	-0.069 (0.043)
Girls	0.139	0.004 (0.034)	0.009 (0.074)	-0.064* (0.032)	-0.107* (0.053)
Boys	0.127	-0.030 (0.031)	-0.074 (0.077)	-0.011 (0.033)	-0.021 (0.067)
Youth School Enrollment Only: Not Employed [SR] (n=1584)					
All youth (Ages 15-19)	0.446	0.062* (0.031)	0.143* (0.073)	0.017 (0.033)	0.032 (0.060)
Girls	0.410	0.102* (0.044)	0.220* (0.095)	0.049 (0.046)	0.083 (0.077)
Boys	0.483	0.021 (0.043)	0.052 (0.107)	-0.016 (0.047)	-0.032 (0.096)
Youth Concurrent Employment and School Enrollment [SR] (n=1584)					
All youth (Ages 15-19)	0.141	-0.015 (0.024)	-0.034 (0.055)	0.018 (0.026)	0.032 (0.047)
Girls	0.180	-0.029 (0.037)	-0.063 (0.079)	0.005 (0.040)	0.008 (0.067)
Boys	0.101	-0.000 (0.029)	-0.000 (0.074)	0.031 (0.031)	0.063 (0.064)
Youth Full-Time School Enrollment [SR] (n=1590)					
All youth (Ages 15-19)	0.558	0.017 (0.030)	0.039 (0.070)	-0.004 (0.031)	-0.008 (0.057)
Girls	0.563	0.045 (0.043)	0.098 (0.093)	-0.012 (0.043)	-0.021 (0.072)
Boys	0.552	-0.012 (0.041)	-0.029 (0.103)	0.004 (0.043)	0.009 (0.088)
Youth School Enrollment [SR] (n=1590)					
All youth (Ages 15-19)	0.589	0.047 (0.030)	0.109 (0.071)	0.034 (0.031)	0.062 (0.058)
Girls	0.593	0.072 (0.043)	0.155 (0.093)	0.052 (0.044)	0.086 (0.073)
Boys	0.585	0.022 (0.042)	0.056 (0.104)	0.015 (0.043)	0.030 (0.088)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Youth survey
Sample: Children ages 15 to 19 as of May 31, 2001
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT D7.2

IMPACTS ON BENEFITS & JOB TENURE , ADULTS & YOUTH AGES15-19

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Employment Benefits [SR]					
Employed at job offering health insurance (n=3483)	0.296	0.023 (0.019)	0.050 (0.041)	0.004 (0.021)	0.007 (0.035)
Employed at job offering paid sick leave (n=3480)	0.300	0.018 (0.019)	0.038 (0.041)	0.007 (0.021)	0.013 (0.035)
Employed at job offering paid vacation (n=3483)	0.346	0.025 (0.020)	0.052 (0.043)	0.007 (0.022)	0.011 (0.037)
Adult Job Tenure [SR] (n=3475)					
Job tenure of more than one year	0.362	0.029 (0.021)	0.062 (0.045)	0.030 (0.022)	0.049 (0.037)
Youth Has Job Tenure of More Than One Year [SR] (n=1572)					
All youth (Ages 15-19)	0.073	0.006 (0.020)	0.015 (0.046)	0.005 (0.021)	0.009 (0.038)
Girls	0.081	0.003 (0.030)	0.006 (0.064)	-0.025 (0.028)	-0.041 (0.048)
Boys	0.064	0.010 (0.025)	0.026 (0.064)	0.036 (0.030)	0.073 (0.060)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult and Youth surveys
Sample: Adults and children ages 15 to 19 as of May 31, 2001
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT D7.3
IMPACTS ON OCCUPATION, ADULTS

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Employed in a White-Collar Occupation [SR] (n=3456)					
Employed in a white-collar occupation (managerial, professional, technical, sales or administrative support)	0.216	0.033 (0.018)	0.071 (0.038)	0.030 (0.019)	0.050 (0.032)
Employed in a managerial or professional specialty occupation	0.043	0.012 (0.009)	0.026 (0.020)	0.014 (0.010)	0.023 (0.017)
Employed in a technical, sales, or administrative support occupation	0.173	0.021 (0.017)	0.045 (0.036)	0.016 (0.018)	0.027 (0.030)
Adult Employed in a Service Occupation [SR] (n=3456)					
Employed in a service occupation	0.238	-0.006 (0.019)	-0.013 (0.040)	-0.012 (0.020)	-0.020 (0.034)
Adult Employed in a Blue-Collar Occupation [SR] (n=3456)					
Employed in blue-collar occupation	0.059	-0.014 (0.009)	-0.031 (0.020)	0.001 (0.012)	0.001 (0.020)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).
e) Respondent descriptions of their occupations and duties were assigned 3-digit occupation codes, consistent with the Current Population Survey, by the Census Bureau's National Processing Center.

EXHIBIT D8.1
ADDITIONAL SURVEY OUTCOMES

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Earnings, Income, Poverty [SR]					
Total earnings of head (n=3365)	$8,969	$136 $443	$289 $941	$108 $491	$182 $826
Calculated total household income (n=3261)	$15,165	$729 $636	$1,546 $1,348	-$389 $692	-$651 $1,156
Household is self-sufficient (n=3499)	0.182	0.001 0.016	0.002 0.035	0.019 0.018	0.032 0.030
Income of head + spouse > poverty line (n=3526)	0.301	0.002 0.020	0.005 0.042	-0.019 0.021	-0.032 0.035
Household income to poverty ratio (n=3211)	0.941	0.029 0.032	0.061 0.068	0.003 0.036	0.004 0.060
Supplemental Security Income [SR]					
Adult is currently receiving SSI (n=3511)	0.134	0.003 0.013	0.006 0.028	-0.010 0.016	-0.016 0.027
Children in household are currently receiving SSI (n=3511)	0.129	0.009 0.014	0.020 0.030	0.024 0.016	0.040 0.027
Adult or children currently get SSI and began after random assignment (n=3416)	0.110	0.013 0.014	0.027 0.030	0.005 0.016	0.008 0.026
Earned Income Tax Credit [SR]					
Did adult receive a tax refund/EITC return in 2001? (n=3482)	0.431	0.012 0.021	0.026 0.045	0.022 0.024	0.037 0.040
Amount of tax refund/EITC return in 2001 (n=3218)	$951	$56 $68	$118 $144	$114 $73	$192 $122
Was adult's tax refund/EITC return > $2,500 in 2001? (n=3218)	0.175	0.013 0.018	0.026 0.037	0.032 0.019	0.054 0.032
Aid to Families With Dependent Children (AFDC)/Temporary Assistance for Needy Families (TANF) [SR]					
Adult or children received AFDC/TANF in past 2 years (n=3512)	0.382	0.002 0.020	0.004 0.042	-0.033 0.022	-0.055 0.037
Adult or children have received AFDC/TANF continuously over past 2 years (n=3513)	0.208	-0.009 0.017	-0.020 0.036	-0.017 0.019	-0.028 0.031

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey. See appendix A for details.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

EXHIBIT D8.2
IMPACTS ON DISTRIBUTION OF THE TWO HOUSEHOLD INCOME MEASURES
COMPARISON OF MEAN AND QUANTILE REGRESSION IMPACTS

Model	MTO Impact	S8 Impact	Control Group Distribution	
Calculated Total Household Income [SR] (n=3261)				
OLS Regression	729	-389	Mean:	15,165
	636	691		
25th percentile regression	375	56	25th percentile:	6,200
	423	466		
Median regression	104	-459	Median:	12,012
	532	582		
75th percentile regression	376	-1,434	75th percentile:	21,000
	720	781		
Self-Reported Total Household Income [SR] (n=3211)				
OLS regression	239	-162	Mean:	15,536
	571	636		
25th percentile regression	-60	-214	25th percentile:	7,000
	285	312		
Median regression	-201	-766	Median:	12,000
	424	464		
75th percentile regression	-345	-1,077	75th percentile:	20,000
	724	793		

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey. See appendix A for details.
Sample: Sample adults
Notes: Control means and impact estimates are regression-adjusted, with robust standard errors in the OLS regressions, as described in appendix B.

EXHIBIT D8.3
DETAILED YEARLY IMPACTS: AID TO FAMILIES WITH DEPENDENT CHILDREN (AFDC)/ TEMPORARY ASSISTANCE FOR NEEDY FAMILIES (TANF)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
AFDC/TANF for Sample Adult					
Fraction of months sample adult received AFDC/TANF, Year 5 (n=1962)	.216	0.009 (0.019)	0.023 (0.050)	0.048* (0.024)	0.084* (0.042)
Fraction of months sample adult received AFDC/TANF, Year 4 (n=2934)	.275	0.029 (0.017)	0.070 (0.041)	0.039* (0.019)	0.072* (0.035)
Fraction of months sample adult received AFDC/TANF, Year 3 (n=2934)	.350	0.041* (0.018)	0.099* (0.043)	0.049* (0.020)	0.090* (0.037)
Fraction of months sample adult received AFDC/TANF, Year 2 (n=2632)	.422	0.044* (0.019)	0.105* (0.045)	0.036 (0.020)	0.064 (0.035)
Fraction of months sample adult received AFDC/TANF, Year 1 (n=2448)	.516	0.033 (0.018)	0.078 (0.043)	0.016 (0.020)	0.028 (0.035)
Total AFDC/TANF benefit received by sample adult, Year 5 (n=1962)	$1,037	$2 ($107)	$6 ($280)	$37 ($121)	$64 ($210)
Total AFDC/TANF benefit received by sample adult, Year 4 (n=2934)	$1,372	$62 ($99)	$149 ($238)	$1 ($105)	$3 ($193)
Total AFDC/TANF benefit received by sample adult, Year 3 (n=2934)	$1,772	$122 ($103)	$294 ($247)	$73 ($114)	$134 ($209)
Total AFDC/TANF benefit received by sample adult, Year 2 (n=2632)	$2,104	$280* ($110)	$670* ($264)	106 (119)	$188 ($211)
Total AFDC/TANF benefit received by sample adult, Year 1 (n=2448)	$2,588	$238* ($108)	$564* ($255)	$177 ($117)	$308 ($204)

EXHIBIT D8.3 (CONT.)
DETAILED YEARLY IMPACTS: AID TO FAMILIES WITH DEPENDENT CHILDREN (AFDC)/ TEMPORARY ASSISTANCE FOR NEEDY FAMILIES (TANF)

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
AFDC/TANF for Household					
Fraction of months household received AFDC/TANF, Year 5 (n=1962)	0.273	0.011 (0.021)	0.029 (0.055)	0.020 (0.025)	0.035 (0.044)
Fraction of months household received AFDC/TANF, Year 4 (n=2934)	0.320	0.031 (0.018)	0.075 (0.043)	0.021 (0.020)	0.039 (0.037)
Fraction of months household received AFDC/TANF, Year 3 (n=2934)	0.394	0.045* (0.018)	0.108* (0.043)	0.031 (0.020)	0.057 (0.037)
Fraction of months household received AFDC/TANF, Year 2 (n=2632)	0.469	0.047* (0.018)	0.112* (0.043)	0.018 (0.020)	0.032 (0.035)
Fraction of months household received AFDC/TANF, Year 1 (n=2448)	0.566	0.033* (0.016)	0.078* (0.038)	0.002 (0.019)	0.003 (0.033)
Total AFDC/TANF benefit received by household, Year 5 (n=1962)	$1,230	$22 ($110)	$57 ($288)	-$73 ($123)	-$127 ($215)
Total AFDC/TANF benefit received by household, Year 4 (n=2934)	$1,538	$65 ($101)	$156 ($244)	-$88 ($107)	-$162 ($197)
Total AFDC/TANF benefit received by household, Year 3 (n=2934)	$1,934	$124 ($104)	$299 ($251)	$3 ($115)	$5 ($211)
Total AFDC/TANF benefit received by household, Year 2 (n=2632)	$2,286	$273* ($111)	$652* ($266)	$49 ($120)	$87 ($212)
Total AFDC/TANF benefit received by household, Year 1 (n=2448)	$2,817	$190 ($109)	$450 ($258)	$76 ($119)	$132 ($206)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey. See appendix A for details.
Sample: Sample adults. TANF data is from three sites: New York, Chicago, and Boston.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

EXHIBIT D8.4a
DETAILED YEARLY IMPACTS: FOOD STAMPS

Outcome	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Food Stamps for Sample Adult					
Fraction of months sample adult received food stamps, Year 5 (n=1423)	0.390	0.034 (0.025)	0.090 (0.066)	0.052 (0.029)	0.082 (0.045)
Fraction of months sample adult received food stamps, Year 4 (n=1853)	0.435	0.018 (0.021)	0.047 (0.054)	0.043 (0.024)	0.073 (0.041)
Fraction of months sample adult received food stamps, Year 3 (n=1853)	0.467	0.046* (0.021)	0.119* (0.054)	0.064* (0.024)	0.109* (0.041)
Fraction of months sample adult received food stamps, Year 2 (n=1853)	0.527	0.049* (0.020)	0.127* (0.052)	0.051* (0.023)	0.087* (0.039)
Fraction of months sample adult received food stamps, Year 1 (n=1853)	0.627	0.024 (0.017)	0.062 (0.044)	0.010 (0.020)	0.017 (0.034)
Total food stamps benefit received by sample adult, Year 5 (n=1423)	$1,056	$95 ($81)	$253 ($214)	$213* ($101)	$334* ($158)
Total food stamps benefit received by sample adult, Year 4 (n=1853)	$1,202	$51 ($75)	$131 ($195)	$104 ($84)	$176 ($143)
Total food stamps benefit received by sample adult, Year 3 (n=1853)	$1,289	$155* ($74)	$401* ($192)	$167* ($85)	$284* ($145)
Total food stamps benefit received by sample adult, Year 2 (n=1853)	$1,394	$193* ($66)	$500* ($170)	$154* ($77)	$262* ($131)
Total food stamps benefit received by sample adult, Year 1 (n=1853)	$1,650	$104 ($60)	$268 ($156)	$39 ($70)	$67 ($119)

EXHIBIT D8.4b
DETAILED YEARLY IMPACTS: AFDC/TANF

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
AFDC/TANF for Household					
Fraction of months household received food stamps, Year 5 (n=1423)	0.429	0.035 (0.025)	0.093 (0.066)	0.043 (0.028)	0.067 (0.044)
Fraction of months household received food stamps, Year 4 (n=1853)	0.465	0.018 (0.021)	0.047 (0.054)	0.036 (0.024)	0.061 (0.041)
Fraction of months household received food stamps, Year 3 (n=1853)	0.498	0.043* (0.021)	0.111* (0.054)	0.054* (0.024)	0.092* (0.041)
Fraction of months household received food stamps, Year 2 (n=1853)	0.561	0.039 (0.020)	0.101 (0.052)	0.039 (0.022)	0.066 (0.037)
Fraction of months household received food stamps, Year 1 (n=1853)	0.660	0.013 (0.016)	0.034 (0.041)	-0.002 (0.019)	-0.003 (0.032)
Total food stamps benefit received by household, Year 5 (n=1423)	$1,185	$89 ($82)	$238 ($219)	$201 ($103)	$315 ($161)
Total food stamps benefit received by household, Year 4 (n=1853)	$1,283	$59 ($76)	$152 ($196)	$107 ($85)	$182 ($144)
Total food stamps benefit received by household, Year 3 (n=1853)	$1,372	$158* ($74)	$408* ($191)	$150 ($84)	$255 ($142)
Total food stamps benefit received by household, Year 2 (n=1853)	$1,483	$172* ($65)	$445* ($167)	$137 ($75)	$233 ($127)
Total food stamps benefit received by household, Year 1 (n=1853)	$1,736	$77 ($58)	$200 ($151)	$19 ($67)	$32 ($113)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Administrative data from state welfare agencies. See appendix A for details.
Sample: Sample adults. Food stamp data is from two sites: Chicago and Boston.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.

Appendix E

Detailed Estimation Results – Mediating Factors

EXHIBIT E4.1
MEDIATORS: HOUSING AND NEIGHBORHOOD PHYSICAL CONDITIONS

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Housing Conditions					
Housing has cat, dog, or other pet with fur [OBS] (n=3269)	0.180	-0.011 (0.018)	-0.023 (0.037)	-0.022 (0.019)	-0.037 (0.033)
Housing has wall-to-wall carpets [OBS] (n=3314)	0.277	0.088* (0.020)	0.187* (0.042)	0.085* (0.022)	0.143* (0.037)
Housing has problem with broken windows or windows without screens [SR] (n=3501)	0.235	-0.051* (0.018)	-0.108* (0.038)	-0.025 (0.020)	-0.041 (0.033)
Interior-of-Home index: 7 negative items (noise, clutter, deterioration, etc.) [OBS] (n=3345)	0.189	-0.013 (0.010)	-0.028 (0.020)	-0.014 (0.011)	-0.024 (0.018)
Neighborhood Conditions					
Exterior-of-Home index: 7 negative items (poor conditions, signs of deterioration and danger) [OBS] (n=3385)	0.189	-0.038* (0.010)	-0.080* (0.021)	-0.030* (0.011)	-0.051* (0.018)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult survey and Interviewer observations.
Sample: Adults from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E4.2
MEDIATORS: EXERCISE AND NUTRITION

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Fraction of Past Week Child Did Aerobic Exercise [SR] (n=1759)					
All children (ages 8 to 11)	0.570	0.010 (0.024)	0.022 (0.052)	-0.035 (0.027)	-0.054 (0.041)
Girls	0.488	0.045 (0.034)	0.091 (0.068)	0.022 (0.037)	0.034 (0.058)
Boys	0.647	-0.021 (0.034)	-0.050 (0.081)	-0.090* (0.038)	-0.133* (0.056)
Fraction of Past Week Youth Did Aerobic Exercise for 20+ Minutes [SR] (n=2803)					
All youth (ages 12 to 19)	0.483	0.041* (0.018)	0.091* (0.040)	0.026 (0.021)	0.045 (0.037)
Girls	0.394	0.061* (0.025)	0.133* (0.055)	0.011 (0.030)	0.018 (0.049)
Boys	0.571	0.020 (0.026)	0.045 (0.058)	0.040 (0.029)	0.074 (0.054)
Fraction of Past Week Youth Did Light Physical Activity (No Sweat) For 30+ Minutes [SR] (n=2790)					
All youth (ages 12 to 19)	0.464	0.025 (0.020)	0.057 (0.044)	-0.008 (0.022)	-0.014 (0.039)
Girls	0.431	0.030 (0.026)	0.066 (0.058)	-0.039 (0.031)	-0.064 (0.051)
Boys	0.497	0.020 (0.029)	0.046 (0.064)	0.023 (0.030)	0.042 (0.056)
Fraction of Past Week Child Had Some Fruits or Vegetables [SR] (n=4565)					
All children (ages 8 to 19)	0.613	0.006 (0.015)	0.014 (0.033)	-0.010 (0.016)	-0.016 (0.027)
Girls	0.607	0.022 (0.020)	0.046 (0.044)	-0.013 (0.022)	-0.021 (0.035)
Boys	0.620	-0.010 (0.021)	-0.023 (0.049)	-0.006 (0.023)	-0.010 (0.038)
Adult Exercise and Nutrition [SR]					
Fraction of past week did moderate physical activity for 10+ min (n=3507)	0.471	0.026 (0.018)	0.055 (0.039)	0.045* (0.020)	0.075* (0.034)
Fraction of past week adult had some fruits or vegetables (n=3498)	0.671	0.029* (0.014)	0.062* (0.030)	0.021 (0.015)	0.035 (0.026)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child and Youth surveys
Sample: All children ages 8 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E4.3
MEDIATOR: ACCESS TO HEALTH CARE

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Access to Health Care [SR]					
Adult and child have access to health insurance (n=3340)	0.798	0.012 (0.018)	0.026 (0.039)	-0.005 (0.020)	-0.008 (0.033)
In past year, adult or children did not get medical care when they needed it (n=3272)	0.062	-0.005 (0.011)	-0.010 (0.023)	0.007 (0.013)	0.011 (0.021)
- Adult and child have usual place to go when they are sick or need health advice (n=3461)	0.935	-0.009 (0.012)	-0.019 (0.025)	0.011 (0.012)	0.019 (0.019)
In past 6 months, someone has seen/talked to health professional about child's health (n=3225)	0.760	-0.013 (0.020)	-0.028 (0.042)	-0.003 (0.021)	-0.005 (0.035)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Adult survey
Sample: Adult from families randomly assigned through December 31, 1997.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.1
MEDIATORS: YOUTH'S PEERS, AGES 12 TO 19

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Has Friends Who Get Involved in School Activities [SR] (n=2717)					
All youth (ages12 to 19)	0.712	0.015	0.034	0.022	0.038
		(0.023)	(0.053)	(0.025)	(0.045)
Girls	0.684	0.055	0.121	0.036	0.060
		(0.035)	(0.077)	(0.037)	(0.062)
Boys	0.740	-0.026	-0.060	0.007	0.013
		(0.032)	(0.074)	(0.035)	(0.064)
Has Friends Who Use Drugs [SR] (n=2659)					
All youth (ages 12 to 19)	0.251	0.046*	0.101*	0.062*	0.108*
		(0.023)	(0.051)	(0.026)	(0.046)
Girls	0.247	0.011	0.024	0.024	0.040
		(0.030)	(0.066)	(0.035)	(0.058)
Boys	0.256	0.083*	0.187*	0.100*	0.184*
		(0.034)	(0.076)	(0.038)	(0.070)
Has Friends Who Carry Weapons [SR] (n=2711)					
All youth (ages 12 to 19)	0.106	0.021	0.046	0.013	0.023
		(0.017)	(0.038)	(0.019)	(0.033)
Girls	0.091	0.006	0.012	0.024	0.040
		(0.020)	(0.043)	(0.024)	(0.040)
Boys	0.122	0.037	0.086	0.002	0.004
		(0.027)	(0.063)	(0.029)	(0.054)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Youth survey
Sample: All children ages 12 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.2
MEDIATORS: CHILD'S SOCIAL NETWORKS

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child Has at Least One Close Friend [PR] (n=4732)					
All children (ages 5 to 19)	0.914	0.024*	0.052*	0.009	0.015
		(0.011)	(0.022)	(0.013)	(0.020)
Girls	0.918	0.026	0.054	0.007	0.012
		(0.014)	(0.029)	(0.017)	(0.027)
Boys	0.910	0.022	0.050	0.011	0.019
		(0.015)	(0.033)	(0.018)	(0.029)
Child Has 5+ Friends [SR] (n=4556)					
All children (ages 8 to 19)	0.520	0.029	0.064	0.032	0.052
		(0.022)	(0.048)	(0.024)	(0.039)
Girls	0.472	0.048	0.103	0.039	0.063
		(0.030)	(0.065)	(0.032)	(0.052)
Boys	0.567	0.009	0.022	0.025	0.042
		(0.030)	(0.070)	(0.032)	(0.054)
Youth Lives In Same Neighborhood Or Visits With Friends From Old Neighborhood [SR] (n=2723)					
All youth (ages 12 to 19)	0.683	-0.108*	-0.246*	-0.117*	-0.207*
		(0.028)	(0.063)	(0.030)	(0.052)
Girls	0.687	-0.120*	-0.273*	-0.149*	-0.251*
		(0.037)	(0.083)	(0.040)	(0.066)
Boys	0.679	-0.096*	-0.219*	-0.086*	-0.161*
		(0.038)	(0.086)	(0.041)	(0.075)
Youth Lives in Same Neighborhood as Random Assignment [SR] (n=2735)					
All youth (ages 12 to 19)	0.461	-0.133*	-0.302*	-0.156*	-0.275*
		(0.028)	(0.064)	(0.029)	(0.051)
Girls	0.468	-0.152*	-0.344*	-0.188*	-0.316*
		(0.036)	(0.082)	(0.039)	(0.065)
Boys	0.454	-0.114*	-0.260*	-0.124*	-0.231*
		(0.038)	(0.088)	(0.039)	(0.072)
Has Brothers, Sisters, Cousins or Friends Who Belong to a Gang [SR] (n=4409)					
All children (ages 8 to 19)	0.115	-0.003	-0.007	-0.014	-0.023
		(0.013)	(0.029)	(0.014)	(0.022)
Girls	0.109	0.005	0.011	-0.027	-0.044
		(0.018)	(0.037)	(0.017)	(0.027)
Boys	0.122	-0.013	-0.030	-0.001	-0.002
		(0.020)	(0.045)	(0.021)	(0.035)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child and Youth surveys
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
c) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.3
MEDIATORS: ADULT SOCIAL NETWORKS AND SOCIAL CAPITAL

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Friendship and Social Networks [SR]					
Has diffuse network of friends, in which only a few friends know each other (n=3510)	0.274	-0.015 (0.019)	-0.032 (0.041)	0.022 (0.022)	0.037 (0.037)
Has no friends who live in neighborhood (n=3517)	0.590	0.023 (0.022)	0.048 (0.046)	0.045 (0.024)	0.076 (0.040)
Visits friends/relatives in their home at least once a week (n=3514)	0.426	-0.021 (0.022)	-0.044 (0.047)	-0.022 (0.024)	-0.038 (0.041)
Stops to chat with neighbor in street or hallway at least once a week (n=3510)	0.490	0.020 (0.022)	0.043 (0.047)	0.015 (0.024)	0.025 (0.041)
Attended church or religious services at least once a month in past year (n=3509)	0.427	-0.033 (0.021)	-0.069 (0.045)	0.011 (0.024)	0.018 (0.039)
Social Capital [SR]					
Likely or very likely neighbors would do something about kids doing graffiti on local building (n=3349)	0.541	0.109* (0.022)	0.235* (0.047)	0.057* (0.024)	0.096* (0.041)
Likely or very likely neighbors would do something about kids skipping school or hanging out on street corner (n=3270)	0.366	0.106* (0.023)	0.229* (0.049)	0.068* (0.025)	0.115* (0.042)
People can be trusted (n=3486)	0.099	0.010 (0.014)	0.020 (0.029)	0.009 (0.015)	0.016 (0.026)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Adult survey
Sample: All child ages as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.4a
MEDIATORS: TIME USE
TV, SPORTS & CHURCH

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child Watches More Than 5 Hours of TV on Typical Weekday [SR] (n=1771)					
All children (ages 8 to 11)	0.274	-0.022	-0.048	-0.002	-0.003
		(0.029)	(0.063)	(0.033)	(0.050)
Girls	0.275	-0.017	-0.035	0.003	0.005
		(0.041)	(0.084)	(0.050)	(0.078)
Boys	0.272	-0.026	-0.063	-0.007	-0.011
		(0.038)	(0.092)	(0.042)	(0.062)
Child Participated in Sport /Physical Activity on Given Weekday After School [PR] (n=2491)					
All children (ages 5 to 11)	0.049	0.002	0.005	0.005	0.008
		(0.013)	(0.027)	(0.014)	(0.021)
Girls	0.045	-0.001	-0.002	0.002	0.002
		(0.018)	(0.035)	(0.017)	(0.027)
Boys	0.054	0.006	0.013	0.009	0.013
		(0.017)	(0.038)	(0.019)	(0.028)
Youth Participated in Sport /Physical Activity on Given Weekday After School [SR] (n=2704)					
All youth (ages 12 to 19)	0.082	0.028	0.062	0.021	0.036
		(0.015)	(0.034)	(0.017)	(0.030)
Girls	0.031	0.051*	0.111*	0.030	0.049
		(0.018)	(0.038)	(0.019)	(0.031)
Boys	0.132	0.005	0.010	0.012	0.023
		(0.025)	(0.057)	(0.029)	(0.054)
Youth Attended Youth Activities at Church, at Least Once a Month [SR] (n=2778)					
All youth (ages 12 to 19)	0.374	-0.005	-0.010	-0.011	-0.019
		(0.028)	(0.061)	(0.031)	(0.054)
Girls	0.397	0.025	0.054	-0.033	-0.054
		(0.039)	(0.084)	(0.043)	(0.071)
Boys	0.351	-0.036	-0.081	0.010	0.018
		(0.037)	(0.084)	(0.042)	(0.078)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child, and Youth surveys
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.4b
MEDIATORS: YOUTH TIME USE
STRUCTURED ACTIVITY, AGES 12 TO 17

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Youth Spent Time in a Structured Activity on Given Weekday at 3:45p.m. [SR] (n=2174)					
All youth (ages 12 to 17)	0.177	0.033 (0.023)	0.070 (0.048)	0.032 (0.025)	0.055 (0.043)
Girls	0.166	0.051 (0.032)	0.108 (0.067)	0.041 (0.036)	0.067 (0.058)
Boys	0.186	0.015 (0.032)	0.033 (0.069)	0.024 (0.035)	0.044 (0.064)
Youth Spent Time in a Structured Activity on Given Weekday at 5:30p.m. [SR] (n=2175)					
All youth (ages 12 to 17)	0.106	0.036 (0.019)	0.078 (0.042)	0.001 (0.020)	0.002 (0.034)
Girls	0.092	0.063* (0.027)	0.133* (0.058)	0.017 (0.029)	0.027 (0.046)
Boys	0.118	0.011 (0.028)	0.024 (0.061)	-0.013 (0.029)	-0.024 (0.052)
Youth Spent Time in a Structured Activity on Given Weekday at 7:30p.m. [SR] (n=2186)					
All youth (ages 12 to 17)	0.060	0.019 (0.015)	0.042 (0.032)	0.015 (0.017)	0.026 (0.028)
Girls	0.069	0.030 (0.022)	0.064 (0.047)	0.017 (0.026)	0.027 (0.040)
Boys	0.051	0.008 (0.020)	0.018 (0.044)	0.014 (0.022)	0.025 (0.039)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child, and Youth surveys
Sample: All child ages as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.4c
MEDIATORS: YOUTH TIME USE
SUPERVISED OR STRUCTURED ACTIVITY, AGES 12 TO 17

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Youth Spent Time in a Supervised or Structured Activity on Given Weekday at 3:45p.m. [SR] (n=2190)					
All youth (ages 12 to 17)	0.843	-0.013	-0.028	-0.014	-0.024
		(0.021)	(0.045)	(0.025)	(0.042)
Girls	0.861	-0.039	-0.082	-0.001	-0.002
		(0.030)	(0.064)	(0.033)	(0.053)
Boys	0.827	0.014	0.030	-0.028	-0.050
		(0.030)	(0.065)	(0.035)	(0.063)
Youth Spent Time in a Supervised or Structured Activity on Given Weekday at 5:30p.m. [SR] (n=2188)					
All youth (ages 12 to 17)	0.871	-0.010	-0.022	-0.018	-0.031
		(0.020)	(0.043)	(0.022)	(0.037)
Girls	0.882	-0.014	-0.028	-0.007	-0.011
		(0.027)	(0.057)	(0.031)	(0.048)
Boys	0.861	-0.007	-0.014	-0.029	-0.052
		(0.029)	(0.064)	(0.032)	(0.056)
Youth Spent Time in a Supervised or Structured Activity on Given Weekday at 7:30p.m. [SR] (n=2190)					
All youth (ages 12 to 17)	0.900	0.001	0.003	0.001	0.002
		(0.018)	(0.040)	(0.020)	(0.033)
Girls	0.930	-0.029	-0.062	-0.017	-0.027
		(0.026)	(0.054)	(0.027)	(0.043)
Boys	0.873	0.031	0.068	0.018	0.033
		(0.027)	(0.058)	(0.028)	(0.051)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child, and Youth surveys
Sample: All children ages 12 to 17 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.4d
MEDIATORS: CHILD TIME USE
STRUCTURED ACTIVITY, AGES 5 TO 11

Mediator	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Child Spent Time in a Structured Activity on Given Weekday at 3:45p.m. [PR] (n=2442)					
All children (ages 5 to 11)	0.182	-0.039	-0.084	0.001	0.002
		(0.023)	(0.048)	(0.026)	(0.040)
Girls	0.214	-0.076*	-0.152*	-0.041	-0.063
		(0.030)	(0.060)	(0.036)	(0.055)
Boys	0.151	-0.003	-0.006	0.045	0.067
		(0.028)	(0.064)	(0.033)	(0.050)
Child Spent Time in a Structured Activity on Given Weekday at 5:30p.m. [PR] (n=2471)					
All children (ages 5 to 11)	0.052	0.000	0.000	0.021	0.032
		(0.013)	(0.027)	(0.015)	(0.022)
Girls	0.069	-0.034	-0.066	-0.010	-0.015
		(0.017)	(0.035)	(0.019)	(0.029)
Boys	0.037	0.033*	0.075*	0.052*	0.078*
		(0.017)	(0.037)	(0.020)	(0.030)
Child Spent Time in a Structured Activity on Given Weekday at 7:30p.m. [PR] (n=2491)					
All children (ages 5 to 11)	0.013	0.002	0.005	0.010	0.015
		(0.008)	(0.016)	(0.009)	(0.013)
Girls	0.008	0.008	0.016	0.010	0.015
		(0.009)	(0.018)	(0.008)	(0.013)
Boys	0.017	-0.004	-0.008	0.010	0.015
		(0.010)	(0.022)	(0.013)	(0.020)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child, and Youth surveys
Sample: All children ages 5 to 11 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.4e
MEDIATORS: CHILD TIME USE
SUPERVISED OR STRUCTURED ACTIVITY, AGES 5 TO 11

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child Spent Time in a Supervised or Structured Activity on Given Weekday at 3:45p.m. [PR] (n=2485)					
All children (ages 5 to 11)	0.967	-0.008	-0.016	-0.008	-0.012
		(0.011)	(0.024)	(0.013)	(0.020)
Girls	0.970	0.002	0.004	-0.001	-0.002
		(0.015)	(0.029)	(0.016)	(0.024)
Boys	0.964	-0.018	-0.040	-0.015	-0.022
		(0.015)	(0.035)	(0.018)	(0.027)
Child Spent Time in a Supervised or Structured Activity on Given Weekday at 5:30p.m. [PR] (n=2488)					
All children (ages 5 to 11)	0.965	0.000	0.000	0.005	0.008
		(0.011)	(0.023)	(0.013)	(0.019)
Girls	0.960	0.018	0.035	0.006	0.010
		(0.015)	(0.030)	(0.019)	(0.029)
Boys	0.971	-0.017	-0.039	0.005	0.008
		(0.013)	(0.030)	(0.014)	(0.022)
Child Spent Time in a Supervised or Structured Activity on Given Weekday at 7:30p.m. [PR] (n=2497)					
All children (ages 5 to 11)	0.990	-0.009	-0.018	0.001	0.002
		(0.007)	(0.014)	(0.007)	(0.010)
Girls	0.986	0.005	0.009	0.013	0.021
		(0.008)	(0.016)	(0.007)	(0.011)
Boys	0.994	-0.022*	-0.050*	-0.011	-0.017
		(0.010)	(0.022)	(0.011)	(0.017)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child, and Youth surveys
Sample: All children ages 5 to 11 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E5.5
MEDIATORS: CHILD EXPOSURE TO VIOLENCE AND VICTIMIZATION, AGES 8 TO 19

Mediator	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Existence of Gangs in Neighborhood or School [SR] (n=4256)					
All children (ages 8 to 19)	0.538	-0.059*	-0.131*	-0.024	-0.039
		(0.022)	(0.049)	(0.025)	(0.041)
Girls	0.515	-0.068*	-0.147*	-0.027	-0.044
		(0.031)	(0.066)	(0.033)	(0.054)
Boys	0.560	-0.050	-0.116	-0.021	-0.035
		(0.030)	(0.069)	(0.035)	(0.059)
Saw People Selling/Using Illegal Drugs in Neighborhood at Least Once a Week in Past Month [SR] (n=4402)					
All children (ages 8 to 19)	0.351	-0.045*	-0.098*	-0.046	-0.076
		(0.022)	(0.048)	(0.024)	(0.040)
Girls	0.367	-0.097*	-0.205*	-0.097*	-0.156*
		(0.029)	(0.062)	(0.032)	(0.051)
Boys	0.336	0.007	0.016	0.004	0.007
		(0.029)	(0.066)	(0.033)	(0.057)
Heard Gunshots in Neighborhood at Least Once a Week in Past Month [SR] (n=4411)					
All children (ages 8 to 19)	0.120	-0.030*	-0.065*	-0.034*	-0.055*
		(0.014)	(0.030)	(0.015)	(0.025)
Girls	0.109	-0.022	-0.046	-0.024	-0.038
		(0.019)	(0.040)	(0.020)	(0.032)
Boys	0.130	-0.038*	-0.086*	-0.043*	-0.073*
		(0.019)	(0.043)	(0.021)	(0.035)

EXHIBIT E5.5 (CONT.)
MEDIATORS: CHILD EXPOSURE TO VIOLENCE AND VICTIMIZATION, AGES 8 TO 19

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Saw Someone Shoot/Stab Another Person in Past Year [SR] (n=4575)					
All children (ages 8 to 19)	0.128	-0.010	-0.023	-0.018	-0.029
		(0.015)	(0.032)	(0.016)	(0.026)
Girls	0.116	-0.014	-0.029	-0.032	-0.052
		(0.021)	(0.044)	(0.021)	(0.033)
Boys	0.141	-0.007	-0.017	-0.004	-0.006
		(0.021)	(0.048)	(0.024)	(0.040)
Someone Pulled a Knife/Gun on Child in Past Year [SR] (n=4587)					
All children (ages 8 to 19)	0.089	-0.010	-0.021	-0.012	-0.020
		(0.013)	(0.028)	(0.013)	(0.022)
Girls	0.056	-0.014	-0.029	-0.014	-0.022
		(0.015)	(0.033)	(0.016)	(0.026)
Boys	0.122	-0.005	-0.012	-0.011	-0.018
		(0.020)	(0.046)	(0.021)	(0.034)
Someone Cut, Shot, Stabbed Child in Past Year [SR] (n=4595)					
All children (ages 8 to 19)	0.035	-0.012	-0.026	-0.010	-0.016
		(0.009)	(0.019)	(0.009)	(0.014)
Girls	0.025	-0.012	-0.025	-0.014	-0.023
		(0.011)	(0.024)	(0.011)	(0.018)
Boys	0.046	-0.012	-0.027	-0.006	-0.010
		(0.013)	(0.030)	(0.013)	(0.022)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child and Youth surveys
Sample: All child ages as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.1
ADDITIONAL CHARACTERISTICS OF THE SCHOOLS ATTENDED BY MTO CHILDREN

	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Charter School [PR] (n=4876)					
Current school	0.007	-0.000	-0.000	0.008	0.013
		(0.003)	(0.007)	(0.005)	(0.008)
Average school	0.008	-0.001	-0.002	0.002	0.003
		(0.003)	(0.006)	(0.004)	(0.006)
Private School [PR] (n=4903)					
Current school	0.039	-0.005	0.010	-0.008	-0.013
		(0.008)	(0.017)	(0.008)	(0.013)
Average school	0.037	-0.002	-0.004	-0.005	-0.009
		(0.007)	(0.015)	(0.007)	(0.011)
Title I School [PR] (n=2069)					
Current school	0.847	-0.047*	-0.099*	-0.029	-0.044
		(0.024)	(0.050)	(0.023)	(0.036)
Average school	0.790	-0.025	-0.051	-0.012	-0.018
		(0.022)	(0.046)	(0.021)	(0.032)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: U.S. Department of Education, Common Core of Data 1993 to 2001, National School-Level State Assessment Score Database, 2000 to 2001.
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT E6.2
MEDIATORS: SCHOOL CLIMATE, AGES 8 TO 17

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
There is a Lot of Cheating in Tests & Assignments [SR] (n=3875)					
All children (ages 8 to 17)	0.416	0.029	0.063	-0.010	-0.016
		(0.023)	(0.051)	(0.025)	(0.039)
Girls	0.416	0.047	0.099	-0.008	-0.013
		(0.031)	(0.066)	(0.034)	(0.054)
Boys	0.414	0.011	0.025	-0.011	-0.018
		(0.033)	(0.074)	(0.034)	(0.056)
Discipline in School is Fair [SR] (n=3943)					
All children (ages 8 to 17)	0.724	-0.007	-0.015	-0.003	-0.004
		(0.020)	(0.043)	(0.022)	(0.035)
Girls	0.687	0.036	0.075	0.050	0.078
		(0.028)	(0.059)	(0.031)	(0.048)
Boys	0.757	-0.046	-0.105	-0.051	-0.084
		(0.028)	(0.064)	(0.030)	(0.049)
Disruptions From Other Students Inhibit Learning [SR] (n=3984)					
All children (ages 8 to 17)	0.640	0.035	0.076	0.008	0.013
		(0.021)	(0.045)	(0.024)	(0.038)
Girls	0.626	0.059*	0.123*	-0.017	-0.026
		(0.029)	(0.061)	(0.036)	(0.056)
Boys	0.652	0.010	0.024	0.033	0.054
		(0.029)	(0.066)	(0.032)	(0.053)
Child Feels Safe in School [SR] (n=3976)					
All children (ages 8 to 17)	0.775	0.015	0.032	0.019	0.030
		(0.018)	(0.038)	(0.019)	(0.031)
Girls	0.780	0.014	0.028	0.012	0.018
		(0.025)	(0.051)	(0.027)	(0.042)
Boys	0.771	0.016	0.036	0.025	0.042
		(0.025)	(0.057)	(0.027)	(0.045)

EXHIBIT E6.2 (CONT.)
MEDIATORS: SCHOOL CLIMATE

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Teachers are Interested in Students [SR] (n=3966)					
All children (ages 8 to 17)	0.810	-0.010	-0.021	0.017	0.027
		(0.018)	(0.039)	(0.019)	(0.030)
Girls	0.805	0.001	0.001	0.012	0.019
		(0.025)	(0.053)	(0.027)	(0.042)
Boys	0.815	-0.020	-0.045	0.022	0.036
		(0.025)	(0.056)	(0.026)	(0.042)
Child Has Own Math Textbook and Can Take it Home To Do Homework [SR] (n=1770)					
All children (ages 8 to 11)	0.821	0.002	0.004	-0.005	-0.008
		(0.026)	(0.057)	(0.029)	(0.043)
Girls	0.865	-0.008	-0.016	-0.039	-0.060
		(0.030)	(0.062)	(0.037)	(0.057)
Boys	0.779	0.010	0.025	0.028	0.041
		(0.040)	(0.095)	(0.043)	(0.064)
School Climate Index on 5 School Quality Items [SR] (n=3996)					
All children (ages 8 to 17)	0.650	-0.011	-0.024	0.009	0.014
		(0.011)	(0.024)	(0.012)	(0.019)
Girls	0.644	-0.010	-0.020	0.022	0.034
		(0.015)	(0.032)	(0.017)	(0.027)
Boys	0.655	-0.012	-0.028	-0.004	-0.006
		(0.015)	(0.034)	(0.015)	(0.025)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child and Youth surveys
Sample: All children ages 8 to 17 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators. See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.3
MEDIATORS: CHILD ABSENTEEISM, TARDINESS, OR PROBLEMS AT SCHOOL

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child is Late for School Once a Month or More [SR] (n= 4529)					
All children (ages 8 to 19)	0.532	-0.010	-0.021	-0.021	-0.035
		(0.022)	(0.049)	(0.025)	(0.042)
Girls	0.529	-0.022	-0.047	-0.021	-0.034
		(0.028)	(0.060)	(0.032)	(0.052)
Boys	0.535	0.003	0.007	-0.022	-0.037
		(0.031)	(0.072)	(0.036)	(0.060)
Youth is Absent More Than 5% of the School Year [SR] (n=2621)					
All youth (ages 12 to 19)	0.367	-0.039	-0.084	-0.032	-0.055
		(0.026)	(0.056)	(0.029)	(0.050)
Girls	0.383	-0.077*	-0.165*	-0.058	-0.095
		(0.035)	(0.074)	(0.039)	(0.064)
Boys	0.352	0.000	0.001	-0.006	-0.010
		(0.037)	(0.082)	(0.041)	(0.076)
Proportion of Days in the School Year That Youth is Absent [SR] (n=2621)					
All youth (ages 12 to 19)	0.059	-0.001	-0.002	-0.003	-0.005
		(0.005)	(0.010)	(0.005)	(0.009)
Girls	0.066	-0.016*	-0.033*	-0.015*	-0.024*
		(0.006)	(0.014)	(0.007)	(0.011)
Boys	0.052	0.014*	0.031*	0.009	0.016
		(0.007)	(0.015)	(0.007)	(0.012)
School Asked Someone to Come in and Talk About Problems Child Was Having With Schoolwork or Behavior in Past 2 Years [PR] (n=4730)					
All children (ages 5 to 17)	0.299	0.015	0.031	0.008	0.013
		(0.018)	(0.038)	(0.020)	(0.032)
Girls	0.231	-0.013	-0.027	-0.018	-0.029
		(0.025)	(0.051)	(0.027)	(0.042)
Boys	0.368	0.042	0.096	0.034	0.056
		(0.026)	(0.059)	(0.030)	(0.049)

EXHIBIT E6.3 (CONT.)
MEDIATORS: CHILD ABSENTEEISM, TARDINESS, OR PROBLEMS AT SCHOOL

Mediator	Control Mean	Experimental vs. Control		Section 8 vs. Control	
		ITT[a]	TOT[a]	ITT[a]	TOT[a]
Child Was Suspended/Expelled From School in Past 2 Years [PR] (n= 5334)					
All children (ages 5 to 19)	0.166	-0.002	-0.005	-0.010	-0.017
		(0.013)	(0.029)	(0.015)	(0.025)
Girls	0.108	-0.007	-0.015	-0.009	-0.014
		(0.016)	(0.032)	(0.017)	(0.028)
Boys	0.224	0.003	0.006	-0.012	-0.020
		(0.021)	(0.048)	(0.024)	(0.039)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult, Child and Youth surveys
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.4

MEDIATORS: CHILD'S FUTURE EXPECTATIONS, AGES 8 TO 19

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Child Thinks Chances are High/Very High He/She Will Complete College [SR] (n=4521)					
All children (ages 8 to 19)	0.645	0.007 (0.020)	0.016 (0.045)	0.004 (0.023)	0.007 (0.037)
Girls	0.688	0.043 (0.027)	0.093 (0.058)	0.019 (0.032)	0.030 (0.051)
Boys	0.602	-0.029 (0.029)	-0.068 (0.068)	-0.009 (0.031)	-0.015 (0.051)
Child Thinks Chances are High/Very High He/She Will Find a Well-Paid, Stable Job As An Adult [SR] (n=4534)					
All children (ages 8 to 19)	0.780	0.002 (0.017)	0.005 (0.038)	-0.005 (0.020)	-0.009 (0.032)
Girls	0.804	0.036 (0.022)	0.075 (0.047)	0.001 (0.026)	0.002 (0.043)
Boys	0.755	-0.031 (0.027)	-0.072 (0.061)	-0.011 (0.028)	-0.019 (0.048)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child and Youth surveys
Sample: All children ages 8 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.5
MEDIATORS: PARENTAL MONITORING

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Parental Monitoring Index [SR] (n=3996)					
All children (ages 8 to 17)	0.417	0.018	0.039	0.014	0.023
		(0.018)	(0.039)	(0.020)	(0.032)
Girls	0.470	0.018	0.038	-0.001	-0.001
		(0.024)	(0.051)	(0.026)	(0.041)
Boys	0.367	0.017	0.038	0.030	0.049
		(0.024)	(0.055)	(0.026)	(0.042)
Adult Knows Child's Friends [SR] (n=4560)					
All children (ages 8 to 19)	0.309	-0.014	-0.032	0.001	0.001
		(0.020)	(0.044)	(0.022)	(0.036)
Girls	0.329	0.007	0.015	0.014	0.023
		(0.028)	(0.059)	(0.030)	(0.049)
Boys	0.289	-0.036	-0.082	-0.012	-0.020
		(0.027)	(0.061)	(0.029)	(0.049)
Adult Knows Who Child Is With When He/She Is Not Home [SR] (n=4557)					
All children (ages 8 to 19)	0.458	0.025	0.056	0.000	0.000
		(0.022)	(0.048)	(0.024)	(0.039)
Girls	0.524	0.025	0.053	-0.002	-0.003
		(0.029)	(0.062)	(0.031)	(0.050)
Boys	0.392	0.025	0.059	0.003	0.004
		(0.029)	(0.067)	(0.033)	(0.055)
Adult Knows Child's Teacher [SR] (n=3960)					
All children (ages 8 to 17)	0.462	0.020	0.044	0.016	0.026
		(0.022)	(0.049)	(0.026)	(0.042)
Girls	0.517	0.008	0.018	-0.032	-0.050
		(0.030)	(0.064)	(0.035)	(0.055)
Boys	0.409	0.030	0.067	0.063	0.105
		(0.030)	(0.069)	(0.036)	(0.059)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child and Youth surveys
Sample: All children ages 8 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.6
MEDIATORS: PARENTING AND PARENTAL SUPPORT

Mediator	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Saw Father At Least Once a Week In Past Year [SR] (n=4521)					
All children (ages 8 to 19)	0.356	0.023 (0.022)	0.051 (0.049)	0.025 (0.025)	0.042 (0.041)
Girls	0.325	0.055 (0.029)	0.116 (0.061)	0.020 (0.032)	0.032 (0.052)
Boys	0.388	-0.009 (0.030)	-0.020 (0.068)	0.031 (0.034)	0.052 (0.057)
Mother or Child's Primary Caregiver Is Very Supportive [SR] (n=4567)					
All children (ages 8 to 19)	0.830	0.008 (0.016)	0.018 (0.035)	-0.010 (0.019)	-0.016 (0.030)
Girls	0.799	0.019 (0.022)	0.041 (0.047)	0.010 (0.025)	0.015 (0.040)
Boys	0.860	-0.003 (0.021)	-0.006 (0.049)	-0.029 (0.025)	-0.048 (0.042)
Father Is Very Supportive [SR] (n=4471)					
All children (ages 8 to 19)	0.345	0.018 (0.021)	0.040 (0.047)	-0.015 (0.024)	-0.025 (0.039)
Girls	0.336	0.027 (0.029)	0.058 (0.061)	-0.032 (0.032)	-0.052 (0.052)
Boys	0.354	0.008 (0.030)	0.019 (0.069)	0.002 (0.033)	0.004 (0.056)
Parenting [OBS]					
Parental warmth scale (n=1711)	0.689	-0.002 (0.017)	-0.004 (0.034)	0.012 (0.019)	0.019 (0.029)
Parental hostility scale (n=1733)	0.061	0.003 (0.011)	0.005 (0.023)	-0.004 (0.011)	-0.007 (0.018)
Parental verbal skills scale (n=1978)	0.970	-0.017* (0.008)	-0.036* (0.017)	-0.016 (0.011)	-0.024 (0.016)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Child and Youth surveys and Interviewer observations.
Sample: All children ages 8 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

EXHIBIT E6.7
MEDIATORS: ENGAGEMENT IN SCHOOL AND PARENTAL INVOLVEMENT

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Engagement in School [SR]					
Always pays attention in class (n=4064)	0.553	0.021 (0.023)	0.046 (0.051)	0.029 (0.025)	0.047 (0.040)
Always completes homework (n=2228)	0.497	-0.022 (0.030)	-0.049 (0.065)	-0.029 (0.033)	-0.048 (0.056)
Works hard in school (n=4062)	0.579	-0.004 (0.023)	-0.008 (0.050)	0.007 (0.024)	0.012 (0.039)
Hours per week spent on homework (n= 2250)	4.835	0.582 (0.309)	1.270 (0.673)	0.582 (0.374)	0.969 (0.623)
Hours per week reading (n= 4630)	3.611	-0.069 (0.206)	-0.155 (0.465)	0.016 (0.227)	0.026 (0.372)
Parental Involvement [PR]					
Number of days family eats together (n=3482)	4.442	0.278* (0.110)	0.589* (0.234)	0.017 (0.122)	0.029 (0.203)
Adult attended an event at child's school in past year (n=3284)	0.552	0.030 (0.023)	0.064 (0.048)	0.023 (0.024)	0.038 (0.041)
Adult attended a meeting at child's school in past year (n=3286)	0.72.5	0.02.5 (0.020)	0.052 (0.042)	0.020 (0.022)	0.033 (0.037)
Adult volunteered at child's school in past year (n=3284)	0.34	-0.02.2 (0.021)	-0.047 (0.044)	0.008 (0.023)	0.014 (0.038)
Adult worked with youth group, sports team or club in past year (n=3286)	0.197	-0.04.4 (0.018)	-0.094 (0.038)	-0.043 (0.019)	-0.071 (0.032)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Source: Woodcock Johnson-Revised tests.
Sample: All children ages 5 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – arrest data, PR – parental report, SR – self-report, OBS—interviewer observations).

EXHIBIT E7.1
MEDIATOR: JOB TRAINING, ADULT AND YOUTH

Outcome	Control Mean	Experimental vs. Control ITT[a]	Experimental vs. Control TOT[a]	Section 8 vs. Control ITT[a]	Section 8 vs. Control TOT[a]
Adult Job Training [SR] (n=3511)					
Adult participated in job-related training since September 2000	0.180	-0.017 (0.016)	-0.035 (0.035)	0.016 (0.019)	0.026 (0.031)
Youth Job Training [SR] (ages 17 to 19) (n=901)					
Youth participated in job-related training since September 2000	0.218	-0.019 (0.037)	-0.044 (0.090)	-0.009 (0.042)	-0.017 (0.076)

* = p<.05 on t-test. Robust standard errors are shown in parentheses.
Sources: Adult and Youth surveys
Sample: All children ages 17 to 19 as of May 31, 2001.
Notes: a) **ITT** = Intent-to-Treat; **TOT** = Treatment-on-Treated. See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted with robust standard errors.
c) Subgroup impacts were estimated in a single equation using interactions with treatment indicators.
See appendix B for detailed explanation of estimation procedures.
d) Abbreviations in brackets indicate the source of the outcome information (ADMIN – administrative records, PR – parental report, SR – self-report, M – direct measurement, OBS – interviewer observations).

Appendix F

Tests for Nonresponse Bias

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Tests for Nonresponse Bias

Many of the outcomes analyzed in this report were measured with survey data. As in all surveys, there was some nonresponse in the MTO interim evaluation surveys. In this appendix, we use administrative data on employment, earnings, and welfare data, which are available for all sample members, to analyze the extent to which the impacts on these outcomes differed between adult survey respondents and the entire sample. While not a definitive test for nonresponse bias in other outcomes, we believe that this analysis provides a strong indication of the likely severity of nonresponse bias in the impact estimates based on survey data. It also provides measures of the effectiveness of the steps taken in the interim evaluation survey to reduce nonresponse bias.

As described in appendix B, after completion of the main field period for the interim evaluation survey, we implemented a subsampling procedure to reduce nonresponse bias. Our strategy was to continue to work 3 in 10 of the cases that had not been completed during the main field period. By focusing survey resources on a random subsample of cases, we were able to achieve a higher effective response rate than if we had used the same resources to continue to work the full sample.[1] This was expected to reduce bias due to nonresponse.

During the main field period, 80 percent of the adult sample was interviewed (this group of respondents is hereafter denoted the “main sample”). Among the 3 in 10 subsample, the completion rate was 48 percent, yielding an effective response rate of 89.6 percent. The analyses of outcomes based on survey data presented in this report are based on the sample of all respondents, with weights reflecting their sampling probabilities—i.e., weights of 1 for those in the main sample and 3.33 for respondents in the survey subsample.

In addition to measuring the bias in impact estimates based on this survey respondent sample, we also test the effectiveness of the subsampling approach in reducing nonresponse bias. To do so, we estimated impacts on outcomes measured with administrative data, which are available for all sample members, for three different adult samples:[2]

- the main survey sample,

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1 The effective response rate is equal to the main sample response rate (MRR) plus the subsample response rate (SRR) multiplied by one minus the main sample response rate: MRR+ SRR*(1 – MRR).

2 These impact estimates were derived from a single regression, in which the treatment indicator was interacted with dummy variables for the main survey sample, respondents in the subsample, and all nonrespondents. If we denote the coefficients of these three interactions m, s, and n, respectively, the impact on the main sample equals m, the impact on the full sample is ($w_1m + w_2s + w_3n$), and the impact on the weighted survey sample is ($w_4m + w_5s$), where w_1, w_2, and w_3 are the unweighted proportions of the overall sample in each of the three subgroups and w_4 and w_5 are the sampling weights of the main survey sample and the survey subsample, respectively. (See appendix B for details of the subsampling procedure and the construction of sampling weights.)

- the survey respondent sample (i.e., the combination of the main sample and respondents in the subsample), and
- the full sample.

Under random assignment, the estimated impacts on the full sample are unbiased estimates of the true impacts. The differences between these estimates and those for the main sample are estimates of the nonresponse bias that would have occurred if we had not attempted to interview the hard-to-interview cases still outstanding at the end of the main field period.[3] Similarly, the differences between the estimated impacts on all sample members and those in the weighted survey respondent sample are estimates of the nonresponse bias in the estimates based on the sample used in this report. The difference between these two estimates of bias measures the contribution of the subsample in reducing nonresponse bias.

Exhibit F.1 shows, for each of six outcomes, the estimated ITT impacts on the main sample, the weighted survey respondent sample, and the full sample. Exhibit F.2 shows the estimated bias in the estimates based on the main sample and the weighted survey respondent sample, derived as the difference between the impact estimates for these subgroups and the corresponding estimates for the full sample.

These tests provide somewhat mixed evidence of the effectiveness of the subsample in reducing nonresponse bias. For 7 of the 8 estimated impacts on welfare outcomes, the bias in the estimates based on the weighted survey respondent sample is in fact smaller than in the estimates based on the main sample. Moreover, for these outcomes, 5 of the 6 biases that were statistically significantly different from zero in the main sample are no longer statistically significant in the weighted survey respondent sample. The opposite pattern holds for the employment and earnings outcomes, however. For all 4 of these estimates, the magnitude of the bias in the weighted survey respondent sample is larger than that in the main sample, and in 2 of the 4 cases a bias that was insignificant in the main sample becomes statistically significant in the weighted survey respondent sample.

These results suggest that the effectiveness of the subsample in reducing nonresponse bias probably varied across outcomes, although the consistency of the tests within each of the two domains represented here suggests that among highly correlated outcomes the effect may have been relatively uniform. Unfortunately, the administrative data required to carry out these tests are only available for a small number of outcomes and are not available at all for the outcomes measured with survey data.

It is important to note that even where the bias in the estimates based on the survey respondent sample is statistically significant, it is relatively small—less than 5 percent of the control mean. Thus, these results provide some confidence that, whatever the effectiveness of the subsample in reducing bias, nonresponse bias in the estimates based on survey data is not a serious concern for this study.

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3 It is important to recognize that the measures of bias presented here are only estimates, based on a single draw from the sampling distribution. The true bias, which is the mean of this difference across all draws from the sampling distribution, cannot be measured in any given sample. We use tests of statistical significance to take account of the sampling variability of this estimate of bias.

EXHIBIT F.1
ESTIMATED IMPACTS ON SELECTED OUTCOMES, FOR MAIN SAMPLE, WEIGHTED SURVEY RESPONDENT SAMPLE, AND FULL SAMPLE

		Experimental Vs. Control			Section 8 vs. Control		
Outcome	**Control Mean**	**Main Sample**	**Weighted Survey Sample**	**Full Sample**	**Main Sample**	**Weighted Survey Sample**	**Full Sample**
Welfare Benefits [Admin]							
TANF receipt, sample adult, year 5	.258	.010	-.001	-0.007	.046	.030	.028
TANF amount, sample adult, year 5	1288	37	15	-36	89	49	50
Food stamp receipt, sample adult, year 5	.440	.037	.029	0.010	.053	.044	.032
Food stamp amount, sample adult, year 5	1272	44	15	-27	109	91	112
Employment and Earnings [Admin]							
Fraction of quarters employed, sample adult, years 1 to 4	.413	-.002	.005	-.006	.005	.021	.002
Annualized earnings, sample adult, years 1 to 4	5302	-151	248	10	-115	192	-69

* = p<.05 on t-test.
Source: State UI, TANF, and food stamp administrative records.
Sample: TANF and food stamps: All sample adults. Employment and earnings: All sample adults in California, Illinois, Maryland, and New York.
Notes: a) All estimates are ITT (intent-to-treat). See Section 1.4 and appendix B for details.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Abbreviations in brackets indicate the source of the outcome information (ADMIN = administrative records).

EXHIBIT F.2
ESTIMATED BIAS, SELECTED OUTCOMES: MAIN SAMPLE AND WEIGHTED SURVEY RESPONDENT SAMPLE

Outcome	Control Mean	Experimental vs. Control: Est'd Bias, Main Sample	Experimental vs. Control: Est'd Bias, Wt'd Survey Sample	Section 8 vs. Control: Est'd Bias, Main Sample	Section 8 vs. Control: Est'd Bias, Wt'd Survey Sample
Welfare Benefits [Admin]					
TANF receipt, sample adult, year 5	.258	.017*	.006	.018*	.002
TANF amount, sample adult, year 5	1288	73*	51	39	1
Food stamp receipt, sample adult, year 5	.440	.027*	.019*	.021*	.012
Food stamp amount, sample adult, year 5	1272	71*	42	-3	-21
Employment and Earnings [Admin]					
Fraction of quarters employed, sample adult, years 1 to 4	.413	.004	.011*	.003	.019*
Annualized earnings, sample adult, years 1 to 4	5302	-161	238	-46	261

* = p<.05 on t-test of null hypothesis that bias = 0.
Source: State UI, TANF, and food stamp administrative records.
Sample: TANF and food stamps: All sample adults. Employment and earnings: All sample adults in California, Illinois, Maryland, and New York.
Notes: a) Bias estimates based on ITT (intent-to-treat) impact estimates. See Section 1.4 and appendix B for detailed discussion of ITT estimates and their derivation.
b) Control means and impact estimates are regression-adjusted, with robust standard errors.
c) Abbreviations in brackets indicate the source of the outcome information (ADMIN = administrative records).

Appendix G

Assessment of the Size and Significance of the Impact Estimates

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Assessment of the Size and Significance of the Impact Estimates

In this appendix we examine several questions related to the size and significance of the estimated impacts of MTO and the likelihood that more and/or larger effects will be observed in the future:

- Do the findings provide evidence of real effects on family outcomes?
- Were the estimated effects of MTO large enough to be relevant for policy?
- Might we have missed some effects that were large enough to be relevant for policy?
- How different would the results have been if the families who moved with program vouchers had stayed in low-poverty areas longer?
- Can larger effects be expected in the longer term?

We examine each of these questions in turn.

Do the findings provide evidence of real effects on family outcomes?

A number of the estimated effects of MTO were statistically significant. For an individual estimate, statistical significance at the .05 level means that the chance of obtaining an estimate that large or larger when the actual effect is zero is less than 5 in 100. This is generally regarded as a low enough chance of a false positive to be disregarded and to treat the estimate as convincing evidence of a real effect. But when large numbers of estimates are derived, the chance that some of them will exceed the .05 significance threshold by chance alone may be substantially higher. If, for example, we derived 100 estimates, we would expect 5 of them to be significant by chance alone. The question therefore arises, given the number of estimates presented here, how much credence should be placed in those that were statistically significant?

Unfortunately, there is no simple answer to this question. For any given number of estimates, the number that would be expected to be significant by chance alone is easily calculated. It is 0.05 times the number of tests. But, as with any expected value, the actual number of false positives in any given sample can be greater or less than the expected value. As usual, we tend to regard large deviations from the mean (many more statistically significant estimates than would be expected by chance) as evidence that the results were not simply due to chance sampling error with zero actual effects. There are several problems with this. First, it is difficult to say what large means in this case. If, to give an extreme example, all of the outcomes were perfectly correlated, then there would be a 0.05 chance that all would be statistically significant if the true effects were all zero. Even so, we can say something about how likely it is to get the observed number of statistically significant effects by chance alone and we can use this to help judge the likelihood of getting the observed results if there actually were no real effects. Second, even if the number of statistically significant effects is no greater than would be expected by chance alone, it is always possible that some of them reflect real effects. If we try to take account of the additional error introduced by looking at large numbers of tests, we always run the risk that a few real effects may be masked by large numbers of tests where there are no effects. Further, the number of statistically significant estimates presented in the report is affected by the apparent significance of the summary measure for a domain, such as when we present the components of the risky behavior index and not the delinquency index in Chapter 5 because the former index has a significant treatment effect and we explore its components in greater detail. Nevertheless, we believe that there is some information to be gained by examining the numbers and patterns of statistically significant estimates across the domains and subdomains analysed.

In several domains, for example, the number of statistically significant estimates is actually less than would be expected by chance alone. These include the employment and earnings domain (one significant estimate out of 46), the subdomain of educational performance (two significant estimates out of 58), and the subdomain of household income, food security, and self-sufficiency (zero significant estimates out of 18).[1] In these cases, it is clear that the interim evaluation provides little or no evidence of real effects of MTO on the outcomes of interest.

Conversely, there are some domains and subdomains where the number of significant estimates substantially exceeds the number that would be expected by chance alone. These include the housing conditions and housing assistance domain (17 significant outcomes out of 24) and the subdomain of school characteristics (16 significant estimates out of 24). It seems unlikely that this many estimates would be significant by chance alone.[2]

The situation is less clear in the remaining domains—health; delinquency, crime, and risky behavior; and public assistance. In these domains, about 15 percent of the estimates were statistically significant, as compared with the 5 percent that would be expected by chance alone. This could simply reflect sampling variability within this particular sample.[3] We leave it to the reader to assess the validity of these estimates.

Were the estimated effects of MTO large enough to be relevant for policy?

As detailed in the previous section, MTO had statistically significant effects on a number of outcomes. Even if we accept these estimates as evidence of real effects of MTO, the fact that they were statistically significant does not necessarily mean that they are large enough to be of practical significance. To assess the importance of these effects for policy, we compare a number of these estimated impacts with the mean outcomes that would have been experienced in the absence of the

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1 These counts, and those given below, are for the ITT estimates presented in the text.

2 By Markoff's Inequality, the probability of obtaining 16 or more significant estimates out of 24, as we did in the school characteristics subdomain, when there are no true impacts on these outcomes, is less than .075, regardless of the correlation among the impacts. In judging this, it may be useful to note that Markoff's Inequality never gives a probability less than 0.05, even if all of the tests are significant. This reflects the fact, mentioned earlier, that if all of the measures are perfectly correlated, then either all of them will be significant or none of them will be. With a 0.05 test, there would be a 0.05 chance of all being significant.

3 By Markoff's Inequality, the chance of 15 percent or more of the estimates being statistically significant when all true effects are zero is less than .33.

demonstration by families who leased up with program vouchers and, where available, with other benchmark values of the outcomes. Exhibit G.1 shows these comparisons.

The first two columns of Exhibit G.1 show the estimated impact on those families in the MTO experimental group who leased up with program vouchers (the treatment on treated or TOT effect) and the mean outcome that these families would have experienced in the absence of the demonstration, which we term the “counterfactual”. We estimate the counterfactual by subtracting the estimated impact on these families from the actual mean outcome for this subgroup. This provides a better benchmark against which to compare the TOT impacts than the control mean shown in the text tables because the control mean includes families who did not lease up with program vouchers. The third and fourth columns show the corresponding impact estimates and counterfactuals for the Section 8 group. We confine our attention to the TOT estimates here because they represent the effect of the demonstration on those families who actually leased up in the two treatment groups. The final column shows other benchmark values of these outcomes, where available.

As can be seen in the exhibit, most of the estimated impacts are quite large by these standards. For example, among adults in the experimental group who moved with program vouchers, MTO increased the proportion who felt safe at night by two-thirds and the fraction who rated their housing good or excellent by 40 percent, while reducing the proportion who saw drugs being sold in their neighborhood by nearly 60 percent. The impacts on these measures in the Section 8 group were only about half as large, but still substantial.

The 11 percentage point reduction in obesity among adults in the experimental group represents more than a 20 percent reduction in the obesity rate for this group. Another way to gauge the size of this effect is to note that 11 percentage points is larger than the difference in obesity rates between the poor and the nonpoor in the U.S. population overall (see Other Benchmark in exhibit G.1). Similarly, MTO reduced the psychological distress index by 20 percent among sample adults. This effect is more than half as large as the poor/nonpoor differential in the U.S. population.

The effects on youth mental health were similarly large. MTO reduced the rate of generalized anxiety disorder by more than two thirds, relative to what it would have been in the absence of the demonstration, for girls who moved with program vouchers in both the experimental group and the Section 8 group, and for youth overall in the Section 8 group.

In the delinquency and risky behavior domain, both the favorable effects for girls and the unfavorable effects for boys were also quite substantial relative to the counterfactual. MTO reduced the rate of arrests for violent crimes by three-quarters among girls in the Section 8 group who moved with program vouchers, and reduced marijuana use and smoking by about half in the experimental group. Among boys who moved with program vouchers in the experimental group, MTO increased the behavior problem index by two-thirds, tripled the rate of arrests for property crimes, and quadrupled the incidence of smoking. In the Section 8 group, the demonstration raised the behavior problem index by one-third and tripled the proportion who smoked.

EXHIBIT G.1
SELECTED STATISTICALLY SIGNIFICANT IMPACTS, RELATIVE TO COUNTERFACTUAL AND OTHER BENCHMARKS

Outcome	Experimental TOT Est.	Experimental Counter factual[a]	Section 8 TOT Est.	Section 8 Counter factual[a]	Other Benchmark (source)
Housing and Neighborhood Conditions					
Feel safe at night	.303*	.498	.156*	.509	
Saw drugs past 30 days	-.248*	.427	-.171*	.446	
Rate housing good/excellent	.210*	.479	.119*	.484	
Adult Health [SR]					
Obesity	-0.108*	0.507	Ns	0.493	Poor: 24.7% obese Not poor: 19.0% obese (Pleis & Cole, 2002, Table 31, p.79)
Psychological distress index for past month	-0.073*	0.368	Ns	0.323	Poor: 0.223 on psych index Not poor: 0.098 on psych index (Pleis & Cole, 2002, Tables 13 and 15, p. 37 and 41)
Youth Mental Health [SR] (ages 12 to 19)					
Generalized anxiety disorder (all youth)	Ns	0.096	-0.061*	0.090	
Generalized anxiety disorder (girls)	-0.092*	0.149	-0.078*	0.117	
Delinquency and Risky Behavior (youth ages 15 to 19) [SR]					
Behavior problems (boys self-report)	.169*	.249	.095*	.301	Difference between median and 70th percentile is .18 among MTO Controls
Ever arrested for property crime (boys)	.115*	.062	Ns	.127	Boy/girl difference is .06 among MTO Controls
Ever smoke cigarettes (boys)	.314*	.078	.305*	.147	Difference between age 12 and age 18 is .11 among MTO Controls.
Ever arrested for violent crime (girls)	Ns		-.155*	.199	Boy/girl difference is .060 among MTO Controls
Ever use marijuana (girls)	-.276*	.501	Ns	--	Difference between age 14 and age 18 is .30 among MTO Controls
Ever smoke cigarettes (girls)	-.184*	.398	Ns		Difference between age 15 and age 18 is .26 among MTO Controls

[a] Counterfactual = estimated mean outcome that families who leased up with program vouchers would have experienced in the absence of the demonstration; estimated as actual mean for this group minus estimated impact

ns = not statistically significant

In viewing these results it should be recognized that while the counterfactual is useful as a benchmark, the size of the impact as a proportion of the mean is not always a good measure of the importance of the effect. This is particularly true when the mean is small or the incidence of the problem is low in the population. In these cases, even effects that are large relative to the mean may be small in relation to the overall population and, therefore, of only marginal importance for policy. For example, MTO reduced the incidence of generalized anxiety disorder among girls in the Section 8 group by over two-thirds, but because this is a problem that affects less than 1 girl in 10, this was an improvement in the lives of only about 6 percent of the girls in the sample.

This caveat notwithstanding, we conclude that virtually all of the statistically significant impacts of MTO were substantial enough to be important for policy.

Might we have missed some effects that were large enough to be relevant for policy?

Random assignment provides unbiased estimates of treatment effects, but it does not guarantee that all real effects will be detected. The likelihood of detecting an effect of any given size depends on sample sizes, the variability of the outcome of interest, and, in this case, the leaseup rates in the treatment group. For any given combination of these factors there is some minimum effect that we can be confident of detecting—the minimum detectable effect (MDE) for that design. If the true effect is substantially smaller than the MDE, there is a good chance that it will not be detected as statistically significant—i.e., it will not be distinguishable from random noise in the data generated by sampling error.[4]

Exhibit G.2 compares the MDEs for a number of outcomes for which statistically significant impacts were not found with the estimated mean of those outcomes in the absence of the demonstration (the counterfactual), for families who leased up with program vouchers and, where available, other benchmark values of the outcome. [5]This comparison shows how large the true impact would have to have been for us to be confident of detecting it.

The MDEs for most of these measures are quite large. For example, for adults and youth in both the experimental group and the Section 8 group, MTO would have to have improved general physical health by an amount that is over half the difference between the poor and nonpoor populations in the U.S. at large for us to be confident of detecting the effect. For girls or boys taken separately, the effect would have to have been even larger to be detectable with high confidence. Similarly, to be confident that we could detect effects on asthma, MTO would have to have reduced its incidence among adults by nearly half and among youth ages 12 to 19 by over two-thirds. To be reasonably sure of detecting an effect among boys or girls separately, the demonstration would have had to virtually eliminate the condition. The same is true of generalized anxiety disorder for boys.

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[4] The minimum detectable effects presented in this section are based on 80 percent power at the .05 significance level (two-tailed test)—i.e., there is an 80 percent chance that a true effect as large or larger than the MDE would yield an estimate that would be statistically significant at the .05 level.

[5] As noted above, we estimate the counterfactual by subtracting the estimated impact on the families who leased up with program vouchers from the actual mean outcome for this subgroup.

EXHIBIT G.2
MINIMUM DETECTABLE EFFECTS FOR SELECTED STATISTICALLY INSIGNIFICANT TOT ESTIMATES, RELATIVE TO COUNTERFACTUAL AND OTHER BENCHMARK VALUES

	Experimental		Section 8		
Outcome	MDE	Counter factual[a]	MDE	Counter factual[a]	Other Benchmark (source)
Adult Health [SR]					
General health is currently good or better	0.11	0.702	0.10	0.683	Difference in proportions of poor and nonpoor with good or better health is .194 [c] (Pleis & Cole, 2002, Table 21, p. 57)
Had asthma attack or wheezing during past year	0.10	0.210	0.09	0.207	
Youth Physical Health [SR] (ages 12 to 19)					
General health is currently very good or better (all youth self-report) [c]	0.15	0.678	0.12	0.671	Difference in proportions of poor and nonpoor with good or better health (according to proxy report) is .213 (Blackwell & Tonthat, 2002, Table 6, p. 17)
General health is currently very good or better (girls)	0.21	0.591	0.15	0.624	
General health is currently very good or better (boys)	0.21	0.771	0.17	0.722	
Had asthma attack or wheezing during past year (all youth)	0.11	0.143	0.09	0.136	
Had asthma attack or wheezing during past year (girls)	0.18	0.157	0.15	0.191	
Had asthma attack or wheezing during past year (boys)	0.16	0.128	0.15	0.076	
Youth Mental Health [SR] (ages12 to 19)					
Generalized anxiety disorder (boys)	0.11	0.037	0.08	0.060	

EXHIBIT G.2 (CONT.)
MINIMUM DETECTABLE EFFECTS FOR SELECTED STATISTICALLY INSIGNIFICANT TOT ESTIMATES, RELATIVE TO COUNTERFACTUAL AND OTHER BENCHMARK VALUES

	Experimental		Section 8		
Outcome	**MDE**	**Counter factual[a]**	**MDE**	**Counter factual[a]**	**Other Benchmark (source)**
Delinquency and Risky Behavior (youth ages 15 to 19) [SR]					
Delinquency Index (all youth)	.048	0.083	0.05	0.089	
– Girls	.050	0.050	0.05	0.056	
– Boys	0.05	0.120	0.08	0.124	
Risky Behavior Index (all youth)	0.08	0.442	0.08	0.444	
– Boys	0.05	0.382	0.20	0.445	
Education					
Pupil-teacher ratio, current school [ADMIN]	1.40	16.17	1.00	16.08	
Broad reading score - WJR	5.79	497	4.85	497	
Broad math score - WJR	4.90	501	3.89	501	
Adult Earnings					
Annual earnings, years 1 and 2 [ADMIN]	1,456	4905	1,285	4417	
Annual earnings, years 3 and 4 [ADMIN]	1,842	7159	1,537	6255	
Youth Earnings					
Total earnings, 2001 [ADMIN]	882	801	605	1,068	
Income and Public Assistance					
Total TANF benefits, year 5 [ADMIN]	784	1472	590	1238	
Household income as % of poverty line in 2001 [SR]	.10%	.95%	.09%	.87%	
% Households food insecure with hunger [SR]	.05%	.13%	.03%	.11%	

Notes:

[a] Counterfactual = estimated mean outcome that families who leased up with program vouchers would have experienced in the absence of the demonstration; estimated as actual mean for this group minus estimated impact.

na = not available

[c] Benchmark source: Pleis JR, Coles R. Summary health statistics for U.S. adults: National Health Interview Survey, 1998. National Center for Health Statistics. Vital Health Stat. 10(209). 2002. “Poor” was defined as below the poverty line and “Not poor” as 200 percent of the poverty line or greater (third category is ‘near poor”).

[d] To calculate the benchmarks for the psychological distress index, we used the NHIS summary statistics on the number of individuals who experienced each of the six items “all or most of the time” or “some of the time” and then divided this by 6 times the total number of persons 18 and over in that income category. The formula is: (#-sad-all-of-time + #-sad-some-of-time + #-hopeless-all-of-time + #-hopeless-some-of-time + #-worthless-all-of-time + #-worthless-some-of-time+ #-everything-effort-all-of-time + #-everything-effort -some-of-time+ #-nervousness-all-of-time + #-nervousness-some-of-time+ #-restlessness-all-of-time + #- restlessness-some-of-time) / (6*#-persons-18-and-over). Note that since missing values are included in the denominator but not in the numerators, missing values are essentially treated as not exhibiting a particular symptom.

[e] Benchmark source for depression: Kessler, R. C., K. A. McGonagle, et al. (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(1): 8-19.

[f] Note that the MTO depression outcome is based on the short form of the CIDI-SF consistent with the DSM-IV whereas the benchmark value is for the full CIDI measure and was based on the DSM-III-R and therefore uses a slightly different criteria for weight loss (10 pounds for DSM-IV versus 15 pounds for DSM-III-R).

ns = not significant

[c] Benchmark source: Blackwell DL, Tonthat L. Summary Health Statistics for U.S. Children: National Health Interview Survey, 1998. National Center for Health Statistics. Vital Health Stat 10(208). 2002. Poor was defined as below the poverty line and Not poor as 200% of the poverty line or greater (third category is near poor). Very good or better was calculated as the sum of the percent reporting excellent and the percent reporting very good health (due to rounding, value may not be precise). Note that the NHIS uses the report of a proxy for children. Also note that these benchmarks are for all children rather than just ages 12 to 19.

Similar results hold for the delinquency and risky behavior outcomes. The demonstration would have to have reduced the delinquency index by half or more for us to be confident of detecting the effect. We would be likely to detect somewhat smaller, though still large, effects on the risky behavior index.

We could have been confident of detecting a 6 to 8 percent reduction in the student-teacher ratio or a 4 to 6 point change in test scores. The latter, though only about 1 percent of the mean test score, corresponds to a fairly substantial movement in the test score distribution. For a 12-year-old, for example, a change in the Broad Reading score from 500 to 505 represents a movement from the 33rd percentile to the 42nd percentile.

For the earnings, household income, and public assistance outcomes, we are only likely to detect large effects with this sample. Impacts on adult earnings would have to be at least 25 percent of the counterfactual to be confidently detected and effects on youth earnings would have to be somewhat larger. To be 80 percent confident of detecting impacts on welfare benefits, those impacts would have be roughly half as large as the benefits that would have been received in the absence of the demonstration. Similarly, MTO would have to reduce the incidence of food insecurity with hunger by 30 to 40 percent for us to be confident of detecting the effect. These are all relatively large impacts.

Larger samples and/or higher leaseup rates would almost certainly have yielded more statistically significant estimates. In an attempt to obtain some indication of the effect that a larger sample might have, we estimated impacts on the two treatment groups combined, thereby roughly doubling the size of the treatment group. The estimates produced by the pooled sample are, of course, hybrids of the effects on the locationally constrained MTO experimental families and those on the unconstrained Section 8 families.[6] Unless the heterogeneity of the two groups outweighs the added precision obtained from the increased sample size, however, this should yield more precise estimates of effects. Because the locations of the two treatment groups have converged a good deal over the followup period, for many outcomes the two impacts may not be all that different. Exhibit G.3 shows the results of this exercise, in comparison with the estimated impacts for the two separate groups, for the outcomes included in the previous exhibit. As can be seen, the pooled estimates are somewhat more precise, but none rise to the level of statistical significance.

On the basis of these results, we conclude that the MTO impact estimates are sufficiently imprecise that we may have missed some impacts that are large enough to be relevant for policy, but not large enough to pass the test of statistical significance. To further investigate this possibility, we examined the 95 percent confidence intervals around the estimated impacts—i.e., the range that, in repeated sampling, would be expected to contain the true value of the impact 95 percent of the time.

These confidence intervals, along with the TOT estimates and the counterfactuals for each outcome, are shown in exhibit G.4. For several outcomes it does appear that substantial impacts are unlikely. For example, these results suggest that the true impact on the delinquency index is highly unlikely to be more than 10 to 15 percent of the counterfactual (i.e., the lower bounds of the 95 percent confidence interval for the experimental and Section 8 groups are -.05 and -.06, respectively,

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[6] The estimated impacts on the two groups combined are in fact weighted averages of the separate impact estimates.

EXHIBIT G.3
ESTIMATED IMPACTS ON THE EXPERIMENTAL AND SECTION 8 TREATMENT GROUPS, SEPARATELY AND COMBINED (TOT ESTIMATES) – OUTCOMES FOR WHICH IMPACTS WERE NOT STATISTICALLY SIGNIFICANT

Outcome	MTO Experimental	Section 8	Combined Treatment Groups
Adult Health			
General health good or better [SR]	-.033 (.042)	-.013 (.036)	-.023 (.034)
Asthma or wheezing attack [SR]	-.031 (.037)	-.016 (.032)	-.024 (.031)
Children's Health (ages 12 to 19)			
Gen health very good/excel [SR]	.001 (.054)	-.010 (.048)	-.004 (.045)
Asthma or wheezing attack [SR]	.065 (.042)	.022 (.036)	.044 (.033)
Delinquency and Risky Behavior (ages 15 to 19)			
Behavior problems index [PR]	.042 (.032)	.006 (.026)	.025 (.025)
Risky behavior index [SR]	-.019 (.053)	.030 (.047)	.005 (.044)
Education (children ages 5 to 19)			
Student-teacher ratio, current school [ADMIN]	.52 (.50)	-.26 (.36)	.12 (.37)
WJR broad reading score	2.04 (2.07)	.74 (1.73)	1.44 (1.62)
WJR broad math score	.50 (1.75)	-1.74 (1.39)	-.64 (1.35)
Employment and Earnings—Adults			
Annualized Earnings, years 1 and 2 [ADMIN]	-552 (520)	-251 (459)	-408 (434)
Annualized Earnings, years 3 and 4 [ADMIN]	-360 (658)	73 (549)	-153 (538)
Employment and Earnings—Youth (ages 15 to 19)			
Earnings, 2001 [ADMIN]	173 (315)	277 (216)	401 (336)
Income and Public Assistance			
Total TANF benefits, year 5 [ADMIN]	6 (280)	64 (210)	2 (158)
Ratio of household income to poverty line [SR]	.032 (.036)	.006 (.031)	.034 (.055)
% households food insecure with hunger [SR]	-.046 (.028)	-.004 (.025)	.027 (.024)

EXHIBIT G.4

95 PERCENT CONFIDENCE INTERVALS (TOT ESTIMATES) – OUTCOMES FOR WHICH IMPACTS WERE NOT STATISTICALLY SIGNIFICANT

	MTO Experimental				Section 8			
	TOT	Confidence Interval			TOT	Confidence Interval		
Outcome	Impact (std err)	Lower Bound	Upper Bound	Counter factual	Impact (std err)	Lower Bound	Upper Bound	Counter factual
Adult Health								
General health good or better [SR]	-.033 (.042)	-0.12	0.05	0.70	-.013 (.036)	-0.08	0.06	0.68
Asthma or wheezing attack [SR]	-.031 (.037)	-0.10	0.04	0.21	-.016 (.032)	-0.08	0.05	0.21
Children's Health (ages 12 to 19)								
Gen health very good/excel [SR]	.001 (.054)	-.10	.11	.68	-.010 (.048)	-0.10	0.08	.67
Asthma or wheezing attack [SR]	.065 (.042)	-0.02	0.15	.14	.022 (.036)	-0.05	0.09	.14
Delinquency and Risky Behavior (ages 5 to 19)								
Delinquency index [SR]	.008 (.017)	-0.03	0.04	.08	.012 (.016)	-0.02	0.04	.09
Risky behavior index [SR]	-.019 (.053)	-0.05	0.04	.44	.030 (.047)	-0.06	0.12	.44
Education (children ages 5 to 19)								
Student-teacher ratio, current school [ADMIN]	.51 (.50)	-0.47	1.49	16.17	-.26 (.36)	-0.97	0.45	16.08
WJR broad reading score	2.04 (2.07)	-2.02	6.10	497	.74 (1.73)	-2.65	4.13	497
WJR broad math score	.50 (1.75)	-2.93	3.93	501	-1.74 (1.39)	-4.46	0.98	501

EXHIBIT G.4 (CONT.)

95 PERCENT CONFIDENCE INTERVALS (TOT ESTIMATES) – OUTCOMES FOR WHICH IMPACTS WERE NOT STATISTICALLY SIGNIFICANT

	MTO Experimental				Section 8			
	TOT	Confidence Interval			TOT	Confidence Interval		
Outcome	Impact (std err)	Lower Bound	Upper Bound	Counter factual	Impact (std err)	Lower Bound	Upper Bound	Counter factual
Employment and Earnings—Adults								
Annualized Earnings, years 1 and 2 [ADMIN]	-552 (520)	-1,571	467	4,905	-251 (459)	-1,151	649	4,417
Annualized Earnings, years 3 and 4 [ADMIN]	-360 (658)	-1,650	930	7,159	73 (549)	-1,003	1,149	6,255
Employment and Earnings—Youth (age 15-19)								
Earnings, 2001 [ADMIN]	173 (315)	-444	790	801	277 (216)	-146	700	1,068
Income and Public Assistance								
Total TANF benefits, year 5 [ADMIN]	6 (280)	-473	331	1,472	65 (210)	-239	393	1,238
Ratio of household income to poverty line [SR]	.032 (.036)	-0.04	0.10	.95	.006 (.031)	-0.05	0.07	.87
% households food insecure with hunger [SR]	-.046 (.028)	-0.10	0.01	.13	-.021 (.025)	-0.07	0.03	.11

compared with counterfactuals of .44 for both groups). Similarly the largest likely true impacts on the student-teacher ratio, the broad reading and math scores, and the ratio of household income to the poverty line are all relatively small for both the experimental and Section 8 groups. For the remaining outcomes, however, on the basis of the current estimates, we cannot rule out moderate to large true impacts. This includes two-thirds of the outcomes shown in the exhibit.

In summary, then, the available evidence suggests that the estimates produced by this sample are sufficiently imprecise that we may have missed some impacts that are large enough to be relevant for policy.

How different would the results have been if the families who moved with program vouchers had stayed in low-poverty areas longer?

The vouchers issued to families in the MTO experimental group were only valid in census tracts with poverty rates below 10 percent in 1990, but these families were only required to stay in such areas for 1 year to keep the voucher. Many of these families moved to higher poverty areas after the first year. Many initially moved to areas where the poverty rate rose between 1990 and 2000, so that even if they stayed in their initial location they were not necessarily in a low-poverty area at the time of the interim evaluation. And, of course, families in the Section 8 group were not constrained in where they could use the voucher.

As a result, even those families in the MTO experimental group who moved with program vouchers spent, on average, only about 20 percent of the followup period in areas with poverty levels below 10 percent, and only about 60 percent of the followup period in areas with poverty rates below 20 percent (see exhibit 2.13). At the same time, many families in the control group left public housing and moved to areas with lower poverty rates. On average, control families spent about 11 percent of the followup period in areas with poverty rates below 20 percent (see exhibit 2.13).

These are substantial differences in the proportion of time spent in low-poverty areas, and one might reasonably expect them to result in a number of positive effects for the families who moved with program vouchers. But the MTO demonstration was not a pure test of the effects of living in low-poverty areas versus living in public housing, even for families in the MTO experimental group. We cannot, therefore, infer those effects from these results with any confidence. We can, however, obtain some suggestive evidence on this question by comparing the results for the experimental group and the Section 8 group.

Because these two groups were randomly assigned from the same pool of applicants, they are comparable in all respects except that one was offered locationally restricted vouchers and the other was offered unrestricted vouchers. Therefore, the resulting differences in impacts on the outcomes of these two groups can be interpreted as the result of that difference in the vouchers they were offered. Furthermore, if we believe this difference is captured by the difference in poverty rates in the areas where these two groups lived during the followup period, we can estimate a relationship between impacts and the proportion of time they spent in low-poverty areas that can then be extrapolated to obtain estimates of the effects on a family that spent the entire followup period in low-poverty areas.[7]

An example may clarify the approach. The experimental group as a whole spent 34 percent of the followup period in areas with poverty rates below 20 percent.[8] The Section 8 group spent 24 percent of the followup period in such areas. If the impact on some outcome for the Section 8 group is .8 and the impact on the same outcome for the experimental group is 1.0, a linear extrapolation to a hypothetical group that spent the entire followup period in areas with poverty rates below 20 percent yields an estimated impact of 2.0 (= (1.0 - .8)/(.34 - .24)).

The first two columns of exhibit G.5 show the TOT estimates for the experimental and Section 8 groups. The third column shows extrapolated estimates obtained in this fashion for each of the outcomes for which the TOT impact was statistically significant for either the experimental or Section 8 group.[9] Each of the extrapolated estimates in column 3 represents the impact of spending the entire followup period in areas with poverty rates below 20 percent, based on linear extrapolation of the difference in impacts on the experimental and Section 8 groups and the proportion of time they spent in such areas. As can be seen, these estimates are as much as 8 times as large as the impact on the experimental group, with over half falling into the range of 1 to 5 times as large as the experimental group estimates. Four are smaller than the estimated impacts for the experimental and Section 8 groups. This reflects the fact that the estimated impact on the Section 8 group is larger than that on the experimental group, despite the fact that the Section 8 group spent a smaller proportion of the followup period in low-poverty areas.

This extrapolation makes the very specific assumption that the impacts are proportional to the fraction of time spent in areas with poverty rates below 20 percent. A different assumption about the functional form of this relationship or a different measure of low poverty would yield somewhat different estimates. For example, we might set the poverty threshold at 10 or 15 percent or we might extrapolate based on the mean poverty level of areas in which the family has lived, rather than the proportion of time spent in low-poverty areas.

The fourth column of exhibit G.5 shows the estimates obtained if one assumes the impact is proportional to the square root of the proportion of time spent in areas with poverty rates below 20 percent. With this specification, the extrapolated impacts are generally much smaller, with all but

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7 For this purpose, we define low-poverty as less than 20 percent poor, because neither treatment group spent a large enough proportion of the followup period in areas with poverty rates below 10 percent to support reliable extrapolations.

8 This figure differs from the 60 percent figure cited at the beginning of this section because it applies to the entire experimental group, whereas the 60 percent figure applies only to those families in the experimental group who moved with program vouchers. In this analysis it is necessary to work with poverty rates and impact estimates for the entire treatment group to maintain the comparability of the experimental and Section 8 groups.

9 The fact that one or both of these estimates is statistically significant does not imply that the difference between the two estimates is statistically significant.

Exhibit G.5

Estimated Effects of Spending the Entire Followup Period in Low-Poverty Areas, Extrapolated from TOT Impacts on Experimental and Section 8 Groups, Selected Outcomes

	ITT Impacts		Extrapolated Effect of Spending Entire Followup Period in Low-Poverty Area—Effect Assumed Proportional to:	
Outcome	**Experimental**	**Section 8**	**Proportion of Time Spent in Low-Poverty Areas**	**Square Root of Proportion of Time Spent in Low-Poverty Areas**
Housing and Neighborhood Conditions				
Feel safe at night	0.142	0.093	0.49	0.36
Saw drugs past 30 days	-0.117	-0.103	-0.14	-0.18
Rate housing good/excellent	0.099	0.071	0.28	0.22
Adult Health [SR]				
Obesity [c]	-0.051	-0.047	-0.04	-0.07
Psychological distress index for past month [c, d]	-0.034	-0.012	-0.22	-0.13
Youth Mental Health [SR] (ages 12 to 19)				
Generalized anxiety disorder (all youth)	-0.016	-0.035	0.19	0.07
Generalized anxiety disorder (girls)	-0.042	-0.047	0.05	-0.02

EXHIBIT G.5 (CONT.)

ESTIMATED EFFECTS OF SPENDING THE ENTIRE FOLLOWUP PERIOD IN LOW-POVERTY AREAS, EXTRAPOLATED FROM TOT IMPACTS ON EXPERIMENTAL AND SECTION 8 GROUPS, SELECTED OUTCOMES

	ITT Impacts		Extrapolated Effect of Spending Entire Followup Period in Low-Poverty Area—Effect Assumed Proportional to:	
Outcome	**Experimental**	**Section 8**	**Proportion of Time Spent in Low-Poverty Areas**	**Square Root of Proportion of Time Spent in Low-Poverty Areas**
Delinquency and Risky Behavior (youth ages 15 to 19) [SR]				
Behavior problems (boys self-report)	0.075	0.052	0.23	0.18
Ever arrested for property crime (boys)	0.060	0.059	0.01	0.06
Ever smoke cigarettes (boys)	0.125	0.152	-0.27	0.00
Ever arrested for violent crime (girls)	-0.031	-0.075	0.44	0.17
Ever use marijuana (girls)	-0.129	-0.079	-0.5	-0.35
Ever smoke cigarettes (girls)	-0.085	-0.015	-0.7	-0.40

three being less than four times the impact on the experimental group. Again, in the four cases where the estimated impact on the Section 8 group exceeds the estimated impact on the experimental group, the extrapolated impacts are smaller than the estimates for the two MTO treatment groups. As this suggests, the estimates obtained by extrapolation are very sensitive to the assumed functional form. Nevertheless, this exercise does suggest that the impacts of living continuously in low-poverty areas might be much more substantial than those observed in the demonstration.

Can larger effects be expected in the longer term?

This is an interim evaluation. A final evaluation of the MTO demonstration is planned in roughly 5 years, 9 to 12 years after random assignment. One potential reason why impacts were not observed for some outcomes is that those impacts have not yet had time to develop. If that is the case, we might expect the final evaluation to find more and larger impacts.

The existing literature provides little guidance on this question because few impact studies follow their samples for more than 5 years. The most relevant precedent is the study of the Gatreaux Program by Rosenbaum (1992), discussed in chapter 1. That study found that 1 to 6 years after their families moved to the suburbs, many children "were still struggling to catch up, and it was not clear if they would succeed." But 7 years later Rosenbaum found substantial, statistically significant impacts on eight of nine education- and employment-related outcomes for the same children.

There are fairly strong theoretical reasons why it may take many years for the full effects of neighborhood to manifest themselves. Developmental outcomes like educational performance almost certainly reflect the cumulative experience of the child from an early age. Children who spend their first 10 years in an environment that does not facilitate educational achievement may never fully overcome that disadvantage, even if they then move to an environment that supports educational achievement. The interim evaluation youth sample is composed of children who moved out of public housing at ages 5 to 15. In the final evaluation, the youth sample will have left public housing at ages birth to 10. These youth will have spent a much larger proportion of their formative years outside the concentrated poverty of public housing and may, therefore, show much greater gains in educational achievement and other developmental outcomes.

It is also true that the move from high-poverty areas to lower poverty neighborhoods is likely to be disruptive and require some adjustment period during which positive behavioral effects may not appear and, in fact, negative effects may be observed. There is some evidence of such transitional effects in the negative behavioral effects observed for male youth in the interim evaluation. If these effects indicate that the first 4 to 7 years after random assignment has been an adjustment period for these youth, we may observe different impacts in the longer term once that transition is complete.

We cannot, of course, predict the impacts that will be observed 5 years after our data were collected. We can, however, examine the interim findings for evidence that impacts are related to time since random assignment.

The most direct evidence on this question is provided by the time path of impacts on those outcomes for which we have longitudinal data: the employment, earnings, and public assistance outcomes measured with administrative data. Exhibit G.6 shows impacts in years 1 and 2, years 3 and 4, and year 5 after random assignment for each of the main outcomes measured with these data. (Note that

TANF and food stamp data are only available for the first 4 years after random assignment for Boston, Chicago, and New York. Earnings data are available for 4 years after random assignment in all sites.)

EXHIBIT G.6
IMPACTS ON OUTCOMES MEASURED WITH LONGITUDINAL ADMINISTRATIVE RECORDS, BY TIME SINCE RANDOM ASSIGNMENT

Outcome	TOT Impact, Experimental Group			TOT Impact, Section 8 Group		
	Years 1 and 2	Years 3 and 4	Year 5	Years 1 and 2	Years 3- and 4	Year 5
Adult earnings	-552 (520)	-360 (658)	Na	-251 (459)	73 (549)	na
TANF benefits	485* (201)	184 (191)	2 (107)	249 (217)	74 (206)	37 (121)
Food stamp benefits	768* (306)	532 (362)	253 (214)	329 (235)	460 (268)	334* (158)

*= p<.05 on t-test
na= not available
Source: State administrative records.
Sample: Earnings: all sample adults. TANF and food stamp benefits: all sample adults in Boston, Chicago, and New York.

The earnings impacts show only a very weak upward trend over the four-year period. The estimated impacts on AFDC/TANF, however, show a distinct downward trend over time, especially in the experimental group. The estimated impact for that group starts with a statistically significant increase in welfare dependency in years 1 and 2, falling to an insignificant effect whose point estimate is near zero. The food stamp estimates show a similar downward trend for the experimental group, but no discernible pattern for the Section 8 group. Overall, then, these estimates provide at most modest support for the hypothesis of increasingly favorable (i.e., less unfavorable) effects over time.

For outcomes measured in the survey, we cannot estimate impacts at different points in time after random assignment for a given sample. The best we can do is to compare the impacts for those who were randomly assigned early and who have therefore been exposed to the treatment longer, with those who were randomly assigned later. This comparison must be viewed with caution, however, because there may be compositional differences between these two groups that would cause their impacts to differ, or moving at different times may affect their response to the intervention (e.g., because of differences in the state of the housing market that affected where they moved), independently of duration of exposure to the new environment.

As a check for such differences between the early and late random assignment cohorts, we examined the estimated TOT impacts in year 5 within each of the two cohorts on outcomes measured with administrative data. If the two cohorts are similar, they should show similar impacts in a given year after random assignment. Because some sites started random assignment earlier in calendar time than others, a simple split of the sample on date of random assignment would heavily concentrate sample members from the early sites in one group and those from the later sites in the other. This would present an unacceptably high risk that any differences in impacts between the two groups reflected site effects, not the influence of length of exposure to the treatment. Therefore, we split the sample into early and late random assignment cohorts within each site. This reduces somewhat the difference in length of exposure between the two groups, but results in groups that are balanced on site. Defined this way, the length of time between random assignment and the interim survey was approximately 81 months for the early cohort and 62 months for the later cohort, a difference of 19 months.

As shown in exhibit G.7, there were large differences in the impact estimates between the two cohorts. In the experimental group, the early cohort showed large negative impacts on both TANF and food stamp benefits, while the late cohort showed equally large positive impacts. In the case of food stamps, both estimates were significantly different from zero and from each other. The differences between the two cohorts were less striking in the Section 8 group, but still substantial.

We take these results to indicate that there are important differences that are unrelated to duration of exposure to the treatment between the families who were randomly assigned early in each site and those assigned later in the same site (recall that these cohorts are balanced on site). Therefore, we cannot interpret differences in impacts between the two cohorts as indicative of the effect of length of exposure to the treatment.

EXHIBIT G.7
YEAR 5 TOT IMPACTS ON OUTCOMES MEASURED WITH LONGITUDINAL ADMINISTRATIVE RECORDS, EARLY VS. LATE RANDOM ASSIGNMENT COHORTS

	Experimental		Section 8	
Outcome	**Early Cohort**	**Late Cohort**	**Early Cohort**	**Late Cohort**
TANF benefits, year 5	-484	617	-252	628*
	(251)	(335)	(218)	(236)
Food stamp benefits, year 5	-421*	464*	20	388*
	(173)	(226)	(153)	(178)

*= p<.05 on t-test
Source: State administrative records.
Sample: All sample adults.

There are many reasons why the families assigned early might be systematically different from those assigned later, even within the same site. Most of the sites were affected at some point by HOPE VI, demolition, or revitalization projects. To the extent that the impact of these activities was greater on one cohort than the other, this would cause systematic differences in both the type of families recruited and in the control experience and, therefore, in treatment-control differences between the two cohorts. There were also some shifts in the geographic focus of recruitment in some sites over the course of the intake period. For example, in July 1996 New York began recruitment in the South Bronx among families that tended to be poorer and newer to public housing than those in the projects from which earlier families came. In Boston, later recruitment focused on less distressed developments in the South End. In Chicago, later recruitment tended to be in more distressed projects such as Robert Taylor Homes. Even in those projects that were relatively stable throughout the intake period, it may well be that the first families to volunteer were systematically different from those who volunteered after the demonstration had been ongoing for some time and was more familiar to them.

For all of these reasons, the two cohorts appear to differ substantially in ways other than exposure to the treatment.

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