The Early Impacts of Moving to Opportunity in Boston

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CEPS Working Paper No. 67

October 2000

The authors thank the U.S. Department of Housing and Urban Development, the National Institute on Aging, National Bureau of Economic Research, Harvard University, and the Center for Economic Policy Studies at Princeton University for research support. The authors are grateful to Yvonne Gastelum for collaborating on qualitative interviews in Spanish, Ying Qian for conducting pilot survey interviews and for compiling family contact information, Adriana Mendez for translating the survey into Spanish, Humberto Reynosa for editing the Spanish translation, Beth Welty for processing the administrative earnings and welfare data, and to Patrick Wang, Lorin Obler, and Ali Sherman for excellent research assistance. We thank all of the members of the MTO teams at MBHP, BHA, Abt and Westat for making our research possible, and Carol Luttrel for facilitating our access to Massachusetts administrative data. We have benefitted from conversations with numerous colleagues. We are particularly indebted to Josh Angrist, Deborah Belle, Xavier Briggs, Anne Case, Janet Currie, David Ellwood, Judie Feins, Richard Frank, Edward Glaeser, John Goering, Yvonne Illich, Christopher Jencks, Ron Kessler, Alan Krueger, Edward Lazear, Willard Manning, Larry Orr, Michael Piore, James Rosenbaum, James Rebitzer, Joshua Sharfstein, Marta Tienda, Julie Wilson, and Aaron Yelowitz for their comments and suggestions. Additional information on the MTO demonstration, including research results from all five participating cities, is available at www.mtoresearch.org.

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Abstract

This study focuses on 540 households originally living in public housing in high-poverty areas of Boston who participated in HUD's Moving To Opportunity (MTO) demonstration. Participants were randomly assigned to one of three groups: Experimental – offered mobility counseling and a Section 8 subsidy valid in a 1990 Census tract with a poverty rate of less than 10 percent; Section 8 Comparison – offered a geographically unrestricted Section 8 subsidy; or Control – offered no new assistance, but continued eligibility for public housing. We find that 48 percent of the Experimental group and 62 percent of the Section 8 Comparison group moved through the MTO program. Both groups moved to areas that differ on many dimensions from their origin neighborhoods, having lower poverty rates, higher education levels, and greater employment rates. In a survey covering participants on average two years after program entry, we find that both Experimental and Section 8 Comparison group households experienced increased safety, fewer behavior problems among boys, and improved health among household heads relative to the Control group. The Experimental group also had fewer injuries and criminal victimizations among children. Although employment rates for all participants have increased substantially since 1994, there were no significant impacts of either MTO treatment on the employment or earnings of household heads in Massachusetts administrative earnings data through December 1998.

The results reported in this study cover only the early impacts of MTO at one site. The long-term impacts of changes in residential location facilitated by MTO may not be apparent for some time. The large early improvements observed for the MTO Experimental group in term of mother's mental health and fewer child problem behaviors may be important intermediating factors in long-run child socioeconomic outcomes. But the short-term impacts of MTO are also of independent importance. Many of the hopes of MTO Experimental and Section 8 Comparison families concerning increased safety, reduced stress, and improved environments for their children already appear to have been realized through moves made possible by the demonstration.

Executive Summary

This study presents evidence on the early impacts of the Moving To Opportunity (MTO) demonstration in Boston. Families in eligible public housing projects in high poverty Census tracts applied to MTO and were assigned by lottery to one of three groups. The Experimental group received some counseling assistance and a Section 8 rental subsidy that could be used only to move to a Census tract that had a poverty rate of less than ten percent. The Section 8 Comparison group received a geographically unrestricted rental subsidy. The control group continued to be eligible to remain in public housing and received no new rental assistance or services. Most families in the Boston site were headed by a single mother who was black or Hispanic.

Among households assigned to the Experimental group, 48 percent used an offered subsidy and moved through the program to a new apartment. In the Section 8 Comparison group, 62 percent moved through the program. At the time of our MTO-Boston Follow-up Survey 1-3.5 years after random assignment to an MTO group, the Experimental families were living in neighborhoods that differed from the Control group families on many dimensions, including poverty rates, racial composition, and employment rates. The magnitude of the differences for Section 8 Comparison program movers were substantial but typically not as large. Both the Experimental and Section 8 Comparison groups had on average moved to neighborhoods with less drug dealing and gunfire. Moreover, the families in these two groups were less likely to be victims of property crimes, and children in the Experimental group were less likely to be the victims of personal crimes.

These differences in residential location appear to have produced significant improvements in the problem behaviors of boys, the physical health of boys and girls, and the overall and mental health of household heads, without substantially affecting the social isolation of participating families.

For example, boys are less likely to be cruel to others or to be depressed in both the Experimental and Section 8 Comparison groups than in the Control group. The findings also indicate that child injuries and asthma attacks were reduced in the Experimental group relative to the Control group. Household heads in the Experimental and Section 8 Comparison groups report that they are more calm and peaceful and that their overall health is better than similar adults in the Control group.

The changes in residential location experienced by the Experimental and Section 8 Comparison groups do not appear to have had a systematic impact on welfare receipt or employment. There is some indication that the prevalence of welfare receipt may have decreased by the end of 1998 for the Section 8 Comparison group relative to the Control group, but additional data will need to be collected and analyzed to see if this difference persists over time.

The results from the contrast between the Section 8 Comparison group and the Control group on the outcomes such as safety, child behavior problems, and adult mental health may be directly relevant to the current policy discussions of an incremental increase in the number of Section 8 vouchers, particularly when new vouchers would be made available to households currently receiving project-based assistance--for example, when projects are renovated and the total number of units in the project decreases. The Section 8 Comparison results for Boston suggest possible marked improvements in neighborhood quality and adult health from offering Section 8 subsidies to public housing residents in high-poverty neighborhoods.

The treatment received by the Experimental group, which is a combination of housing mobility counseling and a geographically restricted subsidy, does not correspond precisely to particular policies now under consideration. To the extent that related counseling initiatives, like HUD's Regional Opportunity Counseling, were to more strongly emphasize information and client

visits to low poverty Census tracts, their outcomes may be similar to the Experimental group. In particular, these families may experience lower incidence of criminal victimization among children and lower rates of injuries and asthma attacks than those participating in Section 8 without additional assistance in moving to different types of neighborhoods. The fact that the Experimental group appears to have outcomes at least as good as the Section 8 Comparison group on most dimensions and better outcomes on others including some (like child safety and health) that are explicitly valued highly by participants, despite the fact that individual choices are restricted, does suggest that regular Section 8 participants may not have sufficient information about the full set of opportunities (and potential benefits) available to them. Counselors may be integral to providing such information.

The results reported in this study represent just the beginning of the research program needed to draw strong conclusions about neighborhood effects and housing mobility policies from the MTO experience. This study covers only the early impacts of MTO at one site. Early outcomes such as improvements in mother's mental health or fewer child problem behaviors may be important intermediating factors in long-run child educational and economic outcomes. The demonstration is intended to provide ten years of assistance in the private housing market to families able to move through the program. Only time and further data collection and research will reveal the full extent of long-term impacts of the changes in residential location facilitated by MTO.

But the short-term impacts on adult and child circumstances are of substantial independent importance. Many of the hopes of MTO family members concerning increased safety, reduced stress, and "a better life" for their children do seem to have been realized through the moves made possible by the demonstration. The Moving to Opportunity program has already significantly improved the well-being of families in Boston who were offered rental subsidies.

I. Introduction

Children who grow up in poor neighborhoods fare substantially worse than those who grow up with more affluent neighbors on a wide variety of socioeconomic outcomes. Significant correlations between current well-being and neighborhood poverty remain in studies that control for measures of family income and background characteristics (e.g., Brooks-Gunn, Duncan, Klebanov, and Saland, 1993). This evidence is suggestive of possible causal effects of residential location on children's life prospects operating through neighborhood peer group influences and the impact of neighborhood wealth on access to opportunity (Wilson 1987, 1996; Jencks and Mayer 1990). Such inferences have disturbing implications for the future course of U.S. social problems given increases in the geographic concentration of poverty and in overall residential segregation by family income over the past several decades (Jargowsky 1996, 1997).

But the determination of the causal effects of living in concentrated-poverty neighborhoods on the current well-being and future prospects of low-income families represents an extremely difficult estimation problem.[1] Such effects have typically been estimated by comparing the behavior and socioeconomic outcomes of low-income residents in high-poverty areas with those of other poor families residing in low-poverty neighborhoods. These comparisons potentially confound the effects of neighborhood with the effects of unmeasured characteristics of families who live in different types of residential areas.[2]

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[1] See Duncan, Connell, and Klebanov (1997), Jencks and Mayer (1990), and Manski (1993) for discussions of conceptual issues in the estimation of neighborhood effects.

[2] Recent research has attempted to better control for family background and made some progress on the issue of selectivity of residential location. For example, Aaronson (1998) examines families that relocate and compares siblings that grow up in different neighborhoods. Aaronson finds substantial negative effects of neighborhood (census tract) high school dropout rates on individual dropout rates in sibling difference models. But differences in sibling outcomes could reflect changes in family circumstances driving residential moves rather than true neighborhood effects.

Credible causal estimates of neighborhood influences could be attained if it were possible to randomly assign the families of interest to different types of neighborhoods. Housing mobility programs in which some low-income, inner-city families are given assistance to move to wealthier neighborhoods could approximate such a design if access to such assistance is randomly assigned. The Moving to Opportunity (MTO) demonstration was designed by the U.S. Department of Housing and Urban Development (HUD) to support a direct analysis of neighborhood impacts by employing such an experimental design using random assignment.

The MTO demonstration has been operating in five cities – Baltimore, Boston, Chicago, Los Angeles, and New York – since the fall of 1994. Families are eligible for participation in the demonstration if they have children and reside in public housing or project-based Section 8 assisted housing in a high-poverty area (a census tract with more than 40 percent of all persons living in poverty in 1990). Interested eligible families who completed an application and survey were then selected from a waiting list and randomly assigned to one of three program groups: the Experimental group, the Section 8 Comparison group, and the Control group (Feins 1994).

Families in the Experimental group received a restricted Section 8 certificate or voucher that provides a rent subsidy which can be used to rent housing from private landlords, but only in a low poverty area (a census tract with under a 10 percent poverty rate in 1990). The Experimental group families also received counseling assistance from a local nonprofit organization to help them search for an apartment and adjust to a new neighborhood. Section 8 Comparison group members received a geographically unrestricted Section 8 certificate or voucher and no counseling assistance. The Control group families did not receive rental assistance vouchers or certificates, although their eligibility for continued project-based assistance was unaffected. Hereafter we refer to the Experimental and Section 8 Comparison groups collectively as the "treatment" groups. Members of

both treatment groups were given four to six months (depending on the site) to submit a request for approval of an eligible apartment they would like to lease, and the apartment then had to pass a quality inspection.

In this study, we provide an evaluation of the early impacts on safety, health, employment, and other outcomes of the MTO demonstration at the Boston site. We exploit the random-assignment design of the demonstration to produce these estimates through comparisons of the outcomes of the Experimental and Section 8 Comparison groups with those of the Control group. These differences in outcomes should be considered estimates of the early impacts of the program because they are measured an average of about 2 years after program entry. We combine information from field observations of the program, qualitative interviews with participants, data on Census tract and block group characteristics linked to geocoded initial and current addresses of participants, responses to a survey of 520 MTO Boston participants, and administrative data from the state of Massachusetts on earnings and public assistance receipt.

We begin in Section II with a description of the actual operation of the MTO demonstration in Boston. Section III describes our data and analytical methods. Section IV presents information on the characteristics of the families participating in the demonstration and on their motivations for signing up for a chance to move. Section V focuses on households who moved through the MTO program. Section VI examines overall mobility and characteristics of census tracts in which households reside. In Section VII, we discuss neighborhood safety and criminal victimization. In Section VIII, we explore program impacts on child social behavior and school experiences. We explore the effects on child health in Section IX, and on adult health in Section X. In Section XI, we present results on social relations. Finally in Section XII, both household survey and administrative

data examine the employment, earnings, and public assistance usage of household heads. Section XIII presents our conclusions.

II. The MTO Experiment in Boston

The MTO demonstration program represents an unusual opportunity to identify the causal effects of a housing mobility program on a wide range of outcomes for low-income families. Nonetheless, the interpretation of our findings for Boston requires an understanding of the nature of the specific interventions that occurred at the Boston MTO site.

Each household assigned to the Experimental and Section 8 Comparison groups was issued a Section 8 subsidy that the household could use to help pay the rent for an apartment in the private market, provided that the unit met HUD standards for quality upon inspection. In general, the households retain this subsidy as long as their income is sufficiently low. Both treatment groups received briefings from Boston Housing Authority staff about program rules and about how to look for an apartment at the time of subsidy issuance.

The Experimental group members were also assigned a counselor from a local nonprofit (Metropolitan Boston Housing Partnership, or MBHP). The counselors made home visits to review housing search strategies, explained which neighborhoods the subsidies could be used in (providing a map marked with low-poverty Census tracts), helped program participants clear up bad credit histories, found apartment listings in newspapers, provided references to landlords, and sometimes

drove participants to see promising apartments. Thus, the counselors played a very large role in determining the destination communities of the Experimental group members.[3]

The counselors made at least one home visit in the year after the move to each family that leased up. Counselors sometimes resolved problems that arose for the participants in their new neighborhoods. For example, they helped mediate disputes between the participants and their landlords, and on a few occasions helped families deal with incidents of racial discrimination. In some cases, MBHP provided small grants to households in the Experimental group to purchase furniture or appliances that were needed in the new apartment.

Our estimated impacts of this complex MTO Experimental treatment in Boston may also be affected by aspects of the Boston housing and labor markets during the period of study and by some changes government policies that impacted the participating families. In our qualitative interviews, we found that actual and potential changes to welfare rules and to Section 8 produced substantial anxiety among the MTO population. There was also a major change to Section 8 in the middle of MTO. For the first time, Section 8 landlords were permitted to require security deposits from prospective tenants.[4] There were also major improvements during the study in the safety of some of the developments from which MTO families came.

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[3] It turned out, for example, that one African American counselor believed in moving families as far away from the city of Boston as possible and developed extensive ties to landlords in suburban communities. A second African American counselor tried to discourage his clients from moving to the suburbs immediately south of Boston, and instead urged them to move to northern suburbs. He told his clients that the southern suburbs are "where all of the people you are trying to get away from are moving to." The two Latina counselors were less directive about where families should move and appeared to have stronger ties to closer-in suburbs. Thus, in interpreting differential move rates between Latino and African American participants, it is important to be aware that the Spanish speaking participants were assigned to the Hispanic counselors.

[4] This requirement could be extremely burdensome for a tenant. For example, a security deposit of one month's rent for a tenant whose share of the rent was 10 percent (and HUD's share was 90 percent) would be equivalent to the amount of rent that the tenant would normally pay over 10 months. Conversations with housing counselors indicated that the security deposits were not major obstacles to mobility, but it is possible that some of the drop off in move rates we observe in later cohorts of enrollees was due to the change in security deposit rules.

III. The Data and Methodology

We gathered information about the program using five methods. We conducted field work to observe the operations of the program. We conducted open-ended qualitative interviews with a random sample of a dozen program participants. We conducted a survey of 520 MTO families. We collected survey and administrative records on residential addresses, geocoded them, and merged them with the STF files of the 1990 Census of Population to describe the attributes of the neighborhoods of MTO families. Finally, we obtained administrative data from the state of Massachusetts on the earnings and public assistance receipt of participating families.

Because so much is going on inside the "black box" of the MTO treatments, we have attempted to supplement quantitative comparisons of outcomes by treatment status with qualitative research. Our field work included observing the administration of the baseline survey, attending intake sessions for families after randomization, accompanying counselors on home visits, and interviewing program staff. In our qualitative interviews, we performed 12 ninety-minute open-ended interviews with MTO household heads in the Experimental and Section 8 Comparison groups. The interviews covered the participants' experiences with the program, their perceptions of their old neighborhoods, and, if they moved, their perceptions of their new neighborhoods.[5]

The sampling frame for the data used in this study consisted of household heads randomly assigned in the MTO program in Boston between October 1994 and May 1996. Over these 20 months, 540 families were enrolled; new cohorts were assigned approximately once a month, for an average of 27 families per month. This study uses data from two surveys. First, each household head

[5] The interviews took place in the respondents' homes, and were tape recorded. Interviews in English were jointly conducted by Kling and Liebman. Interviews in Spanish were conducted by Liebman and Yvonne Gastelum, a doctoral student in clinical psychology at Boston University.

completed a survey prior to enrollment in the MTO program, which we will refer to as the "MTO Baseline Survey."

We also conducted our own survey of household heads, which we refer to as the "MTO-Boston Follow-up Survey."[6] The survey focuses on safety, criminal victimization, adult and child health, child social behaviors and school experiences, family social interactions, and the employment and income sources of the household head. The questions were modeled closely after questions in existing national surveys. For example, the employment questions were drawn from the Current Population Survey, and the questions on criminal victimization were drawn from the National Criminal Victimization Survey.

During June and July of 1997, we completed 340 interviews by telephone. Between November 1997 and April 1998, we completed an additional 180 interviews in person, for an overall survey response rate of 96.3 percent.[7] Although MTO continued to enroll families in Boston throughout 1996-97, we limited our sample to families who had up to 120 days to find a new residence and then at least nine months to have lived in the new residence. The monthly enrollments of new families and different survey completion dates combined to yield an average time between random assignment and the Follow-up Survey of 2.2 years, with a range from 1 to 3.5 years.

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[6] The survey was written by our research team and administered in mixed modes (by telephone and in-person) by Westat Inc. The survey was administered in both Spanish and English. The complete survey instrument is available at http://www.wws.princeton.edu/~kling.

[7] We were unable to complete interviews with 20 household heads. In 13 of these cases, we located the household, but were unable to complete an interview because the sample member was deceased, avoided our interviewer, or refused to be interviewed. In 7 cases, we did not locate the household head, although in five of those cases we were in touch with friends or family members of the household head, and might ultimately have been able to locate the household head or other members of the MTO household with additional efforts. While our overall survey response rate is very high, the different dates of survey completion (the lag between our telephone and in-person surveys and the six months we spent tracing the most difficult-to-find families) are not ideal.

Administrative data on earnings and welfare usage for the Boston MTO families were obtained from the Massachusetts Department of Revenue. The earnings data originate in the Department of Revenue Wage Reporting System. All state employers, including those that do not participate in the Unemployment Insurance system, are required to report quarterly earnings to the state. The welfare records originate in the Massachusetts Department of Transitional Assistance. Members of MTO households were matched to their earnings records using Social Security Numbers (SSNs) and names and to the welfare records using their SSNs, names, and dates of birth.

Our basic empirical approach is to use the data from our follow-up survey and from administrative records to compare a wide range of socioeconomic and health outcomes of treatment-group families (the Experimental or Section 8 Comparison groups) to those of the Control group families. All of these groups were originally living in the same set of public housing projects. The random-assignment design of the demonstration means that differential outcomes for the Experimental and Section 8 Comparison groups relative to the Control group (occurring after program entry) can be interpreted as estimates of treatment effects of eligibility for these programs (commonly known as intention-to-treat effects). We use our qualitative research to help interpret our estimates of the early causal impacts of access to these housing mobility programs.

In interpreting the results in this study, it is worth emphasizing that they reflect the overall impact of the program on the entire Experimental and Section 8 treatment groups, including those who did not move through the program. Under the plausible assumption that the program had little or no impact on those not moving with program subsidies, the impact on the program movers within the Experimental and Section 8 Comparison groups are substantially larger than the average differences between groups reported here. In this case, the simple mean differences in outcomes for the Experimental and Control groups should by inflated by a factor of 2.1 to produce the impact on

program movers in the Experimental group (known as the impact of treatment on the treated). The reported estimates should analogously be inflated by 1.6 for the Section 8 Comparison group.[8]

It is also worth emphasizing that our current study is about the early impact of MTO on Boston families. There are many reasons to expect that the initial outcomes may differ from the long-run outcomes. For example, a move from a distressed environment could improve mental health in the long run, but initially increase the probability that a household head is depressed if the move cuts the head off from her previous social networks. Similarly, children could initially have trouble adjusting to new schools, but eventually have fewer behavior problems and perform better in school because of exposure to peers with higher educational aspirations. Many important possible impacts of changes in neighborhoods on the children in MTO families will not be apparent for years.

IV. Characteristics of the MTO-Boston Families

The characteristics at time of program entry of the 540 Boston households that are the focus of our analysis are presented in Table 1. The first three columns show the proportion having each characteristic in the three MTO groups, and the last column reports the overall mean. As shown in the last column of Panel A, the majority of these families are headed by a single mother who received public assistance. 27 percent of the household heads were employed (either part-time or full-time) at the time of the baseline survey, and 22 percent owned an automobile. While the participating families have children and many of these are younger children (63 percent of households have a child between ages 0 and 5), 66 percent of the household heads are at least 30 years of age at the time of

[8] The adjustment factors to convert the simple mean differences of treatment and control groups into estimates of the treatment on the treated are the inverse of the program-move probabilities for each of the treatment groups. Katz, Kling, and Liebman (2001) present a more formal analysis of the derivation of intent-to-treat and treatment-on-the-treated estimates.

random assignment. These patterns are not surprising given that eligibility for the program was restricted to families with children living in public housing (or project-based assisted housing) in high poverty, inner-city census tracts. In fact, the participants at all five MTO sites are largely minority, female-headed households (Goering *et al.* 1999). The Boston site has among the more ethnically diverse group of participants with 45 percent Hispanics and 37 percent blacks.[9]

According to the Baseline Survey at the time of program enrollment, the main reason a majority of Boston families (55 percent) wanted to move is fear of crime ("to get away from drugs and gangs"). Panel B of Table 1 also reports that improvements in housing ("to get a bigger or better apartment") and school quality ("better schools for my children") were the next most important factors motivating moves. Employment concerns ("to get a job" or "to be near my job") were listed as the main reason to move by only 1 percent of all participants.[10] Fear of crime is also the main factor listed as motivating desires to move out of public housing in all four of the other MTO sites (Goering *et al.* 1999).[11]

The concern about crime may also have been the direct result of having been victimized recently. Panel C of Table 1 reports quite high rates of criminal victimization over the six months

[9] The New York and Los Angeles sites contain roughly equal percentages of blacks and Hispanics. In contrast, the Baltimore and Chicago sites are nearly 100 percent black.

[10] Employment opportunities were rarely listed as people's second most important reason for wanting to move either. In total, drugs and gangs were listed as either the first or second most important reason for wanting to move for 75 percent of the sample, getting a bigger and better apartment was listed for 58 percent of the sample. In contrast, better schools for children was the first or second most important reason for only 29 percent, and being near a job or getting a new job was listed as the first or second reason by only 3 percent of household heads.

[11] In contrast to the emphasis on crime as a motivation for wanting to move among current public housing residents in high poverty areas, participants in the Gautreaux housing mobility program in Chicago in the late 1970s (Peroff *et al*. 1979, p. 114) indicated that good schools (34 percent) and quality of housing (26 percent) were more important considerations than crime (23 percent). The increased concern about crime among inner-city public housing residents likely reflects the increase in violent crime rates that occurred in many urban areas in the late 1980s and early 1990s.

prior to the Baseline Survey for Boston families. For example, 14 percent of households had experienced a purse, wallet, or jewelry snatching in the previous six months; 15 percent contained someone who had been threatened by a knife or gun; and 15 percent contained someone who had been beaten or assaulted over the preceding six months. The reported victimization rates of MTO families are about four times higher than those computed from a recent national survey of public housing households in family developments (Zelon *et al*.1994).[12]

Panel D of Table 1 shows that at the time that MTO families applied for the program, a large proportion of the household heads were dissatisfied more generally with their apartments and neighborhoods. For example, 28 percent reported that their apartment was in poor condition, and 51 percent said that it contained too little space. Also, 55 percent reported that they were somewhat of very dissatisfied with their neighborhood, and 74 percent said that drug dealers were a big problem in the neighborhood.

Some characteristics of the origin (baseline) neighborhoods of our Boston sample are presented in the bottom panel of Table 1.[13] As one would expect given the demonstration's eligibility rules, the typical family lived in a census tract with a very high poverty rate (over 41 percent on average) and with approximately one third of the households on public assistance in 1990. The diversity of the racial and ethnic composition of the mean origin census tracts for the Boston families

[12] Note that the victimization rates may be somewhat exaggerated in the Baseline Survey. Despite explicit instructions that the survey was being conducted by outside researchers and that the housing authority would not receive copies of individual responses, our fieldwork revealed that some respondents assumed their answers could influence their acceptance into the program. This may have encouraged them to over-report criminal victimization. The high victimization rates could also be caused by respondents telescoping events that occurred before the time frame of the questions into the six month period.

[13] Neighborhood characteristics are obtained by geocoding the street address, and linking the resulting location to 1990 Census data on areas such as tracts (contiguous geographic areas with an average of 4000 inhabitants) and block groups (subdivisions of tracts). In interpreting these neighborhood characteristics, it is worth remembering that Census tract characteristics may have changed between 1990 and the time at which the MTO families were surveyed, and that Census tracts do not necessarily correspond to the concept of a "neighborhood."

in Table 1 belies substantial racial and ethnic residential segregation faced by the individual families. Over half of the black families in our Boston MTO sample lived in origin census tracts in which over 70 percent of the residents were black and in tracts where under 15 percent of residents were white. The median white (non-Hispanic) family resided in an origin census tract with over 94 percent white residents. The Hispanic (non-black) families lived in the most racially and ethnically diverse origin census tracts.

Under random assignment to one of three groups in MTO, the Baseline Survey characteristics should be the same on average across the groups except for variation due to sampling. Our sampling universe of 540 Boston households consists of 240 in the Experimental group, 120 in the Section 8 Comparison group, and 180 in the Control group. The first three columns of Table 1 contains means for each of the three groups for a wide variety of baseline survey characteristics. The table indicates striking similarities in the baseline characteristics of the three groups which are consistent with a successful application of random assignment.[14] One additional factor to keep in mind when comparing groups is that the randomization proportions were changed after the first 450 households were randomly assigned; all statistics we report in this study are therefore weighted to account for this change.[15]

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[14] In Katz, Kling, and Liebman (2001) we also conducted a variety of statisticals tests indicated that the distribution of Baseline Survey characteristics is quite consistent with random assignment.

[15] The randomization proportions were adjusted after it became apparent that more Experimental families and fewer Section 8 Comparison families were taking-up the offered subsidy than had been projected. For the earlier 450 sample households, the Exp:Sec8:Control random assignment ratio was 225:85:140. For the later 90 households, from March-May 1996, the ratio was 15:35:40. To account for this change, all statistical estimates presented in this study are computed using weights – which allow us to abstract from this change and address the counterfactual question of what our results would look like if the randomization probabilities had remained constant throughout our time period at the overall sample ratio of 240:120:180, or 4:2:3. For example, there are relatively fewer Control households in the earlier period than in the overall sample, so these observations are upweighted by (180/540)/(140/450) $\approx$ 1.07. Without weighting, simple mean differences will not accurately estimate an average causal effect of the MTO program if the average level of any outcome is changing over time within any of the groups.

V. Program Moves in the Boston MTO Demonstration

We next explore the success of the Experimental and Section 8 Comparison groups in Boston at taking advantage of program subsidies to move to private market apartments. Table 2 presents the program move rates (or "take-up" rates) and shares of families using the program to move to the suburbs (out of Boston) for the Experimental and Section 8 Comparison groups and for various sub-groups. Panel A shows that a substantial fraction of families in both treatment groups -- 48 percent of the Experimental group and 62 percent of the Section 8 Comparison group -- were able to successfully move using program housing vouchers (or certificates).[16] 30 percent of the Experimental group as compared to 16 percent of the Section 8 Comparison group used program subsidies to move out of the city of Boston. In analysis not shown in Table 2, we find that the program movers in the Experimental group concentrated in the eligible, low-poverty tracts in Boston (38 percent of the movers) and the suburbs just south of Boston (35 percent); 3 percent of Experimental program movers left Massachusetts.

Panel B of Table 2 shows that families reporting dissatisfaction with their neighborhood in the Baseline Survey were more likely to make a program move. For example, in the Experimental group, there was a 22 percentage point increase in overall program move rates for those who were dissatisfied in comparison to those who were satisfied, and a 23 percentage point increase in program move rates to locations outside the Boston city limits. Similarly in Panel C, the overall program move rate was 29 percentage points higher among Section 8 Comparison group households who reported

[16] Note that the Experimental group take-up rate of 48 percent substantially exceeded the HUD's expectation of 30 percent, while the Section 8 Comparison group take-up rate of 62 percent was lower then the anticipated 80 percent (Feins *et al*. 1994).

in the Baseline Survey that too little space in their apartment was "a big problem." In Panel D, the program move rates (both overall and outside of Boston) for the Experimental group were much higher among the roughly one-third of households who reported in the Baseline Survey that they had lived at one time in a "mostly white" area.

Program move rates were modestly lower for blacks than for non-blacks in the Experimental group, as shown in Panel E. But the black families that did take advantage of the subsidies in the Experimental group were the most likely to move out of Boston. In contrast, Hispanics in the Experimental group were less likely than others to use the program to move to the suburbs. In fact, the program move rate to the suburbs for Hispanics was actually lower in the Experimental group with restricted vouchers than in the Section 8 Comparison group. These differences may reflect racial attitudes in the neighborhoods of Boston eligible for MTO moves and the distribution of the Hispanic-speaking persons in the Boston area, as well as differences in the emphasis of the MTO counselors handling black and Hispanic participants at the Boston MTO site.

In results not shown in the table, we also analyzed the differences between initial program moves and locations at the time of the MTO-Boston Follow-up Survey (1-3.5 years later). Among all those who moved through the MTO program, we find that about 26 percent had moved at least once more by the time of the Follow-up Survey.[17] Similarly, about 27 percent of those in all 3 MTO groups who did not move through the MTO program had also moved from their Baseline location by the time of the Follow-up Survey. Although these overall mobility rates are quite similar, the

[17] In our qualitative interviews, a number of families explained that they took one of the first apartments they were shown in order to be assured of leasing-up within the time limit necessary to obtain the Section 8 certificate or voucher. Once they had the Section 8 subsidy, they were able to look at a more leisurely pace for another apartment, and often found a better apartment after talking to people in their new neighborhoods.

experiences of Experimental group families who moved through the MTO program do appear to have differed somewhat from other groups.

Nearly all Experimental program movers initially moved to Census tracts with less than a 10 percent poverty rate, as required by program rules, but these families were allowed to move again after one year without a restriction on the Census tract characteristics of their next location. 85 of the Experimental program movers had not moved again from their program move location by the time of the Follow-up Survey, and 32 Experimental program movers did move again. 22 of these 32 families (or equivalently, 19 percent of all Experimental program movers) were located in a Census tract at the time of the Follow-up Survey with a poverty rate at least ten percentage points higher than the tract to which they had made their initial program move. Notably, however, these changes were not initial moves to the suburbs followed by a return to high poverty neighborhoods. Only 11 of the 22 had initially moved out of Boston, and only 6 of the remaining 11 chose to move from outside of Boston back into Boston. None of the 22 moved to a Census tract with a poverty rate of 40 percent or higher. Among other groups, only one of the 16 Section 8 Comparison program movers and three of the 92 non-program movers who moved again by the Follow-up Survey chose to increase their tract poverty rate by more than ten percentage points.

VI. Mobility Outcomes and Neighborhood Characteristics

We next turn to an analysis of the impacts of the Experimental and Section 8 Comparison treatments on the overall residential mobility rates and neighborhood attributes of the Boston MTO families. The MTO program had a substantial impact on the residential location of households offered subsidies to relocate to private apartments. The top part of Table 3 summarizes the residential mobility outcomes for the treatment and control group families at the time of our MTO-Boston

Follow-Up Survey.[18] In this table and in all the tables that follow, the results are presented in a different format than in the earlier tables. The Control group mean is presented in the first column of numbers, while the next two columns show the *difference* in means between the Experimental group and the Control group, and between the Section 8 Comparison group and the Control group. Therefore the mean for the Experimental group can be obtained from the table by adding the Exp-Control difference to the Control group mean, and the mean for the Section 8 Comparison group can be obtained by adding the Sec8-Control difference to the Control group mean.

The Experimental and Section 8 Comparison treatments both greatly increased the rate at which families moved out of their original housing projects. During the 1-3.5 years that elapsed by the time of the Follow-up Survey, a substantial share (27 percent) of Control households had moved out of the housing project or other Census block group in which they were living at the time of the Baseline Survey. Among the Experimental group, 60 percent had moved out of their original location (48 percent through MTO and 12 percent independently). Among the Section 8 Comparison group, a total share of 69 percent had moved (62 percent through MTO and 7 percent independently). Further analyzing the locations of households at the time of the Follow-up Survey, we find overall that few Control (5 percent) and Section 8 Comparison (12 percent) households were living outside the city of Boston, while the Experimental group (28 percent) households were much more likely to reside in outside the city limits.[19]

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[18] We were able to obtain accurate geocoded information on the current residential locations of 525 of the 540 target families at the time of our MTO-Boston Follow-Up Survey: 235 Experimental, 114 from the Section 8 Comparison group, and 176 Controls. The first two rows of Table 2 indicate that the program move rates for both the Experimental and Section 8 Comparison groups are almost identical for the full sampling universe and the geocoded sub-sample.

[19] Nearly all of those outside the city limits were living in Boston's surrounding communities, although there were seven Experimental and two Control households who moved to other states. In addition, there were three Section 8 Comparison households living in Puerto Rico with whom we completed interviews but from whom were unable to obtain street addresses that could be matched to Census tracts; these household are therefore not included in Table 3.

To assess the impact of MTO on neighborhood attributes, we compare the mean neighborhood characteristics (based on 1990 Census tract data) at the time of the Follow-Up Survey of households in the two treatment groups with those of the Controls. The lower panel of Table 3 shows that the areas in which Experimental and Section 8 Comparison households were living at the time of the Follow-up Survey were significantly different on average from the Control households across many dimensions. The treatment groups resided in Census Tracts with lower poverty rates, lower welfare receipt, a lower prevalence of female-headed households, a higher fraction of full-time/full-year workers, a higher proportion of managerial and professional workers, higher education levels, and a higher share of owner-occupied units. Notably, the Section 8 Comparison group did not significantly differ from the Control group in the racial composition of the Census tracts or in the primacy of the English language. Results (not shown in the table) are similar for Census block group comparisons.

The fact that the differences in the average tract characteristics for the Experimental and Section 8 Comparison groups versus Controls are similar on many dimensions does not fully convey the differences in the underlying distribution of the tract characteristics. For example, the Experimental - Control difference in the average poverty rate was 12 percentage points, and the Section 8 Comparison - Control difference was 10 percentage points. However, the Experimental group members were substantially more likely to end up in tracts with very low poverty rates than the Section 8 Comparison and Control groups. This is to be expected given the initial restriction on MTO rental assistance for the Experimental group to units in census tracts with a poverty rate of no more than 10 percent. The overall share of the Experimental group living in low-poverty (under 10 percent) census tracts was more than 23 percentage points greater at the time of the follow-up survey than for the Section 8 Comparison group. In contrast, the Section 8 Comparison group (with its

higher take-up rate) has a larger fraction of families moving out of census tracts with 40 percent or greater poverty rates.

VII. Safety and Criminal Victimization

The previous section documents that the residential environments of those who moved through the MTO program changed on many dimensions. Our qualitative interviews suggested to us that the neighborhood characteristics most salient to the participating families are those affecting the exposure to violence and overall safety of their children. Furthermore, in the Baseline Survey, the main reason that 56 percent of MTO families reported that they wanted to move was "drugs and gangs." We investigated the extent to which perceptions of neighborhood safety changed for those afforded the chance to move through the MTO program by asking a variety of questions in the MTO-Boston Follow-up Survey.

In Table 1, we found that 48 percent of household heads in the Baseline Survey reported feeling unsafe or very unsafe on the streets near home during the day. This level declined to 39 percent in the Control group in the Follow-up Survey, as displayed in the first row of Table 4. There was a further decline of 16 percentage points for the Experimental group, which is statistically significant, and also an estimate of a modest (but not statistically significant) decline for the Section 8 Comparison group. In an attempt to assess the specific issues of drugs and guns that were highlighted as major concerns in our qualitative fieldwork, we asked several additional questions. In Table 4, we find that both the Experimental and Section 8 Comparison groups reported substantially lower prevalence of seeing people using or selling drugs, and seeing or hearing gunfire.

Since a shocking 37 percent of the Boston MTO households reported having experienced some criminal victimization (threat, break-in, purse snatching, assault, stabbing, or shooting) in the

six months prior to the Baseline Survey, we asked a sequence of detailed questions about victimization incidents in the Follow-up Survey to assess the extent of any changes.[20]

The results show that only 26 percent of households in the Control group reported that at least one crime incident occurred in the six months prior to the Follow-up Survey, as shown in the first row of Table 5. The reductions in victimization rates and improvements in neighborhood safety from the time of the Baseline Survey to that of the Follow-Up Survey may be due to the well-documented sharp decline in crime rates in Boston over this period (Piehl *et al.* 1999).[21] Victimization was 12 percentage points lower in both the Experimental and Section 8 Comparison groups, indicating highly significant declines.

In the Control group, 13 percent reported a property crime. The prevalence of property crimes declined by 6 percentage points in the Experimental group and 9 percentage points in the Section 8 Comparison group, with the statistical significance marginal for the former and strong for the latter. There was also a marginally statistically significant decline in the prevalence of personal crimes involving children for the experimental group and a similar decline among household heads for the Section 8 Comparison group. In analyses not reported in this study, we find the same pattern of results by crime type when the outcome is average number of incidents instead of probability of at least one incident.

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[20] Our questions were modeled on the National Criminal Victimization Survey, and designed to evoke recollections of incidents involving the household head or a child that occurred in the prior six months. In Table 5, we report results for personal crimes (assault, robbery, and pick pocketing) and property crimes (theft, and household or motor vehicle burglary), with classifications based on descriptions of the incidents.

[21] Different reporting behavior by respondents in the Baseline and Follow-Up surveys may also have played a role, since the Follow-up Survey did not appear to be viewed be respondents as having the potential to influence their chance of selection for Section 8 in the way that Baseline Survey appears to have been. The wording of the questions also differed in the Follow-up Survey, although the newer questions (Bureau of Justice Statistics, 1994) have generally been shown to increase reporting of incidents.

Overall, both the Experimental and Section 8 Comparison group household heads found their neighborhoods to be less dangerous and reported substantially fewer criminal victimizations than the Controls, similar to the findings for Los Angeles MTO site (Hanratty, McLanahan, and Pettit 1998). The families who moved through the MTO program, especially those in the Experimental group, did succeed in accomplishing one of their main goals of relocating to substantially safer neighborhoods.

VIII. Children's Social Behavior and School Experiences

The experience of previous housing mobility programs, such as the Gautreaux program in Chicago (Rosenbaum 1995), is suggestive of the potential impact that a change in residential location may have on developmental processes that may ultimately affect outcomes later in life. In order to assess the early impact of MTO on children's social behavior and school experiences, we asked household heads a number of questions about up to two randomly selected children per household in the MTO-Boston Follow-up Survey. The results presented in Table 6 and discussed below pool data on children ages 6 to 15.

Researchers in clinical medicine have suggested that living in a violent, stressful environment may lead children to exhibit various negative behaviors (Augustyn 1995). To assess such behaviors, we asked selected questions in the MTO-Boston Follow-up Survey that were drawn from items in the National Health Interview Survey Child Supplement and the National Longitudinal Survey of Youth Child Supplement. Our selections focused largely on questions that asked about observable "external" behaviors, rather than "internal" feelings of children that would be more difficult for the household head to judge.

In general, boys exhibit substantially more behavior problems than girls. For example, our results for the Control group in the first column of Panel A of Table 6 show that boys have higher

prevalence of all 7 behavior problems we measured, though the boy-girl difference for "unhappy, sad, or depressed" is small and statistically insignificant. When we look at the improvements in behavior problems for the MTO treatment groups, we find larger improvements for boys than for girls for 5 out of 7 behavior problems in the Section 8 Comparison group, and 7 out of 7 problems in the Experimental group.

Specifically, we found statistically significant reductions in the incidence of boys being "cruel or mean to others" and being "sad, unhappy, or depressed" in both the Experimental and Section 8 Comparison groups relative to the Control group. Declines were also found for "trouble getting along with teachers" and for "disobedient at home" that were marginally statistically significant in the Experimental group, and also negative for the Section 8 Comparison group. For the remaining three behaviors, "disobedient at school", "hangs around with kids who get into trouble", and "restless or overly active", the sign of the estimates indicated a reduction in problems for both treatment groups, although the differences were not statistically significantly different from zero.

For girls in the Section 8 Comparison group, the reduction in disobedience at school was marginally statistically significant, and the point estimates were negative for 5 of the 7 problems. In the Experimental group, only 4 of the 7 estimated effects for girls were negative and none were significant. Our findings are consistent with those of the recent New Hope experiment in Wisconsin (providing earnings supplements, health insurance, and child subsidies to low income families), which found significant improvements in child behaviors among boys and not girls (Bos *et al.* 1999).

One reason for the differential effect between boys and girls is that girls appear to have had more difficulty socially integrating into their new neighborhoods. Results shown in panel B of Table 6 indicate that girls in both the Experimental and Section 8 Comparison groups are significantly less likely to have at least one close friend in the neighborhood, whereas boys in both groups are actually

more likely to have at least one close friend in the neighborhood. Girls in the Experimental group are also less likely to participate in extra-curricular activities after school, although we note that this difference is only marginally statistically significant, and the contrast with boys is not as large as in the analysis of neighborhood friends.

IX. Children's Health

MTO families are greatly concerned with the many types of danger facing children living in a public housing project. We were told in our qualitative interviews not only about criminal victimizations but also about injuries from broken glass in nearby courtyards and falls on concrete in local playgrounds. A burgeoning medical literature also shows that living in an inner-city is associated with higher rates of accidents, injuries, and asthma for children (Sharfstein *et al.*, 1998; Quinlan, 1996; Sarpong, 1996). Our fieldwork suggested the families who moved through the MTO program may have safer places for their children to play and less exposure to the high-stress environments and housing conditions that may trigger asthma attacks.

In the MTO-Boston Follow-up Survey, we asked household heads about injuries and asthma attacks during the past six months for up to two randomly selected children. Table 7 reports results for children ages 6-15. In analyzing injuries, we focused on non-sports injuries, which turned out to primarily come from falls, fights, or dangerous external factors such as broken glass or needles.[22] For the Experimental group, the proportion with injuries was cut in half, more than 4 percentage points lower than the injury rate of 8 percent in the Control group. The results reported here are marginally

[22] We had hypothesized that sports injuries may have increased for families who moved through the MTO program, since the children were potentially more likely to spend recreational time playing sports in safer neighborhoods. In fact, it turns out that only about 2 percent of children experienced sports injuries requiring medical attention, and the frequency of sports injuries does not appear to differ substantially among MTO groups.

statistically significant; in other work we incorporate additional information about the children to increase statistical precision, and the results for injuries become strongly statistically significant.[23]

For asthma attacks, we had suspected that known asthma triggers, such as cockroach allergens and dust mites in carpets (Gelber *et al.*, 1993) may have been less prevalent in the housing into which families may have moved through the MTO program. The results reported in Table 7 suggest that the prevalence of asthma attacks may have been reduced in the Experimental group by a substantively important magnitude, but the estimate is not statistically significant. As with injuries, we have done further analyses (Katz, Kling, and Liebman 2001) that add additional covariates in an attempt to improve estimation precision, and these results suggest that the difference between the Experimental and Control groups is marginally statistically significant.

X. Adult Health

Some of the most striking results from the MTO-Boston Follow-up Survey are based on responses to questions posed to household heads about self-reported health status. Our fieldwork had suggested to us that the reduction in anxiety from moving to a neighborhood with fewer guns, drug dealers, and violent behavior had the potential to be one of the most salient changes in the lives of adults in families who moved through the MTO program. As discussed in Section VII, the Experimental and Section 8 Comparison groups do appear to have moved to neighborhoods that were safer and less violent. On the other hand, movers may be socially isolated in their new neighborhoods

[23] In Katz, Kling, and Liebman (2001), we estimate linear probability models of treatment effects, include variables from the Baseline Survey to reduce residual variation and improve efficiency of estimation. We also present evidence that these results on children's health outcomes are not being spuriously driven by changes in access to medical care.

and become unhappy. To assess the impact of these changes on health, we used several questions in the Follow-up Survey.[24]

Regarding overall health, 58 percent of the Control group responded that their health was good or better. The fraction in the Experimental group was 11 percentage points higher, and 18 percentage points higher in the Section 8 Comparison group. Given the very large magnitude of the differences, the results are unsurprisingly highly statistically significant.

Our fieldwork suggested that impacts on overall health of MTO in the short run were more likely to be through mental health than physical health. We cannot rule out changes in physical health, but strong increases in calmness and peacefulness do suggest that at least part of the large impact on general health occurred through changes in mental health and positive affect. In the Control group, 47 percent responded that they were calm and peaceful a good bit of the time or more often. The fraction was 10 percentage points higher in the Experimental group, and 14 percentage points higher in the Section 8 Comparison group. Again, the results are statistically significant and of substantively large magnitude. The results also indicate that members of the two treatment groups were happier, but these differences are smaller in magnitude and not statistically significant.

[24] These questions that were originally developed for analysis of the Rand Health Insurance Experiment (Manning *et al.* 1987) and are now commonly used in the SF-36 Health Survey (Ware *et al.* 1994). First, we asked: "In general, would you say that your health is excellent, very good, good, fair, or poor?" Second, we asked: "How much of the time during the past four weeks have you felt calm and peaceful -- all of the time, most of the time, a good bit of the time, some of the time, a little of the time, or none of the time?" Third, we asked: "How much of the time have you been a happy person?" with the same response choices.

XI. Social Relations

One of the potential drawbacks to living in a new neighborhood could be the disruption of established social ties, potentially leading to social isolation of the mover. As we have seen in the previous section, there does not appear to have been a negative effect on mental health in the MTO treatment groups. In this section, we present direct evidence that social contact itself does not appear to be appreciably lower either. Overall, we find remarkably little evidence that households in the Experimental and Section 8 Comparison groups were more socially isolated than the Control group.

In Table 9, we show that household heads in the Experimental and Section 8 Comparison groups are less likely than those in the Control group to report having had a friend to their homes in the past week, but more likely to report having visited a friend or relative at their homes. For neither outcome are these differences statistically significant between any of the groups. Similarly, the treatment group household heads talk even more frequently by telephone with close friends and relatives than Control group heads, although this difference too is insignificant. We also find that virtually the same fraction, 57 percent, of the Experimental and Control groups attended church at least once in the past thirty days. The fraction in the Section 8 Comparison group is lower, but the difference is statistically insignificant.

As one final measure of social relations we asked about social trust, which has been shown to be correlated with membership in local organizations and other measures of civic engagement (Putnam 1995). We hypothesized that people living in neighborhoods where they were more likely to be a racial or linguistic minority may exhibit lower social trust. The question itself, taken from the General Social Survey, is: "Which of the following do you agree with – 'Most people can be trusted' or 'You can't be too careful in dealing with people'?" The fraction in the Control group who feel most people can be trusted is only 8 percent, but somewhat surprisingly, the Experimental group

fraction was 6.5 percentage points higher, a marginally statistically significant difference. The difference for the Section 8 Comparison group was also positive, but not statistically significant.

XII. Welfare and Employment

The decline of the inner-city labor market has been well documented (see, for example, Wilson 1996). A move through MTO may increase the accessibility of employment and introduce different neighborhood social expectations about work and welfare. Alternatively, a move may disrupt the informal networks through which people find jobs, particularly in the short run before new social networks can be established. The most directly relevant previous research is probably the initial short-term study of the Gautreaux housing mobility program, which found no significant employment effects after about 1 year after placement in a suburban location relative to placement in a central city location (Peroff *et al*. 1979). In a later study of a sample of Gautreaux families about 5 years after their initial move, Rosenbaum (1995) finds significantly higher employment among household heads who had been placed in suburban areas in comparison to city placements. The response rate for the early study was 81% versus 67% for the later study, which may have affected the results if employed movers were less likely to move over time and therefore easier to locate and survey. Although the later study may have been differentially biased by sample attrition, these two sets of results also may suggest that there are differences between short-run and long-run effects.

From 1994 to 1998, there were striking changes in the levels of welfare receipt and employment for the entire MTO-Boston sample. Over this period, public assistance receipt fell by almost one-half, and employment increased by more than one-half. We have several sources of information about these outcomes. First, we can use the MTO-Boston Baseline Survey (administered between October 1994 and May 1996), which reflects the status of families as they entered the MTO

program, and the MTO-Boston Follow-up Survey, which was administered between June 1997 and April 1998 (Results from the Follow-up Survey are shown in Panel A of Table 10). Second, we can use administrative data from the state of Massachusetts, including records on welfare usage (AFDC/TANF) from the Department of Transitional Assistance, and quarterly earnings data from the Department of Revenue. Results from these administrative data are shown in Panel B of Table 10. For the administrative data, the time periods are expressed as calendar quarters, where the third quarter of 1994 is denoted as 94:3, etc.

At the time of the Baseline Survey, 64 percent of households reported receiving welfare (see Panel A of Table 1). In the Follow-up survey, only 47 percent of Control households were receiving welfare. Similarly, 73 percent of the MTO sample were receiving welfare in 94:3 according to administrative records, and this level had decreased to 51 percent by 97:3 among Controls. As shown in panels A and B of Table 10, the differences among the three MTO groups are not statistically significant for either the Follow-up Survey or for the 97:3 administrative records. One important reason for this overall decline was undoubtedly the changes in welfare eligibility during this time period. In December 1996, Massachusetts implemented time limits on benefits, such that approximately one-third of the statewide caseload was restricted to 24 months of assistance in a 60 month cycle.[25] Indeed, 76 percent of those receiving welfare benefits at the time of the Follow-up Survey acknowledged that they had been notified that they could only receive their benefits for a certain number of months. The strong economy, the expansion of the Earned Income Tax Credit, and increases in parental work associated with children entering school are likely to have played a role as well.

[25] For details see http://www.magnet.state.ma.us/dta/dtatoday/reform/WelfareReform - Chapter5.htm

The level of welfare receipt appears to have continued to decline over time. In 98:3, welfare receipt in the Control group was only 40 percent, statistically indistinguishable from the Experimental and Section 8 Comparison groups. Out-of-state moves are unlikely to have had much affect on our results from the administrative data. Of the 540 members of our sample, we confirmed that all but 23 were still living in Massachusetts (and of these, only 12 were confirmed to be living outside of Massachusetts and 4 of these reported receiving welfare in the Follow-up Survey).

Regarding other types of public assistance, Food Stamps were received by 68 percent of households in the Baseline Survey, and 52 percent in the Follow-up Survey, among Controls, with no distinguishable differences between groups. Supplemental Security Income (SSI) was received by 17 percent of households in the Baseline Survey, and by 25 percent of all MTO households in the Follow-up Survey. This change over time in SSI receipt is statistically significant, and may indicate some substitution of SSI benefits for welfare benefits over time among this population, as welfare eligibility became more restrictive.

In addition to questions about public assistance, we also asked about employment in the Follow-up Survey. We found that employment increased from 27 percent in the Baseline Survey (as reported in Table 1) to 43 percent in the Follow-up Survey for Controls. These results correspond to those from the data obtained from tax records, in which 29 percent had reported earnings during the 94:3, and 44 percent of Controls had reported earnings in 97:3. As shown in Table 10, the differences among the three MTO groups are statistically insignificant for employment. Moreover, the moderately large point estimate of negative 7 percent for the Experimental-Control difference appears unlikely to be indicative of a systematic difference, both because it is not statistically significant and because the point estimates for this difference based on administrative data are not nearly so large for 97:3 or 98:3, as shown in Table 10, or for any other quarter in between. We also

found no significant differences between the groups in participation in training or job search assistance since the time of random assignment.

As with the analysis of welfare, the analysis of Massachusetts tax records may be influenced by families who have moved out of state. Yet, even the most extreme assumptions about differential employment between MTO groups among families not confirmed to be living in Massachusetts would not generate differences between MTO groups greater than the sampling error on the estimates on employment differences. Therefore, use of state administrative data is extremely unlikely to be driving these results.

To attempt to assess the quality of the jobs at which MTO households were working, the Follow-up Survey asked about various aspects of their employment situation. Again, we found no statistically distinguishable differences between the three MTO groups. We found that only 15 percent of Control households worked in jobs in which health insurance or other fringe benefits were provided. Average wages among Controls working were $8.46 per hour. From the administrative data, we can compute total earnings in each calendar quarter. The administrative data on earnings appear to roughly agree with the implied usual earnings (based on wages and hours) for Controls, in that the Follow-up Survey quarterly earnings were $1455 and the tax data earnings were $1572 for 97:3. In results not reported in the table for the administrative data, average earnings were $921 in the fourth quarter of 1994 and $1838 by 98:3 (adjusted for inflation to 1998 dollars) for all three MTO groups. Given that the official poverty threshold for a family of three in 1998 was about $3400 per quarter (based on a $13,650 annual threshold), these average earnings levels are still quite low, despite the strong increase in labor earnings over time.

In sum, while welfare receipt declined substantially and employment rose over time for all three MTO groups, the differences among the groups were much less dramatic. We found some

suggestive evidence that welfare receipt may have decreased among the Section 8 Comparison group by the end of 1998. We also found no solid evidence of meaningful differences between MTO groups in employment or earnings for the Boston site, which is consistent with evidence on employment for the Baltimore MTO site (Ludwig, Duncan, and Pinkston 2000) and the short-term Gautreaux experience. The possibility that Experimental or Section 8 Comparison group members will increase their employment rates relative to the Control group in the longer run remains an open question for further research.

XIII. Conclusion

In this study, we have presented evidence on the early impacts of the Moving To Opportunity Demonstration in Boston. Among households assigned to the Experimental group, 48 percent used an offered subsidy and moved through the program to a new apartment. In the Section 8 Comparison group, 62 percent moved through the program. At the time of our MTO-Boston Follow-up Survey 1-3.5 years after random assignment to an MTO group, the Experimental families were living in neighborhoods that differed from the Control group families on many dimensions, including poverty rates, racial composition, and employment rates. The magnitude of the differences for Section 8 Comparison program movers were substantial but typically not as large. Both the Experimental and Section 8 Comparison groups had on average moved to neighborhoods with less drug dealing and less gunfire. Moreover, the families in these two groups were less likely to be victims of property crimes, and children in the Experimental group were less likely to be the victims of personal crimes.

These differences in residential location appear to have had significant beneficial influences on the social behavior of boys, the physical health of boys and girls, and the overall and mental health of household heads. For example, we found that boys are less likely to be cruel to others or to be

depressed in both the Experimental and Section 8 Comparison groups than in the Control group. We also present evidence here (and stronger evidence in Katz, Kling, and Liebman 2001) that the prevalence of injuries and asthma attacks were reduced in the Experimental group relative to the Control group. Household heads in the Experimental and Section 8 Comparison groups report that they are more calm and peaceful and that their overall health is better than similar adults in the Control group. In principle, program movers could have been more socially isolated, but there does not appear to be noticeably less social interaction with friends or relatives in the Experimental or Section 8 Comparison group families than in the Control group.

The changes in residential location experienced by the Experimental and Section 8 Comparison groups do not appear to have had a systematic impact on welfare receipt or employment in either the Follow-up Survey or in Massachusetts administrative records. There is some indication that the prevalence of welfare receipt may have decreased by the end of 1998 for the Section 8 Comparison group relative to the Control group, but additional data will need to be collected and analyzed to see if this difference persists over time.

The results from the contrast between the Section 8 Comparison group and the Control group on the outcomes such as safety, child behavior problems, and adult mental health are of potential relevance to the current policy discussions of an incremental increase in the number of Section 8 vouchers, particularly when new vouchers would be made available to households currently receiving project-based assistance--for example, when projects are renovated and the total number of units in the project decreases. The Section 8 Comparison results for Boston are suggestive of possible marked improvements in neighborhood quality and adult health from offering Section 8 subsidies to public housing residents in high-poverty neighborhoods.

The treatment received by the Experimental group, which is a combination of housing mobility counseling and a geographically restricted subsidy, does not correspond precisely to particular policies now under consideration. To the extent that related counseling initiatives, like HUD's Regional Opportunity Counseling, were to more strongly emphasize information and client visits to low poverty Census tracts, their outcomes may be similar to the Experimental group. In particular, these families may experience lower incidence of criminal victimization among children and lower rates of injuries and asthma attacks than those participating in Section without additional assistance in moving to different types of neighborhoods.[26] The fact that the Experimental group appears to have outcomes at least as good as the Section 8 Comparison group on most dimensions and better outcomes on others including some (like child safety and health) that are explicitly valued highly by participants, despite the fact that individual choices are restricted, does suggest that regular Section 8 participants may not have sufficient information about the full set of opportunities (and potential benefits) available to them, and that counselors may be integral to providing such information.

Another factor to consider when assessing the policy relevance of the results from the MTO experience is the scale of the potential policy under consideration. MTO is a relatively small program, and the lessons from it are most directly applicable to other incremental programs, such as

[26] The mechanisms that are the source of these differences between the Experimental and Section 8 Comparison groups remains a subject for further research. On one hand, it may be that families need to move to neighborhoods that are much different, rather than the more moderate differences experienced by the Section 8 Comparison group. Alternatively, it is possible that the results for the two groups differ because the composition of families who moved through the MTO program in the two groups is not the same. For instance, there may be some families who moved through MTO when assigned to the Section 8 Comparison group, but would not have moved if they had been assigned to the Experimental group. If the injury rates of children in these particular families were only minimally affected by the move, then such families could be driving the difference between the Experimental and Section 8 Comparison results by lowering the estimated average effect for the Section 8 Comparison group. Under either of these alternatives, however, a counseling program that resulted in more placements in low poverty Census tracts and resulted in a lower probability of actually moving through the program could have the potential to emulate the outcomes of the Experimental group in the MTO program.

adding several hundred Section 8 vouchers in various cities. The families who move to new neighborhoods through the MTO program are too few in number, for the most part, to substantially change the character of the new neighborhoods. A large scale program, such as the complete elimination of all public housing projects and the issuance of vouchers to all former tenants, may have different effects than a smaller scale program.

The results reported in this study represent just the beginning of the research program needed to draw strong conclusions about the nature of neighborhood effects and the efficacy of housing mobility policies from the MTO experience. We have been able to analyze only the early impacts of MTO at one site. In particular, we have no information on the impact of the moves on very young children. Since the youngest children will likely have the longest exposure to the new neighborhoods, they may eventually show the strongest results. Nonetheless, early outcomes such as improvements in mother's mental health or fewer child problem behaviors are promising, since they may be important intermediating factors in eventual long-run child educational and economic outcomes. The demonstration is intended to provide ten years of assistance in the private housing market to families able to move through the program. Only time and further data collection and research will reveal the full extent of long-term impacts of the substantial initial changes in residential location facilitated by MTO.

Although the eventual long-term effects of MTO on the participating families are a crucial issue for future research, we do believe that the short-term impacts on adult and child circumstances are of substantial independent importance. Many of the hopes of MTO family members concerning increased safety, reduced stress, and "a better life" for their children do seem to have been realized through the moves made possible by the demonstration. The Moving to Opportunity program has already significantly improved the well-being of families in Boston who were offered rental subsidies.

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TABLE 1 – MTO-Boston Descriptive Statistics from Baseline Survey
for Households Enrolled through May 1996

	Exp	Sec8	Control	All
A. Household head characteristics				
Age < 30	.33	.37	.35	.34
Female	.92	.92	.89	.91
Black	.36	.40	.35	.37
Hispanic	.44	.39	.48	.45
Never Married	.55	.63	.60	.59
High school graduate	.45	.44	.40	.43
Employed	.25	.26	.28	.27
Receiving AFDC	.62	.68	.64	.64
Car that runs	.25	.19	.19	.22
Any Children, 0-5 years	.61	.63	.64	.63
Any Children, 6-17 years	.81	.78	.76	.79
B. Most important reason wanted to move				
Drugs and gangs	.62	.47	.53	.56
Bigger and better apartment	.27	.31	.31	.29
Better schools for children	.06	.13	.07	.08
To be near job or to get job	.00	.03	.01	.01
C. Recent criminal victimization				
Purse, wallet, jewelry snatched in past six months	.12	.13	.16	.14
Threatened by knife or gun in past six months	.11	.20	.17	.15
Beaten or assaulted in past six months	.13	.20	.16	.15
Stabbed or shot in past six months	.06	.06	.09	.07
Attempted break-in in past six months	.15	.24	.16	.18

TABLE 1 continued

D. Housing and neighborhood conditions				
Apartment in poor condition	.28	.28	.27	.28
Too little space in apartment is a problem	.78	.79	.74	.77
Somewhat or very dissatisfied with neighborhood	.59	.50	.55	.55
Feels unsafe or very unsafe during the day	.51	.44	.46	.48
Drug dealers are a big problem in the neighborhood	.75	.74	.73	.74
E. Origin Census Tract				
Poverty rate	.41	.41	.42	.41
If White	.34	.36	.35	.35
If Black	.46	.42	.44	.45
If Hispanic	.26	.28	.27	.27
If English not first language	.34	.37	.35	.35
If High School dropout (25 years and older)	.46	.46	.46	.46
Unemployment rate	.09	.09	.09	.09
If households on welfare	.33	.33	.33	.33
Sample size	240	120	180	540

Note: Data are from the MTO Baseline Survey for universe of participants enrolling in MTO in Boston between October 1994 and May 1996. Origin Census Tract data based on geocoded address linked to 1990 Census data. Missing data are imputed at non-missing mean. Estimates are weighted as described in the text.

TABLE 2 – MTO-Boston Program Moves by Baseline Survey Characteristics

	Experimental			Section 8 Comparison		
Baseline Survey Characteristics	Program Move	Program Move beyond Boston	N	Program Move	Program Move beyond Boston	N
A. All	.481	.296	240	.634	.156	120
All (and geocoded Follow-up Survey location)	.479	.298	235	.619	.134	114
B. If dissatisfied with neighborhood	.577	.391	138	.697	.236	59
If satisfied with neighborhood	.355	.164	100	.572	.077	61
Difference by satisfaction	.223** (.067)	.227** (.058)		.125 (.090)	.159** (.067)	
C. If too little space in apt. is prob.	.502	.302	189	.674	.172	96
If space in apt. is not big problem	.433	.267	50	.404	.096	24
Difference by problems with space	.069 (.086)	.043 (.077)		.289** (.115)	.076 (.076)	
D. If prev. lived in mostly white area	.667	.517	75	.692	.230	37
If have not previously lived in a mostly white neighborhood	.401	.192	161	.606	.109	81
Difference by previous neighborhood	.266** (.075)	.325** (.071)		.086 (.098)	.121 (.080)	
E. If race/ethnicity is black	.429	.347	88	.630	.050	44
If race/ethnicity is Hispanic	.483	.192	106	.680	.222	50
If race/ethnicity is not black or Hisp.	.577	.442	46	.557	.233	26

Note: Data on program moves are from Abt Associates. Data on characteristics are from the MTO Baseline Survey. Estimates are weighted as described in the text.
* = p-value <.1; ** = p-value < .05

TABLE 3 – Impact of MTO-Boston on Mobility and Neighborhood Characteristics

	Control	Exp - Control	Sec8 - Control
A. Mobility rates			
Program move	0	.479**	.619**
Move out of project	.271	.325**	.414**
Living outside Boston	.049	.234**	.069**
B. Census Tract Characteristics			
Poverty rate	.359	-.122**	-.100**
If poverty rate < 10%	.018	.344**	.116**
If poverty rate < 20%	.128	.347**	.237**
If poverty rate < 30%	.318	.308**	.352**
If poverty rate < 40%	.407	.288**	.341**
If income > 2x poverty line	.415	.170**	.122**
If race is white	.380	.146**	.060
If race is black	.218	-.095**	-.057
If race is Hispanic	.449	-.056**	-.018
If English not first language	.315	-.053**	.012
If English almost not spoken at all	.097	-.025**	.000
If immigrant	.148	-.001	.042**
If family female-headed	.632	-.172**	-.112**
If public assistance	.294	-.097**	-.066**
If person in renter-occupied unit	.827	-.197**	-.112**
If workers using public transportation	.387	-.104**	-.071**
Unemployment rate	.086	-.018**	-.011**
Full-time, full-year worker	.327	.072**	.064**
If managerial/professional worker	.206	.029**	.022**
If at least some college (25 years and older)	.289	.064**	.066**

Note: Data on residential location was taken from the MTO-Boston Follow-up Survey, and geocoded to link to 1990 Census data on area characteristics. The total sample size is 525 (235 Experimental, 114 Section 8, and 176 Control). * = p-value <.1; ** = p-value < .05

TABLE 4 – Impact of MTO-Boston on Safety

	Control	Exp - Control	Sec8 - Control	N
Streets near home are unsafe or very unsafe during the day	.386 (.038)	-.163** (.047)	-.078 (.059)	509
Household head or child has seen people using or selling drugs once a week or more	.359 (.038)	-.203** (.045)	-.134** (.056)	507
Household head or child has seen or heard gunfire once a month or more	.205 (.032)	-.132** (.036)	-.106** (.044)	513

Note: Data are from the MTO-Boston Follow-up Survey. Estimates are weighted as described in the text; robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 5 – Impact of MTO-Boston on Criminal Victimization

	Control	Exp - Control	Sec8 - Control
If any crime	.255 (.033)	-.118** (.041)	-.115** (.047)
If property crime	.134 (.026)	-.057* (.032)	-.087** (.033)
If personal crimes: at least one child involved	.127 (.026)	-.059* (.031)	-.023 (.039)
If personal crimes: household head involved	.073 (.020)	.003 (.031)	-.042* (.025)

Note: Data are from the MTO-Boston Follow-up Survey. Sample size is 519. Personal crimes are assault, rape, robbery, pick pocketing (attempted or completed). Property crimes are theft, household or motor vehicle burglary (attempted or completed). Robust standard errors are reported in parentheses. * = p-value <.1; ** = p-value < .05

TABLE 6 – Impact of MTO-Boston on Social Behavior Outcomes For Children Ages 6-15

		Control	Exp - Control	Sec8 - Control	N
A. Child behavior problems					
Has trouble getting along with teachers	Boys	.353	-.113*	-.041	267
		(.053)	(.067)	(.087)	
	Girls	.156	.018	.036	291
		(.034)	(.049)	(.060)	
Is disobedient at home	Boys	.316	-.104*	-.029	273
		(.048)	(.061)	(.082)	
	Girls	.174	-.047	-.080	299
		(.040)	(.049)	(.053)	
Is disobedient at school	Boys	.455	-.077	-.124	274
		(.054)	(.075)	(.088)	
	Girls	.333	.038	-.137*	300
		(.053)	(.073)	(.071)	
Hangs around with kids who get into trouble	Boys	.221	-.095	-.100	273
		(.047)	(.058)	(.066)	
	Girls	.115	-.047	-.021	297
		(.032)	(.040)	(.052)	
Cruel or is mean to others	Boys	.190	-.137**	-.123*	274
		(.044)	(.048)	(.065)	
	Girls	.076	-.024	-.033	298
		(.026)	(.033)	(.037)	
Is restless or overly active	Boys	.468	-.038	-.117	273
		(.055)	(.076)	(.088)	
	Girls	.263	.003	.032	299
		(.044)	(.062)	(.076)	
Is unhappy, sad, or depressed	Boys	.284	-.125**	-.163**	274
		(.049)	(.061)	(.067)	
	Girls	.232	-.015	-.026	298
		(.042)	(.061)	(.068)	

TABLE 6 continued

B. Child Social Behavior

At least one close friend in neighborhood	Boys	.747 (.054)	.018 (.075)	.056 (.077)	272
	Girls	.823 (.042)	-.134** (.064)	-.160** (.077)	295
If participated in extra-curricular activities	Boys	.428 (.056)	-.050 (.077)	-.091 (.089)	274
	Girls	.473 (.054)	-.137* (.072)	-.031 (.087)	290

Note: Data are from the MTO-Boston Follow-up Survey. Estimates are weighted as described in the text; robust standard errors are reported in parentheses, adjusted for household clustering.
* = p-value <.1; ** = p-value < .05

TABLE 7 – Impact of MTO-Boston on Physical Health Outcomes For Children Ages 6-15

	Control	Exp - Control	Sec8 - Control	N
If non-sports injury in past six months requiring medical attention	.078 (.019)	-.043* (.022)	-.025 (.029)	569
If asthma attack in past month requiring medical attention	098 (.023)	-.038 (.029)	-.007 (.026)	570

Note: Data are from the MTO-Boston Follow-up Survey. Estimates are weighted as described in the text; robust standard errors are reported in parentheses, adjusted for household clustering.
* = p-value <.1.

TABLE 8 – Impact of MTO-Boston on Adult Health

	Control	Exp - Control	Sec8 - Control	N
Overall health is good or better	.578	.113**	.180**	511
	(.038)	(.050)	(.056)	
Calm and peaceful "a good bit of the time" or more often during the past four weeks	.465	.100*	.136**	508
	(.039)	(.052)	(.062)	
Happy "a good bit of the time" or more often during the past four weeks	.561	.069	.035	506
	(.039)	(.052)	(.062)	

Note: Data are from the MTO-Boston Follow-up Survey. Estimates are weighted as described in the text; robust standard errors are reported in parentheses. * = p-value <.1; ** = p-value < .05

TABLE 9 – Impact of MTO-Boston on Social Relations

	Control	Exp - Control	Sec8 - Control	N
Visited with friend or relative at your home at least once a week in the past month	.482	-.056	-.055	509
	(.039)	(.053)	(.062)	
Visited with a friend or relative at their home at least once a week in the past month	.422	.082	.055	512
	(.038)	(.053)	(.062)	
On the telephone with close friends or relatives 4 times or more in the past week	.561	.018	.075	508
	(.038)	(.053)	(.061)	
Went to church or place of worship at least once in the past 30 days	.573	-.007	-.060	510
	(.038)	(.053)	(.062)	
Agree with: "Most people can be trusted" versus "You can't be too careful in dealing w/people"	.078	.065*	.035	499
	(.022)	(.033)	(.038)	

Note: Data are from the MTO-Boston Follow-up Survey. Estimates are weighted as described in the text; robust standard errors are reported in parentheses. * = p-value <.1.

TABLE 10 – Impact of MTO-Boston on Welfare and Work

	Control	Exp - Control	Sec8 - Control	N
A. MTO-Boston Follow-up Survey				
Receiving welfare benefits (AFDC/TANF)	.472	.030	-.008	519
	(.039)	(.053)	(.062)	
Notified of a time limit on welfare benefits	.415	.018	-.019	519
	(.038)	(.052)	(.061)	
Receiving aid for disabled or needy elderly (Supplemental Security Income)	.242	.030	-.015	516
	(.033)	(.046)	(.053)	
Receiving Food Stamps	.520	.019	-.020	517
	(.039)	(.053)	(.062)	
Worked for pay last week	.434	-.071	.001	520
	(.038)	(.052)	(.062)	
Worked at job with health or other benefits	.149	-.001	.058	520
	(.027)	(.038)	(.049)	
Average hourly wages among workers	8.46	.493	.153	186
	(0.43)	(.619)	(.581)	
Average quarterly earnings	1455	-252	-85	520
	(169)	(222)	(261)	
B. Massachusetts administrative records				
Received TANF in 1997, 3rd quarter	.505	-.001	-.017	540
	(.038)	(.052)	(.061)	
Received TANF in 1998, 3rd quarter	.399	.027	-.067	540
	(.035)	(.050)	(.058)	
If any earnings in 1997, 3rd quarter	.436	-.017	-.007	540
	(.037)	(.051)	(.060)	
If any earnings in 1998, 3rd quarter	.494	-.002	-.026	540
	(.038)	(.052)	(.060)	
Average quarterly earnings in 1997, 3rd quarter	1572	-101	121	540
	(193)	(253)	(305)	
Average quarterly earnings in 1998, 3rd quarter	2045	-328	-278	540
	(226)	(281)	(325)	

Note: Data are from the MTO-Boston Follow-up Survey, the Massachusetts Department of Transitional Assistance, and the Massachusetts Department of Revenue. Estimates are weighted as described in the text; robust standard errors are reported in parentheses.