Moving to Opportunity in Boston:
Early Impacts of a Housing Mobility Program

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Abstract

This paper examines the short-run impacts of a change in residential neighborhood on the well-being of low-income families, using evidence from a program in which eligibility for a mobility subsidy was determined by random lottery. We provide a first look at the experiences of 540 families at the Boston site of Moving To Opportunity (MTO), a demonstration program currently underway in five cities. Families in eligible public housing projects in high poverty Census tracts can apply to MTO and are assigned by lottery to one of three groups. The Experimental group receives some counseling assistance and a Section 8 rental subsidy that can be used to move to a Census tract that had a poverty rate of less than ten percent in 1990. The Section 8 Comparison group receives a geographically unrestricted rental subsidy. The Control group continues in public housing and receives no new rental assistance or services.

We pursue a multi-faceted research strategy. Direct program observation and qualitative interviews with participants are used to form hypotheses and guide the interpretation of results. Our quantitative analyses of program impacts use housing program records on residential location, and data from a new telephone survey of participants. One to three years after participants enter the program, we find that both Experimental and Section 8 Comparison families are fairly successful in using their subsidies to move out of high poverty neighborhoods: 45% of Experimental and 59% of Section 8 Comparison families move through the program. The Experimental group is much more likely to move to suburban and other wealthier communities, while regular Section 8 assistance is modestly more effective in getting a larger share of families out of the most distressed communities. Both the Experimental and Section 8 Comparison groups report fewer property crimes, and Experimental group children are less likely to be victims of a personal crime. The subsidized mobility opportunity has resulted in significant improvements in the general health status and mental health of household heads, and a reduction in behavior problems among children.

I. Introduction

Poverty in the United States has become increasingly concentrated in high poverty areas. For example, between 1970 and 1990, the percentage of poor persons in metropolitan areas living in census tracts with a poverty rate of 40 percent or more increased from 12 percent to 18 percent (Jargowsky, 1997).[1]   The concentration of poverty in inner cities, along with recent increases in residential segregation by family income and the persistence of residential segregation by race in U.S. metropolitan areas (Cutler, Glaeser, and Vigdor, 1999; Massey and Denton, 1993) has potentially disturbing implications because of evidence that residential neighborhoods are associated with both the current well-being and future opportunities of residents. In particular, children who grow up in poor neighborhoods fare substantially worse than those who grow up with more affluent neighbors on a wide variety of outcomes. Children growing up with economically disadvantaged neighbors are more likely to drop out of school, to get into trouble with the law, to be victims of crime, to bear children out of wedlock, to end up on public assistance, and to be out of work as young adults. These differences in socioeconomic outcomes by neighborhood characteristics remain (but are somewhat attenuated) in studies that control for family income and background characteristics (e.g., Brooks-Gunn, Duncan, Klebanov, and Saland, 1993; Jencks and Mayer, 1990).

Many analysts conclude from such findings that residential location greatly affects access to opportunity through peer influences on youth behavior and through a variety of neighborhood characteristics correlated with neighborhood wealth -- such as school quality, safety from crime, and the availability of supervised after-school activities. The willingness of families to pay substantial house price and rental premia to move to wealthier neighborhoods and gain access to better schools strongly suggests that typical citizens believe that neighborhood and school social composition truly affect their children's life prospects.[2]

Although existing empirical studies and the perceptions and actions of parents are suggestive of the importance of neighborhoods, the estimation of the causal effects of

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[1] 56.6 percent of poor persons live in metropolitan areas (Jargowsky Table 2.3).

[2] Black (1999) provides convincing evidence of parents' willingness to pay for higher public school quality by comparing house prices for comparable homes in the same neighborhoods on different sides of the borders between adjacent elementary school attendance districts for suburban areas in Massachusetts. She finds systematically higher purchase prices for homes on such district borders that provide access to elementary schools with higher test scores.

neighborhoods is fraught with difficulties.[3] Observed differences in youth outcomes by type of neighborhood could reflect unmeasured differences in family background. The families of poor children residing in richer neighborhoods may simply be unobservably different from other poor families. The reduction in the magnitude of estimated neighborhood effects when detailed controls for family background are included suggests that even better controls would further reduce estimates of neighborhood effects (Corcoran, Gordon, Laren, and Solon, 1992).[4] But the difficulty of measuring neighborhood attributes may lead to underestimates of neighborhood influences.

These estimation problems would be greatly reduced if we could compare the outcomes of children from truly comparable families growing up in different types of neighborhoods. The ideal evaluation design would be the random assignment of families of interest to different neighborhoods. Housing mobility programs in which some low-income inner-city families are given assistance in moving to less-segregated, wealthier, and/or suburban locations can approximate such a design if access to such assistance is randomly assigned. The Gautreaux program, which has operated in the Chicago metropolitan area since 1976, is suggestive of the potential of learning about neighborhood influences from housing mobility programs. The Gautreaux program resulted from a Supreme Court consent decree in a racial discrimination lawsuit against the Chicago Housing Authority and the U.S. Department of Housing and Urban Development (HUD) filed on behalf of Chicago public housing residents. It provides low-income blacks in Chicago public housing (primarily single female-headed households on AFDC) with special housing certificates and assistance to move to neighborhoods with no more than 30

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

[4] Recent research has attempted to better control for family background and made progress in addressing the issue of selectivity of residential location within a metropolitan area. For example, Aaronson (1997) 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 one worries that the differences in sibling outcomes could reflect changes in family circumstances driving residential moves to better or worse neighborhoods rather than neighborhood effects. Cutler and Glaeser (1997) compare outcomes for black and white youths across MSAs by the extent of racial residential segregation without conditioning on residential location within an MSA and find substantially lower relative educational attainment, employment, and earnings for black youth and young adults in more segregated MSAs. Although this approach reduces the impact of unobserved family factors affecting residential location within an MSA, differences in family attributes across MSAs remain a concern.

percent black population in suburbs and other parts of the city. Quasi-experimental comparisons of the household heads and the children of a sample of suburban and urban movers in the program suggest that moving to the suburbs greatly increased youth educational attainment and labor market outcomes and modestly improved mothers' employment rates relative to moving to other parts of the city (Rosenbaum 1992, 1995). However, even if moves through Gautreaux approximated random assignment, the sample who could be located by survey researchers some years later may have been unrepresentative in ways which could have influenced research results. Thus, unobserved differences in the attributes of the located suburban and urban movers are a concern in interpreting the findings of the Gautreaux studies, especially given the small sample sizes.

The potential for learning about the magnitude of causal neighborhood effects and for assessing housing mobility programs as a policy to move public housing residents into better neighborhoods through the private housing market motivated the development by HUD of an experimental housing mobility program that explicitly uses random assignment. This Moving to Opportunity (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 census tract. Interested eligible families selected from a waiting list are randomly assigned to one of three program groups: the Experimental (or MTO) group, the Comparison (or Section 8) group, and an In-place Control group. Families in the Experimental group receive a restricted Section 8 certificate or voucher that provides a rent subsidy that 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 receive counseling assistance from a local non-profit organization to help them search for an apartment and adjust to a new neighborhood. Comparison group members receive a geographically unrestricted Section 8 certificate or voucher and no counseling assistance. The Control group families do not receive rental assistance vouchers or certificates, although their eligibility for continued project-based assistance is unaffected. The Experimental and Comparison group members are given four to six months (depending on the site) to "lease up" (submit for approval a request for an eligible apartment they would like to lease). By

randomly assigning assistance in moving to wealthier neighborhoods, the MTO demonstration provides a unique opportunity both to assess the effectiveness of using housing mobility programs to move families to these neighborhoods and to measure the causal impacts of neighborhood attributes on family and youth outcomes for poor families.

In this paper, we provide an evaluation of the early impacts of the MTO demonstration at the Boston site and discuss the implications of these results for urban housing policy and for models of neighborhood effects. We combine information from field observations of the operation of the program, qualitative interviews with participating families, data on Census tract and block characteristics linked to geocoded initial and current addresses of participants, responses to survey questions, and state administrative earnings and welfare data.

We begin in Section II by providing a more detailed description of the actual operation of the MTO demonstration in Boston, including the characteristics of the families participating in the demonstration and their motivations for signing up for the program. We find that fear of crime is the primary reason participants want to move out of public housing at all five sites.[5] In section III, we present our econometric and theoretical framework for analyzing neighborhood effects, and discuss the interpretation of parameters can be estimated from the MTO experiment. In section IV we analyze mobility outcomes for the Boston families. We find that approximately 45 percent of the MTO group move to private housing using the MTO vouchers as compared to 59 percent of the Section 8 Comparison group. However, we find that MTO and Section 8 have similarly large positive effects on average neighborhood quality as measured by the poverty rate, median income, and public assistance usage rate of census tracts and block groups: while fewer participants move through MTO, those that move end up in much wealthier neighborhoods than those moving in the Section 8 Comparison group. Thus, MTO has larger impacts than Section 8 on the likelihood of moving to the suburbs (outside of Boston), and on the probability of living in a low poverty (under 10 percent) census tract or census block groups. Section 8, through its higher move rate, gets a larger share of families out of very high (40 percent or more) poverty areas.

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[5] Our interviews with Boston participants strongly reinforce this conclusion and indicate substantial concerns of these public housing residents with protecting themselves and their children from possibly arbitrary outbursts of violence. Kling, Liebman, and Katz (1996) provides a more detailed presentation of the findings from our qualitative interviews on participants perceptions of their neighborhoods.

Next, we present estimates of the early impact of the treatments, focusing on outcomes that directly impact participant well-being in the short-term as well as on short-term outcomes that might provide mechanisms through which longer-term economic outcomes such as increased earnings and educational attainment could be brought about. In section V, we analyze the impact of the program on the outcome that families said was their more important reason for moving -- neighborhood safety. We find that the program leads to large increases in perceived safety and to significant declines in criminal victimization. In section VI, we analyze the impact of the program on the mental health and social interactions of the household head. We find evidence that MTO moves reduce the prevalence of major depressive episodes, and increase the chance that household heads report that their overall health is good or better. We fail to find evidence that potential movers become more socially isolated when they move to their new neighborhoods, and indeed we find weak evidence that the moves increase people's degree of social trust. We find that the program leads to a decline in behavior problems among boys, but no discernable impact on girls. We also find some evidence that the program increases the frequency at which children read for pleasure. In section VII, we discuss econometric issues in interpretation of the estimates, focusing on the outcome of depression.

In a future version of this paper, there will be a section VIII in which we will present estimates of the short-term impact of the treatments on household earnings, welfare participation, and total income. There will also be a section IX, where we will discuss valuation of costs and benefits, and the implications of our results for housing policy and for the literature on neighborhood effects. Section X concludes.

III. The Nature of the MTO Experiment in Boston

The MTO demonstration program presents an exciting opportunity to identify credibly the causal effects of a housing mobility program on a wide range of outcomes. Nonetheless, it is important to be explicit about the nature of the specific interventions that occurred at the Boston MTO site. Although concerns with the extent to which findings can be extended to similar programs or to different settings arise in the evaluation of any social experiment, our field work has made us particularly sensitive to potentially idiosyncratic features of the Boston MTO demonstration.

<u>The Program</u>

The "treatment" provided to the MTO Experimental group is quite extensive. All participants received a Section 8 certificate or voucher and attended a basic session at the Metropolitan Boston Housing Partnership (MBHP) in which general advice was given on how to go about finding an apartment in an eligible census tract. At the end of this meeting, participants were assigned to one of three MBHP counselors, and the counselor scheduled a visit to the respondent's home. During the home visit, the counselor again explained the program,[6] and reviewed the participant's readiness for housing search. In particular, if the participant had a bad credit history, efforts were made to clear it up before housing search began. In addition, participants were urged to assemble recommendations from past landlords and from neighbors saying that the participants were good tenants and that their children were not trouble makers. The counselors helped program participants find apartment listings in newspapers, drove participants to see apartments, and quite frequently found promising apartments and then took participants to see them. Thus, the counselors played a very large role in determining the destination communities of the MTO participants.[7] In some cases, MBHP provided small grants to households to enable them to purchase furniture or appliances that were needed in the new apartment. 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.

Our estimated impacts of this complicated MTO treatment in Boston may also be affected by features of the Boston housing and labor markets during the period of study and by some

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[6] In our field work, we observed that Spanish speaking participants often did not understand the 10 percent poverty rate restriction until it was explained during the home visit.

[7] 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.

changes in government policies that impacted the participating families. In particular, both housing vacancy rates and unemployment rates in the Boston area were very low (and falling) during the period of the study. 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. Indeed, one woman in the Section 8 Comparison group told us that the reason she did not take advantage of her opportunity to move was that she had heard from a friend that as soon as the government convinced everyone to move from the projects, the government was going to end Section 8 and leave everyone to fend for themselves. 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. 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.[8] There were also major improvements during the study in the safety of some of the developments from which MTO families came. Indeed, one participant who moved to the suburbs told us that if the improvements in the projects had occurred earlier, she would not have applied for MTO and moved.[9]

Because so much is going on inside the "black box" of the MTO treatment, we have attempted to supplement quantitative comparisons of outcomes by treatment status with qualitative interviews with the program participants and the counselors. Their insights provide us with supplemental information to assess the plausibility of alternative hypotheses concerning which factors determine who uses MTO to move and which neighborhood attributes (peers, schools, crime risks) are likely to be most important in affecting short- and long-run outcomes for adults and youth.

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[8] At one informational session we attended at MBHP for new enrollees in MTO, we observed considerable concern among program participants over the potential need to come up with money for a security deposit. However, recent conversations with housing counselors indicate that the security deposits have not been a major obstacle to moves. Nonetheless, it is possible that some of the drop off in move rates we observe in later cohorts of enrollees is due to the change in security deposit rules.

[9] She added, however, that she was grateful that she had moved and did not at all regret her decision to participate in the program.

Finally, it is 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 will differ from the final 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 greater expectations of graduating from high school. Indeed, even if MTO had no beneficial impact on the adults in its households, the program could still be extremely valuable if it led to positive child outcomes. Many important outcomes for children in MTO families will not be apparent for years.

The Data

The sampling frame for the data used in this study consists of household head 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 27 families per month. This paper relies on two primary data sources. First, each household head 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, which we refer to as the "MTO Boston Follow-up 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. 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 survey response rate of 96.3% 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) introduce some analytical considerations.

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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 the different survey completion dates combine to yield an average time between random assignment and the Boston Follow-up Survey of 2.2 years, ranging from 1 to 3.5 years with 16% of households having less than 1.5 years, and 14% having 3 years or more.

Characteristics of MTO Families

Table 1 presents characteristics of the initial families in the MTO demonstration from each of the five sites. The characteristics come from the baseline survey that participants completed prior to being randomly assigned into one of the three program groups.[10] The vast majority of MTO families at each site are headed by a single mother who receives public assistance. In this initial wave of enrollees, fewer than 25 percent of the household heads were employed (either part-time or full-time) at the time of the baseline survey. MTO families typically have from one to three children. Very few own an automobile. 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.

Table 1 indicates some substantial variation among the five sites in the racial and ethnic composition of participating families reflecting differences in the composition of families who live in public housing in high poverty areas in each of the cities. The participants in Baltimore and Chicago are almost entirely African-American. The Boston, Los Angeles, and New York sites have more diverse groups of participants with over 40 percent Hispanics. The Boston site is the only site where a minority (40 percent) of the families are African-American and contains the largest share of non-Hispanic whites (10 percent). Household heads in Boston during this period had slightly higher employment rates than those at the other sites, and had higher average levels of education than three of the four other sites in the MTO demonstration.

The baseline survey asked enrollees to indicate the main reason and second most important reason that they wanted to move. Table 1 shows that the main reason the majority of

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[10] The data are for those randomly assigned from October 1994 to approximately October 1995.

families list for wanting to move is fear of crime ("to get away from drugs and gangs"). Crime concern is the leading motivation for moving in all five sites. Furthermore, an additional 30 percent (averaged across the five sites) list crime as the second most important reason they wanted to move. Improvements in housing ("to get a bigger or better apartment") and school quality ("better schools for my children") are 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 fewer than 2 percent of the enrollees. 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 the late 1970s indicated that better schools and better housing were more important considerations than crime (Peroff *et al*., 1979). 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 over this time period. Table 1 shows that the majority of enrollees at all 5 demonstration sites indicate that the streets near their homes are unsafe or very unsafe during the *day,* and over 80 percent believe drug dealers are a big problem in their neighborhood.

The second page of table 1 shows that for many MTO families, concern about crime may be the result of having been victimized recently. Over 20 percent of the household heads indicate that someone who lives with them had been beaten or assaulted in the past six months, and 10 percent had someone stabbed or shot. 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 reported in Zelon *et* al.(1994). The criminal victimization rates and perceptions of neighborhood danger for the Boston respondents are somewhat lower than those of the other sites, but they remain quite high relative to national averages. Even though the criminal victimization rates reported at baseline by MTO families could be overstated, we find in our MTO fieldwork in Boston that the fear of violent crime is a major influence in the daily lives of these families and a key motivator of desires to move.[11]

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[11] See Kling, Katz, and Liebman (1996) for more details on these comparisons. These differences probably reflect the high poverty (and probably also high crime) areas for MTO enrollees even relative to other public housing residents, but 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 Boston Housing Authority would not receive copies of individual responses, our interviews revealed that some respondents assumed their answers could influence

Table 2 presents basic characteristics of the 540 Boston households that are the focus of our analysis. The characteristics are presented separately by treatment status (MTO, Section 8, or Control). The fact that the mean employment rates reported in Table 2 are higher than in Table 1 is consistent with national patterns of substantial increases in employment rates for single female household heads over the period of operation of the demonstration (Liebman, 1998). The similarity in the characteristics of the MTO, Section 8, and Control groups in Table 2 is strongly suggestive of a successful application of random assignment. There are not statistically significant differences between the treatment groups at conventional levels for any of the demographic characteristics reported in Table 2.

Characteristics of the origin (baseline) neighborhoods of our Boston sample are presented in Table 3. The table presents mean census tract attributes for the overall samples and by treatment group status. All of the families were initially located in the Boston central city. As one would expect given the demonstration's eligibility restrictions, the typical family lived in a census tract with a very high poverty rate and with approximately one third of the households on public assistance in 1990.

We have also examined the characteristics of smaller neighborhoods defined by origin census block groups. The census block group characteristics are quite similar, but a bit more extreme than the census tract characteristics reported in Table 3. The mean 1990 poverty rate in origin census block groups was 43.7 percent as compared to 41.1 percent for the mean census tract.[12] Census tract and block group characteristics are identical up to normal sampling error across the three treatment groups.

III. An Econometric Interpretation of the Estimated Effects

A critical issue in the study of the impact of residential location on individual outcomes is the selection problem arising from the likely systematic sorting of individuals among

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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.

[12] We focus on census tract characteristics since some Boston census block groups represented in our sample have very low populations and thereby quite small underlying sample sizes in the Census STF files -- making outliers from sampling error a greater worry than with census tract data.

neighborhoods on the basis of important (unobserved) determinants of socioeconomic outcomes. To identify the causal effect of residential location on an outcome of interest, we must compare people living in different locations who would have experienced the same outcome, at least on average, if they had lived in the same location. Since people cannot be located in two places at once, this comparison necessarily involves a counterfactual that cannot be directly observed.

In our analysis of the MTO program, we study safety, health, and other outcomes for families living in public housing who were offered a Section 8 subsidy to defray their rental payments in a privately owned apartment. We then compare the average outcomes of these families to a Control group of families also living in the same public housing projects, but who were not offered the subsidy. The key to this analysis is that the offer of the subsidy is randomly assigned by lottery. Thus, the Control group is used to identify the average outcomes corresponding to the counterfactual state that would have occurred for families who did not receive a rental subsidy through the lottery. These comparisons are commonly known as "Intent-To-Treat" (ITT) effects.

We illustrate the approaches to the estimation of these parameters of interest in a simple regression framework. We let D be an indicator variable for use of a Section 8 subsidy through the MTO program, or "taking-up" the treatment, Z be an indicator variable for being eligible for the MTO program with coefficient pi, and let eta be the other determinants of subsidy use in equation (1).

(1) $D_i = Z_i \pi_1 + \eta_i$

The ITT effect is captured by the coefficient π_2 in a regression of the outcome (Y) on an indicator for assignment to a treatment or control group (Z) as in equation (2):

(2) $Y_i = Z_i \pi_2 + \varepsilon_i$

The ITT effects are an average of the causal effect for those who take-up the treatment and those who do not. In this application, there are two treatment groups, so we can compute separate ITT estimates for both the Experimental and Section 8 Comparison groups.

Assuming that those who do not receive the treatment have a treatment effect of zero, we can also estimate the parameter commonly known as the effect of "Treatment on the Treated" (TOT), which can then be obtained by dividing the ITT by the proportion receiving the treatment.[13] Thus, TOT = pi_2/pi_1, and can equivalently be uncovered from a regression of Y on D with Z used as an instrumental variable for D, as in equation (3), where the coefficient gamma is the indirect least squares (IV) estimate.

(3) $Y_i = D_i\gamma + v_i$

The characteristics known prior to randomization (X) should have the same distribution within the treatment and control groups because they are statistically independent of group assignment. Thus, including them in a regression like (2) will not change the coefficient pi_2 (unless X happens to differ between groups due to the variability in a small sample). Xs may still be included to reduce the standard error of the treatment effect estimates, however, if they are related to Y and thereby reduce residual variation in the regression.

In the least restrictive sense, the treatment effects identified here are the result of all factors of the experience of the Experimental and Section 8 Comparison group families that differed from the Control group families. In addition to residential location, other potential differences include the form of assistance (public housing versus vouchers) and Experimental group counseling. The pool of MTO applicants all reside in public housing. Families who live in public housing and those receiving a Section 8 subsidy in either of the treatment groups all pay 30% of their income in rent, so take-up of the subsidy only affects residential location and does not affect the budget set of the family. The quality of the apartment itself may differ, so this must be included in our concept of "residential location." In principle, the counselors in the Experimental group could have an effect on outcomes separate from any change in residential location, but our field work suggests that the counselors mainly facilitated apartment location and did not provide large amounts of social services. Thus, we interpret these treatment effects as

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[13] The use of this deflator based on the proportion in the treatment group who actually received the treatment was introduced in the program evaluation context by Bloom (1984). See Heckman, LaLonde, and Smith (1999) for a comprehensive discussion of alternative parameters of interest in the evaluation of social programs.

having been caused primarily by a change in residential location and not by other possible effects of program eligibility.

Another important issue in interpreting our MTO results is their external validity. Our analysis focuses on families who are caused to move into neighborhoods where they are often the only family receiving public rental assistance. Our results have direct bearing on similar expansions of rental subsidies of moderate scale in Boston or in other cities. There may well be additional effects if the subsidized movers become a large enough presence to affect aggregate neighborhood characteristics.[14] These results are also most applicable to forecasting outcomes for situations in which families are offered the subsidy but allowed the alternative of remaining in their original location. Tearing down an entire public housing development and converting all the tenants to Section 8 assistance without creating any new units, for example, would likely increase subsidy take-up. Analysis of the interaction of the treatment effect with the predicted probability of take-up based on observable characteristics provides some evidence which might be used to model and forecast treatment effects for programs having different take-up rates, but the magnitude of treatment effects for different take-up rates is not directly identified in the data.

This analysis differs from much of the previous literature on neighborhood effects (see the review by Jencks and Mayer, 1990) in that it does not specify a linear model for observational data in which various characteristics of neighborhoods are included as regressors. The advantage of this MTO analysis is that the source of identification for the causal effect is clear, whereas results from a regression on observational data may be driven by selective sorting of individuals into particular types of neighborhoods. A limitation of the MTO design is that individuals within each group do choose their own location. We can observe average differences between the treatment and control groups in the entire vector of neighborhood characteristics, and can attribute the causal effect of the treatment to changes in this aggregate bundle of characteristics. The result of this design feature, however, is that the randomization in the MTO experiment does not directly identify which particular neighborhood characteristics contributed to treatment effect. On the other hand, the fact that we have observational data for individuals who all started out in very similar public housing neighborhoods and that many of them now live in quite different

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[14] This type of feedback is also referred to as "endogenous" effects (Manski 1993) or "general equilibrium" effects (Heckman 1999).

neighborhoods may facilitate examination of changes in particular characteristics of neighborhoods, but such an analysis is necessarily non-experimental in nature.

To characterize the average bundle of neighborhood characteristics for each group, we examine data on 1990 Census tracts. The changes in the bundle of characteristics can then be compared with average outcomes for each group in order to assess the relationship between a change in the characteristic bundle and the treatment effects. Indeed, it is informative to examine the scatterplot of changes in average outcomes and averages in particular characteristics such as the neighborhood poverty rate that are representative of changes in the larger bundle. This is similar to estimating a regression using the treatment indicators to instrument for a function (say, linear or quadratic) of the poverty rate, since the average poverty rate for the treatment group is the same as the fitted value from the first stage of two stage least squares (2SLS) regression with indicator variables as instruments. The difference is in the interpretation. The traditional interpretation of 2SLS is that the parameters define a function mapping changes in the endogenous variable to changes in the outcome holding all else constant. This interpretation requires that the entire effect of the program work through the poverty rate, an assumption which does not hold in our data. Our analyses relating outcomes and particular neighborhood characteristics such as the poverty rate are instead intended to proxy for changes in the entire bundle of neighborhood characteristics and not the marginal change in one characteristic.

Much of the theoretical interest in neighborhood effects focuses not on the magnitude of the effects but on the implications on nonlinearities. For example, the social welfare implications will differ if the effect of neighborhoods (say, as measured by the poverty rate) on outcomes is linear or nonlinear. If the relationship is linear there may be no net aggregate change in the outcome from more economically integrated neighborhoods. With a nonlinear relationship, for example, where there was a large benefit to moving out of the poorest neighborhoods and a small reduction in the outcome for residents of richer neighborhoods to which the poor moved, there would be a positive net change in the outcome -- and the converse would also be true.

The fact that the Experimental group families are required to use MTO subsidies in a low poverty neighborhood and the Section 8 Comparison group families are not restricted to these neighborhoods does result in a larger change in the neighborhood poverty rate and other characteristics for those who take-up in the Experimental group. Say, for example that the TOT

effect on a health outcome is found to be larger for the Section 8 Comparison group than for the Experimental group. One inference might be that more moderate changes in location are more beneficial, and larger changes in neighborhood quality might diminish some of the positive impact of the treatment. Although these differences provide some suggestive evidence on the shape of the response function of outcomes to residential location, there is a potentially confounding factor of differences in take-up behavior between the two groups. An alternative inference is that the group of people who take-up the subsidy in the Section 8 Comparison group could have a larger impact from the treatment of residential mobility to a lower poverty neighborhood. If these people had received the Experimental treatment they would have experienced a larger mean effect on health outcomes than those that actually take up when randomly assigned to the Experimental group. This interpretation cannot be directly refuted by the data, since such differences may be due to unobservable factors. We can, however, provide indirect evidence by examining whether the decision to take-up an MTO subsidy differed between the two groups (Section 8 Comparisons and Experimental) by observable characteristics X. If take-up in the Section 8 Comparison group was higher for a particular X, and if that X had a positive interaction with the treatment effect, then an explanation based on take-up behavior would receive indirect support.

IV. Program Moves in the Boston MTO Demonstration

We next explore the success of the Experimental and Section 8 Comparison groups in Boston at moving and leasing up using the MTO voucher or certificates. Table 4 displays the program move rates and share of families using the program to move to the suburbs (out of Boston) for the MTO and Section 8 groups enrolled in the program through May 1996. A substantial fraction of families were able to successfully move using program housing vouchers (or certificates) in both treatment groups. The more restricted MTO Experimental housing vouchers resulted in a 10 percentage point lower program move rate for the MTO group than for the Section 8 group (45.3 percent vs. 59.3 percent) despite the counseling assistance provided to the MTO group members.

The top panels of Tables 4 and 5 show that both MTO and Section 8 greatly increase residential mobility rates -- with move rates out of the origin public housing project increasing

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from 27 percent in the Control group to 60 percent with MTO eligibility and 69 percent with eligibility for regular Section 8 vouchers or certificates. However, MTO has a substantially and statistically significantly larger effect on moving families to locations outside of Boston even after accounting for non-program moves. Approximately 28 percent of the MTO group have final addresses outside of Boston as compared to 12 percent of the Section 8 group and 3 percent of the Control group.

An important issue with respect to the likely effectiveness of housing mobility programs that attempt to move low-income families from public housing to low-poverty (middle class) neighborhoods concerns the adjustment problems of the families and the possibility that many will have difficulties and quickly move back to high-poverty areas. We have verified "final" addresses from the summer of 1997 for the sample of Boston participants in our survey (525 of 540 Boston families overall). This sample allows us to examine how many MTO program movers moved again within 1 to 2 years of their original move. We find that 26.8% of Experimental group movers in our survey sample moved again with most of these moves (24.4%) being to higher poverty census tracts. The re-move rate for the Experimental group movers was similar to the 29.3% rate for Section 8 Comparison program movers that have moved again, but over one-third of these additional moves (12.2% of Comparison group household) were to lower poverty Census tracts. Among those in the Experimental group that used MTO to move out of Boston, only 7 percent have moved back to the central city. Thus, families who make a subsidized move in the Experimental group tend to remain in low poverty suburban census tracts, and their probability of relocating again is not different than a those making subsidized moves in the Section 8 Comparison group.

The most important determinant of making a subsidized move turned out to be the date of random assignment. Families enrolled in the first six months of the program (9/94-3/95) were about 20 percentage points more likely to make a subsidized move, and those in the next six months (4/95-8/95) were 10 percentage points more likely than those in the later period in our sample (9/95-5/96). The housing counselors for the Experimental group reported that they perceived apartments to be more scare on the housing market in later time periods.

The baseline survey, collected data on a variety of potential predictors of making a subsidized move through the MTO program, including demographics, education and

employment, perceptions of neighbors and the neighborhood, and expectations for the future. Among those variables having a significant positive raw correlation with a subsequent subsidized move were indicators of the household head being in school, having had relevant previous experience (having previously applied for Section 8, having previously lived in a neighborhood that was mostly white), a confident outlook (feeling sure of finding a new apartment, liking a new neighborhood, getting along with new neighbors; feeling very good about moving), perception of problems in the neighborhood (trash or graffiti a problem, dissatisfaction with current neighborhood) and perception of problems with current apartment (plumbing, wiring, or available space).

These results were similar to the analysis of the Los Angeles site (Hanratty *et al*, 1998), in that we did not find differences in the probability of making a subsidized move in the following characteristics: large number of bedrooms required by family size, presence of friends or family in the neighborhood, or positive attitudes towards moving to neighborhoods with higher income levels or higher concentrations of whites. While we did find that household heads ages 50 and over were less likely to make a subsidized move (consistent with Hanratty *et al.*'s findings), we did not find that reported victimizations in the six months prior to the baseline survey were significantly related to subsidized mobility.

Looking within the Experimental group only, we found that families with two or more children under age 5 were more likely to make a subsidized move, as were families with no children over age 13. The sample size of 120 for the Section 8 Comparison group, however, is too small to provide statistically significant evidence of even quite large differences in mobility by demographic characteristics specific to this group. We were able, however, to identify several categories of factors for which take-up behavior was substantially different for the Experimental and Section 8 Comparison groups.

One category is attachment to the neighborhood, based on the proportion of responses indicating less interaction among nine questions (e.g. borrowing, lending, chatting, watching other's children). Experimental group members who report less interaction with their neighbors are more likely to take-up a subsidy. Conversely, Section 8 comparison group members reporting more interaction with neighbors in the Baseline survey are more likely to take-up a subsidy. The difference in the effect of neighborhood attachment on take-up rates between the

two groups is statistically significant. This appears to be consistent with the idea that the less attached are more interested in making the longer distance moves often required for Experimental group members to relocate to a low poverty Census tract.

A second category is desire to move outside the city of Boston. Experimental group members who state in the Baseline survey that they would like to move beyond the city limits are more likely to take-up a subsidy. Among Section 8 Comparison group members, those who state they prefer to remain in their own neighborhood or within Boston are more likely to take-up. Again, the difference in the effect of desired location on take-up rates between the two groups by desired location is statistically significant. This also appears to be consistent with the structure of the program, where those who wished to move outside Boston were more successful in doing so when they were in the Experimental group and had a counselor to assist them in locating a unit.

Experimental group members who have applied previously for a Section 8 subsidy have higher take-up rate than those who have never applied, significantly higher than the negligible difference among Section 8 Comparison group members. Section 8 Comparison group members who are high school graduates are more likely to take-up a subsidy, while Experimental group high school graduates had take-up rates that were lower than the rates for those who did not graduate from high school. We speculate that the Experimental group counselors may have been able to assist less educated clients so that they did not experience the relative disadvantage of the less educated Section 8 Comparison group members in taking-up a subsidy.

Mobility and Neighborhood Outcomes

Our analysis of the short-run impacts of eligibility for MTO and regular Section 8 housing assistance on neighborhood quality and other outcomes uses the experiences of the 540 Boston families enrolled in the demonstration from October 1994 until the end of May 1996.[15] We next turn to an analysis of the impacts of the MTO and Section 8 treatments on neighborhood attributes using the 525 families in our phone survey sample for which we have verified final

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[15] We restrict our analysis to this group for two reasons. First, for our survey we wanted to interview families who had the opportunity to live in their new neighborhoods for at least nine months at the time of the interview. Second, because there are long lags between when families enter the program and when baseline data is available on them, we wanted to restrict our analysis to cohorts for which complete baseline data is available.

addresses of the 540 we attempted to contact. Table 4 presents final census tract and block characteristics by treatment status. Table 5 displays the differences in mobility and neighborhood outcomes by treatment group[16]. The differential outcomes for the MTO and Section 8 groups relative to the Control group can be interpreted as estimates of treatment effects of eligibility for these programs (intention-to-treat effects).

The MTO movers end up in lower poverty census tracts and blocks than Section 8 and Control group movers. This is to be expected given the restrictions of MTO rental assistance to census tracts with under a 10 percent poverty rate. Nevertheless the mean poverty rate of the final census tracts and blocks of the entire MTO and Section 8 groups are quite similar. Thus, eligibility for restricted vouchers combined with counseling assistance has a similar mean impact on neighborhood quality measured by the poverty rate as does access to standard Section 8 rental assistance, but this similarity masks the much larger impact (even with lower move rates) of MTO relative to Section 8 in moving eligible families to low-poverty (under 10 percent) areas.. In contrast, Section 8, through its higher move rates, helps a larger fraction of families move out of census tracts and blocks with 40 percent or greater poverty rates.[17]

Tables 4 and 5 also indicate that both MTO and Section 8 have large and statistically significant positive mean effects of somewhat similar magnitude on neighborhood "quality" for a wide variety of census tract attributes including median household income, the unemployment rate, public assistance usage, and the educational attainment of residents. The impacts of MTO tend to be larger in most cases. MTO and Section 8 also lead eligible families into census tracts with a higher share of whites, lower share of blacks, and much higher share of residents living in

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[16] The treatment group means and differences presented in Tables 4 and 5 are weighted by time of randomization into the program to account for changes in the random assignment ratios by group starting in February 1996. The note to Table 4 describes the exact procedure. The estimated treatment effects in Table 6 are weighted averages of the treatment effects before and after the change in randomization rates with the weights reflecting the overall sample sizes before and after the change. Unweighted estimates and estimates based on regression models assuming constant treatment effects over time, but including 2 or 6 time-of-randomization dummies, yield very similar results in all cases. Estimates are also quite similar when covariates, such as indicators for race, ethnicity, age of head, presence of young children, and employment at baseline are included in the specifications.

[17] This finding raises an important policy question. Is it more important for families to get out of very high poverty rate census tracts or are most outcomes affected only by getting families into census tracts with low poverty rates? Understanding the nature of the nonlinearities in the relationship between neighborhood poverty rates and neighborhood effects is crucial for evaluating whether the neighborhood restrictions on the MTO Experimental group members are beneficial.

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owner-occupied housing. MTO has a much larger effect in moving families into areas where a greater share of residents indicate English is their first language.

The overall message of Tables 4 and 5 appears to be that MTO has a larger effect than Section 8 on moving families to "middle class" (low-poverty) areas in the suburbs, but Section 8 is more effective in getting more families out of the poorest neighborhoods.

V. Safety and Criminal Victimization

At the time of the baseline survey, 56 percent of household heads reported that the main reason they wanted to move was "to get away from drugs and gangs."[18] In qualitative interviews, we also found that crime was the dominant factor motivating families to consider participation in the MTO program so that they could move. In one interview, for example, we began by asking a woman a general question about how she had found out about the MTO program.

> The first time I heard about this program, it said Boston Housing. But I already had lived in Boston Housing before. It had got so bad with the crime scene, you know. Every time I looked out my window, there was dead bodies. So I didn't want my kids to grow up in that atmosphere.

In her first sentence, she began to tell us that she had initially thought MTO was a program for families who wanted to move into the projects. But after bringing up "Boston Housing," she began to immediately tell us the most important thing about public housing was the association with crime. She went on to describe in horrible detail her experiences while living in the projects and then in a publicly subsidized building under private management. Our interpretation of her responses is that the images of crime and distress in her previous neighborhoods were so strong that she felt the need to tell us about them immediately.

The suggestion of the central role of crime in the decisions of MTO households from the baseline survey and our qualitative interviews lead us to examine the impact of the MTO program on several aspects of crime and safety issues. First, we examine self-reported safety in

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[18] The second most cited reason (29 percent) reported was that household heads wanted "to get a bigger or better apartment." The third most cited reason (8 percent), reported was that they wanted "better schools for my children." Interestingly, this rank ordering is the reverse of the reasons given for moving to their present location by participants in the Gautreaux housing mobility program in Chicago twenty years ago. In 1979, the most important reasons were the following: good schools (34 percent), quality of house (26 percent), and less crime (23 percent). See Peroff *et* al.1979, p. 114.

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the neighborhood. Second, we analyze differences in reported criminal victimization between the Experimental and Section 8 Comparison groups and the Control group.

Safety

We examined two types of self-reported measures of safety. First, we use the proportion directly reporting that they feel unsafe. Second, we ask about the frequency of three specific types of observable events (gunfire, drug dealing, and carrying of weapons) that our qualitative interviews suggested were important determinants of perceived safety. These results are reported in Table 6.

MTO household heads were asked in the baseline survey: "How safe are the streets near your home during the day?" 53 percent reported that they felt "unsafe" or "very unsafe." In our follow-up survey during 1997-98 (1-3 years later), we found that 39 percent of the Control group felt "unsafe" or "very unsafe." In comparison, the Experimental group was 16 percentage points less likely than the Control group to report feeling unsafe or very unsafe, a relative reduction of 42 percent. The Section 8 Comparison group also reported a lower incidence of feeling unsafe, but this difference was not statistically distinguishable from zero.

In the baseline survey, household heads were asked "In your neighborhood, how bad of a problem are drug dealers or users?" 74 percent reported that drugs were "a big problem", as opposed to a "small problem" or "no problem at all." In an attempt to develop a more interpretable metric of the intensity of the drug problem, we asked in the Follow-up survey how often family members had seen people using or selling illegal drugs in the neighborhood. We found that 36 percent of the Control group had observed this behavior once a week or more in the past six months.[19] In the Experimental group, the incidence was 20 percentage points lower, a relative reduction of 57 percent. In the Section 8 Comparison group, the incidence was 13 percentage points lower, a relative reduction of 37 percent.

Although no baseline survey evidence was collected about gunfire, 41 percent of the Controls in our Follow-up survey reported that they had heard gunfire in their neighborhood at

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[19] While 36 percent of Controls reported observing drug activity once a week or more, less than five percent reported observing drug activity at least once in the past six months but less than once a week – suggesting that these neighborhoods either have a drug presence that is either quite visible or fairly negligible.

least once in the past six months. The incidence is significantly lower in the Experimental and
Section 8 groups. In relative terms, the reduction is even larger for higher frequency gunfire. We
found that 21 percent of the Control group had heard gunfire once a week or more in the past six
months. In the Experimental group, the incidence was 13 percentage points lower, a relative
reduction of 64 percent. In the Section 8 Comparison group, the incidence was 10 percentage
points lower, a relative reduction of 51 percent.

We also asked the household heads whether their children had seen anyone carrying a
weapon (such as a gun or a knife) as far as they were aware. 10 percent of Controls reported that
a child had seen someone with a weapon (where we asked about up to two randomly selected
children between the ages of 6 and 15 for each family). Interestingly, the results appear to differ
between boys and girls. The changes for boys are statistically indistinguishable from zero, but
the there are reductions for both Experimental and Section 8 Comparisons of 8 percentage points
– relative declines of over 75 percent.

In sum, both the Experimental and Section 8 Comparison group household heads found
their neighborhoods to be safer on most of our self-reported measures than the Controls, with the
magnitude of the improvements generally somewhat larger for the Experimental group. The
large magnitude of the relative changes seems plausible, in the sense that the proportion of
neighborhoods reported as unsafe or the incidence of unsafe events is low among those who
make a subsidized move in the Section 8 Comparison group, and very low among Experimental
group subsidized movers.[20]

Victimization

MTO families report quite high rates of criminal victimization in the baseline survey. For
example, 17.9% of household heads report that they or someone who lives with them had been

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[20] For example, the proportion observing drug activity in the past week was 3 percent among subsidized
Experimental group members, and 18 percent among subsidized Section 8 Comparison group members. In contrast, the
proportion was 27 percent among unsubsidized Experimental, 29 percent among unsubsidized Section Comparisons,
and 36 percent among Controls. It is also generally true that the sign of the difference between unsubsidized
Experimental or Section 8 Comparisons and Controls is negative, which is consistent with a process where those who
take-up the subsidy were those who would have been in a more unsafe neighborhood in the absence of the MTO
program. While the sign of these effects is consistent across outcomes, it is not statistically significant. The correlation
between reporting an unsafe neighborhood in the baseline survey and making a subsidized move is also not statistically
significant.

beaten, assaulted, stabbed, or shot within the past six months. In comparison, a 1994 telephone

survey in public high-rise family developments found that respondents or family members had

experienced an assault or robbery in 8.6% of households (Zelon, 1994). Both surveys may

overstate the true level of victimization if respondents "telescope" and report events that

happened more than six months prior to the survey. In addition, our qualitative interviews

revealed that some MTO household heads thought their answers on the baseline survey might

influence their chance of getting a Section 8 subsidy, which may have encouraged them to over-

report criminal victimization.

We asked MTO household heads in our Boston Follow-up survey about victimization of

themselves and their children during the past six months, 1-3.5 years after enrollment in the

program. Our measures of victimization were modeled on the redesigned National Criminal

Victimization Survey (NCVS). We also ask specifically about the dates of incidents in order to

exclude those occurring more than six months prior to the interview -- although we do not have

the multiple interviews with the same respondent that the NCVS does, which would obviously

improve our ability to exclude old incidents. The Boston Follow-up Survey included selected

NCVS cues about people, places and things intended to assist respondents in recalling events,

such as theft (of a purse, wallet, or jewelry), assault (involving a neighbor, friend, relative, or

someone at work or school), or attacks with a weapon (such as a frying pan, scissors, rock, bottle,

knife, or gun). We classified all reported incidents into personal and property crimes. Personal

crimes include physical attacks and threats of attack, as well as theft directly from a person. We

further classified personal crimes as principally involving the household head, at least one child,

or involving both -- where involvement was defined as being present at the incident and the

person being threatened, robbed, or attacked. Property crimes include household burglary and

attempted entry, vandalism, and theft not directly from a person.

Although the Boston Follow-up Survey gathered many additional details not collected in

the baseline survey, the differences in the questions make it difficult to directly assess how

victimization rates have changed over time between the time of random assignment and the

Boston Follow-up Survey. Perhaps the best comparison is the proportion of households at

baseline with a household member who was beaten, assaulted, stabbed, or shot (17.9%) or had an

attempted break-in at home (17.6%) with the proportion families in the Control group of the

Boston Follow-up Survey with a head or child who were physically assaulted or robbed (10.4%) and the proportion with a completed or attempted break-in to a home (6.3%). Given that other research has found the 1997-98 period during the Boston Follow-up Survey to have had significantly lower youth homicide rates than during 1994-96 (Piehl *et al.* 1999), it is likely that part of the lower incidence of reported victimization does reflects a reduction in violent activities. But differences in the questions and the removal of any perceived incentive on the baseline survey to exaggerate victimization to try and improve one's chances of receiving a portable rent subsidy may also explain some of the differences.

The results in panel A of Table 7 show that household heads in the Experimental and Section 8 Comparison groups have insignificantly lower incidence of any personal crimes in the past six months, relative to the 6 percent victimized in the Control group. Among these personal crimes, there were 8 physical attacks involving household heads, and 4 of these were domestic violence incidents. There were 5 thefts of property directly from a household head. The category with the greatest incidence was verbal or attempted physical assault of heads, with incidents in 13 households; 7 had incidents involving neighbors and 3 had incidents involving domestic disputes.

The incidence of personal crimes with at least one child involved is higher, with 14 percent of Control households having at least one incident within the past six months. Experimental households are only half as likely to have had a personal crime involving a child, and this difference is statistically significant with a p-value of .03. (The p-value is the probability that the coefficient is statistically significantly different from zero, where .05 is a conventional level of significance.) The difference for the Section 8 comparison group is negative and insignificant. These personal crimes among children were largely fights. 28 households had a child assaulted. These assaults generally involved punching, kicking, spitting, shoving, throwing of rocks, or "getting jumped" by a gang. 6 households had theft directly from a child, and 7 had attempted direct theft, and 16 had a threat of violence or attempted assault.

For property crimes, both treatment groups show lower incidence within the past six months. 15 percent of Control households had property crimes, while the Experimental households are 6 percentage points less likely to have had a property crime (p-value of .07) and the Section 8 Comparison households are 9 percentage points less likely (p-value of .005). Of

the 49 households with property crime incidents, 21 had thefts (e.g. of a bicycle on the porch) and 10 had burglaries (e.g. VCR stolen) and 15 had attempted break-ins.

In panel B we report the average number of incidents, as opposed the proportion of household with any incident in panel A. We note that the Boston Follow-up Survey did not ask about the dates of each incident when there were multiple incidents of the same type, making it more likely that some of these incidents took place more than six months prior to the interview. Nevertheless, the results for the average number of incidents are qualitatively similar, with both the Experimental and Section 8 Comparison groups having more than one-third fewer personal crimes involving children and property crimes, and with no significant difference for household heads.

VI. Mental Health, Social Interactions, General Health Status, and Child Behavior Problems

In our qualitative interviews, one of the most striking contrasts we observed between adults who were living in public housing and those who had moved out of public housing was that current public housing tenants appeared to be in much more emotional distress. To take one example, a middle-aged Black woman who had lived in public housing for twenty-five years gave the following description of her experience:

> When you go outside, you don't know what's flying around the corner. You gotta look both ways when you open the front door. You had to constantly look behind you to make sure nothing's comin' around you. ... It was like living in prison. I was afraid to get in my own car and go some place, and come back home and not know if your house would be shot out or what ...

After enrolling in MTO in 1994, she moved to a demographically older and more racially mixed part of Boston. "It's so beautiful. So nice. The neighbors are very friendly. ... I like the peace and quiet. I have peace of mind." A potentially countervailing factor, particularly for Experimental group members, is that they are more likely to be in a neighborhood with few members of the same socioeconomic or racial/ethnic group as themselves, and this could increase social isolation and introduce another type of distress.

In our Boston Follow-up Survey, we asked numerous questions in an attempt to quantify our hypothesis that the Experimental and Section 8 Comparison groups experienced improved

mental health relative to the Control group. First, we examine social relations. In general we find that the level of social contact is similar in all three MTO groups. Second, we find that Section 8 Comparison group members are less likely to have experienced a major depressive episode. Third, we analyze self-reported health measures, and find that both Experimental and Section 8 Comparison group members are more likely to report that they have felt calm and peaceful, and that their overall health is good or better. Fourth, we find that children in both the Experimental and Section 8 Comparison groups were less likely to exhibit behavior problems. Since there is a large literature on the impact of maternal mental health on parenting and child outcomes (Weissman *et* al.1972, Weissman *et* al.1984, Gelfand *et* al.1990), we also speculate that this might be a mechanism by which the program can produce beneficial long-run results for the children as well.

Social Relations

One potential consequence of moving to a new residential location may be that it is more difficult maintain social contact with friends and relatives. In order to assess any differences in social relations caused by participation in the MTO program, we asked several questions in the Boston Follow-up Survey, and results are reported in Table 8.

First, respondents reported the number of times they had visited in person with friends and family in the past month. The point estimates suggest that Section 8 Comparison and Experimental families are more likely to make visits to the homes of others and less likely to have visits to their home, but these differences are statistically insignificant. Combining these two measures to look at net social contact, we examined whether a respondent reported visits at least once a week to their home or to the homes of friends or family, and found small differences between the three MTO groups that were statistically indistinguishable due to sampling error.

To provide some context on MTO participants in comparison to the general population, we also computed the frequency of social contacts for a demographically similar population. To construct a demographically similar population, we used weights based on the odds of participating in MTO versus being a national survey sample respondent, as predicted from the exponential of latent index from a logit model -- where the MTO and national survey data are stacked and race, sex, education and age are used to predict MTO participation. These weights

were then used to analyze responses to questions in the 1987 National Medical Expenditure Survey (NMES). These results show, for example, that among all heads of households with children (or spouses) ages 18-64, the proportion who spoke to close friends or relatives "several days per week or more" in the past month was 57%. Of the demographically weighted NMES respondents, the proportion was 52%. In comparison, 56% of Control group members had spoken to close friends or relatives four times a week or more in the past week. On measures involving visits, the Boston Follow-up Survey questions included visits from and to relatives as well as friends while the NMES asked only about friends, and the frequency of visits in the MTO Control group is somewhat higher than in the NMES. Overall, our conclusion is that the contact with friends and relatives among the MTO participants is fairly similar to the general population and to a demographically similar national sample.

Two other measures of social relations we examined was attendance at religious services and social trust. 57% of the Control group reported having been to a church or place of worship within the past 30 days. The point estimate of the difference between the Experimental group and the Control group was essentially zero, and the difference between the Section 8 Comparison group was negative and statistically insignificant. The MTO sample appears to go to a place of worship with the about same frequency (56%) as the general population of adults with children in the General Social Survey (GSS) from 1990-96. Weighting the GSS by sex, age, race, education, the percentage was 51%.

Our indicator of social trust was agreement with the statement "Most people can be trusted" as opposed to "You can't be too careful in dealing with people." The answer to this question has been shown by other researchers to be highly correlated with other aspects of social engagement, such as membership in civic organizations, voting, and spending time with neighbors (Putnam 1995). Only 7.8% of the Control group report that most people can be trusted. We find that the Experimental group exhibits significantly more social trust than the Control group, a difference of 6.5 percentage points where the p-value on the difference is .051. The difference between the Section 8 Comparison and Control groups is also positive, but the magnitude is smaller at 3.8 percentage points and the p-value is .357. In contrast to the previous indicators of social relations, the MTO sample differs substantially from the general population of adults with children, of whom 31% report that most people can be trusted in the GSS from

1990-96. This difference is largely due to demographic factors, however. Weighting the GSS by the sex, race, education, and age of the MTO sample, only 9.3% report that most can be trusted.

Adult Mental Health

In order to work with a clear definition of mental health, we focused our survey questions on symptoms of a particular psychiatric condition, major depressive episode (MDE), which is defined in the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) as having several particular symptoms within a two week time period[21]. To inquire about MDE symptoms, we used a series of questions known as the Composite Diagnostic Interview Short Form (CIDI-SF). The questions in the Short-Form were chosen for their correspondence with a more comprehensive battery of MDE questions from the National Comorbidity Survey (Kessler *et* al. 1994, 1998). In the sequence of MDE questions, respondents are asked whether they have be "sad, blue, or depressed" or have "lost interest in most things" for any two week period within the past 12 months in which the symptoms lasted at least half the day for almost everyday or more during the two week period. For respondents indicating either depression or loss of interest, they are then asked if they have the following additional symptoms during the two weeks: more tired out or low on energy, weight gain or loss of more than 10 pounds, trouble falling asleep nearly every night, more trouble concentrating than usual, feeling down on self/worthless/no good, or thinking about death. Also, anyone volunteering that they are taking prescribed medication for depression is classified as having the maximum number of symptoms. Based on their work with the more comprehensive National Comorbidity Survey, Nelson *et* al. (1998) suggest two scoring rules. First, they find that 3 symptoms is the cutoff level above which the majority of respondents are likely to be classified as MDE cases. Second, they develop a rule for predicted MDE cases by assigning a probability to respondents with a particular number of symptoms.

In Table 9, we report that the proportion of the Control group with 3 or more symptoms is 27%, while the proportion in the Section 8 Comparison group was nearly cut in half, 13 percentage points lower. Experimental group was 6 percentage points lower, but this difference

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[21] See Marsella, Hirschfeld, and Katz (1985) for a discussion of other depression scales.

was not significantly different from zero. The results are essentially the same for predicted MDE cases as for having 3 or more symptoms. Although we did not find systematic differences between the Experimental and Section 8 Comparison groups in our analysis of social contact, we speculate that the reduction in MDE resulting from the opportunity to make a subsidized move out of public housing in the Experimental group may have been partially offset by difficulties from more moves to more unfamiliar neighborhoods than made by the Section 8 Comparison group.

The MTO sample appears to have a much higher prevalence of MDE symptoms than a national population of similar age, race, education, and sex. In tabulations from the CIDI-SF administered in the 1997 National Household Survey of Drug Abuse weighted to reflect the demographics of the MTO sample, only 13% reported having three or more symptoms, as opposed to 27% of the MTO Control group. In the general population of adults ages 18-64 with children, the prevalence of 3 or more MDE symptoms was only 9%. In the 1997 Women's Employment Survey in Michigan, however, a population of single mothers receiving cash assistance were found to have a predicted MDE cases of 27%, within sampling error of the 25% for the MTO Control group (Danziger *et* al. 1999). This suggests that the Control group is actually fairly typical of very low-income single mothers, and that the high rate of MDE is not an exaggerated response to the interviewer in the hope that it will somehow improve their prospects for receiving subsidy assistance in the future.

In order to explore other aspects of mental health in addition to depression, we asked two questions about calmness and happiness used in the evaluation of the RAND Health Insurance Experiment and elsewhere to assess positive affect. We found that the 47% of the Control group reported they had "felt calm and peaceful" a good bit of the time or more in the past four weeks. The proportion was 10 percentage points higher in the Experimental group (p-value of .058) and 14 percentage points higher in the Section 8 Comparison group (p-value of .029). The differences with the Control group are larger for lower frequencies of calmness (such as a little or some of the time as opposed to most of all of the time), suggesting that the impact of MTO is to move respondents out of the most highly anxiety producing situations. The point estimates for happiness are of the same sign but of smaller magnitude and are not statistically significant. A sample from the 1987 NMES weighted to reflect the demographic composition of the MTO

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participants was more likely to be calm and peaceful a good bit of the time or more (57% versus 46%) and was more likely to be happy a good bit of the time or more (69% versus 56%). The differences are even larger between MTO participants and the general population.

These differences in mental health between the Control and treatment groups also appear to be reflected in self-reported overall health. 58% of the Control group reported that they were in good or better health. The proportion report good or better health in the Experimental group was 11 percentage points higher, while the difference was 18 percentage points for the Section 8 Comparison group, both of which were statistically significant.

Children's Mental Health

To assess the impact of residential location on the mental health of children, we asked household heads if their children had exhibited various behavior problems during the past three months. The questions have been fielded previously by in the National Health Interview Survey and the National Longitudinal Survey of Youth Children's Supplement. We selected a particular subset of questions that focused more on behaviors that the mothers could observe, as opposed to questions requiring intuition about how their child was feeling. The questions were about: trouble getting along with teachers, disobedience at home, disobedience at school, hanging around with troublemakers, bullying others, inability to sit still, or depression.

We present results in Table 10 separately for boys and girls because they turn out to be strikingly different. The boys generally had a higher level of behavior problems in the Control group, and had much larger reductions in behavior problems in the Experimental and Section 8 Comparison groups. The two specific behavior problems with the largest effects were cruelty to others and depression. While it was no surprise that boys were much more likely than girls to be cruel or bully others, it is interesting that boys were also more likely to be sad, unhappy or depressed. We have aggregated the information from our seven indicators and also report the effects for the fraction of the behavior problem questions that were answered affirmatively by the household head. On average, boys in the Control group had exhibited 33 percent of behaviors about which we inquired, while girls exhibited 19 percent. There was a statistically significant reduction of about 10 percentage points in both the Experimental and Section 8 Comparison groups, and only a negligible reduction among the girls. Notably, this concentration of effects in

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reducing behavior problems among boys and not girls was also found in the recent evaluation of the New Hope employment assistance demonstration in Milwaukee (Bos *et* al. 1999).

The differences between boys and girls are not restricted to behavior problems. Boys in the Control group were also much less likely to read for enjoyment every day than girls (.29 versus .56), but the beneficial impact of being in the Experimental or Section 8 Comparison group was larger for boys than for girls. In analyzing whether the child reads "several times per week or more" (.74 of the Control group) instead of "every day" (.44 of the Control group), the effects are larger yet for boys than girls in percentage points. Pooling boys and girls together, the effects on reading several times per week or more are positive and statistically significant for both the Experimental group (p-value .015) and the Section 8 Comparison group (p-value .073).

One mechanism through which these differences may be occurring is through social contact with other children. Most boys (.77) and girls (.80) in the Control group have at least one close friend in the neighborhood, and there are small differences between the three MTO groups among boys. However, girls in the Experimental group are 12 percentage points less likely have a friend, and Section 8 Comparison group girls are 14 percentage points less likely. We speculate that these socialization difficulties for girls may offset some of the benefits for mental health of living in a safer neighborhood.

VII. Applying the Econometric Interpretation: Depression

As discussed in Section III, drawing inferences about causal effects of neighborhoods from the MTO program requires careful attention to identification issues. This section focuses on one outcome, depression (specifically incidence of three or more symptoms), as an extended example. Recall from the previous section that the Intent-To-Treat effect for depression is -.128 for the Section 8 Comparison group and -.058 for the Experimental group. Since it is the Section 8 Comparison group that experienced a significant effect, Table 11 focuses on this group. (Note that Table 11 has a different structure than previous tables; the dependent variable differs across the columns).

First, we introduce a large number of covariates X into the regression to adjust for random variation in Baseline characteristics between the three treatment groups. In addition to age, race, sex, and family composition, we attempt to parsimoniously summarize Baseline survey

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information we felt was relevant by constructing indices of the proportion of affirmative responses to groups of questions about neighborhood social interactions, social order, victimization, living conditions, convenience of location, enthusiasm for moving, and involvement in children's schools. The estimates are substantively similar. For the Section 8 Comparison group, the ITT without X is -.128, and with X is -.135 from a linear probability model and .124 from a probit model. In all cases, the p-values are less than .05. For the Experimental group, the magnitude of estimates with covariates are also quite similar, and still negative and statistically insignificant. For simplicity of exposition, we report additional results below that do not include covariates, but their inclusion does not result in substantive differences.

Under the assumption discussed in Section III that those who were offered a subsidy but did not take-up had zero treatment effect, we can identify the effect of Treatment-On-Treated. To compute the effect of Treatment-On-Treated (TOT) for depression, we can simply divide the ITT estimates by the proportion who took-up the subsidy from row 1 of Table 11, in the first two columns respectively. For the Section 8 Comparison group, the TOT effect is -.128/.60 = -.221, as reported in row 3. Our interpretation of these results is that the incidence of depression was 22 percentage points lower for Section 8 Comparison group members who took the offered MTO subsidy than it would have been if they had not been offered the subsidy. For reference, the incidence of depression in the Control group was .276.

To help understand the relationship between depression and residential location, we use the neighborhood poverty rate as an outcome in column 3 of Table 11. We can similarly compute the TOT effect for the change in the Census tract poverty rate, shown in column 3. The purpose of this is to help characterize the types of neighborhoods to which MTO families were caused to move. For the Section 8 Comparison group, this difference was 15.6 percentage points, and 25.5 percentage points for the Experimental group. Since we have seen that the TOT effects on depression are higher for the Section 8 Comparison group, this combined pattern of results suggests that moves to moderately less poor neighborhoods (the experience of the Section 8 Comparison group) had a more beneficial impact on depression than the moves to substantially less poor neighborhoods (the Experimental group).

When comparing the TOT results for the Experimental and Section 8 Comparison groups,

however, it is also important to keep in mind that characteristics of individuals successfully "taking-up" a program subsidy are not the same for the two groups. One way to summarize these characteristics is by computing the predicted probability of take-up using sets of coefficients estimated separately from the Experimental sample and from the Section 8 Comparison sample. The correlation of the predicted probabilities estimated from these two samples is 0.36. The specific characteristics for which take-up behavior differs are discussed in Section IV.

Another way to assess the relationship between the treatment effect for depression and the characteristics of the neighborhoods to which the treated individuals moved is to look within the two MTO groups to see if families who were likely to make larger changes in poverty rates had different treatment effects for depression. To do this, we used Baseline survey characteristics having a significant raw correlation with changes in poverty rates or with program take-up to construct predicted changes in neighborhood poverty rates from an auxiliary regression on a sample who took up an MTO subsidy. We then split the sample into two halves with larger and smaller predicted changes, effectively interacting the treatment indicators with a function of X.

Within the Section 8 Comparison group, the results show that the subsample predicted to have larger changes had a TOT effect of reducing their neighborhood poverty rate by 22.3 percentage points, and the TOT effect on depression was -.306 (p-value .01). For the group predicted to have smaller changes had a TOT effect of reducing their neighborhood poverty rate by 10.6 percentage points, and the TOT effect on depression was -.129 (p-value .28). Within the Experimental group, the effects on poverty rates were of greater magnitude for both subsamples with larger and smaller predicted changes (TOT effects of -30.2 and -18.5). The subsample with the smaller predicted change had a larger point estimate of the TOT effect on depression, but the standard errors were too large to reliably distinguish them from each other or from zero. Overall, estimates are consistent with the interpretation that moving to neighborhoods with a lower poverty rate reduces depression up to a point, but that moves to neighborhoods very different from the origin neighborhood defray some of this impact. The difficulty in interpreting these results is that the differences may be due to the neighborhoods or they may be an interaction of the treatment effect with the Baseline characteristics. These two explanations are not directly distinguishable in our data.

Since the TOT effects for depression are quite large relative to the incidence in the

Control group, we tried to assess their plausibility. We hypothesized that those who chose to take-up the subsidy would have had relatively higher rates of depression if they had instead been assigned to the Control group. To test this, we used a probit model to generate a predicted probability of program take-up (with the same covariates as used to generate the predicted change in neighborhood poverty rate). Notably, the predicted probability of take-up was a quite low correlation of -.076 (p-value .08) with change in neighborhood poverty rate, even though the same covariates were used in both prediction equations. This implies that the factors affecting the decision to take-up are substantially different than those affecting the decision about how far to move.

We then split the sample at the median to form groups with high and low probabilities of taking-up. The average take-up was .35 for the low group and .77 for the high group. For Controls in low predicted take-up group, the incidence of depression was .22. For Controls in the high predicted take-up group, the incidence of depression was .33 (p-value on difference is .10). We interpret this as suggestive evidence that treatment group members who took a subsidy would have had a higher rate of depression in the counterfactual state of not having taken the subsidy. This evidence is consistent with the depression incidence among those in the treatment groups who did not take-up a subsidy. Relative to the .276 level in the Control group, depression among those who did not take-up an MTO subsidy was .110 (p-value .02) lower in the Experimental group and .085 lower (p-value .21) in the Section 8 Comparison group.

Interpreting 2SLS

Another potential way to model the relationship between the neighborhood poverty rate and depression would be to use two-stage least squares (2SLS), treating the poverty rate as endogenous and using the treatment group indicators as excluded instruments. As mentioned in section III, the interpretation of such an exercise in this application should not be that the results represent the marginal effect of changing the poverty rate, but rather should be thought of as a representative metric for an entire bundle of neighborhood characteristics that are changing. More generally, the 2SLS projection of the treatment indicators onto poverty rates rescales the same information (a linear combination of the ITT effects), so it is useful to consider this in some detail.

Consider what underlies the 2SLS computation in an example where the neighborhood poverty rate change is represented by the two indicators P_1 and P_2.

P_1: 1(neighborhood poverty rate change less than -30)

P_2: 1(neighborhood poverty rate change between -30 and -1)

Z_1: 1(Experimental group)

Z_2: 1(Section 8 Comparison group)

M_{Z2}: $(I - Z_2(Z_2'Z_2)^{-1}Z_2')$ residual-maker matrix

Structural equation:

$$Y = P_1\beta_1 + P_2\beta_2 + \varepsilon$$

First stage: Note that empirically, the effect of Z_1 on P_2 is .004; assuming 0 simplifies calculations and gives essentially the same results.

$$P_1 = Z_1\gamma_{11} + Z_2\gamma_{12} + \eta_1$$
$$P_2 = Z_1 \cdot 0 + Z_2\gamma_{22} + \eta_2$$

Second stage:

$$Y = P_1\beta_1 + P_2\beta_2 + v = (Z_1\hat{\gamma}_{11} + Z_2\hat{\gamma}_{12})\beta_1 + (Z_2\hat{\gamma}_{22})\beta_2 + \varepsilon$$

Reduced form:

$$Y = Z_1\pi_1 + Z_2\pi_2 + \upsilon$$

β_1 can be estimated from Experimental versus Control difference after partialling out Z_2.

$$M_{Z_2}Y = M_{Z_2}(Z_1\hat{\gamma}_{11} + Z_2\hat{\gamma}_{12})\beta_1 + M_{Z_2}Z_2\hat{\gamma}_{22}\beta_2 + \varepsilon$$
$$Z_1 \perp Z_2 \Rightarrow M_{Z_2}Y = Z_1\hat{\gamma}_{11}\beta_1 + \varepsilon$$

$$\beta_1 = \pi_1 / \gamma_{11} = -.058/.297 = -.195$$

Thus, β_1 is the ITT effect for the Experimental group, rescaled by the proportion who make a

large change in their neighborhood poverty rate in comparison to the Control group (as opposed to the TOT effect which rescales by the proportion who take-up a subsidy). To estimate the coefficient on P_2, think of the second stage after taking out effect of P_1.

$$(Y - P_1\hat{\beta}_1) = Z_2\hat{\gamma}_{22}\beta_2 + \varepsilon$$
$$\beta_2 = (\pi_2 - \gamma_{12}\beta_1) / \gamma_{22} = (-.128 - .152(-.195))/.172 = -.572$$

The small deflators make the 2SLS point estimates look very large ($\beta_1 = -.17$, $\beta_2 = -.57$). Recall that the TOT effects were about -.13 and -.22 for the Experimental and Section 8 Comparison groups respectively. In particular, the computation makes two assumptions that probably do not hold. The first is that Experimental families who take-up a subsidy but only end up in a moderately less poor neighborhood ($P_2=1$) will have the same outcome as a Control family in a moderately less poor neighborhood. This ignores the fact that most Control families who move out of public housing projects pay full market price for their new apartments, and probably cannot afford units as desirable as those obtained by the subsidized Experimental group members within the same range of neighborhood poverty rate. The computation also assumes that the effect of P_1 is the same for the Experimental and Section 8 Comparison groups. Since the Section 8 Comparison group was unconstrained in their choice of neighborhood, those who did choose to make a large change in their neighborhood poverty rate were probably better suited to cope with it than those who were constrained to do so in the Experimental group.

The point of this example is to show how 2SLS exactly how the reduced form ITT estimates are combined and rescaled to project them onto poverty rates. The same intuition applies when the poverty rate is specified with linear and quadratic terms. The results are also substantively similar, with the effect rising for moderate changes in neighborhood poverty and declining for larger changes. In essence, this 2SLS is another way of representing the fact that the Experimental TOT effect for depression is positive and smaller than the Section 8 Comparison TOT effect, and the Experimental TOT effect for neighborhood poverty change is larger than the Section 8 Comparison TOT effect – but the assumptions necessary to interpret 2SLS as the effect of a marginal change in the poverty rate appear to be violated in this application.

VIII. Short-term impacts on household earnings, welfare participation, and total income.

[revision to incorporate an additional two years of earnings and welfare data in progress]

IX. Cost benefit analysis

[In progress.]

IX. Conclusion

This paper provides preliminary estimates from an on-going investigation of the early impacts of the MTO housing mobility demonstration on families originally residing in high-poverty housing projects in inner-city Boston. We find that approximately half (45 percent) of the households in the MTO treatment group (those randomly assigned to have access to rental assistance restricted to be used in a low-poverty area and provided with counseling assistance to help make such a move) have been able to use the program to move to a qualified neighborhood. In comparison, 59 percent of those provided with less-restrictive, regular Section 8 vouchers or certificates have been able to move through the program. The MTO treatment gets movers into wealthier communities and is more successful than Section 8 in relocating poor families into middle-class (low-poverty) and suburban neighborhoods. Regular Section 8 assistance is more effective in getting a larger share of families out of the most distressed communities (those with very high poverty rates). Data from a baseline survey of program participants at all five MTO sites and extensive qualitative interviews with a smaller sample of participants in Boston indicate that fear of crime (drugs and gangs) and concerns about personal and family safety are the primary reasons motivating desires to move out of public housing in high-poverty, inner-city areas.

We find that both the Experimental and Section 8 Comparison groups report fewer property crimes, and Experimental group children are less likely to be victims of a personal crime. The subsidized mobility opportunity has resulted in significant improvements in the general health status and mental health of household heads, and a reduction in behavior problems among children.

Appendix: Research Methodology

We gathered information about the program using four methods. We conducted field work to observe the operations of the program. We conducted qualitative interviews with a random sample of program participants. We conducted a survey of 525 MTO families and will be returning to the field to expand this sample in the near future. Finally, we analyzed data on census tract and block group characteristics from the STF files of the 1990 Census of Population linked to our sample participants using geocoded addresses to describe the attributes of the neighborhoods of MTO families.

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, and their perceptions of their old and new (if they moved) neighborhoods. 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. The survey was written by our research team and administered in mixed modes (by telephone and in-person) by Westat Inc. The questions were modeled after questions in existing surveys. The survey was administered in both Spanish and English.

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Table 1
MTO Demonstration Family Characteristics by Site from the MTO Baseline Questionnaire
Tabulations Through April 1996 Data Release
(in percentages)

	Baltimore	Boston	Chicago	Los Angeles	New York
Household Heads					
Age < 30	30.6	34.4	41.0	35.8	28.3
Age 30-39	50.1	39.4	43.4	35.2	38.5
Female	97.8	90.3	93.4	87.0	94.7
White, not Hispanic	0.2	10.0	0.0	0.0	0.4
Black	95.4	39.7	96.7	56.2	52.5
Asian	0.0	4.2	0.0	2.5	0.4
Hispanic	2.0	42.8	1.4	41.4	46.8
0 or 1 child	19.4	22.0	18.7	13.4	15.7
2 children	34.2	28.0	26.2	23.8	32.6
3+ children	46.4	50.0	55.1	62.8	51.7
Never married	71.4	59.3	65.9	57.3	49.4
Married	3.4	11.8	3.7	19.5	11.6
HS degree or GED	57.3	63.7	52.1	37.8	64.1
Employed	24.5	24.7	17.8	21.3	24.5
Getting AFDC (Welfare)	79.6	67.6	84.1	84.1	69.7
Mother got AFDC	52.4	40.1	68.2	39.9	46.8
Have a car that runs	3.6	19.5	8.9	38.0	7.5
Main Reason Want to Move					
Crime (drugs, gangs)	51.5	56.3	50.5	62.6	46.1
To get better apartment	26.7	29.1	19.6	9.8	31.5
Better schools	12.4	7.4	20.1	20.3	13.5
Get job or be near job	1.2	0.8	3.7	1.2	0.8

Table 1 (cont'd)
MTO Demonstration Family Characteristics by Site from MTO Baseline Questionnaire
Tabulations Through April 1996 Data Release
(in percentages)

	Baltimore	Boston	Chicago	Los Angeles	New York
Baseline Apartment and Neighborhood					
Apt. in fair/poor condition	68.5	76.1	64.5	74.8	72.8
Too little apt. space is a big problem	58.7	50.0	43.0	38.0	66.7
Dissatisfied with nghd.	80.8	58.5	72.8	63.2	66.8
Streets unsafe during the day	60.4	55.4	68.7	69.9	68.7
Drug dealers big problem	93.2	73.4	93.5	81.6	94.0
Criminal Victimization of Family in Past 6 months					
Purse, wallet, jewelry snatched	20.4	11.8	22.0	35.0	29.6
Threatened by knife or gun	28.2	15.9	23.4	25.8	26.6
Beaten or assaulted	26.0	14.8	28.0	22.1	25.1
Stabbed or shot	12.9	8.2	9.8	9.8	9.4
Someone tried to break into home	23.8	18.7	21.0	47.9	18.0
Children 6-17					
Special class or help for learning problems	16.0	20.1	10.9	14.1	21.6
Sample Size	412	364	214	163	267

Notes: Data are from the responses to the MTO Participant Baseline Survey of the first 1420 families to be randomly assigned in MTO from October 1994 to October 1995. The MTO Participant Baseline Survey is described and documented in Feins (1994) and Abt Associates (1995).

Table 2
Boston MTO Demonstration Household Head Characteristics
at Program Entry (Baseline Survey) by Treatment Status
(in percentages)
Families Entering Demonstration Through May 30, 1996

	MTO	Section 8	Control
Entire Sample (n=540)			
Age < 30	24.2	24.2	27.8
Female	92.5	91.7	89.3
White, Not Hispanic	11.7	10.0	7.8
Black	38.3	36.7	38.2
Hispanic	43.8	40.8	44.4
Never Married	56.5	63.7	61.4
HS Degree or GED	62.3	70.8	69.3
Employed	27.9	32.5	28.5
Any Children, 0-5 years	61.7	63.3	63.9
Any Children, 6-17 years	79.2	78.3	76.7
Sample Size	240	120	180

Table 3
Mean Characteristics of Origin Census Tracts of Boston
MTO Families Entering the Demonstration Through May 30, 1996
(in percentages)

	MTO	Section 8	Control	All
Entire Sample				
Poverty Rate	41.0	40.8	41.6	41.1
White	33.8	35.2	35.2	34.6
Black	46.3	42.6	44.1	44.8
Hispanic	26.1	29.0	27.0	27.0
English not 1st Language	34.1	37.4	35.3	35.2
HS Dropout Share, 25 years and older	45.6	46.0	46.0	45.8
Unemployment Rate	8.9	8.9	8.9	8.9
% of Households on Welfare	32.9	33.4	33.4	33.2
Sample Size	240	120	180	540

Notes: Census tract characteristics are from the STF files of the 1990 Census of Population.

Table 4
Mean Mobility Outcomes and Final Census Tract and Block Group Characteristics
of Boston Follow-up Survey Sample

	MTO	Section 8	Control
Mobility Outcomes (in percent)			
Program Move	45.3	59.3	0.0
Move Out of Project	58.9	68.5	27.8
Living Outside Boston	28.3	11.8	4.9
Final Census Block Group Characteristics (in percent)			
Poverty Rate	25.0	27.3	38.8
Poverty Rate < 10%	29.8	14.9	2.4
Poverty Rate < 40%	75.8	79.9	49.5
Final Census Tract Characteristics (in percent)			
Poverty Rate	23.7	25.9	35.9
Poverty Rate for Movers	15.5	20.9	27.6
Poverty Rate for Non-Movers	39.8	40.8	41.1
Poverty Rate < 10%	36.6	13.4	1.8
Poverty Rate < 20%	46.8	36.5	12.8
Poverty Rate < 30%	61.9	66.5	31.8
Poverty Rate < 40%	68.7	74.7	40.7
Income > 2x Poverty Line	58.5	54.1	41.3
Racial Composition			
White	52.6	44.0	38.0
Black	35.5	39.1	44.9
Hispanic	16.1	20.1	21.8
English not 1st language	26.1	32.8	31.4
Speak almost no English	7.2	9.7	9.7
Immigrant	14.6	19.1	14.8
% of Families that are female-headed	39.6	43.8	53.1

Table 4 (cont'd)
Mean Mobility Outcomes and Final Census Tract and Block Group Characteristics
of Boston Follow-up Survey Sample

	MTO	Section 8	Control
Education, 25 years and older			
Dropout	32.3	35.4	42.6
At least some college	35.3	35.5	28.9
Unemployment Rate	6.8	7.3	8.6
Full-Time, Full-Year Workers	39.8	39.2	32.7
% Households with			
Public Assistance	19.7	22.6	29.4
Earnings	72.1	71.7	64.2
% Workers Using Public Transport	28.3	32.8	38.7
% Persons in Owner-Occupied Units	37.1	28.6	17.3
Median Household Income (1989 dollars)	25550	23132	16726
Median Gross Rent (1990 dollars)	495	482	355
Sample Size	234	114	176

Notes: Tabulations for 525 Boston families with completed MTO Boston Telephone
Surveys and verified final addresses. Reported means are weighted to reflect differences
in random assignment ratios in Boston before and after February 1, 1996. 437
observations entered before February 1, 1996 and 88 entered after. All census tract and
block group characteristics are from the 1990 Census of Population.

Table 5
Differences in Mobility Outcomes and Final Census Tract Characteristics
by Treatment Status
Boston Follow-up Survey Sample (n=525)

	MTO– Control	Sec. 8– Control	MTO– Sec. 8
Mobility Outcomes			
Program Move	45.3	59.3	-14.0
	(4.0)	(4.9)	(4.6)
Move Out of Project	31.1	40.8	-9.6
	(4.9)	(5.7)	(5.8)
Living Outside Boston	23.4	6.9	16.5
	(3.6)	(4.3)	(4.1)
Final Census Tract Characteristics			
Poverty Rate	-12.2	-10.0	-2.1
	(1.4)	(1.7)	(1.6)
Poverty Rate < 10%	34.8	11.6	23.2
	(3.7)	(4.4)	(4.2)
Poverty Rate < 40%	28.0	34.1	-6.1
	(4.7)	(5.6)	(5.4)
Racial Composition			
White	14.5	5.9	8.6
	(3.6)	(4.3)	(4.1)
Black	-9.4	-5.8	-3.6
	(3.3)	(3.9)	(3.7)
Hispanic	-5.7	-1.7	-4.0
	(1.7)	(2.0)	(1.9)
English not 1st language	-5.4	1.3	-6.7
	(1.7)	(2.1)	(2.0)

Table 5 (cont'd)
**Differences in Mobility Outcomes and Final Census Tract Characteristics
by Treatment Status in the Boston Follow-up Survey**

	MTO– Control	Sec. 8– Control	MTO– Sec. 8
% of Families that are Female-Headed	-13.5	-9.3	-4.2
	(1.6)	(1.9)	(1.8)
Education, 25 Years and Older			
Dropout	-10.2	-7.2	-3.0
	(1.3)	(1.6)	(1.5)
At least some college	6.4	6.6	-.23
	(1.2)	(1.5)	(1.4)
Unemployment Rate	-1.7	-1.2	-0.5
	(0.3)	(0.3)	(0.3)
% Households with Public Assistance	-9.7	-6.8	-2.8
	(1.2)	(1.4)	(1.4)
% Workers Using Public Transport	-10.5	-6.9	-3.5
	(1.5)	(1.8)	(1.7)
% Persons in Owner-Occupied Units	19.8	11.3	8.5
	(2.1)	(2.6)	(2.4)
Median Household Income (1989 dollars)	8823	6406	2417
	(1046)	(1263)	(1200)

Reported estimates are differences in the weighted means reported in Table 4. Standard errors are in parentheses.

TABLE 6
IMPACT OF MTO ON SAFETY

	Control	Exp - Control	Sec8 - Control	N
Streets near home are unsafe or very unsafe during the day	.386 (.038)	-.167** (.047)	-.075 (.060)	508
Seen people using or selling drugs once a week or more	.359 (.038)	-.203** (.045)	-.131** (.056)	506
Seen or heard gunfire at least once in the past six months	.411 (.038)	-.176** (.048)	-.126** (.059)	512
Seen or heard gunfire once a month or more	.205 (.032)	-.132** (.036)	-.105** (.044)	512
Boys: if seen someone with weapon during the past three months	.101 (.037)	-.047 (.043)	.011 (.061)	261
Girls: if seen someone with weapon during the past three months	.104 (.031)	-.080** (.034)	-.078** (.037)	297

Note: robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 7

IMPACT OF MTO ON CRIMINAL VICTIMIZATION

	Control	Exp - Control	Sec8 - Control
A. Proportion of households with one or more incidents			
Personal crimes: household head involved	.052 (.017)	-.006 (.025)	-.026 (.022)
Personal crimes: at least one child involved	.121 (.025)	-.064** (.029)	-.053 (.035)
Property crime	.139 (.027)	-.060* (.032)	-.093** (.033)
Any crime	.249 (.033)	-.070 (.044)	-.098** (.047)
B. Average number of incidents for head and children			
Personal crime: household head involved	.077 (.027)	-.016 (.038)	-.046 (.032)
Personal crime: at least one child involved	.196 (.044)	-.112** (.049)	-.092 (.059)
Property crimes	.200 (.043)	-.089* (.050)	-.137** (.051)
All crimes	.446 (.069)	-.161* (.085)	-.218** (.092)

Personal crimes: assault, rape, robbery, pick pocketing (attempted or completed)
Property crimes: theft, household or motor vehicle burglary (attempted or completed)
A "series" of incidents (such as "my child's bicycle was stolen several times") was counted as only two incidents unless specific details of separate multiple instances were given.
N = 519 households; robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 8
IMPACT OF MTO 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 (.039)	-.056 (.053)	-.055 (.062)	509
Visited with a friend or relative at their home at least once a week in the past month	.422 (.038)	.082 (.053)	.055 (.062)	512
Visited with a friend or relative at their home or with friend or relative at your home at least once a week in the past month	.636 (.038)	-.020 (.051)	-.037 (.061)	508
On the telephone with close friends or relatives 4 times or more in the past week	.561 (.038)	.018 (.053)	.075 (.061)	508
Went to church or place of worship at least once in the past 30 days	.573 (.038)	-.007 (.053)	-.060 (.062)	510
Agree with: "Most people can be trusted" versus "You can't be too careful in dealing w/people"	.078 (.022)	.065* (.033)	.035 (.038)	499

Note: robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 9
IMPACT OF MTO ON MENTAL HEALTH

	Control	Exp - Control	Sec8 - Control	N
3 or more symptoms of Major Depressive Episode within past 12 months	.274 (.035)	-.058 (.044)	-.128** (.048)	516
Predicted probability of having had a Major Depressive Episode within past 12 months	.249 (.031)	-.048 (.039)	-.093** (.045)	516
Calm and peaceful "a good bit of the time" or more often during the past four weeks	.465 (.039)	.100* (.052)	.136** (.062)	508
Happy "a good bit of the time" or more often during the past four weeks	.561 (.039)	.069 (.052)	.035 (.062)	506
Overall health is good or better	.578 (.038)	.113** (.050)	.180** (.056)	511

Note: robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 10
IMPACT OF MTO ON OUTCOMES FOR CHILDREN AGES 6-15

		Control	Exp - Control	Sec8 - Control	N
Cruel or is mean to others	Boys	.199 (.046)	-.147** (.049)	-.132** (.066)	270
	Girls	.073 (.026)	-.019 (.032)	-.030 (.036)	302
Is unhappy, sad, or depressed	Boys	.298 (.051)	-.123* (.064)	-.187** (.067)	270
	Girls	.222 (.041)	-.024 (.058)	-.011 (.068)	302
Fraction of 7 behavior problems	Boys	.332 (.032)	-.102** (.040)	-.096* (.050)	270
	Girls	.194 (.023)	-.014 (.031)	-.031 (.034)	302
Reads for enjoyment every day	Boys	.290 (.049)	.062 (.070)	.098 (.089)	269
	Girls	.559 (.049)	-.067 (.070)	.024 (.077)	299
Reads for enjoyment several days a week or more	Boys	.651 (.053)	.146** (.065)	.105 (.085)	269
	Girls	.817 (.040)	.089* (.049)	.078 (.056)	299
At least one close friend in neighborhood	Boys	.771 (.051)	-.002 (.072)	.029 (.076)	268
	Girls	.801 (.044)	-.118* (.066)	-.135* (.078)	299

Note: robust standard errors are reported in parentheses.
* = p-value <.1; ** = p-value < .05

TABLE 11

INTENT-TO-TREAT EFFECTS OF SECTION 8 COMPARISON ON 3+ SYMPTOMS OF DEPRESSION

	Depression	Take-up	Change in Poverty Rate	N
Section 8 Comparison - Control	-.128 (.048)	-.600 (.046)	-9.3 (1.7)	285
Controlling for covariates	-.135 (.049)	.558 (.048)	-8.6 (1.7)	285
Subsample with predicted poverty rate change at or below median (-46 to -29.7)	-.191 (.069)	.624 (.067)	-14.4 (2.5)	137
Subsample with predicted poverty rate above median (-29.7 to +4)	-.072 (.068)	.556 (.068)	-5.9 (2.0)	148
Subsample with predicted take-up at or below median	.029 (.076)	.352 (.067)	-7.8 (2.3)	147
Subsample with predicted take-up above median	-.268 (.062)	.776 (.055)	-11.1 (2.3)	138

Note: The three columns of coefficients represent separate linear regressions, reporting the difference between the Section 8 Comparison and the Control group. The dependent variables for the three columns are Depression (3 or more symptoms), Take-up (if moved using a subsidy offered by the program), and Change in Poverty Rate (difference between initial and final Census tract poverty rate). Robust standard errors are reported in parentheses.

The covariates included in the regressions in the second row are: age, race, sex, and family composition, indices of the proportion of affirmative responses to groups of questions about neighborhood social interactions, social order, victimization, living conditions, convenience of location, enthusiasm for moving, and involvement in children's schools.

Predicted take-up is the predicted probability from a probit model where the dependent variable was moving using a subsidy offered by the program. The covariates included were those found to have a raw correlation with making a subsidized move or with the change in neighborhood poverty rate within the sample of subsidized movers.