

# MOVING TO OPPORTUNITY IN BOSTON: EARLY RESULTS OF A RANDOMIZED MOBILITY EXPERIMENT\*

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We examine short-run impacts of changes in residential neighborhoods on the well-being of families residing in high-poverty public housing projects who received Section 8 housing vouchers through a random lottery. Households offered vouchers experienced improvements in multiple measures of well-being relative to a control group, including increased safety, improved health among household heads, and fewer behavior problems among boys. There were no significant short-run impacts of vouchers on the employment, earnings, or welfare receipt of household heads. Children in households offered vouchers valid only in low poverty neighborhoods also had reduced likelihood of injuries, asthma attacks, and victimizations by crime.

## 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]. Federal housing policies which funded large high-rise public housing developments in central cities during the 1950s and 1960s [Von Hoffman 1996] and which in the 1980s targeted assistance on the poorest eligible households [Epp 1996] are likely to have contributed to this trend.

The concentration of poverty in inner cities, along with recent increases in residential segregation by family income and the

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persistence of residential segregation by race in U. S. metropolitan areas [Cutler, Glaeser, and Vigdor 1999; Jargowsky 1996], 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 on a wide variety of outcomes than those who grow up with more affluent neighbors even in studies that include detailed controls for family income and background characteristics (e.g., Brooks-Gunn et al. [1993]).

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 and safety from crime. The willingness of families to pay substantial house price and rental premiums to move to wealthier neighborhoods and gain access to better schools strongly suggests that typical citizens believe that neighborhood and school social composition affect their children's life prospects.<sup>1</sup> Furthermore, assumptions of causal neighborhood and peer effects are central to theories of social capital [Coleman 1988] and to models of human capital externalities and growth [Bénabou 1993; Lucas 1988], and are often used to justify government provision of housing assistance rather than unrestricted cash grants for low-income families [Quigley 1980].

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 neighborhoods is fraught with difficulties.<sup>2</sup> Observed differences in youth outcomes by type of neighborhood could reflect unmeasured differences in family background. The reduction in the magnitude of estimated neighborhood effects when detailed controls for family background are included suggests that with even better controls estimates of neighborhood effects might be further reduced [Corcoran et al. 1992].<sup>3</sup> But the difficulty of measuring neighborhood at-

1. 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.

2. 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.

3. Recent research has attempted to better control for family background and

tributes and of defining appropriate peer groups may lead to underestimates of neighborhood and peer 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, 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.<sup>4</sup> 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 1995].<sup>5</sup>

The promising findings from the Gautreaux program helped

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to better address the issue of selectivity of residential location within a metropolitan area (MSA). Aaronson [1998] examines families that relocate and compares siblings who grow up in different neighborhoods. Aaronson finds substantial negative effects of neighborhood high-school dropout rates on individual dropout rates in sibling difference models. But differences in sibling outcomes could reflect changes in family circumstances driving residential moves. 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 socioeconomic outcomes for black youth in more segregated MSAs. Currie and Yelowitz [2000] argue that the correlation between living in a public housing project and poor outcomes may be spurious. They use an instrumental variables estimation strategy (where some families are quasi-randomly entitled to a larger unit based on the sex composition of their children) and find that boys in project households are less likely to have been held back in school.

4. 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 provided low-income blacks originally residing in Chicago public housing with special housing certificates and assistance to move to neighborhoods in which the black population has a share of less than 30 percent, both in the suburbs and in other parts of the city of Chicago.

5. Although the Gautreaux findings are quite suggestive, they are far from definitive. First, it remains unclear the extent to which the residential locations (suburban versus urban) of Gautreaux participants approximated random assignment. It is possible that substantial unobserved differences in the families moving to the suburbs and to other parts of central city Chicago could bias the findings. And the very small sample of Gautreaux families who are interviewed by survey researchers some years later may be unrepresentative in ways that could have influenced research findings.

motivate 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 were eligible for participation in the demonstration if they had children and resided in public housing or project-based Section 8 assisted housing in census tracts with a 1990 poverty rate of 40 percent or more.

Interested eligible families who completed an application and survey were then selected from a waiting list and randomly assigned to one of three program groups: the Experimental group, the Section 8 Comparison group, and the Control group [Feins et al. 1994]. Families in the Experimental group received a restricted housing voucher that could be used to help pay for rental housing from private landlords, but only in a low poverty area (a census tract with under a 10 percent poverty rate in 1990). The Experimental group families also received counseling assistance from a local nonprofit organization to help them search for an apartment and adjust to a new neighborhood. Section 8 Comparison group members received a geographically unrestricted housing voucher and no counseling assistance. The Control group families did not receive rental assistance vouchers, although their eligibility for continued project-based assistance was unaffected. The Experimental and Section 8 Comparison group members were given four to six months to submit a request for approval of an eligible apartment they wanted to lease using a housing voucher, and the apartment then had to pass a quality inspection.

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. It also provides for a direct comparison of the two major forms of housing assistance for U. S. low-income families: project-based assistance and tenant-based assistance.

In this paper we provide a comprehensive evaluation of the early impacts of the MTO demonstration on family well-being at the Boston site (measured about two years after program entry). We exploit the random-assignment design to produce these estimates through comparisons of the outcomes of the Experimental and Section 8 Comparison groups with those of the Control group.

Our analysis combines information from field observations of the program, qualitative interviews with participants, baseline survey responses, responses to a follow-up survey of 520 MTO-Boston participants, data on Census tract characteristics linked to geocoded initial and current addresses of participants, and Massachusetts administrative data on earnings and public assistance receipt.

We begin in Section II by discussing the mechanisms through which changes in residential neighborhoods may affect human capital accumulation, economic self-sufficiency, and current family well-being. In Section III we provide a more detailed description of the operation of MTO in Boston. We find that fear of crime was the primary reason participants wanted to move out of public housing at all five MTO sites.<sup>6</sup> In Section IV we present our econometric framework for analyzing neighborhood effects, and discuss the interpretation of parameters that can be estimated from the MTO experiment. In Section V we analyze mobility outcomes for the Boston families. We find that approximately 48 percent of the MTO group moved to private housing using the MTO vouchers as compared with 62 percent of the Section 8 Comparison group. Although the Experimental and Section 8 groups experienced similarly large positive effects on average neighborhood quality as measured by many census-tract characteristics, these averages mask important differences in the mobility patterns between the two treatment groups. Fewer Experimental group families managed to move out of their high-poverty origin neighborhoods, but those that did were much more likely than families in the Section 8 group to end up in suburban, low-poverty neighborhoods.

We present our main substantive findings in three groupings. In Section VI we analyze the impact of the program on children's human capital development as proxied by measures of child behavior and health. We find that the program led to a decline in behavior problems among boys in both the Experimental and Section 8 Comparison groups, but has no discernible impact on girls' behavior problems. The Experimental group children also

6. Our qualitative 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 [2001] provide a more detailed presentation of the findings from our qualitative interviews on participants' perceptions of their neighborhoods.

experienced fewer injuries and asthma attacks. In Section VII we examine adult economic self-sufficiency using data on welfare participation and employment, and find insignificant differences between the three MTO groups. Finally, in Section VIII we examine other factors affecting family quality of life, including safety, adult health, and adult social interactions. We find large improvements in neighborhood safety and in adult health for both MTO treatment groups. Section IX concludes.

## II. THEORETICAL BACKGROUND

The MTO demonstration facilitates moves by members of the treatment groups out of public housing to areas with more affluent neighbors and a lower incidence of many social problems. We are particularly interested in the impacts of such residential moves on child human capital accumulation and adult economic outcomes. In addition, we examine impacts of moves on safety and adult mental health both because these variables are likely to have long-run effects on child outcomes and because they are components of current family well-being.

Neighborhoods may affect the human capital accumulation of children through peer influences, adult influences, and community resources [Jencks and Mayer 1990]. Many child behaviors—including educational choices, crime, and other delinquent behavior—are likely to exhibit positive social interactions in which the net private benefits from pursuing an activity rise with the share of one's peers who pursue the activity. Positive social interactions (or contagion effects) can arise from learning from peers, pure preference externalities (individuals enjoy imitating their peers), stigma effects (the negative signal from delinquent behaviors declines when more people do them), and physical externalities (e.g., higher crime rates reduce the chances of getting arrested because of congestion effects in law enforcement).<sup>7</sup> Adults in a neighborhood may also influence young people who are not their

7. See Glaeser and Scheinkman [1999], Brock and Durlauf [2000], Manski [2000], and Moffitt [2000] for reviews of theories of social interactions and peer influences. Case and Katz [1991], Evans, Oates, and Schwab [1992], and Gaviria and Raphael [1999] have used nonexperimental methods to estimate peer effects on youth behaviors. Sacerdote [2001] uses a compelling quasi-experiment—the random assignment of roommates at Dartmouth College—to estimate peer effects on the academic performance and social behaviors of college students. Ludwig, Duncan, and Hirschfield [2001] present experimental evidence suggesting youth socialized in high-poverty neighborhoods may reduce their delinquency rate if their family moves through MTO at the Baltimore site.

children through human capital externalities [Borjas 1995] and by acting as role models or enforcers of public order [Wilson 1987]. The higher quality schools, better labor market opportunities, and greater community resources (e.g., recreational activities) of more affluent neighborhoods may increase the perceived returns to educational investments relative to delinquent behaviors. These theories suggest that moves to more affluent communities through MTO have the potential to enhance human capital development and reduce child problem behaviors.<sup>8</sup>

Adult economic self-sufficiency—employment and welfare participation—may also be affected by residential location. Spatial mismatch models [Kain 1968] suggest that moves from inner-city neighborhoods to suburban neighborhoods should increase access to employment opportunities leading to higher adult employment rates and lower welfare usage. Community norms in low-poverty areas are likely to be more supportive of work and less accepting of welfare than those in public housing projects. But relocations could—at least in the short run—disrupt social support networks that are important sources of informal child care, job referrals, and other labor market information. The overall effect of moves to low-poverty areas on employment is ambiguous in the short run but likely to become more positive over time as movers form new social networks; similarly, the impact of the program on welfare usage might be expected to rise over time.

Moves to new locations may result in important direct improvements in quality of life. Families moving out of high-poverty public housing to more affluent areas should experience improved neighborhood safety and possibly improved housing conditions. Our fieldwork strongly suggests such changes are likely to be associated with reductions in parental stress and anxiety.<sup>9</sup> Effects on mental health from improvements in safety could be offset, however, by social isolation in a new location.

8. Relative deprivation models in which children judge their success or failure by comparing themselves with others around them have the opposite prediction. Poor children competing against more affluent and academically better-prepared peers may become frustrated, reduce effort in school, and become more likely to engage in deviant behaviors.

9. Moreover, improvements in safety and in mother's mental health may have positive effects on child human capital accumulation. In fact, a large literature suggests positive impacts of maternal mental health on parenting and child outcomes [Weissman et al. 1984; Gelfand et al. 1990].

## III. MTO IN BOSTON

The MTO demonstration program presents an exciting opportunity to identify credibly the causal effects of a housing mobility program (and of neighborhoods) 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.

*A. The Program*

Each household assigned to the Experimental and Section 8 Comparison groups was issued a housing voucher that they could use to help pay the rent for an apartment in the private market.<sup>10</sup> The average value of this voucher was approximately \$700 per month. Both treatment groups received briefings from HUD staff about program rules and about how to look for an apartment at the time of voucher issuance. The net cost of counseling in the Experimental group was \$1827 per household [Goering et al. 1999].

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

10. Note that we use the term "housing voucher" as a generic term for the subsidies offered through MTO. One-third of these subsidies were Section 8 certificates, where recipients made a fixed contribution (usually 30 percent of their adjusted income, or .3AI) toward their rent, and the local housing authority paid the difference between this contribution and the unit rent up to a prespecified payment standard (PS). Two-thirds were Section 8 vouchers, where the housing authority set the subsidy size (usually  $PS - .3AI$ ) and allowed recipients to choose their rental payment within a range. These two subsidy types were eventually merged in 1999. The PS varied by bedroom size, and were based on a percentile (lowered from the forty-fifth to the fortieth in 1995) of the rent level in the metropolitan area; the Boston metropolitan area included most of the city and suburban residential locations of MTO-Boston program moves, while separate rent levels were calculated for Brockton, Lowell, and other surrounding cities. Use of a housing voucher required that a unit pass inspection. Material distributed to MTO families described the inspector's job was to "make sure that the apartment has enough windows, electric lights, and electrical outlets; is well ventilated; has enough space for the people who will be living there; has two ways to get in and out; and is in good condition." In general, households can retain this voucher as long as they demonstrate good conduct and their income is sufficiently low. For example, the voucher would be terminated if  $.3AI > PS$  for six consecutive months.



pants to see promising apartments. Thus, the counselors played a very large role in determining the destination communities of the Experimental group members.<sup>11</sup>

The impact of this complex treatment may also have been affected by features of the Boston housing and labor markets during the period of study and by some 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. 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. There were also major improvements during the study in the safety of some of the developments from which MTO families came.

### *B. The Data*

*Qualitative fieldwork.* Our fieldwork included observing the administration of the baseline survey, attending intake sessions for families after randomization, accompanying counselors on home visits, and interviewing program staff. We also held twelve 90-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.<sup>12</sup> This information was gathered to help understand what was going on inside the "black box" of the MTO treatments and to generate new hypotheses.

*Surveys.* We use data from two surveys. First, each household head completed a survey prior to enrollment in the MTO

11. 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 troublemakers. In some cases, MBHP provided small grants to households to enable them to purchase furniture or appliances for 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 mediated disputes between the participants and their landlords, and on a few occasions helped families deal with incidents of racial discrimination.

12. The qualitative 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.

program, which we refer to as the “MTO Baseline Survey,” which is available for households at all five MTO sites. Second, we conducted our own survey, which we refer to as the “MTO-Boston Follow-up Survey.” The main sampling frame for the data used in this study consisted of household heads randomly assigned in the MTO program in Boston between October 1994 and May 1996. Over these 20 months, 540 families were enrolled; new cohorts were assigned approximately once a month, for an average of 27 families per month. During June and July of 1997, we completed 340 interviews by telephone. Between November 1997 and April 1998, we completed an additional 180 interviews in person, for an overall survey response rate of 96.3 percent.<sup>13</sup> Although MTO continued to enroll families in Boston throughout 1996–1997, we limited our sample to families who had had up to 120 days to find a new residence and then at least nine months to have lived in the new residence at the time our interviews began. The monthly enrollments of new families and the different survey completion dates yield an average time between random assignment and the MTO-Boston Follow-up Survey of 2.2 years, with a range from 1 to 3.5 years. Survey data on residential addresses were geocoded, and we merged them with the STF files of the 1990 Census of Population to describe the attributes of the neighborhoods of MTO families.

*Administrative records.* Our primary source of information for assessing welfare and employment impacts in the Experimental and Section 8 Comparison groups are extracts from the Massachusetts Department of Revenue’s Longitudinal Data Base. The data on monthly welfare (AFDC/TANF) receipt from 1991–1998 originated with the Massachusetts Department of Transitional Assistance. Quarterly earnings data are based on information from employer filings with the Massachusetts Department of

13. The survey was written by our research team and administered in mixed modes (by telephone and in-person) by Westat Inc. The survey was administered in both Spanish and English. The interviewers were not told about the treatment group assignment of individuals. The complete survey is available at <http://www.wws.princeton.edu/~kling>. We were unable to complete interviews with twenty household heads. In thirteen 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 seven 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.

TABLE I

MTO BASELINE SURVEY DESCRIPTIVE STATISTICS OF HOUSEHOLDS IN FIVE CITIES

	Baltimore	Boston	Chicago	L.A.	N.Y.	All
If female household head	.98	.92	.96	.81	.93	.91
If Black	.97	.37	.99	.52	.50	.64
If Hispanic	.02	.46	.01	.45	.49	.32
Main reason to move:						
Drugs, gangs	.55	.54	.51	.60	.48	.53
Better apartment	.26	.29	.18	.13	.26	.22
Better schools	.12	.09	.24	.23	.19	.18
In the past 6 months a householder has been:						
Beaten or assaulted	.25	.17	.25	.27	.26	.24
Stabbed or shot	.12	.08	.12	.13	.12	.11
Sample size	637	961	895	1034	1081	4608

Authors' calculations using the October 1999 extract of the MTO Data System, containing data on all program enrollments (October 1994–July 1998).

Revenue.<sup>14</sup> An advantage of these data sources in comparison to our survey is that we can construct a panel of quarterly observations from the beginning of 1992 through the third quarter of 1998.<sup>15</sup>

### *C. Characteristics of MTO Families*

As shown in Table I, the participants in all five MTO sites are largely female-headed households. The participants in Baltimore and Chicago are almost entirely African-American. The Boston, Los Angeles, and New York sites have more ethnically diverse groups of participants with over 40 percent Hispanics. According to the Baseline Survey at the time of program enrollment, the main reason a majority of families (53 percent) wanted to move is fear of crime (“to get away from drugs and gangs”). Table I also

14. Note that these data from tax records included reports from all employers in the state, which is broader coverage than the Unemployment Insurance records often used in research. By definition, administrative records do not capture “off-the-books” or illegal earnings. However, some research has shown that the undermeasurement appears to be symmetric across treatment and control groups in the national JTPA evaluation [Kornfeld and Bloom 1999], suggesting that differences between MTO groups may not be biased by this omission.

15. This was constructed primarily by matching on Social Security Number (SSN). Names and birth dates were also used to find additional matches and validate the SSN matches; this refinement affected less than 1 percent of the quarterly observations.

reports that improvements in housing (“to get a bigger or better apartment”) and school quality (“better schools for my children”) were the next most important factors motivating moves. Employment concerns (“to get a job” or “to be near my job”) were listed as the main reason to move by less than 2 percent of all participants. 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 [Peroff et al. 1979, p. 114] indicated that good schools (34 percent) and quality of housing (26 percent) were more important considerations than crime (23 percent). The increased concern about crime among inner-city public housing residents likely reflects the increase in violent crime rates that occurred in many urban areas over this time period. The concern about crime may also have been the direct result of having been victimized recently. Across all five sites, 24 percent of the household heads indicated that someone who lives with them had been beaten or assaulted in the past six months, and 11 percent had 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].<sup>16</sup>

In comparison to other tenants in the same public housing development in which the MTO families lived, Goering et al. [1999] found that MTO and non-MTO households were very similar in household size and in number of children under eighteen. MTO households (pooling across all five sites) were more likely to be female-headed (93 percent versus 78 percent), receiving AFDC (75 percent versus 51 percent), not working (88 percent versus 70 percent), and younger (median age 33 versus 39).

Table II presents a list of characteristics of the 540 Boston households that are the focus of our analysis, as reported by the household head in the Baseline Survey. The majority of these families are headed by a single mother who receives public assistance. Twenty-seven percent of the household heads were employed at the time of the baseline survey, and 22 percent own an

16. Victimization rates may be somewhat exaggerated in the Baseline Survey. Despite instructions that the survey was being conducted by outside researchers and that the housing authority would not receive copies of individual responses, our fieldwork revealed that some respondents assumed their answers could influence program acceptance.

TABLE II  
MTO-BOSTON BASELINE SURVEY DESCRIPTIVE STATISTICS  
OF HOUSEHOLD CHARACTERISTICS

A. Households		
Head's age is 19–29		.30
Head's age is 30–39		.46
Head's age is 40–49		.18
Head is Hispanic		.45
Head is Black and non-Hispanic		.37
Head is female		.91
Head has never been married		.59
Family size is 2		.23
Family size is 3		.35
Family size is 4		.22
All household children are ages 0–12		.58
Any householder was robbed, assaulted, or threatened with a weapon within past 6 months		.29
Any householder is disabled		.14
Head is currently working		.27
Head is currently receiving AFDC/TANF		.64
Head is currently enrolled in school		.17
Head is high school graduate		.43
Head is GED recipient		.24
Head has a car that runs		.22
Head has moved 3+ times in past 5 years		.10
Head has no friends in neighborhood		.42
Head has no family in neighborhood		.69
Census tract poverty rate is less than 40%		.20
Census tract poverty rate is greater than 43%		.32
B. Children		
	Boys	Girls
Problems that require special medicine or equipment	.17	.06
Problems that make it hard to get to school or to play active sports or games	.08	.06
Special help for learning problems during past 2 years	.22	.14
Special help for behavioral or emotional problems during past 2 years	.10	.04
Suspended or expelled from school during past 2 years	.06	.03
School asked someone about problems at school during past 2 years	.25	.16
Goes to special class for gifted students or does advanced work in any subjects	.14	.11

Means are weighted, as discussed in Section III. Missing Baseline Survey data are imputed to the mean of nonmissing observations. In panel A, sample size is 540 households. For age, the omitted category is 50+; for family size, the omitted category is 5+. In panel B, sample is based on up to two randomly selected children per household age 6–15. Sample size is 296 boys (from 253 households) and 316 girls (from 268 households).

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.

Under random assignment to one of three groups in MTO, the Baseline Survey characteristics should be the same on average across the groups except for variation due to sampling. Using the 24 characteristics in panel A of Table II, we find that only three of the 48 differences between the control group and the two treatment groups have a  $p$ -value of less than .05; this pattern is quite consistent with a successful implementation of random assignment.<sup>17</sup> One additional factor when comparing groups is that the randomization proportions were changed after the first 450 households were randomly assigned; all statistics we report are therefore weighted to account for this change.<sup>18</sup>

#### IV. ECONOMETRIC FRAMEWORK

The theories of neighborhood effects discussed in Section II posit a relationship between neighborhood characteristics (such as school quality or role models) and outcomes (such as educational achievement). In a simple regression framework, the most direct test of these theories would be to examine the coefficient vector ( $\delta_1$ ) in a regression of the outcome of interest ( $Y$ ) on a set of neighborhood characteristics ( $W$ ), conditioning on controls for individual background variables ( $X$ ) with  $i$  indexing households:

$$(1) \quad Y_i = W_i\delta_1 + X_i\beta_1 + \varepsilon_{1i}.$$

There are two reasons, however, why estimates of equation (1) on

17. A test of the joint significance of the 48 differences has an  $F$ -statistic of 1.2 with a  $p$ -value of .21, indicating no significant differences between the groups. Means of baseline characteristics for each of the three MTO groups are presented in Table 1 of Katz, Kling, and Liebman [2000].

18. For the computation of all descriptive statistics and econometric estimates in this paper, the data are weighted. For the earlier 450 sample households, the Exp:Sec8:Control random assignment ratio was 225:85:140. For the later 90 households, after March 1996, the ratio was 15:35:40. The randomization proportions were adjusted after it became apparent that more Experimental families and fewer Section 8 Comparison families were taking up the offered voucher than had been projected. We weight the data under these two randomization proportions to reflect the overall sample ratio of 240:120:180, or 4:2:3. For example, there are relatively fewer Control households in the earlier period than in the overall sample, so these observations are upweighted by  $(180/540)/(140/450) = 1.07$ . Thus, using the weights allow us to abstract from this change and address the counterfactual question of what our results would look like if the randomization probabilities had remained constant throughout our time period at the overall sample ratio.

standard nonexperimental cross-section (and panel) data sets are unlikely to provide convincing tests of theories of neighborhood effects. First, the selection problem arising from the systematic sorting of individuals among neighborhoods on the basis of important unobserved determinants of socioeconomic outcomes may bias results from equation (1). Second, it can be quite difficult to measure the characteristics of neighborhoods that theories suggest should be important, potentially leading to attenuation biases from measurement error. For example, data on peer influences are difficult to collect, and the neighborhood variables in standard data sets (usually at the census tract or block group level) may not correspond to the relevant neighborhood concept. These measurement problems are compounded since many neighborhood characteristics are highly correlated with each other, making it difficult to parse out their separate contributions.

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 originally living in public housing who were offered a housing voucher to defray their rental payments in a privately owned apartment. We then compare the average outcomes of these treatment-group families to a Control group originally living in the same public housing projects, but who were not offered the voucher. The key to this analysis is that the offer of the voucher 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 in the treatment group if they had not been offered a housing voucher through the lottery. This analysis does not allow us to identify the specific neighborhood characteristics that produce the change in outcome. Instead, it measures the average causal effect of moves to neighborhoods with entire bundles of different characteristics than the public housing projects of origin.

To formalize our analysis, let  $D$  be an indicator variable for use of a housing voucher to move through the MTO program, or treatment take-up. Let  $Z$  (with coefficient  $\pi_2$ ) be an indicator variable for being eligible for an MTO program voucher, or treat-

ment group assignment. Let  $\eta$  be the other determinants of voucher use, which is determined as in equation (2):

$$(2) \quad D_i = Z_i\pi_2 + \eta_{2i}.$$

The difference in outcomes between the treatment and control group are known as "Intent-to-Treat" (ITT) effect, and is captured by the ordinary least squares (OLS) estimate of the coefficient  $\pi_3$  in a regression of the outcome ( $Y$ ) on an indicator for assignment ( $Z$ ) to a treatment or control group as in equation (3):

$$(3) \quad Y_i = Z_i\pi_3 + \varepsilon_{3i}.$$

ITT is an average of the causal effects for those who take up the treatment and those who do not.

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 (3) will not change the coefficient  $\pi_3$  (unless  $X$  happens to differ between groups due to the variability in a small sample).  $X$ s may still be included to improve the precision of the treatment effect estimates, however, if they are related to  $Y$  and thereby reduce residual variation in the regression, as in equation (4):

$$(4) \quad Y_i = Z_i\pi_4 + X_i\beta_4 + \varepsilon_{4i}.$$

The ITT estimate tells us the impact of being offered the opportunity to move with a MTO voucher. From the perspective of a policy maker designing a policy that would offer a similar voucher to a similar population, this parameter is directly of interest.

However, from the perspective of the neighborhood effects literature, it is also desirable to have an estimate of the impact of actually moving to a new neighborhood, rather than simply of being offered the opportunity to move. Under several assumptions (treatment group assignment is random; Control group members are prohibited from receiving program voucher assistance; the effect on outcomes of treatment assignment works entirely through making a subsidized move through the program<sup>19</sup>), we can use treatment assignment as an instrumental

19. This assumption says that those who did not accept the treatment offer had no average treatment effect in comparison to those who would not have accepted the treatment if it had been offered to them. For the Experimental group, this assumption implies that the later outcomes of households who met with a housing mobility counselor were not affected by the counselor if that household did not make a subsidized move through the MTO program. For both treatment



variable to estimate the parameter commonly known as “the effect of Treatment-on-Treated” (TOT). The TOT parameter measures the average effect of the treatment on those in the treatment group who actually receive the treatment (i.e., who actually move using a Section 8 rental voucher provided by the MTO program). One estimate of TOT is  $\pi_3/\pi_2$ , or the ITT divided by the proportion receiving the treatment.<sup>20</sup> We use an alternative estimate, the parameter  $\gamma_5$  from a two-stage least squares (2SLS) regression of  $Y$  on  $D$  with  $Z$  used as an instrumental variable for  $D$ , as in equation (5):

$$(5) \quad Y_i = D_i\gamma_5 + X_i\beta_5 + \varepsilon_{5i}.$$

The 2SLS estimate of  $\gamma_5$  in (5) is a regression-adjusted estimate of TOT, using the information in  $X$  to obtain additional statistical precision, and is asymptotically equivalent to the unadjusted indirect least squares estimate of TOT,  $\pi_3/\pi_2$ .

There are two treatment groups in this application, so we will compute separate estimates for both the Experimental and Section 8 Comparison groups by making  $D$  and  $Z$  matrices of two indicator variables for these groups. In this research we will stack data for all three MTO groups and present regression-adjusted ITT and TOT estimates based, respectively, on OLS estimates of (4) and on 2SLS estimates of (5), using treatment group assignments as instrumental variables for housing voucher use through the program.

Conceptually, the TOT is the estimated difference in outcomes between those who actually use the rental voucher through the program and those in the Control group who would have used the voucher if it had been offered to them. Among those assigned to a treatment group, we refer to those who decide to take up the offered voucher as “compliers” and those who decide not to take up the offered voucher as “never takers,”

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groups, this assumption implies that the experience of housing search induced by assignment to a treatment group did not affect later outcomes if that household did not make a subsidized program move. We believe that this assumption is not strictly true, but we also believe that effects of mobility counselors and of housing search on outcomes such as child behavior, victimization, or health for those who did not make a subsidized move through the program are likely to be an order of magnitude smaller than the effects of moving to a new residential location.

20. 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.

TABLE III  
DEFINITIONS OF COMPLIERS AND NEVER TAKERS

	All	Complier ( $C = 1$ )	Never Taker ( $C = 0$ )
Offered Treatment ( $Z = 1$ )	"Treatment Group" $E[Y Z = 1]$ Sample mean observed. In MTO, offered a voucher in the lottery.	"Treatment Compliers" $E[Y C = 1, Z = 1]$ Sample mean observed. In MTO, offered a voucher and used it to move through program.	"Treatment Never Takers" $E[Y C = 0, Z = 1]$ Sample mean observed. In MTO, offered a voucher, but did not use it to move through program.
Not Offered Treatment ( $Z = 0$ )	"Control Group" $E[Y Z = 0]$ Sample mean observed. In MTO, not offered a voucher in the lottery.	"Control Compliers" $E[Y C = 1, Z = 0]$ Sample mean not observed. In MTO, not offered a voucher—but would have used it to move through program if offered.	"Control Never Takers" $E[Y C = 0, Z = 0]$ Sample mean not observed. In MTO, not offered a voucher—and would not have used it to move through program if offered.

following a literature on treatment effect estimation.<sup>21</sup> Table III summarizes the relationship between compliers and never takers in the treatment and control groups. As before, let  $Y$  be an outcome, let  $D$  be an indicator of program voucher use, and let  $Z$  be an indicator of treatment group assignment by lottery. Now, let  $C$  refer to potential compliance with the treatment. Potential compliance is observed in the treatment group ( $C = D$  when  $Z = 1$ ), but not observed in the control group. Thus, "Control Compliers" ( $C = 1, Z = 0$ ) do not receive the treatment, but they would have accepted the treatment if it had been offered to them. Similarly, "Control Never Takers" ( $C = 0, Z = 0$ ) would not have accepted the treatment if it had been offered to them.

Using this notation, we can more precisely define the Intent-to-Treat (ITT) and Treatment-on-Treated effects (TOT). ITT is  $E[Y|Z = 1] - E[Y|Z = 0]$ . TOT is  $E[Y|C = 1, Z = 1] - E[Y|C = 1, Z = 0]$ . To assess the magnitude of the TOT effect in relative as well as in absolute terms, it is useful to have a benchmark level of the outcome in the absence of treatment for comparison. In

21. For example, see Angrist, Imbens, and Rubin [1996]. In their terminology there are no "always takers" or "defiers" in this application because no one in the Control group is offered a voucher through the MTO program.

equation (6) we use the mean outcome for treated compliers and the TOT difference to impute the Control Complier Mean outcome (CCM):

$$\begin{aligned} (6) \quad \text{CCM} &= E[Y|C = 1, Z = 0] = E[Y|C = 1, Z = 1] \\ &\quad - \{E[Y|C = 1, Z = 1] - E[Y|C = 1, Z = 0]\} \\ &= E[Y|C = 1, Z = 1] - \text{TOT}. \end{aligned}$$

Although  $E[Y|C = 1, Z = 0]$  is not directly observable,  $E[Y|C = 1, Z = 1]$ , and TOT can be estimated. With a mean outcome for treated compliers of .40 and a TOT effect of .10, for example, the mean outcome level among the “Control Compliers” who would have taken up a voucher if it had been offered to them would be .30, and the change in the outcome for compliers relative to the level of the outcome in the absence of the treatment would be 33 percent.

## V. MOBILITY AND NEIGHBORHOOD CHARACTERISTICS

As intended, the MTO program had a substantial impact on the residential location of households offered vouchers to live in a private market apartment. The share of MTO-Boston families in our sample who used a program voucher to move from their original location (which we refer to as the “take-up rate”) was .48 for the Experimental group and .62 for the Section 8 Comparison group, as reported in the first row of panel A of Table IV.<sup>22</sup> The lower take-up rate for Experimental group families resulted from two ways in which the treatment they received differed from the treatment received by Comparison group families. On the one hand, the restriction imposed on Experimental group families limiting them to apartments in census tracts with poverty rates below 10 percent could have reduced their take-up of the program, both because it was more difficult to find apartments farther outside the city and because of possible perceived disamenities associated with moves to low-poverty neighborhoods such as greater transportation costs or social isolation. On the other hand, the extensive housing search counseling provided to Experimental group families presumably increased their take-up rate.

22. This pattern of lower take-up rates for the Experimental group than for the Section 8 Comparison group is found at all five MTO sites. Goering et al. [1999] report that the final take-up rates across all the MTO sites were 47 percent for the Experimentals and 60 percent for the Section 8 Comparison group.

TABLE IV  
MOBILITY AND CENSUS TRACT CHARACTERISTICS FROM THE MTO-BOSTON  
FOLLOW-UP SURVEY

	Control Mean (1)	Intent-to-Treat Difference	
		Exp - Control (2)	Sec8 - Control (3)
<b>A. Mobility</b>			
Subsidized move through MTO program	.000	.479** (.036)	.619** (.047)
Any residential move out of Boston city limits	.049	.234** (.035)	.069** (.035)
Any residential move out of original housing project	.271	.325** (.049)	.414** (.057)
<b>B. Census tract characteristics</b>			
Poverty rate	.359	-.122** (.014)	-.100** (.016)
Welfare receipt	.294	-.097** (.012)	-.066** (.013)
Female-headed households	.531	-.136** (.016)	-.091** (.018)
Managerial and professional workers	.206	.029** (.007)	.022** (.008)
Education beyond high school degree	.289	.064** (.013)	.066** (.015)
Speak almost no English	.097	-.025** (.007)	.000 (.009)
Hispanic	.218	-.056** (.017)	-.018 (.021)
Black	.449	-.095** (.035)	-.057 (.040)
<b>C. Test scores of school child attends</b>			
Reading test score percentile of school	.083	.076** (.021)	.026 (.022)
Math test score percentile of school	.099	.061** (.022)	.027 (.025)

Intent-to-Treat Difference is based on equation (3). Standard errors are reported in parentheses, adjusted for household level clustering; \* =  $p$ -value < .1; \*\* =  $p$ -value < .05. Data in panels A and B are from the 1990 Census. In panel C, percentiles are weighted to represent fraction of Massachusetts students in schools with average test score below average score of school child attends, based on the 1994 Massachusetts Educational Assessment Program data from the Massachusetts Department of Education. Sample size is 525 households (Exp: 236; Sec8: 113; Control: 176) for panels A and B, and 408 children ages 6–15 for panel C.

The restriction on eligible census tracts appears to have been more important than the counseling leading to lower take-up for the Experimental group.

In analyses not reported in the tables, we have examined the

determinants of take-up for both the Experimental and Section 8 Comparison groups. To distill the vast number of variables in the Baseline Survey that are possibly relevant predictors of take-up into a parsimonious (and interpretable) specification, we created several indices of questions on the same or highly correlated topics. We find that indices of Confident Attitudes and Dangerous Living Conditions are highly significant predictors of take-up, while Social Involvement in the Community and Public Assistance Receipt are not significant.<sup>23</sup> A possible interpretation of these findings is that households with greater (perceived) net benefits from a move—because of their better ability to handle life in a new neighborhood and a worse safety situation in their public housing project—are more likely to take advantage of vouchers providing an opportunity to move out of public housing. We also examined the relationship between take-up and an index of behavior problems among children identified in the Baseline Survey.<sup>24</sup> The main finding of this analysis is a large and sig-

23. These indices were constructed to be the proportion of households answering affirmatively to related questions. Confident Attitudes refers to the proportion that were “very sure” about their ability to handle a move (six questions) and that would not feel bad about living in a white, higher-income neighborhood (four questions). Dangerous Living Conditions refers to the proportion reporting personal victimization of a householder (five questions); unsafe neighborhood conditions (four questions); big neighborhood problems with litter, graffiti, drinking, drug dealing, and abandoned buildings (six questions); and apartment problems with paint, plumbing, rodents, locks, windows, wiring, heating, and space (eight questions). Social Involvement in the Community refers to the proportion reporting interactions with neighbors (eight questions), friends or family in the neighborhood (two questions), and use of services such as buses, churches, and clinics within fifteen minutes of home (five questions). Public Assistance Receipt use refers to the proportion reporting receipt of AFDC, Food Stamps, SSI, WIC, or other public assistance payments (nine questions).

An increase in the proportion of Confident Attitudes responses from .2 to .7 (tenth to ninetieth percentile) is associated with a nineteen percentage point increase in take-up. An increase in the proportion of Dangerous Living Conditions responses from .18 to .72 (tenth to ninetieth percentile) is associated with a fourteen percentage point increase in take-up. These results are from a linear probability model of take-up controlling for the covariates in Table II (not included themselves in the indices), using pooled data on the 360 Experimental and Section 8 Comparison group households. The effects of background characteristics on take-up are similar across the two treatment groups, but the limited sample sizes make it difficult to rule out large differences in the determinants of take-up for the Experimental and Comparison treatments.

24. This index was the proportion of behavior problems reported about the child from the six listed in panel B of Table II. An increase in the proportion of boys' behavior problems from 0 to .5 (to ninetieth percentile) was associated with an eleven percentage point increase in take-up ( $p$ -value .24). An increase for girls from 0 to .33 (to ninetieth percentile) was associated with a 22 percentage point decrease in take-up ( $p$ -value .002). These results are from a linear probability model of take-up controlling for a main effect of gender and the covariates in Table II (not interacted with gender), using pooled data on the 388 children in the Experimental and Section 8 Comparison group households.

nificant gender asymmetry; behavior problems among boys are positively related to take-up, problems among girls are negatively related to take-up.

The two MTO treatments also led to differing distributions of destination neighborhoods. In the second row of panel A, we find that at the time of the Follow-up Survey few Control (.05) and Section 8 Comparison (.12) households were living outside the city of Boston, while the Experimental group (.28) households were much more likely to live outside the city limits.<sup>25</sup> During the 1–3.5 years that elapsed by the time of the Follow-up Survey, a substantial share (.27) of Control households had moved out of their origin housing project or other Census block group from the time of the Baseline Survey. Among the Experimental group, 60 percent of households had moved out of their original location (48 percent through MTO and 12 percent independently). Among the Section 8 Comparison group, 68 percent had moved (62 percent through MTO and 6 percent independently).<sup>26</sup>

To assess the differences in residential locations of program participants, we present the mean characteristics of neighborhoods based on 1990 Census tract data. Panel B of Table IV shows that the local areas in which Experimental and Section 8 Comparison households were living at the time of the Follow-up Survey were significantly different on average from the Control households across many different dimensions, including a lower poverty rate, lower welfare receipt, and higher education levels.<sup>27</sup>

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

26. Although some Section 8 Comparison families had moved independently of the MTO program, they did not necessarily stop receiving some form of project-based or tenant-based housing assistance. In the Follow-up Survey, 98 percent reported receiving government housing assistance, a significantly higher rate of receipt than the 93 percent in the Experimental group and the 90 percent in the Control group. Thus, the treatment increased housing assistance receipt (relative to the counterfactual of being in the Control group) for 8 percent of Section 8 Comparison families in addition to influencing residential location for 62 percent in that group.

27. We have also replicated these analyses of neighborhood characteristics with Census block group data, and the results are quite similar. The mean poverty rates for block groups are as follows: Exp .250; Sec8 .275; and Control .388.

The Section 8 Comparison group had smaller differences relative to the control group in racial composition and fraction speaking almost no English.

Panel C provides information about the peers children are exposed to in school. It displays the average reading and math test score percentiles for the schools that MTO children were attending at the time of the Follow-up Survey. These percentiles are calculated by taking the average test score for the students in the school from a statewide test and then determining the fraction of Massachusetts students who attend schools with average test scores below the average score for students in the school that the MTO child attends.<sup>28</sup> The Control group children attend schools that on average have reading test scores in the eighth percentile and math test scores in the tenth percentile of the school test score distribution. The Experimental group experienced an eight percentile point increase in school average reading scores, and a six percentile point increase in average math scores. The results for both reading and math test scores indicate that even though many families are moving through MTO to neighborhoods with substantially less poverty than their origin neighborhoods, the schools MTO children are attending have test scores that are still well below the state average. Indeed, over 85 percent of children in Experimental households that made program moves still attend schools with average test scores below the state median. The Section 8 group experienced smaller and statistically insignificant increases in school quality as measured by test scores. The small changes for the Section 8 group are not surprising since many of the households remain in the city of Boston with many of their children staying in their original schools.

The similarity in the mean characteristics of the neighborhoods of the Experimental and Section 8 Comparison groups does not convey the differences in the distribution of tract characteristics. Data on the distribution of tract poverty rates are pre-

28. The test scores come from the 1996 Massachusetts Educational Assessment Program (MEAP). The test was administered to students in grades 4, 8, and 10. For schools that contained two tested grades, we counted the grade closest to the grade of the MTO child. Our overall response rate for school test score variable was only 67 percent (as compared with about 93 percent for most child outcomes) because of difficulties matching the school name that was provided to us by the MTO parent to our list of all schools in Massachusetts. The MEAP was not administered to students in private schools or charter schools. We did not find noticeable differences in the fraction of students attending private or charter schools across the three groups.

TABLE V  
DISTRIBUTION SHARES OF CENSUS TRACT POVERTY RATES FROM MTO-BOSTON  
FOLLOW-UP SURVEY

Poverty rate	Overall		No subsidized move and overall	No subsidized move through MTO program		Subsidized move through MTO program	
	Exp	Sec8		Exp	Sec8	Exp	Sec8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0-10	.362	.134	.018	.029	.068	.724	.175
10-20	.113	.242	.110	.080	.026	.150	.374
20-30	.151	.294	.190	.196	.162	.102	.376
30-40	.068	.078	.088	.109	.082	.024	.075
40+	.306	.253	.593	.587	.663	.000	.000
Mean	.237	.259	.359	.362	.370	.100	.190
Share of sample	.449	.216	.335	.234	.082	.215	.134

Sample size is 525 households.

sented for the three MTO groups in columns (1)–(3) of Table V. Although many families in both the Experimental and Section 8 groups moved to lower poverty neighborhoods, members of the Section 8 Comparison group were the least likely to remain in a tract with a poverty rate of greater than 40 percent.

Columns (3)–(5) show the distribution of Census tract poverty rates for households who did not make a subsidized move through the MTO program. The mean poverty rates for the three groups are within one percentage point of each other, and the distributions are quite similar. Under the assumption that the program had no impact on families that did not lease up through the program, this implies that the mean poverty rates are essentially the same within the Control group for those who would have taken a program rental voucher if offered (Control Compliers) and those who would not have taken it (Control Never Takers).<sup>29</sup> In other words, those in the Control group who would have

29. As discussed in Section IV, the Overall Control mean is a weighted average of the Control Complier mean and the Control Never Taker mean:  $E[Y|Z = 0] = E[Y|C = 1, Z = 0] \Pr[C = 1, Z = 0] + E[Y|C = 0, Z = 0](1 - \Pr[C = 1, Z = 0])$ . Under the identifying assumptions in Section III, the



utilized the rental voucher if offered were no more likely move to a lower poverty neighborhood on their own in the absence of a voucher than those who would not have utilized the voucher.

The Census tract poverty rate distribution for households who did make a subsidized move through the MTO program, the Treatment Compliers, is shown in columns (6)–(7) of Table V. Nearly all of the Experimental movers initially moved to a Census tract with a poverty rate of less than 10 percent as prescribed by program rules, but by the time of the Follow-up Survey (1–3.5 years after random assignment) only 72 percent were living in tracts with less than a 10 percent poverty rate because some had made subsequent moves to higher poverty tracts.<sup>30</sup> Among Section 8 Comparison group members who moved through the MTO program, only 18 percent were living in a tract with a poverty rate less than 10 percent. But for both groups of movers, there were no families living in a tract with a poverty rate of greater than 40 percent, in comparison to an overall share of 60 percent in columns (3)–(5) among those who did not make a subsidized move.

In summary, about three-quarters of the Experimental Compliers moved and remained in neighborhoods with a poverty rate below 10 percent, while about three-quarters of Section 8 Comparison Compliers ended up in neighborhoods with poverty rates between 10 and 40 percent. However, because take-up was higher in the Section 8 group, fewer Section 8 families remained in Census tracts with poverty rates above 40 percent.

## VI. CHILDREN'S HUMAN CAPITAL: BEHAVIOR AND HEALTH

This section presents results of the impacts of the MTO treatments on child behavior and mental and physical health. These outcomes are of interest both as key short-term measures of child well-being and as potentially important inputs into the human capital production function. Models of neighborhood ef-

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difference between the Control Complier mean and the Control Never Taker mean is equivalent to  $(E[Y|Z = 0] - E[Y|D = 0, Z = 1]) / \Pr[D = 1, Z = 1]$ . Throughout this paper we estimate the statistical significance of this difference without covariates for simplicity.

30. Families using a program voucher were free to move again after one year while continuing to receive the rental voucher, and Experimental group families were no longer restricted to low poverty areas in choosing their location. Among those in both treatment groups who initially made a move outside the Boston city limits through the MTO program, 14 percent had returned to live within Boston at the time of the Follow-up Survey.

fects suggest that moves to safer and more affluent neighborhoods are likely to be associated with reductions in child problem behaviors. Furthermore, the medical literature documents an association in clinical settings between chronic exposure to violence and patterns of child health and behaviors, such as difficulty concentrating because of fatigue due to sleep disturbances or images of traumatic events, fearfulness of being alone, and aggressiveness due to behavior imitation or self-protection [Augustyn et al. 1995; Groves et al. 1993].

In our qualitative fieldwork we also developed a hypothesis that the fear and anxiety of living in a violent neighborhood may cause emotional and behavioral problems that may adversely affect the mental health of children. This hypothesis is encapsulated in the comments of one woman who described to us why she had used an MTO voucher to move to a suburban location.

As long as the kids is safe, that what my main concern was. . . . They can't grow up normally in an atmosphere of fear. They can't play games and stuff. So I had to do it. And that was the best chance when they said we had to move in the suburbs. Living in the same area, that's not good. Even the elderly people, they living in fear, captive in their own homes. My mother lives like that. It's terrible. . . . In the 'hood—my kids, they had friends. But I wouldn't let them go out, because I was afraid. Bullets don't got no name.

This same woman described to us a visit she took to her old neighborhood with her four year old daughter: "Even now, we can't drive up the street. My baby, she so scared that she start cryin'. 'No-no-no.' She don't even want to go near there. It's amazing how little kids remember that stuff."

To assess the impact of residential location on the mental health of children, we asked household heads in the Follow-up Survey if their children had exhibited various behavior problems during the past three months. The questions we administered in the Follow-up Survey were items fielded previously in the National Health Interview Survey and the National Longitudinal Survey of Youth Children's Supplement (NLSY-CS). From those instruments we selected seven questions that focused on types of behaviors that the mothers could observe directly, as opposed to questions generically about behavior problems or questions requiring intuition about how their child was feeling. The behaviors we asked about were the following: trouble getting along with teachers, disobedience at home, disobedience at school, hanging around with troublemakers, bullying others, inability to sit still, and depression. To summarize the responses to the seven behav-

ior problem questions, we created an index that is the fraction of the problems that were “sometimes” or “often” true of the child, as opposed to “not true.”<sup>31</sup>

We present results on behavior problems in Table VI separately for boys and girls. Since we use the same table structure for a variety of outcomes, we explain it here in some detail. Each row of Table VI corresponds to a different outcome from the Follow-up Survey. In the first row the outcome is the household head’s report of the proportion of behavior problems displayed by her male child. We find in column (1) that the average fraction of the seven behavior problems in the Control group was .33.<sup>32</sup> In comparison, the Experimental group had a fraction of behavior problems nine percentage points lower than the Control group, based on the ITT estimates reported in column (2). Assuming that only those who made a subsidized move through the MTO program had a nonzero average treatment effect, the TOT effect for the Experimental group was an eighteen percentage point reduction in behavior problems, reported in column (3). As we discussed in Section IV, it is also possible to estimate the average level of the outcome for those in the Control group who would have used an MTO voucher if it had been offered to them. This Control Complier mean is reported in column (4). Thus, the Experimental group compliers experienced a change in behavior problems of  $-.18$  from a mean  $.44$ , a relative decrease of 42 percent. Columns (5)–(8) show a similarly large decrease in behavior problems for boys in the Section 8 Comparison group.

Both the ITT and TOT estimates are regression adjusted, controlling for the Baseline Survey household variables summa-

31. Using the data on the children from ages eight to fourteen in the NLSY-CS, we find that this index of seven behavior problems and some of its components (especially the measure of trouble getting along with teachers) are statistically significant predictors of socioeconomic outcomes (alcohol use, drug use, criminal involvement, and school enrollment) measured four to eight years later. Thus, child behavior problems may provide a good indicator for human capital development. MTO treatment effect estimates for individual components of the behavior problems index are available in Katz, Kling, and Liebman [2000].

32. For context, we examined behavior problems in a sample of children ages 6–14 from the 1998 NLSY-CS, whose mothers have roughly the same median age as the MTO sample. Weights to adjust the national sample to reflect MTO demographics were based on indicators for age, sex, and gender of the child, and indicators for head of household having twelve years of education, or more than twelve years. These variables were used to predict the odds of participating in MTO versus being a national survey sample respondent, based on the exponentiated latent index of a logit model of participation estimated from stacked MTO and national survey data. The demographically adjusted mean of the behavior problems index was  $.35$  for boys and  $.25$  for girls, slightly higher than the  $.33$  for boys and  $.20$  for girls ages 6–14 among MTO Controls.

TABLE VI  
IMPACT OF MTO-BOSTON ON THE BEHAVIOR AND HEALTH OF CHILDREN AGES 6-15

	Experimental versus Control				Section 8 Comparison versus Control			
	Control Overall Mean (1)	Intent-to- Treat Difference (2)	Treatment- on-Treated Difference (3)	Control Complier Mean (4)	Intent-to- Treat Difference (5)	Treatment- on-Treated Difference (6)	Control Complier Mean (7)	
Fraction of 7 behavior problems								
	Boys	-.090** (.041)	-.184** (.088)	.436	-.113** (.053)	-.160** (.079)	.450	
	Girls	-.023 (.030)	-.046 (.056)	.197	-.050 (.034)	-.078 (.053)	.230	
At least one close friend in neighborhood	Boys	.009 (.079)	.018 (.159)	.766	.019 (.091)	.026 (.129)	.689	
	Girls	-.146** (.064)	-.282** (.123)	.982	-.190** (.080)	-.297** (.128)	.976	
Any injuries or accidents during past 6 months that required medical attention		-.059** (.027)	-.118** (.055)	.160	-.037 (.033)	-.059 (.050)	.137	
Any asthma attacks requiring medical attention during past 6 months		-.051* (.029)	-.103* (.059)	.158	-.004 (.037)	-.010 (.055)	.109	
Been to doctor for regular checkup or immunization during past 6 months		-.043 (.037)	-.087 (.077)	.955	-.071 (.056)	-.106 (.081)	.893	

Intent-to-Treat (ITT) Difference in columns (2) and (5) is based on equation (4), using OLS to control for the 24 household Baseline Survey characteristics and the seven child behavior and learning issues from Table II, and indicators for child's year of age as of 9/1/97. Treatment-on-Treated (TOT) estimates in columns (3) and (6) use 2SLS (with same controls as ITT), and instrument treatment compliance with treatment assignment as in equation (5). Implied means for Control Compliers (who would have moved through the program if they had been assigned to a treatment group) are computed in columns (4) and (7) as the difference of the unconditional means of Treatment Compliers (who did move through the program) and the TOT estimates using 2SLS. Robust standard errors adjusted for household clustering are reported in parentheses; \* =  $p$ -value < .1; \*\* =  $p$ -value < .05. The sample sizes for rows 1-7, respectively, are 274, 300, 272, 295, 569, 570, and 566.

rized in panel A of Table II (age, race, sex, marital status, family structure, disability, welfare receipt, education, employment, car ownership, mobility history, social contact, victimization, and neighborhood poverty rate) and the Baseline Survey child behavior variables in panel B of Table II. These regressions adjust for differences in Baseline Survey characteristics that may have occurred by chance, and absorb residual variation to improve the efficiency of estimation.<sup>33</sup>

Among girls in row 2, the ITT estimates show only modest reductions in behavior problems for both treatment groups, and the estimates are not statistically distinguishable from zero. Notably, this concentration of the effects in reduction of 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]. Leventhal and Brooks-Gunn [2000] report evidence that suggests a behavior problem reduction among children ages 8–18 at the New York MTO site, although results are not reported by gender. The generally high level of behavior problems among Control Complier boys suggests that households who had boys with behavior problems were more likely to accept an MTO program voucher if one was offered to them, a hypothesis that is consistent with our earlier result that households with a boy with behavior problems were more likely to take up an offered voucher.

One mechanism through which the gender differences may be occurring is social contact with other children. Girls in Control Complier households were particularly likely to have had at least one friend in the neighborhood, but girls in Complier households from both treatment groups each had a significant relative decline of about 30 percent in their chance of having a friend in the neighborhood.<sup>34</sup> We speculate that these socialization difficulties for girls may offset some of the benefits for mental health of living in a different neighborhood.

Regarding physical health, a substantial medical literature

33. Because of the random assignment, the Baseline Survey Characteristics are essentially orthogonal to the treatment indicators, so the magnitudes of the point estimates do not change substantively when we include covariates. For example, the simple difference in means of the Experimental and Control groups in behavior problems for boys is  $-.098$ , versus the regression-adjusted ITT estimate of  $-.090$ .

34. For analysis of binary outcomes throughout this paper, we use linear probability models for simplicity in computing 2SLS estimates, and correct the standard errors for heteroskedasticity. In general, our results do not appear to be sensitive to whether we use linear probability models or probit estimates.

shows that living in the inner-city is associated with detrimental child health outcomes with urban children having higher rates of accidents, injuries [Quinlan 1996], and asthma [Weiss et al. 1992]. Our fieldwork reinforced the idea that there were other important types of danger associated with living in a public housing project. For example, one woman described how the park at which they played in the projects was built on cement. "The place was not safe for the children to play. They had swings on concrete. Everything was on concrete. And that's where most of the accidents happened."

In the Follow-up Survey, we asked specifically about any injuries, accidents, or poisonings that a child may have been involved in during the past six months. Most injuries were related to a dangerous environmental factor (such as a used needle), were falls during "horsing around" and other play among children, or were sports-related (cycling, skating, ball-playing). In row 5 of Table VI we report results for injuries and accidents. There was a relative decrease among Experimental Compliers of 74 percent from a Control Complier mean of .160. The effect for the Section 8 Comparison group was about half as large and not statistically significant. For a comparison, we estimated the six-month prevalence of injuries, accidents, and poisonings in a national sample that we weighted to be demographically similar to MTO children. Our estimate of .029 is lower than all of the MTO groups including the Experimental Compliers.<sup>35</sup>

Asthma attacks are thought to be triggered in part by environmental factors, including crowding [Weitzman et al. 1992], poor air quality [Thurston 1997], stress [Wright 1998], lack of heat, and exposure to allergens from cockroaches, dust mites, mice, and rats [Gelber et al. 1993; Sharfstein et al. 1998]. Stress could come from exposure to violence, and many of the environmental irritants could result from poor housing. In the Baseline Survey, for instance, we found that 32 percent of MTO families reported that rats and mice were "a big problem," and 9 percent reported that the heating system was a big problem. Our field-

35. The estimate of .029 is based on the 1988 National Health Interview Survey Child Supplement (NHIS-CS) data, weighted to reflect MTO demographics. Because the NHIS-CS uses a twelve-month recall period instead of our six-month recall period, we then deflate the NHIS-CS estimates for comparison to the MTO-Boston results. Our deflator of .68 (instead of a simple .50) is derived from research on NHIS-CS recall effects [Harel et al. 1994] and accounts for the fact the respondents appear to be less likely to report injuries if they occurred many months ago in the recall period.

work suggested that a move out of a housing project apartment might remove a child from many of these environmental irritants. In row 6 of Table VI we report a relative decline of 65 percent in the probability of an asthma attack requiring medical attention among Experimental Compliers (TOT p-value .083), from a Control Complier mean of .16.<sup>36</sup> There was essentially no effect for the Section 8 Comparison group.

An obvious concern regarding the injury and asthma results is the qualification that the events were defined to “require medical attention.” We had included this qualification for comparability to national data, and to establish a threshold of severity. However, medical care may have been less accessible in the locations to which the Experimental Compliers moved. We attempted to measure this accessibility by asking whether the child had been to the doctor for a regular checkup. In row 7 of Table VI we find that the Section 8 Comparison group actually had a larger point estimate reduction in regular doctors’ visits than the Experimental group, although both effects are statistically insignificant. Based on this evidence, if access to medical care was the sole mechanism driving changes in reported injuries and asthma attacks, we would have expected any access-driven effects to have been at least as large in the Section 8 Comparison group as in the Experimental group.

## VII. ADULT ECONOMIC SELF-SUFFICIENCY

A primary motivation for the MTO demonstration is to measure the impacts of residential neighborhoods on the welfare receipt and employment of low-income families. Many central cities have higher rates of joblessness and slower rates of job growth than their surrounding areas. This pattern suggests that providing low-income city residents with better access to jobs in the suburbs may improve their labor market outcomes. The Experimental treatment might be expected to reduce welfare use and increase employment by moving participants closer to suburban job centers. But moves to new suburban neighborhoods might disrupt preexisting social networks that are an important

36. The 1997 NHIS shows that only .06 of a sample demographically similar to MTO children were reported to have had an asthma attack in the past year, while .13 were reported to have ever had asthma. The proportion of MTO children ages 6–15 in the Control group reported to have had an asthma attack in the past six months was .10, and the proportion reported to currently have asthma was .25.

source of labor market information and connections. It may take a significant amount of time in new neighborhoods to develop new networks. Thus, the theoretical predictions about the short-run effects of MTO on welfare and employment are ambiguous.

To the extent that families in the Experimental and Section 8 Comparison groups are able to move to safer neighborhoods with more plentiful after-school activities for children, parents may have child supervision constraints relaxed and be freer to enter the labor market and to work longer hours. Indeed, Rosenbaum and Popkin's [1991] analysis of a survey of female household heads in the Gautreaux program in Chicago found substantially higher employment rates (fourteen percentage points) for those who moved to the suburbs than for those who moved to other parts of the central city.<sup>37</sup>

The most striking facts in the MTO data are the changes over time for all three groups. Welfare receipt has fallen sharply, from 73 percent in the third quarter of 1994 (prior to any random assignment) to 40 percent in the third quarter of 1998. The employment rate, as measured by the fraction with positive earnings in a quarter, has also increased dramatically over time for all three MTO groups. Over four years, the employment rate rose from 29 percent in the third quarter of 1994 to 49 percent in the third quarter of 1998.

In Table VII we show the results comparing MTO groups based on these data for our main sample of 540 household heads. In the first row we pool data for the first three quarters after random assignment, and find no significant differences between MTO groups in rates of welfare receipt. The results are essentially the same for later periods, 4–6 quarters and 7–9 quarters after random assignment.<sup>38</sup> The results for employment similarly show no significant between-group differences. In results not shown in the table, we found no differences between MTO groups in quarterly earnings in constant 1999 dollars (including zeros for those with no reported earnings) for these periods. Although

37. The differential in survey response rates between the suburban movers (60 percent) and city movers (88 percent) is a source of concern in assessing this evidence, especially if the more successful suburban movers were more likely to have remained in their suburban location and been reached by and responded to the survey mailed to them.

38. Measured in calendar time rather than time since random assignment, there is some modest evidence of a decline in welfare usage among treatment group members in 1998. Pooling all four 1998 quarters, the overall control mean is .425. The Experimental group ITT estimate is  $-.025$  (.031) and the Section 8 group ITT estimate is  $-.064$  (.034).



TABLE VII  
IMPACT OF MTO-BOSTON ON WELFARE RECEIPT AND ADULT EMPLOYMENT

Quarters after random assignment	Control Overall Mean (1)	Experimental versus Control			Section 8 Comparison versus Control		
		Intent-to-Treat Difference (2)	Treatment-on-Treated Difference (3)	Control Compplier Mean (4)	Intent-to-Treat Difference (5)	Treatment-on-Treated Difference (6)	Control Compplier Mean (7)
Received AFDC/TANF							
1 to 3	.611	.002 (.028)	.005 (.060)	.641	-.006 (.031)	-.008 (.050)	.599
4 to 6	.560	-.004 (.036)	-.009 (.076)	.607	-.009 (.045)	-.014 (.072)	.533
7 to 9	.495	.004 (.039)	.008 (.084)	.523	-.035 (.050)	-.055 (.079)	.484
Positive earnings							
1 to 3	.368	-.006 (.035)	-.013 (.073)	.318	.003 (.041)	.005 (.065)	.411
4 to 6	.370	.023 (.036)	.049 (.077)	.302	.059 (.046)	.094 (.072)	.380
7 to 9	.434	.010 (.038)	.022 (.080)	.383	.029 (.047)	.046 (.075)	.475

Intent-to-Treat Difference uses OLS to control for the 24 household Baseline Survey characteristics from Table II, as well as indicators for calendar quarter and quarter since random assignment. Estimates of the Treatment-on-Treated Difference and the Control Compplier Mean are computed as described in Table VI. Robust standard errors adjusted for household clustering are reported in parentheses. Sample size is 540 individuals, and four quarters per individual (except row 3 in which two quarters are available). Welfare data are from the Massachusetts Department of Transitional Assistance. Earnings data are from the Massachusetts Department of Revenue.

these administrative data cover only earnings in the state of Massachusetts, we did confirm that over 95 percent of our sample was still living in Massachusetts in 1997 in the Follow-up Survey. Thus, even the most extreme assumptions about location and welfare or employment status of the remaining 5 percent would not generate significant differences in these between-group results.<sup>39</sup>

The overall lack of impact is consistent with the finding reported in Section III that only 2 percent of households in the Baseline Survey reported employment as one of the main reasons for wanting to participate in MTO. Similar results have been found for other MTO sites. Research from the Baltimore site using administrative data finds no significant effects in employment during the first four years after randomization [Ludwig, Duncan, and Pinkston 2000], although their analysis does suggest a decline in welfare receipt for the Experimental group. A household survey of early enrollees in the Los Angeles site finds no significant employment effects, but it does show modest evidence of increases in hours of work and weekly earnings for Experimental and Section 8 Comparison group household heads relative to those in the control group [Hanratty, McLanahan, and Pettit 1998].

#### VIII. FAMILY QUALITY OF LIFE

At the time of the baseline survey, 56 percent of the 540 household heads in our Boston sample reported that the main reason they wanted to move was “to get away from drugs and gangs.” Our qualitative interviews confirmed that fear for the safety of their children was the most powerful motive for most families in deciding to apply for MTO, and that this fear was greatly reduced among families that moved to new neighborhoods.

We initially hypothesized that improvements in safety may

39. We have also extended this analysis to the complete sample of all 961 household heads randomly assigned through MTO in Boston. The results for welfare, employment, and earnings are unchanged by this extension of the sample. In addition, our MTO-Boston Follow-up Survey asked about welfare receipt and a standard set of questions from the Current Population Survey on current labor market status (employment, unemployment, not in labor force), usual hours of work, and usual weekly earnings, as well as some supplemental questions on fringe benefits. Again, we find no evidence of significant differences between groups on any of these dimensions.

be associated with improvements in adult mental health and perhaps overall health, because of reduced stress. To take one example, a middle-aged Black woman who had lived in public housing for 25 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." In other qualitative interviews we also observed that adults who were living in public housing appeared to be in much more emotional distress than those who had moved out of public housing. One mediating factor influencing mental health that we speculated would potentially offset increased safety among MTO program movers was difficulty in maintaining or establishing social contact with friends and relatives in new locations.

To assess these hypotheses more systematically, we asked a number of related questions in the MTO-Boston Follow-up Survey. This section presents the results on aspects of family safety, adult health, and adult social interactions that may have substantial impacts on family quality of life.

### *A. Family Safety*

We find strong evidence that the MTO program has led to significant improvements in the perceived safety of treatment group families, declines in exposure to violence, and reduced rates of criminal victimization. While we had a sharp hypothesis that the treatment groups would be in neighborhoods with fewer drugs and guns, we had competing hypotheses regarding victimization since families moving into new neighborhoods may be particular targets of crime, especially among children who at least initially may get into more fights or be more likely to have items stolen. The results demonstrate that the program has led to both lower exposure to violence and reduced victimization, improving well-being along the dimension that was most important to program participants. Moreover, improved safety may be an

important contributor to reductions in child behavior problems and improvements in adult mental health.

We summarize the effects of the MTO treatments on self-reported measures of safety in Table VIII. The estimates in rows 1 and 2 show a large and statistically significant improvement in perceived neighborhood safety and children's exposure to weapons for the Experimental group relative to the Control group. The analogous results are reported in columns (5)–(7) for the Section 8 Comparison group, whose members also reported a lower prevalence of feeling unsafe and exposure to weapons—but this difference is not statistically distinguishable from zero. The next two rows of Table VIII presents results on whether family members have been exposed to drugs or gunfire. Families in both treatment groups experienced substantial declines in exposure to these neighborhood problems, with those in the MTO Experimental Group experiencing the largest declines. Overall, the Control Complier means for exposure to weapons gunfire, and drugs are generally higher than the overall Control means for both treatment groups, which is consistent with a decision process in which the families who take up an MTO voucher were those who would have been living in a more unsafe neighborhood in the absence of an MTO voucher offer.

We asked MTO household heads in our Boston Follow-up Survey about victimization of themselves and their children during the past six months. Our measures of victimization were modeled on the redesigned National Crime Victimization Survey (NCVS).<sup>40</sup> Overall, one-quarter of Control group households in the Follow-up Survey in the Control group reported at least one

40. For details on the NCVS redesign, see the Bureau of Justice Statistics [1994] overview. Our 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). In another attempt to improve upon the Baseline Survey victimization measures, we asked specifically about the dates of incidents in order to exclude those occurring more than six months prior to the interview.

Following the NCVS, we classified reported incidents into property and personal crimes. Our property crimes include household burglary and attempted entry, vandalism, and theft not directly from a person. Our personal crimes include physical attacks, threats of attack, and 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 being the person threatened, robbed, or attacked. Since our information on children's victimization is obtained from the household head, we probably underestimate actual victimization of children.

TABLE VIII  
IMPACT OF MTO-BOSTON ON SAFETY

	Experimental versus Control				Section 8 Comparison versus Control			
	Control Overall Mean (1)	Intent- to-Treat Difference (2)	Treatment- on-Treated Difference (3)	Control Complier Mean (4)	Intent- to-Treat Difference (5)	Treatment- on-Treated Difference (6)	Control Complier Mean (7)	
Streets near home are unsafe or very unsafe during the day	.386	-.144** (.048)	-.305** (.100)	.400	-.063 (.058)	-.112 (.095)	.400	
Child has seen someone with a weapon during the past three months	.102	-.071** (.029)	-.143** (.059)	.165	-.034 (.036)	-.055 (.055)	.098	
Head or children seen or heard gunfire once a month or more	.205	-.122** (.037)	-.260** (.079)	.260	-.098** (.045)	-.166** (.073)	.232	
Head or children seen people using or selling drugs once a week or more	.359	-.196** (.045)	-.408** (.093)	.432	-.125** (.054)	-.215** (.088)	.398	
Personal crime during past six months, at least one child involved	.127	-.059* (.031)	-.125* (.066)	.157	-.030 (.040)	-.053 (.065)	.144	
Personal crime during past six months, household head involved	.073	.009 (.029)	.018 (.062)	.038	-.035 (.026)	-.054 (.043)	.062	
Property crime during past six months	.134	-.070** (.034)	-.151** (.074)	.222	-.089** (.033)	-.146** (.057)	.221	

Intent-to-Treat Difference uses OLS to control for the 24 household Baseline Survey characteristics in Table II. Estimates of the Treatment-on-Treated Difference and the Control Complier Mean are computed as described in Table VI. Robust standard errors adjusted for household clustering are reported in parentheses; \* =  $p$ -value < .1, \*\* =  $p$ -value < .05. Sample sizes of households are 509 (row 1), 513 (row 3), 507 (row 4), and 519 (rows 5-7). Sample size in row 2 is 558 children ages 6-15; regressions in row 2 also include the seven child behavior and learning variables in Table II, and indicators for child's age and gender.

crime during the previous six months, and 12 percent had a head or child who was physically assaulted or robbed.

The results in rows 5–7 of Table VIII report the proportion of households with one or more incidents of crimes of various types.<sup>41</sup> Thirteen percent of Control households were victims of personal crimes during the past six months in which at least one child was involved. Over two-thirds of these personal crimes among children involved direct contact with the offender, such as mugging, fighting, punching, kicking, spitting, shoving, throwing of rocks, or “getting jumped” by a gang. The remainder involved attempted assault or verbal threats of violence. Experimental households had a prevalence six percentage points lower than the Control group ( $p$ -value .06). The difference for the Section 8 Comparison group is negative, but insignificant.

Household heads in the Section 8 Comparison groups have a lower prevalence of any personal crimes in the past six months, but the proportion is insignificantly different from the level in the Control group. The difference for the Experimental group is essentially zero. For property crimes, both treatment groups show lower prevalence of any incidents within the past six months.

### *B. Adult Health*

As a comprehensive measure of overall health, we report results in row 1 of Table IX from a standard question on general health (“In general would you say that your health is excellent, very good, good, fair, or poor?”). A consistent finding in the health literature is that self-ratings of health are strong predictors of subsequent health outcomes, such as mortality, in longitudinal studies [Idler and Benyamini 1997]. In the Control Group 58 percent of heads reported health that was good or better.<sup>42</sup> The Experimental TOT effect is .24; the relative change from the Control Complier mean of .46 is a 52 percent increase in the share with health that was good or better. Similarly, the Section 8 Comparison TOT effect is .26; the relative change from the Control Complier mean of .52 is a 51 percent increase. The Control Complier Mean is significantly lower than the Control Never

41. In results not reported in the table, we analyzed the average number of incidents, as opposed to the proportion of households with any incident. The results for the average number of incidents are qualitatively similar to the results for the proportion with one or more incidents.

42. A sample from the 1997 NHIS, weighted to reflect the demographic composition of the MTO participants (using race, sex, age, and education), was substantially more likely to report good or better health (.87).

TABLE IX  
IMPACT OF MTO-BOSTON ON ADULT HEALTH AND SOCIAL INTERACTIONS

	Experimental versus Control			Section 8 Comparison versus Control			
	Control Overall Mean (1)	Intent-to- Treat Difference (2)	Treatment- on-Treated Difference (3)	Control Complier Mean (4)	Intent-to- Treat Difference (5)	Treatment- on-Treated Difference (6)	Control Complier Mean (7)
Overall health is good or better	.578	.115** (.048)	.239** (.100)	.460	.162** (.056)	.264** (.092)	.523
Calm and peaceful "a good bit of the time" or more often during past 4 weeks	.465	.107** (.050)	.226** (.104)	.390	.138** (.063)	.229** (.103)	.429
Happy "a good bit of the time" or more often during the past four weeks	.561	.066 (.051)	.134 (.103)	.493	.049 (.061)	.083 (.097)	.573
Predicted probability of having had a Major Depressive Episode	.253	-.047 (.039)	-.098 (.082)	.343	-.061 (.047)	-.100 (.076)	.232
Visited w/friend or relative at own home at least once a week in past month	.482	-.058 (.053)	-.120 (.109)	.453	-.058 (.063)	-.095 (.101)	.506
Visited w/friend or relative at their home at least once a week in the past month	.422	.050 (.054)	.104 (.113)	.396	.050 (.063)	.083 (.102)	.393
Agree with: "Most people can be trusted" versus "You can't be too careful in dealing with people"	.078	.052 (.033)	.110 (.069)	.037	.017 (.037)	.032 (.061)	.037

Intent-to-Treat Difference uses OLS to control for the 24 household Baseline Survey characteristics from Table II. Estimates of the Treatment-on-Treated Difference and the Control Complier Mean are computed as described in Table VI. Robust standard errors are reported in parentheses; \* =  $p$ -value < .1; \*\* =  $p$ -value < .05. Sample sizes of households for rows 1-7, respectively, are 511, 508, 506, 511, 509, 512, and 499.

Taker mean in the Experimental group, suggesting that those with poorer health were more likely to accept the voucher if offered.

To explore aspects of mental health, we asked two questions about calmness and happiness used to assess positive affect in the evaluation of the RAND Health Insurance Experiment [Ware et al. 1994].<sup>43</sup> In the second row of Table IX, we find large treatment effects in reports of household heads feeling "calm and peaceful" during the past four weeks.<sup>44</sup> The relative increase for Experimental Compliers was 58 percent from a Control Complier mean of .39, and the relative increase for Section 8 Comparison Compliers was 53 percent from a Control Complier mean of .43. The large increase is particularly notable for Experimental group members, since 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 countervailing distress. The relative changes for feeling "happy" were also positive, but smaller in magnitude and not statistically significant.

To work with a clear definition of a mental health problem, we focused on symptoms of a particular psychiatric condition, major depressive episode (MDE), using a series of questions from the Composite Diagnostic Interview Short Form (CIDI-SF) [Kessler et al. 1998]. Based on their work with the more comprehensive National Comorbidity Survey, Nelson, Kessler, and Mroczek [1998] develop a rule for predicted MDE cases by assigning a probability to respondents with a particular number of symptoms. The MTO sample appears to have a much higher prevalence of MDE symptoms than a national population of similar age, race, education, and sex, but may be fairly typical of very low-income single mothers.<sup>45</sup> For the outcome of predicted MDE

43. An alternative interpretation of these self-ratings is that they represent a subjective response to winning a housing voucher in a lottery, and not mental health status caused by residential location. We interpret literature in psychology as evidence against this interpretation, since people adapt to many life events in short periods of up to three months [Suh, Diener, and Fujita 1996] and we are measuring responses an average of two years after the voucher award.

44. We examined a demographically adjusted sample from the 1987 National Medical Expenditure Survey (NMES), and found them more likely to be calm and peaceful a good bit of the time or more (.57) than the MTO Controls (.47). Experimental Compliers (.62) and Section 8 Comparison Compliers (.66) are more similar to the general population ages 18–64 (.66).

45. 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, the predicted MDE probability was .12 as opposed to .25 in the MTO



in Table IX, row 4, we find that the point estimates suggest a decline in depression among the treatment groups, but the differences are not quite statistically significant at conventional levels. We note that the estimated magnitude of the relative declines of 29 percent for Experimental Compliers and 43 percent for Section 8 Comparison Compliers are quite large, which implies that our sample size is simply not big enough to reliably detect changes of large magnitude for this outcome.

### *C. Adult Social Interactions*

To assess differences in social interactions caused by participation in the MTO program, we asked several questions in the Boston Follow-up Survey, and results are reported in rows 5–7 of Table IX.<sup>46</sup> First, respondents were asked about the number of times they had visited in person with friends and family in the past month. The point estimates of the ITT effects suggest that Section 8 Comparison and Experimental families are less likely to have visits to their home and more likely to make visits to the homes of others, but all of these differences are statistically insignificant. To assess social isolation, we examined whether a respondent reported any visits in the past month, either to their own home or to the homes of friends or family. Ninety percent of the households in all three groups had at least one visit, and there were essentially no differences between the groups.

As an alternative measure of social relations that might influence mental health, we also examined an indicator of social trust based on agreement with the statement “Most people can be trusted” as opposed to “You can’t be too careful in dealing with people.” Answers to this question appear to be highly correlated with various aspects of social engagement, such as membership in civic organizations, voting, and spending time with neighbors [Putnam 1995].<sup>47</sup> Only 8 percent of Control group household

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Control group. In the general population of adults ages 18–64 with children, predicted MDE was .07. In the 1997 Women’s Employment Survey in Michigan [Danziger et al. 1999], however, a population of single mothers receiving cash assistance was found to have a predicted MDE of .27, within sampling error of the .25 mean for the MTO Control group.

46. Based on data from the NMES, 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.

47. Recent experimental work by Glaeser et al. [2000] suggests that this question predicts the trustworthiness of a person’s own behavior, but not their trust in the expected behavior of others.

heads report that most people can be trusted. We find that the Experimental group exhibits more social trust than the Control group, with a TOT difference of .11 from a Control Complier Mean of .04, but the statistical significance of this TOT estimate is marginal ( $p$ -value .11).<sup>48</sup>

## IX. CONCLUSION

This paper provides early estimates of the impacts of the MTO housing mobility demonstration on families originally residing in high-poverty housing projects in inner-city Boston. The Experimental treatment is more successful than regular Section 8 in relocating poor families into 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.

Table X highlights our main findings concerning the short-run impacts of MTO on measures of child health and behavior problems, adult economic self-sufficiency, family safety, and adult health (with outcomes defined such that negative changes correspond to improvements). We find that children in both the Experimental and Section 8 Comparison groups exhibit fewer behavior problems, and that Experimental group children have lower prevalence of injuries, asthma attacks, and personal crimes. In contrast, the changes in neighborhoods induced by MTO have not affected the employment rates, earnings, or welfare usage by a statistically detectable amount for household heads. However, there do appear to be significant improvements in the general health status and mental health of household heads. Table X further illustrates that the magnitudes of the estimated relative changes in child and adult health outcomes among treatment compliers are very substantial.

We believe that one of the principal reasons that our results provide credible evidence of improvements in outcomes, particularly in the domain of health, is the consistent direction of the results for a variety of outcomes. Throughout this paper we have considered

48. In contrast to other indicators of social relations, the MTO sample differs substantially from the general population of adults with children, of whom 31 percent report that most people can be trusted in the General Social Survey (GSS) from 1990–1996. This difference is largely due to demographic factors. Weighting the GSS by the sex, race, education, and age of the MTO sample, only 8 percent report that most people can be trusted.

TABLE X  
SUMMARY OF IMPACTS OF MTO-BOSTON ON OUTCOMES

	Experimental versus Control		Section 8 Comparison versus Control	
	Control Complier Mean (1)	Relative Change for Treatment Compliers (2)	Control Complier Mean (3)	Relative Change for Treatment Compliers (4)
A. Children's human capital accumulation				
Behavior problems index (boys)	.44	-42%**	.45	-36%**
Behavior problems index (girls)	.20	-24%	.23	-34%
Injury requiring medical attention	.16	-74%**	.14	-43%
Asthma attack requiring attention	.16	-65%*	.11	-9%
B. Adult economic self-sufficiency				
Receiving TANF 7-9 quarters after enrollment	.52	+ 2%	.48	-11%
Not employed 7-9 quarters after enrollment	.62	-4%	.53	-9%
C. Safety				
Heard gunfire in neighborhood	.26	-100%**	.24	-72%**
Seen drugs in neighborhood	.43	-95%**	.40	-54%**
Child attacked, robbed, threatened	.16	-80%*	.15	-37%
D. Adult health				
Overall health fair or poor	.54	-44%**	.48	-55%**
Calm and peaceful some of the time or less	.61	-37%**	.57	-40%**
Happy some of the time or less	.51	-27%	.43	-19%
Predicted probability of major depressive episode	.34	-29%	.23	-43%

Control Complier Means and Relative Changes for Compliers (based on TOT Differences) are derived from coefficients in Tables VI-IX.

\* =  $p$ -value < .1; \*\* =  $p$ -value < .05 (based on TOT estimates).

each outcome separately, but in assessing the joint significance of the results it is of course important to note that all the outcomes are measured in the same sample, and also that both treatment groups use the same control group. Estimates of the joint probability of such a consistent pattern of results indicate that such a pattern is extremely unlikely to be due to chance.<sup>49</sup>

49. Specifically, we estimated the odds that all eight health-related outcomes in panels A and D for both treatment groups shown in Table X would indicate health improvements simply due to sampling variability. To compute these odds, we simulated the null hypothesis of no true difference between the Experimental, Section 8 Comparison, and Control groups, accounting for the covariance struc-

Although the eventual long-term effects of MTO on the children of participating families remains a crucial open research issue, the short-term impacts on adult and child circumstances are of substantial independent importance and may be harbingers of the long-run effects of changes in neighborhood environments.<sup>50</sup> From the perspective of housing policy, our results clearly indicate that offering housing vouchers to the residents of distressed inner-city housing projects improves the well-being of those residents interested in moving out of public housing.<sup>51</sup> If the (long-run) benefits from moving to new neighborhoods are particularly large for children and if parents do not fully internalize these impacts, housing vouchers for low-income families may generate positive intergenerational externalities relative to other income transfer mechanisms [Lazear 1983].<sup>52</sup> However, we do not know what the impact of MTO was on other families left behind in the housing projects when MTO families left or on the neighbors of MTO families in the new neighborhoods.

In summary, the lack of significant short-run impacts of the MTO Experimental treatment on adult employment and welfare participation suggests that reductions in spatial mismatch alone may not be sufficient to overcome the labor market disadvantages of inner-city, single mothers. The reductions in child problem

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ture within households among outcomes, drawing 100,000 random samples with replacement of 520 households from the Control group in the MTO-Boston Follow-up Survey. These households were then randomly designated as treatment or control groups of the same sample sizes as the Experimental and Section 8 Comparison groups. For each sample, we then computed the between group differences and recorded the signs of the differences. Note that these are all of the health outcomes on which we collected data, so there is no issue of presenting only the significant outcomes. These results show that under the null hypothesis of no difference, the odds were 617 to 1 of randomly obtaining 16 improvements in health-related outcomes for children and adults in the Experimental and Section 8 Comparison groups ( $p$ -value .00162).

50. We attempted to include interactions with time since enrollment in MTO in order to study differences in treatment effects by length of exposure to new neighborhoods. Unfortunately, this analysis did not have sufficient statistical power to detect even large differences.

51. Our findings do not necessarily translate into positive effects for other public housing residents of a switch from project-based housing assistance to vouchers. Those volunteering for the MTO lottery (and especially the MTO movers) are likely to be public housing residents with particularly high expected returns from relocating. In fact, a recent study by Jacob [2000] finds no mean gains in educational outcomes (at least in the short run) for children leaving high-rise public housing in Chicago because of demolitions relative to those remaining in public housing.

52. In this case, housing vouchers may serve as a commitment device to ensure that the transfer is disproportionately used to generate improvements in child well-being valued by society-at-large.

behaviors (especially for boys) and improvements in child health associated with access to more-affluent neighborhoods through MTO are consistent with models of neighborhood effects emphasizing positive peer social interactions and the impacts of adult role models and community resources on human capital accumulation. The large improvements in family safety and adult mental health brought about by moves out of public housing through MTO may have been an important mechanism in bringing about these health and behavior changes among children.

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