

**Neighborhood Effects on Economic Self-Sufficiency:  
Evidence from a Randomized Housing-Mobility Experiment**

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**Abstract**

This paper examines whether residence within high-poverty urban neighborhoods affects individual economic outcomes. Our data are generated by a randomized housing-mobility experiment, with measures of economic self-sufficiency taken from state administrative records. We find that providing low-income families living in public housing units with private-market rental subsidies that can only be redeemed in very low-poverty neighborhoods reduces rates of welfare use by around 15 percent. Most of this reduction appears to be explained by differences in welfare-to-work transitions. We also find that providing families with unrestricted housing vouchers has little effect on economic outcomes beyond the first year.

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## I. INTRODUCTION

Between 1970 and 1990, the number of people living in high-poverty census tracts<sup>1</sup> in the United States nearly doubled, from 4.1 to 8.0 million (Jargowsky, 1997). The growing concentration of urban poverty is of concern in part because of the potential implications for the well-being of the residents of such neighborhoods. An emerging literature in economics and sociology suggests that residence within high-poverty urban areas may depress labor market outcomes because of limited access to job networks, middle-class role models, or other forms of social capital, exposure to anti-social norms, or increased distance to suburban job market opportunities (Wilson, 1987, Kain, 1968, Holzer, 1991, Mills and Lubuele, 1997, Topa, 1997).

The possibility that neighborhood characteristics affect economic outcomes has important policy implications, since the spatial concentration of poverty in America is in part a function of previous policy decisions. For example, during the last 50 years, over 1 million units have been added to the public housing stock in the United States, many of which were, for political reasons, clustered together in central-city neighborhoods (Quigley, 1999). Public housing programs have thus contributed to the concentration of poor (and disproportionately minority) families in densely populated urban neighborhoods.<sup>2</sup> The effects of housing policies in this regard have been exacerbated in part by the migration of middle-class African-American families to the suburbs beginning in the 1970's (Wilson, 1987). If residence within high-poverty neighborhoods depresses individual economic outcomes, programs designed to improve the housing situation of low-income families may essentially impair the ability of recipients to become economically self-sufficient.

Motivated in part by concern about the effects of high-poverty neighborhoods on low-income families, government housing policies have recently emphasized new rental-subsidy programs that have the potential to reduce the concentration of poverty in urban areas. For example, the fiscal year 1999 and 2000 budgets for the U.S. Department of Housing and Urban Development (HUD) include funds for 75,000 additional Section 8 vouchers and certificates that provide eligible families with subsidies to live in private-market rental housing (hereafter "housing subsidies"<sup>3</sup>) as part of the Clinton Administration's Welfare-to-Work initiative (US HUD, 1999). More generally, between 1975 and 1998 the number of housing subsidies provided to low-income families in the U.S. increased ten-fold, from 162,000 to 1.6 million (Quigley, 1999).

Yet too little is currently understood about the effects of neighborhood attributes on economic self-sufficiency. Non-experimental studies have produced mixed findings for whether economic outcomes are correlated with various measures of transportation and job access,<sup>4</sup> but somewhat stronger evidence that such outcomes are correlated with the socioeconomic status (SES) of others in the surrounding neighborhood.<sup>5</sup> Interpretation of these findings is complicated by a fundamental identification problem: Since most families have at least some degree of choice over where they live, correlations between neighborhood variables and economic outcomes may reflect either the causal effects of neighborhood conditions, or the effects of unmeasured family-level attributes associated with both residential decisions and the outcome measures of interest. The magnitude and even direction of bias that may result is difficult to determine; those families most likely to succeed in the labor market could plausibly choose to live in either low-poverty suburban areas (to take advantage of neighborhood effects) or higher-poverty urban areas (to take advantage of lower housing costs, assuming neighborhood amenities are capitalized into housing prices).<sup>6</sup>

The best evidence on the existence of neighborhood effects comes from the quasi-experimental Gautreaux program in Chicago, which moved public housing residents to other parts of the metropolitan area with little choice over where they went. Families that moved to the suburbs were found to have higher employment rates but similar wages compared to city movers (Rosenbaum and Popkin, 1992, Rosenbaum, 1995). Since families were not assigned to different neighborhoods as part of a formal randomized design, there necessarily remains some question about whether these differences reflect the causal effects of neighborhood conditions.

The present paper re-examines the effects of neighborhoods on economic self-sufficiency using data generated by a controlled randomized housing-mobility experiment. Since 1994, the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) demonstration has assigned a total of 638 families from high-poverty Baltimore neighborhoods into one of three different "treatment groups": *Experimental group* families receive housing subsidies, counseling and search assistance to move to private-market housing in low-poverty census tracts (poverty rates under 10 percent); *Section 8-only group* families receive private-market housing subsidies with no constraints on relocation choices; and a *Control group* receives no special assistance under MTO. The randomized experimental design of MTO thus breaks the link between family residential preferences and adolescent outcomes, and helps us overcome the endogenous-membership problem found with most previous studies.

Using state administrative records to measure economic outcomes, we find that families assigned to the experimental group have lower rates of welfare receipt than those in the control group. Most of the difference in welfare use (around 6 percentage points, or 15 percent of the control group's rate of welfare receipt) seems to be explained by differences in the proportion of experimental and control group families that record welfare-to-work transitions in the state's welfare database. Since many of these jobs are apparently not covered by the state's unemployment insurance (UI) system, quarterly UI records fail to detect any treatment effect on employment or earnings, though the 95 percent confidence intervals around our UI estimates are not inconsistent with the treatment effects suggested by the welfare data. We also find little evidence of a treatment effect beyond the first year on any of these outcome measures for families in the Section 8-only group. We hasten to add that since MTO families are a self-selected group of public housing residents, our findings may not generalize to other populations.

The paper is organized as follows. The next section describes the MTO experiment in greater detail. The conceptual framework for our analysis is discussed in Section 3, while the data used in the analysis is reviewed in Section 4. The fifth section presents empirical results for the mobility outcomes of MTO families, quarterly employment and earnings, welfare receipt, and householder cohabitation. The paper concludes with a discussion of the interpretation and implications of our findings.

## II. THE MOVING TO OPPORTUNITY DEMONSTRATION

The Moving to Opportunity demonstration is based in five U.S. cities: Baltimore, Boston, Chicago, Los Angeles, and New York. The MTO program was designed "to evaluate the impacts of helping low-income families move from public and assisted housing in high-poverty inner-city neighborhoods to better housing, education, and employment opportunities in low-poverty communities throughout a metropolitan area."<sup>7</sup> The possibility that living in high-poverty areas may have detrimental effects on children has been particularly important in motivating MTO, as evidenced by the restriction of program eligibility to families with children.

In fact, there is already some evidence to suggest that the experimental and Section 8-only treatments reduce teen problem behavior and involvement in violent crime (Katz, Kling and Liebman, 1999, Ludwig, Duncan and Hirschfield, 1999). This paper builds on this literature by examining MTO's effects on adult economic outcomes in the Baltimore site.

In addition to requiring that participants have children, MTO program eligibility in Baltimore was further restricted to families living in public housing or Section 8 project-based housing in one of the five poorest census tracts in Baltimore City. The average poverty rate in these tracts in 1990 was 67 percent (Goering, Carnevale and Teodoro, 1996). The baseline neighborhoods are also notable for a paucity of affluent neighbors, which previous research suggests has a distinct effect on behavioral outcomes from neighborhood poverty (Brooks-Gunn, Duncan and Aber, 1997). Less than five percent of households in these tracts had annual incomes of \$50,000 or more (in 1990 dollars), and less than seven percent of adults had a college degree.

The program was publicized in the baseline tracts by the Housing Authority of Baltimore (HAB) and a local nonprofit, the Community Assistance Network (CAN). Families that volunteered for the program were added to the MTO waiting list. Families were drawn off the MTO waiting list over time on the basis of a random lottery, and then randomized into one of the three MTO treatment groups. Both types of randomization were conducted by Abt Associates. Families in the experimental and the Section 8-only groups were assigned Section 8 housing vouchers or certificates, which provide subsidies to lease private-market housing.<sup>8</sup> As part of the program's design, the Section 8 subsidies provided to the experimental group can only be redeemed for housing in census tracts with 1990 poverty rates less than 10 percent. Families in both the experimental and Section 8-only groups had up to 180 days from the time at which they officially began the housing search to identify a suitable rental unit and sign a lease.

The experimental group also received services from CAN, the local Baltimore nonprofit, including assistance to resolve credit problems and to locate and lease suitable rental housing. Before the housing search was initiated, CAN required experimental families to complete four workshops on topics such as budgeting, conducting a housing or job search, dealing with landlords, conflict resolution, and utilization of available government services such as the Earned Income Tax Credit (EITC). Families in the Section 8-only group received no additional assistance beyond what is provided to all participants in HUD's Section 8 subsidy program. Families in both the experimental and Section 8-only groups were required to sign one-year leases. Those that wished to move again before the initial lease expiration date were not eligible for a new Section 8 subsidy. Families that wished to relocate with their subsidy after the first year were able to do so without restriction. CAN contacted experimental families twice following relocation; otherwise, post-program monitoring was limited. (For additional details on MTO see Goering, Carnevale and Teodoro, 1996).

### **III. CONCEPTUAL FRAMEWORK**

The potential effects of neighborhood characteristics on economic outcomes are highlighted by the reduced-form equation (1) from Moffitt (1998). Our focus is on outcomes such as welfare receipt, employment, or earnings,  $Y_{in}$ , for some individual (i) in neighborhood (n). These outcomes will be a function of the householder's characteristics,  $X_{in}$ , including educational attainment and age, the characteristics of those living in family (i)'s neighborhood,  $X_{-in}$ ,<sup>9</sup> neighborhood-specific unobservables,

$n$ , and unmeasured variables that are specific to the family and neighborhood,  $G_{in}$ .

$$Y_{in} = \beta_0 + \beta_1 X_{in} + \beta_2 X_{-in} + G_n + G_{in} \quad (1)$$

The possibility that the behaviors or characteristics of one's neighbors ( $X_{-in}$ ) may affect behavior has long been a topic of interest to sociologists, and in recent years has received growing attention from economists as well. Since informal referrals are an important source of information for both employers and less-skilled workers in the job search process (Holzer, 1987, 1996), many studies focus on across-neighborhood variation in the proportion of employed residents (Wilson, 1987, 1996, Montgomery, 1991, Topa, 1996). These models emphasize the effects of the social capital provided by employed neighbors in the form of information about job openings and job references. The social composition of neighborhoods may also affect local social norms supporting work versus welfare (Wilson, 1987), a possibility that receives some support from empirical evidence of a "welfare stigma" effect (Moffitt, 1983). Finally, marriage and cohabitation have been found to be important routes from poverty for many low-income families (Bane and Ellwood, 1986), and suitable marriage partners may be less prevalent in high-poverty areas (Wilson, 1987).

Another possibility is that the *location* of the neighborhood may affect labor market outcomes. Since John Kain's seminal 1968 article, economists have been concerned about the possibility of a "spatial mismatch" between less-skilled minorities in central city neighborhoods and suburban job opportunities (Kain, 1968). If minorities face racial discrimination in suburban housing markets, or if suburban employers are more discriminatory than those in urban areas, the result will be depressed wages in urban labor markets due to crowding, or low net earnings from suburban jobs due to high commute costs. Some workers may respond to these reductions in net wages by dropping out of the labor market. Since measuring a neighborhood's distance or access to job opportunities is quite difficult in practice (Leonard, 1986, Raphael, 1998), we treat this as an unmeasured neighborhood variable,  $G_n$ . Other unmeasured neighborhood characteristics that may affect economic outcomes include the availability of public transportation or other public services, such as the quality of local social services.

One practical problem is that most of these theories yield similar empirical predictions for the parameters in equation (1), which, in turn, makes it difficult to determine which mechanisms are most important in affecting behavior—evidence that residence within a high-poverty central city neighborhood depresses employment rates is consistent with both spatial-mismatch and social-interactions models. MTO provides us with no leverage on this problem because the demonstration changes multiple neighborhood characteristics simultaneously for program-movers.<sup>10</sup> Nevertheless, MTO does enable us to test the joint hypothesis that neither the location nor the social composition of inner-city neighborhoods affect economic self-sufficiency.

The more common concern with estimating the effects of neighborhood characteristics on economic variables stems from the possibility that unmeasured individual-level variables that affect residential choices are also correlated with behavioral outcomes, as in equation (2). In this case, ordinary least squares estimates of (1) will confound the causal effects of neighborhoods with the effects of the unobserved family-level variables.

$$E [ X_{-in} G_{in} ] = 0 \quad (2)$$

MTO helps overcome the selection problem by randomly assigning families into different mobility treatment groups. In practice, only the family’s treatment group is randomly assigned, not their actual mobility outcome. We overcome this problem by comparing mean economic outcomes of all families assigned to each of the three MTO treatment groups regardless of the family’s relocation status, known as the “intent-to-treat” (ITT) effect in the evaluation literature. More formally, assume for simplicity that there is only one treatment and one control group, and let  $Z=1$  indicate assignment to the treatment group (with individual and neighborhood subscripts suppressed for simplicity).<sup>11</sup> The ITT effect is then given by equation (3), which captures the combined effects of changing  $X_{in}$  and  $G_n$  simultaneously.

$$ITT = E [Y | Z=1] - E [Y | Z=0] \tag{3}$$

The ITT estimates are obtained by estimating equation (4), where the program impacts are given by the ordinary least squares estimate for  $\beta_1$ . The model includes a vector of background variables ( $X_{in}$ ) to improve the precision of our estimates. The inclusion of these variables should have little effect on the point estimates for  $\beta_1$  in large samples, but may have some effect in practice if the distributions of these background variables differ somewhat across treatment groups due to random chance.

$$Y_{in} = \beta_0 + \beta_1 Z_{in} + \beta_2 X_{in} + v_{in} \tag{4}$$

In cases where the ITT effects are non-zero, we are also interested in identifying the effects of the program on those who comply with their treatment assignments—that is, the effects on those who relocate through MTO. This parameter is of interest because it is relevant for understanding the effects of similar housing programs that may have different compliance rates. If  $C$  equals 1 for those who would comply with the treatment if they were assigned into the treatment group (that is, “potential compliers”), then the “effect of treatment on the treated” (TOT) is given by equation (5).

$$TOT = E [Y | Z=1, C=1] - E [Y | Z=0, C=1] \tag{5}$$

Since we cannot determine which control group families would have complied with the treatment had they been assigned to the treatment group, the value  $E [Y | Z=0, C=1]$  is not directly observed in the data. However, the TOT effects can be recovered if assignment to each of the MTO treatment groups was in fact random, so that the proportion of potential treatment-compliers is equal across groups, and if the experimental and Section 8-only treatments have no effect on the outcomes of non-compliers (that is, the families that are assigned into one of these treatment groups but do not relocate through MTO).<sup>12</sup> Baseline data for the MTO families (presented below) suggest that the first assumption appears to be met—randomization was in fact conducted properly. Assuming that non-compliers assigned to the treatment group do not experience a “disappointment effect,” the second assumption will unambiguously be met in the case of the Section 8-only treatment since families assigned to this group receive no additional MTO services beyond the offer to relocate. Whether the assumption is met with the experimental treatment is more complicated, because experimental-group non-compliers could in principle be affected by the counseling services offered as part of this treatment. Yet in practice

even intensive adult training and counseling programs appear to have very modest effects (Heckman, Lochner, Smith and Taber, 1997).

If these two assumptions are met, the TOT estimates can be calculated by defining the indicator variable  $D$  for observed (rather than potential) compliance with the treatment, and estimating equation (6) using  $Z$  as an instrument for  $D$ . The parameter of interest from (6) will equal  $\beta_1 = (\beta_1 / P[C=1 | Z=1, X])$ , which is the ITT estimate from equation (4) divided by the predicted probability of compliance with the MTO treatment, conditional on the baseline characteristics (Bloom, 1984, Manski, 1996, Katz, Kling and Liebman, 1999). Finally, in order to gauge the magnitude of the TOT effect it is useful to calculate the average outcomes of those families that are assigned to the control group but would have complied with the treatment (the “control-complier mean,” or CCM), as in equation (7) (Katz, Kling and Liebman, 1999).

$$Y_{in} = \beta_0 + \beta_1 D + \beta_2 X_{in} + \epsilon_{in} \quad (6)$$

$$CCM = E[Y | C=1, Z=0] = E[Y | C=1, Z=1] - TOT \quad (7)$$

The econometric framework for the case of two treatment groups generalizes straightforwardly from the simplified setup discussed here. The next section discusses the data used to estimate the ITT and TOT effects of interest.

#### IV. DATA

Data for this analysis comes from four sources: baseline survey and follow-up address data, both collected by Abt Associates; administrative data on public assistance (PA, or, equivalently, “welfare”) receipt; and administrative data on quarterly employment and earnings.

##### A. Baseline surveys

Applicants to the MTO program were required to complete a self-administered questionnaire designed by Abt Associates, which included questions about the householder’s personal demographic characteristics as well as her educational attainment, current employment status (or job-search and training activities), and participation in social programs. The survey also included questions about the age, educational attainment, employment status and relation to household head of others living in the household.

##### B. Post-program addresses

Abt is responsible for tracking the addresses of MTO families following their entry into the program. The first set of addresses record the initial program-moves made by experimental and Section 8-only compliers. A second set of follow-up addresses, current as of December, 1997, were gathered through administrative data from local housing agencies, searches of change-of-address registries and credit-bureau data, and a brief follow-up survey of families conducted between July and December of 1997. These surveys were conducted on the phone for as many families as possible; those that could not be reached by telephone were interviewed in person. The survey included questions about the current composition of the household, the new addresses of people who were listed as members of the household on the baseline survey but no longer lived with the householder, and the age, gender, and relation to household head of new members of the household. The response rate to Abt’s survey was 91 percent.<sup>13</sup>

### C. Administrative Data for Public Assistance

The Maryland Department of Human Resources (DHR) maintains administrative records on receipt of public assistance (PA) cash benefits by residents of the state of Maryland, including the start and end date of every PA spell, and the monthly benefit amount for the family's most recent spell. In response to the Federal Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (PRWORA), the state of Maryland now requires families receiving PA benefits to spend at least 20 hours per week in an acceptable work or training program beginning in the family's 24<sup>th</sup> month of participation in the Temporary Assistance for Needy Families (TANF) program. It is well-known that AFDC / TANF caseloads nationwide have decreased in recent years, and Maryland is no exception. If the recent reductions in welfare caseloads have changed the "welfare culture" in the MTO baseline neighborhoods, as many proponents of the PRWORA changes claim, our estimates may understate the effects that MTO would have in times of high welfare caseloads.

The DHR used social security numbers to match our list of MTO participants with PA administrative records that are current as of August, 1998, which is on average 3.2 years after random assignment for MTO families (with a minimum of 1.9 years and a maximum of 3.8 years). In cases where no match was found, DHR searched again by the MTO participant's date of birth and completed the match using first and last name. The match rate resulting from this process is of critical importance, since the estimated effect of MTO on employment and earnings in UI-covered jobs will be proportional to this match rate. For example, let  $W_E$  and  $W_C$  represent the proportion of householders who receive welfare benefits in the experimental and control groups, respectively, and let  $M$  represent the match rate. (Since these administrative data are maintained at the state level, we assume the match rate is equal across groups). As seen in equation (8), the estimated impact of MTO on welfare receipt equals  $M$  times the true effect.

$$\text{Estimated Impact} = (M \cdot W_E) - (M \cdot W_C) = M \cdot (W_E - W_C) \quad (8)$$

Comparisons of self-reported welfare receipt on the baseline surveys with the state administrative data suggest that the match rate is on the order of 80 to 90 percent. A full 98 percent of MTO householders report that they have received AFDC benefits at some point during their lives; of this group, the DHR matched administrative AFDC / TANF records for 89 percent. When we focus on PA status at the time of program entry by comparing self-reported PA receipt on the baseline surveys with the DHR records, we find disagreement in the household's baseline PA status in 20 percent of cases. Almost all of the disagreements (106 of 122 families) consist of households that receive PA benefits according to the baseline surveys, but not according to the DHR administrative data.<sup>14</sup>

As part of the state's Work Opportunity Management Information System (WOMIS), the DHR also maintains records on each "job placement de-registration"—that is, cases where a family officially leaves welfare because of employment. Families record welfare-to-work exits in Maryland when their monthly on-the-books earnings (excluding the value of government benefits such as EITC payments) exceed the state's eligibility criteria, so welfare-to-work exits can occur because of transitions into new jobs, or because of sufficiently large increases in earnings in people's present jobs.<sup>15</sup> The number of welfare-to-work exits recorded for MTO participants during the post-program period ( $N=45$ ) is only a fraction of the total number of welfare exits for this population, consistent with findings that only one-quarter of all welfare exits in Maryland from 1996 through 1998 were officially closed because of employment (Born,

1999). Presumably, some welfare-to-work exits were officially recorded as closed for other reasons (for example, because the recipient failed to reapply or formally requested closure of her welfare case).

#### **D. Administrative Data on Employment and Earnings**

The Maryland Department of Labor, Licensing and Regulation (DLLR) maintains complete quarterly employment and earnings histories for people employed in jobs covered by the state's unemployment insurance (UI) system. Government regulations require that any employer who pays more than \$1,500 in wages to one or more employees during a quarter is subject to the state's UI tax. Employers who meet this condition must report the quarterly payments made to each employee. Exempt from this tax are the self-employed, government employees, railroad employees, those who work part-time at non-profit groups, those who work for religious organizations, students who are employed by their schools, and most independent contractors (Kornfeld and Bloom, 1999). Around 93 percent of all jobs in Maryland's formal labor market are covered by the state UI system (Born, 1999).

The DLLR used social security numbers to match our MTO participant lists with UI earnings records covering the period from the second quarter of 1995 through the first quarter of 1999.<sup>16</sup> The UI data thus provide on average 3.8 years of post-program information for MTO families for which there was a match, with a minimum of 2.4 post-program years and a maximum of 4.4 years. The match rate from this process appears to be quite high—for MTO householders who reported holding a job for pay at some point in their lives, the DLLR found a UI earnings history in every case.

These UI data enable us to construct employment and earnings histories that are less susceptible to misreporting problems such as recall error or self-presentation bias than survey data. UI records may also be less susceptible to sample attrition than surveys. The primary drawback of UI data comes from the fact that many income sources are not captured by these records. While previous research finds that survey and UI data typically produce similar estimates for the impacts of government job-training programs (Kornfeld and Bloom, 1999), both standard social-science surveys and UI records are likely to miss off-the-books earnings that account for a substantial share of total income for welfare recipients (Edin and Lein, 1997). These unreported sources of income will bias our estimates for program effects on employment and earnings if opportunities for off-the-books work varies across neighborhoods, for example if more affluent households have a greater demand than poor families for housekeeping, child care, and other personal services that frequently go unreported to government agencies. Nevertheless, employment in a UI-covered job is an interesting outcome in its own right, since this is an important indicator of economic success for the MTO population.

### **V. EMPIRICAL RESULTS**

We begin by presenting information about the baseline characteristics and relocation outcomes of MTO families. We then present our estimates for the effects of the MTO treatments on economic outcomes.

#### **A. Characteristics of the MTO Population**

Table 1 presents information about MTO participants from the baseline surveys. Almost all of the householders in the Baltimore MTO demonstration are unmarried African-American

women. Nearly half of MTO householders have either a high school diploma or GED, almost none had access to a car, and the large majority received AFDC benefits at the time of the baseline survey. While the majority of householders report that they have held a job for pay at some point in their lives, only one-quarter were working at baseline.<sup>17</sup>

Previous research suggests that informal job networks play an important role in the job search process for less-skilled workers (Holzer, 1987), a finding that is confirmed by the baseline information: Around two-thirds of all householders employed at baseline first heard about their current job from a neighbor, friend, or family member.

Despite the very low average earnings and employment rates reported in Table 1, most families did *not* enroll in MTO to gain access to better job opportunities. As shown in Table 2, around 80 percent of MTO applicants report that escaping gangs and drugs is the first or second most important reason for joining the program. This motivation is not surprising given that over half of the MTO applicants also report that at least one household member had been victimized by a crime during the past six months. While this victimization rate may be somewhat over- or under-stated due to telescoping, strategic behavior and other reporting problems (Skogan, 1981), this figure is nevertheless substantially higher than the six-month victimization rate of six percent reported by residents of New York City public housing (Goering, Carnevale and Teodoro, 1996).

With random assignment, the characteristics of families should differ across the MTO treatment groups only by chance. That appears to be the case. Multivariate analysis of variance is used to test the null hypothesis that the full set of means presented in Tables 1 and 2 are equal across the three MTO groups (Johnson and Wichern, 1992). The relevant test statistic is consistent with the idea that the three groups are indistinguishable with respect to these observable characteristics ( $p=.75$ ).

## **B. Relocation Outcomes**

Relative to the experimental group, a larger proportion of Section 8-only families relocated through the MTO program in Baltimore (73 versus 54 percent). Of the Section 8-only families that did not relocate through MTO, almost all contacted the Baltimore housing office and requested a Section 8 subsidy, but then could not sign a lease before the subsidy offer expired. In contrast, only half of the experimental group non-compliers ran up against the Section 8 subsidy time limit. One-quarter of the experimental non-compliers failed to successfully complete the mandatory CAN counseling program, and the remaining quarter never contacted CAN after being assigned to the experimental group.

While compliance rates are higher among the Section 8-only group, the experimental group program-movers are more dispersed throughout Baltimore City and the larger metropolitan area, as seen by their initial relocation addresses (Figure 1). More detailed information about the post-program neighborhoods of MTO families is presented in Table 3. By design, (nearly) all of the experimental compliers moved to low-poverty census tracts with 1990 poverty rates below 10 percent,<sup>18</sup> and around 40 percent of those experimental families that relocated through MTO moved outside of Baltimore City. In contrast to the experimental program-movers, only around one in ten of the Section 8-only compliers voluntarily moved to very low-poverty census tracts (defined as those with rates less than 10 percent). Table 3 also shows that the neighborhoods for the experimental group have proportionately more affluent residents (college-educated adults) than those for the Section 8-only group.

Finally, the MTO data can only help us identify the effects of neighborhoods and residential mobility on economic outcomes if mobility patterns among the experimental and Section 8-only groups are different from the controls. Table 3 shows that this is the case even through December, 1997, by which time all of the experimental families had completed their initial one-year leases and were free to relocate to higher- or lower-poverty neighborhoods. While some control group families moved to lower-poverty neighborhoods on their own, the 12/97 addresses show that only 5 percent had moved to very low-poverty tracts (<10 percent) by this time.<sup>19</sup> In contrast, most of the experimental and Section 8-only compliers remain in neighborhoods that are quite similar to where they originally moved through the MTO program.<sup>20</sup>

### C. Welfare Receipt

Our central finding is that assignment to the experimental group reduces welfare receipt relative to controls, but assignment to the Section 8-only group has little effect beyond the first year. While the results presented here focus on welfare receipt by MTO household heads, we obtain quite similar findings when we examine welfare receipt by households, defined as PA receipt by anyone within the home.

As seen in Figure 2, the rates of welfare receipt by householders in the experimental group are consistently lower than those in the Section 8-only or control groups during the post-program period, with several of these differences significant at the 5 percent level. Figure 2 also shows that the Section 8-only group has somewhat higher rates of welfare receipt than controls during the *pre*-program period. We believe that these pre-program differences are due to chance because we observe no systematic differences across treatment groups in *household-level* PA receipt, or in the baseline survey variables for the MTO demonstrations in either Baltimore (Tables 1 and 2) or Boston (Katz, Kling and Liebman, 1999).

We can adjust for these random pre-program differences by estimating more formal ITT effects. We use a linear probability model to estimate equation (4) with a set of separate indicator variables for welfare receipt during each of the eight quarters prior to random assignment included as controls. The regression model also includes baseline-survey variables such as householder educational attainment (with categories for high school or more, and GED completion), marital status, sex, age, and number of children (Probit models produce very similar results).

Our regression-adjusted ITT estimates (Table 4) reveal persistent differences in welfare receipt between the experimental and control group in the quarters following random assignment. On average, the proportion of experimental families on welfare during the post-program period is just over 6 percentage points lower than what is observed for the control group ( $p < .05$ ), a difference equal to around 15 percent of the average rate of welfare use by the control group.<sup>21</sup> This difference appears to grow over time, from around 6 points during the first two years following random assignment, to nearly 10 points during the third year. While Section 8-only householders are nearly 5 percentage points less likely to be on PA during the first post-program year than those in the control group (significant at the 5 percent level), these differences dissipate in subsequent years.

The ITT estimates shown in Table 4 average the impact of MTO on program-movers and non-movers. Identifying the effects of MTO on those who move through the program is also of some interest. We calculate the effects of treatment-on-the-treated (TOT) by dividing the ITT estimates from Table 4 by the regression-adjusted probability that families in the experimental

and Section 8-only group comply with their treatments. As seen in Table 5, during the first year following random assignment the experimental program-movers have a welfare receipt nearly 25 percent lower than that of the potential compliers who were assigned to the control group, a difference that increases to over one-third of the control-complier mean during the third post-program year.

While Tables 4 and 5 reveal differences across treatment groups in rates of quarterly PA use, these estimates provide limited information about the *dynamics* of welfare receipt among MTO families. Our ITT estimates suggest that during any given quarter the proportion of experimental families on welfare will be around 6 percentage points lower than what is observed among families assigned to the control group. This (essentially cross-sectional) estimate is consistent with several types of changes in welfare-use patterns. One possibility is that 6 percent of the experimental group families would have been on welfare the entire time in the absence of MTO, but now exit and stay off welfare during the full post-program period. Alternatively, 12 percent of experimental families may have been on welfare the entire time but now alternate between quarters of welfare use and non-use (known as “welfare cycling”), or 12 percent of the experimental families may have cycled on and off welfare every other quarter but now exit welfare for good. These underlying dynamics are not irrelevant for public policy—for example, increases in welfare cycling may raise concerns about the necessity of additional supports to help families take advantage of improved opportunities in lower-poverty areas.

The results in Table 6 show that the differences in welfare receipt between the experimental and control groups are driven by a reduction in the share of families that would have been long-term welfare recipients (on welfare for three-quarters or more of all post-program quarters) and an increase in the proportion of families that are off welfare for most of the post-program period (that is, off welfare for three-fourths of the post-program quarters). The pattern is somewhat different for the Section 8-only group, where the reduction comes in the share of families that would have been on welfare for more than half of all post-program quarters.

While our analysis characterizes welfare dynamics in terms of the fraction of time spent on welfare, as in Duncan (1984), an alternative approach focuses on changes in welfare transition probabilities, as in Bane and Ellwood (1986). The results (not shown) reveal that relative to the controls, the experimental group experiences a substantial increase in the probability of exiting welfare during the first year, and a substantial reduction in the probability of entering welfare thereafter. In contrast, families in the Section 8-only group are more likely than controls to exit during the first post-program year, but less likely to exit during the second year—suggesting that the net effect for this group may be to simply change the timing of cycles off and onto welfare.

#### **D. Earnings and Employment**

What causes experimental and (to a lesser extent) Section 8-only families to exit and stay off welfare at a greater rate than controls? These differences in welfare use could be caused by increased employment and earnings in the formal labor market or in under-the-table earnings (which may not translate into documented work histories that can improve workers’ future prospects in the formal labor market), or in response to heightened welfare stigma in their local neighborhoods with no attendant increase in cash earnings. As it turns out, most of the difference in welfare receipt appears to be due to increased earnings in the formal labor market.

Figure 3 shows that the fraction of experimental-group families that officially exit welfare because of employment (according to the Maryland DHR) exceeds the rates observed

among the Section 8-only and control groups. This result is presented more formally in Table 7, which reveals an experimental-group intent-to-treat effect of around 6 percentage points on the fraction of families that record a welfare-to-work exit (significant at the 5 percent level). Put differently, the difference in welfare-to-work transitions between experimentals and controls appears to account for most of the differences in welfare receipt.

In contrast, the UI data provide no clear evidence of systematic differences in UI-covered employment or earnings across treatment groups. This can be seen in Figure 4, which shows the fraction of household heads in each treatment group who hold a UI-covered job by post-program quarter, and Figure 5, which presents the average earnings for household heads (with those not working assigned earnings of zero). Tables 8 and 9 show that regression-adjusted ITT estimates confirm the story suggested by the figures.<sup>22</sup> Our findings are qualitatively similar when we focus on the natural logarithm of quarterly earnings, use the entire household rather than the household head as the unit of analysis, restrict our analytic sample to particular population subgroups,<sup>23</sup> or focus on differences across groups in employment or job-to-job transitions. If families exit welfare only when their income exceeds some threshold level, a mean-neutral increase in the variance of earnings could lead to reductions in welfare use, but need not be reflected in differences in average earnings or employment rates. Yet we find no evidence for a mean-preserving spread in quarterly UI earnings across treatment groups.<sup>24</sup>

What explains the apparent paradox between the DHR welfare-to-work data and the quarterly UI records? Part of the answer comes from comparing the DHR and UI data during the quarters in which MTO families recorded welfare-to-work exits according to the state welfare system. As seen in Table 10, only about 65 percent of the people who make welfare-to-work exits according to the DHR data hold jobs at the time of their exit, as recorded by the UI data.<sup>25</sup> Moreover, for the 65 percent who hold jobs in the UI data at the time of the DHR welfare-to-work exit, we should observe wage growth from the previous quarter in the UI data (otherwise they couldn't have made a welfare-to-work transition). However, only about 87 percent of this group experience earnings growth in their quarter of exit. The UI data not only fails to record many of the jobs that lead to DHR welfare-to-work exits, but it also seems to inaccurately measure earnings for those jobs that it does pick up.

The DHR results for welfare use and welfare-to-work exits are thus not inconsistent with the UI findings for employment and earnings, given the standard errors around our UI estimates. For example, the difference between the experimental and control groups in the fraction of families that exit welfare because they move from unemployment into any job is no more than 5.6 percentage points (Table 7), and probably somewhat less because some families exit welfare because of an earnings increase in their jobs (rather than a change in employment status). Thus the difference between experimentals and controls in the fraction of families that exit welfare because of transitions into *UI-covered jobs* should be no more than 3.6 percentage points (equal to 65 percent times the all-job figure). By comparison Table 8 shows a difference in average quarterly employment rates between the experimental and control families of -0.9 percent during the post-program period. Given the 95 percent confidence interval around this point estimate (which ranges from -6.3 to 4.5 percent), we cannot rule out a difference in UI-covered employment rates between the experimental and control groups that would be large enough to generate a 3.6 percentage point difference in welfare exits caused by entry into UI-covered jobs.<sup>26</sup>

## E. Cohabitation

Another factor that could contribute to the observed differences in welfare receipt across treatment groups comes from changes in household structure. Rates of family formation may be affected by the experimental treatment if the supply of “marriageable men” in high-poverty neighborhoods is depressed because of low employment and high incarceration rates (Wilson, 1987), or if adult children are for some reason more likely to stay at home when families live in more affluent areas. On the other hand, if the “market” for adult companions occurs at a geographic level larger than the neighborhood—for example, at the level of the metropolitan area—then moving families from one part of the metropolitan area to another may have little effect on rates of marriage and co-habitation for household heads.

As seen in Table 11, from the pre- to the post-program period we observe increases in the fraction of household heads who are married or co-habit with another adult of the opposite sex in all three MTO treatment groups, with little difference across groups. While the fraction of experimental families with a second adult in the home (including adult children) is somewhat higher than for the Section 8-only and control groups (33 versus 25 and 28 percent, respectively), these differences are not statistically significant. Since there may be financial incentives for families to conceal the presence of other adults within the home, these survey data may understate rates of cohabitation or marriage for each group. At the same time, if the social norms in support of two-adult families is stronger in low- than high-poverty areas, then any off-setting upward errors from “social desirability bias”—the well-known tendency of survey respondents to present themselves favorably to interviewers (Sudman and Bradburn, 1974)—may be more pronounced among experimental and Section 8-only families than controls. In sum, while there may be important changes in household composition among MTO families, we cannot reliably detect them with these data.

## VI. DISCUSSION

This paper uses data from a randomized housing-voucher experiment to examine the effects of neighborhoods on economic outcomes. We find evidence to suggest that providing families with the opportunity to relocate to neighborhoods with very low poverty rates (under 10 percent) reduces rates of welfare receipt by around 6 percentage points (equal to around 15 percent of the control group’s welfare receipt rate), and that this effect may increase with time. On the other hand, providing housing subsidies to MTO families with no constraints on where they relocate appears to have little effect on welfare use beyond the first six quarters following random-assignment.

Our analysis of state welfare records also finds a nearly 6 percentage point difference between the experimental and control groups in the proportion of families that record an official welfare-to-work exit. These differences in welfare-to-work transitions are, however, not reflected in quarterly employment and earnings data from the state UI system because many of the jobs and earnings changes responsible for these welfare exits are not captured by the UI data. Our findings contrast somewhat with those presented in Kornfeld and Bloom (1999), who find that the estimates of program impacts on earnings are qualitatively similar using UI data versus presumably more complete data sources (in their case, survey interviews).

One alternative explanation for the differences in welfare use observed between the experimental and control groups is that the former may move to areas where welfare offices are less accessible than in the baseline neighborhoods, and thus exit from welfare because they are unable to verify their welfare eligibility every six to twelve months. Yet, each of the suburban

counties has multiple welfare offices, and experimental families have significant financial incentives to find and access these sites (in fact, many experimental families relocate near these offices).<sup>27</sup> Moreover, reduced access to welfare offices cannot explain increases in recorded welfare-to-work exits, since Maryland welfare offices have a special code to record welfare exits that occur because the recipient fails to renew eligibility.

Similarly, it might be the case that welfare regulations are more strict (or are enforced more vigorously) in the suburbs than in Baltimore City, and that suburban caseworkers are more likely to record welfare exits as having been due to work transitions rather than for other reasons. However, welfare eligibility rules are set at the state- rather than county-level in Maryland. Interviews with staff of the Maryland DHR suggest that local caseworkers have relatively little discretion in implementing state regulations and that, in any case, suburban caseworkers are unlikely to be more strict than those in the city.<sup>28</sup>

Another alternative explanation is that our results are driven by higher rates of EITC utilization among the experimental group caused by the pre-move counseling that was provided to families, although by some estimates EITC utilization rates are already quite high—estimated to be on the order of 80 to 86 percent in 1990, and perhaps even higher in recent years given increased outreach efforts and the growth in benefits from participating (Scholz, 1994, Meyer and Rosenbaum, 1999). In any event, while the extra income from EITC payments could explain the observed differences in welfare use between the experimental and control groups, differences in EITC payments would need to be accompanied by changes in labor supply among experimental families to explain differences in official welfare-to-work exits because the Maryland DHR determines eligibility for such exits on the basis of actual labor-market earnings (excluding EITC benefits).

Moreover, follow-up survey data for a sub-set of experimental and Section 8-only program-movers produce some suggestive evidence for changes in economic opportunities among MTO families, which seems to argue against explanations that emphasize welfare enforcement or EITC utilization.<sup>29</sup> Specifically, these survey results suggest that the majority of experimental program-movers report that both job and training opportunities are better in the new versus old neighborhoods (66 and 63 percent, respectively).<sup>30</sup> The proportion of Section 8-only relocators who report improvements in job and training opportunities (53 and 42 percent, respectively) is lower than what is observed for the experimental group, consistent with our findings that the experimental treatment has a larger effect on welfare use and welfare-to-work transitions than the Section 8-only treatment. These results are not definitive, since we do not know what happened to perceived opportunities among the control group during this period, but they are at least suggestive. (We do not draw more heavily on these follow-up surveys in this paper because of the somewhat modest response rates, particularly among experimental non-compliers and control group families).

The results presented here suggest that neighborhoods matter for economic self-sufficiency, at least for the MTO program population in Baltimore. Less clear are the specific mechanisms through which neighborhoods improve economic opportunities and outcomes for families in the MTO experiment. Changes in welfare receipt and work among experimental families could be caused by changes in social capital (such as exposure to employed neighbors who may provide job references or social support for work), reduced distance to transportation and job opportunities, or improvements in local institutions such as social services.<sup>31</sup> Suggestive evidence that part of the change in welfare receipt and work may be due to changes in social interactions or local social services (rather than proximity to jobs or transportation) comes from

evidence that the MTO experimental and Section 8-only treatments have also reduced juvenile delinquency and other teen problem behaviors in Baltimore and Boston, and have improved the mental health of adults (Katz, Kling and Liebman, 1999, Ludwig, Duncan and Hirschfield, 1999).

Evidence that neighborhood conditions affect the economic outcomes of at least some residents of high-poverty urban areas has implications for the redesign of housing policies in the United States, though evidence of the existence of such effects is not decisive in making such policy choices. Also relevant are the effects of housing-mobility programs on the other residents of host neighborhoods, and the effects of such policies on non-market as well as market outcomes. In any case, our findings suggest that the spatial concentration of poor families in many of our urban areas may be a potentially important, yet poorly understood, explanation for persistent poverty in America.

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**Table 1**  
**Baseline Characteristics of MTO Householders from Baseline Survey Data**

	Total	Experimental	Section 8-Only	Control
Families (N)	638	252	188	198
<u>Householder characteristics:</u>				
African-American (%)	97.4	96.8	97.2	98.4
Female householder (%)	94.7	96.0	92.0	95.5
Householder age	35.1	35.8	34.3	34.8
Number of children	2.62	2.57	2.75	2.55
Has h.s. degree	41.7	44.1	45.8	34.8
Has G.E.D.	14.9	15.0	13.0	16.6
Married	3.5	3.3	4.0	3.3
Has driver's license	20.2	17.5	27.4	16.9
Has car that runs	4.1	4.8	4.3	3.0
<u>Householder Earnings/Work:</u>				
Household income (\$'s)	6,876	6,894	6,679	6,750
AFDC at baseline	80.3	79.3	81.6	80.4
AFDC ever	97.6	97.2	97.2	98.4
School or training at baseline	15.8	15.1	16.5	16.2
Has never worked	13.2	14.8	9.9	14.2
Worked all 4 quarters prior to enrolling in MTO	11.4	10.7	9.4	14.1
Work full/part-time baseline*	23.0	22.3	19.3	27.2
Tenure current job (weeks)	106.2	95.6	95.5	125.2
Hours worked per week	31.2	31.0	29.2	32.8
Wages per hour (\$'s)	5.98	5.59	6.68	5.95
<u>Commuting (employed householders):</u>				
Commute under 15 minutes	21.9	21.0	22.2	22.6
60 minutes or more	6.0	6.5	2.8	7.5
Commute by public transp	54.7	51.7	62.9	52.8
Own car	4.1	5.0	0	5.7
Walk	33.8	33.3	31.4	35.8
Carpool	2.0	1.7	2.9	1.9
<u>How householder heard about current job:</u>				
Friend, neighbor, family	60.7	57.6	64.7	61.5
Want ad	0.7	1.7	0	0
Employment agency	34.5	33.9	32.4	36.5
Welfare office	2.1	5.1	2.9	0
Other	2.1	1.7	2.9	1.9

NOTES:\* Includes respondents who work part-time and also attend school or training programs (between 1.2 and 2.5 percent of all respondents, or about one-tenth of the group that is working at the time of the baseline survey).

**Table 2**

### Motivations for Enrolling in MTO Program

	<u>Total</u>	<u>Experimental</u>	<u>Section 8-Only</u>	<u>Control</u>
<u>Criminal Victimization</u>				
<u>During last 6 months, someone in HH:</u>				
Had valuable snatched	23.3	22.6	25.6	22.0
Beaten/assaulted	27.7	31.7	24.6	25.7
Stabbed/shot	11.9	12.8	10.1	12.6
Break-in to home	25.9	27.3	27.9	22.0
Any of above	51.7	55.3	51.7	47.1
 <u>Primary reason for wanting to move:</u>				
Better schools	11.7	9.8	14.4	11.5
To be near job	0.5	0.0	1.1	0.5
Better transportation	0.0	0.0	0.0	0.0
To get a job	1.0	1.2	0.6	1.0
Avoid gangs, drugs	53.5	53.3	52.2	55.0
Better apartment	25.1	26.4	23.9	24.6
Other	4.7	4.5	3.9	5.8
 <u>Second most important reason for move:</u>				
Better schools	30.3	30.1	33.3	27.7
To be near job	0.6	0.4	1.1	0.5
Better transportation	0.3	0.4	0.0	0.5
To get a job	4.7	6.1	3.3	4.2
Avoid gangs, drugs	27.1	27.2	25.0	28.8
Better apartment	28.0	25.2	30.0	29.8
Other	4.7	6.1	3.3	4.2

NOTES: \* Includes respondents who work part-time and also attend school or training programs (between 1.2 and 2.5 percent of all respondents, or about one-tenth of the group that is working at the time of the baseline survey). \*\* Defined as purse-snatching, threatened with gun or knife, beaten/assaulted, stabbed/shot, and break in to home.

**Table 3 Relocation Outcomes for MTO s**

	<u>Baseline (all families)</u>	<u>Experimental</u>		<u>Section 8-only</u>		<u>Control</u>	
	1994-1996	Initial Post-Program	As of 12/97	Initial Post-Program	As of 12/97	Initial Post-Program	As of 12/97
<b>Distribution of MTO Households</b>							
<u>Jurisdiction :</u>							
Baltimore City	100.0	77.1	79.4	89.9	86.7	99.5	98.0
Anne Arundel County	0.0	0.8	2.0	0.0	0.5	0.0	0.0
Baltimore County	0.0	13.0	10.7	5.3	8.0	0.0	1.0
Harford County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Howard County	0.0	7.1	5.9	2.7	2.7	0.0	0.5
Montgomery County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Other	0.0	1.2	1.2	2.1	2.1	0.5	0.5
<u>% Census Tract Poor:</u>							
0 - 9.9	0.0	49.4	43.0	8.7	12.5	0.0	4.5
10 -19.9	0.0	4.8	8.4	14.7	21.2	0.0	7.6
20- 29.9	0.2	0.0	7.6	10.3	15.8	0.0	3.0
30- 39.9	0.3	0.4	4.0	12.5	13.0	0.0	6.6
40- 49.9	2.0	1.6	6.4	9.8	7.1	2.0	6.6
50-59.9	4.4	1.2	4.0	6.5	4.9	5.6	4.5
60-69.9	52.5	22.7	18.7	26.6	19.6	49.0	43.4
70-79.9	20.4	9.6	4.0	7.1	3.8	23.2	14.6
80 plus	20.1	10.4	4.0	3.8	2.2	20.2	9.1
<b>Mean Census-Tract Characteristics</b>							
% White	8.2	33.4	34.7	20.9	28.1	8.5	14.1
% Adults w/out HS Degree	54.2	38.4	37.8	47.7	44.6	54.3	51.0
% Adults w/ College Degree	6.5	15.3	15.5	9.2	10.6	6.7	7.6
% HHs headed by female	80.6	52.0	46.2	59.5	51.3	80.9	68.6
% HH w/ inc >=\$50,000	4.7	16.5	16.8	8.7	10.7	4.5	7.6

NOTES: Neighborhood characteristics are calculated using 1990 Census data.

**Table 4**  
**Regression-Adjusted Intent-to-Treat (ITT) Effects of MTO Program on Welfare Receipt**

	Percent household heads receiving PA		
	Control Mean	Exp vs Control	S8-Only vs Control
<u>Quarters Since</u>			
<u>Random</u>			
<u>Assignment:</u>			
1	0.44 (0.04)	-0.027 (0.015)*	-0.029 (0.022)
2	0.46 (0.04)	-0.058 (0.026)**	-0.051 (0.028)*
3	0.46 (0.04)	-0.093 (0.034)**	-0.057 (0.033)*
4	0.45 (0.04)	-0.089 (0.039)**	-0.057 (0.041)
5	0.46 (0.04)	-0.114 (0.041)**	-0.092 (0.043)**
6	0.42 (0.04)	-0.066 (0.045)	-0.020 (0.050)
7	0.42 (0.04)	-0.062 (0.049)	0.002 (0.055)
8	0.39 (0.04)	-0.017 (0.052)	0.058 (0.059)
9	0.44 (0.04)	-0.098 (0.053)*	0.006 (0.067)
10	0.44 (0.04)	-0.108 (0.054)**	0.021 (0.070)
11	0.44 (0.04)	-0.082 (0.057)	0.018 (0.072)
12	0.46 (0.05)	-0.080 (0.063)	-0.018 (0.081)
13	0.47 (0.05)	-0.098 (0.069)	-0.051 (0.089)
Entire Post-Program	0.44 (0.01)	-0.064 (0.026)**	-0.026 (0.031)
Post-Program Qtrs 1-4	0.45 (0.02)	-0.067 (0.022)**	-0.048 (0.024)**
Post-Program Qtrs 5-8	0.42 (0.02)	-0.065 (0.039)*	-0.018 (0.044)
Post-Program Qtrs 9-13	0.45 (0.02)	-0.092 (0.049)*	-0.001 (0.064)

NOTES: Robust standard errors in parentheses. \* = Difference significant at 10 percent. \*\* = Difference significant at 5 percent. Regression adjustment controls for householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, as well as indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data. Estimates are calculated using a linear probability model; the coefficient estimates present the change in the probability of being on welfare when assigned to the experimental or Section 8-only rather than control group.

**Table 5**  
**Regression-Adjusted Effects of Treatment-on-the-Treated (TOT)**  
**of MTO Program on Welfare Receipt**

	<u>Percent household heads receiving PA</u>			
	<u>Exp vs Control</u>		<u>S8-Only vs Control</u>	
	Control Complier Mean	TOT Effect	Control Complier Mean	TOT Effect
<u>Quarters Since Random Assignment:</u>				
1	0.523	-0.047 (0.026)*	0.621	-0.035 (0.026)
2	0.579	-0.100 (0.045)**	0.649	-0.061 (0.033)*
3	0.573	-0.161 (0.059)**	0.627	-0.068 (0.039)*
4	0.550	-0.154 (0.068)**	0.606	-0.068 (0.049)
5	0.535	-0.197 (0.071)**	0.615	-0.110 (0.051)**
6	0.425	-0.114 (0.078)	0.548	-0.024 (0.060)
7	0.428	-0.107 (0.085)	0.527	0.002 (0.066)
8	0.371	-0.029 (0.090)	0.497	0.069 (0.071)
9	0.467	-0.170 (0.092)*	0.537	0.007 (0.080)
10	0.452	-0.187 (0.093)**	0.504	0.025 (0.084)
11	0.432	-0.142 (0.099)	0.493	0.022 (0.086)
12	0.438	-0.139 (0.109)	0.496	-0.022 (0.097)
13	0.412	-0.170 (0.119)	0.469	-0.061 (0.106)
Entire Post-Program	0.463	-0.111 (0.045)**	0.566	-0.031 (0.037)
Post-Program Qtrs 1-4	0.557	-0.116 (0.038)**	0.625	-0.057 (0.029)**
Post-Program Qtrs 5-8	0.440	-0.113 (0.068)*	0.551	-0.022 (0.053)
Post-Program Qtrs 9-13	0.440	-0.159 (0.085)*	0.501	-0.001 (0.077)

NOTES: Robust standard errors in parentheses. \* = Difference significant at 10 percent. \*\* = Difference significant at 5 percent. Regression adjustment controls for householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, as well as indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data. Estimates are calculated using a linear probability model; the coefficient estimates present the change in the probability of being on welfare when assigned to the experimental or Section 8-only rather than control group.

**Table 6**  
**Effects of MTO Program on Duration of Public Assistance Receipt**

Dependent variable = % MTO families spending specified fraction of quarters on welfare

	<u>Control Mean</u>	<u>Exp vs Control</u>		<u>S8-Only vs Control</u>
<u>ITT Effect</u>				
>3/4	0.413	-0.096 (0.039)**		-0.016 (0.029)
>2/4 and æ3/4	0.135	0.034 (0.035)		-0.072 (0.038)*
>1/4 and æ2/4	0.118	-0.005 (0.018)		0.020 (0.050)
>0/4 and æ1/4	0.084	0.082 (0.019)**		0.056 (0.027)**
0/4	0.249	-0.014 (0.022)		0.011 (0.030)
		<u>Exp vs Control</u>		<u>S8-Only vs Control</u>
	<u>Control</u>		<u>Control</u>	
	Complier Mean	TOT Effect	Complier Mean	TOT Effect
<u>TOT Effect</u>				
>3/4	0.335	-0.169 (0.069)**	0.170	-0.019 (0.051)
>2/4 and æ3/4	0.122	0.060 (0.062)	0.150	-0.087
		(0.046)*		
>1/4 and æ2/4	0.270	-0.009 (0.032)	0.463	0.024
		(0.061)		
>0/4 and æ1/4	0.010	0.144 (0.033)**	0.049	0.068
		(0.033)**		
0/4	0.263	-0.025 (0.039)	0.169	0.013
			(0.036)	

NOTES: \* = Difference significant at 10 percent. \*\* = Difference significant at 5 percent. Regression adjustment controls for householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, as well as indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data. Regression-adjusted estimates calculated from a linear probability model; point estimates represent the change in the probability from a change in MTO treatment-group assignment.

**Table 7**  
**Effects of MTO Program on Welfare Exits Due to Employment from Maryland Welfare Data**

Dependent variable = Percent families record welfare-to-work exit during post-program period

	Control Mean	Exp vs Control	Section 8-only vs Control	
<u>Intent-to-Treat Effect</u>				
Entire Post-Program Period	0.047	0.056 (0.028)**	0.005 (0.025)	
	<u>Exp vs Control</u>		<u>S8-Only vs Control</u>	
	Control		Control	
	Complier Mean	TOT Effect	Complier Mean	TOT Effect
<u>Effects of Treatment-on-the-Treated</u>				
Entire Post-Program Period	0.038	0.098 (0.049)**	0.070	0.006 (0.030)

NOTES: Robust standard errors in parentheses. \*\* = Statistically significant at the 5 percent level. \* = Statistically significant at the 10 percent level. Estimates are calculated using a linear probability model, which controls for baseline survey characteristics such as the householder's age, sex, educational attainment, marital status, and number of children, as well as wage in the eight quarters before assignment.

**Table 8**  
**Regression-Adjusted Intent-to-Treat (ITT) Effects of MTO Program**  
**on Quarterly Employment Rates (UI Data)**

	Percent household heads employed		
	Control Mean	<u>Exp vs Control</u>	<u>S8-Only vs Control</u>
<u>Quarters Since</u>			
<u>Random Assignment:</u>			
1	0.35	-0.015 (0.037)	-0.049 (0.041)
2	0.38	0.002 (0.041)	-0.005 (0.046)
3	0.40	0.019 (0.040)	-0.018 (0.045)
4	0.41	0.019 (0.042)	-0.013 (0.048)
5	0.45	-0.042 (0.043)	-0.056 (0.047)
6	0.49	0.021 (0.043)	-0.039 (0.047)
7	0.46	-0.007 (0.042)	-0.042 (0.048)
8	0.49	-0.033 (0.046)	-0.041 (0.051)
9	0.50	-0.092 (0.044)**	-0.084 (0.047)*
10	0.50	-0.046 (0.050)	-0.033 (0.056)
11	0.47	-0.009 (0.051)	0.055 (0.059)
12	0.47	-0.024 (0.049)	0.031 (0.057)
13	0.49	0.003 (0.051)	-0.005 (0.067)
14	0.48	0.006 (0.041)	-0.021 (0.051)
15	0.52	0.022 (0.055)	-0.065 (0.070)
16	0.51	0.049 (0.059)	0.055 (0.074)
17	0.48	0.081 (0.070)	0.022 (0.088)
Entire Post-Program	0.45	-0.009 (0.027)	-0.023 (0.032)
Post-Program Qtrs 1-4	0.39	-0.001 (0.034)	-0.037 (0.040)
Post-Program Qtrs 5-8	0.48	-0.014 (0.035)	-0.043 (0.041)
Post-Program Qtrs 9-12	0.49	-0.047 (0.043)	-0.013 (0.046)
Post-Program Qtrs 13-17	0.49	0.022 (0.043)	-0.008 (0.054)

NOTES: Robust standard errors in parentheses. \* = Difference significant at 10 percent. \*\* = Difference significant at 5 percent. Regression adjustment controls for householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, as well as indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data. Estimates are calculated using a linear probability model; the coefficient estimates present the change in the probability of being on welfare when assigned to the experimental or Section 8-only rather than control group.

**Table 9**  
**Regression-Adjusted Intent-to-Treat (ITT) Effects of MTO Program**  
**on Quarterly Earnings (UI Data)**

	Effect on earnings of household heads.		
	Control Mean	Exp vs Control	S8-Only vs Control
<u>Quarters Since</u>			
<u>Random Assignment:</u>			
1	0.79	-108.52 (76.42)	-147.74 (80.52)*
2	0.78	33.44 (98.50)	-12.76 (100.17)
3	0.92	13.84 (108.79)	45.59 (123.42)
4	0.88	42.22 (113.45)	45.15 (126.86)
5	1.01	-48.88 (122.02)	-59.58 (126.00)
6	1.06	121.19 (132.90)	-127.32 (125.16)
7	1.04	92.90 (147.91)	-93.30 (152.43)
8	1.22	131.38 (157.70)	-115.90 (172.99)
9	1.28	-260.64 (150.61)*	-50.12 (189.20)
10	1.32	-61.89 (177.22)	-213.89 (173.94)
11	1.34	-46.22 (181.31)	-148.81 (195.04)
12	1.2	-31.69 (164.19)	117.22 (190.97)
13	1.14	0.41 (155.14)	126.63 (211.80)
14	0.98	164.70 (133.84)	318.20 (195.20)
15	1.23	134.43 (185.25)	190.44 (254.80)
16	1.55	-101.82 (266.12)	154.46 (368.90)
17	1.19	66.34 (238.50)	383.47 (344.62)
Entire Post-Program	1.02	-10.42 (96.14)	4.54 (112.91)
Post-Program Qtrrs 1-4	0.84	-3.98 (81.02)	-16.10 (88.03)
Post-Program Qtrrs 5-8	1.07	11.07 (124.57)	-99.57 (123.41)
Post-Program Qtrrs 9-12	1.21	-113.68 (150.29)	-67.10 (162.29)
Post-Program Qtrrs 13-17	1.05	57.32 (157.53)	233.17 (231.68)

NOTES: Robust standard errors in parentheses. \* = Difference significant at 10 percent. \*\* = Difference significant at 5 percent. Regression adjustment controls for householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, as well as indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data. Estimates are calculated using a linear probability model; the coefficient estimates present the change in the probability of being on welfare when assigned to the experimental or Section 8-only rather than control group. Figures reported in 1997 constant dollars.

**Table 10**  
**Comparison of Welfare-to-Work Exits in Welfare Data with Unemployment Insurance Data**  
**During Quarter of Exit**

	Post-Assignment Quarters	
	Exit=1	Exit=0
Probability of Working in a UI Covered Job	0.652 (0.070)**	0.426 (0.005)
<u>If Employed in a UI Covered Job:</u>		
Wage Growth from Previous Quarter	717.71 (147.54)**	223.23 (13.71)
Probability of Wage Growth from Quarter Prior to DHR Welfare Exit	0.867 (0.062)*	0.623 (0.007)

Notes: Standard errors in parentheses. \* = Difference from Exit=0 significant at 10 percent. \*\* = Difference from Exit=0 significant at 5 percent.

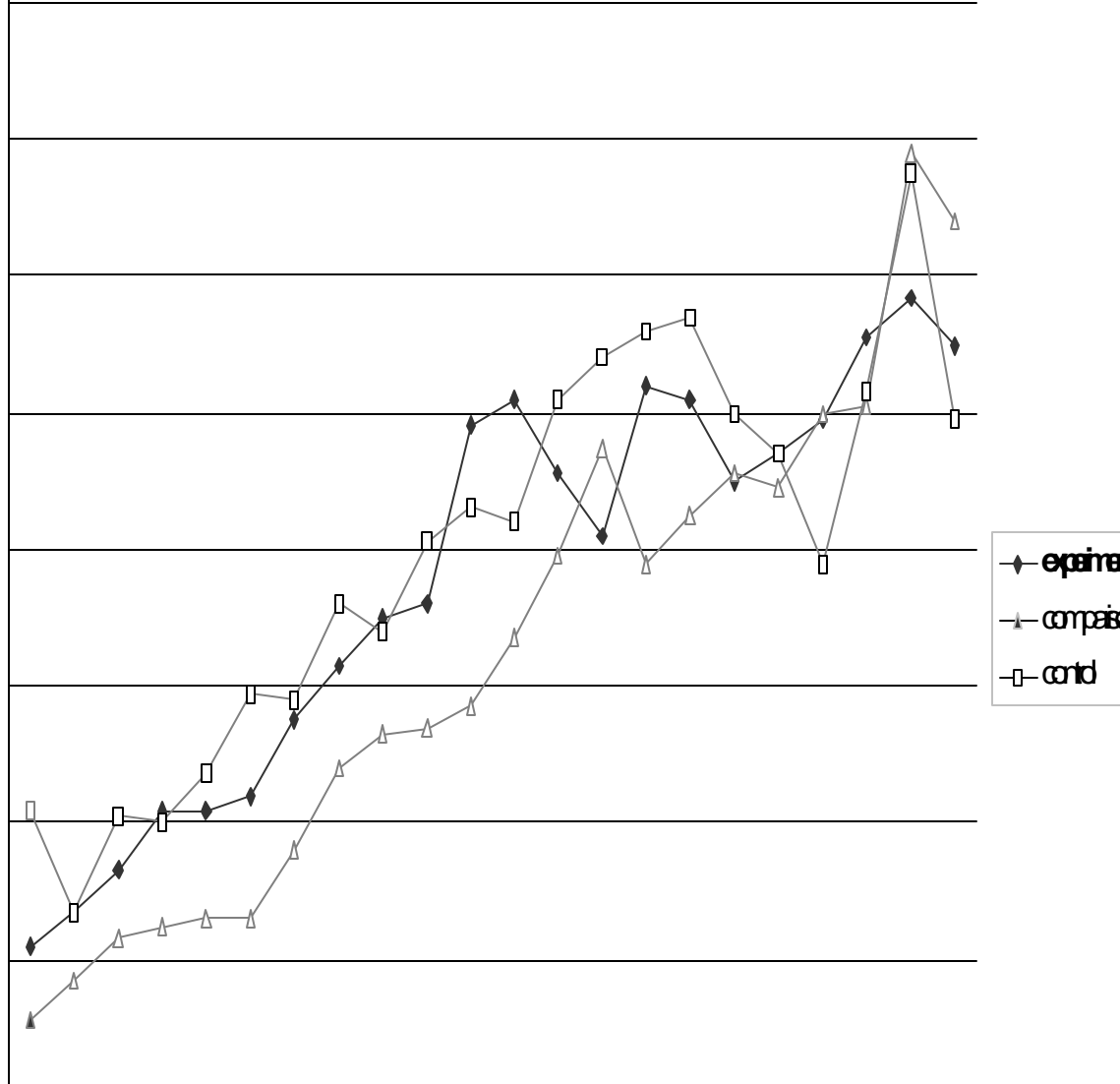
**Table 11**  
**Cohabitation and Household Composition for MTO Households, Pre- and Post-Program**

	Exp (N=252) (%)	S8-Only (N=188) (%)	Control (N=198) (%)
<u>Cohabit w/ Non-related Adult of Opposite Sex</u>			
Pre-Program	2.0 (0.9)	3.2 (1.3)	1.5 (0.9)
Post-Program	9.9 (1.9)	6.9 (1.9)	8.1 (1.9)
<u>Cohabit or Other Adult in Home (not including adult children)</u>			
Pre-Program	2.8 (1.0)	4.3 (1.5)	2.0 (1.0)
Post-Program	9.9 (1.9)	8.0 (2.0)	8.1 (1.9)
<u>Cohabit or Any Adult in Home (including adult children)</u>			
Pre-Program	11.1 (2.0)	12.8 (2.4)	11.1 (2.2)
Post-Program	32.9 (3.0)	24.5 (3.1)	28.3 (3.2)

NOTES: Robust standard errors in parentheses. \*\* = Difference in comparison to control group is statistically significant at 5 percent level. \* = Difference in comparison to control group is statistically significant at the 10 percent level. Intent-to-treat estimates calculated from a linear probability model.

**Figure 5 Quarterly Earnings for MTO Household Heads (Thousands)**

-4  
-3  
-2  
-1  
0  
1  
2  
3  
4  
5  
6  
7  
8  
9  
10  
11  
12  
13



**Appendix Table 1  
Quarterly Public-Assistance Receipt by  
MTO Householders**

	Percent household heads receiving PA	
		Experimental
<u>Quarters Since Randomization:</u>		
0	0.47 (0.04)	0.39 (0.03)
1	0.48 (0.04)	0.38 (0.03)
2	0.52 (0.04)*	0.39 (0.03)
3	0.55 (0.04)*	0.41 (0.04)
4	0.54 (0.04)	0.43 (0.04)
5	0.54 (0.04)	0.44 (0.04)
6	0.53 (0.04)	0.46 (0.04)
7	0.51 (0.04)	0.46 (0.04)
8	0.48 (0.04)	0.45 (0.04)
9	0.45 (0.04)	0.46 (0.04)
10	0.46 (0.05)	0.42 (0.04)
11	0.48 (0.06)	0.42 (0.04)
12	0.50 (0.06)	0.39 (0.04)
13	0.51 (0.09)	0.44 (0.04)
14	0.51 (0.09)*	0.44 (0.04)
15	0.49 (0.09)	0.44 (0.04)
16	0.48 (0.09)	0.46 (0.05)
17	0.43 (0.10)*	0.47 (0.05)

NOTES: \* = Difference with control group significant at 10 percent. \*\* = Difference with control group significant at 5 percent.

**Appendix Table 2**  
**Quarterly Employment and Earnings for MTO Household Heads**

	Percent householders employed			Quarterly earnings for householders (in thousands)		
	Exp	S8-Only	Control	Exp	S8-Only	Control
<u>Quarters Since</u>						
<u>Random Assignment:</u>						
-4	0.19 (0.02)*	0.18 (0.03)*	0.29 (0.03)	0.42 (0.07)	0.31 (0.07)*	0.62 (0.10)
-3	0.24 (0.03)	0.18 (0.03)	0.26 (0.03)	0.47 (0.07)	0.37 (0.08)	0.47 (0.08)
-2	0.25 (0.03)	0.27 (0.03)	0.29 (0.03)	0.53 (0.07)	0.43 (0.08)	0.61 (0.10)
-1	0.28 (0.03)	0.23 (0.03)	0.29 (0.03)	0.62 (0.08)	0.45 (0.09)	0.60 (0.10)
0	0.26 (0.03)	0.23 (0.03)	0.30 (0.03)	0.62 (0.08)	0.46 (0.08)	0.67 (0.11)
1	0.30 (0.03)	0.24 (0.03)*	0.35 (0.03)	0.64 (0.08)	0.46 (0.09)*	0.79 (0.11)
2	0.35 (0.03)	0.31 (0.03)	0.38 (0.03)	0.75 (0.08)	0.56 (0.10)	0.78 (0.10)
3	0.39 (0.03)	0.31 (0.03)	0.40 (0.03)	0.83 (0.09)	0.68 (0.10)	0.92 (0.13)
4	0.40 (0.03)	0.33 (0.03)	0.41 (0.04)	0.90 (0.09)	0.73 (0.11)	0.88 (0.11)
5	0.39 (0.03)	0.33 (0.03)*	0.45 (0.04)	0.92 (0.09)	0.74 (0.11)	1.01 (0.11)
6	0.49 (0.03)	0.37 (0.03)	0.49 (0.04)	1.18 (0.10)	0.77 (0.11)	1.06 (0.11)
7	0.46 (0.03)	0.37 (0.03)	0.46 (0.04)	1.22 (0.11)	0.87 (0.17)	1.04 (0.12)
8	0.43 (0.03)	0.39 (0.04)	0.49 (0.04)	1.11 (0.10)	0.99 (0.14)	1.22 (0.13)
9	0.41 (0.03)*	0.40 (0.03)*	0.50 (0.04)	1.02 (0.10)	1.15 (0.23)	1.28 (0.13)
10	0.45 (0.03)	0.43 (0.04)	0.50 (0.04)	1.24 (0.12)	0.98 (0.14)	1.32 (0.13)
11	0.43 (0.03)	0.47 (0.04)	0.47 (0.04)	1.22 (0.11)	1.05 (0.14)	1.34 (0.14)
12	0.41 (0.03)	0.44 (0.04)	0.47 (0.04)	1.10 (0.11)	1.11 (0.14)	1.20 (0.14)
13	0.49 (0.03)	0.45 (0.05)	0.49 (0.04)	1.14 (0.12)	1.09 (0.19)	1.14 (0.14)
14	0.45 (0.03)	0.41 (0.04)	0.48 (0.04)	1.19 (0.11)	1.20 (0.19)	0.98 (0.13)
15	0.51 (0.03)	0.40 (0.06)	0.52 (0.04)	1.31 (0.13)	1.21 (0.21)	1.23 (0.15)
16	0.51 (0.04)	0.49 (0.06)	0.51 (0.05)	1.37 (0.14)	1.58 (0.27)	1.55 (0.22)
17	0.53 (0.04)	0.46 (0.07)	0.48 (0.05)	1.30 (0.15)	1.48 (0.28)	1.19 (0.19)

NOTES: \* = Difference with control group significant at 10 percent. \*\* = Difference with control group significant at 5 percent. Earnings are reported in constant 1997 dollars.

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ed as census tracts with poverty rates of 40 percent or more.

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<sup>2</sup> For example, while 59 percent of all public housing residents live within high-poverty neighborhoods, the figure is 69 percent for African-American residents (Goering, Kamely and Richardson, 1994, Turner, 1998).

<sup>3</sup> With Section 8 rental certificates, families are allowed to rent private-market units with rents up to the HUD-defined Fair Market Rent (FMR) for the area, and receive a subsidy equal to the difference between the rental rate and 30 percent of the family's income. With Section 8 vouchers, families receive a subsidy of the difference between the FMR and 30 percent of the family's income, and are allowed to lease apartments with rental rates either below the FMR (and thus pay less than 30 percent of their incomes towards rent) or above the FMR (by paying more than 30 percent of income towards rent). For our purposes, we treat the two tenant-based programs as equivalent and term them "housing subsidies."

<sup>4</sup> See, for example, Ihlanfeldt and Sjoquist (1990, 1991), Ihlanfeldt (1993), and Raphael (1998), versus Ellwood (1986) and Leonard (1985). Also see the reviews in Jencks and Mayer (1990) and Holzer (1991).

<sup>5</sup> See Osterman (1991), Vartanian (1992) and the review of Ellen and Turner (1997).

<sup>6</sup> Some analysts believe that this identification problem can be overcome by examining the effects of neighborhood characteristics on teenage labor market outcomes, since adolescents typically have little say over a family's decision about where to live. Yet the unmeasured or difficult-to-measure family-level variables that affect residential choices are relevant for adolescent behavior; see, for example, Ludwig, Duncan and Hirschfield (1999).

<sup>7</sup> Michael Stegman, Assistant Secretary for Policy Development and Research at HUD at the time, in forward to Goering, Carnevale and Teodoro (1996).

<sup>8</sup> Two-thirds of both groups were assigned Section 8 certificates, while the remainder were assigned vouchers.

<sup>9</sup> In principle we could distinguish between the effects of the behaviors of one's neighbors ( $Y_{in}$ ) versus their background characteristics ( $X_{in}$ ), but Manski (1993) and Moffitt (1998) argue that it will not be possible to separate the two types of effects in practice.

<sup>10</sup> Even short-term dips in economic outcomes for MTO program-movers would be consistent with most of these theoretical models. Social-interaction theories might predict such temporary changes as families require time to invest in social capital within their new neighborhoods, while families would also require some period of job search to take advantage of improved job prospects in the suburbs (as with the spatial-mismatch hypothesis). Evidence of no program effects would be consistent with one of several possibilities: Neighborhoods matter in the long-term, but our data series are too short to capture such effects; the version of Kain's

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spatial-mismatch hypothesis, in which suburban employers are more discriminatory than those in urban areas, is correct; or, neighborhood characteristics have no effect on economic outcomes.

<sup>11</sup> The notation and discussion here is similar to that in Katz, Kling and Liebman (1999).

<sup>12</sup> This estimator also assumes that none of the control families are “treated” (Angrist, Imbens and Rubin, 1996). We follow Katz, Kling and Liebman (1999) and define the experimental and Section 8-only “treatments” as “acceptance of the MTO subsidy,” and so by definition none of the control-group families are “treated,” since they are not offered the opportunity to relocate through the MTO program. The treatment impact in this case represents the difference in outcomes caused by moving through MTO versus those caused by whatever residential-mobility patterns the family would have experienced in the absence of MTO.

<sup>13</sup> Personal communication with Judie Feins and Debi Magri McInnis, Abt Associates.

<sup>14</sup> The correspondence for the 609 householders who answered the PA question:

		<u>DHR administrative data</u>	
		Not on PA	On PA
<u>Baseline survey data</u>	Not on PA	105	16
	On PA	106	382

<sup>15</sup> Maryland calculate’s a family’s eligibility based on monthly earnings less allowable child care expenses and a 30 percent deduction, which is then compared to a state benefit limit. Personal communications with Mark Millspaugh and Steve Sturgill, Maryland Department of Human Resources.

<sup>16</sup> Prior to the first quarter of 1995, the UI system started each person’s UI earnings history beginning with their second quarter of employment, and would thus omit the worker’s first quarter in a private-sector job (starting in 1995:2, the system began to record each person’s first quarter of work as well). This idiosyncrasy of the UI reporting system is unlikely to be much of a problem in practice, since (as described in detail below) the large majority of MTO householders had already worked for pay at some point prior to enrolling in the program in late 1994 or early 1995. (Private communication with John Janak, Jacob France Center, University of Baltimore.)

<sup>17</sup> The number of families differs across the three treatment groups because the Abt randomization algorithm attached a higher probability of assignment to the experimental group. In the Baltimore MTO site, the weighting proportions for the experimental, Section 8-only and control groups changed on February 1, 1996 from 8:3:5 to 3:8:5. This change could in principle affect our results if average economic outcomes are different across MTO cohorts. To address this possibility, we weight all of our estimates by the sampling proportions so that weighted fraction of families from each cohort is equal across the three MTO treatment groups.

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<sup>18</sup> A small proportion of experimental relocators in Baltimore moved to census tracts with 1990 poverty rates slightly higher than 10 percent. HUD and Abt Associates quickly detected the pattern and worked with CAN to ensure that all experimental relocators chose neighborhoods that met the program poverty-level requirement.

<sup>19</sup> While the families in the control group received no mobility assistance under the MTO program, a HUD-funded Hope VI project demolished four public housing sites during our sample period, including two located in the baseline census tracts (Lafayette Courts and Lexington Terrace.) Hence all families in these buildings, including around one-fifth of the families in the MTO control group, were forced to relocate either to other public housing buildings, or to private housing with Section 8 subsidies.

<sup>20</sup> The effects of the MTO program on mobility thus stand in contrast to those of the Experimental Housing Allowance Programs (EHAP) of the 1970's, which provided renters with housing subsidies and did not change either the mobility rates or neighborhood characteristics of program participants (Struyk and Bendick, 1981). The difference in mobility outcomes is presumably due in part to the fact that the renters in the EHAP program had more choice over (and thus were more satisfied with) their baseline housing units compared with families in MTO. Racial discrimination in housing markets was also almost surely less of a barrier to economic and racial integration in the 1990's than the 1970's.

<sup>21</sup> These calculations come from stacking the quarter-by-quarter data on PA receipt into a panel, and estimating the average difference in PA receipt across MTO treatment groups using a linear probability model. Robust standard errors are calculated to adjust for the nonindependence of observations in the panel dataset.

<sup>22</sup> The regression-adjusted ITT effects presented in Tables 8 and 9 control for baseline characteristics such as householder age, education, marital status, number of children, and employment status during each of the eight quarters prior to random assignment.

<sup>23</sup> For example, some analysts have hypothesized that social programs may have their greatest impacts on families that are “optimally constrained,” defined as those whose labor market prospects are sufficiently strong such that they can take advantage of the opportunities offered by MTO, but not so strong that they will succeed even in the absence of the program. In order to explore this hypothesis, we created an index that measure the number of “constraints” that each householder faces by summing together indicator variables such as whether the householder is a high school dropout, whether the householder’s mother was on welfare, whether the household contains has one child under six years of age (or two or more young children), and whether anyone in the home has a disability. We find no differences in program impacts when we stratify our analytic sample by the value of this index, which is robust to a number of different definitions for our “constraint” variable.

<sup>24</sup> Specifically, we created a series of indicator dummies equal to 1 when the household head’s UI earnings exceeded some threshold level (cutoffs that we examined included \$1,000, \$2,000, \$3,000, \$4,000, \$5,000, and \$6,000 per quarter), and found no difference across treatment groups in the proportion of householders with earnings above these cutoffs.

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<sup>25</sup> While we do not have information on the sector in which welfare-to-work exiters are employed, we do have job descriptions. Most jobs could plausibly fall into either UI-covered or -uncovered sectors, though around 15 percent hold jobs in either housekeeping or daycare that may be uncovered (if these people work as independent employees).

<sup>26</sup> Thanks to Bruce Meyer for this point.

<sup>27</sup> While Baltimore City has more DHR offices than any other county in the state (14), Baltimore County has five satellite offices, Anne Arundel has two, Montgomery has four, and while Howard County has only one satellite office, it is located in Columbia, Maryland, where almost all of the Howard County relocators live. (Private correspondence, Steve Sturgill, Maryland Department of Human Resources).

<sup>28</sup> Private correspondence with Kay Finegan and Richard Larson, Maryland Department of Human Resources.

<sup>29</sup> Helen Ladd and Jens Ludwig surveyed 121 of the 143 experimental-group families (85 percent) who had been randomized through April 1995 and had successfully relocated through the MTO program, and 83 of the 141 Section 8-only families (59 percent) who had been randomly assigned by our cutoff date.

<sup>30</sup> Householders are asked “Do you think the job opportunities for you are better in your old or new neighborhood?”, and “Do you think the opportunities for you to go to school or get training are better in your old or new neighborhood?” and “Do you think the opportunities for you to provide day care or find someone to watch for your child (or children) are better in your new neighborhood or in your old neighborhood?”

<sup>31</sup> Interviews with Maryland DHR staff suggested that suburban welfare offices have lower caseloads per caseworker than city offices, thus enabling staff to focus more intensively on assisting welfare recipients transition into work. Further, in city office the responsibilities of caseworkers are limited primarily to eligibility determination; support services for welfare-to-work transitions are provided by subcontractors to whom welfare recipients are referred by the DHR office. In contrast, in the suburban offices caseworkers provide both eligibility determination and welfare-to-work assistance, and thus may move families into the workforce more quickly. Suburban DHR offices may also have better relationships with local employers, and typically are more likely to make use of innovative services such as the state’s Welfare Avoidance Grants, which enable families to borrow against future welfare payments to make large one-time expenditures such as automobile repairs. (Private correspondence with Richard Larson, Maryland Department of Human Resources).