Institute of
Business and
Economic Research

Fisher Center for
Real Estate and
Urban Economics

PROGRAM ON HOUSING AND URBAN POLICY

SEMINAR PAPER SERIES

SEMINAR NO. S00-002

HOUSING VOUCHERS AND ECNOMIC SELF-SUFFICIENCY: EVIDENCE FROM A RANDOMIZED EXPERIMENT

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Preliminary – please do not cite or quote

Housing Vouchers and Economic Self-Sufficiency:
Evidence from a Randomized Experiment

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draft date: November 9, 1999

Abstract

Housing policies for low-income families may affect the concentration of poverty in America, which could in turn affect the ability of families receiving housing services to become economically self-sufficient. In this paper we examine the effects of a randomized housing-voucher experiment on welfare receipt and labor market outcomes, both of which are measured using state administrative data. We find that providing families in high-poverty public housing areas with housing vouchers that can only be redeemed in low-poverty neighborhoods reduces rates of welfare use by around 6 percentage points. Most of this reduction in welfare receipt 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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This paper is part of an ongoing evaluation conducted with Helen F. Ladd sponsored by the U.S. Department of Housing and Urban Development. Additional funding has been provided by the Spencer, Andrew Mellon, William T. Grant and Smith Richardson foundations, and the Georgetown University Graduate School of Arts and Sciences. Thanks to Rohit Burman, Dick Doran, Judie Feins, Bob Gajdys, Janice Gentry, Rebecca Hawkins, Paul Hirschfield, Laura Hodges, John Janak, Matthew Kazcmierzak, Julie McGovern, Debbi Magri-McInnis, Juan Carlos Mendoza, John Peterson, Terence Trader, Kerry Whitacre, Amy Yost, and Philip Walsh for valuable assistance in compiling and analyzing the data. Thanks to John Cawley, Duncan Chaplin, Ruth Crystal, Todd Elder, John Goering, Julie Henly, Chris Jepsen, Jeff Kling, Helen Ladd, Charles Manski, Susan Mayer, Bruce Meyer, James Peterson, Steve Pischke, John Quigley, Jim Rosenbaum, Matt Stagner, Julie Wilson, and seminar participants at Northwestern University and the PAA and APPAM meetings for helpful comments. The views expressed here reflect those of the authors alone.

I. INTRODUCTION

In an attempt to improve the housing conditions of low-income Americans, a total of 1.1 million units have been added to the public housing stock in the United States during the past 50 years (Quigley, 1999). Yet many of these public housing units were clustered together in central-city neighborhoods for local political reasons, potentially contributing to the concentration of poor (and predominantly minority) families in densely populated urban neighborhoods. The effects of public 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), and as a result the population living in high-poverty census tracts[1] nearly doubled between 1970 and 1990, from 4.1 to 8.0 million (Jargowsky, 1997). Many observers believe that the concentration of low-income families in high-poverty inner-city neighborhoods increases the prevalence of social problems among these families because of limited access to job networks or middle-class role models, weak social support for work, or distance to suburban job opportunities (Wilson, 1987, 1996, Kain, 1968, 1992, Holzer, 1991, Mills and Lubuele, 1997). In essence, public housing programs designed to improve the housing situation of recipients may impair the ability of these families to escape from poverty.

[1] Defined as census tracts with poverty rates of 40 percent or more.

Motivated in part by concern about the effects of high-poverty neighborhoods on families, government housing policies now increasingly emphasize rental subsidy programs that may reduce poverty concentrations in central city 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 vouchers"[2]) as part of the Clinton Administration's Welfare-to-Work initiative (HUD, 1999). More generally, between 1975 and 1998 the number of housing vouchers provided to low-income families in the U.S. increased from 162,000 to 1.6 million (Quigley, 1999).

Yet very little is currently understood about the effects of neighborhood characteristics on economic outcomes. Previous studies focus on estimating the correlation between economic outcomes and census tract or ZIP code characteristics in observational datasets, with mostly mixed results.[3] But since most families have at least some degree of choice over where they live, these correlations may reflect either the causal effects of neighborhood conditions or the effects of unmeasured variables correlated with both residential decisions and economic success. The magnitude and even direction of bias that may result is difficult to determine, since those families

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[2] 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 vouchers."

[3] For reviews of the relevant literatures see Mayer and Jencks (1990), Holzer (1991), Kain (1992), O'Regan and Quigley (1997, 1998), and Teitz and Chapple (1998).

most likely to succeed in the labor market could plausibly choose to live in either low-poverty suburban areas (to take advantage of positive spillover effects) or higher-poverty urban areas (to take advantage of lower housing costs).[4]

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 who moved to the suburbs were found to have higher employment rates but similar wages compared to city movers (Rosenbaum and Popkin, 1992, Rosenbaum, 1995), and families who moved to neighborhoods with more highly-educated residents had lower rates of welfare receipt (Rosenbaum, Deluca and Miller, 1999). 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 randomized housing-voucher experiment. Since 1994, the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) demonstration

[4] Note that the same omitted variables bias is possible with studies of teenage employment rates. While teens are unlikely to make family decisions about residential location, unmeasured family variables that affect residential choices may in principle be relevant for adolescent behaviors such as employment. Evidence that self-selection into different neighborhoods can impart substantial bias to studies of juvenile behavior is presented in Ludwig, Duncan and Hirschfield (1999).

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 7 percentage points) seems to be explained by a difference in the proportion of experimental and control group families who transition from welfare to work as recorded by state welfare records. Since we find little effect of the experimental treatment on employment or earnings as measured by unemployment insurance (UI) records, we may conclude that many of these jobs are apparently not covered by the state UI system. We also find little difference between the Section 8-only and control groups with respect to welfare use, welfare-to-work transitions, or UI-covered employment and earnings beyond the first year after random assignment. We hasten to add that since MTO families are a self-selected group of public housing residents, our findings may not generalize to other low-income populations.

The paper is organized as follows. The next section describes the MTO experiment in greater detail. The third section discusses the conceptual framework for our analysis. The fourth section discusses the data used in our study. The fifth section presents empirical results for the mobility outcomes of MTO families, quarterly employment and earnings, welfare receipt, and householder cohabitation. The sixth section discusses the 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."[5] The importance of children's outcomes in motivating the MTO program is evidenced by the restriction of eligibility to families with children. And 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). The present paper builds on this literature by examining MTO's effects on adult economic outcomes in the Baltimore site.

Eligibility for the Baltimore MTO demonstration was restricted to very low-income families with children who lived in public 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

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[5] Michael Stegman, Assistant Secretary for Policy Development and Research at HUD at the time, in forward to Goering, Carnevale and Teodoro (1996).

households in these tracts had annual incomes of $50,000 or more (in 1990 dollars), and less than seven percent of adults in these areas 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 who 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.[6] 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 begin 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 also required experimental families to complete four workshops on topics such as budgeting, conducting a housing or job search, dealing with landlords, and conflict resolution. Families in the Section 8-only group receive 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 leases for one

[6] Two-thirds of both groups were assigned Section 8 certificates, while the remainder were assigned vouchers.

year. Those who wished to move again before the initial lease expiration date were not eligible for a new Section 8 subsidy. Families who 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},[7] neighborhood-specific unobservables ε_n, and unmeasured variables that are specific to the family and neighborhood, ε_{in}. Given the large literature on the effects of individual characteristics on earnings, we focus on the effects of neighborhood conditions.

(1) $$Y_{in} = \beta_0 + \beta_1 X_{in} + \beta_2 X_{-in} + \varepsilon_n + \varepsilon_{in}$$

A large literature in sociology and economics suggests that the socio-economic characteristics of one's neighbors may affect labor supply decisions as well as the employment outcomes and earnings of those who choose to work. Many studies emphasize the role that neighborhood "social capital" plays in obtaining a job. For example, William Julius Wilson and

[7] In principle we could allow for a separate effect of the value of the outcome among one's neighbors, Y_{-in}, but Manski (1993) and Moffitt (1998) argue that it will not be possible to distinguish between the effects of neighbor background characteristics and neighbor's behaviors with respect to the outcome variable of interest.

others have argued that jobless neighbors are poor sources of information about job openings, and are of limited value in providing job recommendations (Wilson, 1987, 1996, Montgomery, 1991, Holzer, 1996, Topa, 1996). Other studies emphasize the social support that neighbors provide for work, or the stigma associated with welfare, and the possibility that these social norms may vary across neighborhoods (Moffitt, 1983, Wilson, 1987). Finally, Wilson (1987) suggests that total household income may be higher in low- than high-poverty neighborhoods because of differences in the availability of "marriageable men" across areas.

In the short run, the net effects on economic outcomes from MTO-induced changes in neighborhood socio-economic characteristics are difficult to predict. On the one hand, employed neighbors may provide useful job referrals and may encourage work and discourage welfare receipt. On the other hand, MTO families may have social capital in their baseline neighborhoods (such as access to friends and relatives who can provide informal child care or other assistance) that becomes less accessible following a move, which could depress labor market outcomes until families have developed new social ties in low-poverty areas.

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. 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, $\varepsilon_{n.}$ Other unmeasured neighborhood characteristics that may affect economic outcomes include the quality of local public transportation, or other public services such as the support for welfare-to-work transitions from local welfare caseworkers.

The fundamental challenge in identifying the parameters in equation (1) stems from the possibility that unmeasured individual-level variables that affect residential choices are also correlated with economic 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.

(2) $E [X_{-in} \varepsilon_{in}] \neq 0$

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.[8] More formally, if Z1 and Z2 are indicators for assignment to the experimental and Section 8-only groups, respectively (with the individual and neighborhood subscripts suppressed for simplicity), and Y is the outcome measure as above, the ITT effects for the experimental and Section 8-only treatments are given by equations (3) and (4). These estimates represent the effects of the offer to relocate through the experimental or Section 8-only groups.

o infinity because of random assignment.

[8] The following discussion draws from Katz, Kling and Liebman (1999).

(3) ITT, experimental treatment = E [Y | Z1=1, Z2=0] - E [Y | Z1=0, Z2=0]

(4) ITT, Section 8-only treatment = E [Y | Z1=0, Z2=1] - E [Y | Z1=0, Z2=0]

The ITT estimates are derived by estimating regression equation (5), where the program impacts from equations (3) and (4) are given by the estimates for α_1 and α_2. The model includes a vector of X_{in} of background variables to improve the precision of our estimates. The inclusion of these variables should have little effect on the point estimates for α_1 and α_2. in large samples, but may change the estimates somewhat if the distributions of the background variables differ across treatment groups due to random chance.

(5) $Y_{in} = \alpha_0 + \alpha_1 Z1_{in} + \alpha_2 Z2_{in} + \alpha_3 X_{in} + v_{in}$

Also of interest are the effects of the experimental and Section 8-only treatments on those who comply with their assignments – that is, those who relocate through the MTO program. If C1 is an indicator for those who would comply with the experimental treatment if they were assigned into the experimental group, and let C2 similarly represent an indicator for potential compliance with the Section 8-only treatment, then the "effects of treatment on the treated" (TOT) are given by equations (6) and (7).

(6) TOT, experimental treatment = E [Y | Z1=1, Z2=0, C1=1] - E [Y | Z1=0, Z2=0, C1=1]

(7) TOT, Section 8-only treatment = E [Y | Z1=0, Z2=1, C2=1] - E [Y | Z1=0, Z2=0,C2=1]

Since we cannot determine which control group families would have complied with the experimental or Section 8-only treatments had they been assigned into either of these treatment groups, the values E [Y | Z1=0, Z2=0, C1=1] and E [Y | Z1=0, Z2=0, C2=1] are 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 (equation 8), and if the experimental and Section 8-only treatments have no effect on the outcomes of non-compliers (equation 9). This second assumption is almost surely met in the case of the Section 8-only treatment, since families assigned to this group receive no additional services under MTO beyond the offer to relocate.[9] While experimental-group non-compliers could in principle be affected by the counseling offered as part of this treatment, in practice even intensive adult training programs typically produce quite modest effects on employment and earnings (Heckman, Lochner, Smith and Taber, 1997).[10]

(8) TOT assumption 1: $P[C1=1 \mid Z1=1, Z2=0] = P[C1=1 \mid Z1=0, Z2=0]$

$P[C2=1 \mid Z1=0, Z2=1] = P[C2=1 \mid Z1=0, Z2=0]$

(9) TOT assumption 2: $E[Y \mid Z1=1, Z2=0, C1=0] = E[Y \mid Z1=0, Z2=0, C1=0]$

$E[Y \mid Z1=0, Z2=0, C2=0] = E[Y \mid Z1=0, Z2=0, C2=0]$

If these two assumptions are met, the TOT estimates can be calculated by defining indicator variables D1 and D2 for observed (rather than potential) compliance with the

[9] Since MTO families are given a specific time limit within which they must identify and lease a private-market apartment, it is possible that some families who are unable to relocate before their Section 8 vouchers expire may experience a change in Y as the result of frustration or disappointment. However, these effects are probably modest and short-term.

[10] This estimator also assumes that none of the control families are "treated" (Angrist, Imbens and Rubin, 1996), which is met under our definition of "treatment" as "relocation through the MTO program."

experimental and Section 8-only treatments, and estimating equation (10) via two-stage least squares using Z1 and Z2 as instruments for D1 and D2. Since the parameter estimates of interest from (10) will equal β_1=(α_1 / P[C1=1 | Z1=1, Z2=0, X]) and β_2=(α_2 / P[C2=1 | Z1=0, Z2=1,X]), the TOT estimates can also be calculated directly by dividing the ITT estimates from equation (5) by the probability of compliance with the MTO treatment conditional on the baseline characteristics (Bloom, 1984, Angrist, Imbens and Rubin, 1996, Manski, 1996).

(10) $Y_{in} = \beta_0 + \beta_1 D1 + \beta_2 D2 + \beta_3 X_{in} + \eta_{in}$

Finally, while the MTO experiment enables us to overcome the problem of self-selection into different neighborhoods, we cannot identify which neighborhood characteristics are responsible for any observed effects. This identification problem arises because MTO simultaneously changes all of the neighborhood characteristics of program movers (X_{-in} and ε_n from equation 1), and thus the ITT and TOT estimates reflect the combined effects from all of these changes.

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) 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 includes 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 are current as of December, 1997, and 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 who 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 living 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.[11]

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

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[11] Personal communication with Judie Feins and Debi Magri McInnis, Abt Associates.

PA benefits to spend at least 20 hours per week in an acceptable work or training program beginning in the family's 24th 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 of residential mobility on welfare receipt that would hold 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 that results 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, which for simplicity is assumed to be equal across the two groups. As seen in equation (11), the estimated impact of MTO on welfare receipt equals M times the true effect.

(11) $$\text{Estimated Impact} = (M \times W_E) - (M \times W_C) = M \times (W_E - W_C)$$

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 who receive PA benefits according to the baseline surveys, but not according to the DHR administrative data.[12]

[12] The correspondence for the 609 householders who answered the PA question:

		DHR administrative data	
		Not on PA	On PA
Baseline survey data	Not on PA	105	16
	On PA	106	382

As part of the state's Work Opportunity Management Information System (WOMIS), the DHR also maintains records on each case where a family officially leaves welfare because of employment (either a transition into a sufficiently high-paying job, or a sufficiently large increase in earnings from a previously held job).[13] The number of welfare-to-work exits recorded for the MTO program population (N=83 across all three treatment groups, 45 of which occurred during the post-program period) is only a fraction of the total number of welfare exits for MTO families, which is 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 additional welfare exits were associated with changes in earnings or employment but were officially 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 during a quarter to one or more employees is subject to the state's UI tax and thus must report quarterly payments to each employee, though certain people are exempt from this tax including 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

[13] Private correspondence with Steve Sturgill, Maryland Department of Human Resources.

(Kornfeld and Bloom, 1999). Around 93 percent of all jobs in the formal labor market in Maryland are covered by the state UI system (Born, 1999). Income from off-the-books work will obviously also be omitted from UI earnings records.

The DLLR used social security numbers, dates of birth, and first and last names to match our MTO participant lists with UI earnings records for the second quarter of 1985 through the first quarter of 1999.[14] 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 – of those MTO householders who reported holding a job for pay at some point in her life, 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 produce 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

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[14] 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 idiosyncracy 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.)

account for a substantial share of total income for welfare recipients (Edin and Lein, 1997) and may be affected by residential-mobility programs such as MTO. 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

In this section we begin by presenting the baseline characteristics and relocation outcomes of the MTO program population. We then examine the effects of the program on welfare receipt, employment and earnings, and cohabitation.

A. Characteristics of the MTO Population

Table 1 presents information about MTO participants from the baseline surveys, and highlights the challenges that many families face in escaping from poverty. Almost all of the MTO households are headed by a single woman. Only around 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. Informal social networks play an important role in these labor market outcomes, as evidenced by the fact that around two-thirds of all householders employed at baseline first heard about their current job from a neighbor, friend, or family member. Furthermore, Table 1 also shows that almost all of the families in the Baltimore MTO program are African-American, and thus (under one version of the spatial mismatch hypothesis) may encounter labor market discrimination in the suburbs.

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 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 (73 versus 54 percent). Of the Section 8-only families who 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-relocators ran up against the Section 8 subsidy time limit. One-quarter of the experimental non-relocators did not successfully complete the mandatory CAN counseling program (and were thus not allowed to relocate), and the remaining quarter never contacted CAN after being assigned to the experimental group.

While relocation rates are higher among the Section 8-only group, the experimental families who relocate are more dispersed throughout Baltimore City and the larger metropolitan area, as seen by their initial relocation addresses shown in Figure 1.

Table 3 provides more detailed information about the post-program neighborhoods of MTO families. By design, (nearly) all of the experimental relocators move to low-poverty census tracts with 1990 poverty rates below 10 percent,[15] and around 40 percent of those experimental families who relocate through MTO move outside of Baltimore City. In contrast to the experimental-group relocators, only around one in ten of the Section 8-only relocators voluntarily moved to census tracts with poverty rates under 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.

[15] 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.

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 have completed their initial one-year leases and are free to relocate to higher- or lower-poverty neighborhoods as they wish. While some control group families moved to lower-poverty neighborhoods on their own, the 1998 addresses show that only 5 percent had moved to very low-poverty tracts (<10 percent) by this time.[16] In contrast, most of the experimental and Section 8-only relocators remain in neighborhoods that are quite similar to where they originally moved through the MTO program.

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 rate or neighborhood characteristics of program participants (Struyk and Bendick, 1981). The difference in mobility outcomes is presumably due 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.

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[16] 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.

Presumably racial discrimination in housing markets was also less of a barrier to economic and racial integration in the 1990's than the 1970's.

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. Table 4 presents descriptive statistics for quarterly welfare receipt by householders in each of the three MTO treatment groups. 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. These figures also show 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 most likely due to chance for several reasons. First, we observe no systematic differences across treatment groups in baseline survey variables for the MTO demonstrations in Baltimore (Tables 1 and 2) or Boston (Katz, Kling and Liebman, 1999), suggesting that randomization was conducted properly. There are also no systematic pre-program differences in *household-level* PA receipt (defined as PA receipt by anyone in the household), as shown in the last three columns of Table 4.

Table 5 presents differences in welfare receipt across MTO treatment groups (intent-to-treat, or ITT, effects) after regression-adjusting for the pre-program differences in welfare receipt shown in Table 4. Our estimates are obtained by estimating linear probability models for the difference in welfare receipt across MTO treatment groups separately for each post-program quarter; robust standard errors are presented in parentheses. Explanatory variables in the regression models include indicators for assignment to the experimental and Section 8-only groups, variables from the baseline surveys including householder educational attainment (indicators for high school graduate, and for GED completion), marital status, sex, age, and number of children, as well as separate dummy indicators for welfare receipt during each of the eight quarters before randomization taken from the state administrative records.

As seen in Table 5, the likelihood that a household head in the experimental group is on welfare during a given quarter in the post-program period is around 7 percentage points lower compared with those in the control group, a difference that is statistically significant at the 5 percent level.[17] The difference in PA receipt between experimental and control families seems to grow over time, from around 6 points during the first two years following random assignment to nearly 10 points during the third year. Since households rather than householders may be the appropriate unit of analysis if households alternate which member receives benefits, we replicate our analysis for household-level welfare receipt and obtain similar findings (Table 5).

While Section 8-only householders are about 6 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 disappear shortly thereafter.

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[17] 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.

The ITT estimates presented above average together the impact of the MTO program on program movers and non-movers. Since the effects on families who actually move through MTO are also of some social science interest, in Table 6 we present the effects of the program on those who move (the effects of treatment on the treated, or TOT). We recover these estimates by dividing the intent-to-treat estimates from Table 5 by the regression-adjusted probability that families in the experimental and Section 8-only group comply with their treatments (i.e., relocate). Since around half of the experimental families move through MTO, program-movers in this group are around 12 percentage points less likely to receive welfare at any point during the post-program period than those who would have moved had they been assigned to the experimental group, but were assigned to the control group.[18]

Our comparisons of welfare receipt across MTO treatment groups during the post-program period provide limited information about the program's effects on the *dynamics* of welfare receipt. The reason is that MTO could affect the proportion of families on welfare at a point in time by changing the proportion of families who ever receive welfare during the post-program period, the average length of a welfare spell, or the number of welfare spells per family.

In Table 7, we show that experimental group families exit and stay off welfare, while the MTO program may just change the timing of when Section 8-only families choose to leave welfare. We only present ITT estimates for simplicity, and examine the effects of MTO on welfare dynamics using two different approaches. The first method examines the proportion of post-program quarters that families spend on welfare, following Duncan (1984) and others.

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[18] Table 6 estimates for (dp/dx) in the current version of this paper are calculated from probit models, which will be changed to linear probability-model effects in the next version.

These calculations, presented in Table 7, suggest that the primary effect of the experimental treatment beyond the first year is to increase the proportion of families who are off welfare altogether relative to controls, rather than to change the duration of spells for families who cycle on and off welfare. On the other hand, by the third year following random assignment the proportion of Section 8-only families who have extended welfare spells is lower than for the control group, but the proportion of families who are off welfare altogether is also lower. The results presented in Tables 5 and 6 suggest that the net effects are to leave the overall welfare receipt rate unchanged relative to controls.

Another way to see differences in welfare dynamics across MTO treatment groups is to examine transition probabilities, following Bane and Ellwood (1986).[19] Interpretation of the results presented in Table 8 is complicated by the fact that the probability of a transition onto or off of welfare is conditional on an endogenous post-program outcome (current welfare status). The estimates in Table 8 thus cannot be interpreted as ITT effects, but can nonetheless illuminate suggestive differences in welfare dynamics across groups. For example, we find that among families on welfare during the first post-program year, those in the experimental group are more likely to exit than those in the control group. During the second year, among families who are off of welfare those in the experimental group are less likely to re-apply for benefits relative to

[19] We calculate the probability of transitions onto welfare by constructing a panel dataset of all person-quarters off of welfare, and calculating the difference across MTO treatment groups in the probability that a quarter off of welfare will end in a transition onto welfare. These effects are calculated using a probit model that controls for the householder's age, sex, marital status, educational attainment, number of children, and welfare receipt during each of the eight quarters before random assignment. Transitions off of welfare are calculated in a similar fashion. Standard errors are adjusted for the non-independence of repeated observations for the same individual.

controls. On the other hand, among families on welfare those 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

One explanation for the reductions in welfare receipt by the experimental group (and to a lesser extent the Section 8-only group) may be increased employment rates or earnings. And in fact Table 9 shows that the difference in the proportion of experimental and control group families who, according to the WOMIS data from the Maryland Department of Human Resources, officially exit welfare during the post-program period because of employment (around 6 percentage points, significant at the 5 percent level). Thus the difference in welfare-to-work transitions between the experimental and control group appears to account for most of the differences in welfare receipt. (We do not disaggregate the analysis to look at the timing of these exits because the number of DHR-recorded welfare-to-work transitions during the post-program period is quite small – only 45 total across all three of the MTO treatment groups).

In contrast, analysis of employment and earnings in UI-covered jobs suggests no systematic differences across MTO treatment groups for householder or household outcomes[20] (Tables 10 and 11). The findings are qualitatively similar when we focus on the natural logarithm of quarterly earnings, or calculate more formal ITT or TOT estimates by regression-

[20] We define the employment variable for households as equal to one if anyone in the household holds a job in a given quarter, where household members are defined as those who live with the household head at the time of the baseline survey. The household earnings variable is equal to the sum of earnings for every member of the household.

adjusting to 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. While in principle MTO could have had an effect on job tenure or job transitions that is not reflected in quarterly employment or earnings rates, our empirical analysis provides no support for such changes. We also find little effect of MTO on UI-covered earnings and employment for different population subgroups.[21]

How do we reconcile the differences in findings between the welfare data, which suggest significant differences between experimental and Section 8-only families in welfare-to-work transitions, and the UI data, which suggest no differences in average earnings or employment rates? One obvious explanation is that the welfare data captures moves into primary or

[21] For example, some analysts have hypothesized that social programs may have their greatest impacts on families who 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.

secondary jobs that are not covered by the UI system in Maryland, and in fact we find that fully one-third of those who experience welfare-to-work transitions according to the welfare data do not have employment in UI-covered jobs at that time. Another possibility is that the proportion of families who experience UI-covered earnings above some cutoff value for exiting welfare is higher among experimentals than controls. A third possibility is that experimental families experience an increase in earnings in UI-covered jobs that are not reported to the UI system. One reason that employers may under-report earnings to the DLLR is because these earnings are subject to UI taxes. Some evidence to support this last possibility comes from Kornfeld and Bloom (1999), who find that earnings reported to the Internal Revenue Service (where payroll can be deducted as a business expense) are 14 to 25 percent higher than those reported to state UI agencies.

Neither the WOMIS nor UI datafiles will capture changes in off-the-books earnings, which account for a large share of the total income received by many welfare recipients (Edin and Lein,1997) and could also explain part of the experimental / control difference in welfare use.

E. Cohabitation

Another explanation for the differences in welfare receipt between the experimental and control groups may be differences in family formation, which accounts for a large share of transitions out of poverty (Bane and Ellwood, 1986, Blank, 1997). 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. On the other hand, if the "market" for adult companions occurs at a geographic level larger than the neighborhood – for example, at the metropolitan-area level – then moving families from one part of the metropolitan area to another may have little effect on household composition.

In the top panel of Table 12, we show that there appear to be no differences between the experimental and control groups in cohabitation rates, defined as the presence within the household of a non-related adult of the opposite sex. If anything, MTO serves to reduce the rate at which Section 8-only householders who are not cohabiting at baseline wind up living with men during the post-program period, as shown in the last row of the top panel of Table 12.

Household composition may also change if friends or relatives of the householder move in with the family in their new low-poverty neighborhood, or if adult children are more likely to stay in the home in low-poverty areas. In the bottom panels of Table 12 we show that MTO experimental families are nearly 5 percentage points more likely than controls to have adult children[22] within the home. While this difference is not statistically significant, the difference of 5 percentage points in the presence of other adults within the home represents a substantial share of the 7 percentage point difference in welfare receipt between the experimental and control groups.

Since families may have incentives to mis-report the presence of other adults within the home, our estimates may substantially understate the rate of cohabiting in households in each of the three MTO groups. Any financial incentives to under-state cohabitation or marriage may be partially offset by the tendency of survey respondents to present themselves favorably to survey

[22] We define "adult children" as those aged 18 and older at the time of the baseline survey.

interviewers (Sudman and Bradburn, 1974), a tendency that may be more pronounced among experimental and Section 8-only families who live in areas where single-parent households may be less common than in control neighborhoods. If this is true, our estimates may somewhat overstate any experimental group gain in cohabitation.

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 7 percentage points. 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 after randomization. Data from Maryland's welfare system suggest that most of the difference in welfare use between experimental and control households may be explained by differences in welfare-to-work transitions, even though analysis of unemployment insurance data reveal few differences in UI-covered employment or earnings. Differences in the presence of other adults within the home could explain part of the effects on welfare use as well, though our information on post-program household composition comes from survey data and may be subject to self-reporting errors.

An alternative explanation for our findings is that experimental group families move to areas where welfare offices are less accessible, and thus families exit from welfare because they are unable to verify their eligibility for PA benefits each six or twelve months. While it is true that Baltimore City has more satellite DHR offices (14) than any of the suburban counties (and presumably better public transportation), each of the suburban counties has multiple satellites and in fact many of the experimental families who move to the suburbs locate near these offices.[23] Moreover, access is unlikely to explain differences in welfare use given the substantial financial incentives that families have to navigate the trip to their local DHR office.

A related explanation is that suburban caseworkers enforce welfare regulations more strictly than do caseworkers in the city, and are more likely to record welfare exits as being due to employment rather than other causes. Yet welfare eligibility rules are set at the state- rather than county-level in Maryland. Moreover, 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.[24]

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[23] 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).

[24] Private correspondence with Kay Finegan and Richard Larson, Maryland Department of Human Resources.

We thus believe that the results presented here most likely reflect voluntary welfare exits on the part of MTO experimental-group families rather than differences in access to welfare offices or enforcement of welfare eligibility rules. Some additional support for this view comes from follow-up surveys of MTO program-movers conducted by Helen Ladd and Jens Ludwig between 1997 and 1998.[25] These survey results suggest that the majority of experimental relocators report that job and training opportunities are better in the new versus old neighborhoods (66 and 63 percent, respectively).[26] 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

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[25] Ladd and 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.

[26] 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?"

to perceived opportunities among the control group during this period, but they are suggestive. (We do not draw more heavily on these follow-up surveys in this paper because of relatively low response rates, particularly among experimental non-compliers and control families).

Our analysis cannot formally identify the specific mechanisms responsible for what we believe to be behavioral effects on experimental-group families because MTO changes many different neighborhood characteristics simultaneously for program movers. Yet understanding the specific mechanisms through which neighborhoods affect behavior is of some importance for public policy. The changes in welfare use and welfare-to-work transitions could be due to behavioral responses to changes in neighborhood social conditions, improved access to employed neighbors who may provide job information or referrals, closer physical proximity to job opportunities, or more effective social services. This last explanation receives some support from interviews with staff at the Maryland DHR.[27] Yet some suggestive evidence that at least part of the welfare effect reported in this paper may be due to changes in social interactions comes from findings that the MTO experimental group in Baltimore and Boston experience reductions in juvenile delinquency and other problem behaviors, and improvements in the mental health of adults (Katz, Kling and Liebman, 1999, Ludwig, Duncan and Hirschfield, 1999).

[27] Suburban DHR 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.

Evidence that neighborhood conditions affect economic outcomes for MTO participants is not sufficient to conclude that housing voucher programs are good public policy. Additional evidence is required to determine whether similar effects are observed for different populations of public housing residents, since MTO families are a self-selected subgroup of those living in public housing. Moreover, any systematic cost-benefit analysis of housing voucher programs must incorporate the effects of these policies on outcomes beyond earnings and welfare receipt (such as schooling, crime, and health), and should also account for the effects on the residents of low-poverty host neighborhoods.

(Private correspondence with Richard Larson, Maryland Department of Human Resources).

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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
Householder characteristics:				
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
Householder Earnings/Work:				
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
Commuting (employed householders):				
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
How householder heard about current job:				
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

	Total	Experimental	Section 8-Only	Control
Criminal Victimization				
During last 6 months, someone in HH:				
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
Primary reason for wanting to move:				
Better schools	11.7	9.8	14.4	11.5
To be near job	0.5	0	1.1	0.5
Better transportation	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
Second most important reason for move:				
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	0.5
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 (betwee 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 Families

	Baseline (all families)	Experimental		Section 8-only		Control	
	1994-5	Initial moves	12/97	Initial moves	12/97	Initial	12/97
Distribution of MTO Households							
Jurisdiction :							
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
% Census Tract Poor:							
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
% Adults in Tract w/ College Education:							
0 - 9.9	71.6	41.6	43.6	65.2	61.4	70.7	68.7
10 - 19.9	26.0	26.4	26.8	25.5	25.0	25.8	26.8
20 - 29.9	2.4	22.4	18.4	5.4	8.7	3.5	2.5
30 - 39.9	0.0	2.4	4.4	1.1	2.2	0.0	1.0
40 - 49.9	0.0	0.0	0.8	0.0	0.0	0.0	0.0
50 - 59.9	0.0	7.2	6.0	2.7	2.7	0.0	0.0
60 - 69.9	0.0	0.0	0.0	0.0	0.0	0.0	0.0
70 - 79.9	0.0	0.0	0.0	0.0	0.0	0.0	0.0
80 plus	0.0	0.0	0.0	0.0	0.0	0.0	0.0

NOTES: Neighborhood characteristics are calculated using 1990 Census data. a. The FBI's Uniform Crime Report index crimes are homicide, forcible rape, robbery, assault, breaking and entering, larceny-theft, motor vehicle theft, and arson (Maryland State Police, 1997).

Table 4
Quarterly Public-Assistance Receipt by MTO Householders and Households

	Percent household heads receiving PA			Percent households receiving PA		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.43 (0.03)	0.49 (0.04)*	0.40 (0.04)	0.65 (0.03)	0.67 (0.04)	0.63 (0.03)
-3	0.44 (0.03)	0.50 (0.04)**	0.40 (0.04)	0.66 (0.03)	0.66 (0.04)	0.62 (0.04)
-2	0.44 (0.03)	0.54 (0.04)**	0.40 (0.04)	0.65 (0.03)	0.69 (0.03)	0.63 (0.03)
-1	0.45 (0.03)	0.57 (0.04)**	0.43 (0.04)	0.64 (0.03)	0.72 (0.03)	0.64 (0.03)
0	0.45 (0.03)	0.56 (0.04)**	0.45 (0.04)	0.64 (0.03)	0.71 (0.03)	0.64 (0.03)
1	0.45 (0.03)	0.56 (0.04)**	0.46 (0.04)	0.64 (0.03)	0.70 (0.03)	0.65 (0.03)
2	0.46 (0.03)	0.54 (0.04)	0.48 (0.04)	0.63 (0.03)	0.66 (0.04)	0.64 (0.03)
3	0.44 (0.03)	0.51 (0.04)	0.48 (0.04)	0.62 (0.03)	0.62 (0.04)	0.63 (0.03)
4	0.41 (0.03)	0.48 (0.04)	0.47 (0.04)	0.56 (0.03)	0.56 (0.04)	0.60 (0.04)
5	0.37 (0.03)**	0.46 (0.04)	0.48 (0.04)	0.51 (0.03)*	0.52 (0.04)	0.60 (0.04)
6	0.38 (0.03)	0.45 (0.04)	0.43 (0.04)	0.49 (0.03)	0.51 (0.04)	0.55 (0.04)
7	0.38 (0.03)	0.47 (0.04)	0.42 (0.04)	0.49 (0.03)	0.55 (0.04)	0.55 (0.04)
8	0.37 (0.03)	0.48 (0.04)	0.39 (0.04)	0.46 (0.03)	0.53 (0.04)	0.50 (0.04)
9	0.34 (0.03)*	0.51 (0.06)	0.44 (0.04)	0.41 (0.03)**	0.60 (0.05)	0.52 (0.04)
10	0.33 (0.03)**	0.51 (0.06)	0.44 (0.04)	0.39 (0.03)**	0.56 (0.06)	0.52 (0.04)
11	0.37 (0.03)	0.50 (0.06)	0.45 (0.04)	0.40 (0.03)	0.54 (0.05)	0.48 (0.04)
12	0.38 (0.04)	0.48 (0.06)	0.46 (0.05)	0.41 (0.04)	0.48 (0.06)	0.48 (0.05)
13	0.33 (0.04)**	0.43 (0.07)	0.47 (0.05)	0.34 (0.04)*	0.43 (0.07)	0.47 (0.05)

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

Table 5
Regression-Adjusted Intent-to-Treat Effects of MTO Program on Welfare Receipt

	Percent household heads receiving PA		Percent households receiving PA	
	Exp vs Control	S8-Only vs Control	Exp vs Control	S8-Only vs Control
Quarters Since Random Assignment:				
1	-0.028 (0.017)*	-0.021 (0.023)	-0.018 (0.019)	-0.008 (0.027)
2	-0.060 (0.025)**	-0.052 (0.029)*	-0.032 (0.023)	-0.023 (0.029)
3	-0.083 (0.032)**	-0.056 (0.033)*	-0.045 (0.031)	-0.042 (0.034)
4	-0.079 (0.037)**	-0.054 (0.041)	-0.077 (0.036)**	-0.076 (0.039)*
5	-0.111 (0.039)**	-0.092 (0.044)**	-0.102 (0.039)**	-0.113 (0.042)**
6	-0.069 (0.043)	-0.032 (0.050)	-0.085 (0.044)*	-0.052 (0.048)
7	-0.059 (0.047)	0.004 (0.056)	-0.076 (0.048)	-0.004 (0.053)
8	-0.038 (0.049)	0.049 (0.059)	-0.048 (0.050)	0.043 (0.056)
9	-0.097 (0.053)*	0.006 (0.068)	-0.092 (0.053)*	0.039 (0.065)
10	-0.106 (0.054)*	0.025 (0.070)	-0.113 (0.054)**	0.021 (0.067)
11	-0.079 (0.056)	0.022 (0.073)	-0.081 (0.055)	0.038 (0.068)
12	-0.073 (0.063)	-0.016 (0.081)	-0.045 (0.060)	-0.008 (0.075)
13	-0.093 (0.069)	-0.048 (0.089)	-0.085 (0.068)	-0.038 (0.083)
Entire Post-Program	-0.065 (0.026)**	-0.031 (0.029)	-0.060 (0.028)**	-0.028 (0.029)
Post-Program Qrtrs 1-4	-0.055 (0.022)**	-0.055 (0.026)**	-0.032 (0.023)	-0.054 (0.026)**
Post-Program Qrtrs 5-8	-0.062 (0.038)*	-0.040 (0.043)	-0.060 (0.039)	-0.066 (0.042)
Post-Program Qrtrs 9-13	-0.095 (0.049)**	0.001 (0.065)	-0.088 (0.050)*	0.013 (0.062)

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 calculated using a linear regression model; 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
Regression-Adjusted Effects of Treatment-on-the-Treated for Welfare Receipt by MTO Householders and Households

	Percent householders receiving PA		Percent households receiving PA	
	Effects of Treatment on Program-Movers		Effects of Treatment on Program-Movers	
	Exp vs Control	S8-Only vs Control	Exp vs Control	S8-Only vs Control
Quarters Since Random Assignment:				
1	-0.28 (0.15)*	-0.14 (0.14)	-0.09 (0.11)	-0.03 (0.11)
2	-0.35 (0.13)**	-0.22 (0.11)*	-0.19 (0.11)	-0.10 (0.10)
3	-0.31 (0.11)**	-0.15 (0.10)*	-0.17 (0.11)	-0.11 (0.08)
4	-0.24 (0.11)**	-0.12 (0.08)	-0.24 (0.11)**	-0.18 (0.08)**
5	-0.30 (0.09)**	-0.18 (0.08)**	-0.28 (0.11)**	-0.25 (0.08)**
6	-0.17 (0.09)	-0.07 (0.08)	-0.20 (0.11)**	-0.11 (0.08)
7	-0.13 (0.11)	0.01 (0.10)	-0.17 (0.11)	-0.01 (0.10)
8	-0.07 (0.11)	0.07 (0.10)	-0.11 (0.11)	0.07 (0.10)
9	-0.22 (0.11)*	0.01 (0.11)	-0.20 (0.11)*	0.07 (0.11)
10	-0.22 (0.11)**	0.04 (0.11)	-0.24 (0.11)**	0.03 (0.11)
11	-0.15 (0.11)	0.04 (0.11)	-0.17 (0.11)	0.05 (0.11)
12	-0.15 (0.11)	-0.03 (0.11)	-0.09 (0.13)	-0.01 (0.11)
13	-0.19 (0.13)	-0.07 (0.12)	-0.17 (0.13)	-0.05 (0.12)
Entire Post-Program	-0.17 (0.07)**	-0.07 (0.07)	-0.15 (0.07)*	-0.08 (0.05)
Post-Program Qrtrs 1-4	-0.24 (0.09)**	-0.18 (0.08)**	-0.13 (0.09)	-0.16 (0.07)**
Post-Program Qrtrs 5-8	-0.15 (0.09)	-0.07 (0.07)	-0.15 (0.09)	-0.12 (0.07)*
Post-Program Qrtrs 9-12	-0.19 (0.09)*	0.01 (0.10)	-0.19 (0.11)*	0.03 (0.10)

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. *Table 6 estimates for (dp/dx) in the current version of this paper are calculated from probit models, which will be changed to linear probability-model effects in the next version.*

Table 7 (Preliminary)
Intent-to-Treat Effects of MTO Program on Duration of Public Assistance Receipt

	Exp vs Control	S8-Only vs Control
% MTO families spending specified fraction of quarters on welfare during –		
First post-program year		
4/4	-0.07 (0.03)**	-0.09 (0.04)**
≥3/4	-0.07 (0.03)**	-0.09 (0.04)**
≥2/4	-0.08 (0.03)**	-0.08 (0.03)**
≥1/4	-0.08 (0.03)**	-0.08 (0.03)**
0/4	0.05 (0.03)	0.04 (0.04)
Second post-program year		
4/4	-0.05 (0.04)	-0.00 (0.04)
≥3/4	-0.05 (0.04)	-0.01 (0.05)
≥2/4	-0.06 (0.04)	0.00 (0.05)
≥1/4	-0.10 (0.04)**	-0.07 (0.05)
0/4	0.10 (0.04)**	0.07 (0.05)
Third+ post-program year		
4/4	-0.02 (0.05)	-0.08 (0.01)**
≥3/4	-0.01 (0.05)	-0.07 (0.02)**
≥2/4	-0.01 (0.06)	-0.11 (0.01)**
≥1/4	-0.01 (0.07)	-0.13 (0.01)**
0/4	0.14 (0.04)**	-0.08 (0.03)**

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 probit model; point estimates represent the change in the probability from a change in MTO treatment-group assignment (dp/dx).

Table 8
Welfare Transition Probabilities for MTO Householders During Post-Program Period

	Exp vs Control	S8-Only vs Control
Probability of welfare transition during–		
First post-program year		
Welfare entry	-0.01 (0.02)	-0.01 (0.02)
Welfare exit	0.05 (0.02)**	0.07 (0.02)**
Second post-program year		
Welfare entry	-0.03 (0.01)*	0.01 (0.02)
Welfare exit	-0.02 (0.02)	-0.05 (0.02)**
Third post-program year		
Welfare entry	0.01 (0.01)	-0.02 (0.01)
Welfare exit	0.03 (0.02)	-0.01 (0.02)

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 from a probit model that predicts the probability of a quarter spent on (off) welfare is followed by a quarter off (on) welfare using a panel dataset of person-quarters.

Table 9
Effects of MTO Program on Welfare Exits Due to Employment from Maryland Welfare Data

	Exp vs Control	Section 8-only vs Control
Intent-to-Treat Effect		
Exit from welfare during post-program period due to employment (%)	0.06 (0.03)**	0.01 (0.03)
Effects of Treatment-on-the-Treated		
Exit from welfare during post-program period due to employment (%)	0.11 (0.06)**	0.01 (0.04)

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 probit model, which controls for baseline survey characteristics such as the householder's age, sex, educational attainment, marital status, and number of children. Point estimates are calculated from a linear probability model.

Table 10
Quarterly Employment and Earnings for MTO Household Heads

	Percent householders employed			Quarterly earnings for household heads (in thousands)		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.19 (0.02)**	0.21 (0.03)*	0.29 (0.03)	0.41 (0.07)	0.35 (0.07)	0.62 (0.10)
-3	0.23 (0.03)	0.20 (0.03)	0.26 (0.03)	0.46 (0.07)	0.40 (0.08)	0.47 (0.08)
-2	0.23 (0.03)	0.30 (0.03)	0.29 (0.03)	0.49 (0.07)	0.51 (0.08)	0.61 (0.10)
-1	0.27 (0.03)	0.29 (0.03)	0.29 (0.03)	0.58 (0.08)	0.55 (0.09)	0.60 (0.10)
0	0.25 (0.03)	0.27 (0.03)	0.30 (0.03)	0.61 (0.08)	0.53 (0.08)	0.67 (0.11)
1	0.28 (0.03)*	0.29 (0.03)	0.35 (0.03)	0.60 (0.08)	0.56 (0.09)*	0.79 (0.11)
2	0.34 (0.03)	0.34 (0.03)	0.38 (0.03)	0.71 (0.08)	0.67 (0.10)	0.78 (0.10)
3	0.38 (0.03)	0.36 (0.03)	0.40 (0.03)	0.81 (0.09)	0.78 (0.10)	0.92 (0.13)
4	0.40 (0.03)	0.38 (0.04)	0.41 (0.04)	0.87 (0.09)	0.82 (0.11)	0.88 (0.11)
5	0.39 (0.03)	0.37 (0.04)	0.45 (0.04)	0.87 (0.09)	0.85 (0.11)	1.01 (0.11)
6	0.44 (0.03)	0.40 (0.04)*	0.49 (0.04)	1.02 (0.10)	0.94 (0.11)	1.06 (0.11)
7	0.44 (0.03)	0.41 (0.04)	0.46 (0.04)	1.08 (0.11)	1.04 (0.17)	1.04 (0.12)
8	0.42 (0.03)	0.42 (0.04)	0.49 (0.04)	1.03 (0.10)	1.11 (0.14)	1.22 (0.13)
9	0.42 (0.03)*	0.42 (0.04)	0.50 (0.04)	1.04 (0.10)	1.39 (0.23)	1.28 (0.13)
10	0.45 (0.03)	0.45 (0.04)	0.50 (0.04)	1.20 (0.12)	1.15 (0.14)	1.32 (0.13)
11	0.43 (0.03)	0.49 (0.04)	0.47 (0.04)	1.16 (0.11)	1.19 (0.14)	1.34 (0.14)
12	0.44 (0.03)	0.46 (0.04)	0.47 (0.04)	1.15 (0.11)	1.18 (0.14)	1.20 (0.14)
13	0.48 (0.03)	0.46 (0.06)	0.49 (0.04)	1.15 (0.12)	1.14 (0.19)	1.14 (0.14)
14	0.45 (0.03)	0.40 (0.05)	0.48 (0.04)	1.07 (0.11)	1.07 (0.19)	0.98 (0.13)
15	0.51 (0.03)	0.40 (0.05)*	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.

Table 11
Quarterly Employment and Earnings for MTO Households

	Percent households w/ employed member			Quarterly earnings for households (thousands)		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.21 (0.03)**	0.24 (0.03)	0.31 (0.03)	0.44 (0.07)	0.40 (0.07)	0.73 (0.12)
-3	0.25 (0.03)	0.22 (0.03)	0.28 (0.03)	0.50 (0.07)	0.44 (0.08)	0.62 (0.11)
-2	0.23 (0.03)**	0.34 (0.03)	0.32 (0.03)	0.55 (0.08)	0.57 (0.09)	0.76 (0.13)
-1	0.29 (0.03)	0.29 (0.03)	0.31 (0.03)	0.63 (0.09)	0.58 (0.09)	0.72 (0.12)
0	0.27 (0.03)	0.30 (0.03)	0.33 (0.03)	0.66 (0.08)	0.57 (0.09)	0.82 (0.13)
1	0.30 (0.03)*	0.34 (0.03)	0.38 (0.03)	0.67 (0.09)*	0.64 (0.09)*	0.94 (0.13)
2	0.40 (0.03)	0.38 (0.04)	0.42 (0.04)	0.78 (0.09)	0.76 (0.10)	0.93 (0.12)
3	0.44 (0.03)	0.38 (0.04)	0.45 (0.04)	0.91 (0.09)	0.87 (0.11)	1.06 (0.14)
4	0.44 (0.03)	0.40 (0.04)	0.45 (0.04)	0.96 (0.09)	0.89 (0.12)	1.07 (0.13)
5	0.43 (0.03)	0.41 (0.04)*	0.49 (0.04)	0.99 (0.10)	0.97 (0.12)	1.19 (0.13)
6	0.50 (0.03)	0.44 (0.04)*	0.54 (0.04)	1.16 (0.11)	1.06 (0.13)	1.32 (0.14)
7	0.49 (0.03)	0.44 (0.04)	0.52 (0.04)	1.22 (0.12)	1.15 (0.17)	1.23 (0.14)
8	0.47 (0.03)	0.45 (0.04)*	0.54 (0.04)	1.17 (0.11)	1.27 (0.16)	1.41 (0.15)
9	0.46 (0.03)	0.43 (0.04)*	0.53 (0.04)	1.27 (0.14)	1.46 (0.24)	1.43 (0.14)
10	0.50 (0.03)	0.48 (0.04)	0.55 (0.04)	1.38 (0.13)	1.30 (0.16)	1.51 (0.15)
11	0.48 (0.03)	0.51 (0.04)	0.54 (0.04)	1.36 (0.12)	1.38 (0.17)	1.50 (0.15)
12	0.49 (0.03)	0.48 (0.04)	0.52 (0.04)	1.33 (0.12)	1.30 (0.16)	1.37 (0.15)
13	0.51 (0.03)	0.49 (0.06)	0.53 (0.04)	1.35 (0.13)	1.36 (0.20)	1.27 (0.16)
14	0.45 (0.03)	0.40 (0.05)	0.48 (0.04)	1.27 (0.13)	1.19 (0.21)	1.15 (0.14)
15	0.51 (0.03)	0.40 (0.05)*	0.52 (0.04)	1.67 (0.17)	1.36 (0.23)	1.40 (0.17)
16	0.51 (0.04)	0.49 (0.06)	0.51 (0.05)	1.66 (0.19)	1.69 (0.28)	1.74 (0.24)
17	0.53 (0.04)	0.46 (0.07)	0.48 (0.05)	1.50 (0.17)	1.58 (0.30)	1.34 (0.21)

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

Table 12
Cohabitation and Household Composition for MTO Households, Pre- and Post-Program

	Exp (N=252) (%)	S8-Only (N=188) (%)	Control (N=198) (%)
Cohabit w/ Non-related Adult of Opposite Sex			
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)
Pre- & Post-Program	2.0 (0.9)	3.2 (1.3)	1.0 (0.7)
Pre-Program Only	0.0 (0.0)	0.0 (0.0)	0.5 (0.5)
Post-Program Only	7.9 (1.7)	3.7 (1.4)	7.0 (1.8)
Cohabit or Other Adult in Home (not including adult children)			
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)
Pre- & Post-Program	2.8 (1.0)	4.3 (1.5)**	1.0 (0.7)
Pre-Program Only	0.0 (0.0)*	0.0 (0.0)*	1.0 (0.7)
Post-Program Only	7.1 (1.6)	3.7 (1.4)	7.1 (1.8)
Cohabit or Any Adult in Home (including adult children)			
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)
Pre- & Post-Program	9.1 (1.8)	10.1 (2.2)	9.6 (2.1)
Pre-Program Only	2.0 (0.9)	2.7 (1.2)	1.5 (0.9)
Post-Program Only	23.8 (2.7)	14.4 (2.6)	18.7 (2.8)

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.