ELSEVIER

Journal of Public Economics 89 (2005) 131–156

JOURNAL OF PUBLIC ECONOMICS

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Housing mobility programs and economic self-sufficiency: Evidence from a randomized experiment

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Received 7 February 2000; received in revised form 9 July 2003; accepted 16 July 2003

Available online 28 February 2004

Abstract

This paper examines the effects of a randomized housing-voucher program on individual economic outcomes. Public housing residents who are offered relocation counseling together with housing vouchers that can only be redeemed in low-poverty areas experience a reduction in welfare receipt of between 11% and 16% compared to controls. These effects are not accompanied by changes in earnings or employment rates as measured by unemployment insurance records. Offering families unrestricted housing vouchers without additional counseling appears to have little effect on economic outcomes.

JEL classification: I38; R00; Z13

Keywords: Neighborhood effects; Housing policy; Urban labor markets; Urban poverty

1. Introduction

Each year in the United States, the government devotes nearly US$30 billion to housing-assistance programs, which together serve more families and at greater expense than the Aid to Families with Dependent Children (AFDC) program at its largest in 1994 (Harkness and Newman, 2002). A large majority of the public supports spending even

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0047-2727/$ - see front matter

doi:10.1016/j.jpubeco.2003.07.010

more to help the poor with housing.[1] But what form should government housing assistance take? Should the public sector be directly involved in supplying housing, or instead, simply subsidize poor families to rent housing within the private market?

The question of whether to support demand-side ("tenant-based") or supply-side ("project-based") housing programs is important for at least two reasons. First, there may be cost savings from relying on housing vouchers, although the evidence on this point is currently something less than definitive (HUD, 2000; Olsen, 2000; Shroder and Reiger, 2000; GAO, 2001). Second, compared with project-based programs, tenant-based subsidies, such as housing vouchers, may enable poor families to live in more racially and economically diverse areas. In principle, such neighborhood changes could improve the labor market outcomes of families through increased access to jobs, or by increased exposure to neighbors who support work, frown on welfare and provide useful job contacts (Kain, 1968; Wilson, 1987, 1995; Holzer, 1991; Raphael, 1998; Topa, 2001).

The present paper examines the initial impacts of a randomized housing-voucher experiment on the economic outcomes of public housing residents. In operation since 1994 in five cities (Baltimore, Boston, Chicago, Los Angeles and New York), the U.S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) demonstration assigns low-income families living in public or Section 8 project-based housing within high-poverty neighborhoods into one of three different "treatment groups": *Experimental group* families receive housing vouchers that can only be redeemed in low-poverty census tracts (with poverty rates under 10%), as well as counseling and search assistance from a local nonprofit; *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 the program. The randomized experimental design of the program enables us to identify the causal effects of offering families the chance to move to new neighborhoods, combined in the case of the experimental group with counseling assistance and relocation constraints. In what follows, we use state administrative records to examine the effects of the MTO program on the welfare receipt, employment and earnings of the 638 families who participated in the Baltimore site.

Our analysis suggests that the rate of welfare receipt among families assigned to the experimental group is between 5 and 7 percentage points (11–16%) lower than that of the control group. While we do not observe similar changes in quarterly employment or earnings in data from Maryland's Unemployment Insurance (UI) system, we cannot rule out the possibility that welfare receipt declined among the experimental group because labor market outcomes improved. Many of the jobs into which welfare leavers move in Maryland are apparently not covered by the state UI system, and the 95% confidence interval around our UI employment estimates is not inconsistent with the treatment effects suggested by the welfare data. On the other hand, we find little evidence of a program effect on any of these outcomes beyond the first postprogram year for the Section 8-only comparison group.

We hasten to add that because MTO families are a self-selected group of public housing residents, our findings may not generalize to other populations of public housing residents or low-income families more generally. The findings reported here are most relevant to

[1] For example, in a February 2001 survey, 75% of respondents support the idea of spending more for housing for poor people. http://www.publicagenda.org (accessed on May 10, 2002).

housing voucher programs that are voluntary and focus on public housing residents within the highest poverty neighborhoods of American cities—useful evidence for policy purposes given growing concern about the nation's most distressed housing projects.

The paper is organized as follows. The next section describes the MTO experiment in greater detail. The data used in our study are discussed in Section 3, while Section 4 reviews the conceptual framework for the analysis. Section 5 presents empirical results for the mobility outcomes of MTO families, as well as welfare use and quarterly employment and earnings. Section 6 discusses the findings.

2. The moving to opportunity demonstration

Whether the use of tenant- versus project-based housing subsidies will impact the economic outcomes of low-income families depends largely on the existence of "neighborhood effects", about which the empirical evidence to date is somewhat mixed.[2] However, earlier studies on this point are susceptible to a basic identification problem that stems from the fact that most families have at least some degree of choice over where they live. Differences in labor market outcomes across areas may thus reflect the causal effects of either neighborhoods or unmeasured family attributes that influence both economic and residential outcomes (Evans et al., 1992). The magnitude and even direction of bias that results from residential self-selection is unclear.[3]

The best available evidence to date comes from the quasi-experimental Gautreaux program in Chicago, which offered public housing residents the chance to move to new private-market apartments that were identified by a local nonprofit. Most families reportedly accepted the first apartment they were offered once they rose to the top of the Gautreaux waiting list; as a result, the program may generate random variation in neighborhood conditions. Follow-up data on families suggest that suburban movers have higher employment rates, but not hourly wages, compared with those who moved to other parts of the city (Rosenbaum, 1991).

While there necessarily remains some question about whether neighborhood assignments under Gautreaux were in fact random, these findings were sufficient to motivate HUD to sponsor a formal randomized experiment known as Moving to Opportunity (MTO). Eligibility for MTO was limited to low-income families with children who were living in

[2] On the effects of transportation access or distance to suburban job opportunities, see Raphael (1998) and Weinberg (2000) versus Ellwood (1986), and reviews by Jencks and Mayer (1990) and Holzer (1991). Studies of the effects of neighborhood socioeconomic composition yield somewhat stronger results; see Osterman (1991), Vartanian (1992) and Ellen and Turner (1997).

[3] If the most motivated families are the ones who manage to move from high- to low-poverty areas, unmeasured variables may lead us to overstate the costs of living in a distressed community. On the other hand, it may be that those parents who are most confident in their ability to navigate the perils of high-poverty communities are the most likely to take advantage of the lower housing costs or shorter commuting times sometimes associated with high-poverty urban areas. Some analysts believe that this identification problem can be overcome by examining the effects of neighborhood characteristics on teenage labor market outcomes, because adolescents typically have little say over a family's decision about where to live. Yet, at least within the MTO demonstration, family-level attributes that affect mobility outcomes also appear to be relevant for adolescent behavior (Katz et al., 2001; Ludwig et al., 2001a).

public housing or Section 8 project-based housing located within selected high-poverty census tracts. In the Baltimore site, the average poverty rate in 1990 in these baseline tracts was 67% (Goering et al., 1996); less than 5% of households in these tracts had annual incomes of US$50,000 or more (in 1990 dollars), and less than 7% of adults had a college degree.

The MTO program was publicized in the baseline tracts by the Housing Authority of Baltimore City (HABC) and a local nonprofit, the Community Assistance Network (CAN). The 638 families who volunteered for the program in Baltimore were added to the MTO waiting list, then drawn off over time through a random lottery and randomly assigned by Abt Associates into one of the three MTO treatment groups. Nearly half of the Baltimore MTO families were randomly assigned in the fourth quarter of 1994; another 20% were assigned in 1995, and the rest, by the end of 1996. Families in both the experimental and Section 8-only groups had up to 180 days from the time at which they officially begin the housing search to identify a suitable rental unit and sign a lease. Three-quarters of those who moved within the experimental group did so within a year of random assignment, while three-quarters of Section 8-only movers did so within 6 months of randomization.[4]

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 (Feins et al., 1997). Before the housing search was initiated, CAN required experimental families to complete seven group workshops that took 12–15 hours in total, covering topics related to home management, budgeting, leasing private-market apartments, and tenant–landlord relations.[5] CAN also referred members of the experimental group to job search and other local employment and training services, and may have increased awareness of benefits available through the Earned Income Tax Credit (EITC). Members of the Section 8-only and control groups in principle had access to many of these same services,[6] but may not have been aware of them. Differences in the utilization of job training, job search assistance or the EITC thus provides another possible mechanism beyond moving through which MTO may affect economic outcomes (Gueron and Pauly, 1991; Scholz, 1994; Bloom and Michalopoulos, 2001; Meyer and Rosenbaum, 2001; Gueron and Hamilton, 2002).

Movers in both the experimental and Section 8-only groups were required to sign leases for 1 year. Those who wished to move again before the initial lease expiration date were not eligible for a new Section 8 subsidy, while families who wished to relocate with their subsidy after the first year were able to do so without restriction. That is, MTO requires experimental-group movers to live in low-poverty areas for only the first postmove year.

[4] The actual time between random assignment and relocation may be somewhat longer than 180 days because of lags between randomization and the issuance of the rental subsidy. For the experimental-group relocators, one-quarter moved within one-half year of random assignment, half moved within 9 months, and all have moved within 2 years. For relocators within the Section 8-only group, half moved within 4 months of assignment and all moved within 1 year.

[5] Experimental-group families were also offered three optional workshops, one that helped experimental-group families prepare for interviews with landlords, one on lawn and garden maintenance, and an optional program for children on conflict resolution and other issues that may help them to adjust in their new community.

[6] Not all of the services were available to members of the other MTO groups. For example CAN itself operates employment counseling services that include job search assistance and training that are typically only available to families living in Baltimore County, but were made available to Baltimore City residents who were assigned to the MTO experimental group.

CAN contacted experimental families, on average, around six times following relocation (Feins et al., 1997). Most of these contacts were over the telephone, intended primarily to ensure that families were adjusting to their new neighborhoods. Otherwise, postprogram monitoring was limited. (For additional details, see Goering et al., 1996; Feins et al., 1997; Goering and Feins, 2003).

3. Data

One source of detailed socio-demographic information on MTO participants comes from the baseline surveys that families were required to complete when applying to the program. Follow-up tracking data from Abt Associates provide information on postprogram addresses and household composition through the end of 1997. Our main outcome measures come from Maryland administrative data for the state's public assistance (PA) and unemployment insurance (UI) systems, which require a bit more explanation.

3.1. Public assistance

The Maryland Department of Human Resources (DHR) maintains administrative records on receipt of PA cash benefits by residents of the state of Maryland, including the start and end date of every PA spell and the reason why spells were ended. 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 (minimum=1.9 years, maximum=3.8 years). In cases where no match was found, DHR searched again using the MTO participant's date of birth and completed the match using first and last name.

The estimated impact of the MTO program will be proportional to the match rate that results from this process. To see this, 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 (assumed to be uniform across state residents). As seen in Eq. (1), the estimated impact of MTO on welfare receipt equals M times the true effect. Fortunately, comparisons of self-reported welfare receipt on the baseline surveys with the state administrative data suggest that the match rate is quite high, on the order of 80–90%.[7]

$$\text{Estimated Impact} = [(M)W_E + (1-M)0] - [(M)W_C + (1-M)0] = M(W_E - W_C) \tag{1}$$

One additional complication comes from the fact that during our sample period, each Maryland county transitioned from the state's old welfare data system (AIMS) to a new

[7] A full 98% 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%. 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% 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.

data system (CARES). These transitions introduce two types of errors into the data. First, for reasons that remain unclear even to the DHR, welfare receipt rates appear to surge in the short run (or at least level off from their previous downward trend) when counties transition from AIMS to CARES. Working in the opposite direction from this "CARES spike", some AIMS records are not in the appropriate format to be read into CARES during the automated transition process. These families may erroneously appear to leave welfare following the transition to the new CARES system.

Because of these problems, we do not use welfare data from CARES in our analyses. We focus our attention on the period up to either the fourth quarter of 1996, when Baltimore County moved from AIMS to CARES, or the fourth quarter of 1997, when Baltimore City switched data systems.[8] A relatively small number of families (17 experimental group families and 9 in the Section 8-only group) live outside of Baltimore City or County in other counties that transition to CARES earlier than 96:4. In our analysis, we initially focus on the period through 96:4, and calculate upper and lower bounds on the program's effects by imputing welfare use among families living in counties that transition from AIMS to CARES during our sample period using the procedures in Manski (1989, 1990, 1995) and described below. We then replicate our analysis extending the data through 97:4, which requires that we also impute welfare use for those MTO families living in Baltimore County in late 1996 and 1997, after the county switched to the new CARES system.

3.2. 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. In Maryland, the UI system captures around 93% of all jobs in the formal labor market (Born, 1999).[9] The DLLR used social security numbers to match our MTO participant lists with UI earnings records covering the period from the second quarter of 1985 through the first quarter of 1999.[10] The UI data thus provide on average 3.8 years of postprogram information for MTO families for which there was a match (minimum=2.4 years, maximum=4.4 years). The match rate appears to be quite high—for MTO householders who report on the baseline survey that she has ever held a job for pay, the DLLR found a UI earnings history in every case.

[8] Different data sources within the Maryland DHR suggest slightly different dates on which Baltimore City transitions from the AIMS to CARES data system (10/97 versus 3/98). To be conservative, we use the former.

[9] Those who are exempt include the self-employed, government employees, railroad employees, those who work part-time at nonprofit groups, those who work for religious organizations, students who are employed by their schools, and most independent contractors (Kornfeld and Bloom, 1999).

[10] Prior to the first quarter of 1995, the UI system started each person's UI earning 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, because (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).

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, including jobs not covered by the UI sector or those located in nearby states. 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 surveys and UI records may 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 vary systematically across neighborhoods. Nevertheless, employment in a UI-covered job is an interesting outcome in its own right, because this is an important indicator of economic success for the MTO population.

4. Conceptual framework

A growing theoretical literature suggests that neighborhoods may affect economic outcomes through the behaviors, attitudes or characteristics of others who live within the same area, such as the ability of neighbors to provide job referrals (Wilson, 1987; Montgomery, 1991; Topa, 2001) or the degree of social support for work rather than welfare (Moffitt, 1983; Wilson, 1987, 1995). The attributes of the neighborhood itself, such as distance to suburban jobs or public transportation (Kain, 1968; Holzer, 1991; Holzer et al., 2001), may also be relevant. However, empirically testing the effects of such neighborhood attributes has been complicated by the endogenous sorting of families across areas.

The MTO demonstration overcomes this self-selection by randomly assigning families to different mobility treatment groups. The program enables us to identify the causal effects of the offer to move to a new, more affluent neighborhood combined with (in the case of the experimental group) mobility counseling and referrals to other services. This "intent to treat" (ITT) effect is of interest because offering housing vouchers to public housing residents (with or without complementary mobility counseling) is a prominent policy option for affecting the concentration of poverty within the United States. The ITT parameter is described more formally by Eq. (2), where we assume that there is only one treatment and one control group, with Z=1 indicating assignment to the treatment group and with individual and neighborhood subscripts suppressed for simplicity. (Generalizing the discussion to two treatment groups is straightforward.)[11]

$$\mathrm{ITT} = E[Y \mid Z = 1] - E[Y \mid Z = 0] \tag{2}$$

For our welfare analysis, we present a modified version of this ITT comparison that accounts for our decision to exclude information from Maryland's new CARES data system.

[11] Under our definition, families in the control group who relocate on their own or because their public housing projects were scheduled for demolition under HUD's Hope VI program are not counted as "treated", in part because the timing and conditions of these moves compared to those through MTO are different. Put differently, the ITT estimate reflects the effects of assignment to the experimental or Section 8-only groups in MTO compared to the counterfactual of what would have happened to families absent the MTO program.

This eliminates welfare data for families living in Baltimore County after December 1996 or Baltimore City after October 1997, and for the handful of families living in the other Maryland counties that adopted CARES in late 1993 or 1994. These data are clearly not missing at random, because those families who move out of Baltimore City during our observation period are drawn entirely from the experimental and Section 8-only groups.

We address the missing welfare data problem by calculating upper and lower bounds for the intent-to-treat effect using the methods discussed in Manski (1989, 1990, 1995). The bounds that we present below require no assumptions about the process that determines which observations are missing welfare data. As we demonstrate below, these "no assumption" bounds are informative in our application because the possible values for our outcome (welfare receipt) are bounded (binary), and the number of families for whom welfare data are missing is small. The upper and lower bounds for the ITT effect are given by Eqs. (3) and (4), respectively, where Y=1 if the family is on welfare and P represents the likelihood that a family in the treatment group lives in a jurisdiction that switches to CARES during the sample period. (Note that no control families move outside of Baltimore City or County during our study period, so P=0 for this group). The no-assumptions upper bound is thus derived by assuming that none of the treatment-group families for whom welfare data are missing are on welfare; the lower bound comes from assuming that all of these families receive welfare.

$$\text{Upper bound} = \{(1-P)\times E[Y\,|\,Z=1] + P\times[Y=0]\} - E[Y\,|\,Z=0] \quad (3)$$

$$\text{Lower bound} = \{(1-P)\times E[Y\,|\,Z=1] + P\times[Y=1]\} - E[Y\,|\,Z=0] \quad (4)$$

These ITT estimates are obtained in practice through regression Eq. (5), where the dependent variable is some economic outcome measure of interest for individual (i) in neighborhood (n) during postprogram quarter (t). The program impacts in this case are given by the estimate for α_1. The model includes a set of control variables, namely, a vector of preprogram indicator variables ($\boldsymbol{X}_{\text{in}}$) for welfare receipt during each of the eight quarters prior to random assignment, as well as 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. We also include a set of dummy variables for calendar quarter, δ_{t}. These conditioning variables will be close to orthogonal to the treatment-group indicator and will improve the precision of our estimates by accounting for additional variation in outcomes across individuals. The bounding procedure from Eqs. (3) and (4) is used to address the problem of person–quarters that are missing welfare data during the postprogram period because families live in counties that have switched to the CARES data system. Robust standard errors are calculated to adjust for the fact that individuals contribute multiple observations to the panel that may not be independent.

$$Y_{\text{int}} = \alpha_0 + \alpha_1 Z_{\text{in}} + \alpha_2 \boldsymbol{X}_{\text{in}} + \delta_{\text{t}} + R_{\text{in}} + V_{\text{int}} \quad (5)$$

Estimating Eq. (5) raises two final methodological issues. One complication arises from the fact that the Abt randomization algorithm, which assigns differential probabilities of assignment to each of the three treatment groups, changed on February 1, 1996. This change could in principle affect our results if economic outcomes systematically vary across MTO cohorts. To address this possibility, the regression model includes a set of indicator variables R_{in} for the quarter in which the family was randomly assigned.[12] For our descriptive statistics, we use the sampling proportions as weights, so that the weighted fraction of families from each cohort is equal across the three MTO treatment groups, and adjust the standard errors to account for heteroskedasticity.

The second methodological issue arises in calculating the marginal effects of assignment to the treatment group (Z_{in}) from our probit estimates for the parameters of Eq. (5). The standard marginal effect calculation (which is evaluated at the sample means of the covariates) is problematic in our application because most of the control variables in Eq. (5) are dichotomous, and so no one in the MTO program is at the sample means. We instead use our probit estimates to calculate the marginal effect of treatment-group assignment for the average program participant, given by Eq. (6) where a_0, a_1 and a_2 are probit estimates for the parameters α_0, α_1 and α_2 from Eq. (5) (see Chamberlain, 1984, p. 1274).

$$\text{Marginal Effect} = (1/N)\sum\left[\Phi(a_0 + a_1 + a_2X_{\mathrm{in}}) - \Phi(a_0 + a_2X_{\mathrm{in}})\right] \tag{6}$$

In cases where the ITT effects are nonzero, we may also be interested in the effects of the program on those who comply with their treatment assignments—that is, the ‘‘effects of treatment on the treated’’ (TOT). In large samples, the TOT effect should equal the ITT effect divided by the treatment group’s take-up rate if randomization is conducted properly, no control group families are ‘‘treated’’, and assignment to the experimental or Section 8-only groups has no effect on those who do not relocate through MTO (Bloom, 1984; Angrist et al., 1996). This last assumption may be controversial in our case because some nonmovers in the Section 8-only and experimental treatments were exposed to the counseling services offered to these groups. As a result, in what follows, we focus on the intent to treat effects of assigning families to the experimental or Section 8-only groups.

5. 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.

5.1. Characteristics of the MTO population

Table 1 presents descriptive information about MTO participants from the baseline surveys, calculated using weights that adjust for the change in the randomization algorithm in early 1996 (see above). Almost all of the householders in the Baltimore MTO demonstration are unmarried African–American women. Only around half of MTO householders have either a high school diploma or GED. 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.

[12] Within the first quarter of 1996, no families were randomized prior to February 1, 1996, so the indicator for this random-assignment quarter does not straddle the change in randomization procedures.

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 HS 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 (US$)	6876	6894	6679	6750
AFDC at baseline	80.3	79.3	81.6	80.4
AFDC ever	97.6	97.2	97.2	98.4
School or training at baseline	15.8	15.1	16.5	16.2
Has never worked	13.2	14.8	9.9	14.2
Worked all four quarters prior to enrolling in MTO	11.4	10.7	9.4	14.1
Work full/part-time baseline[a]	23.0	22.3	19.3	27.2
Tenure current job (weeks)	106.2	95.6	95.5	125.2
Hours worked per week	31.2	31.0	29.2	32.8
Wages per hour (US$)	5.98	5.59	6.68	5.95
Commuting (employed householders)				
Commute under 15 min	21.9	21.0	22.2	22.6
60 min or more	6.0	6.5	2.8	7.5
Commute by public transportation	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

[a] Includes respondents who work part-time and also attend school or training programs (between 1.2% and 2.5% of all respondents, or about one-tenth of the group that is working at the time of the baseline survey).

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% 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 6 months.

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	0.0	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
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	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

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 fails to reject the null hypothesis that the full set of means presented in Tables 1 and 2 are equal across the three MTO groups (p=0.75).[13]

5.2. Relocation outcomes

Relative to the experimental group, a larger proportion of Section 8-only families relocated through the MTO program in Baltimore (73% versus 58%). 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 noncompliers ran up against

[13] For the K baseline characteristics shown in Tables 1 and 2, we estimate a multivariate analysis of variance model in which each MTO family's ($K\times 1$) vector $\boldsymbol{Y}_\mathrm{i}$ is the outcome measure of interest, regressed against indicator variables for treatment-group assignment. The relevant F-test for the joint hypothesis that the two treatment-group indicator variables equal zero are based on Hotelling's generalized T^2 statistic (Hotelling, 1951, Johnson and Wichern, 1992).

Table 3
Relocation outcomes for MTO families

	Baseline (all families)	Experimental		Section 8-only		Control	
	1994–1996	Initial postprogram	As of 12/97	Initial postprogram	As of 12/97	Initial postprogram	As of 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
Mean census-tract characteristics							
% 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 ≥US$50,000	4.7	16.5	16.8	8.7	10.7	4.5	7.6

Neighborhood characteristics are calculated using 1990 Census data.

the Section 8 subsidy time limit. One-quarter of the experimental noncompliers did not 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 (Table 3). By design, (nearly) all of the experimental compliers move to low-poverty census tracts with 1990 poverty rates below 10%,[14] and around 40% of those experimental families who relocate through MTO move outside of Baltimore City. 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. In contrast to the experimental program-movers, only around 1 in 10 of the Section 8-only

[14] A small proportion of experimental relocators in Baltimore moved to census tracts with 1990 poverty rates slightly higher than 10%. 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.

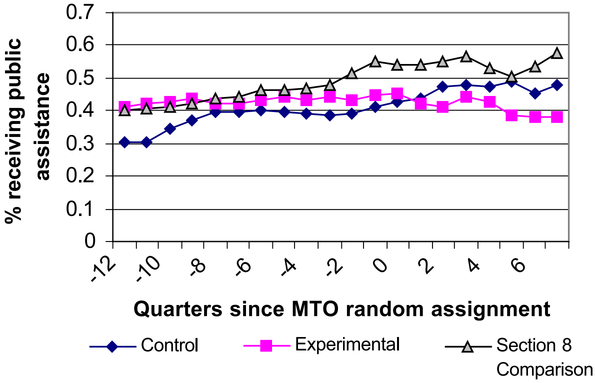

Fig. 1. Quarterly public-assistance receipt by MTO household heads (through 96:3).

compliers voluntarily moved to very low poverty census tracts (defined as those with rates less than 10%). While the Section 8-only group's moves are not quite as dramatic as those of the experimental group, the former nonetheless seem to experience substantial changes in their neighborhood environments.

5.3. 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. The results presented here initially focus on welfare receipt by MTO household heads and on the period that runs through 96:4, the point at which Baltimore County (the one suburban county that contains a nontrivial number of MTO families) transitions from the AIMS to CARES welfare data system.

Fig. 1 shows the average welfare receipt rates for each MTO group. For experimental and Section 8-only families whose welfare data are censored because they live in counties that have switched over to the state's new data system, we impute welfare receipt using the lower bound formula in Eq. (4), which will give us a downward biased estimate of the treatment effect. As seen in the figure, 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 postprogram period, with several of these differences significant at the 5% level. Fig. 1 also shows that the Section 8-only group has somewhat higher rates of welfare receipt than controls during the *pre*program period (also, see Appendix A). We believe that these preprogram differences are due to chance because we observe no systematic differences across treatment groups in preprogram *household-level* PA receipt,[15] or in the baseline

[15] When we focus on household-level welfare use (PA receipt by anyone within the household), the experimental group's welfare receipt rate is somewhat higher than that of the controls in the 11th and 9th quarters before random assignment, but there are no statistically significant differences across any of the three treatment groups during the eight quarters prior to randomization.

survey variables for MTO families in Baltimore (Tables 1 and 2) or Boston (Katz et al., 2001).

Our regression-adjusted ITT estimates (Table 4) reveal persistent differences in welfare receipt between the experimental and control groups in the quarters following random assignment. The upper and lower bounds suggest that on average, the proportion of experimental families on welfare during the postprogram period is between 5 and 7 percentage points lower than what is observed for the control group (p<0.10 and p<0.05, respectively). The table also presents at least suggestive evidence that this effect may grow over time, from between 5 and 6 percentage points during the first postprogram year to between 7 and 11 points during the second. While Section 8-only householders may be

Table 4
Regression-adjusted intent-to-treat (ITT) effects of MTO on welfare receipt through 96:3

Percent household heads receiving PA						
	N	Control	Experimental vs. control		Section 8-only vs. control	
		Mean	Lower bound	Upper bound	Lower bound	Upper bound
Quarters since assignment						
1	543	0.435	−0.034*	−0.037**	−0.014	−0.022
		(0.038)	(0.018)	(0.018)	(0.023)	(0.021)
2	528	0.472	−0.058**	−0.058**	−0.036	−0.057**
		(0.039)	(0.023)	(0.023)	(0.029)	(0.029)
3	423	0.481	−0.044	−0.059*	−0.035	−0.071**
		(0.044)	(0.031)	(0.032)	(0.038)	(0.035)
4	423	0.473	−0.052	−0.082**	−0.042	−0.077
		(0.044)	(0.040)	(0.040)	(0.050)	(0.048)
5	423	0.489	−0.102**	−0.142**	−0.067	−0.101*
		(0.044)	(0.043)	(0.042)	(0.055)	(0.054)
6	356	0.451	−0.042	−0.088	0.029	0.006
		(0.047)	(0.050)	(0.050)*	(0.067)	(0.066)
7	288	0.478	−0.067	−0.091	0.049	0.020
		(0.053)	(0.058)	(0.058)	(0.076)	(0.075)
Entire post		0.467	−0.052*	−0.073**	−0.020	−0.045
		(0.037)	(0.027)	(0.027)	(0.033)	(0.032)
Post quarters 1–4		0.464	−0.046**	−0.057**	−0.032	−0.054**
		(0.037)	(0.022)	(0.022)	(0.026)	(0.024)
Post quarters 5–8		0.473	−0.070	−0.108**	−0.002	−0.031
		(0.043)	(0.044)	(0.044)	(0.059)	(0.058)

Robust standard errors in parentheses. Upper and lower bound estimates use different methods for imputing welfare receipt for person–quarters where MTO participants are in counties that have transitioned from Maryland's old to new welfare data system (see text). Coefficients represent the average marginal effect (dp/dx) of assignment to the experimental and Section 8-only comparison groups on the probability of welfare receipt, derived from a probit model that conditions on householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, indicators for welfare receipt during each of the eight quarters before random assignment, taken from state administrative data, and indicator variables for the quarter in which the family was randomly assigned and the calendar quarter in which welfare use is measured.

* Difference significant at 10%.

** Difference significant at 5%.

somewhat less likely to receive welfare than controls during the first postprogram year, any difference between the groups dissipates by the second year.

Table 5 shows that extending the end of the sample period from 96:4 to 97:4 produces a qualitatively similar pattern of results across the MTO treatment groups. The main difference with Table 4 is that we are now imputing the welfare data for an additional group of families, those who live in Baltimore County following the county's switch to the new problematic data system. The result is to increase the spread somewhat between the upper and lower bounds for the treatment effects.

Table 5
Regression-adjusted intent-to-treat (ITT) effects of MTO on welfare receipt through 97:3

Percent household heads receiving PA						
	N	Control	Experimental vs. control		Section 8-only vs. control	
		Mean	Lower bound	Upper bound	Lower bound	Upper bound
Quarters since assignment						
1	638	0.439	−0.030*	−0.042**	−0.014	−0.017
		(0.035)	(0.016)	(0.018)	(0.020)	(0.021)
2	638	0.465	−0.050**	−0.062**	−0.044*	−0.061**
		(0.036)	(0.022)	(0.023)	(0.025)	(0.027)
3	638	0.460	−0.042	−0.070**	−0.023	−0.057*
		(0.036)	(0.028)	(0.028)	(0.031)	(0.031)
4	592	0.443	−0.039	−0.084**	−0.037	−0.074*
		(0.037)	(0.035)	(0.035)	(0.040)	(0.040)
5	543	0.470	−0.086**	−0.133**	−0.088*	−0.124**
		(0.039)	(0.039)	(0.038)	(0.045)	(0.044)
6	528	0.429	−0.036	−0.081*	0.012	−0.031
		(0.039)	(0.042)	(0.042)	(0.052)	(0.051)
7	423	0.443	−0.029	−0.077*	0.020	−0.006
		(0.044)	(0.047)	(0.047)	(0.062)	(0.061)
8	423	0.420	−0.018	−0.088*	0.094	0.054
		(0.043)	(0.048)	(0.048)	(0.064)	(0.063)
9	423	0.435	−0.039	−0.110**	0.052	0.013
		(0.043)	(0.050)	(0.050)	(0.065)	(0.064)
10	357	0.425	−0.027	−0.111**	0.061	0.031
		(0.047)	(0.056)	(0.056)	(0.073)	(0.073)
11	288	0.456	−0.043	−0.094	0.022	−0.008
		(0.053)	(0.064)	(0.064)	(0.082)	(0.082)
Entire post		0.445	−0.037	−0.082**	−0.005	−0.036
		(0.031)	(0.027)	(0.027)	(0.032)	(0.031)
Post quarters 1–4		0.452	−0.041**	−0.065**	−0.030	−0.052**
		(0.033)	(0.020)	(0.021)	(0.023)	(0.024)
Post quarters 5–8		0.442	−0.044	−0.096**	−0.001	−0.037
		(0.037)	(0.039)	(0.039)	(0.048)	(0.047)
Post quarters 9+		0.437	−0.035	−0.105**	0.049	0.014
		(0.043)	(0.050)	(0.050)	(0.065)	(0.064)

Robust standard errors in parentheses, coefficients represent marginal effects (dp/dx). For additional details about the estimation, see Table 4 legend.

* Difference significant at 10%.

** Difference significant at 5%.

The decline in welfare receipt by the experimental group of between 5 and 7 points suggested by Table 4 is consistent with a number of possible changes in the *dynamics* of welfare receipt. For example, the upper bound estimate might imply that 7% 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 postprogram period. Alternatively, 14% of experimental families may have been on welfare the entire time but now alternate between quarters of welfare use and nonuse, or 14% 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.

Table 6 uses welfare data through 96:4 to compare the fraction of postprogram quarters spent on welfare (as in Duncan, 1984), and suggests that the welfare effect on the

Table 6
ITT effects on duration of public assistance receipt through 96:3

	Control	Experimental vs. control		Section 8-only vs. control	
	Mean	Lower bound	Upper bound	Lower bound	Upper bound
Entire postprogram period					
Average fraction of quarters on welfare	0.450 (0.035)	−0.047* (0.026)	−0.070** (0.027)	−0.019 (0.032)	−0.039 (0.031)
On PA all quarters	0.339 (0.037)	−0.038 (0.029)	−0.065** (0.030)	−0.002 (0.032)	−0.022 (0.033)
≥3/4 Quarters on PA	0.381 (0.038)	−0.062** (0.030)	−0.088** (0.031)	−0.030 (0.033)	−0.050 (0.033)
≥2/4 Quarters on PA	0.452 (0.039)	−0.052 (0.033)	−0.079** (0.032)	−0.051 (0.037)	−0.068** (0.035)
≥1/4 Quarters on PA	0.524 (0.039)	−0.051* (0.031)	−0.069** (0.030)	−0.016 (0.039)	−0.030 (0.038)
On PA no quarters	0.446 (0.038)	0.034 (0.033)	0.044 (0.033)	0.006 (0.042)	0.020 (0.041)
Following first year after randomization					
Average fraction of quarters on welfare	0.458 (0.042)	−0.056 (0.045)	−0.098** (0.044)	−0.014 (0.056)	−0.043 (0.055)
On PA all quarters	0.412 (0.043)	−0.036 (0.044)	−0.083* (0.044)	−0.019 (0.055)	−0.050 (0.052)
≥3/4 Quarters on PA	0.412 (0.043)	−0.036 (0.044)	−0.083* (0.044)	−0.019 (0.055)	−0.050 (0.052)
≥2/4 Quarters on PA	0.466 (0.044)	−0.058 (0.045)	−0.101** (0.045)	−0.003 (0.058)	−0.030 (0.057)
≥1/4 Quarters on PA	0.511 (0.044)	−0.080* (0.046)	−0.119** (0.046)	−0.031 (0.060)	−0.060 (0.058)
On PA no quarters	0.489 (0.044)	0.080* (0.046)	0.119** (0.046)	0.031 (0.060)	0.060 (0.058)

Coefficients other than "average fraction of quarters on welfare" represent marginal effects (dp/dx); robust standard errors in parentheses. For additional details about estimation, see Table 4 legend.

* Difference in averages across groups statistically significant at the 10% level.

** Statistically significant at the 5% level.

Table 7
Effects of MTO on welfare exits by reason for exit, through 96:3

	Control	Experimental vs. control		Section 8-only vs. control	
	Mean	Lower bound	Upper bound	Lower bound	Upper bound
Fraction who report a welfare exit for following reasons					
Participant request	0.006	0.059**	0.094**	0.030**	0.045**
	(0.006)	(0.017)	(0.021)	(0.014)	(0.018)
Failure to reregister	0.091	−0.011	0.029	0.007	0.019
	(0.022)	(0.032)	(0.034)	(0.040)	(0.041)
Welfare-to-work	0.133	−0.019	0.022	−0.036	−0.021
	(0.026)	(0.031)	(0.034)	(0.035)	(0.038)
Transfer to new department	0	0.055**	0.077**	0.036*	0.040**
		(0.016)	(0.017)	(0.021)	(0.018)

Coefficients in top panel represent marginal effects (dp/dx); robust standard errors in parentheses. Welfare-to-work exits defined using closure codes from both the standard Maryland AIMS welfare data system and the WOMIS system that provides details about welfare-to-work transitions. For additional details about estimation, see Table 4 legend.

* Difference across treatment groups is statistically significant at the 10% level.

** Statistically significant at the 5% level.

experimental group arises largely from recipients who exit and stay off welfare. To distinguish between "welfare cycling" and short-term welfare receipt prior to moving through MTO, we also replicate our analysis starting 1 year after random assignment. Much of the decline in welfare use among the experimental group appears to be driven by an increase in the proportion of families who would have been on welfare in at least half of all quarters but now do not receive welfare at all.[16]

The pattern is somewhat different for the Section 8-only group, for whom the main effect of the program seems to be to hasten welfare exits that would have occurred anyway (Table 6). This can be seen more clearly by focusing on 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 no statistically significant changes in the likelihood of entering or exiting welfare thereafter. In contrast, families in the Section 8-only group are more likely than controls to exit during the first postprogram year, but less likely to exit during the second year.[17]

The welfare estimates presented here are robust to a variety of changes in model specification and definitions. For example, when we recalculate our results using welfare

[16] Note that the estimated levels and changes in the fraction of quarters spent on welfare shown in Table 6 do not line up perfectly with the estimates from Table 4 for the fraction of families on welfare in a given quarter. One reason is that the calculations in Table 4 come from using a panel of person-quarter observations, while those in Table 6 rely on a cross-section regression on the fraction of postprogram quarters on welfare where each program participant contributes only one observation.

[17] For the lower bounds on these transition probabilities, we assume that none of the experimental and Section 8-only families living in counties that had switched to the CARES data system exit welfare. For the upper bounds we assume that these families make a welfare exit every other quarter.

receipt by *households* (rather than household heads) we obtain qualitatively similar results. The marginal effects and standard errors from a linear probability model are nearly identical to those from our probit analysis. All of our results are also generally robust to estimating the program impacts using the sampling weights in addition to the indicators for quarter of random assignment to control for changes in the randomization algorithm, or to dropping the controls for preprogram characteristics.

5.4. Earnings and employment

Are the declines in welfare receipt among the MTO experimental group in Baltimore driven by improved labor market outcomes or by something else? One source of

Table 8
Regression-adjusted intent-to-treat (ITT) effects of MTO program on quarterly employment rates (UI Data)

Percent household heads employed	Control mean	Experimental vs. control	Section 8-only vs. control
Quarters since random assignment			
1	0.354 (0.034)	−0.027 (0.035)	−0.044 (0.038)
2	0.384 (0.035)	0.002 (0.039)	−0.017 (0.042)
3	0.404 (0.035)	0.031 (0.038)	−0.025 (0.042)
4	0.414 (0.035)	0.033 (0.040)	−0.021 (0.044)
5	0.454 (0.035)	−0.031 (0.040)	−0.073 (0.044)
6	0.474 (0.037)	0.013 (0.040)	−0.055 (0.044)
7	0.472 (0.037)	−0.012 (0.040)	−0.074* (0.044)
8	0.495 (0.036)	−0.043 (0.042)	−0.069 (0.046)
9	0.511 (0.037)	−0.086** (0.041)	−0.092** (0.043)
10	0.503 (0.037)	−0.035 (0.046)	−0.034 (0.052)
11	0.470 (0.039)	−0.020 (0.046)	0.045 (0.054)
12	0.464 (0.041)	−0.021 (0.045)	0.026 (0.054)
13	0.500 (0.046)	0.004 (0.048)	0.001 (0.063)
14	0.462 (0.045)	0.002 (0.039)	−0.012 (0.051)
15	0.519 (0.044)	0.013 (0.053)	−0.064 (0.067)
16	0.513 (0.047)	0.027 (0.056)	0.039 (0.071)
17	0.478 (0.053)	0.055 (0.065)	0.011 (0.083)
Entire postprogram	0.460 (0.025)	−0.010 (0.026)	−0.032 (0.030)
Postprogram quarters 1–4	0.389 (0.030)	0.009 (0.030)	−0.027 (0.033)
Postprogram quarters 5–8	0.474 (0.030)	−0.018 (0.033)	−0.068* (0.038)
Postprogram quarters 9–12	0.489 (0.033)	−0.045 (0.039)	−0.020 (0.043)
Postprogram quarters 13–17	0.491 (0.033)	0.015 (0.041)	−0.005 (0.053)

Robust standard errors in parentheses. Coefficients represent the average marginal effect (dp/dx) of assignment to the experimental and Section 8-only comparison groups on the probability of employment, derived from a probit model that conditions on householder age, gender, educational attainment (indicators for high school diploma, and for GED), marital status, number of children, all taken from the baseline surveys, indicators for employment during each of the eight quarters before random assignment, taken from state administrative data, and indicator variables for the quarter in which the family was randomly assigned and the calendar quarter in which earnings are measured.

* Difference significant at 10%.

** Difference significant at 5%.

information about this question comes from the welfare data, which include closure codes that explain why families exited the state PA system. Table 7 shows that there are no statistically significant differences across groups in official welfare-to-work exits. However, compared with the control group, families assigned to the experimental group experience an increase in welfare cases that are closed "at the client's request". This is true when measured either in terms of absolute frequency (as shown in Table 7) or as a proportion of all welfare exits experienced by each MTO group (not shown). These types of welfare exits are at least consistent with an improvement in the economic well-being of families, as suggested by welfare recidivism rates among those who request that their welfare cases be closed that are more similar to those who exit because of official welfare-to-work transitions than to those who exit for other reasons (Born, 1999, p. 52).

On the other hand, Tables 8 and 9 reveal little systematic difference across treatment groups in quarterly employment rates in UI-covered jobs (Table 8) or quarterly earnings in such jobs, where those who are not employed are assigned earnings of US$0 (Table 9).

Table 9
Regression-adjusted intent-to-treat (ITT) effects of MTO program on quarterly earnings (UI Data)

Effect on earnings of household heads (US$)			
	Control mean	Experimental vs. control	Section 8-only vs. control
Quarters since random assignment			
1	788 (105)	−106 (76)	−137* (81)
2	781 (102)	27 (93)	−5 (101)
3	921 (126)	22 (107)	42 (116)
4	883 (104)	54 (108)	17 (123)
5	1008 (110)	−35 (114)	−72 (122)
6	1056 (116)	85 (119)	−153 (123)
7	1062 (122)	69 (134)	−114 (155)
8	1272 (143)	−132 (142)	−130 (167)
9	1318 (134)	−194 (153)	49 (209)
10	1318 (127)	−23 (159)	−170 (175)
11	1339 (140)	−52 (163)	−116 (196)
12	1162 (132)	−16 (153)	112 (187)
13	1248 (169)	−2 (150)	115 (202)
14	1060 (144)	166 (127)	321* (189)
15	1234 (150)	122 (179)	172 (247)
16	1546 (217)	−140 (256)	109 (349)
17	1189 (186)	57 (230)	354 (329)
Entire postprogram	1109 (92)	−8 (91)	3 (105)
Postprogram quarters 1–4	843 (96)	−1 (78)	−21 (86)
Postprogram quarters 5–8	1099 (107)	−4 (108)	−119 (120)
Postprogram quarters 9–12	1286 (118)	−75 (138)	−15 (166)
Postprogram quarters 13–17	1227 (137)	57 (151)	222 (223)

Robust standard errors in parentheses. Respondents who are not working are assigned quarterly earnings of US$0. Figures reported in 1997 constant dollars. For additional details about estimation see Table 8 legend.

* Difference significant at 10%.

Our regression-adjusted ITT estimates are qualitatively similar when we focus on the natural logarithm of quarterly earnings for those who are employed in a given quarter, use the household rather than the household head as the unit of analysis, restrict our analytic sample to particular population subgroups,[18] or focus on differences across groups in employment transitions or job-to-job changes. 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.[19]

6. Discussion

The findings reported here suggest that assignment to the MTO experimental group in Baltimore increases the chances that families exit and stay off welfare. On the other hand, we observe no systematic changes in economic outcomes for families assigned to the MTO Section 8-only comparison group. What processes are responsible for these observed changes?

One explanation is that the change in welfare receipt rates across groups is spurious, an artifact of the Maryland welfare data themselves. We do not think that this is likely. One data concern stems from the possibility of moves out of state by members of the MTO experimental group, where welfare spells would not be captured by Maryland administrative records. Yet, fewer than 2% of families appear to have left the state through the end of 1997, with the result that upper and lower bounds that adjust for such moves are quite close to the estimates shown above.

A variant of this argument is that somehow, the welfare cases of experimental group families are "lost" as they move across counties within Maryland. This hypothesis receives some superficial support from Table 7, which shows a difference between the experimental and control groups in the frequency with which welfare cases are closed due to transfers to other local welfare agencies. However, when we exclude all MTO families who experience a welfare exit because of such a transfer, we estimate an

[18] 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 has one child under 6 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.

[19] 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 US$1000, $2000, $3000, $4000, $5000, and $6000 per quarter), and found no difference across treatment groups in the proportion of householders with earnings above these cutoffs.

experimental group ITT effect on welfare use that is quite similar to what is shown in Table 4.

An alternative explanation is that the decline in welfare use among the experimental group is real and driven by variation across neighborhoods in the accessibility of welfare offices or the stringency with which such offices enforce state regulations. However, this type of explanation is not consistent with our findings in Table 7, which show that welfare exits caused by the recipient's failure to reestablish eligibility are not an important part of the reduction in welfare receipt by the experimental group. A second test is to recalculate our estimates excluding those families who move out of Baltimore City. This Baltimore-only sample yields estimates for the experimental ITT effect on welfare receipt that are quite close to those shown in Table 4.

The reduction in welfare receipt among the Baltimore MTO experimental group is presumably due to some combination of changes in cohabitation rates, welfare stigma, or income. Unfortunately we have little direct evidence on either of the first two possibilities,[20] and the available data do not allow us to make any definitive statements about the last mechanism.

At first glance, our findings from the Maryland UI data might seem to rule out a change in labor market earnings as an explanation for the observed declines in welfare receipt among experimental-group families. But only around half of all welfare leavers in Maryland overall (and only 70% of those who leave welfare because their incomes are now too high) work in a UI-covered job in the quarter following their welfare exit (Born, 1999). The confidence intervals around our estimates for the effects of MTO on UI-covered employment (Table 8) is not inconsistent with the idea that half to three-quarters of experimental-group families who leave welfare (Table 4) move into jobs within the UI-covered sector.

Suggestive hints that changes in labor market earnings might play some role in explaining the experimental group's additional welfare exits come from follow-up surveys of a subset of experimental and Section 8-only group movers.[21] 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?" A majority of experimental program-movers report that job and training opportunities are better in the new versus old neighborhoods (66% and 63%, respectively). By comparison, smaller proportions of movers in the Section 8-only comparison group report better job and training opportunities in their new neighborhoods (53% and 42%), consistent with our finding that the Section 8-

[20] While Abt Associates did conduct a follow-up telephone canvass of MTO families, which revealed no statistically significant differences in cohabitation or marriage rates, under the eligibility and benefit rules for many social programs, household heads may have financial incentives to misreport household composition.

[21] Helen Ladd and Jens Ludwig surveyed 121 of the 143 experimental-group families (85%) 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%) who had been randomly assigned by our cutoff date. We do not draw more heavily on these follow-up surveys in this paper because of the somewhat modest response rates, particularly among experimental noncompliers and control group families.

only group does not experience a decline in welfare use or welfare-to-work transitions compared to controls.

Evidence from the other MTO sites reveals changes that we expect to be associated with labor market success, including improvements in the mental and physical health of MTO adults and children, although not in labor market outcomes themselves (Katz et al., 2001; Leventhal and Brooks-Gunn, 2002; Goering et al., 2002). The notion that employment plays a role in explaining the declines in welfare receipt reported here would also provide an explanation for why we observe changes in economic outcomes for Baltimore but not the other MTO demonstration sites: namely, that suburbs accounted for a larger share of job growth during the mid-1990s in the Baltimore metropolitan statistical area than in the other MTO cities.[22] In any case, the findings reported here, together with previous studies, suggest that assignment to the MTO experimental group (and for some outcomes, the Section 8-only group as well) improve the well-being of low-income families along a number of dimensions.

Do these benefits justify the costs of offering public housing residents housing vouchers? If the per-unit costs of housing vouchers are in fact no greater than those associated with project-based programs (HUD, 2000; Olsen, 2000; Shroder and Reiger, 2000; GAO, 2001), then the financial costs of the MTO experimental treatment come largely from the counseling services offered to this group, which are equal to around US$3000 per assigned family (Goering et al., 1999). While the welfare changes reported here are simply transfers, reductions in violent teen behavior and improved educational achievement yield benefits to society on the order of US$22,900 for each family assigned to the experimental group and US$20,600 for the Section 8-only group (Ludwig et al., 2001a,b; Johnson et al., 2002). This calculation ignores other benefits to society from improvements in the mental and physical health of parents and children, and the greater satisfaction families feel with their new housing and neighborhoods (Katz et al., 2001; Leventhal and Brooks-Gunn, 2001).

However, it is important to note that larger-scale efforts to shift from project-based housing programs to housing vouchers may yield different costs and benefits. Because the MTO participants are a self-selected group of families who volunteered to move, the benefits from moving a more representative sample of public housing residents may be more modest. Moreover, if MTO improves the outcomes of program participants by changing the set of people with whom they associate, then we cannot rule out the possibility of some offsetting effects on families who live in the baseline or destination neighborhoods. These types of general-equilibrium effects of shifting from project- to tenant-based housing subsidies and moving low-income families are currently not understood, and remain an important priority for future research.

[22] From 1993 to 1996, the number of workers in the city of Baltimore declined by 3.6% while that in the suburbs increased by 10%, for a city–suburb difference equal to −13.6 percentage points. By comparison, the city–suburb differential in employment growth rates over this period equaled −8.6 percentage points in Chicago, −6.2 points in LA, +1.4 in Boston, and +1.1 in New York. The implication is that the fraction of total MSA employment that is in the central city declined more in Baltimore than in the other MTO cities (Brennan and Hill, 1999).

Acknowledgements

This research was supported by grants from the U.S. Department of Housing and Urban Development, the Georgetown University Graduate School of Arts and Sciences, and the Spencer, Andrew Mellon, William T. Grant and Smith Richardson foundations, and was written in part while the first author was visiting scholar at the Joint Center for Poverty Research and the Brookings Institution. Thanks to Rohit Burman, Judie Feins, John Janak, Debbi Magri-McInnis, John Peterson, Kerry Whitacre, Philip Walsh, Ruth Crystal and the CAN counselors for assistance in compiling the dataset. Thanks to John Cawley, Duncan Chaplin, Ruth Crystal, Todd Elder, John Goering, Julie Henly, Chris Jepsen, Jeff Kling, Helen Ladd, Richard Larson, Charles Manski, Susan Mayer, James Peterson, Steve Pischke, John Quigley, Dan Rosenbaum, Jim Rosenbaum, Matt Stagner, Adam Thomas, Celeste Watkins, Julie Wilson, and seminar participants at Berkeley, Northwestern, New York University, UNC, the annual meetings of the PAA and APPAM, and the Joint Center for Policy Research conference on Tax and Transfer Programs for Low-Income Families for helpful comments. The views expressed here are our own and do not represent positions of the Bureau of Labor Statistics or the U.S. Department of Labor.

Appendix A. Quarterly public-assistance receipt by MTO householders through 96:3

Fraction household heads receiving PA					
	Control	Experimental mean		Section 8-only mean	
	Mean	Lower bound	Upper bound	Lower bound	Upper bound
Quarters since randomization					
−12	0.304 (0.034)	0.410 (0.035)**	0.410 (0.035)**	0.400 (0.043)*	0.400 (0.043)*
−11	0.304 (0.034)	0.421 (0.035)**	0.421 (0.035)**	0.409 (0.043)*	0.409 (0.043)*
−10	0.343 (0.036)	0.425 (0.035)	0.425 (0.035)	0.413 (0.043)	0.413 (0.043)
−9	0.371 (0.036)	0.436 (0.035)	0.436 (0.035)	0.422 (0.043)	0.422 (0.043)
−8	0.394 (0.035)	0.424 (0.035)	0.424 (0.035)	0.437 (0.040)	0.437 (0.040)
−7	0.394 (0.035)	0.422 (0.035)	0.422 (0.035)	0.445 (0.040)	0.445 (0.040)
−6	0.404 (0.035)	0.433 (0.035)	0.433 (0.035)	0.462 (0.040)	0.462 (0.040)
−5	0.394 (0.035)	0.445 (0.035)	0.445 (0.035)	0.462 (0.040)	0.462 (0.040)
−4	0.389 (0.035)	0.432 (0.035)	0.432 (0.035)	0.469 (0.040)	0.469 (0.040)
−3	0.384 (0.035)	0.441 (0.035)	0.441 (0.035)	0.478 (0.040)*	0.478 (0.040)*
−2	0.389 (0.035)	0.433 (0.035)	0.433 (0.035)	0.517 (0.040)**	0.517 (0.040)**
−1	0.414 (0.035)	0.449 (0.035)	0.449 (0.035)	0.549 (0.040)**	0.549 (0.040)**
0	0.426 (0.037)	0.451 (0.035)	0.451 (0.035)	0.541 (0.043)**	0.541 (0.043)**
1	0.435 (0.038)	0.421 (0.035)	0.411 (0.035)	0.543 (0.046)*	0.534 (0.046)*
2	0.472 (0.039)	0.411 (0.035)	0.411 (0.035)	0.552 (0.046)	0.535 (0.047)
3	0.481 (0.044)	0.441 (0.034)	0.427 (0.034)	0.568 (0.055)	0.531 (0.056)
4	0.473 (0.044)	0.427 (0.034)	0.398 (0.034)	0.531 (0.056)	0.494 (0.056)
5	0.489 (0.044)	0.384 (0.034)*	0.346 (0.033)**	0.506 (0.056)	0.469 (0.056)
6	0.451 (0.047)	0.381 (0.037)	0.335 (0.036)**	0.537 (0.061)	0.507 (0.062)
7	0.478 (0.053)	0.382 (0.041)	0.354 (0.040)*	0.574 (0.068)	0.537 (0.068)

* Difference with control group significant at 10%. ** Difference with control group significant at 5%. For details on upper and lower bounds, see Table 4 and text.

Appendix B. Quarterly employment and earnings for MTO household heads

	Fraction household heads employed			Quarterly earnings for household heads (US$)		
	Experimental	S8-only	Control	Experimental	S8-only	Control
Quarters since random assignment						
−12	0.178 (0.027)*	0.193 (0.032)	0.247 (0.031)	393 (71)	346 (75)	512 (83)
−11	0.210 (0.029)	0.171 (0.031)*	0.252 (0.031)	402 (73)	387 (149)	582 (99)
−10	0.197 (0.028)	0.149 (0.027)**	0.242 (0.031)	383 (72)	248 (60)**	590 (105)
−9	0.189 (0.028)	0.168 (0.029)*	0.247 (0.031)	374 (72)**	237 (54)**	624 (103)
−8	0.223 (0.030)	0.164 (0.029)*	0.242 (0.031)	374 (69)*	265 (75)**	559 (88)
−7	0.208 (0.029)	0.170 (0.030)*	0.253 (0.031)	348 (71)*	298 (70)**	575 (92)
−6	0.213 (0.029)	0.175 (0.030)**	0.278 (0.032)	369 (71)**	287 (71)**	691 (107)
−5	0.200 (0.028)**	0.171 (0.029)**	0.293 (0.032)	430 (81)**	301 (75)**	751 (109)
−4	0.194 (0.028)**	0.176 (0.028)**	0.293 (0.032)	419 (76)	312 (72)**	618 (96)
−3	0.241 (0.030)	0.180 (0.030)*	0.257 (0.031)	473 (81)	365 (82)	466 (80)
−2	0.248 (0.031)	0.267 (0.034)	0.293 (0.032)	533 (84)	434 (80)	614 (97)
−1	0.284 (0.032)	0.233 (0.031)	0.293 (0.032)	616 (92)	449 (85)	595 (101)
0	0.265 (0.031)	0.234 (0.032)	0.303 (0.033)	621 (86)	455 (85)	668 (109)
1	0.302 (0.033)	0.238 (0.032)**	0.354 (0.034)	641 (92)	458 (80)**	788 (105)
2	0.351 (0.034)	0.306 (0.036)	0.384 (0.035)	752 (96)	563 (87)	781 (102)
3	0.387 (0.034)	0.314 (0.036)*	0.404 (0.035)	829 (101)	678 (103)	921 (126)
4	0.403 (0.034)	0.331 (0.036)	0.414 (0.035)	897 (104)	731 (116)	883 (104)
5	0.387 (0.034)	0.333 (0.037)**	0.454 (0.035)	918 (108)	740 (104)*	1008 (110)
6	0.486 (0.038)	0.369 (0.037)**	0.474 (0.037)	1180 (137)	773 (98)*	1056 (116)
7	0.459 (0.038)	0.368 (0.037)*	0.472 (0.037)	1223 (160)	872 (124)	1062 (122)
8	0.433 (0.036)	0.385 (0.038)**	0.495 (0.036)	1110 (139)	988 (125)	1272 (143)
9	0.406 (0.039)*	0.397 (0.037)**	0.511 (0.037)	1016 (122)*	1154 (181)	1318 (134)
10	0.454 (0.035)	0.433 (0.042)	0.503 (0.037)	1239 (139)	980 (122)*	1318 (127)
11	0.431 (0.035)	0.466 (0.046)	0.470 (0.039)	1221 (131)	1050 (132)	1339 (140)
12	0.409 (0.035)	0.439 (0.049)	0.464 (0.041)	1095 (115)	1109 (152)	1162 (132)
13	0.490 (0.036)	0.452 (0.059)	0.500 (0.046)	1144 (117)	1089 (183)	1248 (169)
14	0.449 (0.036)	0.410 (0.058)	0.462 (0.045)	1193 (131)	1195 (217)	1060 (144)
15	0.514 (0.035)	0.400 (0.055)*	0.519 (0.044)	1310 (125)	1211 (210)	1234 (150)
16	0.514 (0.038)	0.493 (0.062)	0.513 (0.047)	1370 (141)	1576 (275)	1546 (217)
17	0.528 (0.042)	0.463 (0.068)	0.478 (0.053)	1303 (145)	1484 (284)	1189 (186)

Earnings for those not working in a given quarter set equal to zero. * Difference with control group significant at 10%. ** Difference with control group significant at 5%. Earnings are reported in constant 1997 dollars.

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