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Urban Poverty and Educational Outcomes

BETWEEN 1970 AND 1990, the number of people in the United States living in high-poverty census tracts (with poverty rates of 40 percent or more) nearly doubled, from 4.1 to 8.0 million. Children who live in poor urban neighborhoods are disproportionately likely to be members of racial and ethnic minority groups and are also at greater risk for school failure. For example, only 11 percent of fourth graders attending high-poverty schools in Washington, D.C., scored at or above basic level on the government's National Assessment of Educational Progress (NAEP) math test, far lower than the national average of 62 percent. Dropout rates in Washington remain on the order of 30 to 40 percent, many times higher than the national average.¹

Why do high-poverty urban areas have such problems with schooling outcomes? Sociologists, psychologists, and a growing number of economists believe that the prevalence within a neighborhood of social problems such as poverty and joblessness affect the life chances of area residents. If so, policies

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1. Jargowsky (1997); U.S. Department of Education (1999, 2000); Valerie Strauss, "One-Third of District Students Drop Out; Rate Far Higher Than U.S. Average," *Washington Post*, September 23, 1999, p. B1.

that reduce the degree of economic residential segregation may also improve the educational outcomes of poor children. Given the persistent correlation between race and social class in America, policies that help reduce the degree of neighborhood racial segregation could potentially have the same effect.² Unfortunately, relatively little is currently known about the effects of neighborhood conditions on children's educational outcomes. The central challenge to measuring neighborhood effects stems from the fact that most families have at least some degree of choice over where they live. As a result, correlations between neighborhood characteristics and child outcomes may reflect either the causal effects of neighborhood environments or the effects of unmeasured family attributes that influence both residential choices and children's outcomes.

Ambiguity about even the direction of this "self-selection" or "endogenous-membership" bias makes interpretation of the nonexperimental literature difficult.³ On the one hand, those parents who are most concerned about their children's outcomes may take the initiative to relocate to a lower-poverty area. On the other hand, families whose children are predisposed toward trouble and more likely to succumb to the temptations of the street may be more likely to relocate to more affluent areas to shield their children from negative peer influences.

The best available evidence on the effects of neighborhoods on children's educational outcomes comes from the Gautreaux program in Chicago, which relocated African American public housing residents into different parts of the metropolitan area. Gautreaux families typically accepted the first apartment made available to them by the nonprofit group that administered the program, which suggests that participants had little choice over whether they ended up in a city or a suburban location. Evaluations have found that compared with those who moved to other parts of the city, suburban movers had lower dropout rates (5 versus 20 percent) and higher rates of college attendance (54 versus 21 percent).⁴ But because Gautreaux was not a true experiment, there necessarily remains some question about the randomness of neighborhood assignments.

2. Wilson (1987, 1996); Jencks and Mayer (1990); Brooks-Gunn, Duncan and Aber (1997a,b); Jaynes and Williams (1989, pp. 319–22); Yinger (1998).

3. Findings from the nonexperimental literature are generally mixed on whether neighborhood characteristics affect children's outcomes; for excellent summaries, see Jencks and Mayer (1990); Ellen and Turner (1997); and Brooks-Gunn, Duncan and Aber (1997a,b).

4. Rosenbaum (1995); Rubinowitz and Rosenbaum (2000).

In this paper we estimate the effects of neighborhood conditions on children's educational outcomes using data from the U.S. Department of Housing and Urban Development's (HUD) Moving to Opportunity (MTO) residential-mobility program. In contrast to Gautreaux, the MTO program is a true randomized experiment. MTO has been operating since 1994 in five cities (Baltimore, Boston, Chicago, Los Angeles, and New York). This paper uses data from the Baltimore site and is the first evaluation of the program's impacts on the educational outcomes of participating children.

Eligibility for the MTO program was restricted to low-income families with children living in public housing or Section 8 project-based housing located in selected high-poverty census tracts. Families who volunteered for MTO were randomly assigned into one of three treatment groups. Members of the *experimental group* were offered Section 8 rental subsidies that could be used only for private-market housing in census tracts with very low-poverty rates (defined as 1990 poverty rates below 10 percent). These families also received counseling services and assistance in their housing search from a local nonprofit agency. Members of the *Section 8-only comparison group* were also offered rental subsidies but were not required to move to a low-poverty census tract and were not provided any additional services. Members of the *control group* received no rental subsidies. The presumption was that they would continue to live within high-poverty areas.⁵ The randomized experimental design of MTO thus breaks the link between family preferences and neighborhood conditions and helps overcome the self-selection problem that plagues previous studies of neighborhood effects.

Our study measures children's educational outcomes using data obtained from administrative school records in Maryland. Specific outcome measures include student performance on standardized academic achievement tests, school absences, disciplinary actions, special education placements, grade retentions, and dropout rates. We find that elementary school children assigned to the experimental group achieved scores in both reading and math that exceeded those of the control group children by about one-quarter of a standard deviation. Younger children in the Section 8-only comparison group experience higher scores than controls in reading but not math. For adolescents, only limited data are available on academic achievement. While we find

5. School-level data on student exit and entry often suggest high rates of residential mobility by poor families, which raises the possibility that even the families within the control group may experience high rates of mobility out of the high-poverty baseline communities. But most of the residential mobility experienced by poor families appears to occur between or within high-poverty areas. Gramlich, Laren and Sealand (1992)

that teens in the experimental and Section 8-only groups experience a higher incidence of grade retention than controls, and may experience more disciplinary actions and school dropout as well, these differences appear to be due at least in part to differences in academic and behavioral standards between schools in high- and low-poverty areas. We hasten to add that since MTO participants are a self-selected group of public housing residents, the treatment effects estimated here may not generalize to more representative populations of low-income families.

The Moving to Opportunity Demonstration

Eligibility for the Baltimore MTO demonstration was restricted to very low-income families with children who lived in public housing or Section 8 project-based housing in one of the five poorest census tracts in Baltimore City. These tracts had an average poverty rate in 1990 of 67 percent and a crime rate that was nearly three times the state average. The baseline neighborhoods are also notable for a paucity of affluent neighbors, which previous research suggests has a distinct effect on youth outcomes from neighborhood poverty. Less than 5 percent of households in these tracts had annual incomes of \$50,000 or more (in 1990 dollars), and less than 7 percent of adults in these areas had a college degree.⁶

The program was publicized in the baseline tracts by the Housing Authority of Baltimore (HAB) and a local nonprofit, the Community Assistance Network (CAN). Although we are unable to determine exactly what fraction of eligible families volunteered for MTO in the Baltimore site, data pooled together from four of the five MTO sites (Boston, Baltimore, Los Angeles and New York) suggest that around one-quarter of eligible families applied to participate.⁷ Table 1 compares the sociodemographic characteristics of eligible families who lived in public housing in these four cities and volunteered for MTO with their apparently eligible neighbors who decided not to apply.

6. Goering, Carnevale and Teodoro (1996); Ludwig, Duncan and Hirschfield (2001); Brooks-Gunn, Duncan and Aber (1997a,b).

7. Goering and others (1999). This figure represents the estimated proportion of families with children living in public housing developments in the census tracts targeted by MTO program administrators. Because the proportion of families living in Section 8 project-based housing within the targeted census tracts who volunteer for MTO may be somewhat different from the rate among public housing families, this estimate may slightly over- or understate the actual proportion of all eligible families who volunteered for the program.

Table 1. Comparison of Move-to-Opportunity and Non-MTO Households in Same Public Housing Developments for Baltimore, Boston, Los Angeles, and New York Sites

<i>Item</i>	<i>MTO households</i>	<i>Non-MTO households</i>
N	2,414	6,813
African American (%)	54	51
Hispanic (%)	39	45
Female-headed household (%)	93	78
Household head age (years)		
Mean	35	41
Median	33	39
Number of children under 18 in household		
Mean	2.5	2.3
Median	2.0	2.0
Household head receives		
AFDC (%)	75	51
Household head employed (%)	22	30
Household income (\$)		
Mean	9,365	10,769
Median	8,252	8,645

Source: Goering and others (1999)

Household heads who volunteered for MTO are somewhat more likely than their neighbors to be female, young, and on AFDC. If these households have the most to gain from moving to more affluent areas, then the estimated effects of residential relocation for MTO participants may overstate the effects of involuntarily relocating a more representative population of public housing residents.

Volunteers for the MTO demonstration were added to the program's waiting list. Families were drawn off the MTO waiting list over time on the basis of a random lottery beginning in October 1994 and then randomized by Abt Associates into one of the three MTO treatment groups. The final group to be drawn off the waiting list was randomly assigned into a treatment group in October 1996.

Those families assigned to the experimental group were offered Section 8 housing vouchers or certificates, which provide subsidies to lease private-market housing. As part of the program's design, these subsidies could only be redeemed for housing in census tracts with 1990 poverty rates of less than 10 percent. Families had up to 180 days from the time at which they began the housing search to identify a suitable rental unit and sign a lease. Before the housing search was initiated, CAN required experimental families to complete

four workshops on topics such as budgeting, how to conduct a housing or job search, and conflict resolution. During the housing search itself CAN helped families locate suitable rental housing and negotiate leases with private-market landlords.

Of the experimental group families, 58 percent “complied” with their treatment assignment (that is, relocated through the MTO program). Of the experimental group “noncompliers,” half ran up against the Section 8 rent-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 to begin the counseling program and search process after being assigned to the experimental group.

Families assigned to the Section 8–only comparison group were also given the chance to relocate with Section 8 housing subsidies. The services offered to the Section 8–only group differed from those of the experimental treatment in ways that could in principle either increase or decrease compliance rates. As with the experimental group, Section 8–only families had 180 days to identify a private-market apartment and were required to sign a one-year lease. Yet unlike with the experimental group, the relocation decisions of Section 8–only families were not constrained by the MTO program, which may contribute to higher relocation rates among Section 8–only families. On the other hand, families in the Section 8–only group received no housing search or counseling assistance beyond what was provided to all participants in HUD’s Section 8 subsidy program (which in Baltimore included a brief orientation about the program rules, as well as access to information about private-market landlords who have accepted or are likely to accept housing vouchers). As it turns out, the relocation rate among the Section 8–only group in Baltimore was significantly higher than that observed among experimental families (73 versus 58 percent), a pattern that holds across MTO sites.⁸ 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 were then unable or unwilling to commit to a lease before the subsidy offer expired.

Conceptual Framework

A neighborhood’s social composition may influence children’s educational outcomes through a variety of mechanisms. Most social science theories sug-

8. Goering and others (1999).

gest that moving low-income children from high- to low-poverty neighborhoods will improve these children's schooling performance, although some theories suggest that such a move may be unhelpful or even harmful.

There are a number of reasons to believe that living in affluent neighborhoods filled with high-achieving students will have positive effects on the educational outcomes of poor children. Sociologists emphasize the role that neighbors and peers play in establishing local social norms regarding schooling and work, while economists emphasize the role that working- and middle-class neighbors play in demonstrating the returns to staying in school.⁹ It is also possible that the average achievement level of students within an area may affect an individual child's academic outcomes by raising the level of teacher expectations or classroom instruction, or through additional "human capital spillovers" that arise when children help one another with schoolwork. A very different type of neighborhood effect may arise from variation across areas in the quality of neighborhood schools and other local institutions. However, the negative correlation in the Baltimore area between the prevalence of affordable rental housing within a neighborhood and local school quality suggests that the effects of MTO on children's school quality may be only modest.¹⁰

Some sociologists believe that "neighborhoods matter" but do not agree that moving school-age children from high poverty to more affluent areas will substantially change their educational outcomes. According to this view, children who grow up in areas of high joblessness are instilled with a "culture of poverty" characterized by hopelessness and a critical attitude toward mainstream institutions, attitudes that, once developed, are not readily changed. As Oscar Lewis writes, "By the time slum children are age six or seven, they have usually absorbed the basic values and attitudes of their subculture and are not psychologically geared to take full advantage of the changing conditions or increased opportunities that may occur in their lifetime."¹¹ It is also possible that moves from high- to low-poverty neighborhoods might have negative consequences for children's school performance. For example, low-income children in affluent neighborhoods or schools may suffer in the heightened competition for grades and other rewards in the new environment, or they may revise their opinions of their own abilities downward in response to a higher-achieving peer group.¹²

9. Wilson (1987, 1996); Manski (1993); Ludwig (1999).

10. Ladd and Ludwig (1997).

11. Lewis (1968, p.188).

12. Jencks and Mayer (1990).

The random assignment of MTO families into different mobility treatment groups enables us to overcome the self-selection problem and examine whether neighborhood conditions are related to educational outcomes. But because MTO changes every neighborhood characteristic simultaneously for participating families, we are unable to identify the specific mechanisms through which neighborhoods influence children's schooling performance. The empirical analysis reported below thus uses the MTO data to estimate the combined net effect of changing all of a family's neighborhood characteristics at once.

Our empirical strategy focuses on comparing the average outcomes of families according to the MTO treatment group to which they were assigned, regardless of whether families took advantage of the program's offer to relocate. Since the background characteristics of families in each group should on average be quite similar by virtue of the program's random-assignment design, differences across groups in postprogram outcomes can be attributed to the effects of the MTO program. These "intent-to-treat" (ITT) estimates will understate the actual effects of relocating through MTO because the compliance rates of families assigned to the experimental and Section 8-only groups are less than 100 percent.¹³ The ITT effect, obtained from estimating regression equation 1, identifies the effects of offering families the opportunity to relocate through the MTO program, which are of interest to policymakers since many residential-mobility programs are likely to involve voluntary participation by families.

$$(1) \quad Y_{int} = \alpha_1 + \alpha_2 Z_{in0} + \alpha_3' X_{in0} + \alpha_4' \delta_t + \alpha_5' \lambda_{t'} + v_{int}.$$

Regression equation 1 is estimated using a panel of person-year observations for MTO children where (t) indexes academic years since random assignment ($t > 0$), and the educational outcome for child (i) living in neighborhood (n) in year (t) is given by Y_{int} . The key explanatory variable in the analysis, Z_{in0} , indicates whether families are assigned to the experimental ($Z_{in0} = 1$) or control group ($Z_{in0} = 0$). (The analysis is identical for the effects of the

13. Note that comparing the average outcomes of experimental or Section 8-only *movers* with the average outcomes of the control group as a whole will produce biased estimates for the effects of MTO relocation, because those who choose to move within the experimental and Section 8-only groups are a self-selected sub-group of families assigned into that group. Since the average background characteristics of movers within the experimental and Section 8-only groups will not equal those of the control group as a whole, this type of comparison will produce an estimate for the causal effects of MTO relocation that will be biased either upward or downward, depending on which types of families are more likely to relocate when assigned to the experimental and Section 8-only groups.

Section 8–only treatment.) The use of panel data allows us to control for common trends in academic outcomes over time by including a set of indicators for years since randomization (δ_t) as well as the actual academic calendar year (λ_t), thus improving the precision of our estimates. The model also controls for a set of preprogram child and family characteristics from the baseline surveys and preprogram educational outcomes from the school records (X_{i0}) to account for chance differences in these variables across groups.¹⁴ We calculate robust Huber-White standard errors that account for the panel structure of the data and the presence of multiple children from the same family.

Also of policy interest are the effects of the MTO program on those families who actually move. The effect of “treatment on the treated” (TOT) can be identified if several assumptions are met. First, the postprogram outcomes of experimental and Section 8–only group noncompliers must be identical to the outcomes these families would have experienced had they instead been assigned to the control group. This assumption seems uncontroversial for the Section 8–only noncompliers, who receive no real services from the MTO program. Whether this assumption is met for experimental noncompliers is less obvious since some of these families received the CAN counseling, although previous research suggests that even intensive youth counseling programs appear to have little impact on youth behavior.¹⁵ A second assumption is that 58 percent of families assigned to the control group would have relocated through MTO had they been assigned to the experimental group (which equals the observed compliance rate among those families actually assigned to the experimental group), while 73 percent of control families would have moved through MTO had they instead been assigned to the Section 8–only group. This assumption will be met if the MTO random assignment was carried out properly, which appears to be the case as shown below.¹⁶

14. Our pre-program controls include a series of indicator variables for the child’s age at random assignment, the child’s gender and number of siblings, as well as household-head baseline characteristics such as age at randomization, employment status (full time, part time, or not working), marital status (married, not married), welfare receipt, and educational attainment (high school diploma or more, GED, high school dropout), all taken from the baseline surveys that are available for all MTO households. We also use school administrative data to control for pre-program educational outcomes for the two years prior to randomization. These include controls for pre-program grade of enrollment, school absences, CTBS reading and math scores, special education status, and grade retention for the first and second academic years prior to random assignment. For missing data we set values to zero and include a missing-data indicator in order to avoid losing observations from the analytic sample.

15. Donohue and Siegelman (1998).

16. The TOT estimate also assumes that none of the families in the control group receive either the experimental or Section 8-only treatments. This assumption is met under our defi-

The *TOT* estimate for the experimental treatment (equation 2) essentially compares the outcomes of experimental and control group families who *would* comply with the experimental treatment ($C = 1$), with a similar *TOT* effect defined for the Section 8-only treatment. In practice we can identify the *TOT* effect with the assumptions outlined above, although we cannot identify the specific families within the control group who are “potential compliers”:

$$(2) \quad TOT = E[Y | Z = 1, C = 1] - E[Y | Z = 0, C = 1]$$

$$(3) \quad Y_{int} = \gamma_1 + \gamma_2 D_{in0} + \gamma_3' X_{in0} + \gamma_4' \delta_t + \gamma_5' \lambda_{t'} + \eta_{int}.$$

We derive the *TOT* estimates by applying two-stage least squares to equation 3, using each family’s MTO assignment (Z) as an instrument for a variable indicating whether the family actually relocated through the MTO program ($D = 1$ if so, and equal to 0 otherwise). In large samples this two-stage least squares estimate converges to the ITT effect divided by the probability of compliance with the assigned treatment.¹⁷

We can assess the magnitude of the *TOT* effect by comparing it with the average outcome of those children in the control group whose families would have moved had they been assigned to the experimental group, known as the “control complier mean” (*CCM*). Since we do not know which control families would have complied with the experimental treatment, we estimate the *CCM* as the average outcome of those who relocate through MTO in the experimental group minus the estimated *TOT* effect, as described in equation 4:¹⁸

$$(4) \quad CCM = E[Y | Z = 0, C = 1] = E[Y | Z = 1, C = 1] - TOT.$$

nition of the experimental and Section 8-only treatments as “relocation to subsidized private-market housing through the MTO program.” Control families who relocate on their own into private-market housing are different from those who receive the Section 8-only and experimental treatments because their private-market housing is not subsidized. Some control families may have received something close to the Section 8-only group’s treatment through HUD’s Hope VI program, which funded the demolition of two of the baseline public housing buildings, although the timing of the Hope VI and MTO moves were different. Moreover, none of the control families moved through Hope VI received the additional life-skills counseling and search assistance or the relocation restrictions imposed on experimental-treatment families.

17. Bloom (1984).

18. Katz, Kling and Liebman (2001).

Data

Our study draws on three sources of data: baseline survey responses; follow-up addresses; and educational outcome measures for MTO children taken from school administrative records.

Baseline Survey Data

When MTO families enrolled in the program, household heads were required to complete a survey that asked detailed questions about the condition of the family's baseline apartment and neighborhood, the reasons for enrolling in the program, and sociodemographic characteristics of the household such as educational attainment and employment and welfare experiences. The baseline surveys also ask householders to report basic demographic information for their children, as well as information on each child's educational history such as grade, special education placement, and general academic, mental, and physical status. The response rate to these baseline surveys equals 100 percent by construction.

Follow-up Addresses

Tracking of MTO families was conducted by Abt Associates and provides information for families at two points in time: during the immediate postprogram period, which reflects the initial postprogram moves of families; and follow-up addresses that were current as of the second half of 1997. These postprogram addresses were obtained through the use of administrative data from local housing authorities (which administer the Section 8 rental subsidies to movers in the experimental and Section 8-only groups, as well as the public housing units of nonmovers), together with change-of-address registries, credit bureaus, and a brief follow-up survey of MTO families. Surveys were conducted on the telephone for as many families as possible; those who could not be reached by phone were interviewed in person. The response rate to Abt's survey was 91 percent.

School Records

Our key outcome measures come from school administrative records on children in MTO households. Since the Maryland State Department of Education does not maintain student-level data, we obtained student records for the 1993–94 through 1998–99 academic years from the six school districts that

contained the 1,243 MTO children who were of school age during the sample period (born between 1977 and 1993): Baltimore City, Baltimore County, Anne Arundel County, Howard County, Montgomery County, and Harford County. Fully 98 percent of these school-age children live in three counties during the postprogram period (Baltimore City, Baltimore County, and Howard County), with Baltimore City accounting for the vast majority (85 percent).¹⁹

The school districts merged information on MTO participants with school administrative records using information on the child's name, date of birth, and Social Security number. The match rates were typically fairly high. For example, for the 1998–99 academic year the Baltimore City public school system identified school records for 92 percent of the 1,061 MTO children who lived in the city during the postprogram period. The match rate for the 109 children who lived in Baltimore County at some point was 86 percent and equaled 72 percent for the 47 MTO children whose families moved to Howard County. One reason for the nonmatches may be that some MTO children attend private schools, which in Maryland are not required to report student outcomes to either county or state education agencies. Yet the baseline surveys showed that only 1.5 percent of school-age MTO children attended private schools at that time. While the proportion of children attending private schools may increase somewhat if the experimental or Section 8–only treatments have an income effect on households,²⁰ our best guess is that private schools are on the whole probably a small source of attrition from our data.

Our primary outcome measures come from student performance on two sets of standardized achievement test scores, the Comprehensive Test of Basic Skills (CTBS) for elementary and middle-school students, and the Maryland Functional Tests (MFT) for middle- and high-school students. Other outcome measures include the number of school absences, an indicator for disciplinary actions (equal to one if the student has been suspended or expelled from school during the year), and indicators for whether the student received spe-

19. Of the 1,243 school-age children in our sample, follow-up address data suggested that 109 lived in Baltimore County at some point during the postprogram period, while 47 lived in Howard County at some time, 21 lived in Anne Arundel County, 3 lived in Harford County, and 2 lived in Montgomery County.

20. Ludwig, Duncan and Pinkston (2000) find that the rate of welfare receipt among experimental-group household heads in Baltimore was 15 percent lower than among the control group, although there are no differences in quarterly earnings or employment rates as measured by state unemployment insurance (UI) records. Similar null findings for UI employment are reported for the Boston MTO site by Katz, Kling and Liebman (2001). For evidence of the effects of family income on private school attendance see Figlio and Ludwig (2000).

cial education services, dropped out of school, or was retained in grade.²¹ While most of these outcomes are straightforward, the dropout, CTBS, and MFT measures require some additional explanation.

Our construction of a dropout measure is complicated by the fact that the Maryland education records do not directly indicate whether students have dropped out of school. We instead construct a proxy measure that is equal to one if the student is missing school data for a given academic year, had school data available for the two preceding academic years, and was at least 15 years of age on September 1 of the academic year in which the data are missing.²² (Maryland requires students to attend school up until their sixteenth birthday.) Since some schools may retain dropouts on their active roster for financial or other strategic reasons, we also count as dropouts those teens who are at least 15 years old on September 1 of the given academic year and record 120 absences or more. Our measure is thus more accurately described as a “public school exit” measure; in principle some of these exits could reflect moves to the private school system, although we assume that most exits are due to dropout. For simplicity we refer to this as “dropping out,” although readers should be aware of the limitations of this measure.

The CTBS is a commercial multiple choice standardized achievement test for math and reading skills and is designed in part to reflect what students learn in school based on surveys of local standards and curricula. The CTBS test results are reported as the student’s percentile score relative to the national distribution, which facilitates comparisons of student performance across grades and academic years. The use of percentile ranks is not useful for determining how much a given student has learned over time, since a student’s percentile rank could decline over time even if her achievement in an absolute sense has increased. Yet for our purposes of comparing differences across treatment groups the use of percentile rankings is fine, especially since our regressions control for age (measured through a series of years-of-age dummies).

21. Our grade retention variable is equal to 1 if the student is enrolled in the same grade again during the following academic year and equal to 0 otherwise. While it may seem more natural to some readers to construct a measure indicating whether the student was enrolled in the same grade the *previous* year, note that it is this year’s performance level that determines the grade in which the student is enrolled the subsequent academic year.

22. We do not classify as dropouts those students whose last reported grade was 12, since missing data in the subsequent year could reflect either grade retention and dropout or successful completion of high school.

Data on CTBS test results are available only for some students in some years because of the idiosyncracies of the testing schedules of local school systems in Maryland. The Maryland Department of Education requires school districts to administer the CTBS to at least a random sample of students in grades 2, 4, and 6 sometime in February or March of each year.²³ Some school districts choose to voluntarily test the full census of students in these grades in some academic years, or voluntarily test students enrolled in other grades as well. For example, Baltimore City administered the CTBS tests to the census of students enrolled in grades K through 5 during the spring terms of the 1993–94 and 1994–95 academic years. In the spring of 1995–96 the Baltimore City school system tested only a 10 to 15 percent random sample of students enrolled in grades 3, 5, and 8, while in the spring of 1996–97 the system tested a random sample of students in grades 2, 4, and 6.²⁴ In 1997–98 the system again tested a census of students in grades 1 through 5, and in 1998–99 it tested all students in grades 1 through 6. In the fall of each of these academic years the Baltimore City schools administered the CTBS to the full census of eighth grade students.

Other students may be missing CTBS test score results in some years because they were absent from school during the test dates or have been classified as having a significant learning disability. While Maryland schools offer a number of make-up dates for the CTBS tests for students who are absent on the main testing days, it is possible that students who are chronically absent may miss all of the possible testing periods. Students with significant learning disabilities are exempt from the CTBS tests if their Individual Education Plan (IEP) takes them off the regular academic track (that is, if they are no longer officially working toward a regular Maryland academic diploma). Students can in some cases be taken off the regular academic track through the IEP process quite early in their schooling careers.

The MFT program requires public school students in Maryland to pass tests in reading, mathematics, and (until recently) citizenship in order to graduate from high school. Students are allowed to begin taking these tests as early as the seventh grade, although some counties also allow students to take the

23. Maryland also requires students in grades 3, 5, and 8 to take standardized tests for the Maryland State Performance Assessment Program (MSPAP). Because the MSPAP is designed to generate performance measures at the school rather than student level, these test results are not useful for our efforts to examine the effects of the MTO demonstration on the performance of participating children.

24. The random sample was drawn by a subcontractor to the Baltimore City public school system using data tapes for student enrollment in the system.

reading MFT for practice in the sixth grade. Those who fail one or more of the tests are required to take the test again the next year, until they have passed each of the tests. Most counties report MFT results for each of the different subject tests as a raw numerical score. However, some county offices record only whether the student has passed or failed the test. As a result, we measure MFT performance through a series of dichotomous pass/fail indicators. We also restrict our attention to the MFT reading test because of missing-data problems with the MFT math results.²⁵ As with the CTBS tests, some students may be missing MFT results either because of absences during the primary and make-up testing periods or because their IEP exempts them from the MFT requirements.

The administrative data are thus missing information for some students and years for a variety of reasons. Our estimates for the MTO program's impacts on educational outcomes may be biased if the proportion of observations for which data are missing is unequal across MTO treatment groups in some way that is systematically related to student achievement or behavior. For example, if a larger proportion of experimental than control group students are placed in special education and exempt from standardized testing, our estimates may overstate the gains in achievement scores caused by the experimental treatment. On the other hand, data points that are missing for reasons that are uncorrelated with student characteristics (for example, because of nonmatches between the MTO records and county-level administrative data, or because of limitations with the county databases themselves)²⁶ or for reasons that are correlated with measurable variables (such as student age and academic year) may pose less of a threat to the validity of our estimates.

25. In Baltimore City, the school district from which most of our student-years are drawn, many schools began to phase in a computerized version of the MFT math test during our sample period. We only have results from the paper-and-pencil MFT results, which will thus capture the full set of MFT reading tests but only a subset of MFT math results. Since we might expect the more effective Baltimore City schools to be the first to volunteer for the computer-assisted math testing technology, MFT math results are likely to be missing for a nonrandom sample of MTO students.

26. Many school data-management systems are better at identifying current-year information than historical student data. For example, the Baltimore City public schools were able to provide us with information on each of the outcome variables of interest for each academic year from 1992–93 through 1998–99 with the exception of disciplinary information, which was available only for 1997–98 and 1998–99. Data on disciplinary actions were also missing for the 1995–96 academic year for Howard County. Baltimore County provided us with information on school identifiers, CTBS scores, and MFT results for students in 1996–97, 1997–98, and 1998–99, but school-absence, special education, and disciplinary-action data were only available for the 1997–98 and 1998–99 academic years. The other school districts contain only a small number of children, but similar data limitations arose with their data systems as well.

We examine the sensitivity of our estimates to problems of missing data in the following section.

Finally, working with data that are reported by academic year requires some decision about which data point should represent the child's first "postprogram" year. The goal is to count only those academic years in which the child has spent a significant amount of time in her new environment as "postprogram," while at the same time not be overly conservative and discard useful postprogram information. Our solution is to initially define the first postprogram observation as beginning with the first fall term following the family's random assignment into an MTO treatment group. For example, any student who was randomly assigned into a treatment group between January 1, 1997, and December 31, 1997, would be assigned 1997-98 as the first postprogram year. All of our regression results include as a control variable the number of days between January 1 and the date on which the family is randomly assigned. Using this definition, we have 4.2 academic years of postprogram data available for the average MTO student. In the next section we show that our main findings are not sensitive to alternative definitions.

Results

In this section we provide information about the characteristics of Baltimore MTO children and their families at baseline, followed by a discussion of their relocation outcomes. We then present our estimates for the impacts of the MTO program on children's educational outcomes, including a discussion of the sensitivity of our results to problems of missing data and other estimation issues.

Background Characteristics

Table 2 presents descriptive statistics for Baltimore MTO household heads in each of the three residential-mobility groups taken from the baseline surveys. As expected, the characteristics of household heads are quite similar across treatment groups. Almost all of the household heads in the Baltimore MTO demonstration are unmarried African American women. Only about half have either a high school diploma or GED, and the large majority received AFDC benefits at baseline. Although the majority of householders report that

Table 2. Baseline Characteristics of Baltimore MTO Households

<i>Item</i>	<i>Total</i>	<i>Experimental</i>	<i>Section8-only</i>	<i>Control</i>
<i>Household characteristics</i>				
Families (N)	638	252	188	198
African American (%)	97.3	96.9	96.8	98.4
Female householder (%)	97.9	98.4	96.2*	99.0
Householder age	33.6	33.9	33.4	33.2
Number of children	2.72	2.67	2.98	2.55
Householder w/ high school or GED (%)	57.9	59.4	60.8	53.3
AFDC at baseline (%)	81.3	80.3	83.5	80.4
During last six months, someone in HH had been victim of crime (%)	50.4	54.4*	49.9	45.7
<i>Primary reasons for enrolling in MTO (%)</i>				
Better schools, job access	14.4	13.1	17.9	13.3
Avoid gangs, drugs	53.6	53.9	50.7	55.8
Better apartment	26.4	26.8	27.5	25.0
Other	5.4	6.4	3.8	5.8
<i>Second most important reason (%)</i>				
Better schools, job access	36.1	38.3	36.3	36.7
Avoid gangs, drugs	27.9	27.5	26.8	29.3
Better apartment	29.6	26.4	33.2	30.3
Other	6.5	7.8	3.7	7.4

Source: Authors' calculations from MTO baseline survey, with weighting adjustments to account for the change in the randomization algorithm during the implementation of the Baltimore demonstration (see text).

* Difference with control group is statistically significant at 10 percent.

** Difference with control group is statistically significant at 5 percent.

they have held a job for pay at some point in their lives, only one-quarter were working at baseline.²⁷

Despite the very low average earnings and employment rates reported in table 2, most families did *not* enroll in MTO to gain access to better job opportunities. This finding is not necessarily inconsistent with the “spatial mismatch hypothesis,” which argues that the distance between inner-city neighborhoods and suburban job opportunities helps explain low rates of inner-city employment.²⁸ For example it is possible that central-city residence really does affect

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

28. Kain (1968); Holzer (1991); Raphael (1998).

Table 3. Baseline Characteristics of MTO Children Age Six or Older at Randomization

<i>Item^a</i>	<i>Total</i>	<i>Experimental</i>	<i>Section 8-only</i>	<i>Control</i>
<i>Baseline survey data</i>				
Age at randomization	11.19 (.23)	11.50 (.40)	11.14 (.30)	10.81 (.40)
Male	.478 (.024)	.403 (.044)**,+	.502 (.037)	.558 (.035)
Last grade	5.63 (.19)	5.85 (.34)	5.53 (.26)	5.43 (.34)
Special education status	.167 (.032)	.167 (.068)	.163 (.036)	.169 (.037)
<i>School administrative data</i>				
Retained in grade	.053 (.010)	.036 (.014)**	.042 (.013)*	.087 (.020)
Special education status	.155 (.020)	.102 (.028)++	.236 (.039)*	.151 (.033)
Absences	15.66 (1.11)	13.83 (1.53)*	14.82 (1.58)	18.85 (2.48)
<i>CTBS scores, national percentile</i>				
Reading	37.59 (1.82)	36.40 (3.27)	36.77 (2.62)	39.78 (3.28)
Math	39.48 (2.96)	41.60 (5.62)	40.22 (4.14)	35.91 (4.32)
<i>MFT pass rate, percentage</i>				
Reading	.151 (.058)	.210 (.108)	.104 (.073)	.087 (.062)
Dropout	.008 (.004)	.008 (.006)	.009 (.007)	.008 (.006)

* Pairwise difference with the control group statistically significant at 10 percent.

** Pairwise difference with the control group statistically significant at 5 percent.

+ Pairwise difference between experimental and Section 8-only comparison groups statistically significant at 10 percent.

++ Pairwise difference between experimental and Section 8-only comparison groups statistically significant at 5 percent.

a. Standard errors in parentheses, which are adjusted to account for presence of multiple children from the same MTO family within the sample. Information on disciplinary actions only available for students starting in first year following random assignment.

employment outcomes, but MTO household heads nonetheless rank employment as a low priority in their decision about whether to move.

About 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 more than half of the MTO applicants 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 understated owing to telescoping, strategic behavior, or other reporting problems,²⁹ this figure is nevertheless substantially higher than the six-month victimization rate of 6 percent reported by residents of public housing in the nation's largest public housing authorities. Data from MTO participants in the other program sites reveal similarly high baseline victimization rates.

Table 3 presents descriptive statistics for MTO children in Baltimore during the preprogram period, taken from both the baseline surveys and administrative school records. Both the baseline surveys and the school administrative data suggest that about one in six children were receiving spe-

29. Skogan (1981); Goering, Carnevale, and Teodoro (1999).

cial education services at the time families signed up for the program. On average MTO children missed about three weeks of school per year because of absences and had scores on the CTBS reading and math tests equal to around the fortieth percentile in the national distribution. About one of every 20 MTO school-age children were required to repeat a grade in a typical preprogram year.

Table 3 also shows that the preprogram characteristics for children are generally similar across treatment groups. Although there are a few differences across treatment groups in preprogram characteristics like gender and special education status (at least as measured by school records), an *F*-test confirms that the set of preprogram variables taken as a whole is not significantly different across treatment groups. The chance differences in some preprogram characteristics shown in table 3 highlight the importance of controlling for baseline characteristics in calculating program impacts, which we do for all of the impact estimates shown below. Preprogram characteristics could in principle also interact with the effects of the MTO program, a possibility that we explore below by re-estimating program effects for subgroups defined on the basis of baseline characteristics.

Relocation Outcomes

Of those families assigned to the MTO experimental and Section 8–only groups in the Baltimore site, the proportions who relocate through the program equal 58 and 73 percent, respectively. As noted above, most of the Section 8–only noncompliers and half of the noncompliers in the experimental group did not relocate because they did not sign a lease within the Section 8 program’s 180-day time limit on housing searches. Why were some families willing and able to lease private-market apartments within this time limit while others were not?

Table 4 suggests that additional children make it more difficult to find suitable housing for families in the experimental group, consistent with evidence that three- and four-bedroom apartments are difficult to find in the private rental market.³⁰ We also find that households headed by younger women who are on welfare are more likely to relocate through the program, perhaps because these women feel that they have more to gain from moving. Similarly, families with more to lose appear less likely to relocate. For example, experimental families who report that they have “many” friends and family in the baseline neighborhoods are significantly less likely to relocate through the

30. Popkin and Cunningham (1999); Popkin and others (2000).

Table 4. Baseline Characteristics of Baltimore MTO Movers and Nonmovers

<i>Item</i>	<i>Experimental</i>		<i>Section 8-only</i>	
	<i>Movers</i>	<i>Nonmovers</i>	<i>Movers</i>	<i>Nonmovers</i>
<i>Household characteristics</i>				
Families (N)		146	106	137.51
African American (%)	95.5	98.6	96.3	98.5
Female householder (%)	98.8	97.9	98.1**	89.3
Householder age	32.02**	36.06	32.62**	36.20
Number of children	2.44**	2.95	2.98	2.95
Householder w/ HS or GED (%)	59.7	59.0	57.9	71.9
AFDC at baseline (%)	87.5**	71.5	86.6**	72.6
During last six months, someone in HH was victim of crime (%)				
	54.2	54.5	47.1	59.6
<i>Primary reasons for enrolling in MTO (%)</i>				
Better schools, job access	10.2	16.2	18.0	17.6
Avoid gangs, drugs	57.3	49.8	50.4	51.9
Better apartment	24.4	29.7	28.0	25.8
Other	8.1	4.2	3.5	4.7
<i>Second most important reason (%)</i>				
Better schools, job access	34.3	43.1	37.1	33.3
Avoid gangs, drugs	30.2	24.3	26.9	26.4
Better apartment	27.7	24.8	31.6	38.7
Other	7.9	7.7	4.3	1.6
<i>Prevalence of family in baseline neighborhood ("none" is omitted)</i>				
Few	46.6	46.1	46.0	52.7
Many	5.7**	14.7	7.3	6.8
<i>Prevalence of friends in baseline neighborhood ("none" is omitted)</i>				
Few	19.5	24.9	22.9	20.2
Many	1.2**	6.9	4.5	11.6

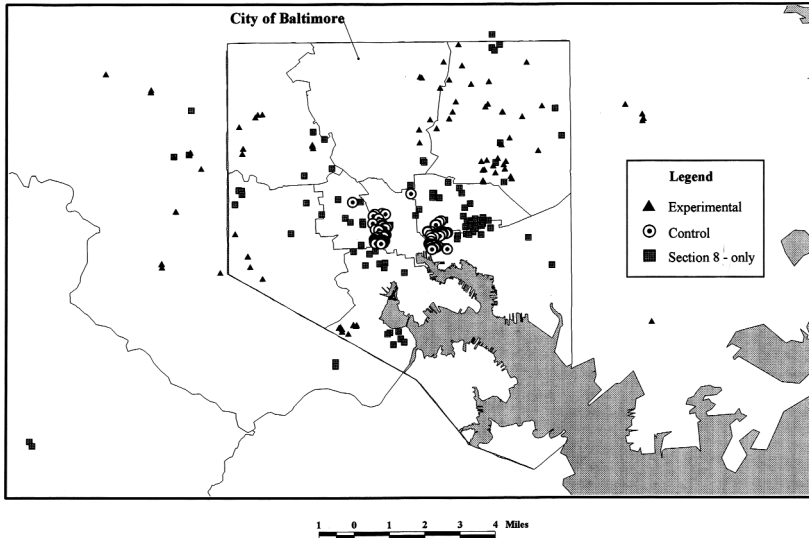
Source: Authors' calculations from MTO baseline survey, with weighting adjustments to account for the change in the randomization algorithm during the implementation of the Baltimore demonstration (see text).

* Difference between movers and nonmovers within MTO treatment group is statistically significant at 10 percent.

** Difference between movers and nonmovers within MTO treatment group is statistically significant at 5 percent.

MTO program. The prospect of losing access to friends and family appears to be less of a constraint for households assigned to the Section 8-only group, consistent with the fact that Section 8-only movers tend to stay close to the baseline neighborhoods (figure 1).

More detailed information about the postprogram neighborhoods of MTO families is presented in table 5. These figures show that (nearly) all of the experimental compliers moved to low-poverty census tracts with 1990 poverty

Figure 1. Relocation Outcomes of Moving to Opportunity Families, 1997

rates below 10 percent, as required by the MTO program's design.³¹ Although the Section 8-only comparison group winds up in lower-poverty areas compared with control group families, only around one of every eight Section 8-only compliers voluntarily move to the lowest-poverty neighborhoods.

Table 5 also shows that the differences across MTO treatment groups in postprogram neighborhood characteristics persist 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. Although some control group families moved to lower-poverty neighborhoods on their own, the 1997 addresses show that only 5 percent had moved to very-low-poverty tracts (<10 percent) by this time.³² In contrast, most of the experimental and Section 8-only compliers

31. 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.

32. 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.

Table 5. Relocation Outcomes for MTO Families

Item	Baseline (all families)		Experimental		Section 8-only		Control	
	1994-96	Initial postprogram	Initial postprogram	As of December 1997	Initial postprogram	As of December 1997	Initial postprogram	As of December 1997
<i>Distribution of MTO households^a</i>								
Jurisdiction :								
Baltimore City	100.0	77.1	79.4	86.7	89.9	86.7	99.5	98.0
Anne Arundel County	0.0	0.8	2.0	0.5	0.0	0.5	0.0	0.0
Baltimore County	0.0	13.0	10.7	8.0	5.3	8.0	0.0	1.0
Harford County	0.0	0.4	0.4	0.0	0.0	0.0	0.0	0.0
Howard County	0.0	7.1	5.9	2.7	2.7	2.7	0.0	0.5
Montgomery County	0.0	0.4	0.4	0.0	0.0	0.0	0.0	0.0
Other	0.0	1.2	1.2	2.1	2.1	2.1	0.5	0.5
<i>% Census tract poor:</i>								
0-9.9	0.0	49.4	43.0	12.5	8.7	12.5	0.0	4.5
10-19.9	0.0	4.8	8.4	21.2	14.7	21.2	0.0	7.6
20-29.9	0.2	0.0	7.6	15.8	10.3	15.8	0.0	3.0
30-39.9	0.3	0.4	4.0	13.0	12.5	13.0	0.0	6.6
40-49.9	2.0	1.6	6.4	7.1	9.8	7.1	2.0	6.6
50-59.9	4.4	1.2	4.0	4.9	6.5	4.9	5.6	4.5
60-69.9	52.5	22.7	18.7	19.6	26.6	19.6	49.0	43.4
70-79.9	20.4	9.6	4.0	3.8	7.1	3.8	23.2	14.6
80 plus	20.1	10.4	4.0	2.2	3.8	2.2	20.2	9.1

a. Neighborhood characteristics are calculated using 1990 Census data.

remain in neighborhoods that are quite similar to those into which they originally moved through MTO.

MTO Program Impacts

The advantage of using administrative school records rather than surveys to measure children's educational outcomes is that multiple years of data are available for each child, information that would be difficult and expensive to obtain through a prospective longitudinal survey design. Administrative records are also less susceptible to the self-reporting problems that may arise with respect to sensitive behaviors such as academic achievement or problem behaviors. One disadvantage is that the administrative school records in Maryland are missing data elements for the variety of reasons discussed above.

Table 6 provides a first look at the missing-data issue by reporting the proportion of school-age MTO children for whom we have at least one observation for our various outcome measures during the postprogram period. In the table and throughout the remainder of the paper we focus separately on children who were at least 5 but less than 12 years of age at the time of random assignment (hereafter "young children") and children who were 12 and older at randomization (hereafter "adolescents" or "teens") for two reasons. First, the availability of achievement data varies considerably by age: while we often have results from both the CTBS and MFT testing programs for younger children, we only have results from the MFT for teens. Second, interventions such as MTO may have greater effects on achievement for younger children because the annual rate of change in children's test scores appears to decrease with age, or because young children from high-poverty areas may be more receptive to changes in social environments or are not yet as far behind in school as older children.³³ Whatever the reason, our analysis does in fact reveal differential program impacts for younger versus older children.

As seen in table 6, for elementary-school children we have at least one CTBS reading and math score for 53 and 46 percent of the sample, respectively. These proportions are roughly equal for the experimental and control groups, although the figures are somewhat lower for the Section 8-only group. We have at least one MFT result for only about one-quarter of the sample of young children but nearly two-thirds of the sample of students ages 12 and older at random assignment. For the large majority of both the younger and older samples we have at least one postprogram observation for disciplinary records, school absences, special education status, and grade retention. As a

33 . Entwistle, Alexander, and Olson (1997).

Table 6. Percent of MTO Children with Postprogram School-Records Data

<i>Item^a</i>	<i>Total</i>	<i>Experimental</i>	<i>Section 8-only</i>	<i>Control</i>
<i>Full sample born 1970–1993 (N = 1,237)</i>				
≥ CTBS reading score (%)	33.33	36.70	28.12	33.87
≥ CTBS math score	29.35	31.96	24.40	30.83
≥ MFT reading result	33.14	36.70	28.12	33.60
≥ Disciplinary-problem observation	94.18	93.20	93.10	96.53
≥ School-absence observation	86.18	85.77	84.08	88.80
≥ Special-education status obs.	86.26	85.77	84.35	88.80
≥ Grade-retention observation	80.27	78.66	78.25	84.53
<i>Students ≥ and <12 at random assignment (N = 669)</i>				
≥ CTBS reading score (%)	52.62	60.66	41.31	54.64
≥ CTBS math score	46.19	52.59	35.74	50.24
≥ MFT reading result	26.16	29.48	20.66	27.80
≥ Disciplinary-problem observation	94.92	94.12	93.90	97.17
≥ School-absence observation	90.73	90.84	88.73	92.68
≥ Special-education status obs.	90.88	90.84	89.20	92.68
≥ Grade-retention observation	88.64	87.65	86.85	91.71
<i>Students ≥2 at random assignment</i>				
≥ MFT reading result (%)	62.17	63.41	59.62	62.73
≥ Disciplinary-problem observation	96.03	95.12	95.19	98.18
≥ School-absence observation	87.25	75.61	75.00	81.82
≥ Special-education status obs.	77.51	76.22	75.00	81.82
≥ Grade-retention observation	66.14	63.41	63.46	72.73

a. Authors' calculations from administrative school records and baseline household information for Baltimore MTO demonstration.

first cut we will assume that outcome data are missing at random (MAR) for MTO children in the Maryland administrative records, although in the next section we explore the sensitivity of our findings to this assumption.

SCHOOLING PATTERNS AMONG CONTROL GROUP CHILDREN. What are the effects of the MTO program on children's educational outcomes? To provide some context for the answer to this question, in table 7 we provide descriptive information about the developmental trajectory of children in the control group. Using data from both the pre- and postprogram periods, we calculate average educational outcomes for control group children by age. To reduce the effects of year-to-year sampling variability, the table presents three-year moving averages (for example, the results for age 11 actually represent the average outcomes for children ages 10 through 12).

Table 7 shows that in general as children in the control group age, their educational outcomes deteriorate either absolutely or relative to the national

Table 7. Developmental Patterns for MTO Control Group Children

<i>Children's age^b</i>	<i>Retained in grade</i>	<i>Dropout</i>	<i>Absences</i>	<i>CTBS^a reading</i>	<i>CTBS^a math</i>	<i>Pass MFT^a reading</i>	<i>Suspended/expelled</i>	<i>Special education</i>
6	.043		10.38	38.73	38.52		.051	.090
7	.078		9.17	32.74	35.67		.041	.107
8	.080		8.58	32.48	33.60		.030	.128
9	.082		9.05	27.59	30.23		.069	.173
10	.059		11.51	22.66	27.77		.140	.208
11	.066		15.55	16.89	25.17	.169	.212	.221
12	.066		19.74	18.96	21.55	.181	.264	.215
13	.109		25.62	18.60	20.19	.212	.240	.203
14	.140		30.95			.241	.167	.184
15	.165	.125	37.51			.287	.074	.165
16	.111	.208	37.26			.316	.014	.176
17	.064	.284	34.69			.394	.039	.234
18	.071	.356	24.64				.080	.300

a. Comprehensive Test of Basic Skills; Maryland Functional Tests

b. In order to reduce sampling variability from year-to-year changes in educational outcomes, the results for each age represent three-year moving averages (so that, for example, the results for age 10 represent the average outcomes for children ages 9–11).

average. For example, while control group children on average score near the fortieth percentile in the national distribution on the CTBS reading and math tests at age six, by age 13 the average score is only at about the twentieth percentile. We also see that the proportion of students who receive special education services increases steadily over time. Grade retentions, school absences, and disciplinary problems all peak in the early or mid-teen years. The subsequent decline is presumably due to the increase in dropout rates at older ages.

The MTO experimental and Section 8-only comparison treatments seem to slow the rate of relative decline in children's test scores as they age, at least for younger children, but MTO also appears to increase the rate of grade retention among adolescents. These findings come from estimating the program's intent-to-treat impacts from equation (2), controlling for baseline child and family characteristics, preprogram educational outcomes for the first and second years prior to randomization, and a series of dummy variables indicating years since random assignment and academic calendar year.

MTO EFFECTS ON YOUNG CHILDREN. Table 8 shows the intent-to-treat effects of the MTO program on each of our educational outcomes for young children. Compared with young children in the control group, those assigned to the experimental group experience substantial gains in academic achievement as measured by standardized test scores. Experimental children are nearly 18 percentage points more likely than controls to pass the MFT reading test, which means that the experimental pass rate on this test is nearly double that of the control group. The CTBS reading and math scores of experimental children are about 7 percentile points higher than those of the control group, equal to around 29 and 26 percent of the control group means on these tests. Another way to judge the magnitude of the CTBS impacts is by comparison to the standard deviation of the test score distribution. The experimental treatment effect on the CTBS reading and math scores equal 27 and 25 percent of the standard deviation in percentile scores observed among the MTO samples, or 25 and 26 percent of a standard deviation in the national CTBS reading and math distributions.³⁴ In contrast to the test score results, we find no statistically significant differences between experimental and control

34. We calculate the standard deviation for the national percentile scores by noting that percentile rankings have by construction a uniform distribution over the integers from one through 100, and then applying the formula for a standard deviation of a continuous uniform distribution over a defined interval. See, for example, McClave and Benson (1985, p. 223).

Table 8. Intent-to-Treat Effects of MTO Program on Young Children

<i>Item^a</i>	<i>N</i>	<i>Control mean</i>	<i>Experimental versus control</i>	<i>Section 8–only comparison versus control</i>
MFT, reading (fraction pass)	347	.192 (.059)	.178 (.083)**	.059 (.093)
CTBS, reading (percentile score)	458	25.13 (2.47)	7.34 (2.75)**	6.39 (3.23)**
CTBS, math (percentile score)	404	28.77 (2.69)	7.48 (3.67)**	1.48 (3.83)
Absences (days)	2,200	12.54 (1.02)	0.57 (1.16)	1.10 (1.31)
Grade retention (fraction retained)	1,711	.075 (.010)	-.013 (.013)	.002 (.015)
Disciplinary actions (fraction suspended/expelled during year)	1,123	.150 (.026)	-.015 (.031)	.001 (.039)
Special education (fraction)	2,206	.162 (.024)	.043 (.032)	.052 (.037)

* Statistically significant at 10 percent.

** Statistically significant at 5 percent.

a. Sample restricted to MTO children ages ≥ and <12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure of dataset and presence of multiple children from the same family within our analytic sample. Estimates are calculated controlling for preprogram educational outcomes, as well as baseline family characteristics (see text).

children with respect to special education placements, absences, grade retention, or disciplinary problems.³⁵

Assignment to the Section 8–only comparison treatment appears to improve young children’s CTBS reading scores by about 6 percentile points relative to controls, although the difference across groups in CTBS math scores is relatively small and is not statistically significant. The Section 8–only group also appears to pass the MFT reading test at a rate that is around 6 percentage points higher than that of controls, but the difference is again not statistically significant.

Since a child’s educational performance at a point in time is a product of her current and past educational “inputs,” we may expect the MTO program’s impacts on educational outcomes to increase over time. Unfortunately, the data reveal no clear patterns in the program impacts over time, because of both the reasonably modest number of data points available for each academic

35. We also examine the probability of being enrolled in the same grade in year 0 and year 1, as would occur if schools in low-poverty areas have higher standards than baseline schools and thus move experimental relocators to a more appropriate grade level. However, we find no differences across treatment groups using this measure.

year and the limited number of postprogram academic years in our data. Revisiting the temporal patterns of program effects using a longer postprogram observation period is an obvious priority for future research.

We might expect some differences in the MTO program's impacts by gender because of differences in how boys and girls socialize with others, or because boys tend to spend more time outside in their neighborhoods than girls.³⁶ But we find that the effects of the MTO demonstration on educational outcomes in general seem to be at least as large for girls as for boys. We are unable to examine the program's effects on separate racial or ethnic groups since (as seen in table 2) almost all of the MTO participants in Baltimore are African American.

It is also possible that low-income children may be more likely to take advantage of whatever new opportunities are afforded by more affluent areas when they are actively encouraged and assisted by their parents. We test this hypothesis by reestimating the MTO program's impacts on those young children whose mothers are most likely to be engaged with school-related matters, as suggested by their mother's educational attainment or reason for volunteering for the MTO demonstration.

The results of estimating separate program impacts by parent education or motivation for program participation are somewhat mixed. Young children in households headed by someone who had a high school degree or equivalent or signed up for MTO for access to better schools seemed to experience above-average gains in CTBS math and reading scores from assignment to the experimental or Section 8-only groups. These children, however, may have experienced below-average benefits in terms of MFT reading pass rates, school absences, and other outcome measures.

MTO EFFECTS ON ADOLESCENTS. For adolescents (those 12 years old and over at random assignment) the only available measure of academic achievement is the MFT reading test, for which there are no statistically significant differences across MTO treatment groups (table 9). However, we do observe a substantial and statistically significant increase in grade retentions for experimental and Section 8-only teens relative to controls. Table 9 also reveals relatively large increases in the proportion of experimental and Section 8-only teens who are suspended or expelled from school relative to control teens, although only the experimental ITT effect is statistically significant (and only

36. Heyns (1978); Furstenberg and Hughes (1997).

Table 9. Intent-to-Treat Effects of MTO Program on Adolescents

<i>Item^a</i>	<i>N</i>	<i>Control mean</i>	<i>Experimental versus control</i>	<i>Section 8-only comparison versus control</i>
MFT, reading (fraction pass)	679	.268 (.063)	.073 (.072)	-.009 (.071)
Absences (days)	825	29.85 (2.20)	2.06 (2.92)	1.78 (3.45)
Grade retention (fraction retained)	564	.092 (.021)	.065 (.032)**	.109 (.052)**
Disciplinary actions (fraction suspended/expelled during year)	328	.126 (.033)	.086 (.050)*	.088 (.069)
Special education (fraction)	826	.202 (.049)	-.017 (.050)	.013 (.059)
Dropout	1384	.187 (.030)	.062 (.036)*	.019 (.043)

a. Sample restricted to MTO children ages ≥12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure of dataset and presence of multiple children from the same family within our analytic sample. Estimates are calculated controlling for preprogram educational outcomes, as well as baseline family characteristics (see text).

* Statistically significant at 10 percent.

** Statistically significant at 5 percent.

at the 10 percent level). As with our study of young children’s outcomes, we are unable to draw any firm conclusions about whether the program effects on these teen outcomes grow or diminish over time because of the relatively short postprogram observation period and the modest number of data points per academic year.

Among teens we observe no systematic differences in MTO effects by parental educational attainment at baseline or by the reasons that parents signed up for the program. However, since boys account for nearly three-quarters of all arrests of people under age 18 in the United States, we might expect that any increase in problem behaviors among experimental and Section 8-only teens will be concentrated among boys.³⁷ But in fact the point estimates for the effects of the experimental and Section 8-only treatment on disciplinary problems in school are positive and sizable for both boys and girls. One possible explanation is that teen problem behaviors in school are actually fairly similar across MTO groups, but schools serving more affluent communities have higher academic and behavioral standards than schools in poor areas. We return to this possibility in the discussion below.

EFFECTS OF TREATMENT ON THE TREATED. Finally, the results in tables 10 and 11 show our estimates for the other policy-relevant measure of MTO’s

37. Maguire and Pastore (1999, p. 341).

Table 10. MTO Effects of Treatment on the Treated for Young Children

<i>Item^a</i>	<i>Experimental treatment</i>				<i>Section 8-only comparison treatment</i>					
	<i>Movers</i>	<i>Nonmovers</i>	<i>CCM^b</i>	<i>Comply rate^c</i>	<i>TOT</i>	<i>Movers</i>	<i>Nonmovers</i>	<i>CCM^b</i>	<i>Comply rate^c</i>	<i>TOT</i>
MFT, reading (fraction pass)	.518	.181	.210	.52	.308 (.166)*	.203	.039	.152	.86	.051 (.105)
CTBS, reading (percentile score)	32.50	27.78	21.27	.68	11.23 (4.39)**	33.79	17.25	27.29	.85	6.50 (3.84)*
CTBS, math (percentile score)	32.15	31.47	22.41	.68	9.74 (6.11)	31.38	18.82	27.10	.85	4.28 (4.54)
Absences (days)	11.56	16.28	9.80	.55	1.76 (2.10)	13.60	16.38	12.41	.82	1.19 (1.57)
Grade retention (fraction retained)	.048	.073	.069	.58	-.021 (.023)	.072	.072	.070	.83	.002 (.018)
Disciplinary actions (fraction suspended / expelled during year)	.130	.149	.151	.57	-.021 (.056)	.150	.055	.170	.82	-.020 (.052)
Special education (fraction)	.189	.198	.102	.55	.087 (.058)	.192	.304	.146	.82	.046 (.045)

* Statistically significant at 10 percent.

** Statistically significant at the 5 percent level.

a. Sample restricted to MTO children, ages \geq and <12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure, of dataset and presence of multiple children from the same family within our analytic sample. TOT estimates are calculated via two-stage least squares using each family's MTO random assignment outcome as an instrumental variable for an indicator for relocation through the MTO program. The two-stage least squares model controls for preprogram educational outcomes, as well as baseline family characteristics (see text).

b. Control complier mean equals the estimated average outcome of those families who are assigned to the control group but would have relocated through the MTO program had they been assigned instead to a treatment group.

c. The "comply rate" is calculated as the proportion of experimental or Section 8-only student-years in our panel dataset that correspond to a student whose family relocated through the MTO program.

Table 11. MTO Effects of Treatment on the Treated, Adolescents

<i>Item^a</i>	<i>Experimental treatment</i>				<i>Section 8-only comparison treatment</i>					
	<i>Movers</i>	<i>Nonmovers</i>	<i>CCM^b</i>	<i>Comply rate^c</i>	<i>TOT</i>	<i>Movers</i>	<i>Nonmovers</i>	<i>CCM^b</i>	<i>Comply rate^c</i>	<i>TOT</i>
MFT, reading (fraction pass)	.348	.318	.190	.47	.158 (.142)	.202	.312	.179	.67	.023 (.102)
Absences (days)	32.32	31.05	30.54	.47	1.78 (5.56)	38.88	29.71	37.39	.67	1.49 (4.88)
Grade retention (fraction retained)	.157	.115	.048	.48	.109 (.060)*	.264	.124	.064	.67	.200 (.077)**
Disciplinary actions (fraction suspended / expelled during year)	.172	.156	.024	.47	.148 (.107)	.213	.167	.163	.66	.050 (.108)
Special education (fraction)	.185	.125	.060	.47	-.015 (.094)	.326	.234	.318	.67	.008 (.081)
Dropout	.213	.177	.100	.47	.113 (.076)	.214	.173	.203	.72	.011 (.060)

* Statistically significant at 10 percent.

** Statistically significant at the 5 percent level.

a. Sample restricted to MTO children, ages \geq and <12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure of dataset and presence of multiple children from the same family within our analytic sample. TOT estimates are calculated via two-stage least squares using each family's MTO random assignment outcome as an instrumental variable for an indicator for relocation through the MTO program. The two-stage least squares model controls for preprogram educational outcomes, as well as baseline family characteristics (see text).

b. Control complier mean equals the estimated average outcome of those families who are assigned to the control group but would have relocated through the MTO program had they been assigned instead to a treatment group.

c. The "comply rate" is calculated as the proportion of experimental or Section 8-only student-years in our panel dataset that correspond to a student whose family relocated through the MTO program.

impacts: the effects of treatment on the treated. As noted above, in large samples the TOT effect for the experimental and Section 8–only group will equal the ITT effects shown in tables 8 and 9 divided by the proportion of families in the experimental and Section 8–only groups who relocate through the MTO program (58 and 73 percent). The TOT estimates shown in tables 10 and 11 differ somewhat from this ratio, in part because the compliance rate among those children for whom we have valid postprogram observations differs somewhat from that observed among all MTO families.

The TOT estimates for young children shown in table 10 show that the gains in CTBS reading and math scores by young children whose families move as part of the experimental group are about twice as large as the gains experienced by Section 8–only movers. Since the average change in neighborhood poverty rates was around twice as large for experimental compared with Section 8–only compliers,³⁸ the TOT estimates provide at least suggestive evidence that changes in children's test scores may be proportional to changes in neighborhood poverty rates. While the improvement in MFT pass rates is six times as large for the experimental than Section 8–only compliers, this is not inconsistent with a linear neighborhood effect since a pass/fail indicator is itself a nonlinear outcome measure indicating whether some underlying continuous variable (in this case reading achievement) crosses a given threshold.³⁹ Because most of the outcome measures available for adolescents (table 11) are also nonlinear indicators, we have little to say about whether neighborhood effects on this group are proportional to changes in neighborhood characteristics.

It is important to recognize that these comparisons of TOT effects for the experimental and Section 8–only groups provide only a rough indication of whether neighborhood effects are nonlinear. While the TOT effects may be

38. Among the Baltimore MTO sample of all school-age children (ages 5 and older at baseline), the average postprogram census tract poverty rate equaled 8.4 and 35.2 percent for the neighborhoods into which experimental and Section 8–only comparison compliers initially moved through the MTO program. In contrast, the average postprogram poverty rate for the census tracts in which the control group resided was around 70 percent. Thus the average experimental-group complier experiences a change in neighborhood poverty rates that is about 1.8 times as large as that of the average Section 8–only complier.

39. For example, suppose that all experimental and Section 8–only children have raw MFT reading scores equal to 205 at baseline, that the pass rate on the MFT reading test is 220, and that all Section 8–only compliers gain 10 points on the raw MFT scale, while experimental compliers all gain 18 points. In this case the actual gain in underlying MFT raw scores is perfectly linear with respect to changes in neighborhood poverty rates, yet there is now a 100 percent-age point improvement in MFT pass rates for experimental movers and no improvement at all in pass rates for Section 8–only movers.

proportional to changes in census-tract poverty rates, they may be nonlinear for other neighborhood characteristics that could be responsible for the observed MTO effects. Moreover, the comparison of experimental and Section 8—only TOT effects may confound nonlinearities in neighborhood effects with heterogeneity in how families respond to mobility programs, since the two subgroups that chose to comply with the experimental and Section 8—only treatments could in principle have different average responses to the exact same intervention.

With these caveats in mind, as best we can judge the MTO data provide no compelling evidence to suggest that the relationship between children's outcomes and neighborhood poverty is very nonlinear. At the same time, the evidence also suggests that the program's effects are due at least in part to moving to different types of neighborhoods rather than moving *per se*, since in table 10 we find some evidence of a "dose-response" relationship in which more substantial changes in neighborhood poverty induce larger changes in young children's test scores.

Lastly, the TOT results can also be used to determine which types of families are most likely to relocate through the MTO program. If the control-complier mean for positive outcomes exceeds the mean for the control group as a whole, families whose children are disposed toward more academic and pro-social outcomes are more likely to move through MTO to more affluent areas ("positive selection"). Table 10 provides some indication of *negative selection* with respect to young children's educational outcomes, particularly with respect to children's academic achievement and special education status. While the evidence on selection with respect to adolescent schooling outcomes is more mixed (table 11), data from both the Baltimore and Boston MTO sites provide more consistent evidence of negative selection with respect to teen out-of-school problem behaviors.⁴⁰

Sensitivity Analyses

The estimates presented in the previous section assume that postprogram school records are missing at random (MAR) for MTO children. In this section we test this assumption in four ways and conclude that as far as we can tell the results are not very sensitive to problems of missing data. We also examine the sensitivity of our findings to other estimation issues.

MISSING DATA. One way to test the MAR assumption directly is to regress a dichotomous indicator equal to one if a given outcome measure is missing

40. Katz, Kling and Liebman, (2001); Ludwig, Duncan, and Hirschfield (2001).

for a given student in a given year against a set of preprogram characteristics such as the child's baseline special education status and number of siblings, as well as the mother's marital status, age, educational attainment, and employment and welfare status, and indicators for the academic year and the child's age in each year. We find that variables indicating the academic year and the child's age in each year are strong predictors for missing data for each of these outcome variables, which is consistent with the idiosyncracies described above in the availability of school records for certain years and grades. We also find that conditional on year and age, formal F-tests suggest that the other baseline child and family characteristics are almost never significant predictors of data availability (the one exception being for our measure of disciplinary actions).⁴¹ Conditional on age, year, and baseline characteristics, experimental and Section 8-only students are more likely than controls to be missing data on grade of attendance, absences, and special education status, but no more likely to be missing data on disciplinary actions or, perhaps more important, CTBS or MFT test results.

A related way to test the MAR assumption is to examine whether our estimates are sensitive to the inclusion of baseline control variables, since systematic patterns of missing data may cause the samples of children for whom we have valid outcome data to be unbalanced across treatment groups with respect to observable preprogram characteristics. Yet we find that our estimates for the program's impacts are generally not very sensitive to whether we control for preprogram child and family characteristics. The one exception is for CTBS math scores among young children, where the experimental ITT effect is equal to 3.7 percentile points with no preprogram controls, an effect that increases to 4.2 points after controlling for age and increases again to 7.5 points (and becomes statistically significant) when we include controls for age and other preprogram student and family characteristics.

A third way to examine the sensitivity of our estimates to the problem of missing data is to replace missing values with the most recent observation available on that variable for the student in question as in Krueger.⁴² Table 12 shows that experimental children are still more likely than controls to pass the

41. Regressing these missing-data indicators against preprogram school outcomes taken from the official school records is complicated because the preprogram school outcomes are strongly correlated with the child's age for two reasons. First, older children are more likely to have valid preprogram outcome measures recorded on their school records. Second, given the gradual deterioration in average educational outcomes as children age documented above, older children will have systematically worse preprogram outcomes compared with younger children.

42. Krueger (1999).

Table 12. Intent-to-Treat Effects of MTO Program on Young Children with Interpolation of Missing Data

<i>Item</i> ^a	<i>N</i>	<i>Experimental versus control</i>	<i>Section 8–only comparison versus control</i>
MFT, reading (fraction pass)	356	.157 (.081)*	.044 (.092)
CTBS, reading (percentile score)	955	2.963 (1.552)*	2.144 (1.743)
CTBS, math (percentile score)	840	3.790 (1.893)**	.398 (1.922)
Absences (days)	2345	.002 (1.062)	.426 (1.220)
Grade retention (fraction retained)	1711	-.013 (.013)	.002 (.015)
Disciplinary actions (fraction suspended / expelled during year)	1174	-.013 (.030)	-.006 (.037)
Special education (fraction)	2346	.034 (.031)	.039 (.035)
Dropout	2674	.001 (.003)	.001 (.003)

* Statistically significant at 10 percent.

** Statistically significant at 5 percent.

a. Sample restricted to MTO children ages \geq and <12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure of dataset and presence of multiple children from the same family within our analytic sample. Estimates are calculated controlling for preprogram educational outcomes, as well as baseline family characteristics (see text).

MFT reading test and have higher CTBS reading and math scores when we interpolate missing values, although the magnitudes of the CTBS test score gains are about half as large as those shown in table 8. Similarly, older children in the experimental and Section 8–only group still appear to be more likely than controls to be retained in grade when we interpolate missing values in this way (table 13).⁴³

One final check on the missing-data problem is suggested by the fact that test score data are more likely to be missing for special education students or those with large numbers of absences. In table 8 we showed that for younger children, special education placements may be somewhat higher among experimental and Section 8–only students than among controls. It is thus possible that the test score differences reported in table 8 could be due to the exclusion of a higher proportion of students from the bottom of the achievement distribution in the experimental and Section 8–only groups relative to controls. We examine the sensitivity of our estimates to this problem by re-

43. Yet another approach for examining the sensitivity of our analysis to missing values is to use the bounding strategy outlined by Manski (1995), although this approach is unfortunately not very informative in our application given the proportion of observations that are missing.

Table 13. Intent-to-Treat Effects of MTO Program on Adolescents with Interpolation of Missing Data

<i>Item^a</i>	<i>N</i>	<i>Experimental versus control</i>	<i>Section 8—only comparison versus control</i>
MFT, reading (fraction pass)	811	.035 (.060)	-.024 (.061)
Absences (days)	1023	-.318 (2.199)	1.088 (2.690)
Grade retention (fraction retained)	564	.065 (.032)**	.109 (.052)**
Disciplinary actions (fraction suspended / expelled during year)	428	.062 (.039)	.056 (.047)
Special education (fraction)	1026	-.032 (.042)	.005 (.048)
Dropout	1258	.035 (.019)*	.005 (.024)

* Statistically significant at 10 percent.

** Statistically significant at 5 percent.

a. Sample restricted to MTO children ages \geq and <12 at random assignment. Robust standard errors shown in parentheses, which are adjusted to account for panel structure of dataset and presence of multiple children from the same family within our analytic sample. Estimates are calculated controlling for preprogram educational outcomes, as well as baseline family characteristics (see text).

estimating the MTO effects after excluding students who are in special education during the preprogram period (who presumably represent most of the students whose IEP's would be upgraded in low-poverty schools to exempt them from postprogram testing) and students who have more than 20 absences during the preprogram year (and thus are presumably at above-average risk for high absences during the postprogram period). We find that the general pattern of MTO impacts is qualitatively similar to those shown above when we drop students who are in special education or often absent during the preprogram period.

OTHER ESTIMATION ISSUES. Our findings for the MTO program's impact do not appear to be sensitive to the particular estimation method used to statistically adjust for chance differences in preprogram characteristics. We use ordinary least squares for the estimates presented above because of the simplicity of calculating TOT effects with linear models. Yet in principle a probit or logit model might yield different estimates for the effects of MTO assignments on dichotomous outcomes such as suspensions, grade retention, or dropout. A log-linear model may provide a better fit for the pattern of CTBS reading and math scores across the MTO sample. And even though ordinary least squares is unbiased for count data such as school absences, Poisson or negative-binomial models may be more efficient. But these alternative esti-

mation methods all yield qualitatively similar findings to those presented above.⁴⁴

Similarly, the results are robust to alternative methods for defining which academic year is counted as each student's first "postprogram" observation. Our benchmark estimates presented above define each child's first postprogram academic year as the first fall following (or including) the family's random assignment. We re-estimated all of our results defining the first postprogram observation as the first full academic year following the student's random assignment to an MTO treatment group (so a student who was randomly assigned some time between September 1, 1996, and August 31, 1997, would be assigned 1997–98 as her first postprogram academic year). Using this definition, the pattern of findings is qualitatively similar to those shown in tables 8 and 9.⁴⁵

Discussion

The Baltimore MTO experiment provides a unique opportunity to overcome the self-selection problems that plague nonexperimental studies of neighborhood effects on children's schooling. In the following pages we review our main findings and discuss the implications for future research and public policy.

Findings

The offer to move families from very high- to very low-poverty neighborhoods (the MTO experimental treatment) appears to substantially improve the

44. Cameron and Trivedi (1998). Our regression adjustment focuses on controlling for pre-program characteristics, the only exceptions being indicator variables for the academic year and year since random assignment. Some might argue for also controlling for the grade in which the student is enrolled during each of the postprogram years, since students who are retained in grade during the postprogram period wind up taking easier standardized tests than they would if they had been promoted on schedule (and thus may look better with respect to the national distribution). Yet differences in grade retention are unlikely to explain our findings for standardized tests for younger children (table 8) since we observe differences in grade retentions across treatment groups for older but not younger children. More generally, when we re-estimate the MTO treatment effects controlling for the grade in which the student is enrolled each postprogram year we obtain similar findings to those shown above.

45. We find that the positive differential in special education placements for young children in the experimental group compared to controls is now statistically significant at the 5 percent level. For adolescents we find a larger effect of the experimental treatment on dropout rates compared with table 9 but a smaller effect on grade retention and disciplinary actions.

reading and math test scores of young children, defined as those between the ages of 5 and 12 at the time of random assignment. Assignment to the experimental group nearly doubles the proportion of young children who pass the Maryland Functional Testing program's reading test and increases standardized CTBS reading and math scores by around one-quarter of a standard deviation. In contrast, assigning families to the Section 8-only group, which produces less dramatic changes in neighborhood poverty rates compared with the experimental treatment, improves young children's performance on the CTBS reading test but not on other achievement measures.

For adolescents in the experimental and Section 8-only groups we find slightly higher rates of grade retention than controls and perhaps more disciplinary actions and school dropout rates as well. These findings should be qualified by the observation that our dropout measure is far from perfect, and our finding for disciplinary actions is somewhat sensitive to how we define the student's first postprogram data point. But more important, unlike with standardized achievement tests that are designed to provide consistent measures of students in different schools, the behaviors that lead to grade retention and disciplinary actions are defined by local teachers and principals and may thus vary across schools. Talking back to a teacher may be a minor offense in a high-poverty school in which guns, drugs, and gangs are common but may earn a suspension or even expulsion in a low-poverty school where behavioral problems are rare. This raises the possibility that experimental and Section 8-only teens may be subject to school sanctions more often than control teens even if there are no differences across groups in school-based problem behaviors. And in fact our data provide some support for this hypothesis.

If the additional grade retention or suspensions observed within the experimental and Section 8-only groups are caused by an increase in teen problem behavior, we would expect the experimental and Section 8-only groups to also experience more school absences and lower MFT pass rates. Absences and MFT test results are correlated with grade retention and disciplinary actions but are presumably not very sensitive to variation across schools in behavioral or academic standards.⁴⁶ We find that conditioning on each teen's absences and MFT reading status in a given postprogram period (t) hardly affects the magnitudes of the experimental and Section 8-only ITT effects on adolescent

46. The correlations between grade retention and absences and MFT status equal 0.40 and -0.17, respectively; the F statistic in a regression of grade retention against these explanatory variables equals 66.3 ($p < .001$). The correlations between disciplinary actions and absences and MFT status equal 0.22 and -0.06, while the F-test of the joint explanatory power of these variables in a regression for disciplinary problems yields a statistic of 3.7 ($p < .05$).

grade retention and disciplinary actions in the same period. MTO's effects on grade retention and disciplinary problems are thus due either to changes in underlying problem behaviors that are uncorrelated with absences and test scores or, perhaps more likely, to higher behavioral and academic standards in the schools serving low-poverty census tracts.

In sum, the evidence presented here seems to suggest that the offer to relocate families in public housing from high- to low-poverty neighborhoods improves standardized achievement test scores among young children. While we have subjected our findings to a variety of sensitivity tests, there nevertheless remains the possibility that the estimated program effects may be due in part to problems of missing data. The effects of the program on teens are more difficult to determine because our measures of in-school problem behavior confound changes in the behaviors of teens with differences across schools in standards and because the measures of academic achievement available for teens are quite limited in our Maryland education data.

Policy Implications

What do these results imply for urban policy? The answer depends in part on why the MTO program improves the academic achievement of young children. For example, suppose that MTO improves young children's test scores by exposing them to schools with smaller class sizes. In this case policymakers could improve the academic outcomes of poor children in high-poverty urban neighborhoods without relocating them to lower-poverty areas, although of course policymakers may wish to relocate poor families for other reasons beyond the desire to improve children's educational outcomes. Policies that change the way that poor or minority students are sorted across schools (without moving their families) might improve the achievement of these children if changes in school quality or school-based peer interactions are important explanations for the MTO program impacts. If the MTO program impacts are because families move rather than because of any specific changes in neighborhood characteristics, then housing voucher programs may improve the educational outcomes of children in public housing even if their families do not move to lower-poverty areas.

The sensitivity of young children's outcomes to the magnitude of the change in neighborhood poverty rates that they experience suggests that MTO does not improve test scores solely through a generic moving effect. Some actual differences between high- and low-poverty neighborhoods appear to be responsible for the reported MTO program impacts, although disentangling the specific neighborhood attributes that are responsible for these effects is

complicated by the fact that the program changes children's schools and all of their neighborhood attributes simultaneously.

The pattern of program impacts on test scores in reading versus math provides some limited information about the relative importance of the effects of changing schools versus changing neighborhoods. Previous studies often find that school characteristics or school-based interventions have more pronounced effects on math than reading scores, while the out-of-school environment has stronger effects on reading.⁴⁷ Thus if MTO's effects on children's test scores were due exclusively to changes in school quality, we would expect the program's impacts to be much larger for math than reading. Because the reading gains for young children in the experimental and Section 8-only groups are at least as large as their math gains, changes in neighborhood characteristics may be responsible for at least part of MTO's impact on academic achievement.⁴⁸ However, without more reliable information about the relative importance of different neighborhood and school attributes for children's outcomes, policymakers will be unable to replicate MTO's effects by targeting interventions at specific neighborhood characteristics within high-poverty urban areas.

On the other hand, policies that enable poor families to move from high-poverty areas to more economically mixed neighborhoods may improve children's educational outcomes even if we do not understand the specific mechanisms through which residential mobility operates. One way to help poor families conduct such moves is through HUD's existing Section 8 housing programs, although the degree of additional economic desegregation that will result from such policies remains somewhat unclear. Previous evaluations

47. See, for example, Ferguson and Ladd (1996); Grissmer, Flanagan and Williamson (1998); Rouse (1998).

48. In principle another way to disentangle school from neighborhood effects is to focus on the difference in neighborhood effects on test score changes during the school year (when children are exposed to family, neighborhood, and school factors) and during the summer term (when children are primarily exposed to family and neighborhood factors, setting aside the possibility of summer school). This disaggregation requires test scores measured at the beginning and end of each academic year, while the school records in Maryland typically only provide a spring test score for children. Previous research by Entwisle, Alexander, and Olson (1997) attempts to distinguish between the effects of school and out-of-school factors by focusing on patterns of summer "fall back" across neighborhoods. The authors find that children attending elementary school in high-poverty neighborhoods have smaller test-score gains than children from low-poverty areas during the summer but not during the school year, suggesting that differences in home and neighborhood environments may be responsible for much of the variation in achievement across neighborhoods. This pattern is presumably not explained by differences across areas in summer school attendance, since children from high-poverty areas are presumably more likely to attend summer school than those from low-poverty areas.

of a 1970s housing experiment suggest that providing rental subsidies to families who already live in private-market housing seems to generate little change in neighborhood quality. In contrast, MTO families relocate to lower-poverty areas compared with controls, even those families who are assigned to the Section 8–only comparison group. One reason for the difference in mobility outcomes may be the relatively brief eligibility period associated with the 1970s program. Another explanation is that MTO participants are a self-selected group of families who have expressed a desire to move.⁴⁹

In any case, the data from MTO suggest that offering housing vouchers to public housing residents may help at least some families who wish to move to lower-poverty neighborhoods to do so. Previous research suggests that the average unit cost is no higher with housing vouchers than with public housing or Section 8 project-based housing.⁵⁰ Thus if the government is committed to providing housing assistance to poor families in some form, the additional budgetary cost of shifting families from public housing to vouchers may be minimal.

While the cost differences between project-based housing and housing vouchers may be small, the differences in the benefits to society may be substantial, particularly if voucher recipients are provided with assistance and incentives to move to lower-poverty areas. Our Baltimore MTO data suggest that even young children assigned to the Section 8–only comparison group (whose families are not constrained to move to very low-poverty areas) experience an improvement in reading test scores of one-quarter of a standard deviation relative to control children, which may increase lifetime earnings by as much as \$9,600 for boys and \$7,900 for girls. MTO also appears to reduce violent criminal behavior by teens (which may be offset somewhat by an increase in property thefts), improve the health of children and their parents, and reduce welfare receipt.⁵¹

49. Struyk and Bendick (1981); Mills and Lubuele (1997). Other possible explanations for the difference across programs in residential mobility include potential differences in housing market conditions between the 1970s and the 1990s or differences in the responsiveness of residents of public versus private market housing to rental subsidies. Some support for the importance of family preferences for mobility rather than these other explanations comes from Jacob's (2000) study of Chicago families who have been involuntarily displaced from public housing buildings that are scheduled for demolition. Jacob finds that these families experience only modest changes in neighborhood conditions, which in turn do not translate into detectable changes in their children's educational outcomes.

50. Weinberg (1982); Olsen and Barton (1983); Mayo (1986); Shroder and Reiger (2000).

51. Krueger (1999); Katz, Kling and Liebman (2001); Ludwig, Duncan and Pinkston (2000); Ludwig, Duncan and Hirschfield (2001).

Economic desegregation may also be achieved through more indirect means. For example, historically many suburban townships have tried to limit the availability of low-cost housing through zoning restrictions. State-level judicial decisions that force municipalities to zone more affordable housing may help open up the suburbs to low-income families. Policies to reduce residential segregation by race and ethnic group may also achieve some degree of economic desegregation since African Americans and Hispanics are more likely than whites to live in high-poverty areas, even after accounting for differences across racial and ethnic groups in poverty rates. Interventions that may help reduce racial segregation in housing markets include stepped-up enforcement of fair housing laws.⁵² More research is needed to determine whether the impacts of such policies on children's educational outcomes would be similar to those reported here for MTO participants.

More generally, a systematic assessment of the benefits and costs of large-scale programs to move poor families from high- to low-poverty neighborhoods requires information about program effects on families who remain behind in central city areas, as well as on those who live in host neighborhoods. If neighborhood effects are proportional to changes in neighborhood attributes, a possibility that we cannot reject in the MTO data, any improvements in outcomes experienced by poor children who move will be offset by negative effects on children in host neighborhoods. Put differently, if neighborhood effects are linear, then reducing the degree of economic segregation will not affect the overall volume of social problems.⁵³ Society may still wish to expand residential-mobility programs in this case if gains to poor families are given more weight than are losses by affluent families, although more evidence is necessary for understanding the magnitude of any such trade-offs. Despite (or perhaps because of) these uncertainties, the estimated effects of the MTO program on children's educational and other outcomes are large enough to motivate increased research and policy attention to new housing-mobility efforts.

52. Kirp, Dwyer and Rosenthal (1995); Mills and Lubuele (1997); Jargowsky (1997); Yinger (1998).

53. Jencks and Mayer (1990).

Comments

Jeffrey Kling: Most social scientists believe that the social environment influences children's educational achievement. The importance of residential location in affecting children's education, however, is a subject on which there is conflicting evidence.⁵⁴ Given the increase in the spatial concentration of poverty, however, this question has taken on increasing urgency. Study of this issue has been hampered for at least two main reasons. First, the range of variation in residential location is often limited among otherwise similar children. Second, the fact that observably similar children (for example, same age and demographics) are living in different locations may reflect choices made by their families that are indicative of factors unobserved by an analyst. It is difficult to credibly identify the importance of residential location as distinct from those unobserved factors.

The research design in this paper uses a randomized demonstration program known as Moving to Opportunity (MTO) to address both of these issues. Section 8 rental vouchers from the Department of Housing and Urban Development (HUD) were provided by lottery to public housing residents. Some vouchers were valid only in low-poverty neighborhoods and were bundled with counseling services (the experimental group), while others could be used to move to any location (the Section 8 comparison group). Those who did not receive a voucher (the control group) retained their eligibility to continue to live in their original public housing project unit. The consequences of this design for research are that similar groups of families are living in vastly different neighborhoods, and that the randomization implies that, on average, the groups offered and not offered the vouchers through the lottery will be similar in observable and unobservable characteristics. This is a tremendous

54. Jencks and Mayer (1990).

advantage for these authors in having potentially convincing results that could identify the impact of residential location on educational outcomes.

Other results using MTO data, collectively summarized in a volume edited by John Goering and Judie Feins, indicate that the impact of residential location on children may be particularly important. Research from the New York site indicates reductions in child behavior problems, in addition to the research cited by the authors on reductions in teen criminal behavior and children's physical health and behavior.⁵⁵ None of the other existing studies using the MTO demonstration effectively studies educational outcomes. This novel and innovative paper addresses a critical gap in our knowledge.

Besides the randomization of the housing voucher offers, the other principal element of this research design is the linkage of MTO participants to administrative data on educational outcomes, such as test scores, grade retention, absences, and disciplinary actions. When Jens Ludwig first discussed the idea for this project with me in 1995, I was frankly skeptical that these data could be produced, mainly because of the multitude of government agencies involved. So I am pleased to be able to congratulate this team of researchers for truly creating new knowledge through their combination of data entrepreneurship and analytical skill.

Use of administrative records in this research does have some fundamental limitations, however. For example, the data indicate increased grade retention and suspensions among teens in families offered vouchers through the MTO program. This may accurately describe the experiences of these teens. Yet the schools attended by those who have moved through the MTO program have changed at the same time as their residential location. It is not clear whether administrative records are indicative of different behavior by the youth or of different standards being applied to the same behavior in different schools. My prior intuition was that the role of different standards could be important, and the authors provide some evidence in support of this idea. Thus, I agree with the authors' conclusion that the results on grade retention and suspensions for teens are difficult to interpret.

Another limitation is that many children do not have records of test scores. The authors are very clear about the reasons for missing data, including problems linked to test records using individual identifiers, schools testing children with differing frequency, and students missing tests because of absence or special education classification. Among children ages 5–12, the

55 . Goering and Feins (2001); Leventhal and Brooks-Gunn (2001); Ludwig, Duncan, and Hirschfield (2001); Katz, Kling, and Liebman (2001).

result is that there is substantial missing data for the sample in the post-randomization time period. Depending on the measures, test scores are available for 46 percent (Comprehensive Test of Basic Skills, math), 53 percent (CTSB, reading), and 26 percent (Maryland Functional Test, reading) of the sample. Although data missing at random would not affect the results of these analyses, these reasons are not random and may well be related to residential location itself.

A number of sensitivity analyses have been provided by the authors to assess the potential impact of missing data on the results. There is substantially more missing data for the Section 8 comparison group than in the other two groups, for example, which could be a source of bias. It is reassuring, therefore, to find that these differences, at least for test scores, appear to be largely driven by differences in the ages and the pre-program characteristics of children—which can occur by chance in any lottery with a small sample.

The results, however, do display some instability. In some cases, the results depend on the control variables used or the estimation method.⁵⁶ Even though the data are generated from a randomized experiment, nonexperimental methods must be used to address the missing data issues, and these methods are subject to standard criticisms about the potential importance of functional form and omitted variables.

To give a concrete example of how missing data could lead to bias, consider families in the experimental group, whose probability of using an offered MTO voucher to move is about 20 percentage points higher for those with valid CTSB scores than for those with missing scores. This clearly indicates that the test score data are not missing at random but missing in a manner directly related to residential location choice. Now consider the counterfactual counterparts of these families in the control group (the “control compliers” who would have moved though MTO if offered a voucher). For unbiased

56. Specifically, the sensitivity analysis of the CTSB math score for the Experimental group indicates that adding statistical controls for pre-program student and family characteristics in addition to age increased the estimated intent-to-treat (ITT) effect from statistically insignificant 4.2 percentile points to a statistically significant difference of 7.5. However, the treatment-on-treated (TOT) effect estimated with covariates is not statistically significant, indicating sensitivity to the estimation method.

For the Section 8 Comparison group CTSB reading score, there is almost no difference between the estimated ITT and TOT effects. Since about 15 percent of this group did not move through the MTO program, we would expect the TOT estimate to be larger. As noted by the authors, the TOT will simply be the ITT divided by the compliance rate when covariates are used that are uncorrelated with treatment group assignment. The fact that this does not hold indicates that the choices of covariates matter and that the results are at least somewhat sensitive to the estimation approach.

estimation, given the authors' evidence that there is selection in mobility and that the compliers do differ from noncompliers, experimental and control compliers would need to be equally over-represented among those with valid test score data. This would not be true if the over-representation of experimental program movers was because of more extensive testing in schools outside the center city (not attended by the control compliers). Although further evidence would be needed to ascertain whether this potential source of bias is important, the authors have not refuted that it could be.

I believe it should be clearly acknowledged that a wide range of biases is possible given the large extent of missing data—which may be missing for systematically different reasons across the MTO groups. The direction of most biases from the missing data is difficult to assess and could be positive or negative. Given the extent of the missing test scores, the small sample sizes, and the focus on outcomes in one city, I also believe we should be cautious in our interpretation of these results.

I do find the results in this paper highly suggestive and a very valuable contribution to our understanding of the impact of residential location on educational outcomes, although far from conclusive. These results should undoubtedly encourage HUD to make the study of children's educational achievement a top priority in their planned interim evaluation of MTO next year. To provide more conclusive evidence, I recommend that consistent data from all five cities in which the MTO demonstration took place be collected on children's educational achievement and the learning environment in their schools.

Katherine M. O'Regan: This is an extremely well-done piece of research on an incredibly important topic. It provides strong evidence for the impact of mobility and improving neighborhoods on families in public housing. The paper addresses one avenue of impact, through educational outcomes of the children, for a specific type of policy option: public housing residents who wish to move to private-market housing and do so through subsidies that must be used in neighborhoods with low poverty rates (at least for one year).

Before the collection of Move-to-Opportunity (MTO) studies currently under way, we have operated with very imperfect assessments of the likely effect of such policies (because of limited data and methodological issues). I believe the benefits of this paper go beyond the much improved evidence for this program option, informing a broader array of policies in somewhat different contexts. In forming policy we are almost always dealing with imperfect

information on likely impact, on whom, and how. Besides the strong evidence on the narrow program gained in this work, there are less strong, but suggestive, findings that can be gleaned from the work. The authors have included some extensions in their work. My comments focus on perhaps a wider range of policies, or on helping current policymakers tailor these results to their specific urban circumstances.

General structure. The basic structure of the research is an experimental design in which the authors have two comparison groups. The experimental group receives the treatment of a voucher, with some limits on the geography and the timing of its use, and with some additional services to aid in the search process. The first comparison group also receives a treatment, but the treatment differs in two critical ways: households are not limited to low-poverty neighborhoods, and these households do not receive additional services. Finally, the control group receives no treatment at all.

Most of the empirical work then focuses on two classic forms of impact. First, the intent-to-treat effect (ITT), which contrasts outcomes for the entire group that is offered treatment to the entire comparison group(s). And second, the treatment-on-treated effect (TOT), which looks solely at changes in outcomes for those who have accepted treatment compared with the appropriate comparison group(s). Each of these effects is important as each one addresses somewhat different policy questions.

Although the authors are discussing the impact of improving neighborhoods, technically the treatment assessed under the TOT is one of changing residence, period. It is not quite one of improving neighborhoods, since enforcement of the neighborhood restrictions may not be complete, and it is not necessarily one of improving schools. Movers include people who have moved out of public housing but not to improved neighborhoods (very few for this sample), people who have moved from the neighborhood but potentially not the school (this may also be quite small for this sample), and those who have improved both neighborhood and school. Although the current experiment may primarily include only the final group, it is worth thinking about what could be learned by distinguishing among these alternative treatments.

Alternative treatments. The authors note in their conclusion that one cannot directly test whether the observed outcomes are because of changes in the schools attended by the children, changes in their environments, or some combination. The nature of the effects (reading scores in particular) are suggestive of more than just school changes.

It might be informative to try to push the assessment further to consider alternative views of the “treatment” and compare the TOT for children who experience at least two possibilities: treatment 1, changing neighborhoods only (same school); treatment 2, changing neighborhoods and schools.

If it is the schools themselves that matter, and only the schools, treatment 1 should have no effect on recipients’ educational outcomes. And treatment 2 is somewhat closer to how people may be interpreting the current TOT numbers in the paper. It would be informative to have estimates of these impacts. This exercise is similar to the one already conducted by the authors, distinguishing the academic outcomes of students by the household’s motivation for moving. These subset analyses are not quite as “clean” as focusing on the ITT effects, but they help push the data as far as possible.

Generalizing beyond Baltimore. The authors are careful to place their results within the context of Baltimore, or when possible, the collection of MTO cities. Here I consider ways to improve our ability to generalize from these results. At least three general categories of characteristics affect the size of the program’s impact and the likelihood that someone will participate in the program. Those issues affect how applicable these results are to another environment. The categories are the characteristics of the people in the program; the characteristics of the neighborhoods and schools they go to; and the characteristics of the neighborhoods and schools they leave.

The first category is explicitly discussed in looking at the take-up rates (likelihood of accepting treatment). The authors find that the pre-program academic performance does not affect the take-up rate. They also provide information in table 4 on a variety of characteristics that could potentially affect take-up (and size of impact) that could vary across cities. The related discussion helps policymakers outside of Baltimore assess what proportion of their target population is likely to participate in such a program.

However, it is likely that more than the household’s characteristics will affect take-up and impact. The authors note that households that seem to have more to gain or less to lose are more likely to participate in the treatment program. Along these lines, the quality of the baseline neighborhood (and the potential new neighborhoods) should also affect participation. Although table 5 presents evidence on the differential improvement in neighborhoods attained for each group, it does so in aggregate. Disaggregating by mover status would help other jurisdictions predict participation rates (and impacts) for their areas and possibly explain some of the variation in participation across MTO sites.

For example, one might think that characteristics of neighborhoods around public housing would greatly affect whether people choose to participate in the program. Nationally, about 20 percent of public housing units with families are located in neighborhoods with jointly above-average rates of various negative characteristics, classified as underclass by Ann Schnare and Sandra Newman.⁵⁷ Sixty-eight percent of Baltimore's family public housing units are in such neighborhoods (second highest in the country in cities with populations of 500,000 or more). This may create a much greater incentive to move in Baltimore than in other cities, but we can not quite tell that from the data in their current form.

Control group and HOPE IV. It would also be helpful to break out those control group families affected by the HOPE VI redevelopment project. While little can be done to have a truly unaffected control group, we can learn a little more by looking at this group specifically.

If one looks at the net change in neighborhood distributions, and assumes they capture most gross flows, approximately forty-nine families moved from those neighborhoods with the highest poverty rates (60 percent or more). If all HOPE VI families made this move, then almost all of the improvement in neighborhoods observed in the control group is related to the HOPE VI program. It is worth identifying this group for several reasons.

First, not all areas have Hope VI relocation as their counterfactual. Such areas might then expect much less mobility among their public housing recipients. Second, some of this neighborhood improvement is transitory. Some fraction of this group will be relocating back to the two HOPE VI sites, Lafayette Courts and Lexington Terrace. In fact, 152 families did return to Lafayette after its redevelopment.⁵⁸

Return rates appear to differ dramatically by HOPE VI development, depending in part on the mobility programs established during relocation, the availability of Section 8 and public housing alternatives, the number of redeveloped units (now that one-to-one replacement on site is not required), and whether any selection criteria are used for returning families. Again, this would differ by locality.

Furthermore, the HOPE VI redevelopment did provide the area with a wide range of additional neighborhood services, from a new police substation in the Lafayette Courts neighborhood, to a Boys and Girls club program, parenting programs, and mentoring programs. Pre- and postsurveys of perceived

57. Schnare and Newman (1997).

58. HUD (2000).

safety find a marked decline in the fear of crime. (It also resides in portions of Baltimore's Empowerment Zone.) There was also an increase in employment and a decrease in welfare use among residents of the housing project over this time.

So, while many stayed in this neighborhood, the quality of the neighborhood appears to have changed, perhaps dramatically. This is not much of an issue for the research at hand at this point, since the time periods barely overlap. It does matter for additional follow-up and for policy options. The programs considered in the experiment are moving with a voucher from public housing, and moving with extra counseling but restrictions on where one goes. This second program leads to bigger neighborhood changes and perhaps bigger impacts on education and more. But an alternative scenario is HUD's redevelopment of the public housing neighborhoods, changing neighborhoods but not necessarily schools.

Program modification. If at all possible, it would be nice to have some additional information on the loss of participants during the early stages of the counseling and search process. The program implemented is but one version of such a voucher program with mobility restrictions, and it would help to get a sense of which program characteristics really matter and which really cost in participation. The contrast with the voucher program is not quite the control one would like, as there are several differences between the programs: restrictions on use of the voucher, requirements for workshops, and the provision of counseling and support services. The authors do provide some summary information that suggests important differences. While half of those not participating in the experiment never started the voucher and workshop process, almost all of the Section 8 comparisons at least applied for the voucher. Of those remaining experimentals who did not find housing, about a quarter dropped out before completing their required workshops. This does suggest that each portion of the program may have its own "cost," and eventually we would hope to assess these components separately.

To push this point, all of the residents affected by HOPE VI were offered the same support for searching as other HUD certificate recipients. In Baltimore and the surrounding counties, this includes no restriction on the location of housing, but it turns out that Baltimore is part of a regional mobility program that *potentially* provides counseling and support services, geared in part to get households out of the poorest neighborhoods. The availability of such services may be most relevant to the HOPE VI voucher recipients, as they would be involved with HUD relocation staff as part of the HOPE VI project.

Some of the approaches taken by this regional mobility program appear similar to those services received by the experimental group, without the geographic requirements. The program reports that 68 percent of the families served moved to low poverty areas. It is possible that a primary avenue of impact in the MTO program is the counseling and support, rather than the limits on the re-location areas. In that case, perhaps a somewhat different version of this program could increase take-up rates with not much loss of impact per treated household.

One way to gain some insight into this issue (whether some modification in program design could improve the impacts) is by examining information on the participants in the MTO experiment group early on. The authors' breakout of when participants dropped out is helpful; additional information on who dropped out—and why—would be extremely useful. If it is not possible to interview some of these participants, perhaps interviews with the counselors with whom these participants interacted would be suggestive.

Conclusion. The entire body of work on the MTO experiments is providing the type of research greatly needed in the literature on neighborhood effects and mobility. This particular piece has focused on the academic impacts but has also attempted to push beyond the narrow (though quite strong) results of this one program option. Both activities provide a serious contribution to our knowledge.

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