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Neighborhood Poverty and Public Policy: A 5-Year Follow-Up of Children's Educational Outcomes in the New York City Moving to Opportunity Demonstration

Tama Leventhal
Johns Hopkins University

Rebecca C. Fauth and Jeanne Brooks-Gunn
Columbia University

Data from the Moving to Opportunity Program, a randomized mobility experiment in which a subset of low-income minority families living in public housing in high-poverty neighborhoods were given vouchers to move to low-poverty neighborhoods, were used to evaluate 1 policy approach for improving children's educational outcomes. Four hundred twenty-five New York City children were seen 2½ and 5 years following relocation (mean age = 14.64 years, *SD* = 3.21 years). Analyses examining program effects on 5-year educational outcomes, accounting for 2½-year outcomes, revealed that program effects on adolescent boys' achievement found at 2½ years were not sustained at 5 years. Rather, male and female youths 14–20 years of age in low-poverty neighborhoods reported lower school grades and engagement relative to youths in high-poverty neighborhoods. From a policy standpoint, the complexity of enhancing low-income minority children's educational outcomes is underscored by the multiple dynamics involved—family, neighborhood, housing, and school.

Keywords: neighborhood, communities, poverty, achievement, social policy

Despite the improving economic circumstances of children during the past decade, racial–ethnic disparities persist. African American and Latino children are approximately three times more likely than European American children to come from poor families (i.e., those whose income falls below a federally established threshold) (Brooks-Gunn & Duncan, 1997; Duncan & Magnuson, 2005). At least in part because their families are poor, minority children, especially those residing in urban areas, are also more likely than their nonminority peers to live in poor neighborhoods (O'Hare & Mather, 2003; U.S. Department of Health and Human Services, 1999), including neighborhoods that are extremely poor (Jargowsky, 1997; Kahn & Kamerman, 1996). These impoverished neighborhoods are often characterized by high unemployment rates, predominantly minority populations, dense public housing, crime and violence, and social isolation (Massey & Kanaiaupuni, 1993; Wilson, 1996).

Research has amassed substantial evidence that poor children of all ages are more likely to display educational deficits (e.g., learning disabilities, grade retention, suspension or expulsion, dropping out of high school) than nonpoor children (Brooks-Gunn & Duncan, 1997). Although the literature is less clear with respect to neighborhood poverty, research is accumulating on the negative educational impacts of residence in a poor neighborhood (Brooks-Gunn, Duncan, & Aber, 1997; Jencks & Mayer, 1990; Leventhal & Brooks-Gunn, 2000). Indeed, in the 1990s, a number of researchers examined associations between neighborhood structural characteristics, such as poverty rate, median family income, unemployment rate, and percent minority, among other measures, from the U.S. Census, and children's and youths' educational outcomes (such studies, however, cannot disentangle neighborhood from school effects; e.g., Brooks-Gunn et al., 1997; Brooks-Gunn, Duncan, Klebanov, & Sealand, 1993; Chase-Lansdale & Gordon, 1996; Crane, 1991; Halpern-Felsher et al., 1997; Jencks & Mayer, 1990; Klebanov, Brooks-Gunn, & Duncan, 1994; Leventhal & Brooks-Gunn, 2000). Although family-level socioeconomic factors typically accounted for more variance in child outcomes than did neighborhood-level socioeconomic factors, residence in high socioeconomic status or affluent neighborhoods consistently demonstrated positive associations with children's and youths' school readiness and achievement outcomes, especially for adolescent boys. In these studies, neighborhood effects accounted for about 5%–10% of the variance in child outcomes (Leventhal & Brooks-Gunn, 2000). Given that minority children are more likely than nonminority children to confront poverty within the home and in their neighborhoods, growing public policy

Tama Leventhal, Bloomberg School of Public Health, Johns Hopkins University; Rebecca C. Fauth, National Center for Children and Families, Teachers College, Columbia University; Jeanne Brooks-Gunn, National Center for Children and Families, Teachers College, Columbia University, and College of Physicians and Surgeons, Columbia University.

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Correspondence concerning this article should be addressed to Tama Leventhal, Institute for Policy Studies, Johns Hopkins University, 3400 North Charles Street, Wyman Building, Baltimore, MD 21218. E-mail: tleventhal@jhu.edu

concern has centered on low-income minority children's educational prospects.

Residential Mobility Programs

One policy approach for potentially improving the life chances of low-income minority children is to expand their families' residential options by offering these families low-cost housing dispersed throughout nonpoor neighborhoods in lieu of public housing concentrated in poor neighborhoods (Briggs, Darden, & Aidala, 1999; Fauth, 2004; Fauth, Leventhal, & Brooks-Gunn, 2004; Goetz, 2003; Popkin, Gwiasda, & Buron, 2000). These so-called dispersal programs have generally taken one of two forms: (a) tenant-based assistance or (b) project-based assistance. Tenant-based approaches operate through the federal Housing Choice Voucher program (formerly the Section 8 program) and provide low-income families with vouchers (or subsidies) to be applied toward the payment of rent in the private housing market. In the case of poverty deconcentration programs, families are often given counseling and assistance to move to nonpoor neighborhoods (Goering & Feins, 2003; Rubinowitz & Rosenbaum, 2000). Alternatively, project- or unit-based assistance, a supply-oriented strategy, entails purchasing land and constructing low-rent government-subsidized housing in neighborhoods varying in socioeconomic composition (Fauth et al., 2004). These approaches are based on the link between housing, neighborhood residence, and family economic circumstances. An underlying premise of such programs is that reducing concentrations of poverty through expanded housing choice leads to improvements in families' economic well-being vis-à-vis better job opportunities for parents, more advantaged social networks and role models, and greater resources (including quality schools), which in turn are expected to enhance children's development (Goetz, 2003; South & Crowder, 1998; Turner, Popkin, & Cunningham, 2000).

Moving to Opportunity for Fair Housing Demonstration and Previous Studies of Children's and Youths' Educational Outcomes

Background

The present study focuses on one initiative using a tenant-based approach—the Moving to Opportunity for Fair Housing Demonstration (MTO)—sponsored by the U.S. Department of Housing and Urban Development (HUD) in five urban sites (Goering & Feins, 2003). In this randomized housing mobility experiment, low-income, predominantly minority families with children were recruited from public housing in high-poverty neighborhoods and assigned to one of the following three groups: (a) the low-poverty voucher group, who received vouchers and special assistance to move into private housing in low-poverty neighborhoods only; (b) the traditional voucher group, who received unrestricted vouchers to move into private housing in neighborhoods of their choice; or (c) the control group, who remained in public housing. The program's goals are multifaceted and include alleviating intergenerational economic disadvantage by improving children's and youths' educational outcomes.

A major impetus for the MTO program was findings from a quasi-experimental study of children who participated in the Gautreaux Program (Rubinowitz & Rosenbaum, 2000). The Gautreaux Program stemmed from a 1976 court order to desegregate Chicago's public housing. As a result of the court mandate, low-income African American families in public housing and on the waiting list for public housing were given housing vouchers—in a quasi-random manner based on housing availability—to move into private housing distributed throughout the entire Chicago metropolitan area. At the outset, it was authorized that 75% of the families had to relocate out of the city limits to the suburbs, and by 1991, all families were required to move to the suburbs. A 10-year follow-up of families found that youths who moved to the more affluent suburbs were more likely to complete high school and attend college than their peers who remained in the city in poorer urban neighborhoods (Kaufman & Rosenbaum, 1992; Rosenbaum, 1995).

In addition to its policy significance in potentially reducing the deleterious effects of poverty on children's and adolescents' development, the MTO program has served as a landmark study of neighborhood income effects because its experimental design overcomes the problem of selection bias in most neighborhood research (Duncan, Connell, & Klebanov, 1997). In general, the field of neighborhood research has been plagued by the lack of appropriate study designs to isolate the effects of neighborhood residence. Most extant neighborhood research incorporates family-level sociodemographic characteristics (e.g., income, parent education, family structure) into analytic models to capture variation in child outcomes that is due to family and not neighborhood effects. However, unmeasured family or school characteristics, not included in analyses, that likely influence neighborhood choice, such as parent mental health or school quality, may exist (Duncan et al., 1997; Tienda, 1991). Exclusion of these omitted variables may lead to biased neighborhood effects. In the MTO program, the random assignment of low-income, primarily minority families into neighborhoods of varying levels of income minimizes problems of selection bias encountered in most neighborhood research.

By way of background, HUD initially opted to fund individual teams of researchers to conduct site-specific evaluations. The aim of this strategy was to inform a planned cross-site (Baltimore, Boston, Chicago, Los Angeles, and New York City) evaluation at the 5-year mark (Goering et al., 1999). The current study entailed an extended follow-up of the New York City MTO site. Leventhal and Brooks-Gunn (2004) previously reported results of a 2½-year evaluation of the New York City site, and this study incorporated data from the recently completed 5-year follow-up to examine longer term educational outcomes as well as change over the 2½- to 5-year postprogram period.

Site-Specific Early Impact Studies

The New York City site 2½-year follow-up was the only site evaluation to collect child- and parent-report data on children's and adolescents' educational outcomes that included the administration of standardized tests. Findings revealed that boys 11 to 18 years of age (at the 2½-year follow-up) whose families received vouchers to move to low-poverty neighborhoods exhibited considerably higher reading and math achievement scores (i.e., 6 to 19 points) compared with peers in the control group whose families did not receive vouchers and remained in high-poverty neighborhoods (Leventhal & Brooks-Gunn, 2004). No corresponding pro-

gram effects on achievement were found for adolescent girls or for elementary school-age children (6 to 10 years). It is interesting to note that program effects on school characteristics were not as large as those found for neighborhood characteristics. Adolescent boys in the low-poverty voucher group attended schools with only somewhat lower percentages of poor and minority students (assessed by the National Center for Educational Statistics) compared with control peers who remained in high-poverty neighborhoods. An examination of potential mediators of program effects on low-poverty voucher adolescent boys' achievement revealed that school composition (percentages of poor and minority students) did not account for the observed program effects. Rather, adolescent boys in the low-poverty voucher group were attending safer schools (as rated by parents) as well as reportedly spending more time on homework compared with adolescent boys in the control group. These two factors, in turn, partially accounted for the program effects on adolescent boys' achievement. For children ages 6 to 10 years (at the 2½-year follow-up), boys in the low-poverty voucher group were marginally more likely to repeat a grade than control boys. Compared with control peers, the program had no effect on girls who moved to low-poverty neighborhoods and minimal effects on children and youths who moved to neighborhoods of their families' choice (traditional voucher group), which were predominantly poor neighborhoods.

The Baltimore site was the only other MTO site to examine early program impacts on children's educational outcomes; this study relied on public school records (Ludwig, Ladd, & Duncan, 2001). Program effects followed a somewhat different pattern than those obtained in New York City. Specifically, Ludwig et al. reported that children who were 5 to 11 years old at random assignment (or 8 to 14 years old at the 2½-year follow-up) in the low-poverty voucher group had higher reading and math achievement scores than did control children who remained in public housing. However, children ages 12 to 18 years old at random assignment (or 15 to 21 years old at the 2½-year follow-up) who moved to low-poverty neighborhoods performed similarly to control peers on reading and math achievement tests and were more likely to be retained a grade compared with controls. No gender differences in program effects were found for children or adolescents. Like the New York City sample, program effects were minimal for children and youths whose families received vouchers to move to neighborhoods of their choice.

Cross-Site 5-Year Impact Study

Despite the positive effects of moving from high- to low-poverty neighborhoods on male adolescents' achievement 2 to 3 years into the New York City program, a recent 5-year five-site evaluation reported almost no program effects on educational outcomes for male youths who were 15 to 20 years old (at the 5-year follow-up), which is more in line with the early Baltimore findings (Kling & Liebman, 2004). Somewhat surprisingly, the few program effects found for all children 5 to 19 years of age (at the 5-year follow-up) were generally negative (Orr et al., 2003). Specifically, low-poverty voucher boys had lower reported grades compared with control peers who remained in high-poverty neighborhoods (Orr et al., 2003). In addition, like the early New York City follow-up, low-poverty voucher children 8 to 11 years of age (at the 5-year follow-up) were more likely to have repeated a grade than control children. Low-poverty voucher children and youths were attending more advantaged schools at the 5-year follow-up, as assessed by percentage of minority and low-income students and average achievement test scores, but these effects continued to be relatively modest (Sanbonmatsu, Brooks-Gunn, Duncan, & Kling, in press). Among traditional voucher children and youths, who moved to neighborhoods of their families' choice, program effects at 5 years were also primarily negative. Compared with control peers, traditional voucher boys had lower grades, and children 8 to 11 years of age (at the 5-year follow-up) had lower math achievement scores (Sanbonmatsu et al., in press).

Current Study of Children in the New York City MTO Site

Thus, it is unclear how to resolve the beneficial program effects for low-poverty voucher adolescent boys' achievement reported in the New York City 2½-year follow-up with the lack of program effects reported in the 5-year five-site follow-up. When examining the same youths, will these effects be sustained 2½ years later when youths are older (14 to 20 years of age)? In this article, we examine this issue by using longitudinal data on a subsample of children and youths from the New York City MTO site seen at both the 2½- and 5-year follow-ups. In addition, the question of potential age differences in program effects remains. In the New York City 2½-year follow-up, no program effects were reported for elementary school boys' achievement; however, a trend was found for low-poverty voucher boys to be more likely to repeat a grade than control boys. Will a similar effect remain when these elementary school-age boys are seen 2½ years later (9 to 13 years of age)? Will these boys demonstrate improvements in achievement now that they are entering adolescence?

Using this unique longitudinal subsample of New York City MTO children and youths, we considered a range of educational outcomes. Our original 2½-year follow-up focused on school-age children who were 6 to 18 years of age. At the 5-year follow-up, these youths were 9 to 20 years of age. In line with previous work, outcomes examined include achievement tests, school behavior problems, participation in gifted and special education programs, grade repetition, homework time, suspensions or expulsions, and school engagement. Consistent with the earlier study, we investigated program effects on educational outcomes at the 5-year follow-up independently for children who were elementary school-age (6 to 10 years) and for adolescents (11 to 18 years) at the 2½-year follow-up and examined whether these effects differed by child gender.

On the basis of results of the earlier follow-up, we expected that adolescent boys who moved to low-poverty neighborhoods would show continued improvements in achievement test scores at the 5-year follow-up compared with youths who remained in high-poverty neighborhoods. If program effects are positive in nature, we may see beneficial program effects of moving from high- to low-poverty neighborhoods on other educational outcomes as well, such as homework time, suspensions or expulsions, school engagement, school behavior problems, and participation in gifted and special education programs. That minority boys perform less well than minority girls on a variety of achievement outcomes is well documented (Horn, Peter, & Rooney, 2002; Journal of Blacks in Higher Education, 1998; Slater, 1994). Several years into the MTO program, low-poverty voucher boys were performing at levels

comparable to others their age, including their female peers (i.e., average achievement scores on age-standardized tests of approximately 100). These results indicated that the MTO program lessened the gender divide in achievement by raising the test scores of adolescent boys in the low-poverty voucher group compared with control peers who remained in poor neighborhoods. We expected these initial achievement gains to facilitate positive educational outcomes for low-poverty voucher boys several years later.

The impact of moving to low-poverty neighborhoods on adolescent girls' educational outcomes (vs. staying in high-poverty neighborhoods) was virtually nonexistent at both the 2½- and 5-year follow-ups, calling into question whether any program effects—positive or negative—would be found for them. The nonexperimental literature also suggests that neighborhood socioeconomic effects may be more pronounced for adolescent boys as compared with adolescent girls (Leventhal & Brooks-Gunn, 2000). The source of these differential gender effects is not clear but may be due to boys being granted greater exposure to neighborhood influences than girls (Ensminger, Lamkin, & Jacobson, 1996; Entwisle, Alexander, & Olson, 1994).

Likewise, program impacts on elementary school-age children have been limited across follow-ups, and those effects obtained were mixed in direction. Thus, it was not clear whether any program effects would be found among children in the current study. In theory, effects may be positive because children have spent the large majority of their elementary school years in low-poverty neighborhoods, thus decreasing their exposure to high-poverty neighborhoods while increasing exposure to more favorable neighborhood and school conditions. Yet, findings to date have not generally supported this hypothesis and indicate that effects may be negative, if present at all. No gender differences were anticipated.

Among traditional voucher children and adolescents who moved to neighborhoods of their families' choice, which were largely poor, we anticipated few program effects on educational outcomes on the basis of findings reported at both 2½ and 5 years. However, if effects are found, they are likely to be negative because of the disruption associated with moving not offset by notably improved neighborhood or school conditions. According to the residential mobility literature, moving is negatively associated with children's school-related outcomes (Astone & McLanahan, 1994; Coleman, 1988; Swanson & Schneider, 1999; Temple & Reynolds, 1999). In fact, recent work revealed that youths who moved out of public housing units in extremely impoverished neighborhoods in Chicago (as a result of project demolition) were more likely to drop out of high school in the first 3 years following relocation than youths who remained in the same housing project in nondemolished buildings (Jacob, 2004). Thus, the potential benefits of relocating out of high-poverty neighborhoods may be suppressed in the face of cumulative stressors (i.e., moving, potential school change, persistent neighborhood poverty, continued school disadvantage) (see Table 1).

Method

The selection of participants, design, and methods of the national MTO evaluation have been described in great detail elsewhere (Feins, Holin, & Phipps, 1996; Goering et al., 1999) and will be summarized here.

Design and Description of the MTO Program

The MTO program is a randomized housing-relocation demonstration sponsored by HUD in five urban sites. Families who resided in public housing or received project-based assistance under the Section 8 program and who had at least one child less than 18 years of age were eligible to participate. The Section 8 program allocates vouchers for rent subsidies to purchase program-approved housing in the private market. In the MTO program, the voucher offer was good for 60 days from the date of issuance, although the local housing authority had the discretion to extend the offer. Participants were recruited from housing projects located in census tracts with poverty rates in excess of 40%, as measured by the 1990 U.S. Census (Goering, Teodoro, & Carnevale, 1996). When compared with families in public housing who did not volunteer for the MTO program, families who signed up for the MTO program appear to be more disadvantaged than their public housing counterparts who did not volunteer for MTO (Goering et al., 1999).

A randomized controlled design was used such that families who volunteered for the program were assigned to one of three conditions: (a) the experimental or treatment group, who received Section 8 housing vouchers and special assistance to move only to low-poverty neighborhoods (census tracts with less than 10% poor residents according to the 1990 U.S. Census; low-poverty voucher group); (b) the comparison group, who received Section 8 housing vouchers under the regular geographically unrestricted program and no special assistance (traditional voucher group); or (c) the control group, who did not receive vouchers or special assistance but continued to receive project-based support (control group). The special assistance received by families in the low-poverty voucher group was provided by local nonprofit organizations and varied across sites. In general, these services entailed assisting families with finding housing and overcoming obstacles to obtaining housing in low-poverty neighborhoods, as well as working with landlords unfamiliar with the Section 8 program or renting to families from public housing. Low-poverty voucher families, however, were required to stay in low-poverty neighborhoods only for the tenure of their first 1-year lease, and after that time, their vouchers could be used in neighborhoods of their choice.

Random assignment was conducted by using specially designed software for the MTO program. Assignment to each condition was based on an expected treatment take-up (or compliance) rate of 25% at each site—that is, a quarter of families offered vouchers from the program were expected to use them to move to new neighborhoods. This estimate was based on prior housing mobility programs for low-income families (Rubinowitz & Rosenbaum, 2000).

Baseline interviews were conducted from 1994 to 1999—prior to random assignment and relocation of movers—with one adult family member identified as the head of household. During this structured interview, primarily demographic information was obtained, with limited data gathered on each household member, including children. During the initial years of the program, different teams of researchers were contracted by HUD to conduct site-specific follow-up evaluations (Goering & Feins, 2003). Approximately 5 years after a majority of mover families had relocated, a cross-site evaluation was conducted by using a uniform protocol (Orr et al., 2003).

New York City MTO Evaluation

This study reports on a follow-up evaluation of the New York City MTO site. Families in the New York City MTO were recruited from 14 targeted housing projects located in 12 census tracts with an average poverty rate of 47%. The average household income in these projects was $11,771; 32% of families in these projects received public assistance; and all residents were racial–ethnic minorities (Goering et al., 1996).

2½-year follow-up. Between 1998 and 2000, approximately 2½ years after baseline interviews were completed, 550 families from a target sample of 794 (families with completed baseline data as of August 1997)

Table 1

Summary of Studies From the Moving to Opportunity for Fair Housing Demonstration (MTO) Used to Examine Neighborhood Effects on Children's and Adolescents' Educational Outcomes

Study	Method	Years postmove	Sample	Findings: LPV	Findings: TV	References
MTO—Baltimore	Collection of administrative school records on children from Maryland public school districts	2.5 years	1,243 mostly African American and Latino, 5–18 years old at randomization	Children (5–11 years old at randomization) LPV higher reading and math achievement scores than C LPV more likely to pass state reading exam than C Adolescents (12–18 years old at randomization) LPV more likely to repeat grade than C LPV marginally more likely to be suspended than C LPV marginally more likely to drop out of school than C	Children (5–11 years old at randomization) TV higher reading achievement scores than C Adolescents (12–18 years old at randomization) TV more likely to repeat grade than C	Ludwig, Ladd, & Duncan (2001)
MTO—New York City	Interviews with children and parents and administration of standardized achievement tests; school data from national database	2.5 years	586 African Americans and Latinos, 6–18 years old at interview	Children (6–10 years old at interview) LPV boys marginally more likely to repeat grade than C boys Adolescents (11–18 years old at interview) LPV boys higher reading and math achievement scores than C boys LPV boys spent marginally more time on homework than C boys	Adolescents (11–18 years old at interview) TV boys marginally higher reading achievement scores than C boys TV girls less likely to be suspended/expelled than C girls	Leventhal & Brooks-Gunn (2004)
MTO—5 site	Interviews with children and parents and administration of standardized achievement tests; school data from state and national databases	5 years	5,074 mostly African American and Latino, 6–20 years old at interview	Children ages 8–11 (at interview) LPV more likely to repeat grade than C Children ages 8–19 (at interview) LPV boys lower reported grades than C boys Children ages 15–20 (at interview) LPV girls marginally more likely to graduate or stay in school than C girls	Children ages 8–11 (at interview) TV lower math achievement scores than C Children ages 8–19 (at interview) TV boys lower reported grades than C boys Children ages 12–19 (at interview) TV less likely to repeat grade than C	Kling & Liebman (2004); Sanbonmatsu, Brooks-bunn, Duncan, & Kling (in press), Orr et al. (2003)

Note. LPV = low-poverty voucher; TV = traditional voucher; C = control.

participated in the New York City 2½-year follow-up evaluation (69% response rate). Families who participated in this follow-up did not significantly differ from nonparticipants on a range of baseline parent and family sociodemographic characteristics examined, including race–ethnicity, education, employment, and welfare receipt, and retention was comparable across the three conditions (Leventhal & Brooks-Gunn, 2004). Detailed in-home interviews and assessments were conducted with parents and up to two randomly selected children per household (Leventhal & Brooks-Gunn, 2003).[1] Parent interviews were administered in English or Spanish, and child interviews were administered in English only.

5-year follow-up. Between 2001 and 2002, 1,002 families from the New York City site that were randomly assigned through December of 1997 participated in a 5-year five-site follow-up evaluation (93% response rate).[2] Families with children between the ages of 9 and 20 years at the time of the 5-year follow-up study who did not participate in the 2½-year follow-up study ($n = 610$; 38% low-poverty voucher, 33% traditional voucher, and 29% control) did not differ in terms of baseline parent age, race–ethnicity, education, employment, and family welfare receipt from families who participated in both the 2½- and 5-year follow-ups. However, compared with families who participated in both follow-up evaluations, families who participated in only the 5-year evaluation were less likely to be married (9.2% vs. 2.8%, respectively) and had fewer children in their households ($M = 2.77$, $SD = 1.28$ vs. $M = 3.03$, $SD = 1.69$, respectively). At the 5-year evaluation, extensive in-home structured interviews were conducted with heads of households and up to two randomly selected children per household between the ages of 5 and 19 years (Orr et al., 2003). Parent interviews and child interviews were administered in English or Spanish, on the basis of preference, by using Computer-Assisted Personal Interviewing on laptop computers.

Sample Description

This study focused on 321 New York City families (low-poverty voucher, $n = 138$; traditional voucher, $n = 98$; and control, $n = 85$) with age-eligible children who were seen at both the 2½- and 5-year follow-ups; the average period between interviews for these families was 3.12 years ($SD = 0.33$) and did not significantly differ across groups.[3] Although the same children were not necessarily sampled in the New York City 2½-year follow-up and the five-site 5-year follow-up, the overlap was quite high. The current sample included 425 children who were school-age (6–18 years of age) at the time of the 2½-year follow-up and were reinterviewed approximately 3 years later (42% low-poverty voucher, 30% traditional voucher, and 28% control). Thus, extensive data on this subsample of children and youths are available at two time points.[4]

Across the three treatment groups, retention was higher for low-poverty voucher families than for traditional voucher and control families (81%, 65%, and 69%, respectively). With two exceptions, families included in both follow-up evaluations did not significantly differ from families seen only at the 2½-year follow-up on baseline parent and family characteristics, including parent age, sex, race–ethnicity, educational attainment, and marital and employment status (see Table 2). Specifically, families included in both the 2½- and 5-year follow-ups had parents who were less likely to graduate high school and had smaller family sizes compared with families included in the 2½-year follow-up only.

A few significant baseline differences were found within each treatment group as well, particularly among control families (see Table 2). For control families included at both follow-ups, parents were younger and had more children in their households compared with control families seen only at the 2½-year follow-ups. Control families seen at both follow-ups were also somewhat more likely to be African American and to receive welfare than controls seen only at the 2½-year follow-up. Traditional voucher families seen at both the 2½- and 5-year follow-ups had older parents and slightly fewer children than did traditional voucher families seen only at the 2½-year follow-up. For low-poverty voucher families, no significant differences in baseline characteristics were found as a function of attrition status.

A comparison of baseline characteristics reported in Table 2 revealed no significant differences between the treatment and comparison groups relative to the control group for the current sample (i.e., families seen at both the 2½- and 5-year follow-ups). At the time of baseline interviews, parents were in their mid-30s and approximately two thirds had a high school degree or graduate equivalency diploma. The sample was split evenly between African American and Latinos (a small number reported "other" for race–ethnicity). Mothers were interviewed in 99% of households, and 90% of households were headed by unmarried parents. Approximately one quarter of parents were employed at baseline, and about three quarters of families were receiving welfare at this time. On average, there were approximately three children per household. When asked their primary motivation for moving from their current neighborhood, a majority of parents (48%) reported getting away from drugs and gangs. The other major reason for wanting to move, cited by more than a quarter of the sample, was to get a larger or better apartment (27%).

Overall, 38% of families in the two voucher groups used the randomly assigned vouchers they were offered to move to new neighborhoods.[5] Across all five sites, the compliance rate was 47% for the low-poverty voucher group and 60% for the traditional voucher group (Goering et al., 1999). These take-up rates were substantially higher than HUD's expected take-up rate of 25%. Of note is that compliance rates for housing programs, such as MTO (Rubinowitz & Rosenbaum, 2000), are lower than those typically found for early child education programs, where take-up rates are often 80% or higher (Gomby, Culross, & Behrman, 1999; Love et al., 2002). In the case of the MTO program, the relatively low take-up rate indicates that many families in the two voucher groups did not move and remained in their original high-poverty neighborhoods in public housing.

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[1] At both the 2½- and 5-year follow-ups, telephone interviews were conducted with a few families who moved out of the New York City area (or other site areas at the 5-year follow-up).

[2] In contrast to most studies, the response rate improved over time from the 2½-year follow-up to the 5-year follow-up. This situation was due in part to substantial resources provided by HUD to ensure a high response rate for the cross-site 5-year evaluation and in part to time restrictions placed on data collection for the New York City 2½-year follow-up that did not permit an exhaustive data collection effort.

[3] A number of families ($n = 77$) had at least one child between the ages of 9 and 20 years who was seen at the 5-year follow-up, but data were not available for the dependent variables of interest. These families were included in the 2½-year only sample. An additional 46 families had age-eligible children who were seen only at the 2½-year follow-up.

[4] By design, however, children who were 20 years of age or older at the 5-year follow-up were not sampled ($n = 31$). Thus, it is not surprising that when compared with children interviewed at both follow-ups, the 245 children who were interviewed only at the 2½-year follow-up were older ($M = 11.52$ years, $SD = 3.23$ years vs. $M = 12.45$ years, $SD = 3.62$ years at the 2½-year follow-up, respectively). Replicating findings reported from the 2½-year follow-up separately for the samples who were eligible and ineligible at the 5-year follow-up did not generally alter reported results (Leventhal & Brooks-Gunn, 2004) for either subsample (results available from authors upon request).

[5] In addition to program moves, at the 5-year follow-up 18% of low-poverty voucher families had moved on their own (i.e., not through vouchers received by the MTO program), and 8% of traditional voucher families had made such moves. Thirty-one percent of control families had relocated at follow-up. No significant group differences in neighborhood poverty rates were found among these nonprogram movers ($M = 36.51$, $SD = 11.88$ vs. $M = 36.19$, $SD = 10.62$ vs. $M = 37.85$, $SD = 8.08$ for low-poverty voucher, traditional voucher, and controls, respectively), $F(2, 55) = 0.14$, $p > .10$.

Table 2
Baseline Parent and Family Characteristics of New York City 2.5-Year Follow-Up Sample by Treatment Group and 5-Year Follow-Up Participation Status

	Control[a]		Low-poverty voucher[a]		Traditional voucher[a]		Total	
Characteristic	2.5-year only (n = 38)	2.5 and 5 year (n = 85)	2.5-year only (n = 32)	2.5 and 5 year (n = 138)	2.5-year only (n = 53)	2.5 and 5 year (n = 98)	2.5-year only (N = 123)	2.5 and 5 year (N = 321)
M (SD) Parental age	38.45 (8.90)	33.43 (8.31)**	35.40 (8.89)	35.70 (9.39)	34.94 (7.97)	36.69 (8.54)*	36.33 (8.66)	35.28 (8.93)
Sex (Female)	97.6	100.0	100.0	97.9	100.0	100.0	99.2	99.1
Parental race–ethnicity								
African American	40.5	57.9†	52.9	46.0	64.3	47.6	52.5	49.8
Latina	54.8	41.1	47.1	51.1	33.3	48.8	44.9	47.3
Other	4.8	1.1	0.0	2.9	2.4	3.7	2.5	2.8
Parent high school graduate/GED	59.5	56.8	73.5	59.3	78.6	61.7†	70.6	59.0*
Parent married	11.9	8.4	20.6	11.4	14.3	7.3	15.1	9.2†
Parent employed	31.0	24.2	17.6	21.4	28.6	25.6	26.1	23.4
Family receive welfare	62.8	77.9†	73.5	72.9	73.8	72.0	69.7	74.1
M (SD) No. children in household	3.55 (2.19)	2.68 (1.17)**	3.10 (1.45)	2.90 (1.33)†	3.16 (1.72)	2.66 (1.30)†	3.29 (1.83)	2.77 (1.28)***

Note. Table presents means (*SD*) and percentages. Sample sizes were based on families with participating children. Descriptive statistics were weighted by date of random assignment because assignment ratio for three groups changed throughout the randomization period.
[a] Significance levels indicate significantly different from respective 2.5-year-only follow-up sample.
† $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

For voucher families in the New York City MTO who moved within the program, they had lived, on average, 3.20 (*SD* = 0.82) years in their new neighborhoods at the 2½-year follow-up (range = 1.98–4.52) and 6.30 (*SD* = 0.83) years in their new neighborhoods at the 5-year follow-up (range = 4.93–7.64). By the 5-year follow-up, families were living in 135 different census tracts (or neighborhoods), with an average of 2.36 (*SD* = 3.27) MTO families per neighborhood (range = 1–18). Within each of the three treatment groups, clustering of families within neighborhoods was low (detailed results available from authors upon request).[6]

To track neighborhood conditions over time, we report group comparisons for neighborhood economic and social characteristics at both the 2½- and 5-year follow-ups by using data from the 1990 and 2000 U.S. Census, respectively (see Table 3). According to U.S. Census measures, low-poverty voucher families lived in neighborhoods with average poverty rates of 34% at both the 2½- and 5-year follow-ups, and control families lived in neighborhoods with average poverty rates of 45% and 44% at these times. Compared with low-poverty voucher families, control families' neighborhoods, on average, had a significantly higher percentage of Latino residents across the follow-ups and a significantly lower percentage of White residents but did not significantly differ with respect to the percentage of Black residents at both evaluations. Across follow-ups, low-poverty voucher families also lived in neighborhoods with fewer rental units.

An important point to keep in mind is that the treatment group comparisons provided include low-poverty voucher families who moved within the program as well as those who chose not to move and remained in public housing in the baseline neighborhoods. When comparing the neighborhoods of low-poverty voucher movers and nonmovers at the 2½-year follow-up, the average poverty rate for low-poverty voucher movers was 14% (*SD* = 16)—closer to the program criterion of 10%—compared with 47% (*SD* = 8) for low-poverty voucher nonmovers—a figure similar to that of controls' neighborhoods. By the 5-year follow-up, however, low-poverty voucher movers were living in neighborhoods with poverty rates of 21% (*SD* = 14); low-poverty voucher nonmovers were in neighborhoods with poverty rates of 41% (*SD* = 10). Thus, low-poverty voucher families who moved within the program relocated to significantly more advantaged neighborhoods than did families who remained in the origin neighborhoods—both low-poverty voucher nonmovers and controls. However, 5 years into the program, low-poverty voucher movers were no longer living in low-poverty neighborhoods (as defined by the MTO program) but neighborhoods that can be characterized as low to middle income.

At the 2½-year follow-up, traditional voucher and control families lived in similar neighborhoods, evidenced by their poverty rates and racial–ethnic composition, with the exception of a higher percentage of Whites in traditional voucher families' neighborhoods relative to control families' neighborhoods. However, at the 5-year follow-up, traditional voucher families lived in neighborhoods with significantly fewer poor residents than control families' neighborhoods, but the neighborhoods of traditional voucher and control families did not significantly differ in racial–ethnic composition, including the number of Whites. Again, when comparing the neighborhoods of traditional voucher movers and nonmovers, the poverty rates were 29% (*SD* = 18) and 48% (*SD* = 8) at the 2½-year follow-up for movers and nonmovers, respectively, and 29% (*SD* = 12) and 45% (*SD* = 10) at the 5-year follow-up for movers and nonmovers, respectively. In short, traditional voucher families moved out of the most disadvantaged neighborhoods yet continued to reside in relatively disadvantaged neighborhoods. Nonetheless, at the 5-year follow-up, the average poverty rate of traditional voucher movers' neighborhoods was not markedly different from the average of low-poverty voucher movers' neighborhoods (*M* = 29% vs. *M* = 21%, respectively).

Measures

All measures, except baseline parent and family characteristics, were obtained during in-home follow-up interviews with families. Information on children's educational and schooling outcomes was available from three sources: (a) standardized assessments administered to all children, (b) interviews conducted with parents of all children, and (c) interviews conducted with adolescents (i.e., ages 11–18 years at 2½-year follow-up). Children and adolescents were evaluated separately from their parents, and trained field staff conducted all interviews and assessments. Comparable

[6] Given the relatively low clustering of families within neighborhoods, multilevel analyses, such as hierarchical linear modeling, were not appropriate.

Table 3

Neighborhood Characteristics as Measured by 1990 (2.5-Year Follow-Up) and 2000 (5-Year Follow-Up) U.S. Census Data by Treatment Group

	2.5-year follow-up			5-year follow-up		
Characteristic	Control (n = 85)	Low-poverty voucher[a] (n = 138)	Traditional voucher[a] (n = 98)	Control (n = 85)	Low-poverty voucher[a] (n = 138)	Traditional voucher[a] (n = 98)
Fraction poor	.45 (.10)	.34 (.20)***	.41 (.15)	.44 (.09)	.34 (.15)***	.39 (.13)*
Fraction rental units	.95 (.12)	.83 (.24)***	.92 (.15)	.89 (.11)	.78 (.20)***	.85 (.16)
Fraction Black	.44 (.22)	.47 (.25)	.40 (.22)	.38 (.20)	.43 (.24)	.37 (.19)
Fraction Latino	.50 (.21)	.41 (.24)**	.47 (.22)	.53 (.20)	.44 (.23)**	.51 (.20)
Fraction White	.04 (.11)	.10 (.17)*	.09 (.20)*	.04 (.06)	.08 (.15)*	.07 (.13)

Note. Table presents means (*SD*) weighted by date of random assignment because assignment ratio for three groups changed throughout randomization period. Sample sizes were based on families with participating children.
[a] Significance levels indicate significantly different from controls.
* $p < .05$. ** $p < .01$. *** $p < .001$.

measures were collected at both the 2½- and 5-year follow-ups; Table 4 reports descriptive statistics on all dependent variables.

Child characteristics. Child characteristics assessed were sex and age (in years) at the time of the 2½-year follow-up.

Baseline parent and family characteristics. These characteristics, reported during baseline interviews, included parent education (high school education = 0; high school graduate or higher = 1), age, race–ethnicity (Latino = 0; African American = 1), employment status (unemployed = 0; employed = 1), marital status (unmarried = 0; married = 1), and number of children in the household. Because there were missing data on parent marital status and number of children in the household, imputation was conducted by using mean substitution.[7]

Treatment status. Program assignment was represented by two indicator variables, one for the low-poverty voucher group and another for the traditional voucher group; the control group served as the omitted referent group. For the two mover groups, take-up status was assessed by a single variable indicating whether the family used their voucher to move to a new neighborhood (no = 0; yes = 1).

Woodcock–Johnson Tests of Achievement—Revised. Three subtests—Letter–Word Identification, Passage Comprehension, and Applied Problems—of the Woodcock–Johnson Tests of Achievement—Revised were administered to all children (Woodcock & Johnson, 1990; Woodcock & Mather, 1990).[8] The tests were administered in English or Spanish on the basis of preference. This battery is a widely used standardized assessment of subjects learned in school settings that also has high reliability. Subtest scores have a mean of 100 and a standard deviation of 15. The Letter–Word Identification subtest requires the child to match pictures to symbols and to identify letters and words. The Passage Comprehension subtest entails the child identifying pictures represented by words as well as identifying a word missing from a sentence in a short passage. The Applied Problems subtest involves the child counting as well as analyzing and solving practical mathematical word problems. Average subtest scores for this sample were slightly below the test norms for adolescents.[9]

School behavior problems. Parents reported whether anyone from their children's schools had contacted them about problems with their children's behavior; the 2½-year survey asked about current school year, whereas the 5-year survey covered the past 2 years.

Gifted program. Parents reported whether their children were in a gifted–talented class or advanced program. Again, the 2½-year survey asked about current school year, and the 5-year survey asked about the past 2 years.

Special education services. At the 2½-year follow-up, parents reported whether their children had received remedial instruction in reading and/or math. At the 5-year follow-up, parents reported whether their children had gone to a special class or school or received special help in school for learning problems.

Grade repetition. Parents of elementary school-age children reported their children's current grade in school as well as whether the children had ever repeated a grade and, if so, what grade(s) were repeated. From this information, whether children had repeated the current or prior grade was calculated.

Suspensions/expulsions. At the 2½-year follow-up, adolescents 11 years of age and older reported on whether they had been suspended or expelled from school during the past 6 months. At the 5-year follow-up, this information was obtained from parents and covered a 2-year period.

School engagement. All adolescents reported how true two statements were about them on a 4-point scale ranging from 1 (*not at all true*) to 4 (*very true*) (Wellborn & Connell, 1991). The items included the following: "I work very hard on my schoolwork" and "I pay attention in class" (r = .47, at 2½-year follow-up; r = .46, at 5-year follow-up). Higher scores represented more engagement.

Grade point average (GPA). Adolescents were asked to report their grades in school on a scale ranging from 1 (*Mostly Fs*) to 5 (*Mostly As*).

Homework time. Adolescents reported, in an average week, how much time they spent on homework on a scale ranging from 1 (*less than 1 hour*) to 4 (*more than 10 hours*).

[7] Very few cases were missing baseline parent and family characteristics in the present sample (n = 14 for marital status, and n = 10 for number of children in household).

[8] Previous research with the 5-year follow-up data indicated potential interviewer effects on children's achievement scores, especially for the New York City site. Preliminary analyses using achievement scores adjusted for interviewer effects were comparable to analyses with unadjusted scores. Thus, for simplicity's sake, we present results using unadjusted scores (results available from authors upon request).

[9] For all analyses, children with scores lower than 55 on the assessments were recoded to have scores of 55 so that results would not be biased by extremely low scores obtained by children who were severely low functioning. For children scoring more than three standard deviations below the mean, the difference between a score of 55 and a score lower than 55 is not meaningful. The number of children with scores lower than 55 was as follows: at the 2½-year follow-up, n = 7 for Letter–Word Identification, n = 7 for Passage Comprehension, and n = 8 for Applied Problems; at the 5-year follow-up, n = 11 for Letter–Word Identification, n = 4 for Passage Comprehension, and n = 5 for Applied Problems.

Table 4
Descriptive Statistics of Children's and Adolescents' Educational Outcome at 2.5- and 5-Year Follow-Ups and Correlations of Outcomes Across Follow-Ups

Outcome	2.5-year follow-up	5-year follow-up	2.5- & 5-year follow-ups (*r*)
Children age 6–10 at 2.5-year follow-up			
Letter–Word Identification	98.40 (17.17)	99.68 (19.68)	.47**
Passage Comprehension	101.45 (15.07)	100.70 (18.40)	.27**
Applied Problems	98.16 (16.37)	99.39 (20.58)	.18**
School behavior problems (%)	17.0	21.0	.42***
Gifted program (%)	15.1	14.9	.29***
Special education services (%)	28.7	12.6	.12†
Grade retention (%)	10.1	6.5	−.09
Children age 11–18 at 2.5-year follow-up			
Letter–Word Identification	94.85 (19.86)	92.51 (20.06)	.41**
Passage Comprehension	91.18 (15.05)	97.27 (21.46)	.24**
Applied Problems	91.91 (12.70)	91.25 (16.42)	.30**
School behavior problems (%)	24.5	22.6	.22**
Gifted program (%)	21.6	12.5	.30***
Special education services (%)	37.0	20.0	.22**
GPA	3.81 (.86)	3.30 (.74)	.24***
Time per week on homework	2.28 (.91)	2.31 (.78)	.23***
School suspension (%)	16.4	9.6	.19**
School engagement	3.61 (.49)	3.39 (.61)	.09

Note. Table presents means (*SD*) and percentages. Descriptive statistics were weighted by date of random assignment because assignment ratio for three groups changed throughout randomization period. At the 5-year follow-up, younger children were 9 to 13 years of age and older children were 14 to 20 years of age. GPA = grade point average.
† $p < .10$. ** $p < .01$. *** $p < .001$.

Results

By the 5-year follow-up, children in the 2½- and 5-year overlap sample, on average, were in eighth grade, and 49% were boys (see Table 5). Traditional voucher children were a grade above control children; all other group comparisons were nonsignificant. Almost all of these children were currently enrolled in school, with a majority (90%) attending public schools; results did not significantly differ by treatment group. Children were attending schools that were composed of 44% minority student bodies and 76% low-income students, as assessed by the National Center for Educational Statistics Common Core of Data and Private School Survey. Moreover, over three quarters of children were attending primarily minority schools (≥50% minority). These figures were comparable across treatment groups.

Table 4 presents correlations among the dependent variables assessed at both the 2½- and 5-year follow-ups. With several exceptions, the correlations were significant and ranged in magnitude from moderate to small. The largest correlation was for children's Letter–Word Identification scores ($r = .47$). Somewhat surprisingly, grade retention and school engagement were not significantly correlated across the follow-ups, and placement in special education programs for 6- to 10-year-olds was only marginally significant. Thus, the general pattern of correlations suggests that program effects at 2½ and 5 years may be discrepant.

The purpose of this study was to examine MTO program effects on children's and adolescents' educational outcomes. As such, the focus of all analyses was comparisons between (a) the low-poverty voucher group and controls and (b) the traditional voucher group and controls. To compare results with earlier work, all analyses were run separately for child (ages 6–10; low-poverty voucher, $n = 80$; traditional voucher, $n = 58$; and control, $n = 57$) and adolescent (ages 11–18; low-poverty voucher, $n = 100$; traditional voucher, $n = 69$; and control, $n = 61$) subgroups (based on age at 2½-year follow-up) to explore differential program effects on educational outcomes during the elementary school and middle to high school years. Because the current sample was relatively small, we were unable to examine age subgroups separately by child gender. However, given the salience of gendered program effects, we included Gender × Treatment Group interactions in a final model for each outcome; this approach has the advantage of being more conservative than subgroup analyses. For all analyses, robust standard errors were used to adjust for clustering of siblings within households.[10]

For each outcome, the first set of analyses evaluated treatment group comparisons. These analyses provide estimates of program effects on the basis of treatment (randomization) status, regardless

[10] In addition, all statistics were weighted to reduce biases associated with differential random assignment ratios to the three conditions throughout the randomization period for the New York City sample. Prior to July 24, 1996, a random assignment ratio of 8:3:5 was used for low-poverty voucher, traditional voucher, and controls, respectively; after July 24, 1996, and before October 24, 1997, a ratio of 5:7:4 was used; after October 24, 1997, and before December 3, 1997, a ratio of 3:7:6 was used; and after December 3, 1997, a ratio of 8:4:4 was used (Goering et al., 1999). Randomization ratio weights were adjusted further for the completion rates of the various measures—standardized assessments, parent interviews, and youth interviews.

Table 5
Children's and Adolescents' School Background by Treatment Group at 5-Year Follow-Up

Variable	Control (n = 118)	Low-poverty voucher (n = 180)	Traditional voucher[a] (n = 127)	Total (N = 425)
Grade in school				
K–4	17.1	12.8	13.4	14.2
5–8	48.1	40.4	38.7	42.2
9–12	34.9	46.8	47.9	43.6
Mean grade (*SD*)	7.03 (2.72)	7.64 (2.73)	8.01 (2.85)*	7.56 (2.78)
Attending school	92.4	93.6	91.0	92.6
Attending public school	92.3	90.1	88.2	90.2
% minority students	43.3	43.6	43.8	43.8
% students receive free or reduced price lunch	73.3	77.0	78.3	75.7

Note. Table presents means (*SD*) and percentages. Descriptive statistics were weighted by date of random assignment because assignment ratio for three groups changed throughout randomization period.
[a] Significance level indicates that it is significantly different from controls.
* $p < .05$.

of whether children's families took up the assigned treatment (i.e., used vouchers to move). Such analyses are often referred to as intention-to-treat program effects because they evaluate treatment group differences between the group the program intended to treat and the control group, which was not treated. These analyses represent the traditional approach to examining randomized clinical trials.

However, it is possible to estimate treatment group comparisons by taking into account the fact that not all families took up the treatment. Such analyses are referred to as treatment-on-treated program effects and represent a more novel approach to examining randomized clinical trials (Angrist, Imbens, & Rubin, 1996; Bloom, 1984; Katz, Kling, & Liebman, 2001). Increasingly, evaluations of child and family policy-oriented programs in fields such as early childhood education (Love et al., 2002) and welfare and employment (Riccio & Bloom, 2001) are using both approaches to estimate program effects, given the relative merits of each strategy. Specifically, intention-to-treat comparisons are not subject to selection bias if random assignment successfully yields no systematic differences between treatment and control groups at baseline except for the opportunity to participate in the treatment program, and thus, they provide the most unbiased estimate of treatment effects (Little & Rubin, 2000). In the case of treatment-on-treated comparisons, randomization status is violated by examining program effects solely among those who received the treatment and will result in biased estimates of program effects; however, certain analytic strategies, as will be described, yield relatively unbiased estimates of program effects among those who were treated or moved (Angrist et al., 1996).[11] Together, intention-to-treat and treatment-on-treated comparisons provide a range within which the true program effect likely lies; the intention-to-treat comparisons can be thought of as lower bound estimates of program effects, whereas the treatment-on-treated comparisons can be thought of as upper bound estimates of program effects.

For the intention-to-treat comparisons, ordinary least squares regression was used. In addition to treatment status (low-poverty voucher and traditional voucher), models controlled for child age and baseline parent and family characteristics (age, race–ethnicity, education, employment status, marital status, and number of children in household). Treatment Group × Child Sex interaction terms were entered into subsequent models.

The treatment-on-treated comparisons were estimated with two-stage least-squares regressions or instrumental variable analysis and used a similar modeling strategy; these analyses necessitated estimating models separately for the two mover groups.[12] The first set of models used random assignment status as an instrument (plus covariates) to predict program take-up for the low-poverty voucher or traditional voucher groups, and the subsequent models used the predicted take-up variable for the respective group (plus covariates) to estimate program effects on each outcome. Although we think these analyses are important to explore because of the modest take-up rate, again we are conservative in our interpretation of results because of the relatively small cell sizes. Thus, we discuss only results that reach conventional levels of significance

[11] Violation of the exclusion restriction by means of the treatment affecting noncompliers via counseling cannot be rejected because of the study design; however, this situation should not be highly problematic for several reasons. Specifically, separating out components of the low-poverty voucher treatment—counseling, voucher, and moving restrictions—in MTO cannot be done; the treatment-on-treated comparisons represent the pooled effect of this package of treatments. At the theoretical level, however, the effect of mobility counseling is likely to have a much smaller impact on children's educational outcomes than the effect of moving (especially to low-poverty neighborhoods). Moreover, observed differences in the traditional voucher group reported, although marginally significant or nonsignificant, support the notion that treatment-on-treated effects are more likely due to moving than to counseling, because the traditional voucher group did not receive counseling. Finally, we compared counseling experiences for low-poverty voucher compliers and noncompliers, and although we found significant differences in interactions with the nonprofit providing mobility counseling, overall both groups had little interaction with the counseling agency and did not find these experiences particularly helpful (results are available from authors upon request).

[12] For both voucher groups, the take-up rate did not significantly differ by child gender. However, for the low-poverty voucher group only, children of families that moved were approximately 1 year younger, on average, than children of families that did not move. No child age differences were found between traditional voucher movers and nonmovers.

and/or in which the intention-to-treat effect is at least marginally significant.

Program Effects at 5-Year Follow-Up

Prior to examining program effects on patterns of change from the 2½- to 5-year follow-ups, we investigated program effects on mean level group differences at the 5-year follow-up.[13] The first set of columns in Table 6 reports results from these analyses. Specifically, the first column presents adjusted means for controls (adjusted for child, parent, and family covariates), and the following columns present intent-to-treat comparisons (main effects followed by Treatment × Child Sex interactions) from the regression analyses for the low-poverty voucher and traditional voucher groups, respectively. For all intention-to-treat program effects, the table presents unstandardized coefficients and standard errors in parentheses (significant treatment-on-treated effects are reported in text).

Children. The top panel of Table 6 presents results of intent-to-treat comparisons for children. For low-poverty voucher children, only one significant effect was found. At the 5-year follow-up, low-poverty voucher children were significantly more likely than control children to be involved in a gifted program (21% vs. 7%, respectively); the corresponding treatment-on-treated effect was also significant ($\beta = 0.28$ [0.13], $p < .05$). For low-poverty voucher children, two trend-level effects for achievement were also found, but for the intention-to-treat comparisons only. Low-poverty voucher children had marginally lower Passage Comprehension and Applied Problems subtest scores than did control children. A comparable effect was found for traditional voucher children's Passage Comprehension subtest scores only. No significant treatment effects were found for Letter–Word Identification, school behavior problems, special education services, or grade retention for either voucher group, nor did any significant Treatment × Sex interactions emerge across groups.

Adolescents. Results of the intention-to-treat comparisons for adolescents are presented in the bottom panel of Table 6. At the 5-year follow-up, low-poverty voucher adolescents reported significantly lower grades than did control peers. The unstandardized coefficients reported indicate that control youths had GPAs of 3.61, whereas low-poverty voucher youths had GPAs of 3.20 (e.g., $3.61 - 0.41 = 3.20$). The corresponding treatment-on-treated effect was also significant ($\beta = -1.37$ [0.50], $p < .01$), indicating that low-poverty voucher youths who moved within the program had lower GPAs (e.g., $3.61 - 1.37 = 2.24$) than did control peers. In addition, compared with their control peers, low-poverty voucher youths reported significantly less engagement in school; the treatment-on-treated effect was marginally significant ($\beta = -0.83$ [0.44], $p < .10$).

For low-poverty voucher adolescents, a significant Treatment × Sex interaction was found for homework time; the corresponding treatment-on-treated effect was nonsignificant. An examination of the interaction effects indicated that low-poverty voucher boys spent less time on homework than did control boys ($M = 2.02$ vs. $M = 2.34$, respectively), whereas low-poverty voucher girls spent more time on homework than did control girls ($M = 2.57$ vs. $M = 2.30$, respectively). A marginally significant interaction was found for Letter–Word Identification (treatment-on-treated effect was nonsignificant) and revealed that low-poverty voucher boys also had lower achievement scores than did control boys ($M = 88.49$ vs. $M = 97.80$, respectively). For low-poverty voucher youths, no program effects or interactions were found for the Passage Comprehension and Applied Problems subtests, school behavior problems, gifted programs, special education services, or school suspensions.

Like their low-poverty peers, traditional voucher youths reported significantly lower grades than control adolescents (treatment-on-treated effect: $\beta = -1.08$ [0.45], $p < .05$). In addition, traditional voucher adolescents had marginally lower Passage Comprehension subtest scores than did control peers, but the treatment-on-treated effect was not significant. Among the traditional voucher group, one significant Treatment × Child Sex interaction emerged for Letter–Word Identification and revealed that traditional voucher boys had lower scores than did control boys (84.51 vs. 97.80, respectively); however, the treatment-on-treated comparison was nonsignificant. No program effects for traditional voucher youths emerged for the Applied Problems subtest, school behavior problems, gifted programs, special education services, homework time, or school suspensions.

Program Effects on Change From 2½- to 5-Year Follow-Up

The next set of analyses replicated the models examining program effects on 5-year outcomes but included the respective outcome at the 2½-year follow-up in order to partial out variance accounted for by initial program effects. These analyses permitted an assessment of individual change over time from the 2½- to 5-year follow-up (Allison, 1990). Findings largely mirror those reported in the previous section. A summary of results is reported in the second half of Table 6.

Children. For low-poverty voucher children, we found that after accounting for status at the 2½-year follow-up, these children were significantly more likely than control peers to be in a gifted program. The treatment-on-treated effect was significant ($\beta = 0.29$ [0.12], $p < .05$). On the contrary, low-poverty voucher children had marginally lower Applied Problems subtest scores, but the corresponding treatment-on-treated effect was not significant. No significant program effects for low-poverty voucher children were found for any of the other educational or schooling outcomes, and no significant Treatment × Child Sex interactions were found.

For traditional voucher children, several trend-level effects for change over time were found for achievement scores. Taking into account achievement at the time of the 2½-year follow-up, traditional voucher children had marginally lower reading (treatment-on-treated effect: $\beta = -22.00$ [12.94], $p < .10$) and math (treatment-on-treated effect: $\beta = -25.27$ [15.29], $p < .10$) scores at the 5-year follow-up as compared with control children. A significant Treatment × Sex interaction was found for the Applied Problems subtest as well (treatment-on-treated effect: $\beta = 58.75$ [25.63], $p < .05$), indicating that traditional voucher girls had

[13] We ran additional models with the full sample of children from the New York City site seen at the 5-year follow-up ($N = 1{,}099$). In general, results were comparable to those reported with the New York City 2½- and 5-year overlap subsample (results are available from authors upon request).

Table 6

Summary of Unstandardized Regression Coefficients and Standard Errors (in Parentheses) for MTO Intention-to-Treat Program Effects on Educational Outcomes at 5-Year Follow-Up and Change From 2.5- to 5-Year Follow-Ups for Child Subgroups

Outcome	5-year findings[a]					5-year findings controlling for 2.5-year findings[a]					
	Control-adjusted *M*	LPV	LPV × Child Sex interaction	TV	TV × Child Sex interaction	Control-adjusted *M*	LPV	LPV × Child Sex interaction	TV	TV × Child Sex interaction	*n*
Children ages 6–10, at 2.5-year follow-up											
Letter–Word Identification	99.05	1.81 (3.78)	−2.34 (7.73)	1.03 (4.98)	−11.13 (9.58)	99.24	1.53 (3.65)	−8.08 (6.75)	−0.57 (4.60)	−9.81 (8.60)	160
Passage Comprehension	106.07	−7.15 (4.14)†	5.71 (9.37)	−7.37 (4.43)†	−.39 (8.82)	105.79	−6.53 (4.14)	3.20 (9.80)	−7.97 (4.37)†	−2.09 (8.89)	159
Applied Problems	105.56	−8.47 (4.96)†	9.80 (8.50)	−7.61 (5.45)	17.25 (8.98)	106.16	−9.26 (4.95)†	9.43 (8.56)	−9.10 (5.46)†	17.89 (8.96)*	160
School behavior problems	.18	.09 (.07)	.21 (.14)	.03 (.08)	.01 (.16)	.19	.10 (.06)	.13 (.13)	.02 (.07)	−.14 (.14)	183
Gifted program	.07	.14 (.06)*	.12 (.12)	.11 (.07)	−.19 (.13)	.08	.14 (.06)*	.11 (.12)	.11 (.07)	−.19 (.13)	181
Special education services	.15	−.04 (.06)	−.03 (.14)	.01 (.07)	.04 (.15)	.14	−.02 (.06)	−.02 (.13)	.02 (.07)	.07 (.15)	182
Grade retention	.07	−.02 (.05)	−.09 (.08)	.03 (.06)	.01 (.14)	.07	−.02 (.05)	−.09 (.08)	.03 (.06)	.01 (.14)	139
Children ages 11–18, at 2.5-year follow-up											
Letter–Word Identification	94.61	−1.64 (4.31)	−14.25 (7.62)†	−3.52 (4.28)	−19.25 (7.01)**	95.94	−2.45 (3.91)	−11.75 (6.60)†	−4.51 (3.87)	−17.89 (6.24)**	199
Passage Comprehension	99.58	−1.20 (4.52)	−2.43 (4.53)	−6.80 (3.78)†	−2.43 (8.71)	99.32	−0.30 (4.30)	−2.77 (8.30)	−6.77 (3.70)†	−3.79 (6.51)	196
Applied Problems	94.77	−5.16 (4.00)	−0.45 (7.07)	−4.36 (4.61)	−7.03 (7.55)	94.92	−5.24 (3.93)	−0.05 (6.81)	−3.67 (4.46)	−3.74 (7.31)	197
School behavior problems	.18	.07 (.08)	−.14 (.17)	.04 (.09)	−.04 (.19)	.19	.06 (.08)	−.15 (.17)	.02 (.10)	−.07 (.19)	163
Gifted program	.13	.00 (.09)	−.15 (.14)	−.02 (.08)	.03 (.16)	.13	.00 (.08)	−.22 (.14)	−.02 (.08)	−.06 (.15)	163
Special education services	.22	−.04 (.07)	.06 (.16)	.02 (.09)	.18 (.19)	.20	−.02 (.08)	.01 (.15)	.02 (0.08)	.10 (.18)	164
GPA	3.61	−.41 (.13)**	.05 (.24)	−.42 (.15)**	−.02 (.30)	3.57	−.34 (.12)**	.04 (.24)	−.40 (.15)**	−.07 (.30)	191
Time per week on homework	2.32	−.02 (.16)	−.60 (.28)*	.07 (.16)	−.33 (.31)	2.34	−.06 (.16)	−.59 (.28)*	.02 (.17)	−.41 (.30)	167
School suspension	.05	.05 (.06)	.02 (.12)	.05 (.05)	−.12 (.10)	.05	.03 (.06)	−.04 (.11)	.07 (.05)	−.13 (.10)	183
School engagement	3.54	−.28 (.11)*	−.29 (.22)	−.11 (.13)	.11 (.25)	3.55	−.28 (.11)*	−.26 (.23)	−.12 (.13)	.11 (.25)	170

Note. Models were adjusted for child age and baseline parental characteristics, including age, race–ethnicity, education, employment status, marital status, and number of children in household and apply weights by date of random assignment because assignment ratio for three groups changed throughout randomization period. Missing baseline characteristics were imputed to mean of nonmissing sample. Robust standard errors adjust for multisibling households. At the 5-year follow-up, younger children were 9–13 years of age and older children were 14–20 years of age. MTO = Moving to Opportunity for Fair Housing Demonstration; LPV = low-poverty voucher; TV = traditional voucher; GPA = grade point average.

[a] Significance levels indicate that respective voucher group is significantly different from controls.

† $p < .10$. * $p < .05$. ** $p < .01$.

lower test scores than did control girls (M = 93.03 vs. M = 109.46, respectively). No significant treatment main effects or interactions were found for traditional voucher children's other schooling outcomes.

Adolescents. Low-poverty voucher youths reported significantly lower grades and school engagement than control youths. The corresponding treatment-on-treated effect was significant for grades ($\beta = -1.18$ [0.47], $p < .01$) and marginally so for engagement ($\beta = -0.83$ [0.44], $p < .10$). In addition, a significant Treatment × Sex interaction for homework time indicated that low-poverty voucher boys spent less time on homework than did control peers (M = 2.04 vs. M = 2.40, respectively; treatment-on-treated effect was nonsignificant). A similar, but marginally significant, effect was found for Letter–Word Identification subtest scores (M = 89.50 vs. M = 98.30 for low-poverty voucher boys and control boys, respectively).

Traditional voucher youths reported significantly lower grades than did control youths (treatment-on-treated effect: $\beta = -1.04$ [0.46], $p < .05$). These youths also had marginally lower Passage Comprehension subtest scores than did control peers, but the treatment-on-treated effect was not significant. Finally, among traditional voucher youths, a significant Treatment × Child Sex interaction for Letter–Word Identification revealed that traditional voucher boys had lower scores than did control boys (M = 84.80 vs. M = 98.30, respectively). As seen in Figure 1, control boys' scores remained stable over time. Both low-poverty voucher and traditional voucher boys' scores declined from the 2½-year and 5-year follow-ups, although only significantly so (at a conventional level) for the latter group. Girls' scores exhibited little fluctuation over time.

Response Bias Analyses

Given that results did not show sustained program effects, we conducted response bias analyses to determine whether the average levels of the educational outcomes differed for those children and youths seen at both the 2½- and 5-year follow-ups and those seen only at the 2½-year follow-up. A series of probit regression analyses was carried out, wherein an attrition indicator variable was regressed on the relevant outcome variables assessed 2½ years into the program (controlling for the standard battery of child and family baseline characteristics). Analyses were run separately for age (i.e., 6–10 years and 11–18 years at 2½-year follow-up) subgroups and Age × Sex subgroups. Of particular interest were interactions between program status and the outcome measures in order to assess whether the probability of remaining in the study

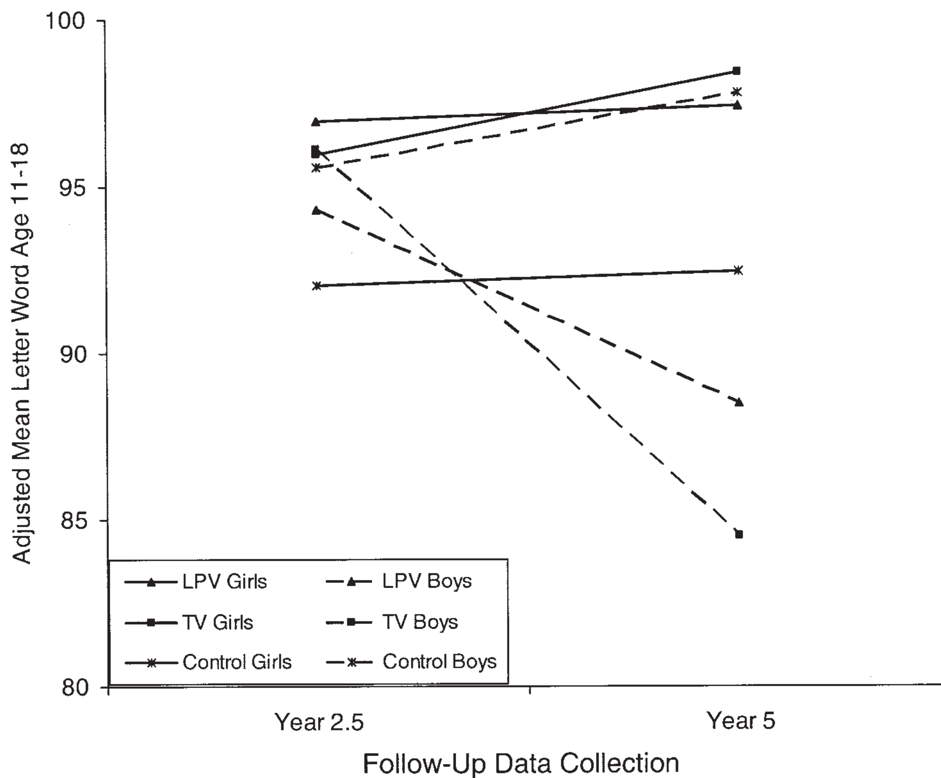

Figure 1. Mean Letter–Word Identification subtest scores for youths ages 11–18 years at 2½-year follow-up by child sex and treatment group (N = 199). Means were computed by ordinary least squares regression and adjusted for child age and baseline parental characteristics, including age, race–ethnicity, education, employment status, marital status, and number of children in household. The analysis applied weights by date of random assignment because assignment ratio for three groups changed throughout the randomization period. Missing baseline characteristics were imputed to the mean of the nonmissing sample. Robust standard errors were adjusted for multisibling households. LPV = low-poverty voucher; TV = traditional voucher.

differed between treatment groups depending on children's scores on respective outcome variables. Only significant interactions ($p < .05$ or less) are discussed here (full results are available from authors upon request).

Children. Across the treatment groups, no significant differences in attrition were found for 6- to 10-year-olds for either the age or Age × Sex subgroup analyses.

Adolescents. For adolescents 11 to 18 years of age, only one significant interaction was found for the age subgroup analyses. Control youths with high engagement scores (i.e., above the mean) were less likely than low-poverty voucher youths—those scoring both above and below the mean—to participate in the 5-year follow-up. For the Age × Sex subgroup analyses, results indicated that low-poverty voucher boys scoring above the mean on the Letter–Word Identification subtest at 2½ years were more likely to participate in the 5-year follow-up than control boys who scored below the mean on this subtest at 2½ years. For low-poverty voucher girls, those with above average Letter–Word Identification scores at the 2½-year follow-up were more likely to participate in the 5-year follow-up than control girls who also scored above the mean on this subtest. A similar pattern was found in terms of girls' engagement at school, with low-poverty voucher girls who reported high school engagement (i.e., above the mean) more likely to participate in the 5-year follow-up than control girls with similar high school engagement. No differences in attrition were found for traditional voucher adolescents.

Together, the findings reveal that by and large the two subsamples of children 6 to 10 years of age followed and not followed at 5 years were quite comparable. However, the findings for adolescents 11 to 18 years of age suggest that results at 5 years may have been biased by selective attrition. The 2½- and 5-year adolescent subsample may not have included the lowest functioning control boys. In contrast, the overlap subsample may have underrepresented high-functioning control girls and overrepresented high-functioning low-poverty voucher girls. Additional analyses replicating our earlier findings at the 2½-year follow-up by participation status in the 5-year follow-up provide further evidence of these attrition patterns (see Appendix).

Discussion

In the present study, we used longitudinal data on a subsample of school-age children and youths who participated in the New York City MTO program to explore one policy approach for improving the educational outcomes of low-income minority children and youth. This unique demonstration program randomly assigned vouchers to low-income minority families living in public housing to be used to move to less poor neighborhoods. Children and their families were interviewed 2½ and 5 years following relocation, during which educational and schooling outcomes were assessed. The primary goal of the present study was to use data on a small subsample of New York City children (9- to 13-year-olds) and youths (14- to 20-year-olds) to examine program effects on educational outcomes over time. We sought to rectify discrepant findings reported in initial MTO site-specific evaluations with results of the 5-year five-site evaluation. Specifically, early findings reported that adolescent boys who moved to low-poverty neighborhoods scored higher on reading and math achievement tests compared with peers who stayed in high-poverty neighborhoods; however, these favorable outcomes were not evident at the 5-year follow-up.

Summary of Findings

Low-poverty voucher group. Contrary to initial expectations, positive program effects on achievement were not sustained for low-poverty voucher adolescent boys. Rather, low-poverty voucher boys spent significantly less time on homework than did control peers at the 5-year follow-up, a reversal of the findings reported in the earlier study. In fact, low-poverty voucher boys' increased homework time relative to controls at the 2½-year follow-up partially accounted for the positive program effects on adolescent boys' achievement reported at that time. Thus, this decrease in homework time between the two follow-ups may also explain the lack of positive program impacts on low-poverty voucher boys' achievement at the 5-year follow-up. The source of this shift in program effects on homework time could be due to lower engagement (compared with controls), as was found among the full low-poverty voucher sample. Likewise, declines in achievement as well as grades (compared with controls) may have contributed to reduced effort exerted on homework.

As mentioned, low-poverty voucher adolescents—both boys and girls—reported significantly lower grades (*C*s vs. *B*s, respectively) and school engagement relative to controls. Although not found at the 2½-year follow-up and counter to expectations, these findings for boys are in line with the 5-year cross-site evaluation. One potential explanation for these negative program effects as well as the lack of sustained program effects on adolescent boys' achievement follows from the last set of analyses assessing response bias across the two follow-ups. These analyses indicated that the lower functioning adolescent boys in the control group were not interviewed in the 5-year follow-up. Why this is the case is not clear. One possibility is that these lower functioning control males may have been harder to locate at the 5-year follow-up for a variety of reasons, such as school dropout or incarceration.

The findings for low-poverty voucher adolescent girls were not consistent with our expectation of no program effects. As noted, we generally found negative program effects on the schooling outcomes of low-poverty voucher girls, like their male peers. It is interesting to note that the attrition analyses revealed that low-poverty voucher girls with high verbal achievement scores at 2½ years were more likely to be seen at 5 years than similarly high-achieving control girls. That this positive selection does not translate into favorable program effects on their achievement and schooling outcomes is somewhat surprising and indicates that low-poverty voucher adolescent girls may be encountering significant difficulties in school.

Although we anticipated that the disruptive effects of moving would negatively impact traditional voucher youth, we think these processes may be operative for low-poverty voucher adolescents—both boys and girls—as well.[14] Moving sets in motion a number of changes that may be especially stressful for adolescents, including changing schools and peer networks (Adam & Chase-Lansdale,

[14] Low-poverty and traditional voucher families moved significantly more often than did control families during the 5-year follow-up period ($M = 2.01$, $SD = 1.43$ vs. $M = 1.91$, $SD = 1.24$ vs. $M = 1.37$, $SD = 0.57$, respectively).

2002; Simpson & Fowler, 1994; Wood, Halfon, Scarlata, Newacheck, & Nessim, 1993). Thus, by the 5-year follow-up, the disruption associated with moving was not necessarily offset by the benefits of highly advantaged neighborhood conditions. Nor were youths attending more advantaged schools. Most youths were attending public schools in large urban areas that were composed predominantly of poor and minority students. As reported elsewhere, the MTO program also had no impact on family economic circumstances (Orr et al., 2003). As such, low-poverty voucher families were still poor, and moving may have compounded the stress of family poverty (Haveman, Wolfe, & Spaulding, 1991).

Very few program effects were found for low-poverty voucher children, and the effects that emerged were mixed in nature. Five years following relocation, children ages 9 to 13 years were more likely to be involved in gifted programs than control children who remained in high-poverty neighborhoods, which bodes well for their future prospects. However, low-poverty voucher children also exhibited marginally lower reading and math test scores than did control children 5 years following relocation. Although these findings may seem discrepant, follow-up analyses revealed that low-poverty voucher children in regular nongifted classrooms had lower reading achievement scores than did their respective control peers (results are available from authors upon request). However, the reading scores of children in gifted programs did not differ by program status. Thus, for low-poverty voucher children who are doing relatively well—at least with respect to reading—their new schools may offer more opportunities for gifted programs (compared with controls). However, a long-term question is whether the effects of participation in gifted programs may ultimately result in low-poverty voucher children displaying higher achievement than control peers, as this beneficial program effect was not evident at the 5-year follow-up. It should be noted that the current child sample did not appear to be markedly different from children who were not seen at the 5-year follow-up, unlike the adolescent sample.

The findings for low-poverty voucher children and youths raise the question of why so few school-related program effects were found and, when found, why they were generally unfavorable. Previous research has supported the notion that residence in affluent or middle-class neighborhoods is positively associated with children's and youths' school readiness and achievement outcomes (Brooks-Gunn et al., 1993; Chase-Lansdale & Gordon, 1996; Duncan, 1994; Halpern-Felsher et al., 1997). It may be that these "neighborhood effects" were actually due to unmeasured school-level characteristics, including quality, norms, and composition. That is, the impact of school composition and quality may outweigh any such neighborhood effects on children's and youths' outcomes (Teitler & Weiss, 2000). In the past, school enrollment was generally determined by neighborhood of residence. However, with the push for school choice and charter schools, especially in urban school districts, many children attend schools outside of their neighborhoods (National Working Commission on Choice in K–12 Education, 2003). Thus, residential mobility may not be accompanied by school change. Notwithstanding whether low-poverty voucher children and youths changed schools, they attended schools with similar economic and racial–ethnic compositions as control peers even though many of them lived in less segregated and higher socioeconomic status neighborhoods. In short, low-poverty voucher children, by and large, were not attending suburban schools, as was the case in the Gautreaux Program.

Despite the fact that changes in school conditions were not found, low-poverty voucher children and youths were in more advantaged neighborhoods. Neighborhood poverty rates among low-poverty voucher families who moved within the program increased from 14% at the 2½-year mark to 21% by the time of the 5-year follow-up. This situation was partially due to the program stipulation that low-poverty voucher families were required to stay in low-poverty neighborhoods for only 1 year, and many families had subsequently relocated to higher poverty neighborhoods. In addition, many low-poverty voucher families chose to move to low-poverty neighborhoods in the midst of economic downturn (Orr et al., 2003). Nonetheless, low-poverty voucher families were still able to escape the highly disadvantaged neighborhoods where control families remained. Thus, it remains to be seen whether any educational benefits for children and youths materialize over the long term from residence in more advantaged neighborhoods.

Traditional voucher group. Among children in the traditional voucher group, few program effects were found, as anticipated. Some evidence emerged that traditional voucher children may have exhibited declines in achievement relative to controls in the period between follow-ups. Although this effect was only marginally significant, it is interesting that no mean level differences were found at the 5-year follow-up. Because traditional voucher children's declining reading scores occurred in the years since the 2½-year follow-up, it suggests that these children are beginning to look more like their older cohort.

For traditional voucher adolescents, results were consistent with our expectation of unfavorable program effects on their educational outcomes. Similar to low-poverty voucher youths, traditional voucher adolescents reported significantly lower grades than control adolescents. In addition, traditional voucher male youths had significantly lower verbal achievement scores than did control peers; these differences were not apparent at the 2½-year follow-up and appear to indicate a decline in achievement over time. As with low-poverty voucher youths, we suspect that turbulence associated with moving may account for these negative program effects. In addition, selective attrition among the control group may be operative as well.

Limitations

With respect to other recent evaluations of MTO children's educational outcomes (Orr et al., 2003; Sanbonmatsu et al., in press), the longitudinal data available for the New York City site permitted the first examination of longer term program effects on these outcomes over time. In terms of the developmental literature on neighborhood effects more broadly, the use of experimental data, resulting from random assignment of families to neighborhoods, has methodological advantages over most extant studies relying on nonexperimental data—notably, overcoming families' self-selection into neighborhoods. Despite these study strengths, several limitations should be noted. First, as discussed earlier, selective attrition may have been present in the current analytic sample (i.e., families and their children seen at both the 2½- and 5-year follow-ups). Second, the MTO program was based on voluntary participation of low-income public housing residents. Although previous work found that participating MTO families

were more socioeconomically disadvantaged than families who declined participation (Goering et al., 1999), unmeasured differences between these two groups of families likely exist. Third, in MTO, it is difficult to separate neighborhood income type with housing type, as families who relocated resided in private housing and control families resided in public housing. Of course, because the traditional voucher group by and large remained in high-poverty neighborhoods, program effects found among this group could be viewed as the impact of private versus public housing. Fourth, the relatively small sample size may have limited our ability to detect program effects, especially for the treatment-on-treated and interaction analyses. Fifth, although outcomes assessed over time were identical in most cases, in some instances, the time frame varied across the two follow-ups and may have limited our ability to capture change over time. Sixth, the measures used to assess most schooling outcomes were restricted to very few items and may not have adequately evaluated the respective constructs. Finally, the findings reported are based on one city's experience with the MTO program, and the size and nature of program impacts on children's and youths' achievement and educational outcomes may differ at the other four sites (Sanbonmatsu et al., in press).

Policy Implications and Conclusions

A primary goal of the MTO program is to enhance the educational prospects of low-income minority children. Thus, the policy implications of this study are substantial. From an implementation standpoint, relocating low-income minority families into private housing without follow-up support services may be insufficient to achieve this goal. Whether these services should be tailored toward keeping families in low-poverty neighborhoods and/or meeting families' needs beyond neighborhood and housing conditions is not clear. What is clear, however, is that strictly providing low-income minority families with vouchers to be used in low-poverty neighborhoods and with short-term assistance is not enough to significantly raise children's and youths' achievement and educational outcomes.

When considering alternative housing assistance programs for low-income families, it may be that providing families with vouchers fosters frequent mobility and housing instability, which is less common with public housing (Newman & Harkness, 2001; Popkin et al., 2003). This study coincided with a tight housing market (especially in New York City) that may have posed unique challenges for low-income families seeking permanent housing in low-poverty neighborhoods, especially once housing counseling services had ceased (Goetz, 2003; Popkin, Harris, & Cunningham, 2001). Housing policy is clearly not moving in the direction of building more public housing, so policies aimed at providing stable affordable housing in low-poverty neighborhoods are crucial.

Finally, if educational benefits of MTO are supposed to accrue, at least in part, through school quality, this pathway does not appear to be operative. In large urban centers, school and neighborhood conditions are not as tightly linked as previously thought, given school choice programs in many of these school districts (Popkin et al., 2001; Shumow, Vandell, & Kang, 1996). In addition, a large majority of schools in urban districts are low performing (U.S. Department of Education, 2003). Thus, it may be that to markedly alter low-income minority children's school conditions requires moving families out of urban areas, as was the case in the Gautreaux Program (in lieu of sending these children to private schools in urban areas). However, such policies may arouse concern within receiving communities (Fauth et al., 2004). In sum, the policy ramifications of enhancing low-income minority children's educational outcomes involve multiple dynamics, including family, neighborhood, housing, and school. Programs such as MTO that attempt to address these factors simultaneously reveal the complexity of effecting change in children's well-being.

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Appendix

We conducted an additional set of attrition analyses that entailed replicating our earlier findings at the 2½-year follow-up by participation status in the 5-year follow-up. For these analyses, we estimated models examining program effects at 2½ years, comparable to those for mean level differences at 5 years. Models were run separately for the 425 children and youths seen at the 2½- and 5-year follow-ups and for the 245 children and youths seen at the 2½-year follow-up only. Table A1 provides a summary of intent-to-treat program effects on children's and adolescents' educational outcomes for the two subsamples—2½- and 5-year sample and 2½-year-only sample, respectively—by using a similar format to results reported for the 5-year follow-up outcomes.

Children

The top panel of Table A1 presents results for children. For children in the 2½- and 5-year overlap sample, who were the focus of the current study, no significant program effects on educational and schooling outcomes were found for the low-poverty voucher group. For the 2½-year-only sample, two marginally significant, negative program effects were found for low-poverty voucher children's Applied Problems subtest scores and their participation in gifted programs. Compared with control children, low-poverty voucher children had lower test scores and were less likely to be in gifted programs at the 2½-year follow-up. Across the two low-poverty voucher subsamples, no significant Treatment × Child Sex interactions were found at 2½ years, nor did any of the treatment-on-treated analyses reach conventional levels of significance.

Two trend-level program effects were found for traditional voucher children in the 2½- and 5-year subsample; traditional voucher children had higher Applied Problems subtest scores and were less likely to receive special education services than control children at the 2½-year follow-up. Results of the treatment-on-treated analyses were marginally significant for special education only ($\beta = -0.35$ [0.20], $p < .10$). Among traditional voucher children in the 2½- and 5-year subsample, a significant Treatment × Sex interaction was found for school behavior problems at the 2½-year follow-up. For girls, traditional voucher parents were less likely than control parents to report recent contact with school officials regarding children's behavior (6% vs. 19%, respectively). For boys, the opposite pattern was found, with traditional voucher boys more likely to have school behavior problems than control boys (40% vs. 16%, respectively). For traditional voucher children in the 2½- and 5-year subsample, no significant program effects were found for reading and verbal achievement, and no significant Treatment × Sex interactions were reported for achievement scores, participation in gifted programs, and grade retention at 2½ years.

Among traditional voucher children in the 2½-year-only sample, one significant program effect was found for the Passage Comprehension subtest. Traditional voucher children had significantly lower scores than did control children at the 2½-year follow-up; the corresponding treatment-on-treated effect was marginally significant ($\beta = -22.10$ [12.45], $p < .10$). For traditional voucher children in the 2½-year-only sample, no significant program effects were found for any other outcomes at 2½ years, nor did any significant Treatment × Child Sex interactions emerge.

Adolescents

The bottom panel of Table A1 reports results for adolescents. For low-poverty voucher adolescents in the 2½- and 5-year overlap sample, a marginally significant program effect was found for grades at 2½ years; the treatment-on-treated effect was also marginally significant ($\beta = -1.04$ [0.61], $p < .10$). Low-poverty voucher youths had significantly lower grades than did their respective controls. Significant Treatment × Sex interactions emerged for school suspensions and school engagement at 2½ years, but the corresponding treatment-on-treated effects were nonsignificant. For the former outcome, low-poverty voucher boys were significantly more likely to be suspended from school than control boys (35% vs. 11%, respectively). Likewise for school engagement, low-poverty voucher boys reported being significantly less engaged than control boys ($M = 3.42$ vs. $M = 3.54$, respectively). In contrast, low-poverty voucher girls reported significantly higher levels of engagement than did control girls ($M = 3.75$ vs. $M = 3.48$, respectively). Along these same lines, a marginally significantly interaction for receipt of special education services indicated that low-poverty voucher girls were less likely than control girls to receive these services at 2½ years (18% vs. 53%, respectively). Results of the treatment-on-treated comparison were significant for this outcome ($\beta = 1.05$ [0.52], $p < .05$).

For the 2½-year-only sample, results for low-poverty voucher youths were generally consistent with previously reported findings, especially for

Table A1

Attrition Analyses: Summary of Unstandardized Regression Coefficients and Standard Errors (in Parentheses) for MTO Intention-to-Treat Program Effects on Educational Outcomes at 2.5-Year Follow-Up for Child Subgroups by 5-Year Follow-Up Participation Status

	2.5- and 5-year follow-ups[a]						2.5-year follow-up only[a]					
Outcome	Control-adjusted *M*	LPV	LPV × Child Sex interaction	TV	TV × Child Sex interaction	*n*	Control-adjusted *M*	LPV	LPV × Child Sex interaction	TV	TV × Child Sex interaction	*n*
Children ages 6–10 at 2.5-year follow-up												
Letter–Word Identification	98.26	0.51 (3.57)	10.23 (6.89)	2.89 (3.37)	−2.35 (7.38)	160	99.10	−9.17 (6.65)	−4.48 (11.55)	−5.92 (6.44)	−1.21 (13.57)	63
Passage Comprehension	102.46	−1.93 (2.69)	7.98 (5.25)	1.88 (2.82)	5.42 (7.21)	159	100.19	−7.30 (5.25)	−6.94 (9.72)	−13.46 (5.42)*	7.00 (12.98)	63
Applied Problems	95.13	3.98 (3.11)	1.78 (6.08)	7.59 (3.97)†	−3.10 (8.67)	160	99.57	−9.03 (5.44)†	−5.09 (11.56)	−7.20 (5.58)	−2.66 (12.49)	63
School behavior problems	.18	−.02 (.07)	.19 (.14)	.03 (.08)	.37 (.16)*	183	.24	−.10 (.15)	.16 (.27)	−.05 (.13)	.27 (.25)	77
Gifted program	.16	−.02 (.07)	.06 (.13)	.00 (.07)	.04 (.13)	181	.17	−.15 (.08)†	−.10 (.17)	.01 (.11)	.00 (.19)	77
Special education services	.34	−.09 (.08)	−.03 (.15)	−.14 (.08)†	−.23 (.22)	182	.32	−.01 (.16)	.46 (.31)	−.08 (.15)	−.13 (.29)	77
Grade retention	.07	.06 (.06)	.07 (.13)	−.01 (.06)	.06 (.11)	139	.21	.13 (.17)	.32 (.33)	−.12 (.16)	−.10 (.28)	63
Children ages 11–18 at 2.5-year follow-up												
Letter–Word Identification	93.59	1.97 (3.41)	−6.24 (6.75)	2.43 (3.57)	−3.40 (6.93)	199	94.80	1.74 (8.02)	36.32 (13.01)**	.51 (5.27)	23.93 (8.01)**	111
Passage Comprehension	92.80	−2.89 (3.24)	1.12 (6.14)	−.11 (3.30)	−.32 (5.99)	196	89.72	0.31 (4.71)	18.85 (11.10)	1.81 (3.31)	17.70 (5.82)**	108
Applied Problems	92.40	0.23 (2.06)	−1.17 (4.54)	−2.01 (2.44)	−9.83 (4.56)*	197	91.42	0.27 (4.97)	18.01 (8.87)*	−1.09 (3.36)	5.44 (5.52)	108
School behavior problems	.18	.04 (.08)	.06 (.19)	.11 (.09)	.19 (.19)	163	.29	−.04 (.11)	−.11 (.20)	.05 (.10)	−.02 (.19)	147
Gifted program	.22	.01 (.09)	.25 (.16)	−.01 (.10)	.36 (.18)	163	.23	−.05 (.09)	.21 (.17)	.01 (.08)	−.05 (.16)	144
Special education services	.45	−.17 (.11)	.38 (.20)†	.02 (.13)	.57 (.24)*	164	.47	−.11 (.11)	−.25 (.20)	.01 (.11)	−.10 (.19)	147
GPA	3.98	−.32 (.17)†	.07 (.32)	−.10 (.16)	.25 (.31)	191	3.86	−.05 (.22)	−.31 (.48)	−.24 (.19)	−.99 (.41)*	112
Time per week on homework	2.14	.22 (.22)	−.06 (.41)	.24 (.23)	.39 (.40)	167	2.22	.28 (.26)	.11 (.56)	.05 (.23)	.48 (.51)	110
School suspension	.16	.08 (.08)	.36 (.14)**	−.12 (.07)†	.05 (.11)	183	.14	.01 (.10)	−.14 (.20)	.00 (.08)	.17 (.18)	110
School engagement	3.57	.01 (.11)	−.49 (.18)**	.12 (.10)	.02 (.18)	170	3.68	−.02 (.12)	−.01 (.21)	−.07 (.10)	−.45 (.22)*	118

Note. Models were adjusted for child age and baseline parental characteristics, including age, race–ethnicity, education, employment status, marital status, and number of children in household and apply weights by date of random assignment because assignment ratio for three groups changed throughout randomization period. Missing baseline characteristics were imputed to mean of nonmissing sample. Robust standard errors adjust for multisibling households. At the 5-year follow-up, younger children were 9–13 years of age and older children were 14–20 years of age. MTO = Moving to Opportunity for Fair Housing Demonstration; LPV = low-poverty voucher; TV = traditional voucher; GPA = grade point average.

[a] Significance levels indicate that respective voucher group is significantly different from controls.

† $p < .10$. * $p < .05$. ** $p < .01$.

(*Appendix continues*)

boys. Specifically, a significant Treatment × Child Sex interaction emerged for the Letter–Word Identification subtest; however, the corresponding treatment-on-treated effect was not significant. Low-poverty voucher boys had significantly higher scores than did control boys (M = 99.51 vs. M = 82.20, respectively), whereas low-poverty voucher girls had significantly lower scores than did control girls (M = 88.05 vs. M = 107.06, respectively). A similar pattern was found for Applied Problems subtest scores for boys (M = 95.46 vs. M = 87.88 for low-poverty voucher and controls, respectively) and girls (M = 84.78 vs. M = 95.21 for low-poverty voucher and controls, respectively).

Among traditional voucher adolescents, the results for the 2½- and 5-year sample indicated a significant Treatment × Child Sex interaction for the Applied Problems subtest at 2½ years (treatment-on-treated effect was nonsignificant). Traditional voucher girls had significantly higher test scores than did control girls (M = 93.80 vs. M = 90.47, respectively), but for boys the opposite was true (M = 88.10 vs. M = 94.60, respectively). A similar pattern was found for receipt of special education services, with traditional voucher girls less likely than control girls to receive these services (26% vs. 53%, respectively), and traditional voucher boys more likely than control boys to receive these services (55% vs. 35%, respectively; treatment-on-treated effect was nonsignificant).

Results for traditional voucher adolescents in the 2½-year-only sample were comparable to findings for corresponding low-poverty voucher youth. A significant Treatment × Child Sex interaction for Letter–Word Identification revealed that traditional voucher boys had higher scores than did control boys (M = 95.88 vs. M = 82.20, respectively). A similar pattern was found for Passage Comprehension for males (M = 92.69 vs. M = 80.91 for traditional voucher and control, respectively). Traditional voucher boys had lower school engagement than did controls (M = 3.32 vs. M = 3.65, respectively) as well as lower reported grades than control boys (M = 3.13 vs. M = 3.96, respectively). None of the treatment-on-treated comparisons were significant.

In sum, very few program effects were found for children, consistent with our earlier study, and those effects reported for both subsamples were primarily marginal in nature and typically in the same direction (negative). However, like the response bias analyses, the findings for adolescents suggest that results at 5 years may be biased by selective attrition. That is, the lack of positive program effects for low-poverty voucher boys relative to control boys at the time of the 5-year follow-up may be due to the high attrition rate among the latter. Replication of reported program effects for low-poverty and traditional voucher adolescent boys at 2½ years was largely restricted to the 2½-year-only subsample.

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Call for Nominations

The Publications and Communications (P&C) Board has opened nominations for the editorships of **Behavioral Neuroscience, JEP: Applied, JEP: General, Neuropsychology, Psychological Methods,** and **Psychology and Aging** for the years 2008–2013. John F. Disterhoft, PhD; Phillip L. Ackerman, PhD; D. Stephen Lindsay, PhD; James T. Becker, PhD; Stephen G. West, PhD; and Rose T. Zacks, PhD, respectively, are the incumbent editors.

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