MOVING TO OPPORTUNITY: A SYMPOSIUM

Neighborhood Effects on Economic Self-Sufficiency: A Reconsideration of the Moving to Opportunity Experiment[1]

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This article revisits the Moving to Opportunity housing mobility experiment, which heretofore has not provided strong evidence to support the hypothesis of neighborhood effects on economic self-sufficiency among adults. The authors undertake a conceptual and empirical analysis of the study's design and implementation to gain a better understanding of the selection processes that occur within the study. The article shows that the study is potentially affected by selectivity at several junctures: in determining who complied with the program's requirements, who entered integrated versus segregated neighborhoods, and who left neighborhoods after initial relocation. Furthermore, previous researchers have not found an experimental treatment effect on adult economic self-sufficiency, relative to controls. The authors propose an alternative approach that involves measuring the cumulative amount of time spent in different neighborhood environments. With this method, they find evidence that neighborhood is associated with outcomes such as employment, earnings, TANF receipt, and use of food stamps.

Under the influence of the Chicago school, American sociology historically placed great emphasis on the ecological context of social behavior, but attention to spatial issues waned in the 1970s and 1980s as the status-

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attainment model, popularized by sociologists at the University of Wisconsin, came to dominate stratification research. The Wisconsin model offered a useful corrective for human capital theory's narrow emphasis on differential rewards to skills within competitive markets by showing that much inequality was inherited from generation to generation through family-based mechanisms that operate outside of markets.

Taking advantage of the burgeoning power of mainframe computers and advances in sampling theory and questionnaire design, Wisconsin school researchers relied on large-scale social surveys to link the behavior of individuals to the characteristics of families and, especially, parents. Although these surveys were originally cross-sectional in design, by the 1980s numerous longitudinal surveys had come into existence to enable developmental studies across time as well as between generations, a task that was greatly facilitated by the invention of new methods of event-history analysis and the falling costs of computation.

During the 1970s, sociologists seemed to become mesmerized by the possibilities for quantitative analysis using survey data, but in their fascination with statistics and methods they somehow forgot about ecology, failing to incorporate into their sophisticated models the fact that human behavior necessarily occurs within (or must transcend) physical space. The neglect of ecology came to an abrupt halt in 1987 with the publication of William Julius Wilson's book *The Truly Disadvantaged*, which pointed out a remarkable feature of urban geography in 1980s America: the increasing spatial concentration of poverty in African-American communities, which yielded neighborhood environments with a serious absence of resources.

Wilson argued for the importance of "neighborhood effects" in accounting for the cycle of black poverty, and in doing so he revolutionized stratification research, hearkening back to the Chicago school's original focus on social ecology. After 1987, sociologists began to geocode survey data to create new multilevel files that linked individuals not only to the characteristics of families, but also to conditions within blocks, tracts, zip codes, and other spatial units (Jencks and Mayer 1990). At the same time,

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methodologists worked to develop new statistical techniques that would allow the efficient estimation of contextual effects using multilevel data sets (see Raudenbush and Bryk 1992; Goldstein 1995).

A plethora of studies ensued, and over time evidence accumulated to suggest that conditions in a person's neighborhood of origin or residence are associated with socioeconomic outcomes and thus play an important role in the broader process of stratification (for recent reviews, see Leventhal and Brooks-Gunn [2000], Small and Newman [2001], and Sampson, Morenoff, and Gannon-Rowley [2002]). For example, numerous studies have found a correlation of socioeconomic neighborhood-level variables with adolescent sexual behaviors (Hogan and Kitagawa 1985; Crane 1991; Billy and Moore 1992; Coulton and Pandey 1992; Brewster, Billy, and Grady 1993; Brooks-Gunn et al. 1993; Ku, Sonenstein, and Pleck 1993; Billy, Brewster, and Grady 1994), child maltreatment (Coulton et al. 1995), crime (Sampson and Groves 1989; Peterson, Krivo, and Harris 2000), and dropping out of school (Crane 1991; Coulton and Pandey 1992; Brooks-Gunn et al. 1993).

Despite the abundance of survey-based evidence developed to support Wilson's hypothesis of neighborhood effects, however, work based on multilevel survey data was subject to a serious methodological weakness: using survey data, it is nearly impossible to eliminate selectivity as a competing explanation for apparent neighborhood effects. As Tienda (1991) put it, the question is, Do poor places make poor people, or do poor places attract poor people? In the absence of random assignment to neighborhoods, it is difficult to know whether living in a disadvantaged neighborhood lowers one's life chances in some causal way or whether the observed correlation between concentrated poverty and low socioeconomic status (SES) simply reflects patterns of in- and out-migration or other class-selective processes.

The earliest findings on neighborhood effects that attempted to deal with these methodological issues came from the quasi-experimental Gautreaux residential mobility program. As part of a court-ordered legal settlement to redress past racial discrimination, from 1976 to 1998 the Chicago Housing Authority (CHA) provided a limited number of low-income African-American families the means to relocate, with an emphasis on moving to predominantly white suburbs. Each year, thousands of people vied to apply by telephone for a few hundred vouchers handed out on a first-come, first-served basis. These vouchers could be used in the private rental market, with the federal government making up the difference between a unit's rent and the individual's contribution (based on income). Over the program's 22-year history, some 7,100 families relocated into private housing in Chicago and its suburbs (Rubinowitz and Rosenbaum 2000).

While assignment to groups was not random, the project nonetheless created two natural comparison categories: persons who received a voucher and moved to a neighborhood in the city of Chicago, and those who received a voucher and moved to a low-minority neighborhood outside of the city. A comparison across groups carried out by Rosenbaum and colleagues found that children in families who moved to predominantly white suburbs experienced lower dropout rates, higher rates of college attendance, and higher rates of employment (Rubinowitz and Rosenbaum 2000). Adults, meanwhile, achieved a modest gain in rate of employment compared with those who moved into city neighborhoods (Popkin, Rosenbaum, and Meaden 1993). These findings for adults and children came from a small survey of Gautreaux families and have recently been challenged, as we discuss below, but we mention them here because they laid the groundwork for future residential mobility initiatives.

While these results seemed to support the case for neighborhood effects by removing selection bias in a quasi-experimental way, critics were not mollified because assignment to comparison groups was nonrandom. Given the promise of Gautreaux, however, the U.S. Department of Housing and Urban Development (HUD) under presidents George H. W. Bush and Bill Clinton designed a demonstration project called Moving to Opportunity (MTO). MTO sought to control selectivity by randomly allocating vouchers to residents of public housing in five cities and then requiring that those in the experimental group only use the vouchers to move into low-poverty neighborhoods (no more than 10% poor). Those families not assigned to the experimental group were either assigned to a traditional voucher group—which allowed them to use their voucher without geographic restrictions—or a control group, in which they received no change to their situation. Built into the program's design was an evaluation that surveyed participants prior to the offer of vouchers, kept track of subsequent moves, and surveyed participants at an interim point four to seven years after households were randomly assigned.

The MTO data, gathered in the interim evaluation survey, have been extensively analyzed in a series of reports and publications (see Orr et al. 2003; Kling et al. 2004; Kling, Ludwig, and Katz 2005; Kling, Liebman, and Katz 2007). These studies concluded that MTO had a significant effect on the type of neighborhoods that experimental group members lived in four to seven years after they signed up, relative to controls. The average neighborhood poverty of experimentals was lower than that of controls (30% vs. 39%); these families were also more likely to feel safe in their neighborhoods, less likely to have been victimized in recent months, and less likely to report conditions of local disorder, such as drug activity. But while experimental households were located in neighbor-

hoods with (statistically) significantly lower proportions of minority residents, they were still in tracts that were overwhelmingly minority—83%, versus 88% for controls.

Only 47% of those families assigned to the experimental group actually used their MTO vouchers.[2] To make full use of the unique experimental design of MTO, researchers typically compare the full experimental group (including noncompliers) to the control group, a comparison that yields what is known as the intent-to-treat (ITT) effect. This approach has been used to estimate the treatment effect of being offered a voucher to move to a low-poverty neighborhood. Another related effect measured with MTO data is the treatment-on-the-treated effect (TOT). TOT estimates are constructed by dividing ITT estimates by the take-up rate of the experimental group. Thus, the TOT estimates are larger in magnitude than the ITT estimates. For example, TOT estimates find that experimental compliers were living in neighborhoods that were 18 percentage points less poor than those of controls, four to seven years after random assignment. But since the standard errors are adjusted in the same way, the TOT estimates are only statistically significant when the ITT estimates are. While TOT estimates are nonexperimental, they do take into account selectivity at the point of voucher take-up (Orr et al. 2003).

These results offer substantial evidence that MTO accomplished an important goal: it moved many families into neighborhoods that were better off in terms of poverty, crime, and disorder. Investigators have often sought to move a step further, however, by assessing the effect of the MTO treatment on individual outcomes. They have found that adults in the experimental group showed improvement with respect to mental health and were less likely to be obese, relative to controls. However, they found no significant effects of treatment assignment on adults' economic self-sufficiency (Kling et al. 2004). Likewise, among children and youth, no positive treatment effect was found for educational or physical health outcomes, though girls in the experimental group did experience significant mental health benefits and a reduction in risky behavior (Kling et al. 2007). Although experimental compliers may have lived in less poor neighborhoods than controls, therefore, investigators found no corresponding benefit across a range of individual outcomes, including adult economic self-sufficiency.

While these findings show that the MTO treatment did have a clear effect on neighborhood quality and several individual outcomes, they do not provide the ringing confirmation of neighborhood effects that many advocates of the Wilson hypothesis had hoped to find. Researchers hoped

[2] We refer to this group as "compliers" and to those that received vouchers but did not use them as "noncompliers."

to use MTO data to measure neighborhood effects in an experimental way, given the careful design of MTO. It was therefore surprising to many observers not to find benefits to residential mobility similar to those found in Gautreaux research. This lack of a strong effect on individual outcomes has undermined support for the presumed causal relationship between concentrated poverty and individual SES.

In this article, we return to the MTO data to reconsider that conclusion. We argue that the MTO experiment had certain features of design and implementation that worked against the detection of strong neighborhood effects on individual outcomes, and that once these features are taken into account, we cannot dismiss the potential for neighborhood effects on a wider range of outcomes. We begin by elucidating design features of the MTO experiment in comparison with the Gautreaux demonstration and speculate about how these characteristics limit the study's ability to detect neighborhood effects. We then show that, despite program administrators' efforts to carry out random assignment of households to treatment groups, compliance with the terms of the program was highly selective. In addition, residential mobility after relocation proved to be not only selective but extensive, yielding short periods of exposure to target neighborhoods as well as additional sources of selectivity. We argue that neighborhood conditions are only likely to influence social and economic outcomes gradually over time. If this is so, then the simple inclusion of a variable measuring assignment to the treatment group (the ITT estimate) or the use of the nonexperimental TOT estimate can successfully measure the *effects of the policy initiative*, but is not well suited to capturing *neighborhood effects*. We conclude by estimating models that assess the relationship of cumulative exposure to different neighborhood environments and selected economic outcomes. We show that living in low-poverty neighborhoods is positively associated over the long term with higher levels of employment, greater earnings, and lower levels of public service dependency, as expected under the neighborhood effects hypothesis.

DATA

Beginning in 1994, tenants in high-poverty public housing developments (those located in census tracts with a 40% or higher poverty rate) in Baltimore, Boston, Chicago, Los Angeles, and New York were given the opportunity to sign up for the Moving to Opportunity demonstration.[3]

[3] MTO applicants who were deemed eligible for the program were actually a more economically disadvantaged population than the remaining public housing families in

To be eligible for MTO, tenants had to have children under the age of 18 and had to go through the Section 8 eligibility determination process. After eligibility was determined, 4,608 families were randomly assigned to one of three groups. The control group received no change to their situation, the experimental group received housing counseling and a Section 8 voucher that could only be used to move to a tract where the poverty rate was under 10%, and the Section 8 comparison group received a Section 8 voucher with no geographic restrictions (Orr et al. 2003). Only the control and experimental groups are considered in this article.

For this analysis, we draw on three sources of data from the MTO study: the baseline survey, a geocoded file of census tract data linked to this survey, and the interim evaluation survey. The baseline survey was administered to every MTO household before the random assignment of housing vouchers. In order to predict whether and how the experimental voucher was used, we employ a set of baseline variables similar to those used in Shroder's (2001) analysis of take-up in the MTO demonstration; to predict individual outcomes at the time of the interim survey, we use the same set of baseline control variables as the interim survey report (Orr et al. 2003). Address data were compiled for most of the sample at several points over the period from random assignment until the interim survey and were geocoded by Tele-Atlas to 1990 and 2000 census geography so they could be matched to census tract data from the 1990 and 2000 SF3 files.

The MTO interim evaluation took place seven years after the first families were randomly assigned. Since random assignment took place over a period of years, the respondents had been in the program for different lengths of time—a range from four to seven years—at the time of evaluation. This survey was conducted from January to September 2002, and the sample included all families randomly assigned up through December 31, 1997 (see Orr et al. [2003] for a detailed description of the data collection and analysis of the survey data). All of the families randomly assigned in four of the cities were included in the survey, but 356 households in Los Angeles that were randomly assigned after January 1, 1998, were excluded.

Out of the full MTO population of 4,608 households, 4,248 adults were included in the interim evaluation sample. Adjusting for the sampling methods used in the two phases of data collection, the effective response rate for adults in the interim evaluation was 90%. The dependent variables for individual outcomes come from the interim survey. These are self-reported measures of employment, income, and receipt of food stamps

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the five cities, so the MTO program is not subject to allegations of "creaming," as the Gautreaux program has been (Goering, Feins, and Richardson 2002).

and cash assistance. While there can certainly be validity issues with self-reported measures, we assume that any bias in self-reporting would be the same across the experimental and control groups. All of the estimates reported here are computed using sample weights (described in detail in Orr et al. [2003], app. B).

Fieldworkers conducted in-person surveys with all adults in sample households as well as with children 8–19 years old. Educational achievement tests were administered to all children aged 5–19. On average, the sample comprised 2.6 members (including 1.6 children) per family. The interviews primarily took place in the respondents' homes, using a computer-assisted personal interviewing program on laptop computers.

EXPERIMENTAL DESIGN: MTO VERSUS GAUTREAUX

The Gautreaux residential mobility program grew out of a class-action lawsuit filed in 1966 against the CHA on behalf of public housing tenants led by Dorothy Gautreaux (Hirsch 1983; Varnarelli 1986). The suit alleged that the CHA had violated federal law by racially discriminating in the selection of public housing project sites and in the allocation of people to project units. The initial decision in 1969 was in favor of the plaintiffs, but a series of appeals and negotiations delayed final resolution of the case until 1981, when a federal judge approved a consent agreement. Under the agreement, the CHA accepted responsibility for past racial discrimination and agreed to allocate some 7,100 subsidized rental vouchers to public housing residents for use in securing private rental units in the city and suburbs (Kaufman and Rosenbaum 1991; Rosenbaum et al. 1991).

The essential feature of the Gautreaux agreement was that it was explicitly racial, coming in response to a civil rights lawsuit (Metcalf 1988). Since this remedy was intended to effect racial desegregation, the goal of the program was to place families in neighborhoods that had 30% or fewer African-American residents. The suburbs of Chicago, which had large areas that were predominantly white and middle class, were specifically targeted in the Gautreaux agreement as well (Rubinowitz and Rosenbaum 2000). A provision in the ruling, however, allowed Gautreaux participants to move to predominantly black tracts in Chicago if these neighborhoods were determined to be in "revitalizing" communities (Varnarelli 1986; Keels et al. 2005).

In their analysis of a random half-sample of Gautreaux participants who moved before 1990, Keels et al. (2005) found that almost all of the suburban movers went to tracts that fit the goal of the program, and nearly half of the city movers moved to tracts that had 25% or fewer

African-Americans. Taken as a whole, the city movers ended up in tracts that were on average 47% African-American and 27% poor, whereas suburban movers ended up in tracts that were on average 6.5% black and 5% poor (Keels et al. 2005). Though neighborhood poverty rates were not mentioned in the consent decree, by channeling families out of public housing and into low-minority areas—particularly in the suburbs—the program necessarily guaranteed that some participants would enter very advantaged neighborhoods.

It turns out, however, that it was not necessarily suburban targeting that led to the differences in outcomes that Gautreaux researchers uncovered. Indeed, some inner suburbs experienced problems with resources and crime similar to those in central cities. Keels (2005) found no city/suburb difference in public assistance receipt or time spent with earnings for young adults who had moved as children in the Gautreaux program. Mendenhall, DeLuca, and Duncan (2006) also analyzed administrative data for Gautreaux movers in order to look at neighborhood context and adult earnings. Although they found no differences between city and suburban movers, they did find that women placed into predominantly white neighborhoods that had moderate-to-high resources, as well as those placed into integrated neighborhoods (11%–60% black) with high resources, spent significantly more time with earnings than those placed in predominantly African-American neighborhoods that had a low level of resources.

The improvement in neighborhood socioeconomic circumstances achieved under Gautreaux thus occurred because of a well-documented connection between residential segregation and the class composition of minority neighborhoods (Massey and Denton 1993). If income inequality is high within a group that is highly segregated on the basis of race, then geographically concentrated poverty follows axiomatically (Massey 1990; Massey and Fischer 2000). Sharp increases in the black poverty rate observed during the 1970s and 1980s interacted with black hypersegregation in Chicago and other cities (Wilkes and Iceland 2004) to produce the spatial concentration of black poverty noted by Wilson (1987), giving rise to the concentration effects he observed (see Massey and Denton 1993).

As a result of the interaction between high rates of black poverty and high rates of black segregation throughout metropolitan America, poor African-Americans continue to experience by far the highest concentrations of poverty in the United States (Massey and Fischer 2004). At the same time, because of the history of segregation and continuing barriers to realizing residential preferences, middle-class African-Americans experience neighborhood conditions that are remarkably disadvantaged compared with the middle class of other groups (Massey and Denton 1993; Sampson and Wilson 1995). Relative to areas inhabited by middle-class

whites, Asians, or Latinos, those inhabited by the black middle class exhibit lower property values, higher crime rates, lower employment rates, higher levels of unwed childbearing, poorer schools, lower educational achievement, and higher rates of welfare dependency (Massey, Condran, and Denton 1987; Massey and Fong 1990; Pattillo-McCoy 1999). Even though middle-class black areas may not themselves display concentrated poverty, because of racial segregation they tend to be located adjacent to or very near areas of concentrated deprivation and often share common service catchment areas (Sampson, Morenoff, and Earls 1999; Morenoff, Sampson, and Raudenbush 2001).

As a result, nonpoor black areas are not comparable socially or economically to the nonpoor neighborhoods inhabited by other groups (Charles 2003; Massey 2004). To put it simply, the average resident of a nonpoor black neighborhood experiences a more disadvantaged environment than the average resident of a nonpoor white neighborhood; this renders critical a difference in design between the Gautreaux and MTO demonstration projects. Unlike Gautreaux, which required that most recipients of housing vouchers move to *low-minority neighborhoods* (under 30% black), MTO investigators only required households to relocate to *low-poverty neighborhoods* (under 10% poor—see Orr et al. 2003). Under the criteria established by MTO, in other words, Gautreaux's explicit emphasis on race was lost and the focus shifted to class.

This shift would not have resulted in a limited range of neighborhood resource options in a truly desegregated society. But by only requiring households to relocate to nonpoor neighborhoods, MTO inadvertently ensured that many participants would remain within a racially segregated environment and thus continue to be vulnerable to the chronic scarcity of human, social, and financial resources associated with highly segregated urban neighborhoods, such as poor-quality city schools. In essence, this decision stacked the deck against the detection of neighborhood effects in the experiment's results, by restricting the range of neighborhood conditions to which participants were exposed. For example, whereas the average MTO experimental mover entered a census tract that was 10.8% poor,[4] the average Gautreaux suburban mover entered a tract that was 5% poor. Though these MTO neighborhoods were only slightly poorer than the Gautreaux neighborhoods initially, it is important to note that the poverty rate significantly *increased* in 45% of these tracts between 1990 and 2000, rising 5 percentage points or more (Orr et al. 2003).[5]

[4] This figure is an interpolation of poverty rates between the 1990 and 2000 censuses (Orr et al. 2003).

[5] Almost all of the remaining tracts experienced no change greater than 5 percentage points in either direction.

Table 1 illustrates the problem by showing the distribution of MTO households across categories defined on the basis of class *and* race. Following Wilson (1987), Jargowsky (1997), and others, we define tracts under 20% as "nonpoor neighborhoods" and those with poverty rates above this threshold as "poor neighborhoods." (Although MTO defined 10% as the poverty threshold, in this analysis we employ the more conventional dividing line of 20%, which yields tabulations that are conservative with respect to our underlying hypotheses.) Following the criteria used by Gautreaux, we classify census tracts that are under 30% minority (black and Hispanic) as "integrated neighborhoods" and those whose minority percentage exceeds this level as "segregated." We realize, of course, that "nonintegrated" is a more accurate description of the latter neighborhoods, but we use the term "segregated" for ease of exposition. The cross-classification of these variables yields four kinds of neighborhoods: integrated nonpoor, segregated nonpoor, integrated poor, and segregated poor.

Table 1 shows the distribution of MTO households across these categories at different points in time, before and after the relocation of experimental households. Part A presents neighborhood distributions for households randomly designated as controls, which received no offer of a housing voucher. According to Orr et al. (2003), approximately 70% of all controls moved after random assignment, though not with a voucher provided by MTO and, as we shall see, typically not to a nonpoor neighborhood. Part B shows neighborhood distributions for experimental group members who complied with the program's relocation criteria, meaning that they were offered a voucher at random assignment and used it to move to a nonpoor neighborhood. Part C shows neighborhood distributions for experimental group members who did not comply with the program's criteria—that is, they were offered a voucher through random assignment but did not accept it.

Here we focus on control group members and those who received a voucher and used it to relocate to a low-poverty neighborhood (compliers). As can be seen, before random assignment, households in both the experimental and control groups were overwhelmingly concentrated in poor, segregated neighborhoods, as one would expect for a sample of tenants from public housing developments. In both groups, most of the remaining households at baseline were located in integrated, poor neighborhoods (almost all of these tracts were in Boston). Less than 1% of participants lived in low-poverty neighborhoods, either integrated or segregated. By definition, after relocation all of the experimental complier households were in nonpoor neighborhoods.

Relatively few of these compliers, however, relocated into racially integrated nonpoor neighborhoods, which is not surprising, given the persistent racial discrimination in U.S. housing markets (Massey and Fischer

TABLE 1
PERCENTAGES OF EXPERIMENTAL AND CONTROL SUBJECTS BY NEIGHBORHOOD TYPE FROM BASELINE THROUGH INTERIM EVALUATION (4–7 years after assignment)

Group and Neighborhood Type	Baseline	Initial Relocation	Year 4	Interim Years 4–7
A. Control group:				
Nonpoor neighborhoods:				
Integrated	.4		5.0	5.7
Segregated	.2		9.0	10.9
Poor neighborhoods:				
Integrated	9.8		7.3	5.8
Segregated	89.6		78.7	77.6
N	1,303		1,296	1,150
B. Experimental complier group:				
Nonpoor neighborhoods:				
Integrated	.1	27.9	20.7	18.3
Segregated	.2	72.1	45.6	40.6
Poor neighborhoods:				
Integrated	7.6	.0	1.2	1.7
Segregated	92.0	.0	32.5	39.4
N	814	814	812	710
C. Experimental noncomplier group:				
Nonpoor neighborhoods:				
Integrated	.3		3.7	5.2
Segregated	.1		7.6	8.7
Poor neighborhoods:				
Integrated	4.3		4.7	3.3
Segregated	95.3		84.1	82.8
N	909		903	791

NOTES.—Overall $N = 3{,}039$. Individual N's vary because of missing geocoded data. Baseline measures are taken from 1990 census data. All other measures are from 2000 census data.

2003) and the fact that there was no program requirement for racial integration. Among experimental households moving to nonpoor areas, just 28% entered a neighborhood that was racially integrated, whereas 72% went to an area that was segregated. In other words, nearly three-quarters of the households participating in the MTO experiment did not satisfy the original criteria for receipt of a subsidy voucher in the Gautreaux program. Thus, in design, the two programs are quite different.

In table 2, we consider selected social and economic characteristics of the neighborhoods inhabited by experimental compliers after relocation and compare them to the characteristics of a complier's typical neighborhood before relocation. Within poor, segregated neighborhoods—home to over 90% of the entire MTO sample at baseline—one-third of those

TABLE 2
CHARACTERISTICS OF NEIGHBORHOODS INHABITED BY EXPERIMENTAL COMPLIERS AT BASELINE AND AFTER RELOCATION

	SEGREGATED POOR AT BASELINE		INTEGRATED NONPOOR AFTER RELOCATION		SEGREGATED NONPOOR AFTER RELOCATION	
CHARACTERISTICS	1990	2000	1990	2000	1990	2000
%high school dropout	33.1	30.8	11.0	10.0	16.7	15.8
%teenagers not in labor force or school	20.0	13.5	4.4	5.1	7.8	7.8
%employed	32.6	34.3	65.8	63.3	64.5	57.3
%in poverty	57.5	49.7	6.2	8.6	7.8	13.6
%female-headed families	72.8	63.6	15.6	21.6	23.9	33.9
%owner-occupied homes	8.4	10.9	61.7	61.8	61.8	59.4
Median household income[a]	10,030	16,242	39,318	50,931	36,065	42,882
%have college degree	8.1	11.5	33.0	38.2	23.9	26.3
N	716	715	228	228	568	565

[a] The 1990 figures for median household income are in 1989 dollars; the 2000 figures are in 1999 dollars.

18 years old and over were high school dropouts in 1990, 20% of teenagers were neither in school nor employed, only 8% of adults had a college degree, only a third of all adults were gainfully employed, and most families (58%) lived in poverty and were female headed (73%). Median household income was quite low, at $10,030. Very few respondents (just 8%) owned their own homes. The 1990s witnessed a sustained economic boom, of course, and in some locations (notably Chicago and Baltimore) authorities demolished a large number of housing projects, leading to a slight improvement of conditions within segregated baseline neighborhoods by 2000.

These modest improvements in socioeconomic context pale in comparison to those achieved through residential mobility under MTO, however. Experimental compliers moved to neighborhoods that not only were substantially less poor (as required by the restricted voucher) but also had levels of adult employment that were nearly double those the compliers had faced in their old neighborhoods. Moreover, the percentage of adults without a high school diploma, as well as the percentage of female-headed households, was much lower in the new neighborhoods; conversely, the percentage of adults with a college degree more than doubled, and the

median household income was triple what it had been in the baseline neighborhoods in 1990. Since public housing developments generally consisted of rental units, the home ownership rate in the new neighborhoods was six times higher than in the baseline neighborhoods. Clearly, those who used their vouchers to move were in more advantageous neighborhoods than they were living in at baseline.

Nonetheless, the degree of improvement for each mover varied by whether the move was to an integrated or segregated neighborhood. Overall, segregated low-poverty neighborhoods were somewhat more disadvantaged than their integrated counterparts, particularly in 2000. Whereas socioeconomic indicators for integrated low-poverty neighborhoods remained stable or improved between 1990 and 2000, segregated nonpoor neighborhoods generally declined in status or saw improvements at more modest rates, despite the decade-long economic boom. For example, during the 1990s, segregated nonpoor areas displayed a falling rate of adult employment (from 65% to 57%), a near-doubling of the rate of poverty (8% to 14%), a rising rate of female headship (24% to 34%), and a falling rate of home ownership (62% to 59%). While median household income improved by nearly 30% in integrated areas, it improved by only 19% in segregated areas; similarly, the proportion of adults with college degrees rose by 16% in integrated areas, compared to a 10% rise in segregated areas. We find, then, that there are qualitative differences in neighborhood resources between nonpoor integrated and nonpoor segregated areas in the MTO sample.

SELECTIVITY DURING IMPLEMENTATION

Like other researchers, therefore, we underscore the fact that not all nonpoor neighborhoods are alike: minority areas are more disadvantaged in socioeconomic terms than nonminority areas and are more likely to experience social and economic decline over time. Thus, even when the experimental compliers relocated to nonpoor neighborhoods, if these neighborhoods were segregated they were in a less advantageous environment than if the neighborhoods were integrated. As a proxy for political, social, and economic resources, racial composition matters, and given that nearly three-quarters of those using MTO vouchers moved to a segregated rather than an integrated neighborhood, the simple comparison of experimental and control group members relies on a more narrow variation of neighborhood conditions than it would have if the experimental group had been required to move to integrated nonpoor neighborhoods, as in the Gautreaux program. This tendency of compliers to move to a segregated neighborhood is built into the design of the MTO

by virtue of its emphasis solely on class and not on race in neighborhood relocation.

Of course, there is no guarantee that people randomly offered a rental subsidy voucher will actually use it, which introduces another potential source of selection bias into the experiment: selective acquiescence to the experimental manipulation. Of the 1,729 experimental group members that were randomly offered a housing voucher to move to a nonpoor neighborhood, only 47% accepted the offer and moved. As can be seen by contrasting parts A and C of table 1, the neighborhood circumstances experienced by noncompliers were not significantly different from those experienced by controls, which is not terribly surprising.

Moreover, even when experimental subjects accepted the offer and used their vouchers to move to nonpoor neighborhoods, they were not required to remain more than one year in the new environment to experience its advantages. This fact introduces yet another source of selection bias into the experiment: selective out-migration. Although the TOT estimates used in previous MTO research take into account selectivity at the time of initial use of the voucher, they do not take into account subsequent mobility. As shown in table 1, there was considerable residential mobility among MTO compliers following relocation, and most of this movement was to less advantaged neighborhoods. By the fourth year of the study, for example, the percentage of compliers residing in integrated nonpoor neighborhoods had dropped from 28% to 21%, while the share occupying nonpoor segregated neighborhoods had fallen from 72% to 46%. Most of the apparent out-migration was to poor, segregated neighborhoods. Whereas, by definition, 0% of MTO compliers lived in such neighborhoods immediately after relocation, by year four the figure was 33%; thus, the effects of the relocation on individual outcomes were diluted.

At the time of the interim evaluation four to seven years after initial relocation, we continue to observe mobility toward poor, segregated neighborhoods among the compliers. Thus, 39% of those households that had originally relocated to a nonpoor neighborhood were back in a segregated, poor neighborhood, though in all likelihood with a lower level of poverty than at baseline. Meanwhile, the share living in integrated low-poverty neighborhoods had dropped to 18%, and the share in segregated nonpoor neighborhoods fell to 41%. In other words, mobility by complier households subsequent to relocation produced a steady shift back into more disadvantaged neighborhood conditions. Nevertheless, though over time the distribution of subjects across neighborhood types came to approach the distribution of controls, a few years after the initial move the experimental compliers were still three times more likely to be living in a nonpoor neighborhood.

Noncompliance during relocation and residential mobility afterward

were not only extensive quantitatively; they were also highly selective qualitatively. Like Sampson et al. (2002), we believe that this selectivity in implementation is something that should be explored rather than simply controlled for. If those turning down the vouchers initially and those leaving neighborhoods after relocation were random cross-sections of the population, no selection bias would result; but this is decidedly not the case, as shown in table 3. Model 1 presents coefficients for a logistic regression model estimated across all experimental subjects, predicting whether they complied with the MTO requirement (i.e., whether they used the voucher to move to a nonpoor neighborhood).

All predictor variables are defined at the baseline survey, just before random assignment. We model the initial decision to use the voucher as well as the more specific decision to use the voucher in an integrated neighborhood. On the basis of the earlier comparison with Gautreaux, it could be argued that we should have measured the use of the voucher in the suburbs; however, our focus on the combination of racial composition and class is in line with Mendenhall et al. (2006), who did not find the city/suburb contrast to be salient. We consider the potential influence of a variety of factors in this decision making. These variables measure demographic background, socioeconomic status, baseline neighborhood circumstances, preferences for the new neighborhood, and motivations for mobility.

The full set of variables encompasses a variety of factors that might influence an individual's decision to accept a voucher and to move to a particular neighborhood. For example, the degree of racial integration in the baseline neighborhood might make an individual more or less comfortable about choosing to move to an integrated neighborhood. Motivations for mobility (measured at baseline) might also predict different outcomes. If, for example, a person is mainly moving to escape the drugs in the neighborhood, she may be satisfied with moving to a neighborhood that is still within the same urban school district, but less drug-ridden. Each metropolitan area offers a unique set of residential options. We include dummy variables for the five metropolitan areas in which the MTO experiments were run (with New York serving as the reference category) as well as a measure of whether the respondent had applied for a housing voucher prior to MTO (indicating a history of mobility aspirations).[6]

As indicated by the overall significance of the model ($P < .001$), membership in the population of experimental compliers was indeed highly

[6] We were unable to run separate models for each city in this article, because the number of people who moved to each type of neighborhood was too small when broken down by location.

TABLE 3

LOGIT MODELS PREDICTING COMPLIANCE AND MOVE TO AN INTEGRATED AREA AMONG EXPERIMENTAL GROUP MEMBERS

VARIABLES AT BASELINE	USED VOUCHER TO MOVE (1)		MOVED TO INTEGRATED AREA (2)	
	B	SE	B	SE
Age of head of household:				
19–29	1.143**	.243	−1.073*	.475
30–39	.621**	.204	−.738†	.407
40–49	−.012	.214	−.754†	.439
50+	. . .	. . .	. . .	. . .
Household size:				
<2 persons	.605**	.180	−.371	.365
3 persons	.344**	.166	−.173	.330
4 persons	.218	.176	−.360	.360
5 persons	. . .	. . .	. . .	. . .
Race:				
African-American	.076	.154	−.640*	.298
Socioeconomic status:				
In school	.509**	.162	−.110	.293
Completed high school or GED	.115	.125	.527*	.252
Has car	.266	.185	−.673*	.322
On AFDC	.113	.172	.616†	.373
Average hours worked/week	−.005	.005	.014	.010
Neighborhood circumstances:				
No family in neighborhood	−.024	.129	−.386	.254
No friends in neighborhood	.120	.123	.547*	.245
Integrated neighborhood	.643*	.267	.430	.505
Attitude about neighborhood:				
Feels safe	−.180	.134	.009	.264
Very dissatisfied	.493**	.134	−.158	.260
Preferred neighborhood mix:				
Wants single-race area	. . .	. . .	. . .	. . .
Wants integration	.292†	.164	.373	.347
Wants other mixed area	.958*	.378	−.011	.635
Desired distance of move:				
Same neighborhood, same city	. . .	. . .	. . .	. . .
Different neighborhood, same city	.581*	.274	.437	.620
Suburbs of same metro area	.848**	.299	1.090	.667
Out of metro area	.865**	.299	.693	.655
Some other location	.386	.445	−.462	.957
Attitude about mobility:				
Wants to get away from drugs	.152	.144	.401	.331
Wants to find better schools	.087	.124	−.287	.239
Confident will find apartment	.162	.121	−.472*	.237
Feelings about nearly all-white school:				
Feels bad or very bad	. . .	. . .	. . .	. . .
Feels good or not sure	.369†	.214	−.296	.417

TABLE 3 (*Continued*)

Variables at Baseline	Used Voucher to Move (1)		Moved to Integrated Area (2)	
	B	SE	B	SE
Feels very good	.585*	.258	−.789	.512
Prior Section 8 experience:				
Applied previously	−.053	.139	−.185	.274
Applied previously × Boston	.768**	.286	−.379	.562
Metropolitan area:				
New York	. . .	. . .	. . .	. . .
Baltimore	.217	.193	1.130**	.406
Boston	−.730**	.252	4.386**	.559
Chicago	−.761**	.197	.367	.500
Los Angeles	1.005	.218	1.540**	.399
Intercept	−2.760**	.450	−2.411*	.958
−2 log likelihood	1,746.055**		529.626**	
Omnibus χ^2	241.442*		300.832**	
N	1,482		720	

NOTE.—Variables with no data (ellipses) are reference categories.
† $P < .10$ (all two-tailed tests).
* $P < .05$.
** $P < .01$.

selective. Those who accepted the voucher and used it to relocate to a low-poverty neighborhood were generally younger, lived in smaller households, were more likely to be enrolled in school and to inhabit an integrated neighborhood, and were more dissatisfied with their current neighborhood circumstances. Other things being equal, they preferred to reside within neighborhoods integrated by two or more racial-ethnic groups, were looking to leave their current neighborhood, and generally felt comfortable with the prospect of having their children attend a predominantly white school. Compared with New York, the odds of compliance were the same in Baltimore, higher in Los Angeles, and lower in Boston and Chicago, though the interaction term indicates that among those Bostonians who had previously applied for a Section 8 certificate, the odds of compliance were the same as in New York (which may be seen by combining the negative effect of Boston with the equal-sized positive interaction coefficient).[7] These findings are similar to those found in Shroder's (2001) earlier analysis of take-up in the MTO demonstration.

[7] This interaction term was included because there are 64 housing authorities in the Boston metropolitan area with separate waiting lists. Since these waiting lists made Section 8 potentially more accessible to the applicant, an interaction term for Boston and previous application is a reliable indicator that the individual perceived a net benefit to moving (Shroder 2001).

Model 2 of table 3 presents coefficients for a model estimated across experimental compliers to predict whether or not they moved to an integrated neighborhood. Again, the choice of integrated versus segregated was highly selective ($P < .001$). Among the participants who accepted a voucher, those who used it to enter a racially integrated neighborhood tended to be older, not African American,[8] high school graduates, on welfare, and without a car. Having friends in the baseline neighborhood deterred people from relocating to an integrated area, as did having a high degree of confidence about finding an apartment. Compared with New York, the choice of an integrated neighborhood was more likely in Baltimore, Boston, and Los Angeles, but about the same in Chicago.

The foregoing analyses indicate the substantial degree to which selection is likely to have entered the MTO experiment in the course of its implementation. First, the decision to accept and use subsidy vouchers was selectively taken by younger people from integrated neighborhoods who were still in school, living in smaller households, dissatisfied with their current neighborhood, and willing to move farther away; they were also more open to the possibility of their children attending predominantly white schools and living in an integrated neighborhood. Second, after going through this selection process, those who moved to an integrated rather than a segregated neighborhood were older, better-educated non-African-Americans who lacked friends in the baseline neighborhood and were not so confident about finding a new apartment.

In addition to these two stages of selective decision making, mobility subsequent to initial relocation for experimental compliers was likewise selective and evidently quite path dependent. In table 4 we show coefficients for two logistic regression models. Model 1 predicts residence in a low-poverty neighborhood four years after relocation, given conditions at baseline and whether the initial move was to an integrated neighborhood. Model 2 predicts residence in an integrated neighborhood four years after relocation using the same variables.[9] Given the reduction in the number of cases and consequent limitations on degrees of freedom, we were unable to estimate a multinomial model to predict residence across all four types of neighborhoods simultaneously, and so we estimated separate models for poor/nonpoor and integrated/segregated residence. Likewise, we also had to reduce the number of independent variables used in

[8] Ninety-three percent of MTO household heads were African-American or Hispanic, and the remainder were white or Asian (Orr et al. 2003).

[9] Table 4 predicts residence in nonpoor and integrated neighborhoods. These may be the same neighborhoods that compliers lived in at placement, or they may be different neighborhoods. As shown in table 1, there had been substantial mobility among compliers by four years after random assignment, so for many of the households, these are indeed different neighborhoods.

TABLE 4

Logit Models Predicting Place of Residence Four Years after Placement

Independent Variables	In Nonpoor Neighborhood (1)		In Integrated Neighborhood (2)	
	B	SE	B	SE
Placement neighborhood:				
Integrated	.657**	.247	4.095**	.354
Age of head:				
19–29	−.295	.348	.128	.557
30–39	−.201	.315	.492	.480
40–49	−.196	.332	−.055	.500
50+	. . .	. . .	. . .	. . .
Household size:				
<2 persons	−.305	.253	.182	.415
3 persons	−.317	.234	.003	.390
4 persons	−.209	.246	.092	.413
5 persons	. . .	. . .	. . .	. . .
Race:				
African-American	−.160	.213	.134	.314
Socioeconomic status:				
In school	−.071	.204	.165	.344
Completed high school or GED	.145	.170	.035	.287
Has car	−.105	.232	−.050	.375
On AFDC	−.058	.246	.059	.390
Working	−.103	.205	−.522	.357
Baseline neighborhood circumstances:				
No family in neighborhood	.234	.177	.733*	.310
No friends in neighborhood	−.062	.167	.018	.278
Integrated	1.224*	.500	1.411**	.475
Attitude about baseline neighborhood:				
Feels safe	−.144	.181	−.128	.297
Very dissatisfied	−.148	.183	−.286	.295
Attitude about mobility:				
Wants to get away from drugs	.197	.208	.058	.371
Wants to find better schools	.053	.168	.193	.281
Confident will find apartment	−.256	.164	.490†	.283
Metropolitan area:				
New York	. . .	. . .	. . .	. . .
Baltimore	.499†	.266	.198	.482
Boston	−.163	.345	−.245	.478
Chicago	.116	.259	−.586	.564
Los Angeles	−.276	.259	−.260	.506

TABLE 4 (*Continued*)

Independent Variables	In Nonpoor Neighborhood (1)		In Integrated Neighborhood (2)	
	B	SE	B	SE
Intercept	.947*	.455	−4.375**	.766
−2 log likelihood	927.454		407.655	
Omnibus χ^2	51.171		368.922	
N	761		761	

Note.—Variables with no data (ellipses) are reference categories.
† $P < .10$ (all two-tailed tests).
* $P < .05$.
** $P < .01$.

order to achieve convergence, which we accomplished by removing indicators that proved not to be significant in table 3 or in earlier trials for the models in table 4.

The path dependence of neighborhood circumstances is indicated by the powerful effect of initial relocation. As can be seen, those whose initial moves were to integrated, nonpoor areas were much more likely to live in a low-poverty neighborhood four years later and were markedly more likely to remain in an integrated setting.[10] In other words, the group of experimental households that we see moving back to poor neighborhoods in table 1 most likely consists of those who moved to segregated nonpoor neighborhoods initially. The only other factors that appear to affect subsequent location are neighborhood circumstances at baseline. Those living in an integrated neighborhood at baseline were much more likely to live in nonpoor neighborhoods that were also in integrated areas four years later. In contrast, having family in the baseline neighborhood lowered the odds of living in an integrated tract four years later, suggesting that participants may have been reluctant to rupture familial networks for the sake of integrated living, a finding consistent with recent results from a new Gautreaux initiative (Boyd et al. 2006). However, those who were confident in their ability to find an apartment were significantly more likely to reside in an integrated neighborhood in the study's fourth year.

LENGTH OF TIME IN NEIGHBORHOODS

The foregoing analyses hold several important lessons for the measurement and interpretation of neighborhood effects using MTO data.

[10] These results are similar to those found in a sample of Gautreaux participants 6 to 22 years after placement (Keels et al. 2005).

Researchers analyzing MTO data have long been aware of endogenous variation in the use of the voucher and its potential influence on subsequent mobility. Because of random assignment, they assume quite correctly that this individual variation is the same in the experimental and control groups—that is, the control group represents a true counterfactual for the experimental group, because a similar proportion of the group would have used the voucher had they been offered it. In this sense, previous analyses of MTO data are highly successful in measuring the effects of the policy initiative, namely, the offering of a housing voucher to public housing residents.[11] They are less successful, however, in capturing neighborhood effects (the effect of living in an advantaged rather than a disadvantaged neighborhood).

In order to capture neighborhood effects, it is not enough to simply measure the effect of the voucher offer (i.e., the ITT estimate), since over half of all vouchers offered were not used by recipients. As a result, members of the experimental group were exposed to a range of neighborhood conditions over four to seven years, making it difficult to see how neighborhood effects can be measured with this comparison. At best, it measures a policy treatment. It is also insufficient to consider only the circumstances of those who complied with the MTO criteria and moved into a nonpoor neighborhood (i.e., the TOT estimate), as nearly three-quarters of these people went to segregated areas, which are more disadvantaged than integrated areas. Finally, the simple fact of initial relocation into a poor or nonpoor neighborhood, whether integrated or segregated, is unlikely to yield strong neighborhood effects in the aggregate, because subsequent residential mobility was extensive and selective.

One possible remedy for the erosion of neighborhood advantage gains over time is to use a duration variable that captures time spent in certain types of neighborhoods. Over the years, a variety of mechanisms have been hypothesized to account for "neighborhood effects," but one thing they all have in common is that they require time to operate (Jencks and Mayer 1990). Whatever the specific mechanism hypothesized—whether it is the presence of negative role models or the absence of positive role models, as predicted by socialization theories (Wilson 1987); the lack of

[11] A recent article by Sobel (2006) questions the ability of the ITT and TOT estimates to capture the policy effects of MTO. He argues that the parameters are estimated using the no-interference assumption and that this assumption is violated in the MTO demonstration because social interaction among participants cannot be ruled out definitively. This potential for social interaction means that the response by an individual unit may depend on the treatment received by another unit. If this interference is present, this means that the ITT is measuring not a causal parameter, but rather "the difference between two neighborhood effects" (p. 1399)—namely, the effect on the experimental group and the spillover effect on the control group.

information about jobs or the absence of personal links to workers, as predicted by network models (Wilson 1996); the lack of public trust and collective efficacy, as predicted by social capital models (Sampson 2004); the spatial isolation from jobs and resources, as predicted by the mismatch hypothesis (Kain 1968); or the side effects of stress-induced allostatic load, as predicted by biosocial models (Massey 2004)—no mechanism takes effect overnight. All commonly postulated mechanisms of neighborhood influence require the passage of time to exert their effects, and therefore, all involve an interaction of space and time.

Despite this fact, most studies to date have examined neighborhood effects at a certain point in time, rather than in a dynamic way, since longitudinal data on neighborhood change are not as readily available or easy to analyze. Two recent studies have focused on this temporal variation in neighborhoods, with different results. Jackson and Mare (2005) looked at neighborhood change over a two-year period and found that using a dynamic measure of neighborhood poverty was not significantly different from using current neighborhood poverty when estimating children's behavioral well-being. The length of time that neighborhoods are allowed to vary is also important. Wheaton and Clarke (2003) found that neighborhood socioeconomic disadvantage during childhood had a negative effect on early adult mental health and that this effect explained the association between current neighborhood and mental health. In fact, one might argue that adults have a more difficult time benefiting from the MTO initiative because they have been exposed to neighborhood disadvantage for a longer period of time than their children. As mentioned above, however, some treatment effects for adults have been found in the areas of mental health and obesity.

The MTO interim data were collected four to seven years after random assignment, allowing for a substantial measure of length of time in poor and nonpoor neighborhoods. Recently, Kling et al. (2007) also included a measure of neighborhood poverty weighted by duration since random assignment to MTO. Using ordinary least squares (OLS) regression on just control group households (a nonexperimental test), they found that there was little significant effect of neighborhood poverty on an array of adult and youth outcomes (only teen male physical health was negatively associated with neighborhood poverty). However, when they used site-by-treatment-group interaction variables as excluded instruments for neighborhood poverty (with the full sample), neighborhood poverty was significantly associated with several outcomes (and had the opposite sign for teen male physical health). Results for adult economic self-sufficiency alone were not shown, but duration-weighted neighborhood poverty was negatively associated with an overall index of adult outcomes (showing

a beneficial effect of lower poverty rates), which was mainly accounted for by its significant effect on adult mental health.

Nonetheless, in most prior analyses of MTO data, researchers found no significant effect of the treatment on adult employment, earnings, or receipt of public benefits (Kling et al. 2004). TOT estimates for the compliers have documented large improvements to neighborhood conditions in comparison to controls (increases of 20 percentage points in feeling safer and 7.5 percentage points in share of employed adults, and a decrease of 17 percentage points in poverty rate) sustained four to seven years after random assignment (Orr et al. 2003). With these neighborhood-level improvements, shouldn't we see an effect on adult economic self-sufficiency? Various explanations have been offered as to why no treatment or "neighborhood" effect has been found. The economic boom in the 1990s raised the employment levels of adults in both the control and experimental groups, which perhaps made it more difficult to detect a significant change in the experimental group. Analyzing qualitative interview data in Baltimore from MTO experimental and control group adults, Turney et al. (2006) found that adults who were experimental compliers were not using their local networks for job information, and this was partly due to a skills mismatch between their neighbors and themselves. Another explanation may be that adults who moved with the low-poverty MTO voucher simply were not in higher-resource neighborhoods long enough to affect their employment as measured by TOT and ITT estimates.

If neighborhood effects take time to operate, then a measure of duration should be included in models as a covariate, rather than only using a measure of whether someone was offered a voucher. Though this inclusion can take us away from the experimental design, it is necessary in order to test the association between the length of time spent in a nonpoor neighborhood and adult economic self-sufficiency, as hypothesized under prevailing theoretical models. It is also important to note that reverse causality may play a role here as well. Individuals who live in a nonpoor neighborhood and are employed may be more likely to remain in that neighborhood or a similar neighborhood rather than move back to a poor neighborhood. The goal of this part of the analysis is not to somehow find a "clean" neighborhood effects estimate that will control away all elements of selectivity or of human interaction with the environment. As Sobel's (2006) critique of MTO research points out, it is perhaps impossible, and certainly unrealistic, to suggest that there is a neighborhood effect or a residential mobility treatment that is devoid of human agency. Rather, our goal is to explore another way of using MTO data, albeit nonexperimentally, that takes into account selectivity at each stage of mobility by measuring duration in a certain type of neighborhood.

In our statistical analysis, therefore, we predict socioeconomic outcomes

using a comprehensive set of baseline variables, and in addition to including dummy variables for treatment category (control, experimental complier, or experimental noncomplier), we add into the equation the total amount of time accumulated in different kinds of neighborhoods: segregated poor, integrated poor, segregated nonpoor, and integrated nonpoor. In such a specification, the design effect is captured by the dummy variables for treatment category, but the influence of neighborhood conditions is captured by the cumulative experience in different types of neighborhoods.

The Need for Reanalysis

The need for such an approach to analysis is suggested by figure 1, which shows the total number of months spent by members of each treatment group in each kind of neighborhood. In tabulations not shown, we found that 60% of the entire sample spent no time at all in a low-poverty tract. The average amount of time spent within integrated, low-poverty neighborhoods was, naturally, minuscule for controls (2.7 months) and for experimental noncompliers (only 1.8 months). Even among experimental compliers, however, relatively little time was logged within integrated low-poverty settings, with the average being just 14.9 months, or a little over a year.

Moreover, the amount of time spent by compliers in integrated, nonpoor neighborhoods was not only below that spent in segregated nonpoor neighborhoods—it also fell below the amount of time logged in segregated poor neighborhoods. Compared with 14.9 months spent in an integrated, low-poverty setting, complier subjects spent an average of 32.3 months in segregated low-poverty neighborhoods and 21.0 months in segregated poor neighborhoods. It is thus not surprising that comparisons between experimental subjects and controls carried out in prior studies have failed to detect clear differences between the two groups on outcomes such as economic self-sufficiency and children's education. Even experimental compliers spent an average of just a year and three months in the kind of neighborhood that was characteristic of suburban movers who relocated under the Gautreaux program.

Modeling Economic Self-Sufficiency

In seeking to replicate the results of the Gautreaux experiment, then, we would not really expect to see significant differences between experimental and control groups with respect to socioeconomic outcomes, given the relatively small amount of time that the experimental group as a whole spent in nonpoor neighborhoods (a total of 26 months). Instead, we hy-

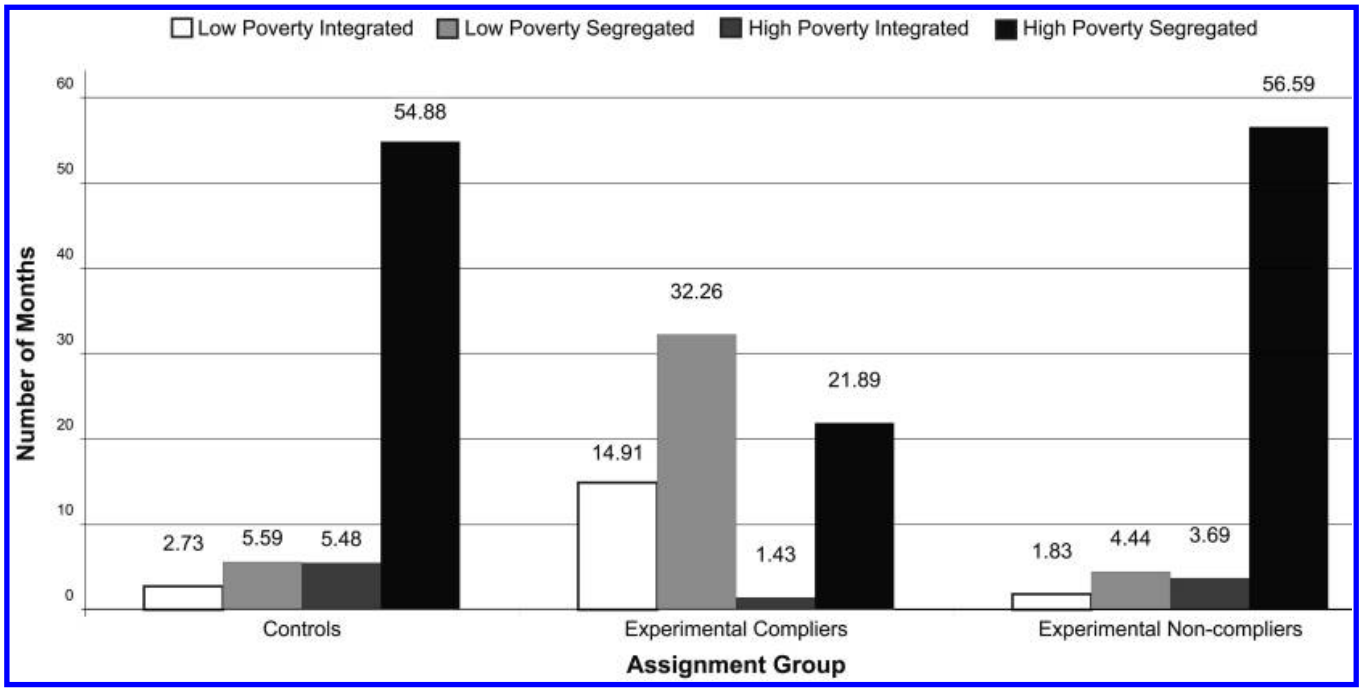

FIG. 1.—Time spent in different kinds of neighborhoods by assignment group

pothesize that the cumulative time spent living in nonpoor neighborhoods will be positively associated with socioeconomic status, net of assignment and other factors, and furthermore, that total time spent within integrated, nonpoor settings will be more beneficial socioeconomically than time spent in segregated, nonpoor neighborhoods. Table 5 tests these hypotheses by regressing adult employment and earnings on the total time spent in segregated nonpoor neighborhoods, integrated nonpoor neighborhoods, and poor neighborhoods while controlling for assignment group, location, and a set of baseline variables, most of which overlap with those used in the earlier models predicting use of the voucher.[12]

Model 1 shows results based on whether or not adult members of MTO households were employed as of the interim assessment. As in other analyses of MTO data, we found that receiving a randomly allocated housing voucher had no significant effect on adult employment, irrespective of whether or not the voucher was used to relocate. Neither of the two experimental coefficients (complier or noncomplier) was significantly different from zero, a result that has induced others to conclude that neighborhood conditions have no effect on employment.

However, this conclusion is premature. As can be seen from the significant neighborhood exposure coefficients ($P < .05$, $P < .01$; one-tailed test), cumulative time logged in a nonpoor environment has a rather strong, significant, and positive association with the odds of being gainfully employed by the time of the interim evaluation. Each additional month of residence in a nonpoor neighborhood is associated with a 1.1% increase in the odds of holding a job. In violation of our expectations, however, this statement holds true whether the neighborhood is integrated or segregated (the coefficient is 0.011 in both cases). Thus, in terms of getting a job, exposure to a nonpoor environment is important, not whether the neighborhood is integrated or segregated.

Model 2 of table 5 presents the results of a Tobit analysis predicting the weekly earnings reported by adults at the interim evaluation. In this case, accumulating time in an integrated nonpoor neighborhood appears to be slightly more beneficial than accumulating time in a segregated nonpoor neighborhood, though the difference between the two coefficients is not statistically significant. According to the model, each additional month spent in an integrated, nonpoor neighborhood is associated with a \$1.89 increase in weekly earnings ($P < .05$, one-tailed test), whereas a month spent in a segregated nonpoor neighborhood is associated with a \$1.53 increase ($P < .10$, one-tailed test). In contrast, time spent in a poor

[12] Because the number of cases in integrated poor tracts was so small, we combined integrated and segregated poor tracts into one category.

TABLE 5
Logit Model Predicting Adult Employment and Tobit Model Predicting Weekly Earnings (at interim evaluation)

	Employed (1)		Weekly Earnings (2)	
Independent Variables	B	SE	B	SE
Assignment group:				
Control	. . .	. . .	. . .	. . .
Experimental noncomplier	.050	.109	18.135	20.742
Experimental complier	−.078	.147	−20.467	26.489
Months of neighborhood exposure:				
Low-poverty integrated	.011*	.005	1.891*	.912
Low-poverty segregated	.011**	.004	1.530†	.810
Poor	.005	.004	1.110	.732
Metropolitan area:				
New York	. . .	. . .	. . .	. . .
Baltimore	.031	.169	−28.644	32.113
Boston	−.122	.156	−3.865	28.849
Chicago	−.004	.154	−61.755*	29.157
Los Angeles	−.088	.159	−36.778	29.860
Constant	−2.575**	.465	−463.922**	87.857
χ^2	477.308**		473.040**	
−2 log likelihood	2,935.173**		8,695.903**	
N	2,293		2,166	

Notes.—Baseline controls are not shown. Variables with no data (ellipses) are reference categories.
† $P < .10$ (nonpoor neighborhood coefficients tested with one-tailed test).
* $P < .05$.
** $P < .01$.

neighborhood has no significant relation with weekly earnings, other things being equal.

Table 6 considers two indicators of economic self-sufficiency: whether or not the respondent was receiving Temporary Assistance for Needy Families (TANF) at the interim evaluation and whether or not the respondent received food stamps at that time. As indicated by model 1, although assignment group membership has no effect on TANF receipt, the more time spent in nonpoor neighborhoods, the lower the odds of receiving payments by the time of the interim assessment. Each additional month spent in an integrated nonpoor neighborhood is associated with a 0.9% decrease in the odds of receipt ($P < .10$, one-tailed test), and each additional month spent in a segregated nonpoor neighborhood is associated with a decrease in the odds of receipt by 0.8% ($P < .10$, one-tailed test).

Model 2 of table 6 continues the analysis of self-sufficiency by estimating the effect of assignment group and neighborhood exposure on the use of

TABLE 6
LOGIT MODELS PREDICTING RECEIPT OF TANF AND FOOD STAMPS AT INTERIM EVALUATION

	USING TANF (1)		ON FOOD STAMPS (2)	
INDEPENDENT VARIABLES	B	SE	B	SE
Assignment Group:				
Control	. . .	. . .	. . .	. . .
Experimental noncomplier	−.108	.123	−.116	.109
Experimental complier	.071	.169	.279†	.149
Months of neighborhood exposure:				
Low-poverty integrated	−.009†	.006	−.015**	.005
Low-poverty segregated	−.008†	.005	−.014**	.004
Poor	−.003	.004	−.009*	.004
Metropolitan area:				
New York	. . .	. . .	. . .	. . .
Baltimore	−.879**	.197	−.200	.169
Boston	−.381*	.176	−.305†	.157
Chicago	−.708**	.172	.498**	.152
Los Angeles	.871**	.167	.472*	.159
Constant	−1.058†	.522	−.031	.461
χ^2	432.260**		465.301**	
−2 log likelihood	2,442.763**		2,982.015**	
N	2,287		2,292	

NOTES.—Baseline controls are not shown. Variables with no data (ellipses) are reference categories.
† $P < .10$ (nonpoor neighborhood coefficients tested with one-tailed test).
* $P < .05$.
** $P < .01$.

food stamps at the interim assessment. In this equation, being in the complier group is associated with a significantly higher risk of using food stamps at the interim, compared with others. In other words, those respondents who were offered a housing voucher and used it to move were significantly more likely to use food stamps than controls ($P < .10$). Despite this underlying proclivity, however, living within low-poverty neighborhoods was also associated with a steady reduction in the odds of receiving food stamps over time. Living in an integrated low-poverty neighborhood had the strongest association with reduced dependency, followed by living in a segregated low-poverty neighborhood. Each month spent in an integrated nonpoor setting was associated with a 1.5% reduction in the odds of receiving food stamps, whereas each month spent living in a segregated nonpoor setting was associated with a 1.4% reduction, compared with a 0.9% reduction in poor neighborhoods. Though these differences are not significant statistically, they nonetheless correspond to the hypothesized order of effect size. All subjects tend to move off of food stamps over

time, but those living in low-poverty neighborhoods seem to do so a little more rapidly.

All four models in tables 5 and 6 test the idea that how long a person lives in nonpoor neighborhoods may make a difference in terms of economic self-sufficiency. The flip side of this idea is that moving, and the disruptions that it entails, may have a negative impact on adult economic outcomes. If this were the case, then including the number of moves since random assignment into the models might change the neighborhood duration relationship. When we included the number of moves into the same models used in tables 5 and 6, we found that, controlling for all other variables, the number of moves is in fact unrelated to any of the dependent variables except for employment. The coefficients for neighborhood duration as documented in tables 5 and 6 did not change in the models.

In separate models not shown here, we regressed each of the dependent variables on the set of factors that had been used to predict use of the voucher and use in an integrated nonpoor neighborhood (table 3). We did this in order to determine how the factors that are significant in selectivity in mobility are associated with these individual outcomes.[13] Though the variables that one might intuitively expect to be related to adult economic self-sufficiency (baseline receipt of AFDC, baseline hours worked per week, high school diploma) have significant coefficients in the expected direction, other variables are not consistent in their relation to the dependent variables. For example, a variable that was significantly associated with moving to an integrated neighborhood—no friends in the baseline neighborhood—was not associated with any of the self-sufficiency outcomes. Living in an integrated neighborhood at baseline was significantly associated with raising the odds of taking up the voucher, as well as with living in an integrated neighborhood or a nonpoor neighborhood four years later. However, this coefficient was only significant for employment ($P < .05$) and earnings ($P < .10$); it was associated with lowering the odds of employment and lowering earnings four to seven years later.

As with all survey-based estimates, of course, endogeneity represents a potential bias in estimating cross-sectional models from survey data. For example, as we mentioned earlier, if an individual is employed, she may be more likely to stay in a nonpoor neighborhood. Although we cannot eliminate the threat of endogeneity from such regressions, we can adjust for its effects using instrumental variable techniques. Following the approach of other MTO researchers (Kling et al. 2007; Ludwig and

[13] Most of these variables were included as controls in tables 5 and 6. We omitted a few (such as feelings about a predominantly white school) in order to stay consistent with the models that are traditionally used in MTO analysis, so as to be able to compare results.

Kling 2007), we attempted to use treatment-by-location interactions as instruments to predict time spent in low-poverty and moderate- to high-poverty neighborhoods in two-stage least squares estimation (2SLS). In theory, such interactions can be appropriate instruments, because assignment to treatment is random at each site. However, since site is correlated with the error term, this interaction may violate the exogenous requirement for instrumental variable estimators. In practical terms, this approach yielded very unstable coefficient estimates. Standard errors associated with duration coefficients estimated using 2SLS varied dramatically compared with those estimated using OLS (the inflation factor ranged from 34 to 149). This apparent unreliability in the estimates gave us little confidence in the efficacy of 2SLS as a means to overcome potential endogeneity bias in the MTO data for this analysis.

NEIGHBORHOOD EFFECTS RECONSIDERED

In this article, we have undertaken a detailed conceptual and empirical analysis of data from the Moving to Opportunity experiment. In order to overcome potential selection biases, investigators associated with the MTO program drew upon Chicago's Gautreaux program to design and implement an experimental study in which residents of public housing projects were randomly offered housing subsidy vouchers that were to be used for renting private units in low-poverty neighborhoods. Prior studies based on the resulting data generally have found that while MTO had a significant effect in improving neighborhood conditions for those in the experimental treatment group, it had a limited influence on individual outcomes, causing some to question the importance of neighborhood effects.

Several features of MTO's design and implementation, however, work against the detection of strong neighborhood effects. Whereas Gautreaux explicitly focused on race in promoting residential mobility out of poor, segregated housing projects, MTO focused on class. Participants in MTO were thus required only to use their vouchers to move to a low-*poverty* neighborhood, whereas half of those in the Gautreaux demonstration project were required to use theirs to move to a low-*minority* suburb. As a result, Gautreaux participants were exposed to a wide range of neighborhood conditions, and MTO experimental families experienced a more limited range of neighborhood types. Because of the legacy of racism and segregation in American cities, nonpoor black neighborhoods are not equivalent in terms of resources to nonpoor white neighborhoods. Most MTO participants used their vouchers to move into a segregated rather than an integrated neighborhood. To a significant degree, therefore, MTO

shuffled families around within the confines of racially segregated neighborhoods, exposing them to a limited range of resources and opportunities.

In addition, despite attempts at random allocation of subjects to treatment conditions, considerable selectivity entered the study's design because families randomly allocated housing vouchers were not required to use them. Among those assigned to be experimental subjects, selection into the category of people who complied with the experimental treatment and moved to a low-poverty neighborhood was decidedly nonrandom. A second layer of selection occurred even among those who complied with experimental criteria, because the decision to take up residence in an integrated versus a segregated nonpoor neighborhood was also nonrandom. Substantial research suggests that African-Americans are reluctant to enter white neighborhoods for fear of ostracism and harassment (see Charles 2003). Those who chose to enter the more advantaged of the two kinds of eligible neighborhoods were thus a distinct subset of all MTO movers.

Finally, a third layer of selection occurred after the initial relocation to nonpoor neighborhoods. Since compliers were under no obligation to remain in such neighborhoods, over time there was widespread movement out of low-poverty neighborhoods back into poor settings. As a result of widespread and selective out-migration from neighborhoods after initial relocation, experimental subjects accumulated relatively little time in low-poverty settings, and the time spent within low-poverty neighborhoods that were also racially integrated was particularly limited. Given that entry into neighborhoods and compliance categories was highly selective and the length of stay quite variable, it is hardly surprising that comparisons made between experimental and control group members in MTO have failed to yield the robust and consistent evidence of neighborhood effects found in survey-based studies.

In order to model neighborhoods in a way that takes into account selectivity and duration, we developed an alternative analytic strategy that involved (1) measuring the cumulative amount of time spent by subjects in different kinds of neighborhood environments (poor, nonpoor segregated, and nonpoor integrated); (2) measuring the influence of variables known to affect selection into categories of compliance, entry into integrated versus segregated areas, and out-migration; and (3) including these measures in statistical models predicting employment, earnings, and service dependency, in addition to dummy variables for assignment group. Following this approach, we found that time spent in more advantageous neighborhood environments was indeed associated with the self-sufficiency of adult participants in the MTO program.

Despite the random allocation of participants to treatment and control groups, of course, the regression results we obtained do not themselves

constitute experimental evidence in favor of neighborhood effects. Significant coefficients linking time spent in nonpoor and poor neighborhoods to employment, wages, and the receipt of transfers cannot be taken to indicate causality, only correlation, and our attempts to improve the estimates using instrumental variables met with little success. Fortunately, our goal here has not been to prove the existence of neighborhood effects per se, but to show that the MTO experiment does not refute them either. We simply point out that when we measure the amount of time spent in advantaged and disadvantaged neighborhoods, we find patterns consistent with what we would expect if there *were* causal neighborhood effects.

This research carries important implications for future work using MTO data. Measuring the effect of a voucher offer is vital to assessing the experimentally sound results of the policy demonstration. However, if MTO data are used to measure neighborhood effects, different assumptions and techniques should be considered. Researchers should pay attention to whether the offer was accepted and used, whether those who complied with the program's requirements and moved to a low-poverty area entered an integrated or segregated neighborhood, and, finally, how long they remained in the new environment. Furthermore, when modeling neighborhood effects it is critically important to measure the cumulative time spent in different kinds of environments, while holding constant the influence of variables responsible for selection into the category of compliers, into integrated versus segregated neighborhoods, and into the population of out-migrants from new neighborhoods. Yet we should be clear that including these features in the model sacrifices MTO's experimental design. In future research, we propose to apply these lessons to studying neighborhood effects on health and other developmental outcomes.

This research also has implications for policy. Though the number of families receiving voucher assistance has declined in recent years, the Section 8 voucher program remains the largest source of low-income housing subsidies. Most of these vouchers do not have place-based restrictions, as the MTO experimental group did. However, there are initiatives around the country that offer geographically restricted vouchers in an effort to move families from concentrated poverty into more resource-rich neighborhoods. For example, the proposed remedy in the Thompson desegregation case in Baltimore calls for a regional approach in moving families out of poor segregated neighborhoods and into racially integrated and resource-rich areas. Moreover, one of the 2008 presidential candidates called for an expansion of the voucher program targeted to areas of job growth. Previous researchers have shown that MTO movers experienced significant improvement in neighborhood safety and mental health. Our analyses here provide strong evidence that neighborhoods may "matter" in terms of adult economic self-sufficiency as well, though having more

specific neighborhood targets and a longer required stay than MTO did are important. Thus, there is substantial evidence for keeping geographically specific vouchers on the table in order to offer low-income families a chance to improve their well-being.

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