



# Moving to Opportunity: the Political Effects of a Housing Mobility Experiment

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## **Moving to Opportunity: The Political Effects of a Housing Mobility Experiment**

### **Abstract:**

In 1994, the U.S. Department of Housing and Urban Development launched the *Moving to Opportunity for Fair Housing Demonstration Program* (MTO), a lottery that offered poor families vouchers to move out of public housing into private apartments. Drawing on recently collected vote history data, this study reveals that MTO has had the unintended consequence of reducing voter turnout among participating adults. The low turnout may be due to the loss of social ties that accompanied mobility. The findings suggest that residential mobility, a popular tool in the fight against poverty, may strain poor Americans' weak ties to the political system.

**Keywords:** MTO, poverty, mobility, neighborhoods, political participation

### **Author Note:**

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More than a quarter of the metropolitan poor, nearly seven million people, live in neighborhoods with high concentrations of poverty, defined as census tracts with poverty rates of 30 percent or more (Kingsley and Pettit 2003). Researchers have long noted that the “spatial concentration of poor people acts to magnify poverty and exacerbate its effects,” by isolating the poor from the job opportunities, better schools, private investment and social supports essential to struggling families (Jargowsky 1997, 1). In 1994, the U.S. Department of Housing and Urban Development (HUD), in an effort to reduce concentrations of poverty and move poor families to self-sufficiency, recruited over 4000 families, from five different cities, to participate in an ambitious social experiment: a lottery that offered participants the opportunity to move out of public housing in high-poverty neighborhoods into private apartments in low-poverty communities. For the policy designers, the goal of the *Moving to Opportunity for Fair Housing Demonstration Program* (MTO) was to improve the labor market outcomes, educational achievement and health of low-income adults and their children, using the tool of residential mobility. But residential mobility also has the potential to disrupt and, perhaps, transform—for better or for worse—the political lives of poor Americans. By imposing new administrative burdens (e.g. the need to change voter registration), while at the same time exposing participants to new social environments and networks, the MTO experimental intervention, unintentionally, manipulated both the costs and incentives to electoral participation, with unknown consequences for the behavior of the targeted adults. This article examines the unintended political consequences of the move to opportunity, and considers whether residential mobility, an increasingly popular tool for combating poverty, is likely to strengthen or to further strain poor Americans' weak ties to the political system.

Drawing on vote history data for the adult participants in the MTO demonstration, I measure the effect of the mobility lottery on subsequent rates of voter registration and voter turnout. The analysis reveals that, on balance, the MTO intervention has had detrimental effects on political participation. Participants who “won” the lottery—that is, adults whose families were randomly allocated a housing voucher for use in moving to a low-poverty neighborhood—were less likely to vote in a subsequent national election. In the domain of politics, the costs of residential mobility may outweigh the benefits that come from access to the resources, role models, and recruiting agents available in more advantaged neighborhoods. The results have implications for policymakers concerned with poverty, as well as for scholars interested in the determinants of political participation.

### **The Perils of Concentration and the Promise of Mobility**

There is a large and growing empirical literature documenting the considerable challenges faced by the residents of high-poverty neighborhoods. Ethnographic accounts, as well as large-scale statistical analyses, confirm that poor families in areas of concentrated poverty fare substantially worse on a wide range of outcomes (e.g. chronic joblessness, adolescent delinquency, criminal behavior, depression, obesity) than do similarly poor families with more affluent neighbors (Wilson 1987; Jencks and Mayer 1990; Ellen and Turner 1997; see Sampson, Morenoff and Gannon-Rowley 2002 for review of this literature). A critical mass of economically stable residents provides a community with an essential buffer against physical decay and social disorder, while also helping to maintain the institutions and networks vital to economic and social well-being.

Emerging alongside, and in response to, the research on concentration effects have been a number of studies that examine what happens when policy interventions offer residents a way out of their high-poverty neighborhoods. Beginning with the Gautreaux program in Chicago, and continuing with studies of HOPE VI and MTO, economists, sociologists, and public health experts have used longitudinal data on program participants to measure the benefits of mobility, and to determine whether living among more affluent neighbors generates positive externalities for the poor (Rubinowitz and Rosenbaum 2000; Keels et al. 2005; Mendenhall, DeLuca and Duncan 2006; Popkin et al. 2004; Popkin 2007; Orr et al. 2003; Kling, Liebman, Katz 2007; Kling, Ludwig, Katz 2005; Leventhal and Brooks-Gunn 2003; Ludwig, Duncan, Hirschfield 2001; Katz, Kling, Liebman 2001; Popkin, Leventhal and Weismann 2008). The findings have been mixed. Mobility programs have been linked to improvements in quality of life, perceptions of safety, and mental and physical health. For children, moving to low-poverty areas has contributed to gains in educational achievement and success, and declines in the incidence of delinquent and risky behavior, especially for girls. But, with the exception of Gautreaux, the interventions have had no effect on employment, earnings or self-sufficiency. Furthermore, emotional and behavioral outcomes for boys rarely improved and in some cases worsened following relocation. As for the political consequences of these mobility programs, the extant research is silent.

Yet political scientists have hardly been silent on the subjects of poverty, mobility and political participation. Political engagement is sharply stratified by class, with the poor considerably less active than the rich (Wolfinger and Rosenstone 1980; Verba, Schlozman and Brady 1995). This pattern reflects, in part, the role of individual resources such as education and income in facilitating political activity. While attention to individual poverty has dominated the

research, with a consensus that the poor participate less because they have fewer resources to spare, scholars have hypothesized that the physical environments in which many poor people live—namely, neighborhoods with high concentrations of poverty—also shape their political behavior, and may be contributing to the low participation typical of this group. In fact, studies show that low-income residents of high-poverty neighborhoods are less likely to vote in local and national elections (Alex-Assensoh 1998, 2001), and to make political donations and attend community meetings (Cohen and Dawson 1993) than similarly poor residents of more affluent areas. These relationships are most pronounced among poor blacks, but are not limited to them. Alex-Assensoh (1998, 2001) finds that even non-poor residents, black and white, of high-poverty neighborhoods suffer some degree of “political isolation,” either manifested in lower rates of voter registration and turnout, or in the infrequency of acts such as signing petitions.

Whether “extreme neighborhood poverty *acts to weaken* the attachment of residents to the participatory process of ‘democratic’ life,” as Cohen and Dawson (1993, 297; emphasis added) conclude, cannot be resolved on the basis of observational studies alone. The validity of an empirical strategy that relies exclusively on naturally occurring correlations between individual political behavior and contextual characteristics is undermined by the fact that individuals self-select into neighborhoods, sorting themselves across communities for reasons that may be related to the determinants of political behavior. The poor residents of low-poverty neighborhoods may be different, in unobservable but politically relevant ways, from the poor residents of high-poverty neighborhoods.<sup>1</sup> Without taking these background traits into account, it is not possible to distinguish true (neighborhood-level) “concentration effects” from the effects of the (individual-level) attributes of people who live in different types of areas.

Although observational studies documenting the distinctive patterns of political behavior in areas with high concentrations of poor people are not well-suited to casual inference, the extant research is nonetheless significant for the theoretical insights it offers about the potential mechanisms of concentration effects and, by extension, the possible political implications of a policy intervention like the MTO. The neighborhood environment can be a source of politically-relevant resources, in the form of institutions and organizations serving community residents. In addition to their official mandates, locally-based institutions—libraries, churches, family support centers, social and cultural clubs, block groups—provide associational spaces where political information may be disseminated, and where residents can be contacted and recruited into political activity (Rosenstone and Hansen 1993). As physical structures, local institutions can provide for safe and accessible polling places, as well as sites for political candidates to visit. And for active members, participation in local groups and organizations facilitates the development of civic skills that may be transferable to the political arena (Verba, Scholzman and Brady 1995). Neighborhoods with high concentrations of poverty, when compared to more affluent communities, may lack the institutional resources that facilitate political activity, leading to lower participation among residents.<sup>2</sup>

Concentration effects may also emerge from the distinctive characteristics of social networks in high-poverty areas, and the importance of social interactions for political activity. Rarely does political activity occur in individual isolation; most political acts are “socially learned and stimulated” (Huckfeldt 1979, 581; Huckfeldt and Sprague 1987, 1995; Lake and Huckfeldt 1998; Giles and Dantico 1982; Fowler 2005; Kenney 1992; Knack 1992; Knack and Kropf 1998; Leighley 1990). Through political discussion, as well as the casual observation of others’ behavior (e.g. putting up campaign yard signs), people send and receive messages about

the importance of participation, and transmit information about participation opportunities. These messages matter, as political activity is highly correlated within discussant networks, even after controlling for socioeconomic status and selection effects (Fowler 2005; Kenney 1992). Given the tendency toward imitation, exposure to social networks characterized by strong participatory norms and frequent political discussion can generate “turnout cascades,” as one member’s decision to participate magnifies the incentives to participate for others in the network (Fowler 2005; see also Lake and Huckfeldt 1998; McClurg 2003). Huckfeldt (1979) and Giles and Dantico (1982), for example, find that “high-status” adults, who are already inclined to participate by virtue of their educational attainment, are even more likely to engage in a variety of (socially-based) political acts when they live amongst and interact with other high-status adults.<sup>3</sup> Notably, this participation advantage does not accrue to the low-status adults in these environments; such individuals, as a consequence of their minority status, are more likely to be alienated from rather than integrated into local social networks. In neighborhoods with high levels of poverty, where few residents have the individual resources, skills or information necessary for effective political action and where, as a consequence, an anti-participation norm may prevail within social networks, the tendency toward imitation may offer socially-connected residents still more reasons to opt out of politics.

In short, some neighborhood environments, by virtue of their institutional capacity or the civic norms that prevail among local social networks, may encourage political participation more than others. We might reasonably expect, therefore, for a policy intervention such as the MTO, because it manipulates participants’ neighborhood environments, to have consequences for individual political behavior. The nature of those consequences, however, is difficult to predict. By reducing participants’ exposure to contextual poverty, and permitting access to communities



with more institutional resources, the intervention could motivate participants toward greater political participation. In addition to the institutional supports for political engagement, the intervention also brings participants into contact with people whose political information, habits and connections to mainstream political institutions (e.g. political parties) may reinforce the importance of participation.

But these hypothesized effects depend, in no small part, on participants' level of social integration into their low-poverty communities. The MTO may be sufficient to bring participants into contact with new people, but it is the resulting social relationships that draw them into public affairs and political activity. Certainly, norms are of little consequence when people are unaware of the expectations of others; in the absence of social integration, one is spared the choice between bearing the costs of political activity or incurring your neighbors' disapproval (Knack 1992). If MTO participants are unable to establish social relationships, or to connect to local institutions and organizations, the potential political benefits of living in low-poverty areas may go unrealized, as Giles and Dantico (1982) observed for "low-status" adults living in "high-status" contexts. Moreover, people whose individual attributes (e.g. welfare dependency) mark them as social or political minorities within the community may experience ambivalence and social anxiety, negative psychological states that can inhibit participation, even where environmental norms encourage political activity (Huckfeldt and Sprague 1987, 1995; Mutz 2002; see also Knoke 1990). The status dissimilarity between low-income MTO participants and the residents of low-poverty neighborhoods, as well as the preference for social networks of like-minded people (McPherson, Smith-Lovin and Cook 2001), may create obstacles to social integration and relationship formation, with unknown implications for political behavior.

Empirical predictions about MTO impacts are complicated further by an experimental design that manipulated not only the exposure to contextual poverty, but also the experience of residential mobility, a significant life event with its own consequences for political behavior. Mobile citizens tend to participate in low numbers, with the likelihood of political activity increasing with years at a given address (Wolfinger and Rosenstone 1980; Squire, Wolfinger and Glass 1987; Highton 2000; Marschall 2001; Knack 1992; Rosenstone and Hansen 1993). In part, mobility depresses participation because it imposes administrative costs, such as the need to update voter registration, to locate a new polling place and so on. As Rosenstone and Hansen (1993) describe it, people who move “must reestablish themselves politically” (157). The longer people live in one place, the more time they have to attend to such political tasks. Mobility also imposes social costs. Mobile citizens report fewer social ties to people in their neighborhood, and social connectedness is a powerful predictor of civic-minded activity (Berry, Portney and Thomson 1991; Knack 1992; Marschall 2001; Marschall and Stolle 2004; Putnam 2000). Social connectedness fosters trust, efficacy and “sense of community” (Marschall and Stolle 2004; Berry et al 1991), and also enables the social pressure, recruitment, and information exchange that can motivate participation (Knack 1992; Rosenstone and Hansen 1993). The administrative and social costs to moving may be enough to disrupt the habit of voting for the residentially mobile.

The implication is that the MTO intervention, because of its effects on residential mobility, may well decrease political participation. MTO participants face the administrative burdens that come with “starting over,” burdens that can be particularly cumbersome for adults with few resources. The move exacts a social cost as well, by disrupting existing social networks and ties. And it takes time to make new friends, even more so in contexts of status dissimilarity

(McPherson, Smith-Lovin and Cook 2001). The central question, then, is whether MTO participants are sufficiently socially embedded to realize any positive externalities from living in low-poverty neighborhoods? Or, do weak attachments to neighbors and community institutions, along with the administrative costs of moving, mean that the benefits go unrealized? In fact, is it possible that circumstances conspire to depress political involvement for the MTO participants who “won” the voucher lottery? In other words, what has been the net effect of the move to opportunity on political participation?

### **The Moving To Opportunity Experiment**

From 1994-1997, HUD recruited 4248 families from five metropolitan areas—Baltimore, Boston, Chicago, Los Angeles, and New York—to participate in the MTO demonstration program. The families, mostly female-headed minority households, were recruited from among the residents of public housing developments located in census tracts with 1990 poverty rates of at least 40 percent. The families were randomly assigned to one of three groups: a control group (n=1310) whose members remained in their initial public housing development; a “Section 8” treatment group (n=1209) whose members received a housing voucher to be used to rent an apartment in the private market, under the standard terms of the Section 8 program; and an “experimental” treatment group (n=1729) whose members also received a voucher, but one that could only be used to lease an apartment in a census tract with a 1990 poverty rate of less than ten percent.<sup>4</sup> That is, recipients of an experimental voucher were required to move to a low-poverty area. To facilitate compliance with the experimental treatment, local non-profits offered mobility counseling to the assigned families, helping them locate units, negotiate rents, and complete the credit review process.

Forty-seven percent of the experimental group families and 61 percent of the Section 8 families used the housing vouchers to move to a new apartment (i.e. “lease up”). While many Section 8 households chose apartments near the center of the city and often in close proximity to their original public housing developments, experimental group families were likely to venture farther from the city center to locate low-poverty communities with affordable rental housing (Orr et al. 2003; Sampson 2008). Figure 1 reports the post-treatment address spells (top panel) and census tract poverty levels (bottom panel) for experimental and Section 8 movers and non-movers, as well as for members of the control group. The address spells measure is a count of the number of different addresses, on average, a family has lived at from the year of random assignment until 2002, when HUD conducted a comprehensive canvass of the MTO families as part of an interim evaluation. The poverty levels depicted in the kernel densities are duration-weighted averages over all neighborhoods lived from random assignment until 2002.<sup>5</sup> The graphs show that 4-7 years after random assignment, adults who received *and used* the experimental and standard Section 8 vouchers were more mobile (i.e. had a larger number of address spells) and lived in areas with significantly lower poverty levels than the control group. The average number of address spells for experimental movers was 3.3; for Section 8 movers, 3.4.<sup>6</sup> For the experimental movers, the average 1990 poverty rate in the new census tract was 7.5 percent; for the Section 8 group movers, 26.9 percent. Meanwhile, the mobility averages and poverty densities for non-movers are similar to that for members of the control group.

[PLACE FIGURE 1 ABOUT HERE]

While the MTO demonstration was not designed to address issues of racial and ethnic concentration directly, the racial characteristics of the new neighborhoods differed modestly from the original locations and between voucher groups. The experimental movers relocated to

tracts that averaged 67 percent non-white; the Section 8 group, 72 percent non-white. The original neighborhoods in which the families had lived averaged 91 percent non-white.

Thus, through random assignment to different voucher groups, the MTO demonstration introduced an exogenous source of variation in neighborhood conditions and mobility. Specifically, assignment to a voucher group contributed to two dynamics with potential implications for political behavior: (1) members of the experimental and standard Section 8 group were able to move; and (2) members of the experimental group, in particular, were able to move to much better neighborhoods.

### **Sample and Data Sources**

To assess the political impact of the MTO experiment, I focus on voter registration and vote history data collected in 2006, 9-12 years after random assignment, for adult participants.<sup>7</sup> The sample is restricted to one adult per family; in virtually all cases, the sample adult is the female head-of-household at the time of randomization.<sup>8</sup> At the launch of the MTO demonstration, prospective families completed in-person surveys that provide baseline information about the sample adults. Table 1 presents selected baseline characteristics, showing means for the treatment and control groups. The statistics reveal that the sample adults are primarily non-white (almost two-thirds are black); female; low education; unemployed and heavily dependent on public assistance. Importantly, the data reported also confirm the success of the randomization, as there are no statistically significant differences in the demographic composition of each group. Differences that emerge *post*-randomization are the consequence of the voucher intervention.<sup>9</sup>

[PLACE TABLE 1 ABOUT HERE]

Registration and vote data for sample adults come from the official registered voter lists maintained by each of the six counties from which the MTO families were originally recruited: Baltimore City; Suffolk County, MA; Cook County, IL; Los Angeles County; New York County and Bronx County, NY. While the counties differ in the amount of data they maintain on registered voters, in all cases, the available data indicate which county residents were registered to vote as of February 2006 and in which (if any) recent elections each registered voter had cast a ballot. The vote history data available in all six counties include participation in the elections held in November 2002 and November 2004. This analysis restricts its attention to registration and turnout in those two elections.

In collaboration with Abt Associates and HUD, the county voter lists were matched to the database of MTO participants based on name, gender, and date of birth.<sup>10</sup> This process identified 57 percent (n=2428) of the MTO adult sample among the residents registered to vote in the six baseline counties. (Nationally, 61 percent of adults with family incomes of less than \$20,000 were registered to vote in 2004 (Holder 2006).) Another 26 percent (n=1113) of the sample adults are not registered to vote, although they still live in one of the six baseline counties. Sixteen percent of the participants (n=707) no longer reside in one of the six baseline counties. (This figure includes 18 percent of the experimental group, 16 percent of the Section 8 group, and 15 percent of the controls.) For these out-migrants, voter registration and turnout are both unobserved.<sup>11</sup> While it would be possible to measure differences in registration and turnout between treatment and control for just the subgroup of MTO adults who remained in the baseline counties, because this subgroup is endogenous to the treatment (i.e. participants who received a voucher were more likely to move at all, including moving out of the county), *such differences would not constitute experimental impacts.*

To avoid the creation of endogenous subgroups, two sets of registration and vote outcome measures are constructed. For the first set of outcome measures, the dichotomous indicators for registration status, *Registered<sub>b</sub>*, and turnout, *Voted<sub>b</sub> 2004* and *Voted<sub>b</sub> 2002*, are defined such that zero identifies any MTO sample adult not registered in a baseline county, including those who migrated out of the county. In effect, the outcome of interest is no longer voter registration (turnout) in general, but voter registration (turnout) *in the baseline county*; the result is a conservative estimate of political participation. To construct the second set of outcome measures, I apply multiple imputation, using Honaker, King, and Blackwell's (2007) EMB algorithm (and accompanying software, *Amelia II*), to impute values for the unobserved data, i.e. registration status and voter turnout among the 16 percent of the sample who are out-migrants. Multiple imputation, which assumes that information in the observed data provides indirect evidence about the likely values of the unobserved data (and that, after controlling for the observables, missingness is independent of the unobserved data), is known to outperform listwise deletion (i.e. removing all out-migrants from the analysis) by correcting for the inefficiency and bias that result from the latter approach (Schafer and Olson 1998). Whether the assumptions that underlie multiple imputation are reasonable for the data at hand depends on how well missingness can be predicted; for the MTO voter data, missingness can be predicted by factors associated with out-migration (e.g. owning a car at baseline). With this method, I impute five values for each missing cell in the original data matrix, thus generating five "complete" data sets for analysis.<sup>12</sup> In these data, the indicators for registration status (*Registered<sub>a</sub>*) and turnout (*Voted<sub>a</sub> 2004*, *Voted<sub>a</sub> 2002*) are defined such that zero identifies MTO sample adults who are not registered and/or did not vote *in any county*. The two sets of outcome measures, both of which enable the inclusion of

partially observed observations, are evaluated in parallel; as will be seen below, however narrowly or broadly the outcome is defined, the estimated treatment impacts are similar.

### **Models and Empirical Expectations**

To identify the effects of the MTO demonstration on political participation, I compare the registration and turnout behavior of adults whose families were *offered* housing vouchers (experimental and Section 8 groups) to those whose families were *not offered* vouchers (control group). There are two estimates of interest that follow from MTO's experimental design and are reported in the tables: the intent-to-treat (ITT) and the treatment-on-treated (TOT) effects. The ITT effect, estimated from the difference in mean outcomes for the treatment and control groups as a whole, is the effect of being offered the voucher, regardless of subsequent compliance (i.e. whether or not the family offered the voucher actually used it to lease up). The TOT effect is the effect of the voucher on the compliers—the MTO sample adults who actually leased up using the program voucher. Whereas the offer of a voucher was extended to every member of the experimental and Section 8 treatment groups, not every group member used their assigned voucher. Thus the TOT effects, unlike the ITT effects, are non-experimental, in the sense that they are not directly observed for whole randomly assigned groups, but only for the subset of compliers within the groups. While the TOT is arguably of greater substantive interest, the ITT, though attenuated by non-compliance among some members of the treatment groups, is the only direct measure of the effect of the MTO experiment; the experiment was to *offer* rental vouchers, not to mandate their use. Examining the ITT and TOT effects together is essential to a thorough assessment of the MTO intervention.



In a regression model, we estimate the ITT impact on an outcome with the coefficient on the indicator for treatment assignment:

$$(1) Y_i = \alpha + Z_i\pi_{ITT} + \varepsilon_i$$

where  $Z_i$  indicates assignment status for an individual (indexed by  $i$ ) and  $\pi_{ITT}$  captures the ITT effect. We infer the TOT impact from the ITT impact  $\pi_{ITT}$  based on the weak assumption that the effect of the treatment occurs entirely through moving using a program voucher (i.e. individuals in the treatment group were not affected if they were offered a voucher but then did not use it). Under this assumption, we know that the treatment impact for the non-compliers (i.e. individuals who did not use the voucher) is zero, making the ITT estimate  $\pi_{ITT}$  a weighted average of the impact on compliers and the zero effect on non-compliers (Bloom 1984); the weights are the portions of the sample that are compliers and non-compliers. As a result, the TOT impact can be estimated by dividing  $\pi_{ITT}$  by the program compliance rate for the treatment group (for similar approaches to estimating MTO impacts, see Katz, Kling and Liebman 2001; Ludwig et al. 2008; Orr et al. 2003).<sup>13</sup>

If the dominant effect of the MTO intervention is to impose on participants the administrative and social costs of moving, then we should observe negative ITT ( $\pi_{ITT}$ ) impacts, on registration and on voting, for the two groups offered housing vouchers—adults assigned to the experimental or to the standard Section 8 groups. Since members of the experimental group typically had to move greater distances from their baseline neighborhoods than members of the Section 8 group in order to comply with the treatment (i.e. locate an apartment in an eligible low-poverty neighborhood), the negative ITT impacts may be larger for recipients of the experimental vouchers. If the dominant effect of the MTO intervention is to provide participants with access to the institutional resources, role models and recruiting agents available in less poor

neighborhoods, then we should observe positive ITT impacts on registration and voting. The positive ITT impacts should be largest for members of the experimental group, who were required to move to more affluent areas than members of the Section 8 group in order to comply with the treatment.

The models estimating treatment impacts include as covariates individual factors—race/ethnicity, gender, age—measured prior to random assignment, as well as fixed effects for each of the MTO sites (with New York as the omitted category). The covariates are included in order to reduce residual variation, thereby increasing the precision of the impact estimates. Finally, all statistical estimates are weighted to adjust for changes in the random assignment ratios during the demonstration period, as well as for differences across sites in the random assignment ratios (Orr et al. 2003, Appendix B).

### **Registration, Turnout, and the Move to Opportunity**

The net effect of the changes induced by the MTO demonstration has been to depress political participation among voucher recipients in general, and recipients of the experimental vouchers in particular. Table 2 reports the results of logit models estimating treatment impacts on voter registration and turnout, for the experimental and Section 8 groups combined.<sup>14</sup> To begin, consider voter registration. As the first row of the table makes clear, voter registration among MTO adult participants is reasonably high. Nearly 60 percent of the control group was registered in a baseline county as of 2006; this figure increases to two-thirds once we take into account out-migrants registered in other counties. Adults who were *offered* a housing voucher, with or without locational restrictions, are less likely than the adults randomly assigned to the control group to be registered to vote in a baseline county. The model estimates a negative intent-to-treat

effect of more than three percentage points for the pooled treatment groups. But these effects are largely due to the number of out-migrants among the treatment groups. Allowing for the possibility that out-migrants may be registered in other counties, we observe no statistically significant difference in voter registration relative to the control group. If there are negative treatment impacts on voter registration, they are too small to be detected reliably.

[PLACE TABLE 2 ABOUT HERE]

Where the negative impact of the MTO demonstration is most evident is on voter turnout, also reported in Table 2. The adults randomly assigned to either voucher group (experimental or standard Section 8) were *less* likely than members of the control group to vote in the 2004 Presidential election. Whether defined narrowly as voter turnout in the baseline counties, or defined so as to allow for out-migrants living and voting outside of the baseline counties, the models estimate a negative ITT effect of more than three percentage points for the pooled treatment groups. Among the treatment compliers, the effect of receiving *and using* a housing voucher was to lower 2004 voter turnout by 6.8 percentage points, relative to the control group. Participation in the 2002 midterm election was uniformly low among all MTO adult participants, regardless of random assignment to treatment or control.

The treatment impacts on voter turnout are especially large for adults assigned to the experimental group, whose vouchers could only be used in low-poverty areas. Table 3 reports the ITT and TOT effects on voting in the 2004 election for each voucher group. The model estimates a statistically significant negative intent-to-treat effect of 3.9 percentage points and a negative treatment-on-treated effect of eight percentage points for the experimental group with restricted vouchers. That is, for poor adults who used rental vouchers to move out of public housing and into low-poverty neighborhoods (i.e. the compliers), the effect was to lower voter

turnout by 19 percent (.082/.428). Only the treatment impacts for the experimental group reached statistical significance, a result that may in part reflect the noise introduced by splitting the sample into two treatment groups. The data suggest that the MTO intervention may have had some effect on voter turnout among the adults who received the unrestricted Section 8 vouchers as well. However, the treatment impact for this group, equivalent to a (statistically insignificant) 12 percent drop in voter turnout among compliers, is smaller than that observed for the experimental voucher group. In sum, the group whose residential circumstances improved most dramatically became the group least likely to be active in electoral politics.

[PLACE TABLE 3 ABOUT HERE]

### **The Costs of Mobility: Interpreting Low Turnout among Experimental Voucher Recipients**

The pattern of negative treatment impacts on voter turnout suggests that the MTO intervention succeeded primarily in imposing new costs on participants. Specifically, with respect to voter participation, the administrative and social costs of residential mobility may have offset any benefits to be realized from exposure to low-poverty contexts. These costs (and, thus, the negative treatment impacts) were particularly large for members of the experimental group, whose residential moves to areas well away from their baseline communities were likely the most disruptive. The result is that the MTO demonstration has had the unintended consequence of lowering voter turnout among the “winners” of the housing voucher lottery.

The design of the MTO demonstration—namely, the use of random assignment via lottery—provides critical leverage for causal inference. We can now claim that the receipt of an experimental voucher, and its use to rent an apartment in a low-poverty neighborhood, *caused* a decline in subsequent voter turnout. But because the intervention simultaneously manipulated

multiple correlates of participation—levels of contextual poverty, administrative and social costs of mobility—these data can provide only clues to the substantive interpretation of these causal impacts, in particular to the relative importance of administrative burdens versus social disruptions as drivers of the negative mobility effects. Among the administrative costs associated with residential mobility and responsible for lower voter turnout among frequent movers is the need to re-register with each change of address (Highton 2000; Squire, Wolfinger and Glass 1987). While residentially mobile citizens have benefited disproportionately from electoral reforms such as the 1993 National Voter Registration Act (NVRA, aka “Motor Voter”), which eased the registration process by permitting citizens to register to vote at public offices that they visit for other purposes, mobility remains a statistically significant predictor of registration status (Highton and Wolfinger 1998; Knack and White 2000; Wolfinger and Hoffman 2001).<sup>15</sup> In the MTO demonstration, not only did compliance with the voucher treatment—experimental or standard Section 8—require at least one residential move and, therefore, the need to re-register at least once, but many of the voucher compliers undertook additional moves. (Recall Figure 1 reporting the number of address spells, on average, for MTO participants between the year of random assignment and the 2002 interim evaluation. Compliers averaged more than three address spells post-assignment.) For a variety of reasons, from landlord conflicts to the need for larger apartments, nearly two-thirds of the experimental compliers—and a similar proportion of the Section 8 compliers—made one or more additional moves *after* their initial lease up, a level of ongoing mobility that HUD had not anticipated when designing the demonstration (Orr et al. 2003). Many of these repeat movers changed neighborhoods as well as apartments (Briggs and Turner 2006; Kingsley and Pettit 2008; Comey, Briggs and Weismann 2008). Yet, we observe no treatment impacts on voter registration, for either members of the

experimental or the standard Section 8 groups. Voucher recipients are no less likely to be registered to vote than are members of the control group, despite the greater effort required. The implication is that the administrative burden of having to re-register to vote is likely not the cause of the negative treatment impacts.

The administrative costs of mobility also include the task of locating a new polling place. Brady and McNulty (2004) estimate that a polling-place change is sufficient to lower voter turnout by nearly 2 percentage points, with most of this effect due to increased “informational and search costs” (see also McNulty, Dowling and Ariotti 2009). For members of the experimental group, who ventured farther from the familiar territory of their baseline neighborhoods than did the members of the section 8 group, this logistical burden may be particularly difficult. Moreover, the ongoing mobility of many voucher recipients—not simply the initial move out of public housing, but the pattern of repeated moves to new apartments and new neighborhoods—would lead to frequent changes in polling locations. These disruptions may be contributing to the 19 percent drop in voter turnout among the experimental compliers.

However, if the costs associated with finding a new polling place were the sole factor in explaining the negative treatment impacts, then we might reasonably expect the adults who experienced the most residential instability post-assignment—and, thus, the most frequent polling-place changes—to exhibit the lowest rates of voter turnout. Figure 2 presents a lowess curve of voter turnout by residential stability (measured by the number of address spells since randomization) for experimental compliers. The graph reveals that, contrary to expectation, even the adults with relatively more stable residential circumstances, e.g. the experimental compliers who moved only once or twice and then stayed put, voted at rates as low as their more peripatetic counterparts. This pattern is notable when one considers that Wolfinger and Rosentone (1980)

found negative mobility effects to diminish sharply after only two years at a given residence, as the administrative costs of mobility recede (see also Highton 2000, Highton and Wolfinger 1998). But as late as November 2004, 7-10 years after the MTO experiment, and despite being registered to vote, low turnout rates persist among even the most residentially stable experimental compliers. The persistence of low turnout suggests that the logistics of locating a new polling place, and the administrative costs of moving more generally, cannot account fully for the negative treatment impacts of the MTO intervention.<sup>16</sup>

[PLACE FIGURE 2 ABOUT HERE]

Mobility not only imposes administrative costs, it is also socially disruptive. The social disruptions associated with the MTO demonstration may explain the low voter turnout among the experimental compliers, including the more residentially stable members of this group. Because it takes a lot more time to make new friends than it does to find a new polling place, the social costs of mobility accrue for years. The costs may be particularly high for recipients of the experimental vouchers, whose socio-economic attributes (and, to a lesser extent, racial characteristics) do not align with those that are dominant in their low-poverty neighborhoods, and who moved to areas less physically proximate to their baseline communities. Status dissimilarity makes relationship formation more difficult (McPherson, Smith-Lovin and Cook 2001), and distance complicates efforts to maintain existing social relationships.

A 2002 interim evaluation survey of the MTO families (Orr et al. 2003), as well as evidence emerging from in-depth interviews and ethnographic fieldwork conducted in 2004 and 2005 as part of the Three-City Study of MTO (Comey, Briggs and Weismann 2008; Briggs and Tanner 2006; Popkin 2008), hint at the social disruptions experienced by members of the experimental group. Even 6-10 years after their initial relocation, Briggs and Tanner (2006) find movers who

remain “quite isolated” from social ties and support systems, and who have failed to “convert new locations into social capital” (Popkin 2008). As they describe it, “new neighborhoods, at least for a significant period of time, are residential locations rather than important social worlds” (Briggs and Tanner 2006, 50), with few MTO movers participating in community institutions.

In the 2002 survey, respondents were asked a short battery of questions about their social networks, including the frequency with which they visit with friends and attend church. (Of the 4248 sample adults, 3526 were interviewed by HUD for the interim survey.) Members of the experimental group were significantly less likely than adults assigned to the control group, or to the Section 8 voucher group, to report having visited with friends “during the past thirty days” (Figure 3, top panel). In addition, these adults were less likely to report attending church at least once a month in the previous year (Figure 3, bottom panel). While the impact on church attendance is significant at only the  $p < .10$  level, a Wald test confirms that the predicted probabilities for the experimental and standard Section 8 groups are significantly different from one another at  $p < .02$ .

[PLACE FIGURE 3 ABOUT HERE]

The available survey evidence is limited, but the patterns are consistent with the ethnographic accounts of the social disruptions associated with MTO-related mobility. Long after relocation, adults whose families moved to low-poverty areas continued to pay the social costs of mobility, measured in less time with friends and at church. These lost social ties have been cited, particularly by critics of MTO and other housing mobility programs (Venkatesh and Celimli 2004; Venkatesh et al. 2004; Curley 2009; Clampet-Lundquist 2004; Garshick Kleit 2010), as undermining the ability of the poor to cope with material hardships, adjust to their new neighborhoods, and take advantage of their new opportunities. MTO policymakers, while still



optimistic about the promise of the program, also suspect that the social disruptions may be to blame for the null effects of the intervention on indicators of social and economic well-being such as employment and earnings (Briggs and Turner 2006; Ludwig et al. 2008). The disruptions may also be undercutting political activity. The effect of mobility on church attendance is particularly noteworthy in this regard, as it suggests less access to the networks of recruitment and mobilization embedded within such institutions (Verba, Schlozman and Brady 1995; Harris 1999).

## **Discussion**

The spatial concentration of poverty is a phenomenon associated with a wide array of negative outcomes, from chronic joblessness to delinquency and depression. Poor families confined to deeply poor neighborhoods suffer structural disadvantages that exacerbate the hardships of individual poverty and limit the opportunities for social mobility. The *Moving to Opportunity Demonstration Program* experimented with a new approach to combating poverty: the random allocation of Section 8 rental vouchers to families living in public housing, with some vouchers restricted to use only in low-poverty neighborhoods. The ambition of federal policymakers was to improve the social and economic well-being of the recruited families, using the tool of residential mobility. And, in fact, the effects of the MTO demonstration on dimensions such as mental health and educational achievement have been amply documented (Orr et al. 2003; Kling, Liebman, Katz 2007; Kling, Ludwig, Katz 2005; Leventhal and Brooks-Gunn 2003; Ludwig, Duncan, Hirschfield 2001; Katz, Kling, Liebman 2001). The MTO intervention, however, has had unintended consequences for the political lives of the participants as well. Until now, those effects were unknown.

Analysis of the vote history of MTO adult participants, 7-10 years after the voucher lottery, reveals that adults whose families received Section 8 vouchers were less likely to vote in November 2004, although no less likely to be registered. The negative effect of the MTO intervention was most visible for the participants whose vouchers could only be used in low-poverty areas. For the compliers among this group, the experiment reduced voter turnout by eight percentage points, or 19 percent, when compared to voter turnout among the lottery “losers,” who did not receive rental vouchers.

The pattern of negative treatment impacts suggests that, with respect to vote participation, the mobility enabled by the MTO intervention imposed costs substantial enough to offset any benefits to be realized from improvements in participants’ residential circumstances. The nature of these costs cannot be determined with certainty from the data available. As Sampson, Morenoff and Gannon-Rowley (2002) explain, “the random assignment of housing vouchers does not address causal processes of *why*” (467; italics in original). But there is reason to believe the negative impact is due less to the short-term administrative burdens that accompany mobility, such as the need to locate a new polling place, than to the longer-term social disruptions. Consistent with the social costs of mobility, the MTO participants who received low-poverty vouchers report less frequent church attendance and less social interaction with friends and family. Moreover, voter participation is depressed among low-poverty voucher recipients regardless of how long they have lived in their new neighborhoods. As low-income newcomers to low-poverty areas, settling in neighborhoods well away from their baseline communities, adults who used the experimental vouchers may have experienced social disruptions profound and persistent enough to lower voter turnout, even among the more residentially stable members of the group.

The MTO demonstration was designed to combat poverty, not to test academic theories of political participation. Nonetheless, the intervention has implications for such theories. The absence of participatory gains among adults offered experimental vouchers should remind scholars to be cautious when making strong claims about the existence, direction or magnitude of neighborhood effects on political behavior, particularly among the poor (Cohen and Dawson 1993; Alex-Assensoh 1998). Where scholars have previously documented, on the basis of observational data alone, concentrated poverty to be negatively associated with participation, such results may be the spurious consequence of endogenous neighborhood selection. Poor people who live in high-poverty neighborhoods may be different in politically relevant ways from poor people who seek out and settle in low-poverty neighborhoods. Past research potentially has confused the effects of neighborhoods with the effects of the (unobserved) characteristics of individuals who live in different types of neighborhoods. What MTO suggests is that neighborhood effects on participation may be non-existent—or, more likely, conditional on a level of social integration into community life that is not easily achieved by poor residents of low-poverty neighborhoods.<sup>17</sup> By comparison, the evidence is more favorable to theoretical accounts of residential mobility, especially those that emphasize the social disruptions that result from moving. When considering the political behavior of the poor, scholars should bear in mind that the social costs of mobility may exceed the benefits of neighborhoods.

The finding that the MTO demonstration has lowered voter turnout for the adults who received, and used, housing vouchers restricted to low-poverty neighborhoods presents a dilemma for policy practice. On the one hand, a federal program that leads to less political activity among the poor may seem of little consequence, if these effects are the unintended result of efforts to combat the underlying causes of poverty (Berry, Portney and Thomson 1991). It is

no small victory that MTO has improved neighborhood conditions, adult mental health, and educational outcomes for girls (Kling, Liebman and Katz 2007). On the other hand, there are few groups with more at stake in public policy than the urban poor. In that respect, political activism, including voting, must be viewed as part of the solution to the problems of the poor. As Callahan (1998) observed, “in a country where tens of millions of low-income people don’t vote, politicians face few penalties when they cut poverty programs and redistribute income upward” (see also Plutzer and Wiefek 2006). The tension that is evident in the MTO demonstration—between the goal of improved social and economic well-being and the value of political engagement—is a problem to be examined and solved, and not simply a nuisance to lament but dismiss.

A policy solution will first require a fuller grasp of the nature of the problem. Future study of the MTO demonstration, and similar mobility interventions, should document the range of political effects. Are the negative treatment impacts limited to voting turnout, or do they extend to other modes of political participation? Is it only the adults who are voting less, or does the negative treatment impact span generations? How have the children of MTO, particularly those who are now of voting age, fared? And when, if ever, do the negative impacts attenuate? Finally, we must bring more evidence to bear regarding the causal mechanisms. To be sure, the preferred policy remedy would still enable greater residential mobility for the poor. But if the administrative and/or social costs of moving are particularly burdensome for the poor—and the optimistic assumptions about the ease with which low-income movers establish social ties in low-income neighborhoods have proven to be unrealistic—then future mobility interventions should include more comprehensive post-move support, focused on helping families to acclimate to their new communities and to develop social and institutional connections (Briggs and Tanner

2006; Popkin 2007, 2008). Without that, the cumulative effect of these increasingly popular interventions may be to further weaken poor Americans' already fragile ties to the political system.

## Notes

<sup>1</sup> For example, poor people who seek out and settle in low-poverty neighborhoods may possess a degree of motivation or cultural capital and skills that (1) sets them apart from the equally poor residents of high-poverty neighborhoods *and* (2) promotes political engagement.

<sup>2</sup> A notable exception would be the churches and other religious institutions that are present even in areas of concentrated poverty (Alex-Assensoh 1998; Alex-Assensoh and Assensoh 2001; McRoberts 2004). Religious institutions can be instrumental in facilitating political mobilization, as Harris (1999) and others have demonstrated.

<sup>3</sup> Each of these studies distinguishes between socially-based (e.g. campaigning, attending rally) and individually-based (e.g. voting, contacting) participation, and conditions the effect of neighborhood context on individual socio-economic status. Neither study finds evidence of a relationship between neighborhood composition and individually-based participation, regardless of individual socio-economic status. However, Huckfeldt (1979) finds that living in high-status neighborhoods mobilizes high-status individuals to greater socially-based political participation, while demobilizing low-status adults. In contrast, Giles and Dantico (1982), while finding similar mobilizing effects among high-status adults, find neighborhood composition to have no effect on socially-based political participation among those of low socio-economic status.

<sup>4</sup> The locational restrictions on the experimental group applied only for the first year following random assignment. Treatment group families who failed to lease up were able to remain in public housing.

<sup>5</sup> The available census tract data are categorical: <10%; 10-19%; 20-29%; 30-39%; 40% or more. The data provided to the author by HUD do not include the precise poverty rate in each census tract lived from random assignment through December 31, 2001.

<sup>6</sup> The minimum number of address spells post-randomization for an experimental or Section 8 mover is two: (1) the spell at the baseline (i.e. pre-move) address, plus (2) the spell at the address to which the family moves using the program voucher.

<sup>7</sup> Voter registration data for the MTO participants were not collected at baseline; as such, it is not possible to calculate a pre-post estimate of the treatment effect.

<sup>8</sup> There are 29 sample adults without baseline data on race/ethnicity. They were excluded from analysis.

<sup>9</sup> Appendix Table A1 reports the post-randomization demographic characteristics of the experimental and Section 8 movers and non-movers.

<sup>10</sup> The match to the county voter files was conducted by Abt Associates, a HUD contractor, using confidential MTO data (e.g. name, date of birth) to which the author was not permitted access. The matched data made available to the author do not include any identifying information.

<sup>11</sup> To be clear, registration and turnout are observed for 84 percent of the sample, the adults still living in one of the six baseline counties. The remaining 16 percent of the sample, for which registration and turnout are unobserved, are scattered across more than 90 different counties, making it prohibitively expensive to assemble the additional county voter lists needed to determine the registration and turnout status of these participants. Additionally, the author does not have access to the confidential MTO data (name, gender, date of birth) required to match the voter files to the MTO database.

<sup>12</sup> Across the five imputed datasets, the observed values are the same, but the missing values are filled in with different imputations. Each complete data set is analyzed using standard complete-data statistical methods. The results presented in the tables are the combined results across imputed datasets, calculated using formulas that incorporate missing-data uncertainty (see King et al. 2001, 53 for formulas).

<sup>13</sup> The standard errors for the TOT estimates are similarly adjusted. Thus, while TOT impact estimates are substantially larger than ITT estimates (because they are not attenuated by zero effects of the intervention on the non-compliers), they are statistically significant only if the ITT estimate is significant.

<sup>14</sup> For ease of interpretation, Tables 2 and 3 report first differences (and 95% confidence intervals), calculated using Clarify (Tomz, Wittenberg and King 2001), with baseline covariates held constant at mean values. The logit coefficient tables are available upon request.

<sup>15</sup> In the post-Motor Voter era, mobile citizens remain less likely to be registered, but they are more likely than the residentially stable to take advantage of the NVRA provisions—namely, completing their registration at a drivers' license registration center or a public assistance agency (Wolfinger and Hoffman 2001). Wolfinger and Hoffman (2001) report that, in the year after NVRA took effect, nearly 40 percent of recently-moved new registrants had registered at motor vehicle or public assistance offices, compared with only a quarter of more settled new registrants.

<sup>16</sup> It is also significant that the subset of adults who ultimately complied with the experimental treatment had been more likely, at baseline, to have a high-school diploma (39.7%), to own a car (19.4%), and to have a smaller family size (3.5). (See Appendix Table A1.) In other words, the

people who moved were the people relatively more able to shoulder the administrative costs of moving. Yet, despite these advantages, we observe negative treatment impacts. This is further evidence that administrative costs alone cannot account for the lower turnout.

<sup>17</sup> An unanswered question is whether the lack of social integration is a temporary consequence of the MTO participants' newcomer status, or a permanent condition. If it proves to be a permanent condition, and voter turnout remains depressed, then this would suggest that it is not simply the social costs of mobility that lowers participation among the experimental compliers, but also perhaps the social experience of being poor in a well-off community (see Huckfeldt 1979; Giles and Dantico 1982). These questions can only be answered with continued tracking of the MTO participants.

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**Table 1: Select Demographic Characteristics of MTO Participant Families, at Baseline**

	Control Group	Experimental Group	Section 8 Group
Site Sample Size			
Baltimore	197	252	187
Boston	326	366	267
Chicago	232	460	202
Los Angeles	260	250	168
New York	295	401	385
Head of Household			
Female	97.9%	98.6%	97.8%
Hispanic	30.0%	30.2%	30.9%
Black Non-Hispanic	63.6%	62.8%	62.0%
White Non-Hispanic	2.5%	3.1%	2.6%
Never Married	63.6%	62.1%	61.9%
Average Age	33.3	33.5	33.7
High School Graduate	35.6%	39.7%	37.3%
GED	20.3%	17.2%	18.9%
Working for pay	25%	27.2%	25%
Average Family Size	3.7	3.7	3.8
Percent of Households with AFDC/TANF Income	74.6%	74.5%	75.3%
Percent of Households with a Car	15.2%	17.4%	16.8%
Percent of Households Previously Applied to Section 8	43.8%	42.0%	39.0%

**Table 2: Treatment Effects on Registration and Turnout, in Baseline County and Any County, for Pooled Experimental and Section 8 Treatment Group**

	Participation in Baseline County <i>(Out-Migrants=0)</i>	Participation in Any County <i>(Out-Migrants Imputed)</i>
<i>Outcome Measure</i>		
Registered		
Control Mean	0.599	0.673
Pooled ITT	-.034 [-.066, -.002]*	-.028 [-.060, .005]
Pooled TOT	-.064 [-.124, -.004]*	-.053 [-.113, .009]
Voted 2004		
Control Mean	0.383	0.428
Pooled ITT	-.038 [-.073, -.003]*	-.036 [-.071, -.001]*
Pooled TOT	-.071 [-.137, -.006]*	-.068 [-.133, -.002]*
Voted 2002		
Control Mean	0.199	0.233
Pooled ITT	-.031 [-.057, -.006]*	-.027 [-.059, .004]
Pooled TOT	-.058 [-.107, -.011]*	-.051 [-.111, .008]
<b>Total N</b>		<b>4219</b>
<b>Pooled Lease Rate</b>		<b>0.533</b>

Note: Results from analysis with Section 8 and experimental groups pooled into single treatment group. Control means and treatment effects are regression-adjusted, using logit models. Treatment effects (and 95% confidence intervals) on probability of registration and turnout are estimated with site indicators and baseline covariates (race, age, gender) held constant at mean values. Cell entries in column two ("Participation in Any County") are the combined results across five multiple imputation datasets (King et al. 2001, 53). \*p<.05

**Table 3: Treatment Effects on Registration and Turnout, in Baseline County and Any County, for Each Treatment Group**

	Participation in Baseline County <i>(Out-Migrants=0)</i>	Participation in Any County <i>(Out-Migrants Imputed)</i>
<i>Outcome Measure</i>		
Registered		
Control Mean	0.599	0.673
Experimental Group ITT	-.032 [-.073, .009]	-.020 [-.060, .017]
Experimental Group TOT	-.068 [-.154, .019]	-.042 [-.127, .036]
Section 8 Group ITT	-.037 [-.081, .007]	-.039 [-.082, .003]
Section 8 Group TOT	-.060 [-.131, .011]	-.063 [-.133, .005]
Voted 2004		
Control Mean	0.383	0.428
Experimental Group ITT	-.041 [-.079, -.003]*	-.039 [-.078, -.001]*
Experimental Group TOT	-.086 [-.167, -.006]*	-.082 [-.165, -.002]*
Section 8 Group ITT	-.032 [-.072, .007]	-.033 [-.078, .001]
Section 8 Group TOT	-.052 [-.117, .011]	-.053 [-.126, .002]
Voted 2002		
Control Mean	0.199	0.233
Experimental Group ITT	-.037 [-.066, -.008]*	-.031 [-.063, .004]
Experimental Group TOT	-.078 [-.139, -.017]*	-.065 [-.133, .008]
Section 8 Group ITT	-.022 [-.052, .009]	-.021 [-.056, .014]
Section 8 Group TOT	-.036 [-.084, .015]	-.034 [-.091, .023]
<b>Total N</b>		<b>4219</b>
<b>Experimental Lease Rate</b>		<b>0.474</b>
<b>Section 8 Lease Rate</b>		<b>0.617</b>

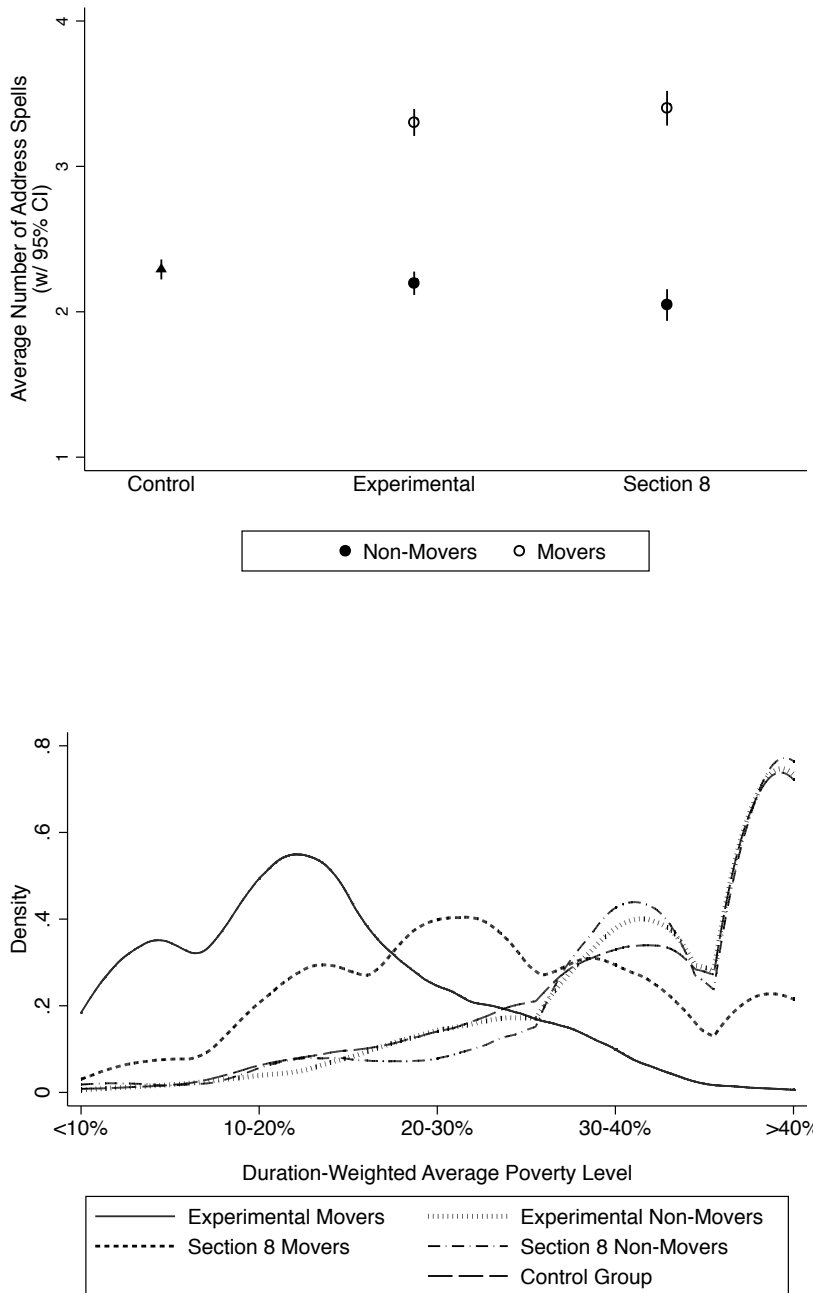
Note: Control means and treatment effects are regression-adjusted, using logit models. Treatment effects (and 95% confidence intervals) on probability of registration and turnout are estimated with site indicators and baseline covariates (race, age, gender) held constant at mean values. Cell entries in column two ("Participation in Any County") are the combined results across five multiple imputation datasets (King et al. 2001, 53). \*p<.05

**Appendix Table A1: Select Demographic Characteristics (at Baseline) of MTO Voucher Movers and Non-Movers**

	Control Group	Experimental Group		Section 8 Group	
		Non-Movers	Voucher Movers	Non-Movers	Voucher Movers
Head of Household					
Hispanic	30.0%	41.10%	28.70%	48.2%	27.0%
Black Non-Hispanic	63.6%	50.4%	62.9%	40.4%	67.2%
White Non-Hispanic	2.5%	3.4%	4.0%	5.1%	1.9%
Age	33.3	36.8	31.8	38.2	32.1
HS Graduate	35.6%	35.5%	39.7%	30.2%	39.3%
GED	20.3%	18.1%	19.4%	18.0%	18.9%
Percent Working for Pay	25%	27%	27%	23%	26%
Average Family Size	3.7	3.8	3.5	3.7	3.7
Percent Households with AFDC/TANF Income	74.6%	67.2%	77.5%	71.1%	78.6%
Percent Households with a Car	15.2%	13.7%	19.4%	13.1%	18.4%
Percent Households Previously Applied to Section 8	43.8%	38.6%	44.6%	33.9%	40.0%

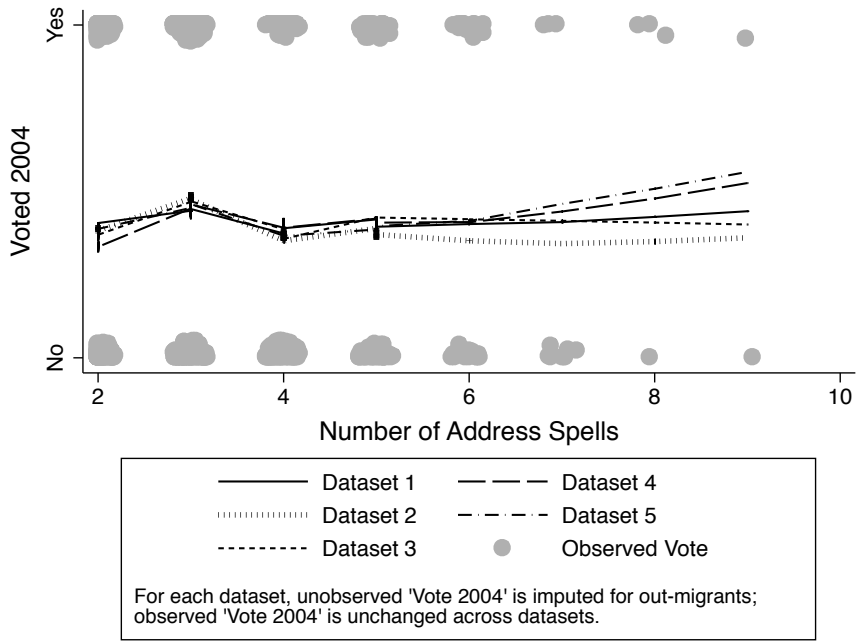
Note: Columns 2-4 report the baseline characteristics of MTO voucher recipients who ultimately did or did not move using the assigned voucher.

**Figure 1: Post-Treatment Residential Mobility and Neighborhood Poverty, By Group**

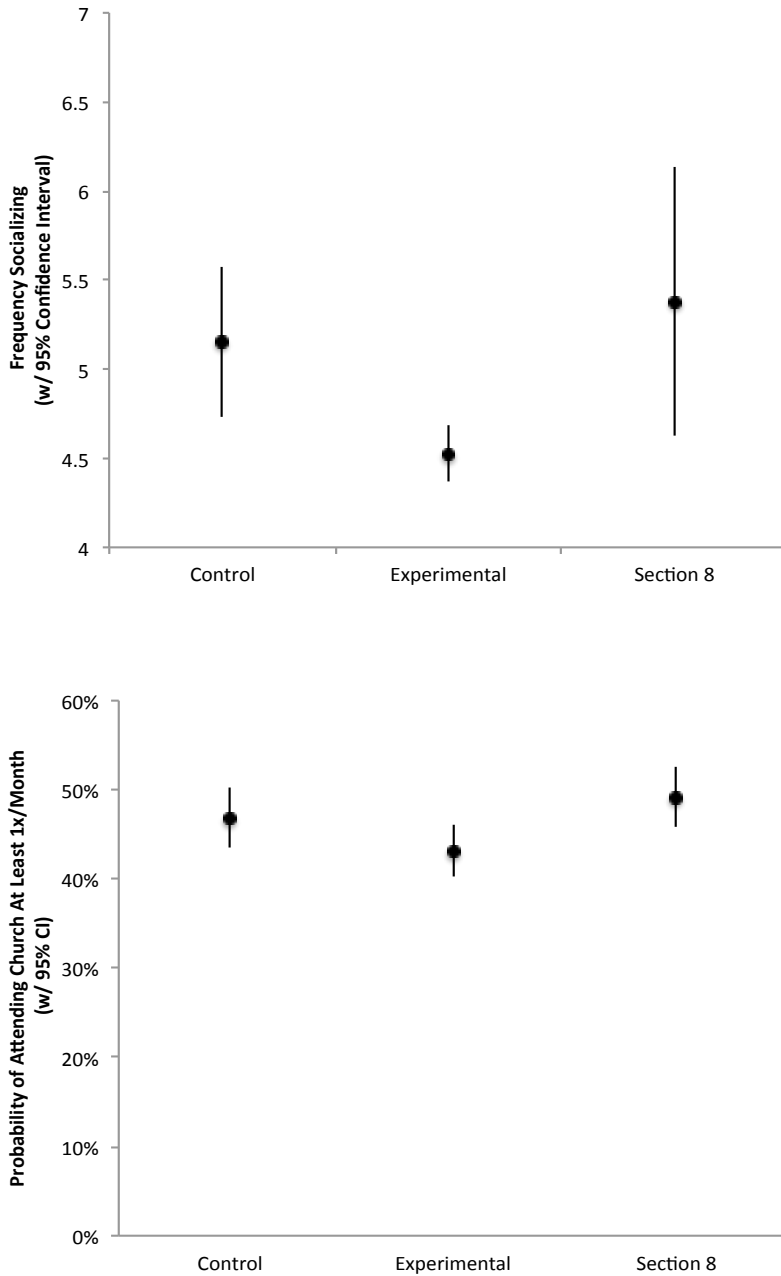


Note: Data from MTO Interim Impact Evaluation, 2002 (Orr et al. 2003). An address spell is the period of time a family resides at one address. Each change of residence initiates a new address spell. The top panel plots the average number of address spells from the year of random assignment until 12/31/2001. Average poverty level (bottom panel) is a duration-weighted average of all census tract locations lived from randomization until 12/31/2001.

**Figure 2: Lowess Curves of 2004 Turnout by Residential Mobility, for Experimental Movers Only**



**Figure 3: Post-Treatment Social Ties and Church Attendance, By Group**



Note: Data from MTO Interim Impact Evaluation Survey, 2002 (Orr et al. 2003). "Frequency Socialize" is a scale, with values ranging from 0-12, created by summing responses to two survey items: (1) During the past 30 days, about how often did you have friends or relatives over for dinner? (2) During the past 30 days, about how often have you visited with friends or relatives at their homes? "Church Attendance" is a binary variable, where 1 indicates attendance at least once per month. Expected values (and 95% confidence intervals) are estimated using OLS ("Frequency Socialize") and logit ("Church Attendance") models, with site indicators and baseline covariates (race, age, gender) held constant at mean values.