

**Moving Out, Moving Up:  
Housing Mobility and the Political Participation of the Poor**

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**Abstract:** A large and growing literature documents the negative sociological and economic outcomes associated with the ecological concentration of poverty. Yet few empirical studies have sought to identify how living in high poverty areas shapes the political participation of the poor. Using data from a randomized housing mobility experiment, this article provides new evidence on the existence, direction, and magnitude of neighborhood effects for voter registration and turnout among the poor. The results indicate that, contrary to findings from non-experimental studies, living in areas of concentrated poverty does not depress electoral participation. Rather, the social experience of being poor in a relatively affluent community may have detrimental effects for voter turnout.

Voting in the United States is an activity sharply stratified by class, with the poor considerably less politically active than the rich. In 2004, only 48 percent of citizens with annual family incomes under \$20,000 reported voting in the presidential election, compared with 77 percent for citizens with annual incomes of \$50,000 or more (Holder 2006). Low voter turnout among the poor reflects, in part, the critical role of individual-level resources, such as income and education, in facilitating political participation, largely by lowering the costs associated with political activities (Wolfinger and Rosenstone 1980; Verba, Schlozman, and Brady 1995; Harris et al. 2005). However, scholars have hypothesized that the physical environments in which many poor people live also may shape their decisions to register and to vote, and may be contributing to the low turnout typical of this group (e.g. Alex-Assensoh 1998; Cohen and Dawson 1993). Attention has focused, in particular, on the consequences for political activity of living in neighborhoods characterized by high concentrations of poverty, a contextual attribute strongly associated with a host of negative sociological and economic outcomes. Quite apart from the question of whether low-income adults themselves possess the resources necessary for electoral participation is the question of whether their neighborhoods provide the social and institutional conditions that encourage and enable participation.

To date, efforts to identify the relationship between neighborhood poverty and political participation have relied exclusively on observational data and naturally occurring correlations between individual political behavior and contextual characteristics. The reliability of this empirical strategy 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 politically-relevant ways, from the poor residents of high-poverty neighborhoods. Without taking these (unobservable) characteristics into account, it is not possible to distinguish neighborhood effects independent from the effects of individual-level attributes. As a result, the political behavior consequences of concentrated poverty remain undetermined.

In this article, I examine the effects of contextual poverty on the political behavior of the poor, using data from a randomized housing mobility experiment to overcome the chief obstacle of prior

observational research—the problem of endogenous neighborhood selection. By randomly allocating housing vouchers that permitted poor families to move from public housing in high-poverty neighborhoods to private apartments in lower poverty areas, the experiment introduced an exogenous source of variation in residential environments. Analysis of the post-treatment political behavior of participants in the experiment provides critical leverage for assessing the existence, direction, and magnitude of neighborhood effects for voter registration and turnout among the poor. The results indicate that, contrary to findings from non-experimental studies, living in areas of concentrated poverty does not depress electoral participation among the poor. Rather, the social experience of being poor in a relatively well-off (i.e. low-poverty) community may have detrimental effects for voter participation.

### **The Perils of Concentrated Poverty**

Results from the 2000 Census revealed that more than a quarter of the metropolitan poor, 6.7 million people, lived 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). As William Julius Wilson (1987) and others have documented, a critical mass of economically stable families is not only a buffer against widespread physical decay in a neighborhood but also is essential to maintaining the (informal and formal) institutions and networks that facilitate residents’ social and economic participation and help to preserve social order. A growing empirical literature suggests that residents of high-poverty neighborhoods fare substantially worse on a wide range of outcomes (e.g. chronic joblessness, adolescent delinquency and criminal behavior, depression) than poor families with more affluent neighbors (Jencks and Mayer 1990; Ellen and Turner 1997; see Sampson et al. 2002 for review of this literature).

Considerably less is known about how the ecological concentration of poverty shapes political engagement, or whether living among neighbors of higher socioeconomic status generates positive

externalities for the poor. Just as concentrated disadvantage fosters weak labor force attachment, might it also encourage weak attachment to the political system? Or are neighborhoods (and neighbors) inconsequential, with participation among the poor constrained only by their lack of individual resources?

Certainly, there exists a considerable theoretical foundation for the expectation that neighborhoods matter for political behavior. The multidisciplinary neighborhood-effects literature identifies a number of intervening processes—“social-interactional and institutional mechanisms” (Sampson et al. 2002)—that contribute to neighborhood-level variation in problem behaviors and health-related outcomes, and may also be important in shaping activities such as voter registration and turnout among the poor. First, the sociodemographic composition of neighborhoods determines the quality, quantity, and diversity of institutional resources—libraries, schools, churches, family support centers, neighborhood associations, social and cultural clubs, and recreational facilities—available to community residents. Neighborhoods with high concentrations of poverty differ markedly in their institutional capacity from communities with more affluent residents. In addition to their official mandates, locally-based institutions can serve a number of politically-relevant roles. They are associational spaces where political information may be disseminated and potential voters contacted and mobilized (Rosenstone and Hansen 1993). For active members, participation in local groups and organizations may provide opportunities for the development of civic skills that are transferable to the electoral arena (Verba et al. 1995; Harris 1999). And, as physical structures, local institutions can provide for safe and accessible polling places, as well as sites for political candidates to visit.

Second, neighborhood-level variation in political behavior may also emerge from the distinctive characteristics of social networks in areas of concentrated disadvantage. Because voting propensity varies systematically with individual socio-economic status, the social networks within high-poverty neighborhoods may consist primarily of low engagement adults. The prevailing “anti-participation” norms, together with a lack of political knowledge, skills and experience, in these networks may further discourage participation. Conversely, where the poor live among neighbors of higher socioeconomic

status, they are exposed to participation role models (and recruiting agents), whose behavior and experience may provide support and motivation for greater political participation.

Finally, neighborhood-level variation in political behavior may result directly from the psychic costs and benefits of exposure to different physical environments. The stress, fears and general malaise that characterize daily living in deeply poor neighborhoods may discourage political participation by making such activity appear both burdensome and pointless—in short, a distraction from the overwhelming concern with individual well-being. In the midst of the physical decay associated with concentrated poverty, individuals also may be unable to perceive the utility of voting participation, or to perceive any influence in the decision making of elected officials. With access to more economically stable neighborhoods, individuals, although still poor, may experience a greater sense of efficacy and less of the stress and anxiety that necessarily crowd out attention to politics. Accordingly, the tendency toward political disengagement may attenuate.

Efforts to identify the empirical relationship between neighborhood poverty and voting participation have been limited. Scholars of political behavior have more often focused on poverty as an individual-level characteristic, rather than as a collective attribute of the environments in which individuals live (e.g. Rosenstone 1982; Lawless and Fox 2001). Moreover, the few direct tests of the effects of neighborhood poverty have yielded mixed results. Cohen and Dawson (1993), as well as Alex-Assensoh and her colleagues (1995, 1998; Alex-Assensoh and Assensoh 2001) link concentrated poverty to low levels of political efficacy and limited involvement in many forms of political activity, including voting. Yet Huckfeldt (1979) and Giles and Dantico (1982), whose research is concerned less with the potentially negative externalities of contextual poverty and more with the positive externalities of contextual affluence (as measured by educational attainment), find no relationship between neighborhood sociodemographic composition and voting among “low status” adults.<sup>1</sup> Still more puzzling, Marschall (2004) presents evidence suggesting that living in poor neighborhoods may actually have a politically mobilizing effect, as the social needs of such communities provide residents with a powerful incentive for political engagement.

Although they reach vastly different conclusions, all of these political science studies of neighborhood effects share a common empirical strategy. The evidentiary basis is non-experimental data, typically linking opinion surveys to Census data on respondents' local areas (e.g. census tract). Causal inferences are made based on naturally occurring correlations between survey respondents' reported political behavior and their neighborhood characteristics, whether median household income, poverty rate, or proportion college-educated. While the findings from such observational studies can be suggestive, the causal relationship of interest remains unclear. Observationally-equivalent survey respondents may sort themselves into different neighborhoods based on unobserved, background characteristics, which themselves may be relevant to political behavior.<sup>2</sup> In short, these studies potentially confuse the effects of neighborhoods with the effects of the characteristics of families who live in different types of areas. 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 sets them apart from the equally poor residents of high-poverty neighborhoods and that promotes political engagement. Based on observational data alone, it is not possible to conclude whether conditions of concentrated poverty inhibit activities such as voter registration and turnout. With few studies, conflicting results, and empirical methods that are unreliable for causal inference, there remains considerable uncertainty about the participation effects of neighborhood poverty. Data from a randomized housing mobility experiment present a unique opportunity to assess how and how much neighborhoods matter for voting participation among the poor.

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

Launched by the U.S. Department of Housing and Urban Development (HUD), the Moving to Opportunity (MTO) for Fair Housing demonstration program enrolled over four thousand low-income families from five metropolitan areas—Baltimore, Boston, Chicago, Los Angeles, and New York—in an experiment designed to assess the impact of providing poor families with the opportunity to move to communities with lower levels of poverty. The program recruited families from among residents of public housing developments located in census tracts with 1990 poverty rates of at least 40 percent.<sup>3</sup> From

1994-1997, 4248 eligible families, mostly female-headed minority households, 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=1739) whose members also received a voucher, but one that could only be used to rent 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. For the experimental “compliers” (i.e. those who leased up), the average 1990 poverty rate in the new census tract was 7.5 percent; for the Section 8 group, 26.9 percent. Figure 1 presents kernel densities of post-treatment census tract poverty levels for experimental and Section 8 compliers and non-compliers, as well as for members of the control group. The poverty levels are duration-weighted averages over *all* neighborhoods lived from the year of random assignment until 2002, when HUD conducted a comprehensive canvass of the MTO families as part of an interim evaluation of program impacts.<sup>5</sup> The graph shows that, in the 4-7 years after random assignment, adults who received and used the experimental and standard Section 8 vouchers lived in areas with significantly lower poverty levels than did the controls; the shift is especially dramatic for the experimental group. Meanwhile, the densities for non-compliers are similar to that for members of the control group.

[FIGURE 1 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 compliers moved 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. Specifically, assignment to a voucher group contributed to two dynamics relevant to the questions of interest here: (1) members of the experimental and Section 8 groups were able to move *out* from public housing; (2) members of the experimental group in particular were able to move *up* from poor neighborhoods.

## **Sample and Data Sources**

This analysis of the housing mobility experiment focuses on voter registration and vote history data collected in 2006, 9-12 years after random assignment, for adults who participated in the MTO demonstration.<sup>6</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. 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 full sample. The statistics reveal that the sample adults are primarily non-white (almost two-thirds are black); female; very low income; unemployed and heavily dependent on public assistance.

[TABLE 1 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 files were matched to the database of MTO participants based on residential address, name, gender, and date of birth.<sup>7</sup> This process identified 57 percent (n=2428) of the MTO adult sample among the residents registered to vote in the six baseline counties.<sup>8</sup> The sample adults not listed in the county voter files include MTO participants who were not registered to vote, as well as participants who no longer reside in one of the six baseline counties. For this latter group (n=707), voter registration and turnout are both unobserved.<sup>9</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 (e.g. through listwise deletion of all out-migrants), because this subgroup is endogenous to the treatment, such differences would not constitute experimental impacts.<sup>10</sup>

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 out-migrants.<sup>11</sup> Five values are imputed 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*. These 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.<sup>13</sup>

### **Empirical Expectations and Models**

Informed by the findings from earlier observational studies documenting the myriad deleterious consequences of concentrated poverty, I hypothesize that moves to lower poverty neighborhoods would lead to greater voter participation for poor adults. In order to identify the effects of neighborhoods, 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). In particular, I begin with the following empirical expectations:

- If concentrated poverty lowers political participation among the poor, then adults offered a housing voucher (with or without geographic restrictions) are more likely to be registered and to vote than adults in the control group.
  
- If concentrated poverty lowers political participation among the poor, then adults offered housing vouchers restricted to low-poverty neighborhoods are the most likely of the three treatment groups to be registered and to vote.

There are two parameters of interest that are estimated in the regression models and reported in the tables: the intent-to-treat (ITT) and the treatment-on-treated (TOT) effects.<sup>14</sup> The ITT effect, estimated from the difference in mean outcomes for the treatment and control groups across all sample adults, is the impact of being *offered* the treatment, regardless of subsequent compliance. The TOT effect is the impact of the treatment on the compliers—the MTO sample adults who actually leased up using a program voucher.

In a linear 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.<sup>15</sup>

We infer the TOT impact from the ITT impact  $\pi_{ITT}$  by dividing the latter by the lease up rate for the treatment group (Bloom 1984; Ludwig et al. 2008).<sup>16</sup> The TOT estimates are non-experimental, in the sense that they are not directly observed for whole randomly assigned groups. They are based on the weak assumption that the effect of the treatment occurs entirely through moving using a program voucher (i.e. families in the treatment group were not affected if they did not use the voucher).

The models estimating treatment impacts include as covariates select individual and household factors measured prior to random assignment, as well as fixed effects for each of the MTO sites (with New York as the omitted category).<sup>17</sup> The covariates are included in order to reduce residual variation, thereby increasing the precision of the estimates; in a randomized experiment, the unbiased estimation of treatment impacts does not require the inclusion of covariates. Mediating factors that may account for observed treatment effects—for example, the socioeconomic characteristics of the voucher recipients' new neighborhoods—are not included as right-hand side variables, as such factors were measured after random assignment; their inclusion would contribute to post-treatment bias. While this approach does not permit one to model the path of effects, it does permit for accurate estimates of total treatment impacts.

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

What happens when poor adults have the opportunity to move out and up? Table 2 reports the results of linear regression models estimating the *combined* impact of the experimental and Section 8 treatments on voter registration and turnout.<sup>18</sup> The results suggest that, far from enhancing electoral participation, to be poor in a community of plenty may be, if anything, a demobilizing experience.

[TABLE 2 HERE]

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.<sup>19</sup> Adults who were *offered* a housing voucher and the opportunity to move, with or without geographic 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. For those MTO participants who actually used the housing voucher to move to a new apartment, i.e. complied with the treatment, the (TOT) effect of the move was to reduce the likelihood of registering by more than six percentage points. 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 any treatment impacts on voter registration at all, they are too small to be detected reliably.

The fact that rates of voter registration are high and relatively stable (across control and treatment groups) for the very poor adults whose families participated in the MTO demonstration is not entirely surprising, given the ease of registration as a result of the 1993 National Voter Registration Act (i.e. “Motor Voter”). Among the act’s key provisions is that state governments make voter registration forms available at a variety of government agencies that citizens visit for other purposes—not only drivers’ license registration centers, but also disability, unemployment and welfare offices. With all of the MTO families, by definition, receiving some form of means-tested public assistance—housing, AFDC or TANF, food stamps, SSI—the sample adults likely have numerous opportunities to access voter registration forms. This access would not be expected to vary across treatment and control groups, as even members of the treatment groups continue to receive public assistance (e.g. the housing voucher).

But while Motor Voter may facilitate voter registration among the poor, offsetting any potential advantages or disadvantages associated with their residential circumstances, the provisions of the act do little for voter participation itself. This is evident from the 2004 and 2002 turnout results also reported in

Table 2. Rates of voter turnout are low relative to registration and, more importantly, the rates vary across control and treatment groups. The adults randomly assigned to a voucher group were *less* likely than members of the control group to vote in the 2004 Presidential election, a pattern directly at odds with the hypothesized effects of concentrated poverty. Whether defined narrowly as voter turnout in the baseline counties, or defined so as to allow for out-migrants 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 compliers, the effect of receiving and using the housing voucher was to lower 2004 voter turnout by 6.5 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 differ in magnitude by voucher group. The demobilizing effect of the housing vouchers is more dramatic for adults assigned to the experimental group, whose vouchers could only be used in low-poverty neighborhoods, than for those assigned to the standard Section 8 group, whose vouchers were unrestricted. Table 3 reports the treatment impacts on voting in the 2004 election for each voucher group. The model estimates a statistically significant negative intent-to-treat effect of 3.7 percentage points and a negative treatment-on-treated effect of nearly eight percentage points for the experimental group with restricted vouchers. That is, for poor adults who used rental vouchers, together with mobility counseling, to move out of public housing and into low-poverty neighborhoods (i.e. the compliers), the effect was to lower voter turnout by 18 percent ( $.078/.432$ ). Significance tests indicate that 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. However, whether or not the treatment impact for the Section 8 group is statistically differentiable from zero, the important finding is the more substantial decline in turnout for the experimental (restricted) as opposed to the Section 8 (unrestricted) group. Thus, contrary to expectations, the group whose residential circumstances likely improved most dramatically—the group required to move up and provided with the assistance to make that possible—was the group least likely to be active in electoral politics.

[TABLE 3 HERE]

### **Mobility or Marginality?: Two Interpretations of Low Turnout**

The negative treatment impact associated with the move to low-poverty neighborhoods is unexpected, in light of the extensive sociological and economic evidence documenting the deleterious effects of concentrated poverty, as well as findings from the few observational studies of political participation (e.g. Cohen and Dawson 1993; Alex-Assensoh 1995, 1998). It is not immediately clear what inference we should draw from this result. The design of the MTO experiment, while it provides critical leverage for causal inference (and, in that way, represents a substantial improvement over non-experimental approaches), nonetheless does not allow for precise specification of the causal mechanisms responsible for the negative treatment impact. The random assignment of Section 8 housing vouchers addresses the fundamental problem of endogenous neighborhood selection, but it does so with an intervention that manipulates more than just the socioeconomic attributes of the participants' residential environments. The MTO treatment simultaneously alters the exposure to contextual poverty, along with the experience of residential mobility, which itself is an important life event with consequences for political participation (Sampson 2008). As a result, MTO is not strictly a test of concentrated-poverty effects on low-income people. This confound does not threaten the validity of the core causal claim: the receipt of an experimental voucher, and its use to rent an apartment in a low-poverty neighborhood, *causes* a decline in subsequent voter turnout. But with each confounding intervention comes a plausible hypothesis about the substantive interpretation of the negative treatment impact. The hypotheses are not mutually exclusive; they point, however, to distinct causal mechanisms.

One interpretation of the negative treatment impact is that it reflects the disruptive nature of residential mobility. Following random assignment, adults in the Section 8 and experimental treatment groups experienced more mobility than those in the control group. Obviously, compliance with the treatment required at least one residential move, out of public housing and into a private rental apartment; and, for the experimental voucher group, that apartment had to be in a different neighborhood. But some

MTO participants experienced repeated moves. In 2002, as part of an interim impact evaluation, HUD collected data from MTO participants about each of their residential moves in the 4-7 years after random assignment, and discovered more ongoing mobility than they had anticipated when designing the demonstration. 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. And many of these repeat movers changed neighborhoods as well as apartments.<sup>20</sup> Figure 2 plots the average number of moves and number of years at current address (as of 2002), and reveals the greater mobility of the treatment compliers in the years following random assignment.

[FIGURE 2 HERE]

The disruptions associated with the act of moving itself, e.g. the need to re-register and to locate a new polling place, by increasing the costs of participation, can deter citizens from electoral involvement (Wolfinger and Rosenstone 1980; Squire, Wolfinger and Glass 1987; Brians 1997; Highton 2000).<sup>21</sup> Yet, while low voter participation is common among the residentially mobile, there is reason to doubt that mobility is to blame for the negative treatment impacts observed in Table 3. First, there are no treatment impacts estimated for the adults assigned to the standard Section 8 group, although they also experienced greater residential mobility relative to the control group. Second, the research on residential mobility identifies the costs of re-registering as substantially responsible for lowering electoral participation among frequent movers. But there are no treatment impacts on voter registration; adults assigned to the experimental group are just as likely to be registered to vote as those in the control group. Finally, if it is the ongoing mobility of the experimental compliers—not simply the initial move out of public housing, but the pattern of repeated moves—that is lowering their voter participation, then we might reasonably expect the adults who experienced the most residential instability post-assignment to exhibit the lowest rates of voter turnout. In fact, contrary to that expectation, lowess curves of voter turnout by various indicators of residential stability (e.g. number of moves, years at current address, years in current neighborhood; all measured as of 2002) reveal that even the adults with relatively more stable residential

circumstances, e.g. experimental compliers who moved only once or twice, voted at rates similar to their more peripatetic counterparts.<sup>22</sup> This descriptive evidence, together with the null impacts on registration and for the Section 8 group, suggests that the act of moving itself may not account for the lower voter turnout among the experimental group. There is reason to look elsewhere—in particular, to the neighborhood experience itself—for an explanation.

When one considers the mediating role of social networks and social engagement in many theoretical accounts of contextual effects, the notion that poor people in poor neighborhoods may be more politically active than poor people in non-poor neighborhoods begins to look less surprising. Membership in neighborhood-based social networks and access to and engagement in neighborhood-based social institutions are hypothesized to be among the causal mechanisms through which divergent residential circumstances lead to divergent sociological, economic, and political outcomes. Through their exposure to social networks and institutions dominated by comparatively high-status individuals, with stronger participatory norms, more political information and greater contact with recruiting agents (e.g. political parties and candidates), poor people are expected to be motivated toward greater political participation. But is such embeddedness a realistic depiction of the social world of poor people in non-poor neighborhoods? More pointedly, is it a realistic depiction of the social experience of the poor adults whose families were randomly assigned to the MTO experimental treatment? Implicit in the initial expectations regarding MTO treatment effects are optimistic assumptions about the ease with which old ties from the baseline neighborhood are replaced by new ties in low-poverty neighborhoods. Perhaps these assumptions are too optimistic.

As described earlier, experimental compliers, those who used a housing voucher to lease an apartment in a low-poverty neighborhood, ventured to areas relatively far from the city center and from their original public housing developments. (By comparison, the Section 8 compliers chose apartments in close proximity to their baseline development.) In moving to these more distant communities, members of the experimental group settled among neighbors whose socioeconomic status and, to a much lesser extent, racial and ethnic characteristics were strikingly dissimilar from their own—and, certainly, more dissimilar

than the status and characteristics of the neighbors of the control or Section 8 groups. This dissimilarity, given the widespread tendency toward status homophily (Lazarsfeld and Merton 1954; McPherson et al. 2001), may impede the integration of the MTO adults into the social fabric of their new neighborhoods. Commonality makes communication and relationship formation easier; status dissimilarity can breed social distance.<sup>23</sup> Thus, Huckfeldt (1979) finds that the positive externalities associated with high-status areas—more involvement in socially-based political activities—accrue only to high-status individuals. He concludes that the low-status residents of high-status neighborhoods experience alienation, which limits their exposure to the group-based norms and social networks that promote political participation by turning it into a social obligation.

Beyond the isolating nature of status dissimilarity, there are still other factors that may work against the social integration of the experimental compliers into their new communities. The spatial mismatch between low-poverty residential locations, well away from the city center, and low-skilled employment opportunities may result in members of the experimental group commuting significant distances in search of work, making for less time spent in the neighborhood. Continuing familial and friendship ties to their baseline communities may be similarly distracting. Finally, as Briggs and Turner (2006) describe, based on ethnographic studies of MTO participants, adults from these families are not “joiners” in general, typically wary of thick social engagements of the sort thought to drive neighborhood effects. It is not insignificant that the families who volunteered for the MTO demonstration sought to escape their communities. In short, experimental compliers may have neither the time nor the inclination to connect with their new neighbors, or with the institutions these neighbors create. Our theoretical expectations about neighborhood effects assume a level of social integration with the immediate residential environment that, in practice, may be rare for low-income people in general and for the experimental compliers, in particular.

Results from a post-treatment survey of the MTO families, administered in 2002 as part of the interim evaluation of program impacts, provide some evidence suggesting that experimental compliers on average were not positioned to take full advantage of the opportunities available in their new

neighborhoods. As the descriptive statistics summarized in Table 4 indicate, a sizable minority (48%) of the compliers continued to maintain close relationships with friends in the baseline community, with nearly 40% returning to their old neighborhood at least once a month and 18% returning at least once a week. Additionally, for the subset of experimental compliers who were employed, their daily commute from home to work averaged over 30 minutes. The cumulative effect of these and other factors is evident in the failure of the experimental compliers to establish a social network within their new communities. More than two-thirds indicate that they have no friends in their neighborhood. Even casual social interaction is rare; 40% of the compliers state that they “stop to chat with a neighbor in the street or hallway” almost never and certainly not more than a few times a year.<sup>24</sup>

[TABLE 4 HERE]

The fact that experimental compliers failed to establish social networks within their new communities offers some explanation for why we do not observe *greater* electoral participation among this group, relative to the control group. As for the negative treatment impact (i.e. the finding that the control group votes at a higher rate than the experimental group), we might again look to the mediating role of social networks and institutions. Perhaps it is the presence of institutions such as churches and other religious organizations, even in neighborhoods suffering from high levels of poverty (Alex-Assensoh 1995, 1998; McRoberts 2004), that facilitates the political engagement of the poor.<sup>25</sup> Churches can be important sites for electoral mobilization, positioned at the center of networks of dialogue and participation—especially for African Americans, as Harris (1999) and others have demonstrated. For reasons of proximity and status similarity, members of the control group may be more connected than the experimental group to these local institutions—either directly as congregants, or indirectly through members of their social network who are active participants. Meanwhile, experimental compliers not only are disconnected from the institutions that are spatially proximate, but also are more tenuously connected—because of distance and disrupted social networks—to whatever institutions existed in their baseline communities. Even Section 8 compliers, who chose to rent apartments in neighborhoods that were demographically similar and spatially proximate to their old neighborhoods, likely maintained

stronger connections than experimental compliers to local institutions such as churches, whether in their new or baseline communities.

Evidence from the 2002 interim impact evaluation provides some preliminary support for this hypothesis. A model estimating treatment impacts on the frequency of church attendance finds a negative and marginally statistically significant ( $p < .08$ ) intent-to-treat effect for the experimental group. Members of the experimental group were almost four percentage points less likely than the control group to attend church at least once a month; for the experimental compliers, the estimated TOT effect was 7.7 percentage points. There was no treatment impact associated with assignment to the Section 8 group. Thus, it may be that poor people in low-poverty neighborhoods experience a level of social alienation that, at least with respect to political engagement, puts them at a disadvantage relative to poor people in high-poverty neighborhoods. To paraphrase DuBois (1903), to be poor is hard, “but to be a poor race in a land of dollars is the very bottom of hardships.”

## **Discussion**

The ecological 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 severely limit the opportunities for social mobility. But while the perils of contextual poverty—and, by extension, the promise of contextual affluence—for family well-being have been amply documented, we know far less about its consequences for the political participation of the poor. Using data from a randomized housing mobility experiment, this article provides new evidence about the existence, direction, and magnitude of neighborhood effects for voter registration and turnout among the poor. The analysis shows that, for adults whose families were offered vouchers to rent apartments in low-poverty neighborhoods, the opportunity to move out (of public housing) and up (to low-poverty) had no effect on rates of voter registration and depressed voter turnout among this group. While the design of the MTO experiment limits our ability to identify with precision the causal mechanisms responsible for these

effects, the evidence suggests that it may be the social alienation experienced by poor people in low-poverty neighborhoods that prevents them from realizing the potentially positive externalities associated with living among more affluent neighbors. For adults assigned to the low-poverty voucher group, the experimental intervention separated them from local webs of (similarly poor) friends and family, while confining them to the social margins of their new, economically stable neighborhoods. The result was lower voter participation. Thus, whatever housing mobility may achieve in terms of labor market outcomes, mental health, or educational attainment, it alone may not be enough to strengthen poor Americans' weak ties to the political system.

The results from this experimental study are directly at odds with the findings from earlier empirical research on neighborhood-related participation effects, all of which relied on observational data. These discrepant findings may underscore the limits of non-experimental methods for making causal inferences about contextual effects, given the problem of endogenous neighborhood selection. Perhaps the reason why scholars previously found concentrated poverty to be negatively associated with participation is because poor people who live in high-poverty neighborhoods are different from poor people who seek out and settle in low-poverty neighborhoods. The characteristics that enable the latter group to move out—without the aid of an experimental intervention, including a rental voucher and mobility counseling—may be the same attributes that facilitate participation in the political process, as well as full engagement in the social life of their communities. Empirical strategies that fail to take into account these unobservable characteristics are not able to identify neighborhood effects.

While analysis of the MTO demonstration provides critical leverage for identifying causal effects, the limitations of the current study must also be recognized. The design of the voucher experiment confounds residential mobility with exposure to contextual poverty, complicating the substantive interpretation of the treatment impacts.<sup>26</sup> Once causal impacts are estimated, theory and non-experimental evidence, in the form of correlations among post-treatment variables, must be brought to bear in adjudicating between competing interpretations. Using such an approach, I identify several reasons to doubt that residential mobility per se is to blame for the low voter turnout among adults assigned to the

experimental voucher group. To wit, there are no treatment impacts for adults assigned to the Section 8 group, though they also experienced residential mobility; registration rates are unchanged relative to the control group, though the need to re-register is the primary (participation-related) cost associated with moving; and, even the most residentially-stable of the experimental compliers vote at lower rates.

Admittedly, this approach does not compare to the sharp inferences that would be possible with the benefit of a more ambitious experimental design: rather than an individual-level intervention that relies on vouchers and voluntary compliance, a neighborhood-level intervention that manipulates only participants' exposure to contextual poverty. The MTO demonstration falls short of that ideal, but it nonetheless brings us much closer to isolating the causal role of this key contextual attribute.

For those concerned about democratic citizenship, one aspect of which is participation in the electoral process, the findings here—that low-poverty neighborhoods are associated with lower political participation—should not be interpreted as an argument in favor of relegating poor people to neighborhoods of concentrated poverty. A more reasonable inference we might draw from the negative treatment impacts is that mobility without social integration is of limited benefit—with respect to electoral participation—and, in fact, may be counter-productive. Whatever deleterious consequences result from living in areas of concentrated poverty cannot be offset simply with a ticket out. To be poor in a community of plenty, if it consigns one to a life on the social margins, can be a uniquely demobilizing experience. Yet the possibility remains that the opportunity to move out and up, if coupled with interventions that facilitate integration into the social life of the community (its networks and institutions), could enable the poor to participate more fully in the political process.

## Appendix A: Estimating Treatment Effects with Censored Voter Data

Because voter files were collected for only the six baseline counties, we have incomplete outcome data for the MTO adults who migrated out of these counties post-randomization. For this subgroup ( $n=707$ ), registration status and vote history are unobserved. Given this censored sampling process, there is insufficient information to estimate key quantities of interest—e.g. population mean registration and turnout; effects of treatment assignment—without imposing restrictions on the population distribution. Specifically, we are unable to fully identify, with the observed data alone, the regression function  $E(y|x)$ , where:

$$E(y|x) = E(y|x, z=1)P(z=1|x) + E(y|x, z=0)P(z=0|x)$$

In this equation,  $z$  is a binary indicator equal to one when the outcome variable,  $y$ , is uncensored; zero, when censored. The terms  $E(y|x, z=1)$ ,  $P(z=1|x)$  and  $P(z=0|x)$  (the mean outcome of the uncensored observations and the response and non-response probabilities) are observable from the data, but we do not observe  $E(y|x, z=0)$ , the mean outcome of the censored data. To identify  $E(y|x)$ , it is necessary to impose certain assumptions about the nature of the missing data.

Table A1 reports estimates for  $E(y|x)$  and for  $E[y|x=x_1] - E[y|x=x_0]$ , the effect of treatment assignment, calculated under different distributional assumptions. The analyses demonstrate the sensitivity of inferences to assumptions, not all testable, about the process generating missing data.

[TABLE A1 HERE]

[1] The first set of inferences uses Manski's (1989) nonparametric method to identify "worst-case" bounds on the population means and treatment effects. The method imposes (essentially) no assumptions about the distribution of the missing data, beyond the restriction that  $E(y|x, z=0)$  is bound by the interval  $[0,1]$ . With this restriction, bounds on  $E(y|x)$  can be calculated:

$$E(y|x, z=1)P(z=1|x) \leq E(y|x) \leq E(y|x, z=1)P(z=1|x) + P(z=0|x)$$

Similarly, we can derive bounds for the effect of treatment assignment on the outcome variable:

$$[E(y|x_1, z=1)P(z=1|x_1)] - [E(y|x_0, z=1)P(z=1|x_0) + P(z=0|x_0)] \leq$$

$$E[y|x=x_1] - E[y|x=x_0] \leq$$

$$[E(y|x_1, z=1)P(z=1|x_1) + P(z=0|x_1)] - [E(y|x_0, z=1)P(z=1|x_0)]$$

The “worst-case” bounds reported in Table A1 are sharp, in that they leverage all of the information available in the data, but very wide. The width of the bounds makes clear what limited inferences can be made from the data without imposing distributional assumptions. Across outcomes, the bounds consistently include a conclusion of no treatment effect. (Manski’s method can produce wide bounds no matter how strong the true effect is.) Within these “worst-case” bounds, however, are hypothetical values for registration (and turnout) that correspond to scenarios that are very unlikely, despite being technically possible. The lower bounds on the ITT effects correspond to scenarios in which every MTO out-migrant in the control group, but none in the voucher groups, is registered to vote (voted in 2004, 2002); for the upper bound, every out-migrant in a voucher group, but none in the control group, is registered (voted). Such circumstances are extreme and unlikely; given the positive correlation between income and voter participation, it is highly implausible that any subgroup of the low-income MTO adults would have registration or turnout rates of 100 percent.

The remaining columns of Table A1 represent efforts to narrow the bounds on the estimated treatment effects by imposing various structural assumptions. These assumptions enable point identification of mean outcomes and treatment effects. Note that all of the point estimates fall within the Manski worst-case bounds.

[2] The second set of estimates is derived under the assumption that the outcome data are “missing at random” (MAR), conditional on the observed data. This assumption underlies the multiple imputation approach used in the article (King et al. 2001). (The estimates reported in Table A1 are identical to those in column two of Tables 2 and 3.) While it is not possible to verify absolutely the validity of the MAR assumption—whether missingness depends on the unobserved value of the missing response, after controlling for observed data, is unknowable—its appropriateness depends substantially on

the specification of the imputation model and whether (and to what degree) the model includes variables that perform well in predicting the pattern of missingness. In addition to all of the variables from the analysis models, the imputation model used here included the following variables, which can be shown empirically to be highly predictive of missingness in the outcome data: MTO voucher use, family size, age of children, marital status, car ownership, disability status, crime victim (at baseline), perceptions of (baseline) neighborhood safety, satisfaction with (baseline) neighborhood, certainty about ability to find apartment, moving because of drugs or to find better schools, friends or family in (baseline) neighborhood. The success of these variables at predicting missingness suggests that the MAR assumption is plausible and that imputations generated from the observed data will not drastically mislead. Moreover, because errors in the imputation model have no effect on the observed data, which remains unchanged, and because the fraction of incomplete cases is not very large (~16%), the imputation approach used here may be fairly robust to violations of MAR.

[3] The third set of estimates does not so much impose structural assumptions as redefine the outcome of interest such that the data are complete. This is one of the approaches used in the article. (The estimates reported in Table A1 are identical to those in column one of Tables 2 and 3.) By incorporating auxiliary information known with certainty—namely, that only the residents of a county can be registered to vote in that county—and defining the outcomes as registration and voter turnout in baseline counties only, the missing data on out-migrants can be equated to non-registration and non-voting *in the baseline county*.

[4] The fourth set of estimates is derived under the assumption that the missing data are ignorable, i.e. that the censored and uncensored cases do not differ from one another in any salient ways, observed or unobserved. This identifying assumption enables listwise deletion of all observations with incomplete data, leaving only uncensored cases for analysis. When the assumption fits, estimates are unbiased if inefficient. However, given that it is possible to predict missingness using the observed data, the assumption of ignorable missing data can be rejected here, at least in favor of MAR (see [2]).

## Endnotes

<sup>1</sup> Each of these two studies distinguishes between socially-based (e.g. campaigning) and individually-based (e.g. voting) participation, and conditions the effect of neighborhood context on individual socio-economic status. Neither study finds evidence of any 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>2</sup> The reliability of self-reported behavior presents an additional challenge to inference, although one that is arguably much less worrisome than bias due to selection on unobservables.

<sup>3</sup> Eligible families met the following additional criteria: had very low income that met the Section 8 income limits; had at least one child under the age of eighteen; and were in good standing with the housing authority.

<sup>4</sup> The geographic restrictions on the experimental group applied only for the first year following random assignment. Experimental and Section 8 treatment group families who failed to lease up were able to remain in public housing, as long as they remained eligible.

<sup>5</sup> The available census-tract poverty data are categorical: <10%; 10-19%; 20-29%; 30-39%; ≥ 40%. 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> Sample adults include only those participants who were over age 18 at the time of randomization and does not include those participants who had entered the MTO program as

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children. There are 29 sample adults without baseline data on race/ethnicity. These 29 cases were excluded from analysis. Even with these cases excluded, the remaining adult sample (n=4219) remain balanced on observable characteristics across treatment and control groups.

<sup>7</sup> Abt Associates, under contract with HUD, has tracked the address histories of MTO families since baseline. The match to the county voter files—a process conducted by the firm, using confidential data to which the author was not permitted access—was based on the most current address available for each participant.

<sup>8</sup> By comparison, 61 percent of adults with family incomes of less than \$20,000 reported being registered to vote in 2004 (Holder 2006).

<sup>9</sup> To assemble county voter files for the dozens of counties to which this relatively small subset of MTO participants had migrated was prohibitively expensive, in terms of time and money.

<sup>10</sup> For similar reasons, it would not be appropriate to restrict the voter turnout analysis to just the subgroup of MTO sample adults listed as registered voters, even though we observe turnout only for that subgroup. Descriptive statistics could still be useful for understanding dynamics within these various subgroups. But the risk of selection bias argues forcefully against the use of non-experimental methods for estimating treatment impacts.

<sup>11</sup> Statisticians have demonstrated that 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 observed data, missingness is independent of the unobserved data), outperforms listwise deletion by correcting for the inefficiency and bias that result from the latter approach (e.g. Schafer and Olson 1998). The assumptions that underlie multiple imputation are

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reasonable for the MTO voter data, where missingness can be predicted by factors associated with out-migration of MTO participants.

<sup>12</sup> Schafer and Olsen (1998) note that multiple imputation “can be highly efficient even for small values of  $m$  [number of imputations]” (Schafer and Olson 1998, 548). Given the fraction of missing information in the MTO voter data, I achieve about 97 percent efficiency with  $m=5$ .

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 then analyzed using standard complete-data statistical methods. The results presented in the proceeding tables are the combined results across imputed datasets, calculated using formulas that formally incorporate missing-data uncertainty (see King et al. 2001, 53 for formulas).

<sup>13</sup> Appendix A (“Estimating Treatment Effects with Censored Voter Data”) reports the results of a sensitivity analysis in which MTO treatment impacts were estimated under different assumptions about the nature of the missing data.

<sup>14</sup> The tables also report the control mean, the average value for the control group on each outcome measure. As with the ITT and TOT impacts, this estimate is regression-adjusted with robust standard errors.

<sup>15</sup> When estimating separate ITT effects for each of the two voucher groups, standard Section 8 and experimental Section 8, the model includes two dichotomous indicators for treatment status,  $Z^s$  and  $Z^e$  :

$$(2) Y_i = \alpha + Z_i^s \pi_{ITT}^s + Z_i^e \pi_{ITT}^e + \varepsilon_i$$

<sup>16</sup> The standard errors for the TOT estimates are similarly adjusted. Thus, while TOT impact estimates are substantially larger than ITT estimates, they are statistically significant only if the

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corresponding ITT estimate is significant. In addition to the method proposed by Bloom (1984), I also estimated the TOT effect with an instrumental variable approach, using treatment assignment ( $Z$ ) as the excluded instrument for compliance (i.e. MTO voucher use) in a two-stage least squares model. The results are identical to those reported here.

<sup>17</sup> The full model, with baseline covariates and separate indicators for assignment to the standard Section 8 and experimental voucher groups:

$$(3) Y_i = \alpha + Z_i^s \pi_{ITT}^s + Z_i^e \pi_{ITT}^e + X_i \beta + \varepsilon_i$$

where  $X_i$  includes site indicators, as well as the following baseline covariates: race/ethnicity, gender, age, work status, education, receipt of AFDC, years in neighborhood, number of recent moves. All of the models estimated in this paper use weights, created by the MTO investigators, to adjust for changes in the random assignment ratios over the course of the demonstration, 1994-1997 (Orr et al. 2003, Appendix B).

<sup>18</sup> Although the outcome measures are dichotomous, I use a linear regression model (with robust cluster standard errors) for its ease of interpretation. All of the analyses reported here were replicated with logit specifications and the substantive results were unchanged.

<sup>19</sup> Some of the public housing developments from which MTO participants were recruited were later targeted as part of the Hope VI program. As a result, some members of the control group ended up moving out of public housing and, in some cases, out of the baseline county. In addition to moves prompted by Hope VI demolitions, some members of the control group eventually received rental vouchers through other (non-MTO-related) sources.

<sup>20</sup> Most repeat movers reported in 2002 that, in the moves that followed their initial MTO lease up, they had begun their apartment search in the same or similar neighborhoods.

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<sup>21</sup> In addition, residential mobility—across neighborhoods, if not simply across apartments—separates the MTO treatment compliers from their local webs of friends and family. We might hypothesize that this social dislocation engenders negative psychological states, e.g. feelings of loneliness, that might reduce interest in electoral involvement. Perhaps not surprisingly, however, given that MTO participants volunteered for randomization and voluntarily complied with the treatment, the 2002 interim impact evaluation found no evidence of negative impacts on mental health and well-being among either the experimental or Section 8 groups. To the contrary, the “mental health benefits of the voucher offers for adults...were substantial,” comparable in magnitude to “some of the most effective clinical and pharmacologic mental health interventions” (Kling et al. 2007; 83,102).] It seems unlikely that any emotional traumas associated with mobility could be to blame for the negative treatment impact on voter turnout.

<sup>22</sup> See Appendix Figure 1.

<sup>23</sup> Status dissimilarity within neighborhoods also may contribute to an acute sense of relative deprivation (Crosby 1976; Jencks and Mayer 1990), which, if it produces negative orientations such as low political efficacy, might reduce levels of voter participation.

<sup>24</sup> The children of the MTO families may be better positioned than the adults to, eventually, realize any participation benefits associated with living in low-poverty neighborhoods. Children, especially if they attend local schools, may be integrated more fully into the social world (and norms) of their neighborhood. In addition, their exposure to participatory norms, political information, and recruiting agents comes at a more formative stage in their political socialization.

<sup>25</sup> Alex-Assensoh (1995, 1998) also notes that, in addition to religious institutions, racial organizations found in poor minority communities can be effective in mobilizing African

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Americans. She cites this in explaining why poor whites may suffer greater social and political isolation in concentrated poverty neighborhoods than do similarly poor black residents.

<sup>26</sup>Residential mobility is also to blame for the censored outcome data among out-migrants, requiring the use of multiple imputation methods.

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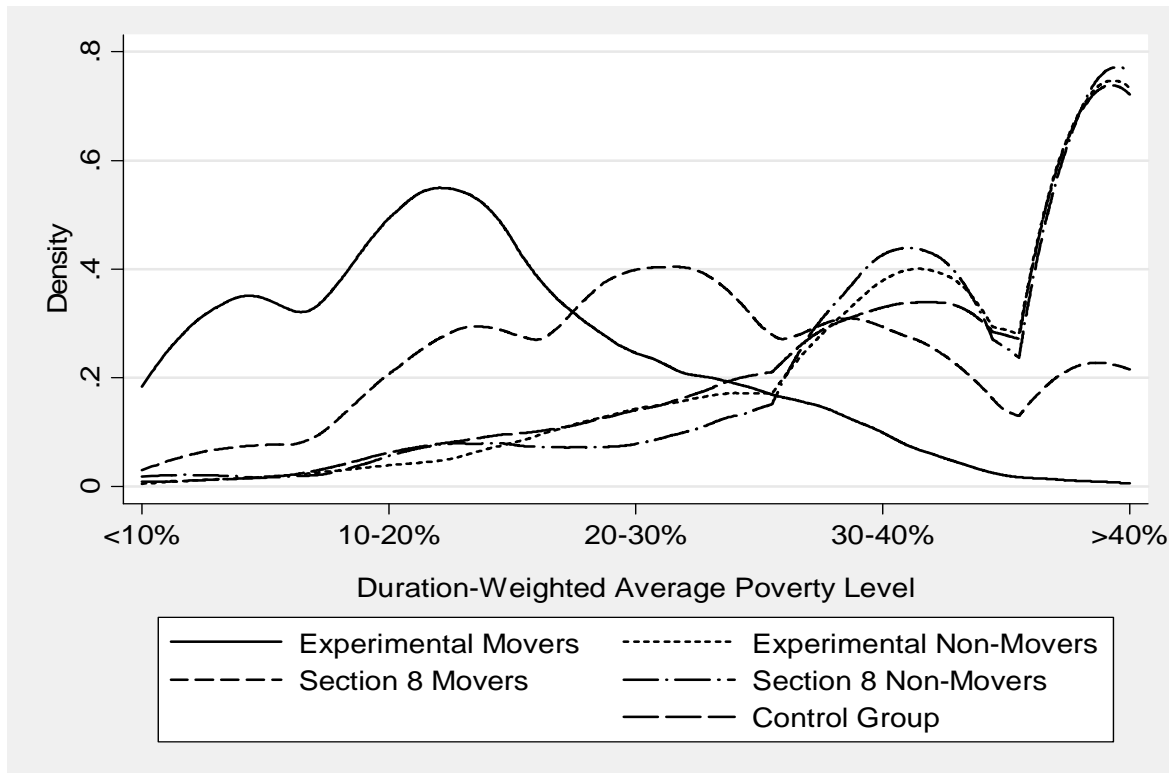
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**Figure 1: Densities of Average Census-Tract Poverty Level, By Group**



Note: Average poverty level is a duration-weighted average of census tract locations lived from the year of random assignment until 12/31/2001. Source: MTO Interim Impact Evaluation, 2002

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

Site Sample Size	
Baltimore	636
Boston	959
Chicago	894
Los Angeles	678
New York	1081
Sex of Head of Household	
Male	8.4%
Female	91.6%
Race/Ethnicity of Head of Household	
Hispanic	30.4%
Black Non-Hispanic	62.6%
White Non-Hispanic	2.9%
Head of Household Marital Status	
Never Married	62.2%
Married	11.3%
Divorced	9.5%
Widowed or Separated	17.1%
Average Household Income	\$9,314
Percent with AFDC as Primary Income Source	61.6%
Head of Household Education	
High School Graduate	40.6%
GED	19.7%
Neither	39.7%
Head of Household Work Status	
Full-time	16.1%
Part-time	11.6%
Not Working	72.2%

**Table 2: 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	0.595	0.666
Pooled ITT	-.034 (.017)*	-.027 (.017)
Pooled TOT	-.064 (.031)*	-.051 (.032)
Voted 2004		
Control Mean	0.389	0.432
Pooled ITT	-.037 (.016)*	-.035 (.017)*
Pooled TOT	-.069 (.031)*	-.065 (.033)*
Voted 2002		
Control Mean	0.210	0.245
Pooled ITT	-.031 (.014)*	-.028 (.016)
Pooled TOT	-.059 (.026)*	-.052 (.030)
	<b>Total N</b>	<b>4219</b>
	<b>Pooled Lease Up Rate</b>	<b>0.533</b>

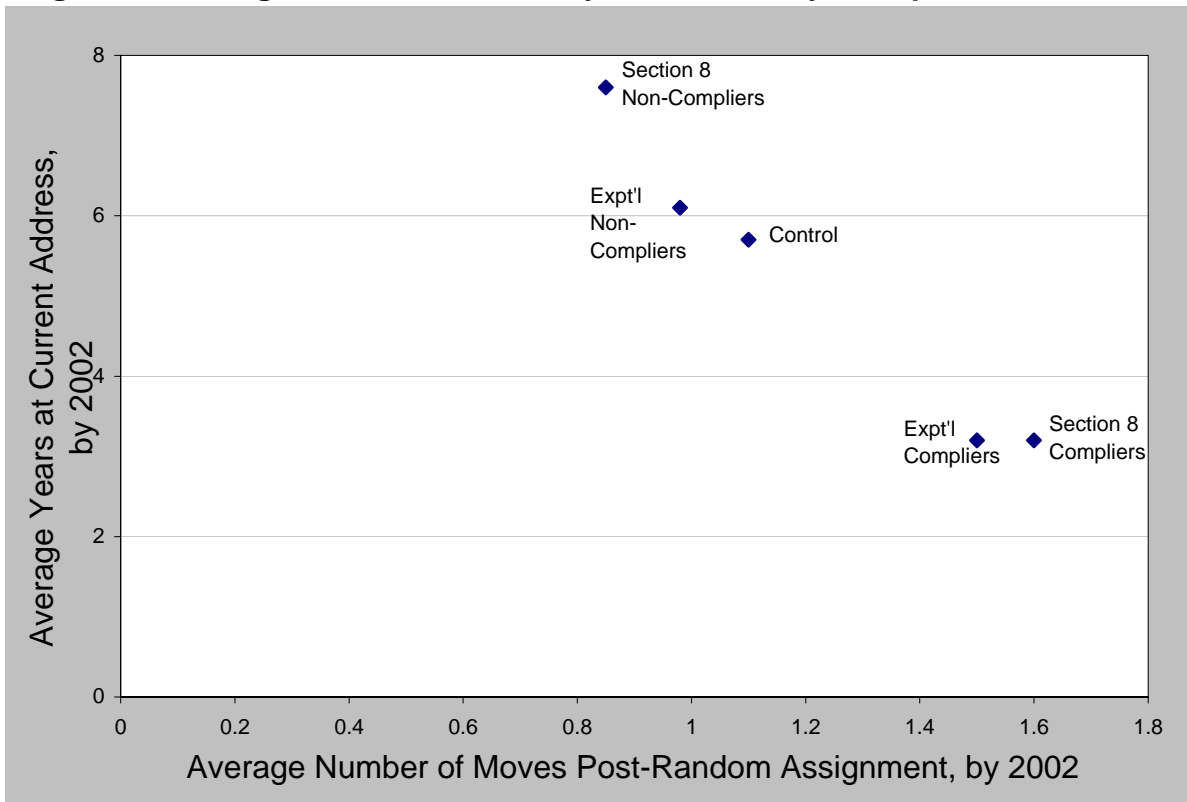
Note: Results from analysis with Section 8 and experimental groups pooled into single treatment group. Control means and impact estimates are regression-adjusted with robust cluster standard errors. Estimated equations all include site indicators and baseline covariates. Control means and impact estimates, and accompanying standard errors, in column two ("Participation in Any County") are the combined results across five multiple imputation datasets; combined results are calculated using formulas presented in King et al (2001, 53). \*p<.05

**Table 3: 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	0.595	0.666
Experimental ITT	-.032 (.019)	-.020 (.020)
Section 8 ITT	-.037 (.020)	-.038 (.020)
Experimental TOT	-.067 (.039)	-.041 (.042)
Section 8 TOT	-.060 (.033)	-.062 (.033)
Voted 2004		
Control Mean	0.389	0.432
Experimental ITT	-.041 (.018)*	-.037 (.018)*
Section 8 ITT	-.031 (.020)	-.031 (.022)
Experimental TOT	-.086 (.038)*	-.078 (.039)*
Section 8 TOT	-.051 (.032)	-.050 (.036)
Voted 2002		
Control Mean	0.210	0.245
Experimental ITT	-.038 (.015)*	-.032 (.018)
Section 8 ITT	-.022 (.016)	-.021 (.019)
Experimental TOT	-.080 (.031)*	-.067 (.038)
Section 8 TOT	-.036 (.027)	-.035 (.030)
<b>Total N</b>		<b>4219</b>
<b>Experimental Lease Up Rate</b>		<b>0.474</b>
<b>Section 8 Lease Up Rate</b>		<b>0.617</b>

Note: Control means and impact estimates are regression-adjusted with robust cluster standard errors. Estimated equations all include site indicators and baseline covariates. Control means and impact estimates, and accompanying standard errors, in column two ("Participation in Any County") are the combined results across five multiple imputation datasets; combined results are calculated using formulas presented in King et al. (2001, 53). \*p<.05

**Figure 2: Average Residential Mobility As of 2002, by Group**



Source: MTO Interim Impact Evaluation Survey, 2002

**Table 4: Select Indicators of Neighborhood Social Integration for Experimental Compliers, 2002**

Old Social Network	
Still Have Friends in Old Neighborhood	47.8%
Visit Old Neighborhood Atleast Once Per Week	18.0%
Visit Old Neighborhood Atleast Once Per Month	37.9%
New Social Network	
Have No Friends in New Neighborhood	68.7%
Chat with Neighbor Never or Only Few Times Per Year	39.7%
Average Commute to Work (if working)	32.4 minutes

Note: Sample means only for experimental voucher recipients who leased up. Source: MTO Interim Impact Evaluation Survey, 2002.

**Table A1: Estimating  $E(Y|X)$  and Treatment Effects, With Missing Outcome Data on Out-Migrants**

	[1] Manski "Worst-Case" Bounds (No assumptions)		[2] Multiple Imputation ( $Y=MAR$ )	[3] $Y=0$	[4] Listwise Delete ( $Y=MCAR$ )
	Lower Bound	Upper Bound			
	<hr/>				
<i>Outcome Measure</i>					
<b>Registered</b>					
Control Mean	0.594	0.740	0.666	0.595	0.697
ITT <sub>Pooled</sub>	-0.178	0.143	-0.027 (.017)	-0.034 (.017)	-0.016 (.017)
ITT <sub>Experimental</sub>	-0.176	0.160	-0.020 (.020)	-0.032 (.019)	-0.002 (.019)
ITT <sub>Section8</sub>	-0.182	0.120	-0.038 (.020)	-0.037 (.020)	-0.036 (.021)
<b>Vote 2004</b>					
Control Mean	0.387	0.533	0.432	0.389	0.457
ITT <sub>Pooled</sub>	-0.180	0.142	-0.035 (.017)	-0.037 (.016)	-0.030 (.018)
ITT <sub>Experimental</sub>	-0.183	0.153	-0.037 (.018)	-0.041 (.018)	-0.029 (.020)
ITT <sub>Section8</sub>	-0.175	0.126	-0.031 (.022)	-0.031 (.020)	-0.032 (.022)
<b>Vote 2002</b>					
Control Mean	0.208	0.354	0.245	0.210	0.247
ITT <sub>Pooled</sub>	-0.174	0.148	-0.028 (.016)	-0.031 (.014)	-0.030 (.016)
ITT <sub>Experimental</sub>	-0.180	0.156	-0.032 (.018)	-0.038 (.015)	-0.035 (.017)
ITT <sub>Section8</sub>	-0.165	0.136	-0.021 (.019)	-0.022 (.016)	-0.024 (.019)

Note: Table reports intent-to-treat effects for experimental, Section 8 and pooled voucher groups, under different assumptions about the missing outcome data on MTO out-migrants: [1] No assumptions about missing data; [2] Data are "missing at random" (same as Tables 2 & 3); [3] Non-registration and non-voting in baseline county among out-migrants (same as Tables 2 & 3); [4] Missing data are ignorable ("missing completely at random").

Appendix Figure 1: Lowess Curves of 2004 Turnout by Residential Mobility, With Multiple Imputation for Out-Migrants

