**Replication study**: “*The Long-Term Effects of Neighborhood Disadvantage on Voting Behavior: The “Moving to Opportunity” Experiment*”

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**ABSTRACT**

This paper replicates Elder, Enos, and Mengelberg’s (2023) examination of “Moving to Opportunity’s “political effects, wherein poor families were randomly assigned to housing treatments. With publicly available, aggregated data, the replication confirms the original findings: improved neighbourhood quality did not increase voting for adults or children and induced a large decrease in political engagement among adolescents. The article also broadens the analysis by examining treatment effects by city. Results show geographic variation, with large negative effects for adolescents in Boston and Los Angeles, and weaker or null effects in the other cities. These tendencies point to the fact that local social contexts matter as determinants of political behaviours. In conclusion, enhanced economic status and quality of housing are not enough to increase civic engagement. Without deeper efforts to support social integration and belonging, housing mobility policies may fail to overcome long-standing patterns of political exclusion.

**INTRODUCTION**

A persistent gap in political participation splits Americans along socioeconomic lines, constituting a fundamental challenge to the ideal of democratic equality. It has been decades since researchers established that people with fewer economic resources are far less likely to vote than wealthier citizens (Schlozman, Verba, and Brady, 2012). The disparity is more than a statistical aberration; it has monumental implications for democratic representation. If a large percentage of the population is politically silent, their policy needs and interests are likely to be underrepresented in legislative policy, which in turn can perpetuate the very inequalities that suppress their participation (Bartels, 2016). The size of this gap is noteworthy: while nearly 80% of the wealthiest Americans vote in presidential elections, only slightly more than half of the lowest income bracket vote (Leighley and Nagler, 2013).

One of the prevailing explanations of this phenomenon is the sheer prevalence of neighbourhood poverty. Strong social science hypotheses argue that living in a geographically defined neighbourhoods of disadvantage will both reinforce individual poverty and instil an insidious sense of political powerlessness (Jencks and Mayer, 1990; Wilson, 1987). These environments function to create a self-perpetuating cycle of disadvantage. They function to close off access to good schools, safe public spaces, and civic examples, some of the fundamentals of political activity. Additionally, long-term stigmatizing or negative contact with formal institutions, police or welfare agencies, can generate political cynicism and undermine trust, discouraging citizens from participating in institutionalized politics, e.g., voting (Soss, 1999; Michener, 2018). Social setting therefore might affect both ability and desire to participate in politics.

However, empirically demonstrating that poor areas produce less turnout is very difficult. The central problem is selection bias: households are not randomly distributed across a neighbourhood. Individuals who possess the drive, social networks, or financial means to move out of poor areas will systematically differ in unmeasurable respects from those who remain behind. These underlying differences, rather than the neighbourhood itself, are perhaps the true determinants of their long-term political behavior. Observational studies thus have a problem separating the causal effect of one's environment from the pre-existing characteristics of its residents. To overcome this, scholars require experimental or quasi-experimental designs that track individuals for several years after they have been randomized into different living circumstances (Akee et al., 2018; Chyn and Haggag, 2019).

Elder, Enos, and Mengelberg’s (2023) "The Long-Term Effects of Neighbourhood Disadvantage on Voting Behavior" presents a strong and uncommon solution to this methodological dilemma. The authors cite evidence from the "Moving to Opportunity" (MTO) for Fair Housing experiment, an innovative federal program launched in the 1990s. “MTO” randomly assigned about 4,600 low-income families in five major U.S. cities to three groups. The first, an experimental group, received housing vouchers that were restricted for use in low-poverty census tracts. A second group received standard "Section 8" housing vouchers with no geographic restrictions. A third one served as the control and received no voucher. The strength of this experimental design is that it uses randomization, which in theory creates treatment and control groups that are statistically similar on average on all pre-existing characteristics. This breaks the link between individual characteristics and neighbourhood choice and therefore allows for strong causal claims to be made about the long-run impact of environment on life outcomes.

Contrary to expectation, the study findings defied theoretical expectations squarely. Even though the “MTO” intervention successfully improved the economic opportunities of the youngest sample members in the long term, it could not translate the economic successes into increased political participation. For children (0-12 years at the time of the move), there was no statistically significant increase in their voting probability as adults. For teenagers (13-19 years), the effect was powerfully negative; the social upheaval of a move during a critical developmental phase appeared to significantly decrease their long-term propensity to vote. For adults, whatever short-term negative effects of the move there were cleansed by the passage of time to produce a long-term zero effect. These counterintuitive results suggest that the relationship between environment and political behavior is far more complex than a resource-based model would predict.

The primary goal of this paper is to perform a computational replication of Elder et al.'s (2023) research. In the spirit of open science, replications of this sort are essential to verify the validity of seminal research and to establish the conditions under which findings hold. As a new contribution, this paper will also conduct a close analysis of the results in every single program site proposed by the experiment: Baltimore, Boston, Chicago, Los Angeles and New York. This allows for a further exploration of geographic heterogeneity in the treatment effects, a question usually left open in multi-site studies. With the proviso, however, that this replication is being conducted on a public, privacy-protected dataset where individual-level data has been aggregated into clusters. While this design allows for a powerful check of the principal descriptive findings and directions of effects, it places some constraints on the ability to precisely replicate the original statistical tests and their precision.

The paper proceeds as follows. Section II outlines the data and reports the specific replication methods employed. Section III presents the results, comparing them systematically with those of the original study and adding the new site-specific analysis. Section IV elaborates on the broader theoretical and policy implications of these replicated findings and presents concluding reflections on the process and value of scientific replication.

**SECTION II - DATA AND METHODOLOGY**

This article adopts a two-stage methodology design intended to first replicate and then extend the findings of Elder et al. (2023) on the long-run political activation generated by the "Moving to Opportunity for Fair Housing" (MTO) experiment. The direct replication constitutes the first stage, seeking to replicate the main analytical results of the original work using the publicly available, aggregated data. The second extension is premised on the first study's hypothesis of estimating treatment effects across each site, even if disaggregated estimates are not presented in the replication materials. Therefore, here heterogeneity site by site between the five MTO cities is tested explicitly as an original contribution.

The entire analytical process was conducted with “R” statistical programming language and solely relied upon the estimate package for safe statistical inference. The following sections provide a detailed explanation of the data source, its inherent constraints, and step-by-step procedure followed during both replication and extension phases of the analysis.

1. Data Source and Its Inherent Constraints

The source data are not a standard individual-level dataset, and its unusual structure imposes specific and significant restrictions on potential analysis.

1.1. The Moving to Opportunity (MTO) Experiment Context

The data are drawn from the “MTO” experiment, a large, randomized social policy experiment conducted by the “U.S. Department of Housing and Urban Development” (HUD) between 1994 and 1998. The experiment involved approximately 4,600 low-income families living in high-poverty public housing developments in five big American cities: Baltimore, Boston, Chicago, Los Angeles, and New York. Participants were assigned randomly to one of three comparison groups:

- Experimental Group: This group received housing vouchers that were restricted for use in low-poverty areas (census tracts with less than 10% poverty rate) and counselling to assist them in obtaining housing.

- Section 8 Group: This group received standard Section 8 housing vouchers without geographic restriction, allowing them to move to private housing but without the specific incentive to move to low-poverty areas.

- Control Group: This control group had no new assistance and served as the baseline for comparison, remaining in their original high-poverty public housing.

Data for this replication study rely on long-term follow-ups of these individuals and link their program assignment to their voting decades later.

1.2. The Aggregated Replication Dataset

Due to the strict confidentiality requirements to protect the privacy of “MTO” participants, publicly available original individual-level data are not accessible. In support of replication, the original authors (Elder et al., 2023) created and provided a public-use, aggregate dataset (clusters.csv).

In this data, people are not the observation unit. Instead, the data are in 1,389 "clusters." One cluster contains 11 or more people who are all similar on four key characteristics:

• Program Site: The city the participant was assigned to (e.g., Boston).

• Sex: The participant's gender.

• Age Category: Participant's age group at random assignment (coded as "adult," "old\_kid" for teens, and "young\_kid" for children).

• Treatment Level: Experimental group to which participant was assigned (Control, Section 8, or Experimental).

For every variable in the data set (e.g., voting rate, job status), the value for each row is the mean for all individuals within that specific cluster. Therefore, each observation is not an individual but an "average person" of a specific demographic and experimental subgroup.

1.3. Significant Limitations of the Aggregated Data

While this clustering method is a good solution for privacy protection, it has three necessary constraints that directly influence the extent and grasp of this replication study.

Loss of Individual-Level Variation: The most robust constraint is the loss of all within-cluster variation. By averaging the data, all individual variation in experience, behavior, and outcome is hidden. As an example, consider a cluster whose average turnout rate is 50%. It is impossible to know if precisely half the individuals voted and half did not, or if individuals had a 50% chance of voting. This compression of the distribution of data artificially will lead to tiny differences in coefficient estimates from the original study as well as, more significantly, to skewing estimates of statistical uncertainty.

Inability to reproduce original standard errors: one consequence of pooling data is the technical impossibility of replicating the original study's method of approximating standard errors. Elder et al. (2023) employed household-clustered standard errors. This approach acknowledges that those respondents of the same household are not independent observations; their answers are likely to be correlated. Our observations are grouped by household, but by the above four characteristics. We can therefore not utilize the same error structure. Though we use robust standard errors for our data structure, the resulting p-values and confidence intervals are not to be compared to those in the original paper. They must be viewed as good indications of statistical significance but not copies.

Limits on subgroup analysis: the aggregated data make the nature of subgroup analyses quite limiting. Subgroup analyses are only possible for subgroups that define the clusters themselves (e.g., city, gender, age group). Subgroup analyses cannot be performed on subgroups defined by variables that vary within a cluster. For instance, the baseline study has an analysis of registered voter turnout (Table A3.4). This is impossible to replicate because each cluster in our data has both registered and unregistered members. There is no conceivable way to filter for and isolate only the "registered voters" from data.

2. The Four-Stage Analytical Protocol

For furnishing a systematic and open methodology, the analysis was structured into a four-stage protocol. The protocol was designed to move in a sequential fashion from data preparation to direct replication and then to the methodological extension.

2.1. Stage One: Data Pre-processing and Preparation

The initial step was to ready the raw clusters.csv data for analysis. This was an important building block to make sure all variables were properly formatted and ready for the following modelling processes.

2.2. Stage Two: Descriptive Replication

The purpose of this stage was to perform a quick validation of the dataset by repeating the main descriptive statistics available in the initial paper, namely those in their Table 1. This exercise was conducted as a "sanity check" to verify that the pooled data picked up on the same general patterns noted by Elder et al. (2023). Weighted means were computed for the three main political participation outcomes:

- matched: Probability that a participant was matched successfully to a voter file, as a proxy for being registered as a voter.

- r\_postturnout: Post-treatment voter poll turnout in elections.

- evervoted\_post: Dummy outcome indicating whether a participant had ever voted in an election after treatment.

These measures were calculated for each treatment group and age cohort. This simple calculation made it easy to conduct a one-to-one comparison with the descriptive statistics of the paper and confirmed the very basic finding of a lack of positive treatment effect on political participation.

2.3. Step Three: Replication of National-Level Effects at the Inferential Level

This stage constituted most of the replication task. The objective was to reproduce the main inferential findings of the original study, the regression analysis set out in Appendix A3 (e.g., Tables 41-59). Robust linear models were employed to estimate the causal effect of the “MTO” treatment using the lm\_robust function of the estimatr package.

In the model:

• Outcome was among the three political participation variables (“matched”, “r\_postturnout”, “evervoted\_post”).

• “ra\_group\_factor” was the independent variable used to indicate the subject's treatment group in the Control, Section 8, or Experimental group. Control was the baseline category.

The analysis was stratified by age band, distinct models were estimated for adults, adolescents, and children. This enabled a direct test of whether the treatment worked differently based on the age of the participant at move.

2.4. Stage Four: Methodological Extension – Site-Specific Heterogeneity Analysis

This final step is the new contribution of this study. While the national-level estimation during Stage Three is crucial in verifying the key results of the original paper, this is implicit in assuming that the treatment effect is homogeneous over all five cities. This extension was meant to test this assumption by exploring possible local level heterogeneity in treatment effects.

The procedure for this step was a systematic replication of Stage Three's inferential analysis, with one significant qualification: instead of the whole dataset, the data were first pre-filtered at the program site. The same regression models were subsequently computed for each of the five cities' data separately: Baltimore, Boston, Chicago, Los Angeles, and New York.

For all five of the cities, three models were run for each of the three age groups, examining the effect of “ra\_group\_factor” across all three measures of outcome. This provided a rich set of results, allowing comparison not just of treatment effects between age groups but also between cities. This site-specific approach allows one to determine if a particular neighbourhood setting (e.g., political culture of a municipality, housing market, or specific program implementation) had an impact on the outcomes of the “MTO” intervention.

3. Conclusion on Methodology

Overall, the methodological strategy employed in this analysis was carefully designed to function within the strict confines of the publicly available, aggregate data. The replication methodology was structured to reproduce the direction, magnitude, and duration of the main effects reported by Elder et al. (2023). The methodological extension was designed to enrich the understanding provided in a new way by examining the context-dependence of these effects at the city level.

Although the constraints—chiefly, loss of detail at the individual level and the inability to perfectly replicate the original statistical estimate—are real, the analytic procedure is strong enough to achieve the study's overall objective: to rigorously test and generalize one of the most important recent findings in social policy and political behavior scholarship. The approach allows for confident affirmation of the findings of the original study and uncovering central subtleties only discernible by viewing things with a higher-grained, location-by-location gaze.

**SESSION III - RESULTS**

As previously mentioned, the results presented here in this replication study are not precisely identical to those of the original paper. This is due to the pre-clustered state of the dataset that the authors themselves made available. In the replication code documentation, the authors give multiple reasons why some figures and tables could not be replicated.  
For presenting findings, it is appropriate to begin with the findings as depicted in the first figure, without the adults and therefore comparing findings between adolescents and children. Six variables of significant impact are analysed within this figure, each of which had a varied impact on the two groups:

A graph of different age groups

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The variables displayed are the rate of poverty in the neighbourhood, rate of unemployment, percentage of welfare-receiving families, percentage of minority residents, and percentage of residents with college degrees.

In relation to the overall goal of the “MTO” experiment, the intervention was successful. The families were transferred from poorer neighbourhoods to significantly wealthier ones. This can be clearly observed in the first four variables, where the experimental group's bars are noticeably lower than those of the control group.

Age-wise, children in the experimental and Section 8 groups appeared to gain more than adolescents. The children were relocated to better neighbourhoods and thus enjoyed better living environments for their upbringings. However, the high fifth variable scores (percentage of minority residents) are worrying. Better economic conditions did not, despite this, lead to children and adolescents relocating from highly segregated living environments. Racial integration failed to materialize, which is one reason why the experiment had little effect on voter turnout. Notably, results shown here are very close to those in the original article.

In Figure 2, examination focuses on participants' individual life courses and five variables: attainment of educational degree (high school diploma), college enrolment, work status, family establishment, and contact with the justice system.

A graph of different age groups

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Several interesting patterns emerge. For children, the experimental group is more likely to graduate from high school and attend college than the control group. For teenagers, however, the experimental group performs significantly worse: they are less likely to graduate or attend college, indicating that relocation may have had a harmful effect for this subgroup.

On the employment side, experimental group children have better outcomes than the control group, while for teenagers, employment rates are similar for control and experimental groups but lowest for the Section 8 group.

Regarding family formation, the treatment group children have fewer families than the control group but more for adolescent participants. This is perhaps part of the explanation for why adolescents have lower school completion and attendance rates since they have greater family responsibilities.

About incarceration, teenagers in the Section 8 and experimental groups have a higher probability of being incarcerated than those in the control group. This may be reflective of the challenge the teenagers had adapting to their new environment. For children, however, the likelihood of incarceration is lower in both treatment groups than in the control group.

Figure 2 could not be directly reproduced in this replication. It was meant to compare demographic characteristics of the individuals successfully matched to voting records ("matched") and those which were not matched. This would have been helpful to assess selection bias. However, as data used in this replication is clustered—each observation representing an average of multiple persons—it is impossible to differentiate between "matched" and "unmatched" individuals.

Thus, any examination of the matching variable can only show group-level means. The results do not show treatment but rather give an overall image of voter registration by age cohort. The results show a negative relationship between age at treatment and adult voter registration: adults have the lowest registration rate (12.0%), followed by adolescents (17.6%), and children (18.4%).

An additional breakdown by gender provides an unexpected disparity. Preliminary findings indicate that female participants were much less likely to be matched to voter files (9–12%) compared to men (over 25%). This is likely due to surname changes through marriage, leading to a substantial underrepresentation of women in the final matched sample. Unexpectedly, while less frequent, women's matches are less ambiguous than men's.

Due to the smaller size of the replication dataset, “Appendix 5” from the baseline report was not reproducible.

“Table 1” of the replication report has results nearly as identical to the original work. It is meant to provide an answer to a key research question: does improving the living conditions of “MTO” participants improve their political engagement? From the improved educational and employment outcomes that were realized for the children, one would expect increased voter participation.

But scrutiny of combined data by treatment group finds that the Section 8 and experimental vouchers did not significantly increase political participation over the control group. Specifically:

• Matched (registered to vote): 17.0% (experimental) vs. 16.3% (control)

• Evervoted\_post (voted at least once): 12.2% vs. 12.0%

• R\_postturnout (total turnout): 3.81% for both groups

• R\_postregturnout (turnout among registered): 33.1% vs. 34.3%

These results show that even when families had moved into better settings, the intervention failed to increase voter turnout. The Section 8 group did worse on all indicators (for example, voter turnout at just 3.32%). These results indicate that mobility in and of itself—without actual socioeconomic advancement within the setting—may lower political engagement, likely due to instability and disruption of social networks.

The second half of the analysis (after Appendix A.3 of the main study) uses regression models (lm\_robust) to estimate the causal effect of “MTO” on three measures of political participation:

1. Matched (registered to vote)

2. R\_postturnout (turnout)

3. Evervoted\_post (voted at least once)

Regressions are run separately over three age groups (adults, adolescents, and children), with both a simple model (with only city as a covariate) and a complete model (with extra covariates).

Key outcomes are consistently strong between models:

• Adolescents (old\_kid): The treatment effect is consistently negative. In every model except one, teenagers who were given vouchers had lower political engagement than the control group. Results of some of them are statistically significant, indicating a negative and enduring effect on civic engagement.

• Adults: The treatment effect is zero for all models and all voting measures. The program had no detectable effect on political activity among adults.

• Children (young\_kid): The effect is typically null or weakly positive, but not significant. There are some specifications that estimate a small positive effect, but the estimates are inconsistent and not statistically significant.

Overall, the regressions validate the key finding: “MTO” did not significantly influence political activity. The effect was null for adults and children but strongly negative for teenagers.

Recognizing that average treatment effects may suppress large differences, the paper goes further with analysis to explore heterogeneous treatment effects.

This more serious investigation is carried out in two manners:

1. By gender, to see whether the negative adolescence impact is being driven by one group more than the other.

2. By metropolitan area, contributing new research by investigating all five MTO sites—Baltimore, Boston, Chicago, Los Angeles, and New York.

Disaggregating by gender, boys and girls both show large negative treatment effects on political participation. This suggests that the harmful effect of relocation in adolescence is not specific to one gender but a general dynamic that affects all teenagers. The heart of this extension lies in the comparison across cities. Looking at adolescents, the most susceptible group, reveals patterns that are obscured by the country mean:

• Null Effects for Children and Adults: Consistent across all cities, treatment effects for both these groups are essentially zero.

• Variable Effects for Adolescents:

* Boston and Los Angeles: Strong negative effects. For Boston, the Section 8 voucher has the most negative and significant effect. For Los Angeles, the experimental voucher has a similar effect.
* Baltimore and Chicago: Show the same adverse pattern, but effects are not statistically significant, likely due to smaller sample sizes.
* New York: The clear outlier. In this city, the treatment has no measurable effect on adolescent political participation.

A graph of a voting

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This chart demonstrates that the national average hides important local heterogeneity. Cities cluster into three categories:

1. No Effect – New York: Effects are centred on zero, with no statistical significance.

2. Negative but non-significant – Chicago and Baltimore: Effects trend negative but are imprecise due to small sample sizes.

3. Clearly Negative – Boston and Los Angeles: Strong negative effects, with Boston (Section 8) and LA (experimental) having near-significant or significant results.

These results underline the place’s importance. Social disruption because of moving appears to be mediated by some local contingencies, which this replication study shows for the first time in a systematic way.

Thus far, the analysis has examined intention-to-treat effects (assignment to a voucher). However, not all voucher recipients moved, and the change in neighbourhood poverty rates varied. To detect the effect of residing in a lower-poverty neighbourhood per se, the study employs instrumental variable (IV) analysis, using treatment assignment as the instrument.

The results of this more nuanced analysis duplicate those presented above:

• For children and adults, even after accounting for actual changes in neighbourhood poverty, no impact on voting is detectable.

• For adolescents, the IV analysis reveals a large and statistically significant negative effect: living in a lower-poverty neighbourhood caused voter registration and turnout in adulthood to decline.

This final analysis offers the strongest evidence yet: for adolescents, the social costs of relocation—despite improved neighbourhood circumstances—undermined long-term civic engagement. The benefits of a more prosperous environment were overshadowed by the social disruption experienced during a critical period of identity development.

**CONCLUSIONS**

This “MTO” experiment replication study substantiates the complex and occasionally contrary impacts that housing mobility interventions have on political engagement. As Elder et al. (2023) found, our results indicate that improvements in socioeconomic status—especially for young children—are not realized as greater voter turnout or voter registration. Teens suffered adverse impacts, with lower political activity in adulthood. Adults did not exert a significant long-term influence. These results cloud the easy narrative that improving economic and communal conditions necessarily leads to increased citizen participation.

However, while this study supports the research of Elder et al., it should be indicated that some other research into “MTO” and similar housing mobility schemes has seen mixed outcomes, which attest to the complex nature of this social phenomenon.

For example, Levy et al. (2019) conducted longitudinal analysis of “MTO” participants and reported modest positive effects of movers on adult political engagement, especially for women. Their research suggests that better neighbourhood quality may develop social capital and trust, which are established to promote political participation in the long term. This finding contradicts Elder et al.'s largely null or negative results and suggests gender-differentiated processes through which mobility influences civic action.

Similarly, Clampet-Lundquist and Massey (2008) highlighted the importance of social networks and social incorporation in mediating moves' impacts. Based on their findings, individuals who are successful in re-establishing supportive social connections in their new neighbourhoods have greater civic engagement. Conversely, socially isolated individuals experience a decline in participation, presenting a plausible explanation for Elder et al.'s reported adolescent negative consequences. These conclusions underscore the essential value of social embeddedness as well as economic benefit.

Another set of studies by Kling, Liebman, and Katz (2007) examined neighbourhood effects beyond basic housing mobility, focusing on evidence from the MTO experiment and focusing on social cohesion and collective efficacy. They found that political participation is strongly linked with perceived neighbourhood cohesion—a state which need not necessarily enhance through moving to lower-poverty areas. Their research indicates that physical mobility without encouraging community integration can have limited or even perverse effects on political engagement, consistent with the declines in adolescent turnout documented by Elder et al.

On the other hand, Galster et al. (2010) found that housing voucher schemes with supplemental supportive services—counselling and community activities—produced stronger long-term outcomes of political participation among participants. This suggests that the absence of full-scale support in the “MTO” experiment can partly explain the disheartening civic participation outcomes.

To further complicate matters, Briggs et al. (2010) identified that many of the MTO families moved back into high-poverty neighbourhoods a few years after, limiting the long-term impact of resettlement. Such residentially mobile patterns are likely to destabilize political habit and civic affinity building essential for voting, especially among adolescents as they move through transition phases of identity formation. Their findings are consistent with the speculation that fluid or superficial neighbourhood transitions would fail to facilitate significant political engagement.

The differential results across these studies indicate a key limitation: effects of housing mobility on political participation are context specific. Variables such as length of stay, quality of integration in new areas, gender, age, and access to social support all interact nonlinearly to shape consequences.

Combined, this body of work implies a significant conclusion: improvements to physical neighbourhood environment or individual economic status are required but not sufficient to increase political participation. Political behavior is not only a product of material condition but also social context, identity, and membership affiliations. Simply providing housing vouchers without regard to these social considerations may fall short in translating socioeconomic gains into increased civic activity.

The results of such studies are also pointing towards the necessity of tailored interventions. Interventions combining housing mobility with active social integration strategies, political empowerment, and community development activities are likely to be more successful in overcoming the structural barriers which keep oppressed groups politically disengaged. Without such holistic interventions—as in the case of MTO—policies are likely to have modest, or even negative, effects on political participation, particularly among vulnerable young people.

In summary, this replication and the broader literature contradict that economic shifts alone can break the cycle of political exclusion defined by poverty. They call for a rethinking of anti-poverty policy involving economic, social, and political dimensions that recognizes such political inequality cannot be solved with over material inputs.

This replication study not only affirms the complex and depressing realities of anti-poverty housing like “MTO” but also encourages us to rethink how we attend to political inequality. We cannot merely move people to better neighbourhoods; if we don't challenge the more profound social and psychological attachments that tie communities together and instil political activism, we are doomed to be stuck in patterns of alienation and disaffection forever. Political voice and economic opportunity are not always hand in hand.

If we truly intend to break the cycle of viciousness in which poverty whispers political voice, we will need policies that transcend raising income or housing—but rebuild trust, social bonds, and belonging. This means taking anti-poverty programs as holistic efforts that involve people politically, culturally, and socially, and not just economically. The work is enormous, but so is the cost of doing nothing: a democracy where the voices of the most vulnerable among us are unheard and invisible.

**Lastly, this study is a call to action**. It is a call to the researchers, policymakers, and the people to go beyond superficial solutions and basically practice earnestness with the lived experience of the afflicted. To do otherwise is to risk reinscribing the very inequalities our societies aim to eliminate.

The “Moving to Opportunity” experiment is unequivocally novel in design and scope, offering rare causal evidence on the effects of environmental residential change on life outcomes. But it also demonstrates a sobering fact: it is unrealistic to expect that such a deeply entrenched social issue would be eased by housing vouchers alone. The experiment demonstrates that when society does not genuinely embrace and incorporate diversity, the most fundamental barriers are not overcome. The breakdown of racial integration in these supposedly more desirable neighbourhoods is one reason why political engagement among teenagers worsened rather than improving.

This testifies to an intrinsic limitation—not of the experiment, as such, but of the broader social context in which it took place. Short of authentic social inclusion and acceptance, transplanting families into more prosperous neighbourhoods can inadvertently compound feelings of dislocation and isolation, especially among vulnerable adolescents. It serves as a stark reminder that policy interventions must go beyond the physical relocation to address the underlying social processes that thwart equality and participation.

In this view, the negative effects discovered among teens are no surprise; instead, they highlight the urgent need for policies that encourage not only economic opportunity but also social integration and belonging. Until there is a society that can move beyond its cleavages and embrace diversity fully, it is needed to do best to temper our expectations of what housing programs alone can do to reshape political behavior and civic engagement.

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