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

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**ABSTRACT**

This article replicates the analysis by Elder, Enos and Mengelberg (2023) on the development and outcomes of the “*Moving to Opportunity”* program. Randomly divided into three groups, low-income families took part in a socio-political experiment in which, through the allocation of different vouchers, they were able to improve their economic condition. The aim was to identify, if present, a correlation between quality of life and propensity to vote. The analysis was conducted using a reduced dataset provided by the authors, in full compliance with the privacy of the individuals involved. The methodologies applied are linear regression and heterogeneity analysis through separate regressions for subgroups (age, gender, race, city). Considering the replication, the findings are consistent with the original experiment: relocation did not influence the electoral participation of the subjects in an absolute or uniform way. The article also broadens the analysis by examining treatment effects across different cities. This extension makes it possible to move beyond the simplistic and dichotomous distinction between “rich” and “poor” neighbourhoods, showing that other variables, such as geographical and socio-cultural conditions specific to the territories examined, can affect individuals’ political attitudes. The results indicate that local social contexts are important determinants of political behavior. In conclusion, the improvement of socio-economic conditions alone is not sufficient to increase civic engagement. Without deeper efforts to foster social integration and a sense of belonging, housing mobility policies risk failing to overcome persistent patterns of political exclusion.

**INTRODUCTION**

The study by Elder, Enos and Mengelberg (2023), *The Long-Term Effects of Neighbourhood Disadvantage on Voting Behavior*, offers a solid solution to a socio-cultural dilemma: what are the real factors that distance the lower classes from voting? The authors use data from the federal experimental program *Moving to Opportunity (MTO) for Fair Housing*, launched in the 1990s. The MTO randomly assigned about 4,600 low-income families in five large U.S. cities into three groups. The first, experimental, received housing vouchers to be used exclusively in low-poverty districts. The second received the standard “Section 8” vouchers without geographical restrictions. The third, control, received no voucher. The strength of this experimental design lies in randomization, which in theory creates treatment and control groups statistically similar on average with respect to all preexisting characteristics. This breaks the link between individual characteristics and neighbourhood choice, thus allowing strong causal arguments about the long-term impact of the environment on life outcomes. This experimental approach makes it possible to directly observe how housing conditions influence political participation, shedding light on a broader phenomenon: the persistent socioeconomic gap in electoral participation in the United States.

For decades, researchers have established, also through the experiment that we will discuss in the text, that people with fewer economic resources tend to vote less than wealthier citizens (Schlozman, Verba, and Brady, 2012). The disparity is not a mere statistical anomaly since it is closely linked to a problem of political underrepresentation: if a large percentage of the population remains politically silent, its needs and interests will be ignored, which in turn can be both a cause and a consequence of lower participation in voting (Bartels, 2016). The size of this gap is significant: while almost 80% of the wealthiest Americans vote in presidential elections, only just over half of the lowest-income group participates in voting (Leighley and Nagler, 2013).

One of the prevailing explanations for this phenomenon is the widespread diffusion of poverty across neighbourhoods. Solid hypotheses in the social sciences, in fact, hold that there is a correlation between individual poverty and the socioeconomic context of belonging, and furthermore, that this condition instils a pervasive sense of political powerlessness (Jencks and Mayer, 1990; Wilson, 1987). The causes are to be found in education and opportunities, since disadvantaged neighbourhoods limit access to good schools, do not offer safe public spaces, suitable and appropriate architectural structures and adequate civic models, leading to generalized political disinterest. In addition, a conflictual and distrustful relationship with formal institutions, such as the police or welfare agencies, can generate political cynicism, distancing citizens from participating in institutionalized politics, for example by voting (Soss, 1999; Michener, 2018). This leads us to consider the environment a decisive element for electoral participation.

However, empirically demonstrating that poor areas produce lower voter turnout is not simple at all. If this were the only determining factor, in fact, the experiment should show that everyone who received a housing voucher exploited it to improve their quality of life and then began to actively participate in voting. Instead, the outcome was that only 50–60% of recipients made use of the opportunity. This figure opens new points of reflection, for example, what impact motivation, social networks, or financial means have on families’ choices. These variables, rather than the neighbourhood itself, could be the true determinants of long-term political behavior. Observational studies therefore struggle to disentangle the causal effect of the environment from residents’ preexisting characteristics. To overcome this limitation, experimental designs are needed that follow individuals, after being randomized into different housing circumstances, for several years (Akee et al., 2018; Chyn and Haggag, 2019).

The analysis proposed by the authors therefore leads us to conclude how much the city itself can also influence the propensity to vote, considering five different locations: Baltimore, Boston, Chicago, Los Angeles, and New York, focusing on Baltimore. However, it would be interesting to deepen the study by analysing all of them specifically, to understand what causes different reactions in various places. This addition could open new horizons for the future, suggesting which aspects work and which do not in each context, thus fostering research aimed at creating a greater economic-political consciousness.

The text is a replication of the study by Elder, Enos and Mengelberg (2023), with the clarification, however, that it is conducted on a public dataset protected for privacy, in which individual data were aggregated into clusters. Although this design allows for a solid verification of the main descriptive results and of the direction of the effects, it imposes limits on the possibility of precisely replicating the original statistical tests and figures. Moreover, there is an extension of the study concerning the effects of the MTO experiment in the five locations proposed by the authors.

**DATA AND METHODOLOGY**

The article uses information from two different datasets: a national voter registry and the data registry of the Moving To Opportunity experiment. To protect the privacy of participants, it is not possible to make all personal information public; for this reason, NBER provided the authors with anonymized data. Moreover, to allow this replication to be conducted, the authors created a clustered dataset in which clusters consist of at least eleven individuals (cluster.csv). Consequently, the rows of the tables presented in the article show a value resulting from the average of a group of eleven. However, there are limitations that will be analysed later in the text (see 1.3).  
The limitations and characteristics reported above also come from a document provided by the authors called “Overview.DOCX.”

After an initial direct replication of the study by Elder et al. (2023), which reproduces the analytical results using public aggregated data, the article aims to deepen the analysis with an extension regarding the effects of the MTO experiment in the five locations mentioned in the original experiment. The goal is to specifically test heterogeneity across sites.

**1.1. The context of the Moving to Opportunity (MTO) experiment**  
The data come from the "MTO" experiment, a political-social experiment conducted by the "United States Department of Housing and Urban Development" (HUD) between 1994 and 1998. About 4,600 low-income families living in high-poverty contexts, in public housing complexes, in five major American cities were involved: Baltimore, Boston, Chicago, Los Angeles, and New York. To conduct this experiment, assistance vouchers were provided to participants, although, to ensure the validity of the results, not everyone received the same treatment. Individuals were randomly divided into three groups. The first, experimental, received housing vouchers limited to use in low-poverty areas (poverty rate below 10%), along with counseling to understand how to make the most of the opportunity. Another group, the Section 8 group, instead benefited from a standard voucher which, unlike the first, had no geographic restrictions. Subjects in this category had the opportunity to move to private housing, located in areas with varying poverty rates. Finally, as a baseline for comparison, the third group constitutes the control group, which did not receive any assistance and remained in the original high-poverty condition. Based on long-term follow-ups of all individuals, their assignment to the program is associated with their voting decades later.

**1.2. The aggregated replication dataset**  
The data are organized into 1,389 "clusters." Each cluster contains 11 or more people who are all similar in some key characteristics. Participants in the same cluster share the city, gender and age category (coded as "adult," 19 years and older, "old\_kid" for adolescents, 13–19 years, and "young\_kid," 0–12 years). Moreover, the type of treatment (Control, Section 8, or Experimental) must be the same.

For each variable in the dataset (e.g., voter turnout, employment status), the value for each row is the average of all individuals within that specific cluster. Therefore, each observation does not relate to a single individual but to an "average person" of a specific demographic and experimental subgroup.

**1.3. Significant limitations of the aggregated data**  
Although this grouping method is a good solution for privacy protection, it has three necessary constraints that directly affect the scope and understanding of this replication study. The most significant constraint is the loss of individual variation within the cluster. By averaging the data, all individual variation in experience, behavior and outcome is hidden. For example, considering a cluster whose average turnout rate is 50%, it is impossible to know precisely whether half of the individuals voted and the other half did not, or if the probability that each individual voted was 50%. This compression of the data distribution is the cause of small differences in coefficient estimates compared to the original study.

Another limitation is the technical impossibility of reproducing the original standard errors. These were grouped by family in the original study, to observe that individuals within the same family responses are probably correlated rather than strictly independent. Instead, in this article, observations are grouped according to the characteristics mentioned above; therefore, the structure of the errors cannot be the same. Although robust standard errors are used, the resulting p-values and confidence intervals cannot be compared to those of the original study. In some cases (see Table A0.1), p-values cannot even be calculated. Regardless, the information should not be interpreted as exact copies.

Finally, aggregated data make analyses specific within clusters unfeasible as information about the single individual is unknowable.

**2. The four-phase analytical protocol**  
To adopt a systematic and transparent methodology, the analysis was divided into four phases constituting a protocol. This was structured to proceed sequentially from data preparation to direct replication and then to methodological extension.

The first step was to prepare the raw clusters.csv data for analysis to ensure that all variables were properly formatted and ready for subsequent modelling processes. The second phase was descriptive. The aim was to replicate all possible tables, considering the restrictions in the dataset (see tables in the attached document “tables.docx”). This practice was conducted as a "consistency check" to ensure that the aggregated data captured the same general patterns noted by Elder et al. (2023). Furthermore, weighted averages were calculated for the three main political participation outcomes. These are: “matched,” i.e., the probability that a participant was successfully matched to a voter registry; “r postturnout,” which consists of post-treatment voter turnout; and finally, “evervoted post,” a fictitious outcome indicating whether a participant ever voted in any election following treatment. These measures were calculated for each treatment group and by age. This calculation allowed a one-to-one comparison with the descriptive statistics of the article and confirmed the result: the lack of a positive treatment effect on political participation.

Subsequently, in the third phase, a task of replicating national-level and inferential effects was carried out. The goal was to reproduce the main inferential results of the original study and the regression analysis shown in Appendix A3 (e.g., Tables 41–59). Robust linear models were employed to estimate the causal effect of the "MTO" treatment.

The analysis was specific and stratified by age group; distinct models were estimated for adults, adolescents and children. This allowed a direct test to verify whether the treatment worked differently depending on the participant's age at the time of relocation. Moreover, particular attention was paid to the variable of sex: as mentioned above (see 1.2), the subdivision of individuals into clusters also considered sex, which had to be the same. This variable led to a further observation, namely that men are numerically more likely to vote than women. However, as also specified by the authors, this data should not be misinterpreted. Indeed, such a difference does not result from nature or culture making men vote more than women, but from certain events over time that led to this result. For example, considering the "matched" variable, women are often not correctly matched to voter registries because, in many cases, once married, they change their last name (see Table A.2). Other possible cultural variables (e.g., social disparities) are not analysed in this paper, which only reports the numerical data.

To conclude, a further investigation beyond the authors’ original article was carried out. The fourth phase was dedicated to the inclusion in the text of a methodological extension concerning site-specific heterogeneity analysis. Although the national-level estimate during Phase Three is crucial to verify the key results of the original article, it implicitly assumes that the treatment effect is homogeneous across all five cities. This extension aims to test this hypothesis by exploring possible local heterogeneity in treatment effects.

The procedure began with a systematic replication of the inferential analysis of Phase Three but deepened by creating new site-specific datasets, using the same regression models, for each site mentioned in the program. For all five cities, three models were run for each age group, examining the treatment effect based on the explained variables above (matched, r postturnout, and evervoted). This provided a rich set of results that allowed comparison not only of treatment effects between age groups but also between cities. This site-specific approach allows determining whether a particular environment (e.g., a municipality's political culture, housing market, or program-specific implementation) impacted the "MTO" intervention outcomes.

**3. Conclusion on methodology**  
Overall, the methodological strategy employed in this analysis is carefully designed to respect the limitations imposed by the reduction of the original dataset. The replication setup aims to reproduce the direction, magnitude and duration of the effects of the experiment reported by Elder et al. (2023). The methodological extension, finally, functions to enrich the results by examining the relationship between the socio-cultural context and treatment effects. This investigation aims to understand which specific factors influence voter propensity, with the goal of proposing reflection on possible solutions to improve it. Indeed, merely stating that in one city people vote more than in another, always in relation to the treatment, does not explore the intrinsic causes of the phenomenon. The executed extension, therefore, allows the results of the original study to be affirmed with confidence, while analysing the details for each site.

**RESULTS**

The results presented in this study do not constitute an exact replication of the original paper. In the replication code documentation, the authors provide all the reasons why some figures and tables cannot be replicated, due to the reduction of the original dataset.

A first study carried out by the authors, and replicated below, concerned the effects of the treatment on some distinct variables. In this very first phase, the objective was therefore not to find a correlation with voter propensity, but rather to verify the impact of the experiment on more specific socioeconomic fronts. Essentially, this first approach serves as a basis for subsequent analyses. The production of this figure is the result of the work carried out in the first phase (see Data and Methodology 1.3).

**Figure 1.1**

A graph of different age groups

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The variables examined are: the neighbourhood poverty rate, the unemployment rate, the percentage of families receiving social assistance, the percentage of residents belonging to minorities, the percentage of families headed by a woman, and the percentage of residents with a college degree. Figure 1.1, therefore, shows the future outcomes of the indicators presented for adolescents and children, at the time of the experiment, following the random assignment to one of the experimental groups proposed by the original experiment.

As can be observed, individuals in the groups that received assistance vouchers show a clear reduction in the poverty rate in the neighbourhoods where they reside, a higher employment level compared to the control group and a higher percentage of college graduates. However, the high scores of the fifth variable (percentage of residents belonging to minorities) are in contrast. Despite the improved economic conditions, racial integration did not materialize, due to pre-existing residential segregation, as evidenced by the percentage of minorities that remains particularly high in the bars of the experimental group and the Section 8 group. Therefore, the experiment had little effect on voter turnout.

**Figure 1.2**

A graph of different age groups

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In Figure 1.2, the examination focuses on the individual life paths of the participants, analysed through five variables: obtaining a diploma (high school graduation), college enrolment, employment status, family formation, and having been in jail.

The graph shows that children in the experimental and Section 8 groups are more likely to graduate and attend college compared to those in the control group. In contrast, adolescents show significantly worse outcomes, as the likelihood of graduating or attending college is lower than in the control group, indicating that relocation to a wealthier geographic area may have had a harmful effect on them.

Regarding employment, children in the experimental group have a higher employment rate compared to those in the control group. However, the situation changes for adolescents, who show similar employment rates whether in the control or experimental group, while those in the Section 8 group even exhibit lower rates.

The fourth panel captures an additional difference between the two age groups examined, which relates to a higher propensity for forming a family at a young age among adolescents in the experimental and Section 8 groups. The percentage is decidedly lower for children. This could be explained by the fact that adolescents spend a shorter period within the school environment, which may anticipate the choice to form their own family unit.

The last section introduces the variable of incarceration, which reaches higher percentage levels for adolescents subjected to the treatment in the experimental and Section 8 groups, unlike children who exhibit behavior more aligned with civil and social rules and do not engage in illegal activities. The explanation could lie in the typical adolescent difficulties in coping with change and adapting to different realities, which involve continuous searches for new balances, already precarious due to the developmental stage the youth are experiencing.

This study cannot follow the same structure as the original article, due to the limitations already mentioned in paragraph 1.3. Nevertheless, it was possible to replicate Table A.1, provided by the authors in the published code. Within it, the correspondences between individuals and their respective inclusion in voter registries are analysed. The variable “matched” consists of whether such matching was successful. The table specifies the success percentages by age group, which are 12% for adults, 17.6% for adolescents, while the highest rate is recorded for children, with 18.4% successful matches. In Table A.2, the analysis becomes more specific as sex is also considered.

This investigation allowed the observation that, among the subjects, men, regardless of age, have a higher matching percentage compared to women. The authors, intending to explain this phenomenon, hypothesized that the cause may be attributed to the custom of women adopting their husband’s surname after marriage. This change creates a significant discrepancy that prevents successful matching.

Subsequently, further studies were conducted, the results of which are shown in the figures in the original article (Figure 2, Figure 3). However, these are not replicable, again due to the limitations imposed by the reduced dataset and the code.

To evaluate the effects of the MTO intervention on voter registration and turnout, Table 1 was considered appropriate. Within it, four variables are reported that clarify the results: whether an applicant is registered to vote (measured by being matched to the voter registry, **matched**), whether the applicant has ever voted in an election after the treatment (**evervote**), the proportion of elections in which an applicant voted in the period following random assignment (with the denominator limited to elections in which the participant was at least 18 years old, **voterate**), and the proportion of elections in which an applicant voted after being registered (limited to respondents matched in the voter registry, **votepostreg**).

**TABLE 1**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| TREATMENT GROUP | MATCHED | EVERVOTE | VOTERATE | VOTEPOSTREG |
| CONTROL | 0.163 | 0.120 | 0.0381 | 0.343 |
| EXPERIMENTAL | 0.170 | 0.122 | 0.0381 | 0.331 |
| SECTION 8 | 0.158 | 0.113 | 0.0332 | 0.315 |

Table 1 highlights result almost identical to the original study.  
The key research question is therefore the following: does improving the living conditions of MTO participants increase their propensity to vote?

The theoretical study and the consequent deduction of an increase in electoral participation among children are not confirmed by empirical observation, which, in fact, does not record a significant increase in their turnout. Therefore, the answer to the question above is negative. For example, from the analysis of the variable “votepostreg,” it emerges that the percentage of individuals in the Section 8 group is lower than that of the control group. This means that the proportion of elections in which an applicant voted after being registered is higher in the group that did not receive any assistance voucher than in the group benefiting from one of the treatments proposed by the experiment.

The present discussion, unable to use Appendix A.3 of the main study, continues by using regression models (**lm\_robust**) to estimate the causal effect of the "MTO" on three measures of political participation – **matched**, **r\_postturnout**, and **evervoted\_post** – including the age variable.

Regressions are run both with a simple model (with only the city as a covariate) and with a full model (with additional covariates). Only the results for the adolescent group deserve attention, for which the treatment effect consistently appears negative. Essentially, adolescents who received vouchers show lower political engagement compared to the control group.

Overall, the regressions validate the key result: the "MTO" did not significantly affect political activity.

To obtain a picture closer to reality, the investigation expands with additional data derived from the observation of two more variables: gender and geographic location.

Female subjects in adolescence have a reduced propensity for political participation, corroborated by a decrease in their presence in voter registries (see Tables 21, 22, 23). In contrast, males (although the dataset does not include data for the adult age group) show different behaviours: adolescents exhibit the same low electoral participation attitude as their female peers, while children show a slightly increased level of political interest (see Tables 25, 26, 27).

This suggests that the harmful effect of the treatment during adolescence is not gender-specific; it is simply a general dynamic affecting all adolescents.

For the evaluation of the impact of geographic location, attention was given to the five metropolitan areas presented in the original study: Baltimore, Boston, Chicago, Los Angeles, and New York. Even for this extension, only the results for the adolescent group deserve attention.

A graph of a voting

AI-generated content may be incorrect.

The city of Baltimore proves to be the most emblematic case of the negative effect of the program on adolescent subjects, for whom both voter registration and the probability of having ever voted decrease drastically, mainly in the experimental group.

In Boston, the negative effect is instead predominant for the Section 8 group. Similar results are observed for the cities of Chicago and Los Angeles.

New York is an exception, for which no impact of the program on political participation is recorded, likely influenced by factors different from those examined in this study. (see Tables 41/45/49/53/57).

In summary, the data from multiple cities suggest that, although the MTO program aimed to improve the lives of participants, it did not achieve the expected effect on civic participation. For adolescents, relocation to a new neighbourhood seems to have caused a disconnection from the political process, perhaps due to the loss of pre-existing social ties or the difficulty of adapting to a new environment. For adults, voting habits appear to have been too entrenched to be influenced, while for children, the lack of a clear impact may be due to the fact that they are still too young to show lasting effects. The case of New York, where no impacts are observed, reinforces the idea that the effectiveness of the program can vary greatly depending on the urban context and local social dynamics.

**CONCLUSION**

The replication study of the article concerning the “Moving To Opportunity” experiment presented here confirms the original results.  
As observed by Elder et al. (2023), improvements in socioeconomic status—especially for young children—do not lead to greater voter turnout or an increase in voter registration. Indeed, adolescents experienced negative effects, as a decrease in political participation over time is observed. On the other hand, the treatment did not influence adults in any way, thus revealing a disconnection between the environment of belonging and political engagement. Moreover, there are many pre-existing factors that can influence the propensity to vote of individuals who are already formed and have a life path behind them.  
However, to promptly dismiss this initiative would be reductive, as it must be specified that, in addition to Elder et al.’s research, other studies on “MTO” and on similar housing mobility schemes have reported mixed results, demonstrating that the nature of this social phenomenon is much more complex.  
Combining what was inferred from the results and what was stated in other studies concerning MTO, it is possible to identify some causes that led to only a partial success of the experiment. Among these, first, emerges the impossibility of tracing the reason for the lack of political participation back to a single factor, such as the poverty rate. Another hypothesis is that, to observe significant improvements in political engagement, it would have been necessary to provide participants with more substantial subsidies, thus creating a clearer break between their previous life and the one lived thanks to the vouchers. Indeed, as explained in the article “Moving in circles MIT scholar’s new book scrutinizes the successes and failures of a unique government experiment meant to help America’s urban poor,” the vouchers did not cover all expenses, forcing some families to return to their old neighbourhoods due to unsustainable costs. Moreover, civic participation in less affluent neighbourhoods is historically lower; consequently, by maintaining pre-existing relationships and social networks, an exchange of ideas aimed at change is not encouraged. Finally, for correct social integration, time and stability are necessary; thus, constant relocation does not produce a deep enough transformation to drastically influence individuals’ lifestyles.  
What, then, can we draw from this study? Do economic availability and the environment of belonging make no difference in the relationship with voting? The answer might be not directly. In fact, although it is evident from our study that the city or neighbourhood itself is not strictly connected to political life, in some more specific aspects it can have an impact. “The benefits seem to derive not so much from the lower concentration of poverty itself, but from the structural advantages of suburban areas, such as schools, public services, and job accessibility” (PNAS, analysis on MTO residential distribution over time). For example, a better economic condition within a city may result in better-functioning schools, thus raising the level of literacy and education and encouraging young people to engage in political issues.  
Furthermore, the psychological factor, not analysed within this work but in my opinion an interesting point for reflection, can greatly affect interest in political life. This is exemplified by the seventeen studies on MTO (conducted from 1996 to 2019), presented in the article “The Price of Growing Up in a Low-Income Neighbourhood: A Scoping Review of Associated Depressive Symptoms and Other Mood Disorders among Children and Adolescents,” which find, in 80% of cases, a direct correlation between missed voting and depression.  
Mental health gives the individual more propensity toward social issues and, above all, more time to devote to collective engagement. A better economic condition, in addition, could help in this respect by strengthening mental health support services.  
Finally, in recent times, social media also play a fundamental role in political propaganda. Nowadays, many political statements, new laws or updates are presented to the public directly on social media platforms. Consequently, the ability or inability to own a phone or any digital device affects political participation. Furthermore, an adequate education on the correct use and filtering of information disseminated on social media is not something everyone has.  
In conclusion, the factors influencing the propensity to vote are multiple and varied, not attributable solely to the economy. However, a repetition of the experiment with more substantial subsidies could bring further reflections to light. Beyond the extension I have presented, it also emerges that none of the cities proposed achieved success in this experiment; consequently, the solution cannot be sought if the study of the problem is reduced to merely relocating families.  
As Briggs and his colleagues admit, a single tool to fight poverty is not enough. In the future, it will be necessary to “make relocation part of a broader recipe for progress — to escape poverty.”  
Ultimately, the Moving to Opportunity experiment attempted to investigate a correlation between the poverty rate and the propensity to vote. More precisely, it reflected on the actual probability that improving the economic conditions of the participants in the social experiment could also increase voter turnout. The results of the study showed that there is no direct influence between the voting rate and the economic condition of the subjects; therefore, the intervention carried out was not useless, but, probably, to increase political participation, it would be necessary to intervene on several fronts that do not exclusively embrace the economic sphere.

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