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The Long-Term Effects of Neighborhood Disadvantage on Voting Behavior: The "Moving to Opportunity" Experiment

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Socioeconomic disadvantage is a major correlate of low political participation. This association is among the most robust findings in political science. However, it is based largely on observational data. The causal effects of early-life disadvantage in particular are even less understood, because long-term data on the political consequences of randomized early-life anti-poverty interventions is nearly nonexistent. We leverage the Moving to Opportunity (MTO) experiment to test the long-term effect of moving out of disadvantaged neighborhoods—and thus out of deep poverty—on turnout. MTO is one of the most ambitious anti-poverty experiments ever implemented in the United States. Although MTO ameliorated children's poverty long term, we find that, contrary to expectations, the intervention did not increase children's likelihood of voting later in life. Additional tests show the program did not ameliorate their poverty enough to affect turnout. These findings speak to the complex relationship between neighborhood disadvantage and low political participation.

INTRODUCTION

Americans living in poverty are far less likely to participate in the political process than those with more resources (Schlozman, Verba, and Brady 2012). Approximately half of American adults in the lowest income quintile vote in presidential elections, compared with nearly 80% of Americans in the highest quintile (Leighley and Nagler 2013, 29). People with low incomes tend to support policies that alleviate financial hardship, but these preferences receive little representation, partly because poor people are less likely to vote (Bartels 2016; Carnes 2013; Griffin and Newman 2012; Hill and Leighley 1992; Leighley and Nagler 2013).

Neighborhood poverty is an important explanation for the association between poverty and participation (Cohen and Dawson 1993). An influential theory in social science argues that places with concentrated poverty worsen individual poverty (Jencks and Mayer 1990; Sampson, Morenoff, and Gannon-Rowley 2002; Wilson 1987). Similarly, concentrated poverty may worsen political disempowerment (Alex-Assensoh 1997; 1998; Burch 2013; Cohen and Dawson 1993; Gay 2012; Gimpel, Lay, and Schuknecht 2003; Lerman and Weaver 2014; Michener 2013; Oliver and Mendelberg 2000). High-poverty neighborhoods provide less access to employment, education, civic organizations, and social ties, and decrease efficacy. These experiences may reinforce poverty and reduce political participation. Conversely, moving to places with less poverty may alleviate individual poverty and increase political participation (Gay 2012, 148; Ross, Mirowsky, and Pribesh 2001).

Neighborhood poverty—and interventions against it—may especially affect people early in life (Chetty, Hendren, and Katz 2016). According to theories of human development, preadolescence is a particularly malleable developmental stage (Heckman 2006; Holbein 2017; Holbein and Hillygus 2020; Sampson 2008). Thus, socioeconomic contexts, and interventions in them, have especially lasting effects if experienced before young adulthood. Early-life poverty is also negatively associated with political participation (Akee et al. 2018; Chyn and Haggag 2019). For example, even accounting for current income, adults who grew up in poor homes are less likely to vote (Ojeda 2018). These findings align with theories emphasizing the importance of early experiences for political behavior (Brown et al. 2021; Campbell 2008; Jennings, Stoker, and Bowers 2009; Holbein and Hillygus 2020; Prior 2019).

However, it is unclear if poor neighborhoods cause lower participation or are merely correlated with it. Identifying the long-term, early-life effects of neighborhood poverty requires randomly (or as-if randomly) assigning individuals to high- and low-poverty neighborhoods, and measuring their participation decades later.

This article reports such a study. We evaluate the causal impact of a program that allowed people to move to a low-poverty neighborhood—at different points in

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the lifespan—on political participation. We merge voter records with data from Moving to Opportunity (MTO), a large federal initiative in five large cities across the country that randomly distributed housing vouchers among approximately 4,600 poor families in public housing (approximately 16,000 individuals). The low-poverty voucher condition specifically instructed the recipients to move to low-poverty neighborhoods, with the hope of improving social and economic outcomes. Unfortunately, the low-poverty vouchers did not alleviate the poverty of the adult and teen participants. In fact, they worsened some life outcomes for teens. However, the low-poverty vouchers did ameliorate *preadolescents'* socioeconomic disadvantage (Chetty, Hendren, and Katz 2016). We investigate the long-term effects of this intervention on political participation.

Identifying the effect of poverty on political participation is important because it illuminates the relationship between poverty, voice, and the systematic underrepresentation of the interests of the poor in the United States (Carnes 2013; Verba, Schlozman, and Brady 1995). In an important contribution to this effort, Gay (2012) investigated the initial effect of the MTO intervention on turnout for adult participants. Perhaps because the program did not reduce adults' poverty—and thus, failed to improve the antecedents of political participation—it did not increase their political participation. Indeed, adult voucher recipients were *less likely to* vote, possibly because of the disruption of moving. However, Gay's study only had data collected soon after the disruption of moving, and only from adult participants. Thus, the long-term and early-life effects of the intervention—and of neighborhood poverty—remain unknown.

We are now in a position to substantially advance this literature, in three ways. Most crucially, we can investigate the effects on children. Given the other positive impacts on preadolescents, we expected downstream positive effects on their turnout. Second, we can test the developmental stage hypothesis by comparing the effect of the intervention on teens and preadolescents. Third, with more than 20 years elapsed, we can re-assess the negative effect on adult participants, using turnout data from elections far removed from the disruption of the move.

Contrary to our expectations for the preadolescents, the intervention did not increase their turnout. In addition, as predicted, we find no positive effect for adult or teen participants. Adults are unaffected by the treatment, while teens participate *less*, consistent with their respective null and negative effects on other life outcomes.

The results for children are surprising, given the positive effects of the intervention on the antecedents of political participation, including education and income. However, as we discuss, the effects on these antecedents, while statistically significant, may be too small to increase voting. Additionally, participants often did not stay in low-poverty neighborhoods long enough to integrate into their social networks (Briggs et al. 2008), so they may not have been exposed to norms or resources that encourage participation. Thus, the vouchers may not have sufficiently increased the exposure to—and participatory benefits of—low-poverty neighborhoods.

To our knowledge, this study is the first to randomly offer a poverty-targeting intervention to families[1] and measure its long-term effect on the political behavior of participants of different ages. The participants were sampled from five varied, large cities around the United States, and the intervention is a significant federal policy. This study thus allows a better claim to causal identification, as well as external validity, than has been possible to date. Our finding of a null effect, then, is a meaningful contribution to the literature on the role of early experiences of poverty in shaping political behavior. In the conclusion, we discuss scope conditions for generalizing the results to other forms of political activity and other populations, and for designing more effective interventions. The negative effects of housing vouchers on teens invite closer scrutiny of the broader impacts of this mainstay of US housing policy.

POVERTY AND POLITICAL PARTICIPATION

Living in a poor neighborhood is strongly associated with lower political participation. People who live in neighborhoods with high poverty rates engage less in civic life, including voting (Casciano 2007; Cohen and Dawson 1993; Giles and Dantico 1982; Kelleher and Lowery 2004; Levine et al. 2017; Stoll 2001). Why might neighborhood disadvantage affect individual political participation? The literature offers several possible mechanisms. As we detail below, one set of mechanisms works through the personal experience of poverty, while another has a direct impact through features of the neighborhood (Alex-Assensoh 1997; 1998; Cohen and Dawson 1993).[2]

First, neighborhoods can affect individual-level traits, which in turn affect political participation. Neighborhood disadvantage early in the lifespan affects later educational attainment (Ellen and Turner 1997), income (Chetty and Hendren 2018; Chetty, Hendren, and Katz 2016), and marital and single-parent status (Chetty, Hendren, and Katz 2016). Each of these variables is associated with political participation (Alex-Assensoh 1998; Lawless and Fox 2001; Leighley and Nagler 2013; Lerman and Weaver 2014; Smets and van Ham 2013; Stoker and Jennings 1995; Verba,

[1] About half of the families offered the housing vouchers accepted them; because the choice to accept the vouchers is not random, our analyses focus on the variation created by the randomized offer.

[2] Even when accounting for individual poverty, the percentage of poor residents in one's neighborhood depresses individual political efficacy, engagement, and action (Cohen and Dawson 1993). And even non-impoverished adults living in poor neighborhoods are less likely to vote than comparable individuals who live in more affluent neighborhoods (Alex-Assensoh 1997; 1998). These studies are consistent with influential theories arguing that social context influences political behavior above and beyond individual characteristics (Huckfeldt 1986; Oliver 1999; Cho, Gimpel, and Dyck 2006; Enos 2017).

Schlozman, and Brady 1995; Wolfinger and Wolfinger 2008). Poor people are also more likely to have negative interactions with the government in daily life, and these interactions have consequences for their engagement with politics. For example, stigmatized means-tested financial assistance reduces political engagement (Johnson, Meier, and Carroll 2017; Michener 2018; Soss 1999), while socially valued forms of assistance may increase it (Mettler 2005; Mettler and Soss 2004). Thus, people in better-off neighborhoods may participate more in politics because they possess individual-level material and psychological resources that encourage political engagement.

Second, concentrated poverty may create a social context that discourages political participation (Alex-Assensoh 1998; Cohen and Dawson 1993; Gay 2012; Gimpel et al. 2003). Disadvantaged places offer fewer opportunities to join civic organizations or social networks that disseminate civic information and mobilize residents to political action (Gimpel, Lay, and Schuknecht 2003; Marschall 2004). Membership in these organizations is especially consequential for the young (Campbell 2008). Additionally, residents of disadvantaged neighborhoods are less likely to believe that problems in their community are solvable and that the government is open to their input (Cohen and Dawson 1993). This sense of efficacy is highly correlated with voting, particularly for African American youth (Cohen 2010; Gimpel, Lay, and Schuknecht 2003). Moreover, for people of color, living in a disadvantaged neighborhood can increase perceptions of racial discrimination, which may depress turnout for young African Americans (Cohen 2010; Gimpel, Lay, and Schuknecht 2003). Relatedly, disadvantaged neighborhoods are more likely to experience harsh policing and punitive contact with the criminal justice system, which may further decrease participation (Lerman and Weaver 2014; McDonough, Enamorado, and Mendelberg 2022; White 2019). In addition, physical features of high-poverty neighborhoods may decrease participation: litter, graffiti, and dilapidated buildings, all more common in disadvantaged neighborhoods, are associated with decreased safety (Branas et al. 2018), which may weaken social ties and political trust (Michener 2013).[3] This body of prior work thus gives us several reasons to expect that concentrated poverty decreases political participation.

The evidence from experimental and quasi-experimental studies of anti-poverty interventions suggests that intervening early matters for the long-term reduction of both poverty and the associated consequences. Chetty, Hendren, and Katz (2016) find positive effects of housing vouchers on poor children's socioeconomic status, but not on teens or adults (as we elaborate below). Bastian and Michelmore (2018) show that increases in family earnings through the Earned Income Tax Credit improve children's later educational attainment and income. Goodman-Bacon (2021) shows that poor children's receipt of Medicaid coverage powerfully improved their health and well-being in adulthood. García, Heckman, and Ronda (2021) show that a preschool program for poor youth improved cognitive and economic outcomes for participants and their children. These studies suggest that interventions which improve disadvantaged children's access to economic and educational opportunities at a young age can improve their well-being decades later.

Not only can early-life interventions on poverty alleviate poverty later in life, but early-life interventions targeting the correlates of poverty can also increase adult political participation. Gill et al. (2020) find that attending Democracy Prep charter middle schools increases voter turnout, and Holbein (2017) finds that a school program to increase disadvantaged children's psychosocial skills increases their participation later in life. Akee et al. find that positive income shocks from a Native American casino increase participation only among poor children (2018). Moreover, a natural experiment in Chicago found that children who were moved to lower-poverty neighborhoods after the demolition of their public housing voted at higher rates in adulthood (Chyn and Haggag 2019). These studies echo the conclusion from studies of political socialization that early-life experiences matter to adult political behavior (Greenstein 1965).

These experimental or quasi-experimental studies, though important, have not examined the long-term effect of a randomized anti-poverty program on people at different developmental stages. In addition, we are not aware of long-term experimental studies of neighborhood poverty at different developmental stages. Absent such interventions, most studies of neighborhood context are hampered by the intractable problem of selection bias. People who live in a poor neighborhood differ systematically from those who do not, on many variables that predict participation. These differences make it difficult to identify the effects of the neighborhood. Thus, researchers must make strong and often untestable assumptions to conclude that neighborhood context is the cause of the difference. Random assignment of people's likelihood of living in higher- and lower-poverty neighborhoods solves the problem of selection. It ensures that people living in different kinds of neighborhoods are, in expectation, similar in all other characteristics. One practical, scalable way to do this is to study individuals who seek to move to better neighborhoods and randomly assign some of them the ability to do so. Any variation in neighborhood poverty induced by this random assignment does not lead to bias, allowing scholars to study the effects of poverty reduction on downstream outcomes, including political participation.

MOVING TO OPPORTUNITY AND ANTECEDENTS OF VOTER TURNOUT

The Moving to Opportunity for Fair Housing Demonstration Program (MTO) was administered by the U.S. Department of Housing and Urban Development

[3] See Burch (2013) on similar effects of the prevalence of former felons in a neighborhood.

(HUD) in five large cities from 1994 to 1998 (Sanbonmatsu et al. 2011). The program randomly assigned 4,604 low-income households with children, living in public housing in high-poverty neighborhoods in Baltimore, Boston, Chicago, Los Angeles, or New York City, to receive vouchers for use in the private housing market. The intent was to give families living in highly distressed circumstances the ability to live in lower-poverty, less disadvantaged neighborhoods and improve their life chances. It is one of the most ambitious anti-poverty experiments undertaken in the United States and the single largest randomized study of the impact of moving people to less distressed environments. It has been the focus of many prominent studies in the social sciences (e.g., Chetty, Hendren, and Katz 2016; Gay 2012; Kling, Liebman, and Katz 2007; Ludwig et al. 2012; Sanbonmatsu et al. 2011).

Households were randomly assigned to one of three treatment conditions: 1) receive a housing voucher that could only be used in a Census tract with less than 10% poverty[4] (low-poverty or experimental voucher condition); 2) receive a traditional Section 8 housing voucher (Section 8 condition); or 3) receive no voucher (with the ability to remain in public housing) (control condition). Section 8 vouchers are one of the primary forms of rental assistance for low-income people in the United States. Receivers of Section 8 vouchers can rent homes from private landlords, and rental assistance will pay at least part of the cost. The experimental low-poverty treatment was intended to address the concern that beneficiaries of traditional Section 8 vouchers often remained in areas of concentrated poverty, which limit economic opportunity (Orr et al. 2003); vouchers requiring participants to move away from these high-poverty neighborhoods were expected to improve their socioeconomic outcomes more than traditional vouchers.

We obtained data from the MTO study from the National Bureau of Economic Research (NBER). These data include administrative records of individuals' treatment assignment and compliance. MTO participants were also administered several surveys before and after random assignment, including a baseline survey at the time of treatment (1994–1998), an interim evaluation survey (2002), and a final evaluation survey (2008–2010), which contain individual- and household-level data for adults and youth, alike. To protect participant confidentiality after merging with the voter file, we were not allowed access to all outcomes collected in these surveys, but we did obtain a variety of pre- and post-treatment outcomes that could mediate the connection between assignment to a voucher and political participation. The data also include characteristics of the neighborhoods in which participants lived, including the neighborhoods' economic and racial composition.[5] The effects of the experiment on poverty and other nonpolitical outcomes were measured in the post-treatment surveys.[6]

The full dataset from NBER contains 24,590 participants, which represents every participant and everyone living in their household at the baseline or any subsequent survey. We restrict our analysis to participants who were present in the households at the time of randomization and therefore were randomly assigned to a treatment group. This includes 4,604 adult heads of household and 11,300 youth (i.e. children, n = 8,512 [age 0–12], and teens [age 13–19], n = 2,758, at the time of random assignment). Because the original study targeted female-headed households, the adult participants are 98% female. The youth are evenly split between males and females. The sample is composed predominantly of people of color, with 63% Black participants, 30% Hispanic, 3% non-Hispanic white, and 4% of another race[7].

Households in both voucher groups complied in reasonably high numbers. Specifically, 47% of the low-poverty voucher households took up the offered housing vouchers, and 62% of the Section 8 households did so (Orr et al. 2003, 26).[8] As in any experiment, the decision to comply with one's treatment assignment is not random, so we focus here on the effects of the randomly assigned voucher rather than the nonrandom decision to use the voucher to move to a new neighborhood[9]. For those families which did comply, the MTO intervention changed participants' neighborhood context, and for some, life outcomes also improved. Figure 1 uses administrative and survey data from MTO participants to show these effects on neighborhood characteristics[10] (top row) and individual nonpolitical outcomes (bottom row), for participants who were children and teens at the time of the intervention.[11]

As the top row shows, the offer of vouchers succeeded in moving participants to lower-poverty

[4] This treatment required participants to remain in these low-poverty neighborhoods for at least one year after.

[5] These data were compiled by NBER using the 1990 and 2000 decennial Censuses and the 2005–2009 American Community Survey to capture characteristics of the Census tracts in which participants lived between the time of random assignment and 2008.

[6] See Orr et al. (2003) for design details.

[7] For details on the data and analysis, see the replication files in the APSR Dataverse (Elder, Enos, and Mendelberg 2023).

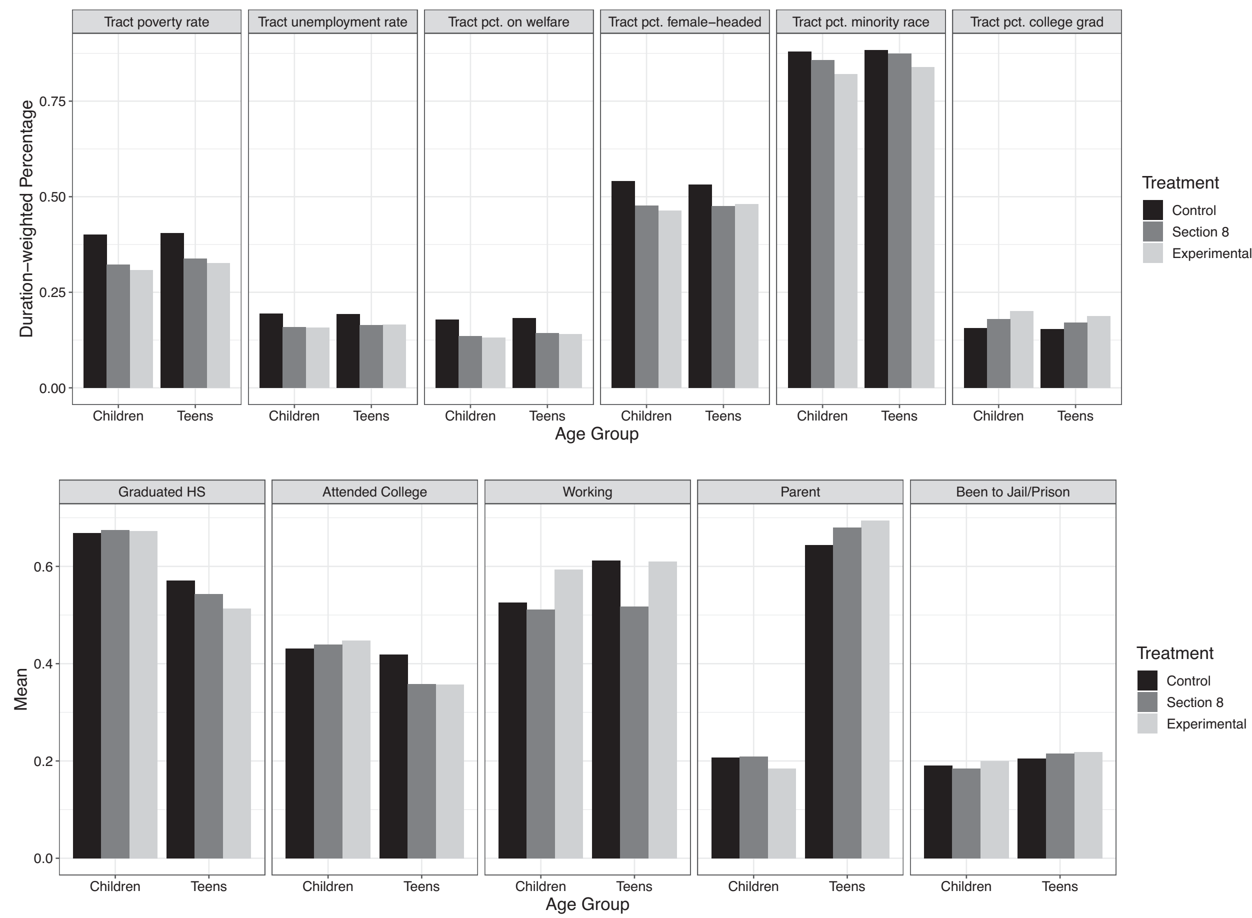
[8] Nationally, about 70% of people offered Section 8 vouchers during this period accepted and used them (Orr et al. 2003, i). The uptake rate for Section 8 vouchers in this study is 62%. This is a difference of only 8 percentage points. That relatively minor difference could simply be explained by tighter housing markets in the sample cities than nationally. Compared to Section 8 vouchers, the experimental vouchers require moves to a specific set of neighborhoods that may be harder to move to. For example, affordable housing may be more scarce, and the residents may be culturally unfamiliar. For the experimental vouchers, an uptake rate about 20 percentage points lower is not unreasonable.

[9] That is, our tables and figures present differences between all subjects who were assigned to each treatment group, without limiting to subjects that complied with their treatment assignment.

[10] Neighborhood characteristics are measured using a duration-weighted average of the characteristics of the Census tracts in which participants lived between the time of randomization and the time of the final survey in 2008.

[11] Figure 1 presents treatment group means, weighted by values provided by the original investigators to account for uneven probability of assignment to treatment. Further details on the data can be found in the "Data and Methods" section below.

FIGURE 1. Outcome Means for Children and Teens in Each Treatment Group



Note: The top row shows neighborhood characteristics, measured for each participant by the average outcome for each Census tract in which they lived from the time of randomization to 2008, weighted by their length of residence. The bottom row shows individual-level outcomes. Means and significance tests are presented in table form in Section A0 of the Supplementary material.

neighborhoods. Both the Section 8 and the low-poverty vouchers decreased neighborhood poverty for teens and children, but the low-poverty vouchers were especially effective. Specifically, after receiving vouchers, children and teens in the low-poverty group lived in tracts with 32–33% poverty, about 8 percentage points lower than children in the control group (40% poverty). Along with that reduction in concentrated poverty came a reduction in concentrated socioeconomic distress: lower rates of unemployment, welfare use, female-headed households, and racial segregation, and higher college graduate rates. Taken together, these changes in neighborhood characteristics could encourage participation.

Furthermore, as expected, the treatment reduced individual poverty and improved some associated outcomes—but only for children. The bottom panel of Figure 1 shows the effects on several measures, all of which are possible antecedents of voter turnout: education, labor outcomes, parenthood, and contact with the criminal justice system. The teens in the treatment conditions fared worse than those in the control condition on three key outcomes: as the first, second, and fourth plots in the bottom panel of Figure 1 show, treated teens graduated high school and attended college at lower rates, and were more likely to have children at a young age. Likewise, Chetty, Hendren, and Katz (2016)'s tax data show that the treatment decreased college attendance and perhaps earnings for teens. These results suggest the intervention harmed teens' later socioeconomic outcomes, giving us reason to doubt the treatment will increase their participation. These findings are consistent with the expectation that the low-poverty intervention would be less effective for adolescents than for younger children. (As noted, the vouchers also failed to improve the economic, educational, or social outcomes of the adult participants.[12])

[12] The treatment did improve adults' health. Full findings on these effects on adults are in the primary reports on MTO, including (Chetty, Hendren, and Katz 2016; Kling, Liebman, and Katz 2007; Ludwig et al. 2012; Sanbonmatsu et al. 2011).

By contrast, the effects on children at random assignment are more positive, as expected. The strongest effect is on employment: as the bottom panel shows, the experimental vouchers increased the likelihood of working. Chetty, Hendren, and Katz (2016) further show that experimental vouchers increased children's earnings by about 14%. The experimental vouchers also ameliorated other aspects of the children's poverty in adulthood, though only slightly and inconsistently: they had small positive effects on college education (Figure 1 and Chetty, Hendren, and Katz 2016), marriage rates (Chetty, Hendren, and Katz 2016), and delayed parenthood (Figure 1), but not on carceral contact (Figure 1).[13] (See Supplementary material Section A0 for significance tests).

Because of these positive effects on children, we expect their turnout and registration to also be positively affected. However, we note that these effects on antecedents of political participation were not large. That may be because the decrease in neighborhood poverty was modest, even among the half of the treatment group who actually used the experimental vouchers, and especially over the long term. These participants lived in neighborhoods that averaged 20% poverty over the 10–15 years after treatment. While that level of poverty is lower than the control group, it is nevertheless higher than the <10% poverty threshold they were required to select initially. This still placed them in the top quintile of concentrated poverty in the US.[14] In other words, these contexts improved in degree, not in kind (see also Sampson 2008).

To summarize, the effect of the experimental vouchers on antecedents of voter turnout depends on the participants' age. Though moving with experimental vouchers decreased the neighborhood poverty level of all participants, the benefits of that move were heavily conditioned by developmental stage. For adult participants, the experimental treatment did not increase income, education, or other participatory antecedents. For teen participants, the treatment *decreased* income and education, and had mixed results on other antecedents. Finally, for children, the treatment decreased important elements of adult poverty, by increasing education, employment, income, and relationship stability. Given these increases in participatory antecedents, we expect the intervention to increase children's turnout in adulthood. However, the modest effects on neighborhood and individual poverty even for children underscore an important point for analyses that follow: even the largest positive effects on these antecedents may not be enough to affect turnout. We will elaborate on this point in the results section.

MERGING MTO AND TURNOUT DATA

To study the effects of neighborhood poverty on political participation, we compare turnout and voter registration rates among three groups: the experimental group, the Section 8 group, and the control group. Though we present results for all three treatment conditions, we focus on the experimental (low-poverty) condition, as we have the strongest reason to expect this condition to increase voter turnout and its antecedents. That said, the Section 8 condition represents a large and important component of anti-poverty programming in the US, so our findings for that treatment also have notable implications. Prior to receiving the data, we preregistered that hypothesis, along with the others tested here.[15]

We combine comprehensive national voter file data from 2016 with MTO data drawn from participant surveys and applications. To measure voter registration and turnout, we use data from L2, a commercial vendor of publicly available voter files. L2's voter file data include each voter's name, address, age, gender, and whether they voted in recent elections.[16]

These data allow us to extend and improve on Gay's 2012 initial analysis of MTO's effects in a number of ways. Gay analyzed turnout in two elections (2002 and 2004) only a few years after random assignment. As Gay recognized, this short window made it difficult to disentangle the positive effect of improved life chances from the negative effect of moving, which may explain the negative treatment effect Gay observed. In addition, with only one presidential election in the analysis, it is difficult to generalize across elections. Gay's analysis also only considered those who remained in the counties in which they were living at the time of random assignment. Anyone moving out of the county (say from the Bronx to Queens) would be missing from her sample. Given that the treatment encouraged movement by design, this is an important limitation, and one alleviated by the availability of a national voter file for our analysis. Additionally, because the match was performed by an external firm, the quality of the match could not be validated; by contrast, we were able to design our own matching procedure and can evaluate the quality of our match. Finally, and most importantly, Gay's analysis was limited to participants who

[13] Though Chetty, Hendren, and Katz find a significant effect of the intervention on college attendance, while the MTO survey does not, the point estimates are quite similar: 2.5 and 2.2 percentage points. Only the former is statistically significant, because its standard error is much smaller, likely from the larger sample (with four additional years of data) or the greater precision of administrative measures. These administrative tax data may be more accurate than the MTO survey, which measured children's outcomes too early and with more error. For more on the children's outcomes, see Ludwig et al. (2012).

[14] In 2000, 82% of Americans lived in lower-poverty neighborhoods than the average neighborhood of compliers in the low-poverty group (Bishaw 2014).

[15] Our expectations and methods were pre-registered at https://osf.io/fjdn4/?view_only=9e43416f7f5a4c21bedd3debc8701954. The pre-analysis plan, and departures from the plan (which are immaterial), can be found there.

[16] Because people only appear in the voter file if they are or were recently registered to vote, we can also measure voter registration, conditional on successfully matching participants to the voter file. Because of states' routine voter file maintenance, inactive registrants are cleared from these records regularly. We therefore cannot capture people who are no longer registered to vote.

were adults at random assignment. The years elapsed since then allow us to measure participation among participants who were children at the time of the intervention, the population which experienced the largest increases in the antecedents of participation.

We match the MTO data to the voter file using participants' genders, names, and birthdates from the baseline survey conducted just prior to treatment assignment in 1994–1998[17]. We attempt to match all individuals included in the initial study to the voter file, including those who were youth at the time of the intervention. To link the MTO and voter records, we first stratified the sample and voter file by gender and then used the fuzzy-matching algorithm fastLink (see the fastLink package in R; Enamorado, Fifield, and Imai 2018). The algorithm compares the gender, name (first, middle, last, and suffix), and birthday (date, month, and year) of each pair of observations in the MTO and voter file data and calculates the likelihood the pair of observations are a match. The algorithm can return many possible matches; we keep only matches assigned a posterior likelihood of 0.75 likelihood or higher, and the mean posterior of the used matches is 0.95.[18]

We found matches in the voter file for 16.4% of MTO participants. This includes a 12% match rate for adults (n = 551), 17% for teens (n = 474), and 18% for children (n = 1,518). A person cannot be matched if they are not registered to vote, and a person can be registered to vote but fail to be matched if there are sufficient differences in their name or birthdate between the MTO data and voter file. In the adult sample, 98% are women, and in our match process, women were matched to the voter file at a lower rate than men, perhaps because of name changes from marriage.[19] Some participants, especially adults, are also more likely to have died, leaving us unable to locate them in the voter file.

When evaluating the quality of this match, it is important to keep in mind that a person can only be matched if they were registered to vote; the denominator for the match rate, then, is the proportion of the experimental population that was registered. For example, if 50% of the population is registered to vote, the match rate is 16.4/50% = 32.8% of possible matches were found. We estimate that 45% of citizens[20] with similar demographics to the MTO sample are registered to vote.[21] Using a 45% expected registration rate as the baseline implies that we successfully matched 36% of registered voters.

It is useful to compare this 36% rate of matching registered voters to other matches using the voter file. Gay (2012) reports a 57% match rate across a shorter time period, so mortality and name changes may have been less likely to disrupt the matching process, but the matching process was done by a third-party firm, so the quality of the match is not reported and one cannot account for the potential for false positives or negatives. Brown et al. (2021) match living men on voter files with Census records over a seven-decade period and obtain approximately a 50% match rate while using the information on place of birth as well as name and birthdate. That a process using additional information and conducted on individuals known to be living and unlikely to have a name change obtained a match rate of 50% suggests that our implied match rate of 36% is of similar quality as other published research. Nevertheless, as with all research involving matching, we should assume that we failed to match an unknown portion of participants who were actually registered.

Most importantly, we must consider potential biases in our match process. The match process could introduce bias into our analyses if an individual's likelihood of matching—conditional on their presence in the voter file—is correlated with their treatment assignment or their response to treatment. For example, if people who are harder to match are also more strongly affected by treatment, our treatment estimates will be attenuated. We therefore conduct two analyses to diagnose the quality of the matching process.

First, we compare the demographics of the matched and unmatched samples. Differences in observable characteristics between matched and unmatched samples can provide suggestive evidence about whether we are less able to match the kinds of people who would be more or less affected by the treatment. Figure 2 presents the averages for the matched and unmatched respondents on a variety of relevant variables. One large difference stands out: women comprise about 70% of the unmatched sample but only 40% of the matched sample.[22] On all other variables, however, the matched and unmatched respondents have similar means. A regression of match rate on the demographic variables in Figure 2 excluding gender has an F statistic of 4.7, suggesting limited joint predictive value, while a regression including gender has an F statistic of 55.6.

[17] We do not use any geographic information about participants in the match; instead, each participant is considered a potential match for every record in the national voter file. Incorporating geographic information on participants could risk biasing our estimates if the intervention increased moving, making a match to pre-treatment location less likely.

[18] We chose the threshold of 0.75 to allow a more exhaustive list of matches while remaining in the range Enamorado, Fifield, and Imai (2018) found to provide similar results to a more-restrictive threshold in a similar application. We also excluded matches that linked people to voter records showing them voting in elections before they turned 18. For analyses incorporating all possible matches and posteriors, see supplementary material section A4.

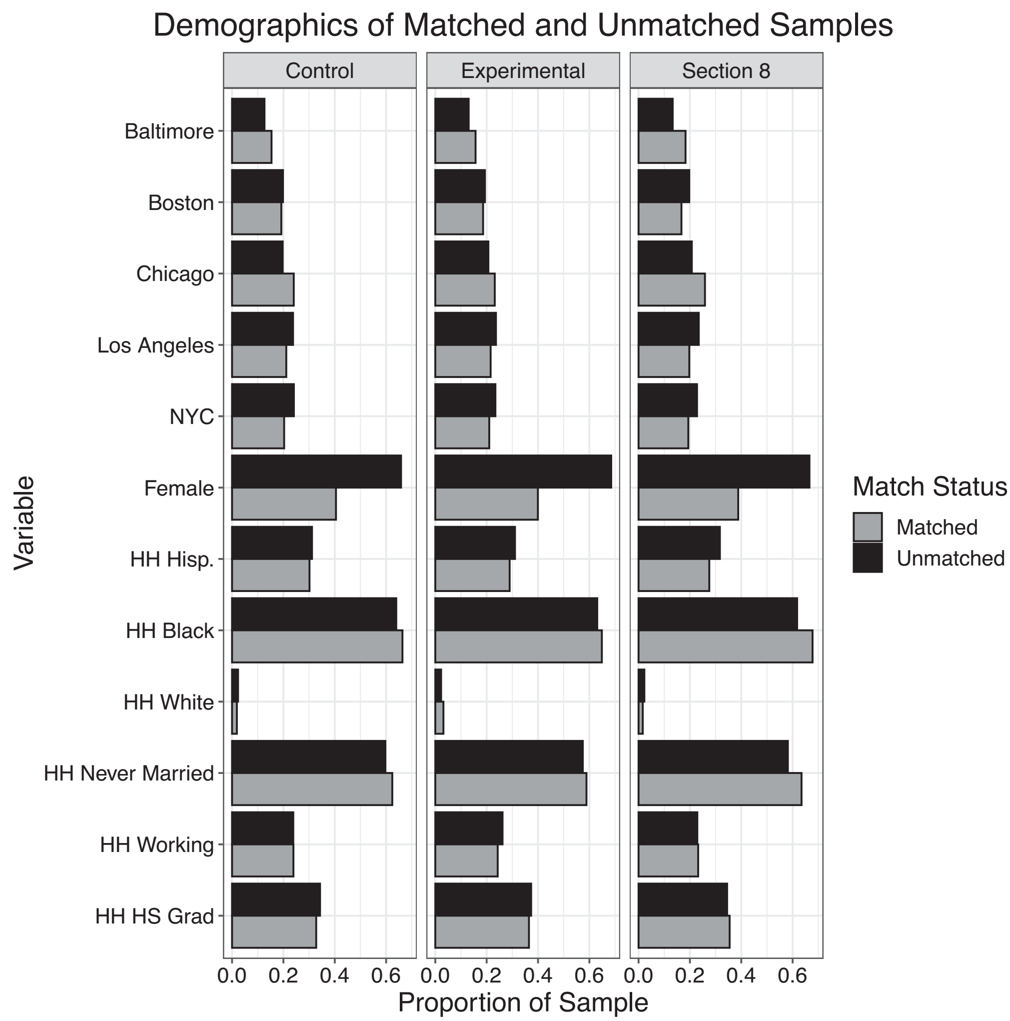
[19] Women in general are no less likely to register to vote than men (Center for American Women and Politics [CAWP] 2022). See Section A1.2 of the Supplementary material for further discussion of the gender gap in match rates.

[20] We do not have direct evidence on the proportion of our sample that is eligible to vote, but participants ineligible to vote would make the denominator smaller. Other data suggest less than 4% of people receiving public housing benefits are non-citizens (Carruthers, Duncan, and Waldorf 2013), so nearly all of our participants are likely citizens. It is unclear how many participants may be disenfranchised due to felony convictions, but previous scholarship suggests the proportion may be non-trivial (Lerman and Weaver 2014).

[21] We calculated this baseline using CES data reweighted to match the demographics of the MTO sample. Further details can be found in the supplementary material section A1.1.

[22] See Section A1.2 of the Supplementary material for an exploration of the potential causes of this gap. Some of it may be due to marriage, as women changing their names will be more difficult to match to the voter file, but the bulk of the gap remains unexplained.

FIGURE 2. Average Characteristics of Participants in Each Treatment Group Who Could and Could Not Be Matched to the Voter File



The limited power of other demographic variables in predicting match rates provides little evidence of bias from other sources.

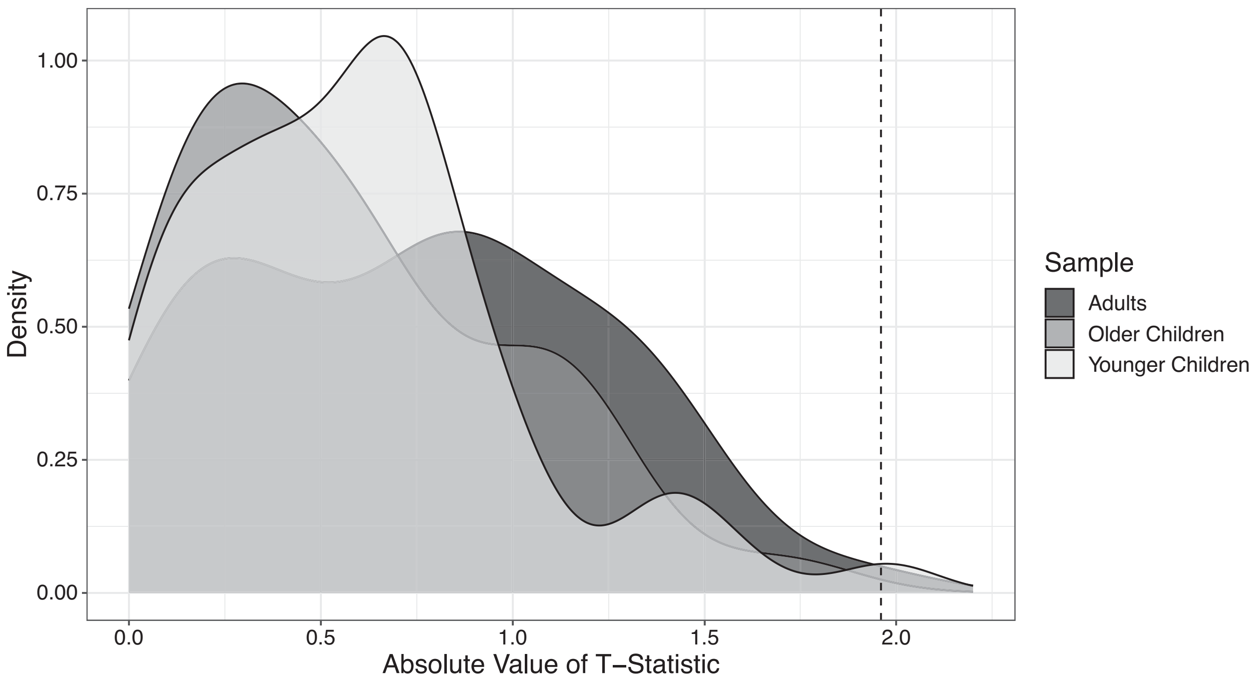
How likely is it that this male–female gap in match rates will bias our estimates? Chetty, Hendren, and Katz (2016) report that the intervention's effects on youths' education and income, two important antecedents of political participation, did not differ by gender. Kling, Liebman, and Katz (2007) and Ludwig et al. (2012), however, find that the treatment affected girls' risky behaviors and health more than it did boys'. If risky behavior and health are related to voter turnout and registration, this implies that a lower match rate of girls could attenuate our treatment effects. We probe this below when we look for heterogenous treatment effects by gender.[23]

Second, we conducted a test to determine whether matched and unmatched participants responded differently to the MTO intervention on outcomes not relevant to our study, such as participants' comfort in their neighborhoods and whether participants feel well-treated by police. If they do, this would be evidence that characteristics correlated with matching to the voter file are correlated with stronger responses to treatment, which could bias our estimates. To test this, we calculate differences between treatment groups on a wide range of 41 outcomes from the final survey, separately for the matched sample of respondents and the full sample of respondents. We then compare the average treatment effects in the matched sample to the average treatment effects in the full sample, using t-tests. We repeat this process for adults, teenagers at the time of random assignment, and preadolescent children at the time of random assignment.

Figure 3 displays the distribution of t-statistics across the 41 outcomes. A low t-statistic suggests the

[23] We find no evidence of differences in treatment effect size by gender; see Figure 4 for details.

FIGURE 3. T-Statistics of the Difference in ATEs between Matched and Full Samples of Participants on 41 Final Survey Outcomes



Note: The dotted line at 1.96 represents the conventional barrier for statistical significance. T-statistics less than 1.96 represent outcomes for which the treatment effects for matched and all participants did not significantly differ.

treatment effects for the matched and full samples were not substantially different. For each subgroup, the bulk of the t-statistics are small. Only two rise above the conventional threshold for statistical significance, indicated with a vertical line at 1.96, well within the proportion expected to be significant by chance, and none are greater than 2. That is, the treatment effects on nonpolitical outcomes did not differ systematically between the matched and full samples.

We conclude that the matched sample did not respond to the MTO intervention differently than the full sample. This suggests that any differences in unobservable characteristics that lead some participants to match at higher rates are not correlated with how those subjects respond to treatment, so those differences between the matched and full samples should not bias our treatment effects. These tests are limited to the observable measures available in the MTO surveys, and it remains possible that other unmeasured differences could bias our estimates. However, in all, these tests fail to find evidence of differences between the matched and full samples that could bias our treatment estimates.

RESULTS

The tables below present estimates of the effect of the MTO intervention on voter registration and turnout. We present the effects of the intervention on four outcomes, each of which we discuss in further detail below: whether an applicant is registered to vote (measured by whether they matched to the voter file), whether the applicant ever voted in an election post-treatment, the proportion of elections in which an applicant voted in the period after random assignment (with the denominator limited to elections in which the participant was at least 18 years old), and the proportion of elections in which an applicant voted after having registered to vote (limited to respondents matched in the voter file). For confidentiality reasons, the proportion variables were rounded to the nearest tenth of a point.[24] If someone cannot be matched to the voter file, we code them as never having voted for the second and third measures. This is standard practice in this literature and avoids potential problems with conditioning on registration, which can itself be affected by treatment (McDonough, Enamorado, and Mendelberg 2022). For completeness, though, the last measure captures turnout among only those who are matched to the voter file.

Matching to the voter file indicates that a participant was linked to a record in the 2016 voter file snapshot used for our merge. Due to voter file maintenance, many

[24] For example, respondents who voted in between 0.15 and 0.24 of elections are all given a value of 0.2.

people who had been registered to vote at some point post-treatment but were not as of 2016 will not be matched because they will have been removed from the voter file for inactivity or other reasons. This also means that a voter's turnout in any given election will only be available if linked to their 2016 registration. Although L2 attempts to link records for voters who have moved or changed names, this is an imperfect process, so some voters who moved or changed names since their first time registering to vote and the time of our voter file snapshot will be coded as not voting prior to their latest registration, meaning that our average estimate of voting frequency will likely be biased downward. Because, based on analysis above, we do not expect a consistent relationship between treatment response and the likelihood of a registration lapse, we do not expect this problem to bias our treatment effect estimates.

The three variables measuring voter turnout provide different advantages. The post-treatment voting rate refers to the proportion of elections in which a participant voted after the year in which they were assigned to a treatment group. While this variable allows us to capture the largest amount of variation in participants' level of participation, its interpretation is complicated by the fact that, as discussed in the previous paragraph, voters with breaks in their registration history are not linked to all the elections in which they voted. The other two turnout outcomes address this concern. Measuring whether a participant ever voted in an election post-treatment is coarser than the turnout rate, but it ameliorates the concern that we are treating some participants as less frequent participators than others due to a broken registration record. Finally, measuring turnout rates as a proportion of elections since the voter's most recent registration date also avoids the problem of undercounting turnout due to broken registration records. However, analyses with this outcome are only possible for the 16% of participants matched to the voter file, a population defined by an outcome—registration—that could be affected by treatment.

Table 1 presents the raw means of each outcome for each treatment group. These raw means indicate that there is little overall difference between the experimental and control groups on the outcomes of interest. To investigate these relationships further, we estimate the effects using OLS regressions of the form:

$$Y = \alpha + \beta_1 D + \beta_2 X, \qquad (1)$$

TABLE 1. Outcome Means by Treatment Group

	Control	Section 8	Experimental
Matched	0.162	0.155	0.168
Ever voted	0.120	0.112	0.122
Voting rate post-treatment	0.038	0.032	0.038
Voting rate post-registration	0.335	0.313	0.334

where Y is a measure of voter registration or turnout, D is an indicator for assignment to the MTO experimental or Section 8 group (relative to the control group), and X is a vector of covariates. We compute cluster-robust standard errors clustered at the level of the family, as treatment was assigned to families. All models incorporate weights, provided by the MTO study's original investigators, which account for differing probabilities of assignment to treatment over time (Orr et al. 2003). The vector of controls X always includes indicators for the study site (city).[25] We estimate separate models for participants who were adults at the time of random assignment, teens (age 13 or older), and children (aged 0–12). Tables reporting the full regression models can be found in section A3 of the supplementary material.

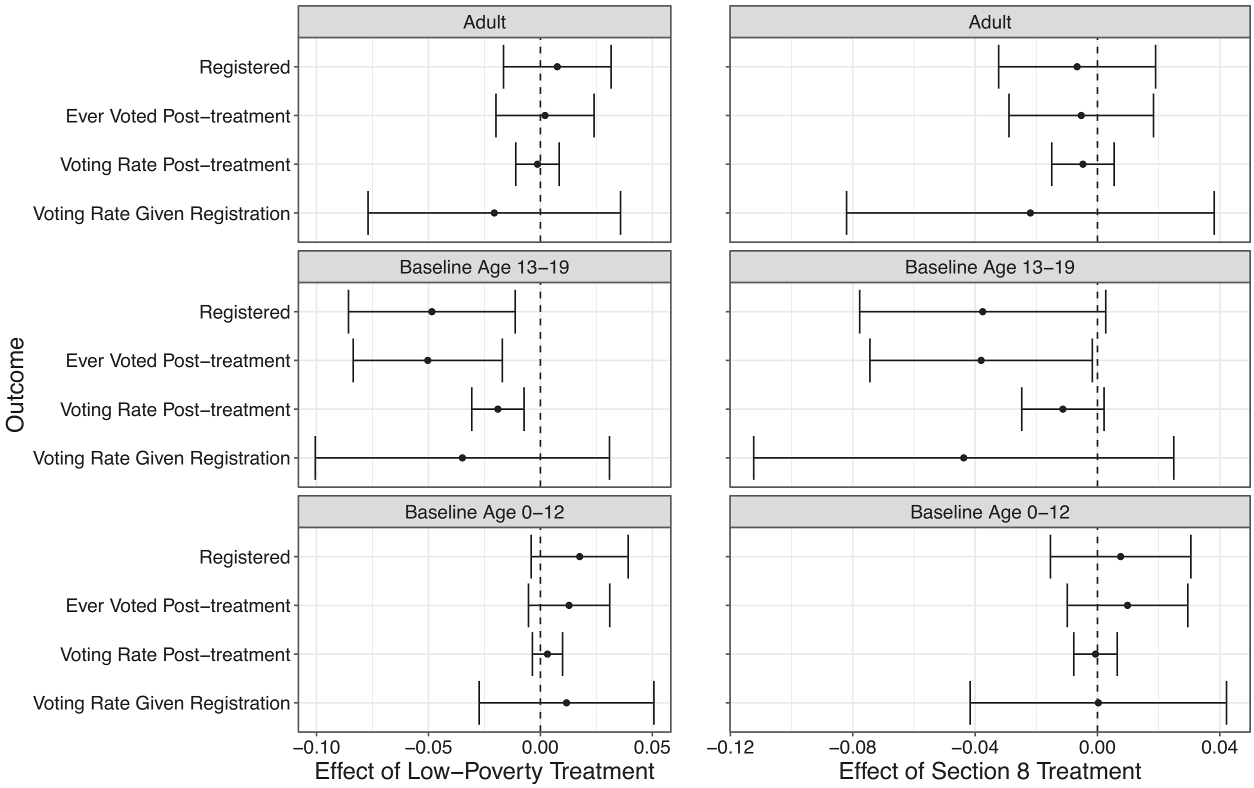
Figure 4 presents the effects of the experimental (left column) and Section 8 (right column) treatments on each measure of voting behavior.[26] The figure has three sets of panels, one each for results for participants who were adults, teens, and children at the time of random assignment. The first line in each panel presents the effects of the MTO intervention on matching to the voter file, which indicates a participant was currently registered to vote when the file was collected. The second line presents the effects on whether a participant ever voted after treatment. The third line presents the effects on the proportion of eligible post-treatment elections in which a participant voted. The final line presents the effects on the proportion of eligible post-treatment elections in which a participant voted, after the participant had registered to vote (and therefore limited to participants who are registered to vote). Each point represents the estimated treatment effect, and the bars represent 95% confidence intervals.

Across all four outcomes, both treatments, all age groups, and all model specifications, the MTO intervention either decreased the participants' rate of registration or turnout or did not increase it enough to be statistically distinguishable from zero increase. For adults, the coefficients on the treatment indicator are either close to zero (registration, ever voted, and voting rate) or too imprecisely estimated to detect effects statistically distinguishable from zero (post-registration turnout). For teens at the time of treatment, the effect of the experimental vouchers is negative and distinguishable from zero in three of the four outcomes. The effect of the Section 8 vouchers is consistently negative as well. This is notable, given that Section 8 vouchers are one of the main anti-poverty housing policies in the United States. These negative effects thus have important policy implications, which we elaborate on in the conclusion. For preadolescent children, for whom we expected positive effects, the results suggest no effect. While all the specifications show slightly positive effects

[25] Results available in Supplementary material Section A3 add a suite of baseline control variables recommended by the original investigators (Sanbonmatsu et al. 2011) to increase the precision of our estimates. These results do not differ substantively from the models without these controls.

[26] For full model results in table form, see Supplementary material Section A3.

FIGURE 4. Effects of MTO Treatments on Voting Behavior



Note: Each point represents the coefficient on the treatment indicator in a regression of voting behavior (each measure on the Y axis) on the experimental treatment (left panel) or the Section 8 treatment (right panel).

of the experimental treatment, they are small and imprecisely estimated. In all, we find evidence that the intervention decreased turnout for teenagers and, if it did increase turnout for children, the effects are substantively very small.

These results represent the effect of being *offered* an experimental or Section 8 voucher; because only 50–60% of participants used the offered vouchers, they may understate the effect of actually *using* the vouchers to move to a less poor neighborhood. Results in section A3 of the supplementary material use treatment assignment as an instrument for changes in the poverty of a participant's neighborhood. The results are similar to those in Figure 4: moving to a lower-poverty neighborhood decreased registration and voting among teens at random assignment, but it did not significantly affect adults or children.

This result goes against our expectation that the MTO intervention would increase turnout for preadolescent children. The null effect is especially surprising given that the intervention increased their average education and income (Chetty, Hendren, and Katz 2016), as these are robust predictors of voting. Perhaps the treatment was effective in increasing voting for certain populations and so the average effects conceal heterogeneity in responses to treatment across demographic groups. In the results above, we already tested for response heterogeneity across age groups at the time of assignment; below we further test for heterogeneity across other demographics.

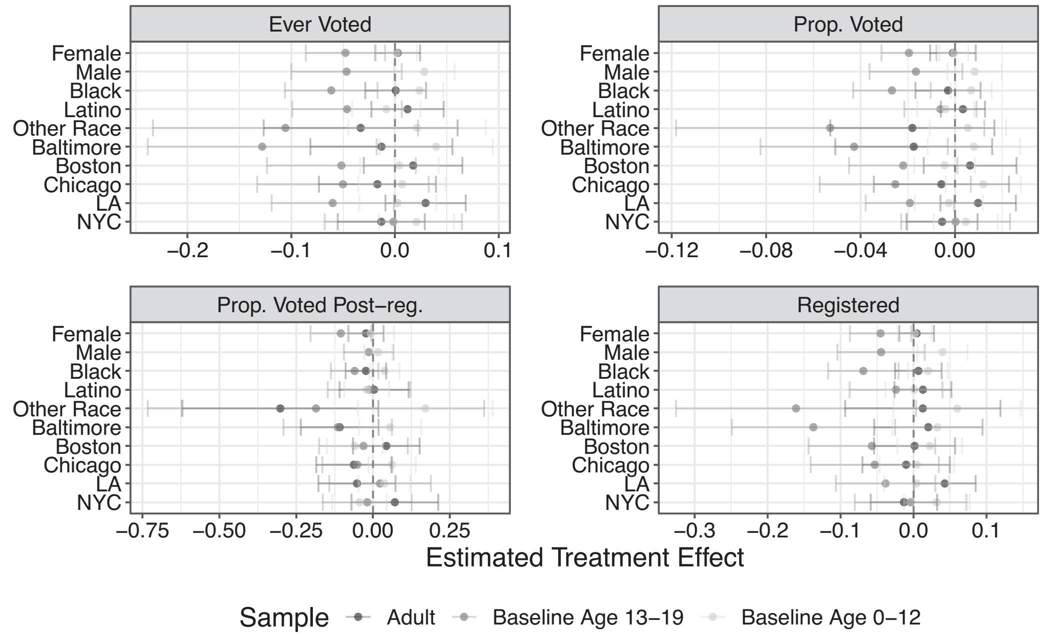
As we reported above, gender shapes both treatment response and match likelihood. One reason may be marriage. Chetty, Hendren, and Katz (2016) show that young treated participants were more likely to get married. Because many women change their names upon marriage, women in the treated group could be less likely to match than women in the control group. This gender-based pattern could depress our estimated turnout effects. To guard against this possibility, we repeat analyses on male and female participants separately.

We also repeat analyses for the subgroups that previous work has shown to display heterogeneity in the degree of benefits from the MTO intervention in other domains, separating participants by race and city. Focusing on groups where the intervention was especially effective in improving socioeconomic outcomes could uncover effects not apparent in the full sample.

Figure 5 shows the average effect of the treatment by gender, race, and city by age group.[27] These estimates are obtained by repeating the analysis in Equation 1

[27] We exclude the coefficients for men in the adult sample due to limited sample size.

FIGURE 5. Effects of Experimental Treatment Within Population Subgroups



separately for each subgroup. In this figure, each panel represents a different outcome variable with each line representing a different subgroup and a different color for each age group. Each point represents the coefficient on a treatment indicator in a regression of participation on treatment, and the lines represent 95% confidence intervals.[28] As in the full sample, no subgroup shows evidence for a positive effect on any of the four outcomes for any age group. The average effects do not appear to be masking meaningful subgroup effects. A systematic search for treatment effect heterogeneity, described in Section A6 of the Supplementary material, also did not locate any groups with noticeably larger or smaller effects.

WHY MIGHT THE INTERVENTION HAVE FAILED TO INCREASE POLITICAL PARTICIPATION?

Why did the opportunity to move to lower-poverty neighborhoods fail to increase turnout among children? One possible explanation is that the increases in education and income induced by the treatment are of insufficient size to produce detectable effects on political participation. To test the plausibility of this explanation, we calculate what the size of the turnout effect would be given the size of the intervention's effect on a powerful antecedent of voting. We start with college attendance, an important predictor of voting (Willeck and Mendelberg 2022). Chetty, Hendren, and Katz (2016) estimate a 2.5 percentage point effect of the experimental vouchers on children's college attendance. We multiply that by Dee's (2014) 20-point estimate of the effect of college entrance on recent voting. (We use one of the larger estimates in this literature to illustrate a "best-case" scenario for detecting treatment effects on turnout.) The result is an effect much too small to detect in a reasonably powered experiment or to usually matter substantively. Specifically, the treatment would increase voter turnout by $0.025 \times 0.2 = 0.005$, or half of one percentage point[29] (see section A8 of the supplementary material). An analogous analysis, available in the Supplementary material, suggests that the intervention's effects on income would produce about a 1-percent increase in turnout, also substantively small. Therefore, if the MTO intervention increased turnout only by increasing college attendance and increasing

[28] See the supplementary materials for an analogous figure for the Section 8 voucher treatment. See the extended tables Supplementary document for full tabular results.

[29] Given the statistical power available in most turnout studies, a treatment effect of 0.005 is too small to detect, and power analysis indicates that this effect would also be too small to detect given the precision of the measures available to us. The measurement of turnout in Dee (2014) is most comparable to our "ever voted" outcome, so we can compare the size of the hypothetical 0.005 turnout effect to the standard errors on our estimates in SI table A3.3. Column 5 presents a point estimate of 0.013 for the effect of the experimental treatment on turnout, with a standard error of 0.009. This suggests we would not have been able to detect an effect of half a percentage point. The smallest effect we could detect with this level of precision would be a turnout increase of 0.018, which would 3.6 times as large an effect of the intervention (i.e., a 9-percentage point effect on college attendance, or alternatively a 24-percentage point decrease in neighborhood poverty).

income, the effect on turnout would be very small—too small for us to detect with the methods used here.

Those tests consider whether the intervention could have indirectly increased turnout by affecting its antecedents. A second possible explanation for the null intervention effect on turnout is that living in lower-poverty neighborhoods does not affect turnout directly, either. In hypothesizing that the treatment would increase turnout by moving participants to lower-poverty neighborhoods, we assumed, based on the literature, that living in a lower-poverty neighborhood can increase turnout. However, for this sample, there is no observational association between neighborhood poverty and participation. Adults and youth in both age groups are no more likely to vote if they live in a lower-poverty neighborhood. These findings hold for each of the participation outcomes considered here (see Supplementary material Section A2 for full results).[30] The lack of relationship between turnout and neighborhood poverty is unusual, given that this association is well-established in the literature. This lack of association may be due to our sample's more limited variation in neighborhood poverty, which suggests that MTO may not have changed neighborhood poverty enough to affect participation directly or that the change in poverty was not effective in changing participation due to other characteristics of the sample, such as educational attainment.

Alternatively, the beneficial effects of low-poverty neighborhoods may not have materialized because the participants did not socially integrate into them. Based on interviews and ethnographic work, Briggs et al. (2008) describe neighbors in these areas as "cordial strangers or casual acquaintances at best" to the MTO participants. The interim and final surveys asked participants about their social ties, but at least two-thirds of experimental participants had already moved at least once by the time these data were collected, often into higher-poverty neighborhoods more like the ones they had left[31] (Orr et al. 2003). These frequent moves—in combination with the qualitative evidence—suggest experimental participants did not develop strong social ties in lower-poverty neighborhoods that could have encouraged political participation. If the expected relationship between poverty reduction and participation did not materialize because subjects did not socially integrate, that speaks to the difficulty of encouraging political empowerment through anti-poverty reductions that may not affect social integration and other important antecedents of participation.

CONCLUSION

By relocating young children and their families from high-poverty neighborhoods to more prosperous ones, the Moving to Opportunity experiment improved children's social, economic, and educational prospects later in life. However, the results here suggest the intervention did not make those children more likely to register or to vote in adulthood.

Moreover, teens who received it were actually less likely to vote: teens' turnout remained depressed many years after their initial move. This suggests a durable negative effect of the intervention on teens. The different effects on teens and preadolescent children suggest that their developmental stages may have made a difference to their life outcomes and to their political behavior. Those results imply that studies of political socialization should distinguish between life stages more finely than simply adult and preadult, as Holbein and Hillygus (2020) argue; see also Sapiro (2004).[32]

The results for adults offer a somewhat more optimistic conclusion than Gay's (2012) findings. Gay's study suggested that the treatment actually *lowered* participation, likely because the disruption of moving created social and bureaucratic hurdles to voting (see also Highton 2000). Our investigation suggests that in the long run, this negative effect disappeared. Though the intervention did not improve turnout for adults, it did not durably depress it.[33]

The design of the MTO experiment allows a well-founded claim to causal inference in the study of residential context, and the rich, long-term, participant-level data allow us to examine effects among the population most likely to be affected by the intervention: young children, who became somewhat more educated and less poor due to the treatment (Chetty, Hendren, and Katz 2016). That we find no effect on turnout for these most-likely participants should call into question whether moving from a very poor to a somewhat-less poor neighborhood, and from deep to moderate individual poverty, is sufficient to increase voting, especially when participants were unable to remain in low-poverty neighborhoods for very long. The treatment did not cause large enough changes in individual-level socioeconomic status to produce a detectable effect on turnout. In addition, treated children may not have

[30] Alternatively, some unobserved factor may make it more difficult to match participants who live in lower-poverty neighborhoods to the voter file, which could obscure higher registration and voting rates among this group—though there remains no relationship between neighborhood poverty and turnout rates even when conditioning on registration.

[31] By the time of the interim survey in 2003, 5–10 years after initial moves, two-thirds of complying experimental group families no longer lived in the lower-poverty neighborhoods they initially chose (Orr et al. 2003). Therefore, though adults and children in the experimental group do not report fewer close social ties than the control group at the interim or final surveys, these questions are not well-suited to measuring participants' integration into their initial low-poverty neighborhoods.

[32] The intervention may have a sleeper effect we failed to detect. On average, participants who were children at the time of the intervention were in their mid-twenties when our voter data was collected. It is possible that a positive effect of the treatment on turnout will manifest with more time. Chetty, Hendren, and Katz (2016) find that the positive effects on children's incomes grow as they age. As the preadolescent participants age further into adulthood, their turnout rates may climb as well. However, that possibility remains speculative.

[33] Alternatively, the differences between our results and Gay's (2012) could be attributed to our improvements to the matching scope and process rather than true changes in the effect of treatment over time.

been exposed to social and institutional features that encourage higher turnout in lower-poverty neighborhoods—because their family did not live in low-enough poverty neighborhoods or live in these neighborhoods for long, or because these neighborhoods did not foster participating families' integration into their social networks and norms.[34]

How much do these results generalize? While the Moving to Opportunity experiment represents a unique opportunity for causal identification, and took place in five varied cities, its effects may not generalize to the effects of residential context more broadly. The intervention was applied to very poor families largely headed by women of color. It required those families to move to places with less poverty. For different populations, or when residential context is chosen organically rather than induced by a program, so that social ties and other correlates of social capital are undamaged, the effects on turnout may well differ. In addition, the effects on other types of political action may be quite different. People living in poverty may be more interested in actions that stand to have an immediate impact, such as neighborhood associations or attending local board meetings (Michener 2013). Vouchers may facilitate those forms of participation even if they leave voting unaffected. Still, these results remain the best-identified and most-current estimate of the effects of neighborhood disadvantage on voter turnout.

Finally, the results suggest that more effective anti-poverty interventions than MTO may be necessary. We mean that in two ways. First, in order to increase turnout, the antecedents of turnout have to increase much more than they did under MTO. The treatment would have needed to increase education or income by 2–4 times as much as it did. Second, these vouchers may not be the most effective possible program for reducing poverty. Not only did vouchers fail to reduce poverty enough to increase turnout; for teens and adults, they had mostly null or negative effects on poverty (e.g., Chetty, Hendren, and Katz 2016; Ludwig et al. 2012; Sanbonmatsu et al. 2011). The experimental-group families who moved to lower-poverty neighborhoods did not remain long and did not integrate effectively into their new communities: most quickly moved back to poor neighborhoods, and did not form social ties with their new neighbors (Briggs et al. 2008). Short-lived or superficial changes in neighborhood are unlikely to produce large effects on any meaningful aspect of life, even when these changes come at a young age (e.g., Goodman-Bacon 2021). Moreover, moving disrupted teens' lives to an extent not offset by the improved opportunities in the neighborhood.[35] Even the offer of Section 8 vouchers—which, unlike the low-poverty vouchers, are regularly offered as a core part of rental assistance programs—seems to have disrupted teens' lives enough to lower their voter turnout years later, a pattern which may concern practitioners of housing policy. In that sense, both voucher types in MTO—the low-poverty vouchers, and Section 8 vouchers—are not very effective in their main purpose, which is to significantly alleviate poverty and its consequences.

Ultimately, our findings, along with the mixed effects on other outcomes, point to the shortcomings of the main anti-poverty housing programs in the US. Indeed, the lessons learned have already led scholars and policymakers to explore programs with higher voucher amounts and robust assistance in securing—and remaining in—satisfactory housing and well-served neighborhoods (Bergman et al. 2019). Robust funding and housing services are notably missing from both the low-poverty voucher program and the Section 8 program. The latter is the mainstay of anti-poverty housing policy. Our results, along with others, suggest it has serious shortcomings, not only for alleviating poverty but for promoting the type of political engagement necessary for long-term political capital (Finkel and Buron 2001; Varady 2010).

We have argued that poverty and political participation are important to study together because they can become linked in a vicious cycle where poverty suppresses participation and, in turn, low participation can result in reduced political power for those in poverty, so that policy solutions to address poverty are not prioritized. We have shown here that extant anti-poverty programs, in which political participation was not directly addressed, do not appreciably affect voter turnout. With these findings, policy makers may consider whether inequalities in political participation should be addressed directly, rather than as a side effect of anti-poverty programs. Ultimately, the most effective way of combating poverty and other inequalities may be by directly addressing inequalities in political participation and, thus, alleviating inequalities in political influence.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055423000692.

DATA AVAILABILITY STATEMENT

Replication data and code are available at the American Political Science Review Dataverse: https://doi.org/10.7910/DVN/CTGKEJ, and limitations on data availability are discussed in the Dataverse materials.

[34] By the time of the final survey 10–15 years after randomization, adults and children in the experimental group were not more socially isolated than those in the control group, but qualitative evidence suggests participants were not fully incorporated into the low-poverty neighborhoods to which they initially moved (Briggs et al. 2008).

[35] This disruption could directly lower turnout by severing social ties, or indirectly reduce turnout through income and education (as reported by Bergman et al. [2019]). Unfortunately, data to test whether social connection or the disruption of the move mediate these effects are unavailable.

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CONFLICT OF INTERESTS

The authors declare no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The authors declare that the Princeton University Institutional Review Board determined that this analysis does not represent human subjects research. The authors further declare that this research was conducted in compliance with APSA's Principles and Guidance for Human Subjects Research.

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