<u>Supplemental Information</u>

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

Elizabeth Mitchell Elder (emelder@stanford.edu)
Hoover Fellow, Hoover Institution, Stanford University

Ryan D. Enos (renos@gov.harvard.edu)
Professor of Government, Harvard University

Tali Mendelberg (talim@princeton.edu)
John Work Garrett Professor of Politics, Princeton University

Section A0. Treatment effects on intermediate outcomes.

We expect the MTO intervention to increase voter turnout by affecting a series of intermediate variables: neighborhood-level variables, like the affluence and education of participants' Census tracts, as well as individual-level variables, like participants' education, income, and relationship stability. In this section, we present the treatment group means for children and teens at the time of randomization for a series of intermediate variables; the individual-level outcomes were measured at the final survey, while the tract-level variables are averaged across all tracts of residence between random assignment and the final survey, weighted by length of residence in each tract. For each variable, we present the means for each group (weighted to adjust for varying probabilities of assignment), as well of the p-values of tests for differences between the control group and each treatment group. The p-values are drawn from regressions of each outcome on a treatment indicator and indicators for study site, with weights for assignment probabilities and standard errors clustered by family.

Table A0.1: Treatment effects on intermediate outcomes

Age Group	**Variable**	**Control Mean**	**Exp. Mean**	*p-value*	**Section 8 Mean**	*p-value*
Teens	Tract % Unemployment	0.19	0.17	*0*	0.16	*0*
Teens	Tract % on AFDC	0.18	0.14	*0*	0.14	*0*
Teens	Tract % Female-headed	0.53	0.48	*0*	0.47	*0*
Teens	Tract % Minority Race	0.88	0.84	*0*	0.87	*0.18*
Teens	Tract Poverty Rate	0.4	0.33	*0*	0.34	*0*
Teens	Tract % with College Degrees	0.15	0.19	*0*	0.17	*0*
Teens	Graduated HS	0.57	0.51	*0.09*	0.54	*0.49*
Teens	Attended College	0.42	0.36	*0.04*	0.36	*0.11*
Teens	Work full-time	0.61	0.61	*0.85*	0.52	*0.02*
Teens	Is a Parent	0.64	0.69	*0.06*	0.68	*0.31*
Teens	Has Been in Jail/Prison	0.21	0.22	*0.64*	0.22	*0.8*
Children	Tract % Unemployment	0.19	0.16	*0*	0.16	*0*
Children	Tract % on AFDC	0.18	0.13	*0*	0.14	*0*
Children	Tract % Female-headed	0.54	0.46	*0*	0.48	*0*
Children	Tract % Minority Race	0.88	0.82	*0*	0.86	*0*
Children	Tract Poverty Rate	0.4	0.31	*0*	0.32	*0*
Children	Tract % with College Degrees	0.16	0.2	*0*	0.18	*0*
Children	Graduated HS	0.67	0.67	*0.83*	0.67	*0.82*
Children	Attended College	0.43	0.45	*0.5*	0.44	*0.75*
Children	Work full-time	0.53	0.59	*0.1*	0.51	*0.88*
Children	Is a Parent	0.21	0.18	*0.06*	0.21	*0.88*
Children	Has Been in Jail/Prison	0.19	0.2	*0.71*	0.18	*0.83*

Section A1. Registration Rate Analyses

Section A1.1: Calculating Reference Registration Rates

Matching data on MTO participants to the voter file resulted in acceptable matches for 16% of participants. To gauge how successful our procedure was in finding matches for every registered MTO participant, we must calculate the rate of voter registration for a population similar to that of the MTO sample. We accomplish this using data from the 2020 Cooperative Election Study, which contains both reported and validated registration rates.

To make the CES sample more comparable to the participants in the MTO, we restricted and reweighted the CES's participants. CES respondents with incomes greater than $100,000/year were removed, as no MTO participants reported incomes that high in the final wave of the survey. The CES data was then reweighted to match the distribution of the MTO sample on race, education, age group, and income.

The table below reports rates of self-reported registration and registration validated against the voter file for CES respondents, as well as registration rates for the MTO sample.

Table A1.1: Registration rates in MTO and CES samples

	MTO sample- matched to voter file	**CES sample-** validated registration	**CES sample-** reported registration
Adults	.12	.67	.77
Older youth	.17	.42	.35
Younger youth	.18	.35	.21

Combining all age groups, the average registration rate among the reweighted CES respondents was 47% (validated) and 44% (reported).

As an alternative baseline metric, we calculated the proportion of a similar population registered to vote by combining voter file and Census records. We identified Census tracts in the study site cities that had a poverty rate greater than 40% in 2010, as only people living in tracts with greater than 40% poverty were eligible for the MTO study.

Using data from the 2010 Census and the 2013-17 ACS, we calculated the number of residents living in these tracts in each age group available in these surveys. We then located all registered voters in the voter file who resided in these Census tracts as of our 2016 snapshot and divided them into age groups matching those available in the Census. We then added these voters together to produce a count of the registered voters in each age group in these tracts.

Dividing the number of records in the voter file for each age group-tract by the Census 18-and-over population in each tract should give an estimate of the proportion of residents registered to vote in each group. However, this method produces registration rates at or over 100% for many tracts and age groups and implausibly low rates for others. We investigated several possible reasons for this, including mis-identification of tracts by the vendor, population change over time, and deadwood in the voter file, and while each of these could contribute, we were not able to identify a single cause. Importantly, we have no reason to expect any of these problems to differentially affect any of the MTO treatment groups and thereby threaten our inference. However, we therefore conclude that the voter file/Census count comparison is not suited to calculating registration rates in this way, and we do not report these figures as a baseline.

Section A1.2: Match rate by gender

Men in our data matched to the voter file at substantially higher rates than women, in each age group. The table below shows match rates by gender and age.

Table A1.2: Match rates by age and gender

Age Group	Men Match Rate	Women Match Rate
Adults	.368	.114
Older children	.205	.090
Younger children	.208	.092

One possible reason for this is that women change their name upon marriage, and we cannot link women with their maiden name in the MTO data to women with their married name in the voter file. This could help explain why the gap is larger among adult women than the younger generation—they may be more likely to have changed their names for marriage- or divorce-related reasons between the study and the voter file snapshot.

However, the marriage rates in our sample are insufficient to explain the full size of the gender gap we observe. For children younger than 13 at random assignment, Chetty et al. report that ~3-7% were married as of 2012 (depending on treatment group and gender). This is not a large enough proportion to account for the difference in match rates, especially given that Goldin and Shim (2004) report only 65% of black women (~70% of our sample is black) change their name upon marriage. So, the ~2x higher match rate for men observed among the younger generations cannot be attributed to marriage.

Why else might men match at higher rates than women, given that women and men in general are registered to vote at similar rates (Strode and Flores 2021)? Because privacy concerns prevent us from accessing the names and birthdates in the MTO data used for the matching, we cannot directly investigate differences in naming and birthday patterns and missingness that could shed light on this question.

However, we have some evidence that men in the MTO data are more likely to be paired with low-quality matches. Men, on average, matched to more observations in the voter file than women (in absolute terms and conditional on matching to the voter file at all), and those matches have lower posterior probabilities. Men in the youth generation are also more likely to be matched to observations in the voter file that record turnout in elections from before the child was eligible to vote, signifying a mistaken match.

We therefore theorize that the higher match rate among men is because men are more likely to be paired with low-quality matches in the voter file. Again, because we cannot access the names and birthdates, we cannot directly investigate the reasons for this. It could be that, because male names are less likely to be unique or unusual than female names (Hahn and Bentley 2003), men have highly similar names to more other observations in the voter file.

Section A2. Education, Neighborhood Poverty, and Turnout

This section shows the relationship between participants' education and the poverty rates of their neighborhoods of residence and their participation in elections. Each cell presents the weighted mean level of the relevant participation measure for the indicated group. In general, there is no relationship between education or neighborhood poverty and higher turnout. The exception are results showing higher post-registration turnout rates among higher-education respondents, though these should be interpreted with caution given the small number of participants in each of these cells (n=59 with 13 matching to the voter file for adults with a BA or more; n=84 with 22 matching to the voter file for pre-1990 children with more than a HS diploma).

Table A2.1: Participation rates by educational attainment (adults)

	Participation by Education (adults only)			
	Matched to File	Ever Voted	Voting Rate	Voting Rate post Registration
No HS Diploma	.12	.10	.03	.37
HS Diploma	.13	.11	.05	.39
AA Degree	.11	.08	.03	.41
BA or more	.10	.09	.04	.59

Table A2.2: Participation rates by educational attainment (children born before 1990)

	Voting by education (children born before 1990)			
	Matched to File	Ever Voted	Voting Rate	Voting Rate post Registration
No HS Diploma	.21	.15	.05	.31
HS Diploma	.18	.13	.04	.30
More than HS	.23	.17	.04	.28

Table A2.3: Participation rates by neighborhood poverty

	Participation by Neighborhood Poverty Quartile (all ages)			
	Matched to File	Ever Voted	Voting Rate	Voting Rate post Registration
Lowest poverty	.16	.12	.04	.34
Second Quartile	.16	.12	.03	.31
Third Quartile	.16	.11	.04	.33
Highest Poverty	.17	.13	.04	.34

Section A3. Main results: full regression tables

The tables in this section present the full regression models corresponding to Figure 4 in the main text. That is, each table shows models that regress a measure of voting behavior on assignment to the experimental and section 8 treatments. 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).

The models presented in Figure 4 of the main text can be found in the odd-numbered columns in the tables below. The even-numbered columns add a suite of baseline control variables recommended by the original investigators (Sanbonmatsu et al. 2011) to increase the precision of our estimates. We use the recommended control variables for adults for all participants, including children, where these controls represent characteristics of their household heads. Because of the age groupings in our analysis, we cannot use the children's control variables without dropping many observations for missing data. The full set of control variables is listed in appendix section A9.

Finally, each table presents an instrumental variables estimate of the effect of neighborhood quality on voter turnout. In these regressions, we use treatment assignment as an instrument for the proportion of a participants' posttreatment Census tract of residence[1] who were below the poverty line. We then reverse code this measure by multiplying it by –1 to make the direction consistent with the other estimates, so each coefficient shows the effect of the increase in neighborhood income caused by the treatment on the participation outcome. In particular, each coefficient represents the effect of moving from a neighborhood with 100% poverty to a neighborhood with 0% poverty based on an experimental voucher. This is the causal effect of the treatment on compliers (CACE), with compliers being those who were induced to move to a lower-poverty neighborhood by the treatment.

In direction and significance, the CACE estimates align with the other estimates: teens show significant negative effects of the neighborhood changes induced by treatment, while estimates for other groups are small and not significant. The magnitude of the CACE estimates, however, is larger. For example, the coefficient on local poverty in column 3 of table A3.1 suggests that an MTO-induced move to a neighborhood with a 10-point lower poverty rate—about the size of the vouchers' effect on neighborhood poverty—would cause a nearly 6-percentage point drop in registration rates for teens. Receipt of the experimental treatment in itself, on the other hand, causes only a 5 percentage point decrease. The larger CACE estimate reflects the fact that compliance by this measure is relatively low—a moderate proportion of the

[1] If participants lived in multiple Census tracts after treatment, this is an average of local poverty in all those tracts weighted by the length of time the participant lived there. Data collection, and thus this neighborhood quality variable, ended in 2008.

participants assigned to treatment moved using the vouchers, and the decline in poverty rate associated with those moves was fairly modest[2].

For extended versions of these tables showing coefficients for all control variables, please see the extended tables supplemental information file in the study Dataverse.

Table A3.1: Voter registration/match rate by MTO treatment

	Outcome: Matched to Voter File					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	0.008	0.012	-0.048*	-0.050*	0.018	0.019+
Standard Error	*0.012*	*0.012*	*0.019*	*0.019*	*0.011*	*0.011*
Section 8 Group	-0.007	-0.003	-0.037+	-0.039+	0.008	0.007
Standard Error	*0.013*	*0.013*	*0.020*	*0.021*	*0.011*	*0.011*
Local Poverty (experimental IV)	0.053		-0.593*		0.181	
Standard Error	*0.135*		*0.231*		*0.113*	
Observations	4604	4604	2758	2758	8513	8513
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X
This table presents regression estimates of the effect of the experimental and section 8 treatments (relative to the control group) on whether a participant matched to the voter file. Observations are weighted to account for unequal probabilities of random assignment, and standard errors are clustered by family. Models 2, 4, and 6 contain baseline covariates listed in Appendix section A10. + indicates significance at the p<.1 level; * indicates significance at the p<.05 level.						

Table A3.2: Post-treatment turnout rate by MTO treatment

	Outcome: Average Turnout Post-Treatment					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6

[2] See Clampet-Lundquist and Massey (2008) for a discussion of MTO compliance.

Experimental group	-0.001	0.001	-0.019*	-0.019*	0.003	0.004
Standard Error	*0.005*	*0.005*	*0.006*	*0.006*	*0.003*	*0.003*
Section 8 Group	-0.005	-0.003	-0.011	-0.012+	-0.001	-0.000
Standard Error	*0.005*	*0.005*	*0.007*	*0.007*	*0.004*	*0.004*
Local Poverty (experimental IV)	-0.025		-0.214*		0.023	
Standard Error	*0.055*		*0.075*		*0.035*	
Observations	4604	4604	2758	2758	8513	8513
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

This table presents regression estimates of the effect of the experimental and section 8 treatments (relative to the control group) on the proportion of elections in which a participant voted after their random assignment. Observations are weighted to account for unequal probabilities of random assignment, and standard errors are clustered by family. Models 2, 4, and 6 contain baseline covariates listed in Appendix section A10. + p<.1 level; * p<.05 level.

Table A3.3: Voting at least once post-treatment by MTO treatment

	Outcome: Ever Voted Post-Treatment					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	0.002	0.006	-0.050*	-0.052*	0.013	0.014
Standard Error	*0.011*	*0.011*	*0.017*	*0.017*	*0.009*	*0.009*
Section 8 Group	-0.005	-0.002	-0.038*	-0.040*	0.010	0.010
Standard Error	*0.012*	*0.012*	*0.019*	*0.019*	*0.010*	*0.010*
Local Poverty (experimental IV)	0.001		-0.596*		0.141	
Standard Error	*0.123*		*0.209*		*0.095*	
Observations	4604	4604	2758	2758	8513	8513
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X

Baseline Covariates	X	X	X

This table presents regression estimates of the effect of the experimental and section 8 treatments (relative to the control group) on whether a participant ever voted after random assignment. Observations are weighted to account for unequal probabilities of random assignment, and standard errors are clustered by family. Models 2, 4, and 6 contain baseline covariates listed in Appendix section A10. $^+$ p<.1 level; * p<.05 level.

Table A3.4: Turnout rate by MTO treatment, registered voters only

	Outcome: Average Turnout Post-Registration					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	-0.020	-0.021	-0.035	-0.050	0.012	0.010
Standard Error	*0.028*	*0.030*	*0.034*	*0.035*	*0.020*	*0.020*
Section 8 Group	0.022	-0.014	-0.044	-0.059$^+$	0.000	-0.001
Standard Error	*0.031*	*0.031*	*0.035*	*0.038*	*0.021*	*0.022*
Local Poverty (experimental IV)	-0.259		-0.463		0.085	
Standard Error	*0.334*		*0.383*		*0.205*	
Observations	551	551	474	474	1518	1518
Clusters	551	551	444	444	1269	1269
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

This table presents regression estimates of the effect of the experimental and section 8 treatments (relative to the control group) on the proportion of elections in which a participant voted after their registration to vote (among participants registered to vote). Observations are weighted to account for unequal probabilities of random assignment, and standard errors are clustered by family. Models 2, 4, and 6 contain baseline covariates listed in Appendix section A10. $^+$ p<.1 level; * p<.05 level.

Section A4. Analyses incorporating match probabilities

The analyses in the main text pair MTO subjects with the voter file match with the highest posterior probability, breaking ties through random sampling. Because of this strategy and the generally high quality of identified matches, variation on posterior probabilities in the sample used for the main text analyses is limited. The analyses in this section test the robustness of our main findings to an alternative method of dealing with multiple possible matches.

We begin with all the possible matches for each subject. For confidentiality reasons, we received only the possible matches with the top 5 values of posterior probability. Then, for each matched MTO subject, we reweight all the possible matches by their posterior probability times the inverse of the number of matches. This gives a weight that reflects the quality of each match and the quantity of possible matches for each subject. After rescaling this weight to have mean 1, we multiply it by the MTO-provided weights that reflect different probabilities of treatment assignment.

We then repeat the key analyses from the previous section's tables 1-4 using the updated weights.

For extended versions of these tables showing coefficients for all control variables, please see the extended tables supplemental information file in the study Dataverse.

Table A4.1: Voter registration/match rate by MTO treatment

	Outcome: Matched to Voter File					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	0.008	0.012	-0.036*	-0.037*	0.014	0.015*
Standard Error	*0.012*	*0.012*	*0.015*	*0.015*	*0.009*	*0.009*
Section 8 Group	-0.007	-0.003	-0.030+	-0.032*	0.008	0.007
Standard Error	*0.013*	*0.013*	*0.017*	*0.016*	*0.009*	*0.009*
Observations	6871	6871	5059	5059	15455	15455
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

Table A4.2: Post-treatment turnout rate by MTO treatment

	Outcome: Average Turnout Post-Treatment					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	0.002	0.003	-0.009*	-0.010*	-0.000	0.000
Standard Error	*0.005*	*0.005*	*0.004*	*0.004*	*0.002*	*0.002*
Section 8 Group	-0.005	-0.004	-0.003	-0.004	-0.001	-0.001
Standard Error	*0.005*	*0.005*	*0.005*	*0.004*	*0.003*	*0.003*

Observations	6871	6871	5059	5059	15455	15455
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

Table A4.3: Voting at least once post-treatment by MTO treatment

	Outcome: Ever Voted Post-Treatment					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	0.002	0.005	-0.036*	-0.038*	0.008	0.008
Standard Error	*0.011*	*0.010*	*0.013*	*0.013*	*0.007*	*0.007*
Section 8 Group	-0.011	-0.008	-0.028*	-0.029*	0.007	0.006
Standard Error	*0.011*	*0.011*	*0.014*	*0.014*	*0.008*	*0.008*
Observations	6871	6871	5059	5059	15455	15455
Clusters	4604	4604	1962	1962	4029	4029
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

Table A4.4: Turnout rate by MTO treatment, registered voters only

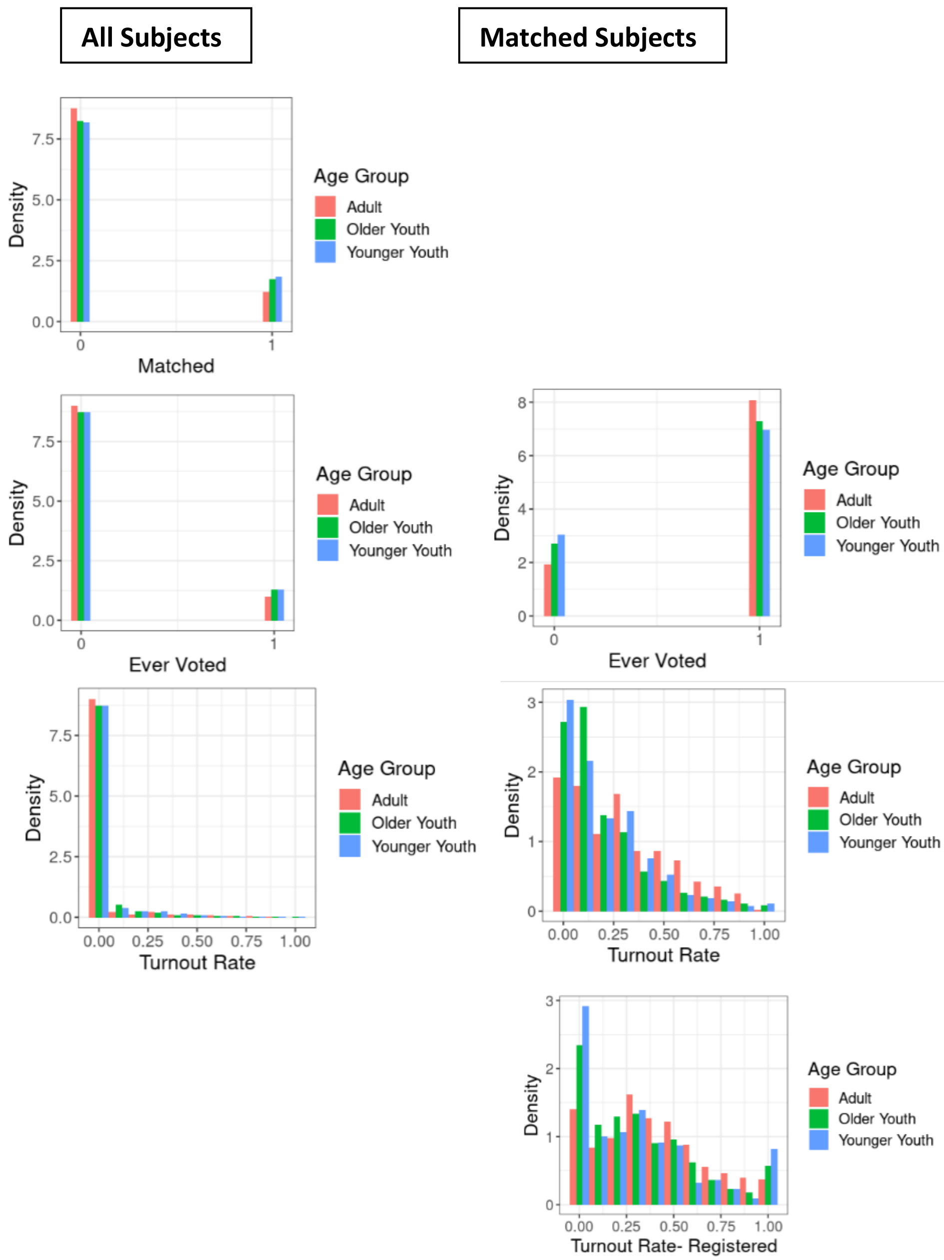
	Outcome: Average Turnout Post-Registration					
	Adults		Age 13-19 at baseline		Age 0-12 at baseline	
	1	2	3	4	5	6
Experimental group	-0.001	-0.004	0.001	-0.007	-0.011	-0.010
Standard Error	*0.022*	*0.022*	*0.030*	*0.033*	*0.017*	*0.017*
Section 8 Group	-0.026	-0.021	-0.015	-0.020	-0.015	-0.014
Standard Error	*0.024*	*0.023*	*0.031*	*0.033*	*0.017*	*0.018*
Observations	2806	2806	1595	1595	5162	5162
Clusters	552	552	446	446	1279	1279
Site Indicators	X	X	X	X	X	X
Baseline Covariates		X		X		X

As in the analyses in the main text, nearly all the estimates are insignificant and close to zero for adults and young children. The effect of the treatment on the teenagers' likelihood of matching to the voter file, ever voting, and rate of turnout is negative and significant. In all, the results of these analyses are similar to those in the main text including only the highest-posterior matches.

Section A5. Outcome distributions

The figures below present the distributions of each outcome variable for all subjects (left column) and for subjects matched to the voter file (right column). The rows present the following outcomes: matching to the voter file, ever having voted after treatment assignment, rate of turnout after treatment assignment, and rate of turnout after treatment when a subject was registered to vote.

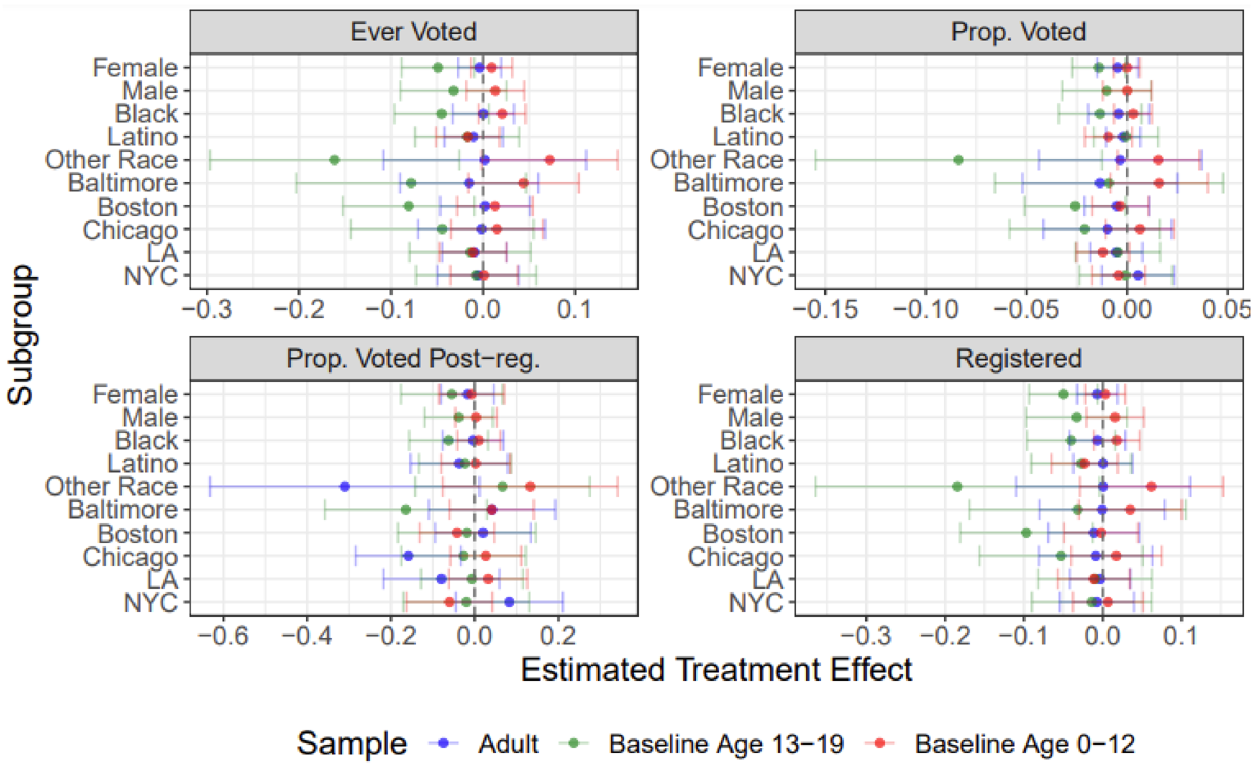
Figure A5: Distributions of participation outcomes: matched and unmatched subjects



Section A6: Heterogeneous Treatment Effects

Figure 5 in the main text presents the treatment effects for gender, race, and treatment site subgroups for each age group for the experimental treatment. The figure below shows analogous results for the section 8 treatment. Each point represents the estimated treatment effect on being in the section 8 condition, and the bars represent 95% confidence intervals clustered at the family level. See the extended tables supplemental document for full tabular results.

Figure A6: Treatment effects by subgroup



For a more comprehensive assessment of possible heterogeneous treatment effects than the analysis presented in the main text, we use Bayesian Additive Regression Trees (BART) to explore the entire suite of pretreatment controls. BART models the relationship between an outcome and predictors, flexibly incorporating nonlinear relationships and interactions between variables (for a review, see Hill et al. 2020; for an application in political science, see Green and Kern 2012). Tree models split a sample into successively smaller strata based on values of covariates, then calculate outcomes for the smallest strata. By repeating this process many times, the models can determine which covariate splits and combinations of splits explain the greatest variation in outcomes. This method allows us to canvas all the pretreatment covariates to determine whether any condition the relationship of the treatment to the outcome.

We first focus this analysis on the youngest age group and the experimental treatment, the case for which we had the greatest reason to expect possible effects. To explain the two continuous outcomes, voting rates and voting rates post-registration, we trained an array of 10 BART models with 50 trees each, using the treatment indicator and all pretreatment covariates available for the younger children as predictors. We then extracted the variables that were among the most important predictors in multiple models and ran prediction models to determine the CATE at all levels of each variable. The difference in the CATE at different levels of the variable represents our estimate of heterogeneity in treatment effects. As in the analyses main text, however, no variables significantly predicted variation in treatment effects. None of the calculated CATEs were positive, and all were close to the full-sample ATE. We conclude that there was no heterogeneity in treatment effects by the available covariates.

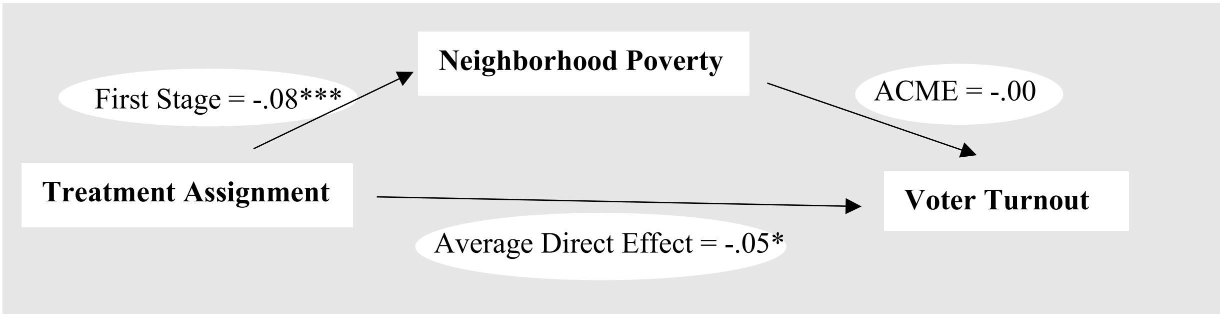
We then performed a similar analysis for the teen age group and the experimental treatment, the case for which we observed the largest negative effects. We trained an array of 10 BART models with 50 trees each, using the treatment indicator and all pretreatment covariates available for the teenage children as predictors of voting post-treatment. We again extracted the variables that were among the most important predictors in multiple models and ran prediction models to determine the CATE at all levels of each variable. As in the analysis of younger children, no variables significantly predicted variation in treatment effects, though there is suggestive evidence that teenagers from Baltimore were more negatively affected by the treatment than were those at other sites. Otherwise, all the calculated CATEs were close to the full-sample ATE. We conclude that there was no heterogeneity in treatment effects by the available covariates.

Section A7: Mediation Analysis

Next, we investigate potential mechanisms for the negative effect of treatment on participation among participants who were teens at the time of the intervention. To do so, we turn to two key mediators of the hypothesized effect. We first test whether the decrease in turnout can be explained by the intended effect of the treatment: a decrease in neighborhood poverty. The MTO study conductors measure a participant's exposure to residential poverty by combining the poverty rates in all the Census tracts where the participant lived in the decade after treatment assignment, weighted by the length of time the participant lived there. We can use this measure to test whether variation in exposure to neighborhood poverty induced by the treatment increased participation in politics—that is, can the decline in participation caused be explained by decreasing exposure to local poverty, the treatment's intended effect, or was the decline related to some other feature of the treatment?

To test whether neighborhood poverty mediates the relationship between treatment and turnout, we use the "mediate" package in R to conduct causal mediation analysis (Tingley et al. 2014). Figure A7.1 illustrates our findings. First, using OLS regression with the MTO-provided weights and standard errors clustered at the family level, we calculate the effect of assignment to the experimental condition on neighborhood poverty (the "first stage;" we focus on the experimental condition here as it had a larger effect on neighborhood poverty than the section 8 condition). We confirm that the treatment significantly decreased the length of time participants spent living in poor neighborhoods. We then calculate the average direct effect (ADE) of treatment on whether a participant voted in an election in the posttreatment period—that is, the effect of the treatment on turnout holding the mediator (neighborhood poverty) constant. The ADE is significant and of similar size to the effect reported in the main text. Finally, we calculate the average causal mediation effect (ACME)—that is, the average effect of treatment-induced variation in neighborhood poverty on turnout, holding treatment condition itself constant. This estimate is close to zero and not significant. We conclude that neighborhood poverty does not mediate the relationship between the treatment and turnout—the negative effect of the treatment on turnout cannot be explained by its effects on neighborhood poverty, but instead is likely to have arisen from some other feature of the treatment.

Figure A7.1: Mediation analysis results



What else could explain the treatment's negative effect on turnout for participants who were teens at the time of randomization? To test this, we chose a set of variables that could plausibly a) be affected by treatment b) explain part of the treatment's effect on participation and c) were measured for teens at some point after treatment. These variables included education, marital status, parental status, incarceration, moving frequency, and neighborhood level measures of social and economic characteristics. There is little evidence any of these variables mediated the effect of treatment: though two variables (neighborhood unemployment and neighborhood proportion female-headed households) were significantly affected by treatment and significantly mediated its effect, the proportion of the effect they were estimated to explain was less than 8% in each case.

The first set of variables are available through parents' reports on their children's lives collected at the final survey. Education (grad HS) is an indicator for whether the subject finished high school, education (attend coll.) is an indicator for whether the subject had attended college (regardless of graduation), work status is an indicator for whether the subject was working full- or part-time at a job at the time of the survey, marital status indicates whether the subject was married, parental status indicates whether the subject had any children, and time in jail/prison indicates whether the subject had spent any time in jail or prison.

The following 4 variables measure aspects of the subject's post-treatment neighborhoods of residence. Each represents a duration-weighted average of a characteristic of the Census tracts in which the subject lived between treatment assignment and the final survey in 2008. Finally, the moving frequency variable represents the number of times the household head reported having moved as of the interim survey in 2002.

The table below presents the results of the mediation analyses. These analyses follow the process described above for neighborhood poverty: we first regressed an indicator for assignment to the experimental treatment (relative to control; the section 8 group is excluded from these analyses) to determine whether the treatment caused an increase in the relevant mediator. The first column of the table below indicates whether the treatment had a significant effect on each outcome, along with the t-statistic on the treatment indicator.

For variables significantly affected by the treatment, we then tested whether the effect of treatment on the variable mediates the effect of treatment on turnout using causal mediation analysis with the "mediate" package in R. The second column of the table below reports whether the variable significantly mediated the effect of treatment, along with the p-value of the significance test on mediation. For the two variables that significantly mediated the effect, we proceed to report the estimated proportion of the total effect mediated by the variable in the third column.

Table A7.1: Mediation effects for teenage participants

Variable	Sig. ATE? (T)	Sig. mediates? (p)	Prop. Mediated
Education (grad HS)	Y (-1.7)	N (p=.11)	
Education (attend coll.)	Y (-2.0)	Y (p=.03)	-0.05
Work status	N (0.0)		
Marital status	N (1.3)		
Parental status	Y (1.7)	N (p=0.12)	
Time in jail/prison	N (0.5)		
Tract unemployment	Y (-7.6)	Y (p=0.03)	0.07
Tract TANF	Y (-9.2)	N (p=0.84)	
Tract female-headed	Y (-7.4)	Y (p=0.07)	0.05
Tract share minority	Y (-5.4)	N (p=0.53)	
Moving frequency	N (1.5)		

The treatment significantly affected all the neighborhood-level variables, decreasing unemployment, TANF usage, share female-headed homes, and share minority of the subjects' tracts of residence, and treated subjects were marginally more likely to have children. While college attendance, as well as tract-level unemployment and share female-headed households, significantly mediated the effect of treatment on turnout, the share of the effect each mediated is quite small—5% to 7% each. We conclude that none of these variables mediated the effect of treatment on turnout to a substantively significant degree.

Similarly, we are interested in understanding why the treatments failed to affect turnout among the youngest children. Because there is no main effect of the treatments on turnout, we do not run mediation analyses as we did for the older youth. However, we do test whether the treatment affected the variables we would expect to mediate a positive effect: education, employment, parental status, time in jail/prison, and moving frequency. If the treatment failed to move these variables, this could explain why the treatment had no effect on turnout.

These outcomes are available via self-reports from youth aged 10-20 at the final survey, or through parents' reports for older children. We combine these sources of information for the purposes of these test. We operationalize education as whether a subject graduated from high school or attended college, conditional on their being old enough to have either left school or taken one of these steps. Work status is measured by whether a subject reported working full time (only for those over age 22, as the expected effect is complicated by college attendance in younger subjects). Subjects and their parents reported the younger subjects' parental status, and time in jail/prison was reported by parents. Finally, we measured moving frequency based on the number of times the head of household reported having moved since treatment at the interim survey.

The table below reports the results. The second column lists the coefficient and standard error on the experimental treatment in a regression of the outcome on treatment condition, with site controls, assignment weights, and standard errors clustered at the family level.

Table A7.2: Effects on intermediate outcomes, young participants

Variable	Estimate (SE)
Education (HS graduation)	.013 (.021)
Education (college attendance)	.022 (.022)
Work full-time (age 22+)	.079 (.041)
Parental status	-.024 (.013)
Time in jail/prison	.009 (.025)
Moving frequency	.110 (.068)

The only significant effect here is on parental status, with experimental subjects less likely to have become parents by the time of the final evaluation. This mirrors the effect Chetty et al. (2016) find, that these subjects are less likely to become single parents. However, the treatment did not significantly improve other outcomes measured at the final survey in 2008—including college attendance, an outcome for which Chetty et al. report significant positive effects as of 2012. We find weak evidence of positive effects on employment, while Chetty et al. find no effects (though their measure is of the presence or absence of W2 earnings, while ours is a survey measure). We do not have a measure of income or college quality here, nor of marital status, other outcomes for which those authors report a significant effect.

Section A8. Effect Size Comparison: Income

Chetty et al. estimate that the experimental treatment increased children's annual income by $1,624, from a mean of $11,270 in the control group. Markovich and White (forthcoming) estimate that a $1.75 increase in the minimum wage increased turnout by about two percentage points for affected New York City municipal workers. A $1.75 increase in the minimum wage, for a worker working 40 hours a week for 50 weeks a year would gain $3,500 in yearly income, which is about 2 times the size of the MTO intervention's effect. We could therefore expect an increase in turnout due to the income effect of about one percentage point. As with the education comparison in the main text, we can compare this to the effect in column 5 of table A3.3 above, which presents an estimate of .013 for the effect of the intervention on turnout, and a standard error of .009. We would therefore not be able to detect an effect of this size given the power available to us.

Section A9. List of Baseline Covariates

Listed below are the control variables included in even-numbered models in tables A3.1-4. All controls are measured at the level of the household.

- Randomization site (Baltimore, Boston, Chicago, Los Angeles, or New York)
- Head of household's age at baseline (categorical: age 35 or lower, 36-40, 41-45, 46-60)
- Head of household's educational status at baseline (categorical: GED, high school diploma, currently in school, missing)
- Head of household's gender (categorical: male or female)
- Head of household's ethnicity (Hispanic, Black, other race)
- Head of household's marital status (ever married)

- Head of household's age at birth of first child (under 18 or over)
- Head of household's employment status (currently working)
- Household receives AFDC
- Household owns a car
- Household member has a disability at baseline
- Household contains at least one teen at baseline
- Household size (categorical: 2 or fewer, 3, 4)
- Household contains victim of crime in past 6 months
- Household head has lived in neighborhood at least 5 years
- Household head chats with neighbor at least 1x per week
- Household head would very likely report misbehavior from neighborhood child
- Household head has no family living in neighborhood
- Household head has no friends living in neighborhood
- Household head feels very unsafe on neighborhood streets at night
- Household head very dissatisfied with neighborhood
- Household head very sure of finding apartment if looking
- Household head moved more than 3 times in the past 5 years
- Household head listed drugs as 1st or 2nd most important reason for wanting to move
- Household head listed schools as 1st or 2nd most important reason for wanting to move
- Household head had applied to section 8 before

Section A10. Ethical Considerations.

This research was conducted in compliance with APSA's Principles and Guidance for Human Subjects Research. The Princeton University Institutional Review Board determined that this analysis does not represent human subjects research. We draw on two existing data sources: publicly available voter files, and data collected by the Department of Housing and Urban Development. We did not collect any original data, nor did we have access to individually-identifying information about the Moving to Opportunity program participants. The results presented here have been cleared by HUD to ensure they do not disclose information about program participants that could allow them to be identified.

References

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. "The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment." American Economic Review 106.4 (2016): 855-902.

Green, D.P. and Kern, H.L., 2012. Modeling Heterogeneous Treatment Effects in Survey Experiments with Bayesian Additive Regression Trees. Public Opinion Quarterly, 76(3): 491-511.

Hill, Jennifer, Antonio Linero, and Jared Murray. 2020. "Bayesian additive regression trees: A review and look forward." Annual Review of Statistics and Its Application 7: 251-278.

Tingley, Dustin, et al. 2014. "Mediation: R package for causal mediation analysis."