MOVED TO VOTE: THE LONG-RUN EFFECTS OF NEIGHBORHOODS ON POLITICAL PARTICIPATION

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Abstract—How does one's childhood neighborhood shape political engagement later in life? We study voting rates of children who were displaced by public housing demolitions and moved to higher opportunity areas using housing vouchers. Those displaced during childhood had 11% (2 pp) higher participation in the 2016 Presidential election and were 10% (2.9 pp) more likely to vote in any general election. We argue that the results are unlikely to be driven by changes in incarceration or parental outcomes but rather by political socialization or improvements in education and earnings. These results suggest that housing assistance programs may reduce inequality in political participation.

I. Introduction

A growing body of research shows that childhood neighborhoods exert a powerful influence on later-life economic outcomes (Chyn & Katz, 2021). It is also possible that neighborhoods generate benefits to society that are not reflected in earnings or education. Enhanced political participation is one important outcome potentially shaped by a person's childhood residence. Theory and prior empirical research suggest that neighborhoods could impact voting. For example, neighborhoods may shape voting through intermediate channels that have been previously linked to political behavior, including income (Akee et al., 2018), education (Sondheimer & Green, 2010), and incarceration (White, 2019), as well as pathways related to voting norms and pressures (Wolfinger & Rosenstone, 1980; Wilson, 1987).

Despite its potential importance, relatively little evidence speaks directly to the effects of childhood neighborhoods on later-life voting. Instead, an existing literature has focused on credibly estimating how adults are affected by their current neighborhoods. For example, Gay (2012) tracked adults who moved to lower poverty neighborhoods through the Moving to Opportunity (MTO) experiment, finding evidence of reduced voter turnout for these adults 7 to 10 years after their move. However, these findings for MTO adults may not be a good guide for understanding the effects of neighborhoods and poverty on the eventual voting behavior of children. Prior research demonstrates that conditions in childhood have distinct and large impacts on a number of longer-run outcomes (Garces et al., 2002; Chetty et al., 2011; Heckman et al., 2013; Chetty et al., 2016; Hoynes et al., 2016; Carrell et al., 2018; Bald et al., 2019).

This paper provides the first causal estimates of the impact of moving to higher opportunity neighborhoods during childhood on political behavior. We rely on a natural experiment created by public housing demolitions. During the 1990s, the Chicago Housing Authority (CHA) began selectively destroying high-rise public housing buildings that suffered from poor maintenance. Households who lived in buildings selected for demolition received housing vouchers and relocated to lower crime and higher income neighborhoods. Jacob (2004) and Chyn (2018) studied this same setting to estimate short-run effects on education and long-run effects on labor market activity, government assistance program use, and criminal arrests.

We compare long-run voting outcomes of children displaced by public housing demolition to their peers who lived in nearby public housing buildings that were not destroyed. This comparison estimates causal impacts of relocating from public housing if displaced and nondisplaced children are similar prior to demolition. Institutional features of the setting support the plausibility of this assumption, and we provide statistical evidence that shows no detectable differences in the background characteristics of displaced and nondisplaced individuals.

We find that relocating to lower poverty areas due to public housing demolition has large and statistically significant impacts on measures of political participation. Our analysis is based on linking administrative records for 5,933 displaced and nondisplaced children to statewide voter registration records from Illinois and its bordering states. We find that displaced children were 2 percentage points (11%) more likely to vote in the 2016 Presidential election and 2.9 percentage points (10%) more likely to vote in any general election (up to 2018). We find suggestive evidence that part of this movement is driven by new voters, with registration increasing by 1.7 percentage points (4%). The impacts on political participation are driven by larger effects for children displaced at younger ages, though the difference between the two groups is not statistically significant.

To study mechanisms, we undertake several exercises. First, we estimate impacts on incarceration, and show that demolition and relocation significantly lowered the likelihood of being incarcerated in adulthood. However, we argue that incarceration is unlikely to explain the voting impacts. We make this argument both by calibrating relative to the best available evidence on the impact of incarceration on voting (White, 2019) and by examining patterns in treatment effect heterogeneity. For the latter, we find that the impacts on voting are entirely driven by females, while the impacts on

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incarceration are driven by males. Second, we examine changes in parents' outcomes and find these are unlikely to drive the voting results, as the intervention had no significant impact on their voting. Third, we argue that improvements in high school graduation and labor market outcomes in adulthood are plausible mechanisms, as Chyn (2018) shows that the effects of demolition on these outcomes are large and prior studies show that these outcomes affect voting (Sondheimer & Green, 2010; Akee et al., 2018). Fourth, we examine whether the impacts could be due to living closer to polling places or in areas with higher voter participation during adulthood. We find that displaced and nondisplaced children live similar distances to polling places in adulthood and in areas with similar voting rates, suggesting these access and peer effects channels are not important mechanisms for our results. Finally, we discuss psychological channels, such as potential changes in beliefs and norms that come with moving to a new neighborhood, though we have limited ability to shed light on these mechanisms.

This paper contributes to an important literature studying neighborhood effects. To the best of our knowledge, we are the first to provide credible estimates of the impact of neighborhood conditions experienced in childhood on long-run voting. Our results complement recent studies that show childhood residence has important later-life impacts on earnings (Chetty et al., 2016; Chyn, 2018; Chetty & Hendren, 2018a,b), criminal behavior (Damm & Dustmann, 2014), and health (Kling et al., 2007).

Our results also provide insight on the determinants of political behavior. Recent work has placed more emphasis on credible identification of causal impacts. For example, Holbein (2017) and Akee et al. (2018) use experimental and quasiexperimental approaches to show that family income and education-based interventions that foster noncognitive skills affect political participation.[1] We complement their results in demonstrating the importance of early-life neighborhoods for voting. Our work is distinguished by the fact that the intervention we study has no impacts on parental income and does not directly target skills of children.

We conclude by noting that our findings have implications for public policy. The results indicate that moving to higher opportunity areas generates externalities by increasing political involvement. This may affect the distribution of public expenditures, as prior research finds that politicians are responsive to voters' preferences (Husted & Kenny, 1997; Lott & Kenny, 1999; Cascio & Washington, 2014; Brunner et al., 2013; Fujiwara, 2015). Thus, by reducing political inequality, housing programs that help low-income families relocate could generate reductions in economic inequality. This may be particularly relevant given that policymakers have recently sought to encourage moves to higher income neighborhoods by creating housing counseling programs (e.g., the Creating Moves to Opportunity program in Seattle and King County) and reforming housing voucher payment caps (e.g., Washington DC) (Bergman et al., 2019; Aliprantis et al., 2019).

[1] Billings et al. (2021) and Kaplan et al. (2019) also provide credible evidence on the effects of school busing and integration programs on political behavior. In contrast to these other works, this paper focuses on voting rather than partisanship.

II. Background and Data

A. *History of Public Housing and Demolition in Chicago*

At the start of the 1990s, the CHA managed the third largest inventory of public housing buildings in the U.S. (Popkin et al., 2000). Many of these buildings were high-rises with more than 75 apartments. Households were eligible for public housing units if their income was at or below 50% of Chicago's median income. Since this assistance was not an entitlement, eligible families spent years on a waitlist and typically accepted the first unit offered to them. At this time, the vast majority of Chicago's public housing population was African American, and a large share were single-parent, female-headed households.

The CHA started public housing demolition during the mid-1990s as a reaction to severe maintenance issues (Jacob, 2004). The dilapidated housing conditions stemmed from age and consistently poor maintenance (Popkin et al., 2000). Importantly, federal funding facilitated demolition. In 1993, the U.S. Congress created the HOPE VI program, which provided funds for public housing demolition and revitalization. The CHA was one of the largest beneficiaries of the HOPE VI program, receiving nearly \$160 million in HOPE VI grants by 1998.

Due to the size of the public housing inventory in Chicago, the CHA could only finance the demolition of a relatively small number of buildings during the 1990s. In general, policymakers chose to close and destroy buildings that had the most problematic maintenance issues. For example, pipes burst and caused flooding in several Robert Taylor project buildings in 1999. These buildings were subsequently closed and demolished.

When a building was closed for demolition, the CHA provided residents with Section 8 housing vouchers to subsidize relocation to private market housing. This voucher subsidy was equal to the difference between the family's rental contribution (30% of adjusted income) and the lesser of either the Fair Market Rent (FMR) or the price of rent. During the mid-1990s, the FMR was equal to the fortieth percentile of the local private market rent distribution. Families retained their housing voucher as long as they remained eligible for housing assistance based on their income.

B. *Data Sources and Sample*

We use records from multiple administrative sources to analyze the impact of public housing demolition on voting outcomes. We combine building records from the CHA and social assistance case files (1994–1997) from the Illinois Department of Human Services (IDHS) to create a sample of children who lived in public housing projects where demolitions occurred during the 1990s. We link this sample to voter files (as of 2019) from Illinois and its bordering states to obtain measures of registration and voting. Appendix B provides details for the data sources and sample.

Our analysis focuses on the same set of public housing projects and buildings studied in Chyn (2018). We study non-senior-citizen, high-rise projects that experienced demolitions during the initial wave of housing demolitions in Chicago during the 1995–1998 period. We exclude the Cabrini Green and Henry Horner projects due to qualitative evidence suggesting that building demolition could be correlated with unobserved tenant characteristics.[2] The final public housing building sample contains 53 high-rise buildings located in 7 projects. The date when a building was closed is based on Jacob (2004). During the study period, there were 20 demolished (treated) buildings and 33 comparison (control group) buildings. Note that the comparison group buildings did not close during the 1995–2000 period.[3]

To create the sample of children, we link the demolished and comparison group buildings to social assistance records for welfare recipients based on address information. Specifically, the sample is the set of children living in welfare recipient households that have a street address matching a public housing building in the year prior to building closure for demolition.[4] Focusing on the address in the year prior to demolition ensures that the sample definition is unrelated to the potential impact of demolition on welfare receipt. The sample contains 3,002 households with 5,933 children who are ages 5–18 at the time of displacement due to demolition.[5,6]

We merge the sample of public housing children to the Illinois voter file as of 2019 using name and date of birth. To guard against out-of-state attrition, we similarly match our sample to voter files from six additional states that border Illinois (Iowa, Indiana, Kentucky, Michigan, Missouri, and Wisconsin).[7,8] The voter files include registration information for each state, voter turnout in the 2000–2018 general and primary elections, and a modeled variable indicating the party of the voter. The last general election that we observe occurs in 2018, when the youngest and oldest children in the sample are 28 and 41 years old, respectively. All children are the minimum voting age (18) by 2008, which implies that we observe voting outcomes in at least six elections (three presidential elections) for every child. On average, children in our sample are eligible to vote in about eight elections (four presidential elections).

Finally, as in Chyn (2018), we use data from unemployment insurance earnings records (1995–2009) and arrest records (up to 2009) to measure baseline (prior to demolition) and other long-run outcomes of children.[9] We also link the sample to sentencing records (up to 2012) to measure incarceration as an additional long-run outcome. In section V, we study these long-run outcomes to interpret the analysis of voting.

III. Empirical Strategy

We follow Jacob (2004) and Chyn (2018) to study the impact of demolition and neighborhood relocation on children. Specifically, we compare children within the same public housing project, but who lived in buildings that were either selected for demolition ("treatment" buildings) or were left standing during the 1990s ("comparison" buildings). Under the assumption that demolition was quasirandomly assigned across buildings within the same public housing project, we can compare the displaced to the nondisplaced to estimate causal effects.

For voting outcome y_i, we estimate the following model for the impact of displacement and relocation:

$$y_i = \alpha + \beta D_{b(i)} + X_i'\theta + \phi_{p(i)} + \epsilon_i, \tag{1}$$

where i is an individual, and the indices $b(i)$ and $p(i)$ are the building and project for individual i. The term $\phi_{p(i)}$ is a set of project fixed effects. The vector X_i is a set of control variables (i.e., gender and race) included to improve precision. The dummy variable $D_{b(i)}$ takes a value of 1 if an individual lived in a building slated for demolition during the mid-1990s. We cluster standard errors at the building level. The main parameter of interest is β, which captures the net impact of demolition and relocation on children's outcomes.[10] In section IIIB, we analyze postdemolition household location to aid interpretation of this reduced-form parameter.

A. *Baseline Balance, Attrition, and Spillovers*

Estimates of β have a causal interpretation if we assume that CHA's selection of buildings for demolition was

[2]As highlighted by Jacob (2004), the housing authority demolished Cabrini-Green buildings associated with gang activity and crime. We also exclude the Henry Horner project because selection of buildings for demolition and voucher distribution was notably different at this site (Vale & Graves, 2010).

[3]In the 2000s, the CHA closed and demolished the comparison group buildings. Appendix B provides details on the timing of demolitions for the comparison group.

[4]This process includes identifying individuals living in the nondemolished buildings in the year before a building closure for demolition. For example, Buildings #1 and #2 in Rockwell Gardens were closed in 1998 while Buildings #4 and #6 were not demolished until 2006. Children residing at Buildings #4 and #6 in the year prior to 1998 comprise the nondisplaced (control) group for Rockwell Gardens.

[5]Data on children under age 5 in the year of demolition are not available. See appendix B for details.

[6]As detailed in appendix B, we estimate that the social assistance sample covers at least 73% of households living in the buildings we consider for our analysis.

[7]We obtained all voter records from L2, a commercial data vendor that works with major U.S. political parties.

[8]One concern is that we do not have data on registration and voting for the entire U.S. We discuss this issue further in section III and appendix C.

[9]For the analysis of these long-run outcomes, we rely on data for the sample of 6,135 children ages 5–18 created for Chyn (2018).

[10]As noted in section II, the CHA closed comparison group buildings during the 2000s. Due to this, the reduced-form parameter β can also be interpreted as the effect of being displaced earlier from public housing.

unrelated to the characteristics of children. The historical evidence suggests this condition is plausible because maintenance issues were a driving concern when the CHA selected buildings for demolition. In addition, residents living in different buildings within the same project should have similar characteristics. This stems from the fact that the tenant allocation process restricts choice for public housing residents. Most families spent years on a public housing wait list, and many accepted the first unit that was offered to them.

In addition to the context, Jacob (2004) and Chyn (2018) provide statistical evidence for our identifying assumption by testing for differences between displaced and nondisplaced residents in terms of predemolition characteristics. Appendix table A1 reports results from a similar balance test exercise for the sample considered in this study. We regress child and adult (parent) characteristics in the year before demolition on a dummy variable for living in a treated (i.e., later demolished) building and a set of project fixed effects following equation (1). Column 1 reports means for the outcomes for the nondisplaced children in our sample, while column 2 shows the mean difference between displaced and nondisplaced children within a project (i.e., the coefficient from the regression). We find no statistically significant differences in terms of age, gender, predemolition juvenile arrests, or schooling outcomes.[11] Finally, since we examine parent voting outcomes in section V, we show in column 5 that parents of children in our sample are also balanced on gender, labor market, and crime measures, though the displaced are slightly older.[12]

In addition to balance prior to demolition, it is important to discuss three potential issues for identification and interpretation: sample attrition, spatial spillovers, and changes in voting records over time. First, the concern for attrition is that the treatment could induce individuals to move out of the states for which we have voting records (i.e., Illinois and its six bordering neighbors). In appendix C, we provide detailed tests and discussion of this issue. Specifically, we provide evidence from the prior literature that attrition is unlikely (Jacob et al., 2015; Chyn, 2018), and we perform a standard check of out-of-state migration that produces no evidence of differential attrition. Second, we test and find no evidence of spillover effects for the control group. Finally, a third concern is that our results may be affected by the fact that we rely on voting records from 2019. Our measures are subject to measurement error because routine cleaning of voting records removes records for individuals who are deceased or have moved. In appendix B, we show that we obtain results in line with our main findings when we use an earlier vintage of voting records.

B. *Relocation After Demolition*

Finally, the interpretation of β from equation (1) depends on the type of relocation for displaced households. The housing vouchers provided to displaced households led to increased housing choice, and we study location outcomes using address histories based on social assistance records. Figure 1 compares neighborhood characteristics for displaced and nondisplaced children in the years leading up to and following demolition. Note that we only observe locations when a child is receiving social assistance—a potential concern if displacement has impacts on the likelihood of assistance receipt. Reassuringly, panel A shows that the likelihood of having an address (i.e., being on assistance) is balanced across groups in all years. Consistent with our identification argument, we find no pretrends in the years preceding demolition in terms of any neighborhood characteristics.

After demolition, figure 1 shows statistically significant declines in the neighborhood fraction black (panel B), poverty rate (panel C) and violent crime rate (panel D) in the years immediately following the demolitions. The short-run differences attenuate to zero within 10 years of the demolition because nondisplaced households gradually move from public housing. In appendix table A2, we report differences across these outcomes three years after demolition, and find that the effects on neighborhood poverty and crime are large relative to the nondisplaced household mean. As one point of comparison, the magnitude of the effects on the poverty rate is similar to the impact for the Section 8 treatment group in the MTO experiment. Appendix figure A1 summarizes differences in location over time by plotting densities of duration-weighted neighborhood poverty rates (i.e., averages over all postdisplacement locations).

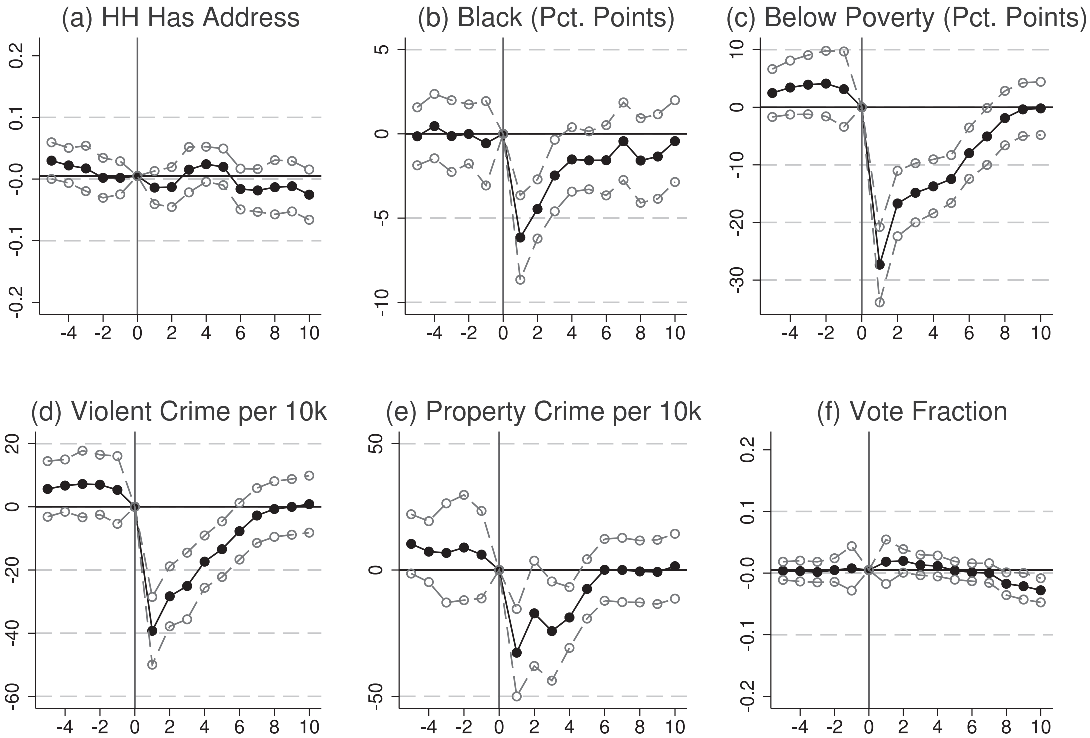
IV. Results

We have shown that the demolition and offer of housing vouchers led families to relocate to less disadvantaged neighborhoods—we now turn to whether this intervention translated into increased voter participation later in life. To do so, we estimate equation (1) across a variety of voting-related outcome measures. Figure 2 reports estimates from this equation where the outcome variables correspond to each of the 6 general elections that we observe for all individuals in our sample (during 2008–2016). The gray bars display the control group means, while the black bars correspond to the estimated voting rates of the displaced. We see that voting rates are low—between 5% to 10%—across the midterm elections (2010, 2014, 2018) and not statistically different between the groups. By contrast, we find a significant increase in voting across all three Presidential elections, where voting rates are much higher (e.g., 17.7% for the control group in the 2008 election) and we thus have more power to detect

[11] Schooling results are reported for the subsample of 5,250 children who move at or after age 7 (i.e., the sample from Chyn et al. (2017) for which schooling outcomes are reported). The data on schooling outcomes are not available for this study.

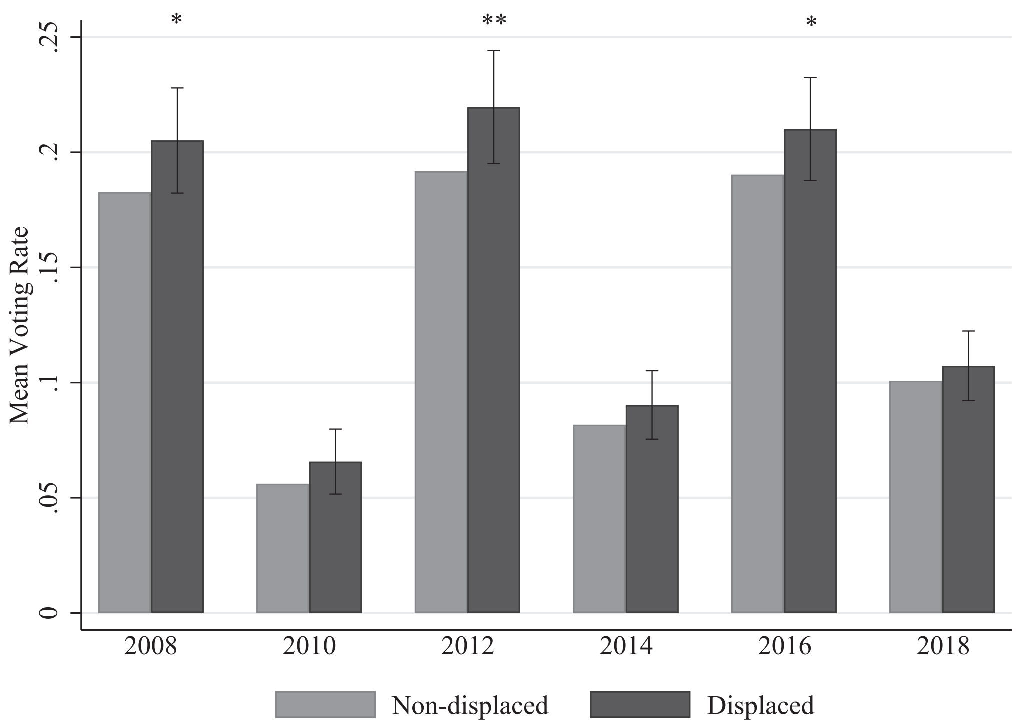
[12] Of course, it is possible that nondisplaced households may differ on unobserved dimensions. If nondisplaced households had unobserved advantages that were positively correlated with political behavior, then we expect estimates from equation (1) to represent a lower bound of the effect of demolition and relocation.

FIGURE 1.—IMPACTS OF DEMOLITION AND RELOCATION ON NEIGHBORHOOD CHARACTERISTICS OVER TIME



Panels show impacts of demolition on neighborhood (Census tract) characteristics over time. The unit of analysis is a household with at least one child. Neighborhood characteristics are based on the 1990 Decennial Census. Location is measured using address data from IDHS social assistance files. The x axis measures the number of years since relocation due to demolition. Each point in a panel is an estimate of the difference between displaced and nondisplaced households in a given period. Robust standard errors are clustered at the public housing building level, and the grey dots and dashed lines illustrate the 95% confidence interval for the coefficients.

FIGURE 2.—IMPACTS OF DEMOLITION AND RELOCATION ON GENERAL ELECTION VOTING



Bars display voting rates for the 2008–2016 general elections. The gray (left) bar for each election displays the mean voting rate for nondisplaced (control) children. The black (right) bar for each election displays the estimated voting rate for displaced children. The estimate for displaced children is based on analysis for each outcome using equation (1). The black bars on the maroon (right) bar illustrate the 95% confidence interval for each general election outcome. Statistical significance is denoted by: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table 1.—Impacts of Demolition and Relocation on Long-Run Voting of Children

	(1) Control Mean	(2) Diff.: Displaced-Nondisplaced, Within Est.	(3) N
Voting			
Ever Voted, General	0.301	0.029** (0.014)	5,933
Ever Voted, Primary	0.145	0.018* (0.010)	5,933
Voted General, 2016	0.185	0.020* (0.011)	5,933
Voted General, 2012	0.185	0.028** (0.013)	5,933
Voted General, 2008	0.177	0.022* (0.012)	5,933
Voted General, 2004	0.149	0.028* (0.015)	3,364
Share of Pres. Elections Voted	0.169	0.025** (0.010)	5,933
Registration			
Registered	0.408	0.017 (0.012)	5,933
Registered, Nonpartisan	0.256	0.003 (0.012)	5,933
Registered, Republican	0.005	−0.001 (0.001)	5,933
Registered, Democrat	0.148	0.015 (0.011)	5,933

This table analyzes adult voting outcomes for displaced (treated) and nondisplaced (control) children. The control mean statistics in column 1 refer to averages for nondisplaced children. The mean difference between displaced and nondisplaced children is reported in column 2. This difference is computed using the regression model specified in equation (1) where the voting outcome (each row) is the dependent variable for individual i. The independent variables in the regression include an indicator for treatment (displaced) status, a set of project fixed effects, and controls for sex and race. Statistical significance is denoted by: $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

differences. Table 1 reports these regression point estimates. Displaced children are 2.2 (12%), 2.8 (15%), and 2.0 (11%) percentage points more likely to vote in the 2008, 2012, and 2016 Presidential Elections, respectively.

To summarize voting impacts, we draw on previous practice to motivate our analysis of two types of measures. First, similar to Sondheimer and Green (2010) and Akee et al. (2018), we define binary variables indicating whether the individual *ever voted* in any general election we observe (2000–2018) or in any primary election we observe. Second, similar to Holbein (2017), we define a voting rate measure that is restricted to Presidential elections where we have power to detect effects—specifically, this is a continuous measure of the proportion of eligible Presidential elections that the individual voted in. The denominator for the latter measure ranges from 3 eligible elections (for the youngest in our sample) to 5 eligible elections, with an average of 3.9 in our sample. We find that displaced individuals are 2.9 (10%) percentage points more likely to ever vote in a general election and 1.8 (12%) percentage points more likely to vote in a primary. Finally, summarizing across the presidential elections, we see that the displaced have a 2.5 (15%) percentage point increase in the share of elections in which they voted.

These increases in voter participation could be driven by new registrants or by increased participation of those who would be registered regardless. While it's possible that individuals may register and never vote, the fact that we find effects on the ever vote measure suggests this is not simply driven by movements on the intensive margin of voting. The estimates for the voter registration outcome provide suggestive evidence in support of this hypothesis: the effect of displacement on the likelihood of registration is positive at 1.7 percentage points (4%) but statistically insignificant. This suggests that a portion of the increase in voting may be driven by new voters. Finally, we break this down by partisanship. While many states do not record party affiliation for voters, L2 models partisanship using votes cast in partisan primaries (in Illinois and Indiana) and other data (in the remaining states).[13] We use this modeled variable as a proxy for partisanship (with the caveat that its measurement is often an outcome of treatment itself). The results near the bottom of table 1 shows imprecise evidence that the new voters are those identified as Democrats, while there are smaller effects on being identified as a Republican or Nonpartisan.

V. Mechanisms

Several mechanisms could explain why moving into more advantaged neighborhoods during childhood could increase later-life voting. This section and appendix D consider incarceration, labor market outcomes, education, transmission of parent voting preferences, distance to polling places during adulthood, voting rates of one's neighborhood during adulthood, and more "psychological" channels (e.g., political socialization) that remain unobserved. As these are posttreatment outcomes, we refrain from including them in regressions because the strong assumptions required for mediation analysis are unlikely to hold. Instead, we examine patterns in these outcomes, their parallels to the voting results, and the prior literature to provide suggestive evidence. We argue the results are more consistent with being mediated by education and labor market improvements, as well as socialization and norms, rather than following directly from incarceration, parental outcomes, distance to polling stations, or neighborhood voting rates.[14]

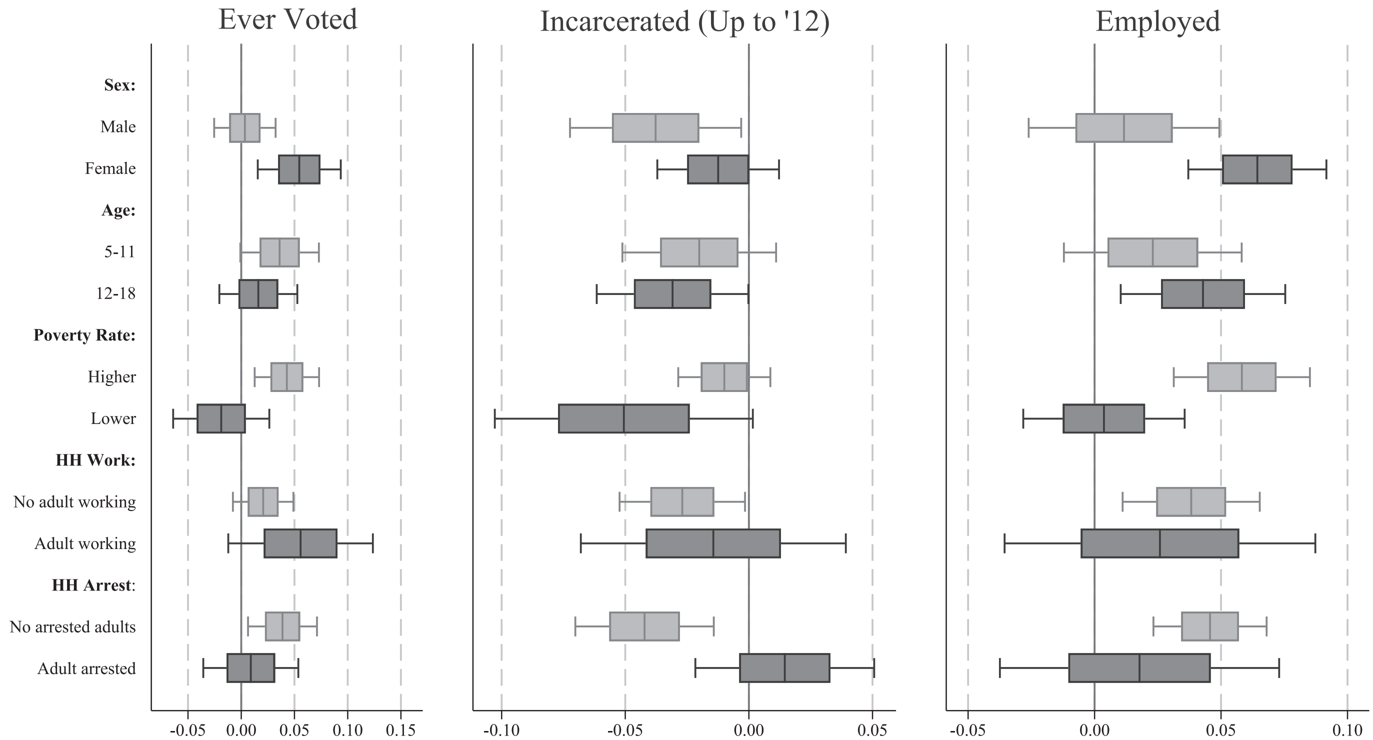
A. *Incarceration*

Voting and incarceration could be linked through several channels: disenfranchisement, incapacitation or discouragement. Regarding the first channel, roughly 6 million Americans were disenfranchised in 2016 due to current or past felony convictions (Sentencing Project, 2013). Illinois is less strict, as those convicted of felonies are only restricted from voting while serving their prison sentence. Outside of felony

[13] See appendix table B2 for more details.

[14] As discussed in appendix D, we have noisy proxies for neighborhood (nearest precinct) voting rates and distance to the nearest polling place. For the former, we find null effects, and for the latter, we find a small, negative effect (using one of the measures), suggesting it could only explain a small portion of the observed effects.

FIGURE 3.—IMPACTS OF DEMOLITION AND RELOCATION ON VOTING, INCARCERATION, AND EMPLOYMENT BY SUBGROUP



Rows present box and whisker plots for effects estimated separately for subgroups defined by baseline characteristics. The line at the center of each box is a point estimate for the estimated impact of demolition and relocation. The whiskers display the lower and upper ends of the 95% confidence interval. The left and right ends of the boxes display the points that are one standard error above and below the point estimate. Note that the point estimates and standard errors for voting outcomes are reported in appendix table A7.

convictions, individuals in the prison system (e.g., those awaiting trial in jail) may be legally allowed to vote, but that ability may be limited in practice.

We conduct two exercises which suggest incarceration-related mechanisms do not explain the voting impacts. First, Chyn (2018) found that displaced children had fewer violent crime arrests. Given this result, we use sentencing data (up to 2012) from the Illinois State Police to estimate impacts on incarceration outcomes. Appendix table A5 reports effects on being in prison during the entire postdemolition period (up to 2012) and during specific election years. While demolition reduces the likelihood of ever being incarcerated, we see insignificant reductions in individual election years (i.e., a 0.7 percentage point decrease in 2008), which suggests that incapacitation is unlikely to explain increases in voting across each of those elections (i.e., the 2.2 percentage point voting increase in 2008). To further understand the discouragement channel, we conduct a back-of-the-envelope calculation based on White (2019). She finds that incarceration from misdemeanors (with no formal consequences for voting eligibility) reduces voting in a subsequent election (after release) by 13 percentage points. Given that we observe a 2.5 percentage point decrease in the likelihood of ever being incarcerated, a simple estimate is that this discouragement channel explains roughly less than 12% ($0.025 \times 13 = 0.325$ percentage points) of the 2.9 percentage point ever voting effect.

Second, we study heterogeneity in effects on ever voting and incarceration in figure 3. The difference between displaced and nondisplaced children is the center (line) in each box plotted for a given group. The results show that voting effects are driven entirely by females. However, incarceration effects for females are small and insignificant. This pattern suggests incarceration is unlikely to be a mediator.

B. *Parent Outcomes*

Both children and their parents were displaced by demolition. One possibility is that relocation could have increased parental voting, thereby promoting civic engagement for their children. We find weak evidence for this channel in appendix table A6. On the one hand, displaced parents are 2.8 percentage points (7%) more likely to be registered to vote, an effect larger than what we find on children's registration likelihood (a 1.7 percentage point increase). Note that this increase in registration contrasts with Gay (2012) who finds that MTO had no statistically significant effect on registration using county voter files (after using an imputation approach to account for the possibility of registration in counties outside the data sources).[15] On the other, there is a no detectable impact on the likelihood of ever voting in a general election

[15]There are important differences between the neighborhood environments in Chicago and the other major cities included in MTO. In our sample, the Census tract poverty rate was about 78% prior to the demolitions. In the MTO experiment, the poverty rate in the baseline Census tracts was approximately 53%. This could matter if the effects of relocation and demolition are larger for adults from more disadvantaged neighborhoods.

(2000–2018) and a relatively precise null effect on the "vote share." The latter result contrasts with the significant effect on "vote share" for children. More broadly, our results are consistent with Akee et al. (2018), which found that an unconditional cash transfer to parents had no effect on their voting share, but increased that of their children. Finally, while it is possible that effects on other parent outcomes shaped the voting preferences of their children, Chyn (2018) finds that the displacement had no detectable impacts on parents' labor market outcomes.

C. *Education*

Jacob (2004) found no short-run impacts of demolition and relocation on schooling outcomes. In longer-run follow-up work, Chyn et al. (2017) found that displacement decreased the likelihood that children dropped out of high school by 5.1 percentage points (8%) for those who moved while young (ages 7–12), but had no detectable effect on those who moved when older (ages 13–18). This parallels the pattern of results for voting, where we find a large and significant effect on voting for those who moved while young, and a smaller and insignificant effect for those who moved while older (see figure 3). This raises the question of whether effects on education are driving voting impacts. To assess this channel, we refer to Sondheimer and Green (2010) who studied voting using three randomized interventions that increased high-school graduation rates. Their pooled estimate suggests that graduation increases voting by 1.4 probits. Applying this optimistic estimate to our control group mean of 30.1% (ever voted) suggests that high school graduation alone could increase their turnout to 81.0%. Thus, this calibration suggests that roughly 90% ($0.051 \times (0.810 - 0.301) = 2.6$ percentage points) of the 2.9 percentage point effect could be explained through education. If we use the bottom of their 90% confidence interval (0.51 probits), education could explain roughly 34% ($0.051 \times (0.495 - 0.301) = 1.0$ percentage points) of the effect.

D. *Employment and Earnings*

Improvements in later-life labor market outcomes are another channel that may drive effects on long-run voting. Chyn (2018) found that displaced children are 4.0 percentage points more likely to be employed in adulthood. The effects on employment by gender and poverty status mirror the pattern for voting (i.e., effects are larger for females and those who move away from higher-poverty neighborhoods). Figure 3 and appendix figure A2 show larger impacts on employment and earnings for older displaced children, which contrasts with the impacts on voting being driven by younger children.

The effect of employment on voting behavior is not *a priori* obvious. Employment increases the opportunity cost of voting, particularly for low-income individuals who have limited schedule flexibility on Election Days. However, employment increases earnings and may affect one's social environment. Since prior research (to our knowledge) does not provide causal evidence on the effects of employment on voting, we turn to the income channel. Chyn (2018) found that displaced children earned an additional $600 per year in adulthood. As noted in the introduction, the (largely correlational) literature on the effects of income on voting is mixed. A meta-analysis of 90 studies shows that half find income to be a significant predictor of voting, while the other half do not (Smets & van Ham, 2013). Akee et al. (2018) provide causal estimates of the effects of an unconditional cash transfer in the U.S. They find that a transfer of $4,700 to parents increased later-life voting of children by 8 percentage points. While it's unclear how much this raised the earnings of children, it's plausible that effects on later-life earnings were a mediator of their voting impacts.

E. *Psychological Channels*

Finally, the effects on voting may be mediated through psychological channels, such as political socialization. Neighborhoods could change the value of voting by shaping perceptions of inclusion and empowerment, moving beliefs about the efficacy of voting, or affecting social pressures (Wilson, 1987; Cohen & Dawson, 1993; Desmond & Travis, 2018). These are not necessarily distinct channels from those previously discussed—psychological effects may be produced by the intermediate changes in income, education, or other outcomes.

The CHA's decision to demolish buildings and provide housing vouchers represents a salient government policy. Prior work posits that such action may affect perceptions of political institutions (Pierson, 1993; Campbell, 2012). These "interpretive effects" may result in positive or negative feedback. On the one hand, the demolitions could have conveyed the sense that government services are low quality or encouraged feelings of powerlessness (Soss, 1999; Schneider & Ingram, 1993). On the other, the move to better neighborhoods, facilitated through the government-provided vouchers, could have sent a message of inclusion that encourages participation (Wilson, 1987; Skocpol, 1991). A body of empirical research, largely outside the U.S., has shown that assistance programs, such as conditional cash transfers, may improve voter turnout (e.g., De La O, 2013). Within the U.S., Baicker and Finkelstein (2018) and Clinton and Sances (2018) find that Medicaid expansion led to positive, temporary improvements in voter turnout. By contrast, we find a persistent increase in voting many years later for children, but no improvement for adults and a smaller improvement for older children. While it's possible that such interpretive effects operate differently for young children, this set of results suggest the standard account may not explain our results.

More broadly, the political science literature posits a role for childhood in shaping political socialization. For example, the "impressionable years" hypothesis argues that young people may have more malleable political behavior because their attitudes and identities have yet to ossify by that point

(Krosnick & Alwin, 1989; Sears & Funk, 1999). This is consistent with work showing that interest in politics is relatively stable and resistant to intervention after late adolescence (Prior, 2010, 2018). While some of this work focuses on intergenerational socialization (i.e., the learning of norms and values around voting from parents), socialization can be shaped by various institutions tied to a neighborhood including schools, church, media, peers, and others. As noted previously, we find larger effects for children who moved while younger. This would be consistent with a theoretical argument from the literature that, "the more important a political orientation is in the behavior of adults, the earlier it will be found in the learning of the child" (Greenstein, 1965). While it's likely that the economic channels have similarly more pronounced effects when received earlier, in line with Chetty et al. (2016) and Chetty and Hendren (2018a), it is also plausible that these could be operating indirectly through the psychological channels (e.g., a common set of norms and values, instilled earlier in childhood, leading to improved graduation and political participation). Ultimately, while psychological channels are a compelling way that neighborhoods may shape political behavior, we can only speculate with the data at hand.

VI. Conclusion

To the best of our knowledge, we provide the first causal estimates of the impact of moving to higher opportunity neighborhoods on long-run voting. We study a natural experiment in which public housing demolition in Chicago forced children from low-income households to relocate to lower-poverty areas using housing vouchers. Our analysis complements other recent studies that have used experimental and quasiexperimental approaches to provide credible evidence on the effects of neighborhoods on children (Kling et al., 2007; Damm & Dustmann, 2014; Chetty et al., 2016; Chetty & Hendren, 2018a,b; Chyn, 2018; Deutscher, 2020; Laliberte, 2021; Chyn & Shenhav, 2021).

We find that demolition and relocation had significant and positive impacts on later-life political behavior. Displaced children were 2.9 percentage points (10%) more likely to vote in any general election (up to 2018) relative to their nondisplaced peers. An important caveat for our results is that our analysis is limited to voting records for those currently (as of 2019) registered to vote in Illinois or one of its six neighboring states. The main implication is that our data may not fully reflect all voting behavior for displaced persons in our sample since we lack both national data and comprehensive historical records.[16]

Overall, our results have potential implications for public policy. Housing authorities and policymakers have introduced new housing counseling programs (e.g., the Creating Moves to Opportunity program in Seattle) and reformed housing voucher payment caps (e.g., Washington DC) to encourage low-income families to relocate to higher income neighborhoods. Recent studies find that these reforms, particularly in terms of counseling, can successfully promote relocation to higher opportunity areas (Bergman et al., 2019; Aliprantis et al., 2019). Our results suggest these policies generate externalities by increasing long-run involvement in the political process. This may be important for political outcomes given that prior research shows politicians are responsive to the interests of their constituents.

[16]Appendices B and C discuss measurement issues and conduct several tests to address concerns over sample attrition and our reliance on voting records from 2019.

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