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% (2pp) higher participation in the 2016 Presidential election and were 10% (2.9pp) 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.

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1 Introduction

A growing body of research shows that childhood neighborhoods exert a powerful influence on later-life economic outcomes (Chyn and 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 and Green, 2010), and incarceration (White, 2019), as well as pathways related to voting norms and pressures (Wolfinger and 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 the 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 non-displaced 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 non-displaced 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 non-displaced children to statewide voter registration records from Illinois and its bordering states. We find that displaced children were 2 percentage points (11 percent) more likely to vote in the 2016 Presidential election and 2.9 percentage points (10 percent) 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 percent). 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 – we find that the impacts on voting are entirely driven by females, while the impacts on 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 and 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 non-displaced 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 and Hendren, 2018*a*,*b*), criminal behavior (Damm and 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 quasi-experimental approaches to show that family income and education-based interventions that foster non-cognitive 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 and Kenny, 1997; Lott and Kenny, 1999; Cascio and 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) also provide credible evidence on the effects of school busing and integration programs on political behavior. In contrast to their work, this paper focuses on voting whereas their analysis focuses on partisanship.

2 Background and Data

2.1 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 percent 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 percent 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.

2.2 *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]

[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 and 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 non-demolished 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 non-displaced (control) group for Rockwell Gardens.

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

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

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 5, we study these long-run outcomes to interpret the analysis of voting.

3 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 ("displaced" or "treatment") or were left standing during the 1990s ("non-displaced" or "comparison"). Under the assumption that demolition was quasi-randomly assigned across buildings within the same public housing project, we can compare the displaced to the non-displaced to estimate causal effects.

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

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

[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 3 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).

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 3.2, we analyze post-demolition household location to aid interpretation of this reduced-form parameter.

3.1 Baseline Balance, Attrition, and Spillovers

Estimates of β have a causal interpretation if we assume that CHA's selection of buildings for demolition was 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 waitlist, 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 non-displaced residents in terms of pre-demolition 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 non-displaced children in our sample, while Column 2 shows the mean difference between displaced and non-displaced children within a project (i.e., the coefficient from the regression). We find no statistically significant differences in terms of age, gender, pre-demolition juvenile arrests, or schooling outcomes.[11] Finally, since we examine parent voting outcomes in Section 5, we show in Column 5 that parents of children in our sample are also balanced on gender, labor

[10]As noted in Section 2, 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.

[11]Schooling results are reported for the sub-sample of 5,250 children who move at or after age 7 (i.e., the sample from Chyn (2018) for which schooling outcomes are reported). The data on schooling outcomes is not available for this study.

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 Section 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 Section B, we show that we obtain results in line with our main findings when we use an earlier vintage of voting records.

3.2 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 non-displaced 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 pre-trends 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 demoli-

[12]Of course, it is possible that non-displaced households may differ on unobserved dimensions. If non-displaced 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.

tion because non-displaced 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 non-displaced 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 post-displacement locations).

4 Results

We've 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 blue bars display the control group means, while the red bars correspond to the estimated voting rates of the displaced. We see that voting rates are low – between 5 to 10 percent – 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 percent for the control group in the 2008 election) and we thus have more power to detect differences. Table 1 reports these regression point estimates. Displaced children are 2.2 (12 percent), 2.8 (15 percent) and 2.0 (11 percent) 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 percent) percentage points more likely to ever vote in a general election and 1.8 (12 percent) 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 percent) 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 percent) 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 Non-Partisan.

5 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 post-treatment 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

[13] See Appendix Table B2 for more details.

as socialization and norms, rather than following directly from incarceration, parental outcomes, distance to polling stations, or neighborhood voting rates.[14]

5.1 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 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 post-demolition 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 12 percent ($0.025 \times 13 = 0.325$ 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 non-displaced 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

[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.

unlikely to be a mediator.

5.2 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 percent) 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 (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.

5.3 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 percent) 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

[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 percent prior to the demolitions. In the MTO experiment, the poverty rate in the baseline Census tracts was approximately 53 percent. This could matter if the effects of relocation and demolition are larger for adults from more disadvantaged neighborhoods.

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 percent (ever voted) suggests that high school graduation alone could increase their turnout to 81.0 percent. Thus, this calibration suggests that roughly 90 percent ($0.051 \times (0.810 - 0.301) = 2.6$ percentage points) of the 2.9 percentage point effect could be explained through education. If we use the bottom of their 90 percent confidence interval (0.51 probits), education could explain roughly 34 percent ($0.051 \times (0.495 - 0.301) = 1.0$ percentage points) of the effect.

5.4 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 (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 and 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.

5.5 *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 and Dawson, 1993; Desmond and 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 and 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 and Alwin, 1989; Sears and 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 (2018*a*), it's 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.

6 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 quasi-experimental approaches to provide credible evidence on the effects of neighborhoods on children (Kling et al., 2007; Damm and Dustmann, 2014; Chetty et al., 2016; Chetty and Hendren, 2018*a*,*b*; Chyn, 2018; Deutscher, 2020; Laliberte, 2021; Chyn and 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 percent) more likely to vote in any general election (up to 2018) relative to their non-displaced 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

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

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.

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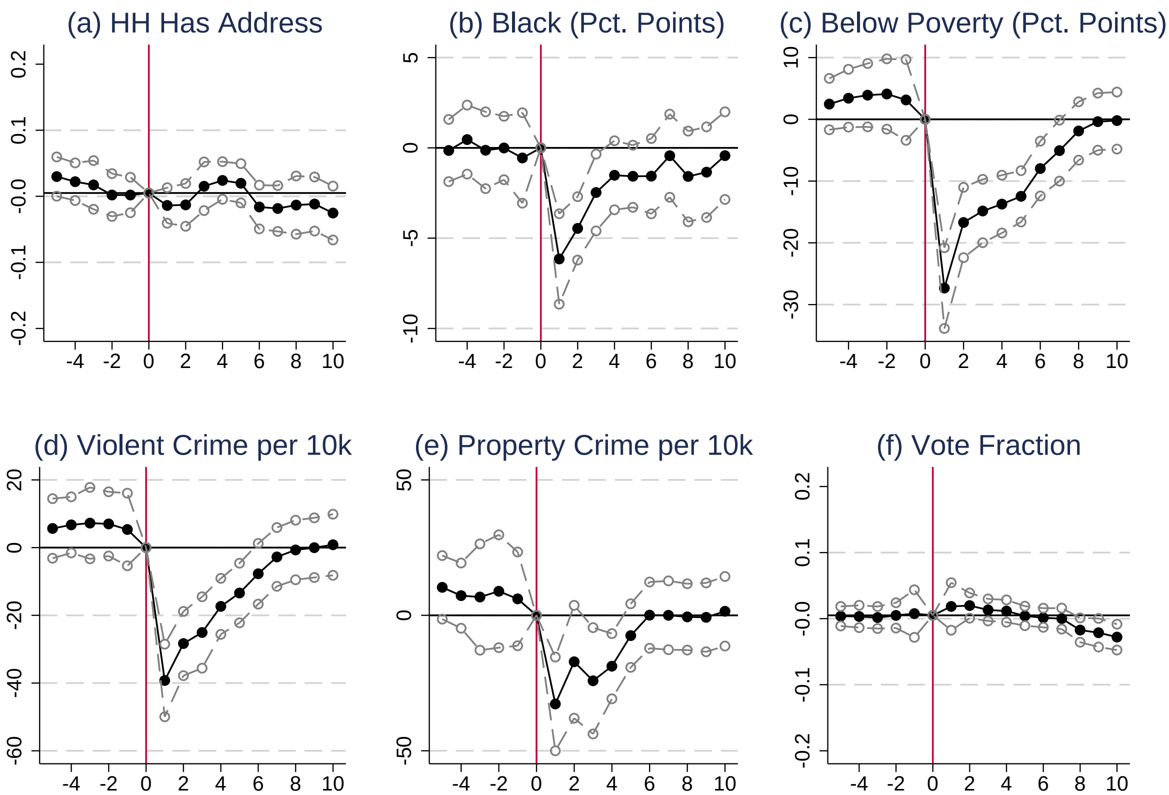
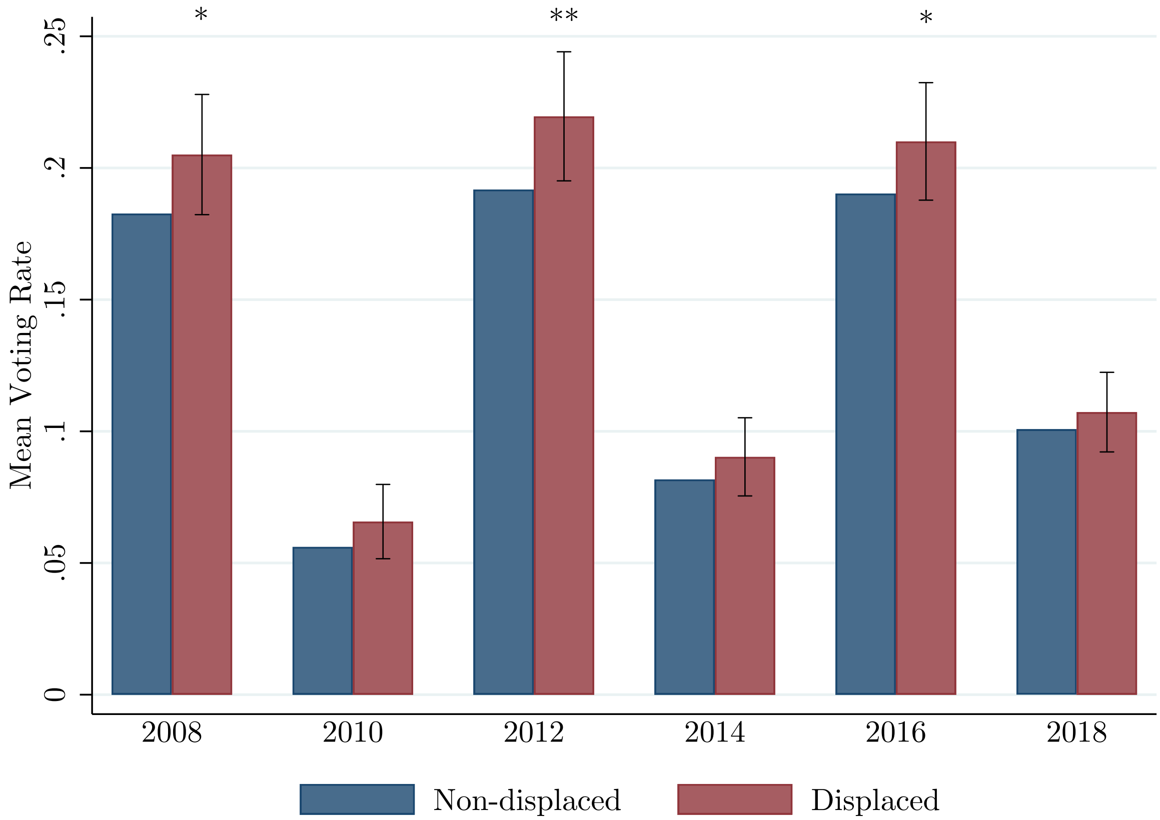
7 Figures and Tables

Figure 1: Impacts of Demolition and Relocation on Neighborhood Characteristics Over Time



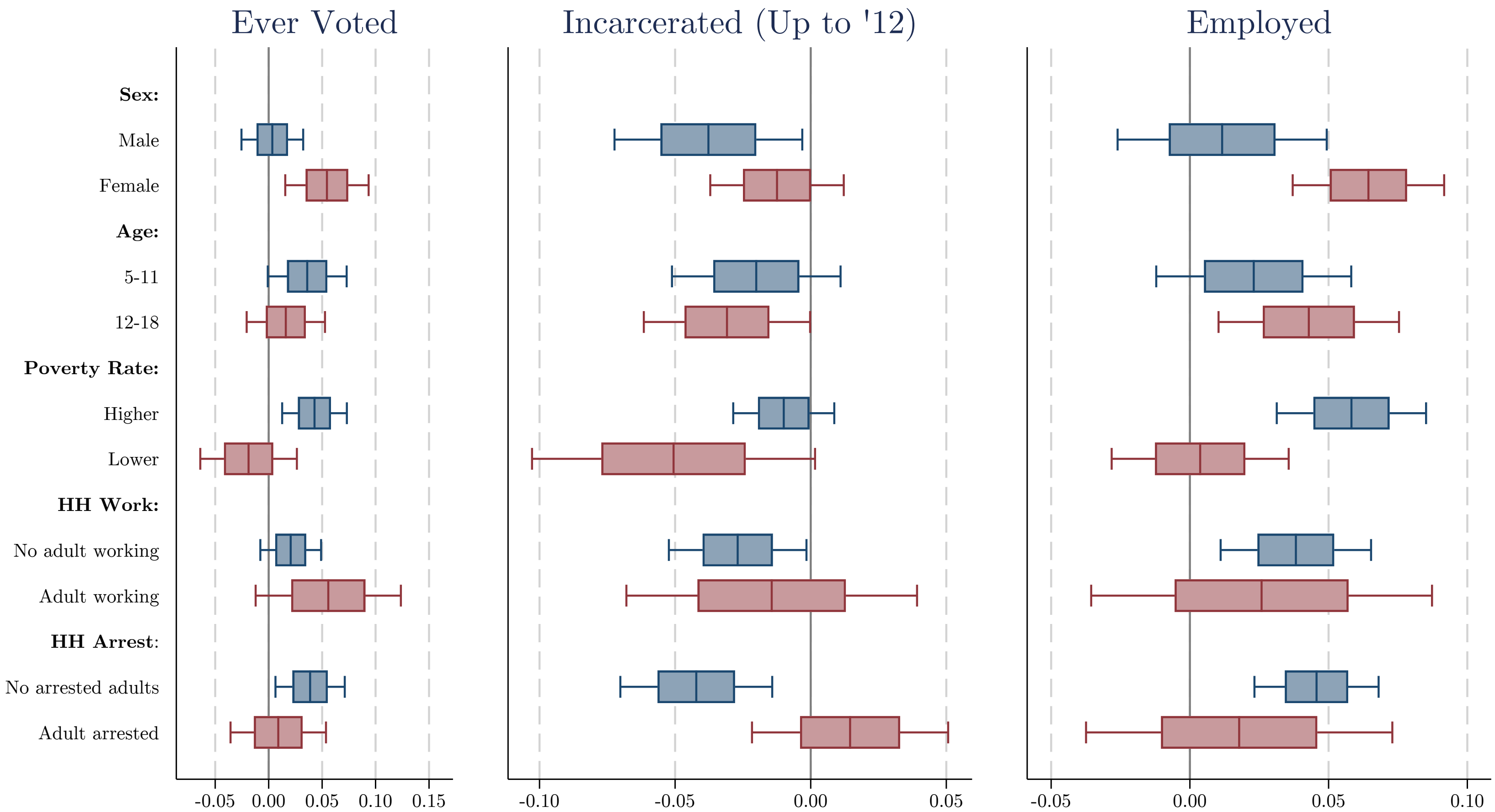
Notes: Panels show impacts of demolition on neighborhood (Census tract) characteristics over time. The unit of analysis is a household with at least one child. Neighborhood characteristics are based on the 1990 Decennial Census. Location is measured using address data from IDHS social assistance files. 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 non-displaced 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-percent confidence interval for the coefficients.

Figure 2: Impacts of Demolition and Relocation on General Election Voting



Notes: Bars display voting rates for the 2008–2016 general elections. The navy (left) bar for each election displays the mean voting rate for non-displaced (control) children. The maroon (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-percent confidence interval for each general election outcome. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Figure 3: Impacts of Demolition and Relocation on Voting, Earnings and Incarceration by Subgroup



Notes: 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-percent 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.

Table 1: Impacts of Demolition and Relocation on Long-run Voting of Children

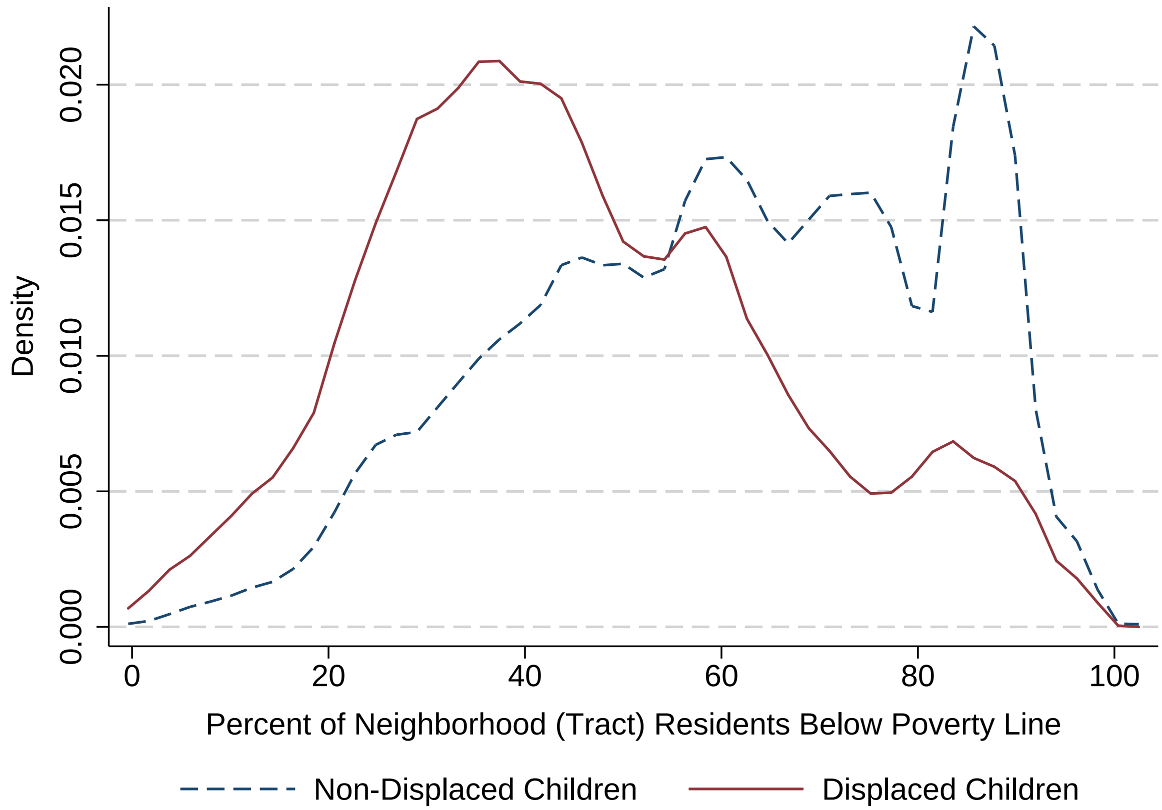
	(1)	(2)	(3)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Voting:			
Ever Voted, General	0.301	0.029**	5,933
		(0.014)	
Ever Voted, Primary	0.145	0.018*	5,933
		(0.010)	
Voted General, 2016	0.185	0.020*	5,933
		(0.011)	
Voted General, 2012	0.185	0.028**	5,933
		(0.013)	
Voted General, 2008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

Notes: This table analyzes adult voting outcomes for displaced (treated) and non-displaced (control) children. The control mean statistics in Column 1 refer to averages for non-displaced children. The mean difference between displaced and non-displaced 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 *** $p < 0.01$.

Online Appendix

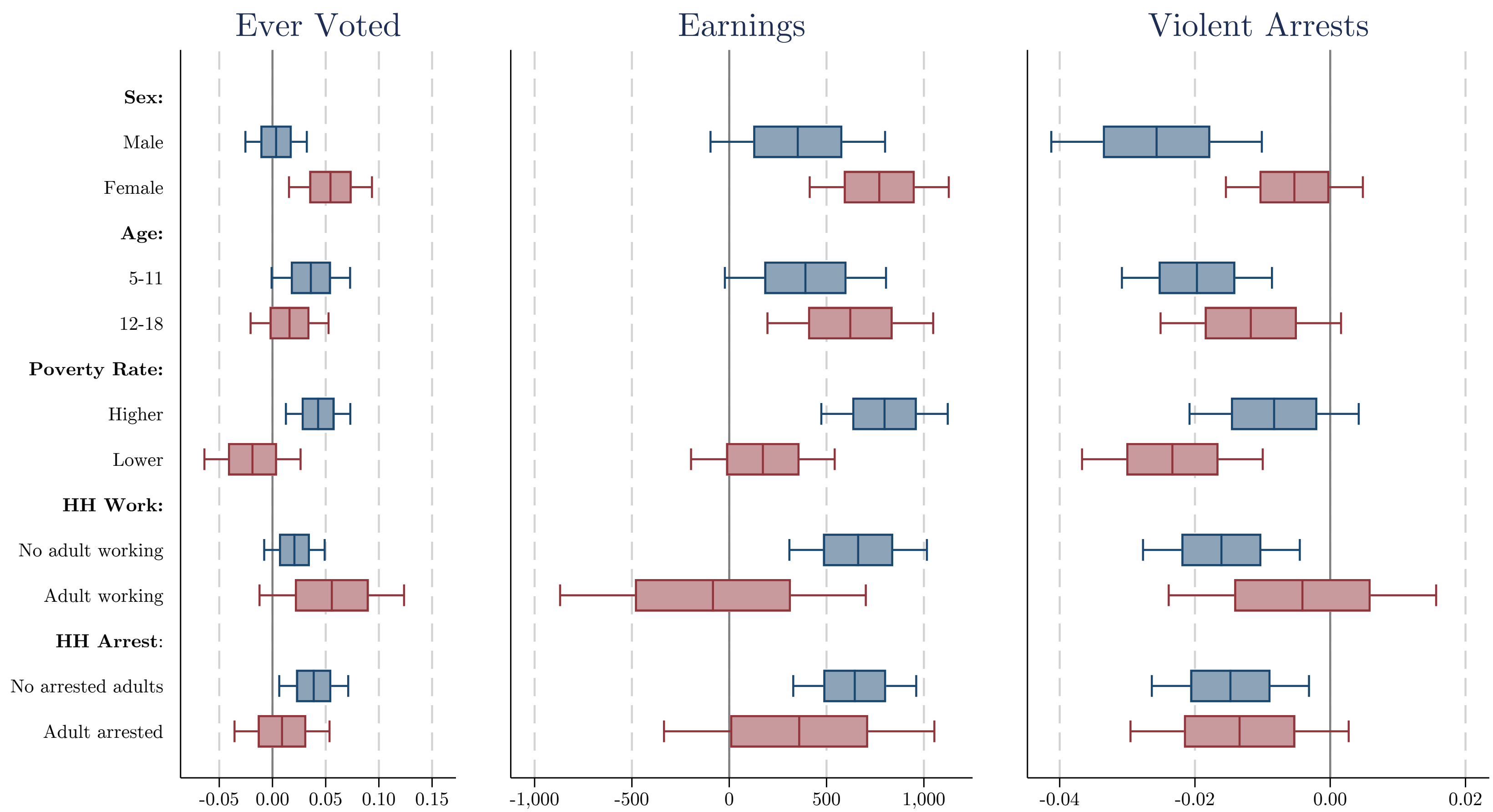
A Appendix Figures and Tables

Figure A1: Density of Neighborhood Poverty After Demolition



The figure shows statistics for the duration-weighted average poverty rate for each household in the sample ($N = 3,002$). We compute the average over all post-demolition locations (up to 2009) for the household regardless of whether a child is still present. Neighborhood poverty is based on the 1990 Decennial Census. Location is measured using address data from IDHS social assistance files.

Figure A2: Impacts of Demolition and Relocation on Voting, Employment and Violent Arrests by Subgroup



Notes: 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-percent confidence interval. The left and right ends of the boxes display the points that are one standard error above and below the point estimate.

Table A1: Comparison of Displaced and Non-displaced Adults and Children at Baseline (Prior to Demolition)

	Children			Adults (Parents)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Demographics						
Age	10.650	-0.104 (0.140)	5,933	28.898	0.706** (0.284)	4,290
Male	0.498	0.001 (0.013)	5,933	0.128	0.000 (0.010)	4,286
Arrests (Age $>$ 13)						
Violent	0.014	0.003 (0.007)	2,069	0.184	-0.014 (0.032)	4,178
Property	0.011	0.008 (0.009)	2,069	0.158	0.014 (0.020)	4,178
Drug	0.026	-0.005 (0.013)	2,069	0.166	0.031 (0.023)	4,178
Other	0.021	0.004 (0.008)	2,069	0.228	-0.014 (0.028)	4,178
Schooling†						
Enrolled CPS	0.948	0.003 (0.014)	5,250			
Reading Score	-0.443	0.024 (0.074)	2,519			
Math Score	-0.449	0.048 (0.061)	2,502			
Employment‡						
Employed				0.172	0.006 (0.016)	4,265
Earnings				1,501.820	-68.514 (197.028)	4,265

Notes: This table analyzes baseline characteristics for displaced (treated) and non-displaced (control) individuals. The control mean statistics in Columns 1 and 4 refer to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Columns 2 and 5. This difference is computed using a regression model where the baseline outcome (each row) is the dependent variable for individual i. The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. For the analysis of arrest and labor market outcomes, we exclude outliers (less than one percent). † Administrative data on employment begins in the first quarter of 1995. For individuals who experience a demolition in 1995, we use this quarter of earnings (scaled to an annual figure) to measure earnings prior to displacement because this quarter precedes demolition. ‡ Education outcomes are only available for the main sample in Chyn (2018) which examines children age 7-18 at baseline. See Section 2 and Appendix B for further details. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A2: Impacts of Demolition and Relocation on Neighborhood Characteristics, Three Years Post Displacement

	(1)	(2)	(3)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Has Address	0.789	0.011	3,002
		[0.019]	
Only HHs with Address			
Pct Black	95.182	-2.446**	2,297
		[1.085]	
Pct Below Poverty	64.093	-14.804***	2,297
		[2.615]	
Violent Crime Rate	69.327	-25.033***	2,253
		[5.374]	
Property Crime Rate	103.331	-24.235**	2,253
		[10.005]	

Notes: The table reports analysis of location and neighborhood characteristics. The unit of analysis is a household with at least one child. Neighborhood poverty is based on the 1990 Decennial Census. Location is measured using address data from IDHS social assistance files. The control mean statistics in Columns 1 refers to averages for non-displaced households. Using the model from Equation 1, the mean difference between displaced and non-displaced households three years after building demolition is reported in Columns 2 and 4. Robust standard errors are clustered at the public housing building level. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A3: Impacts of Demolition and Relocation on Measures of Attrition

	(1)	(2)	(3)
Post Demo. Year	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
1	0.011	-0.001	5,933
		(0.004)	
2	0.020	0.002	5,933
		(0.007)	
3	0.031	-0.003	5,933
		(0.008)	
4	0.038	0.005	5,933
		(0.009)	
5	0.044	0.010	5,933
		(0.011)	
6	0.054	0.011	5,933
		(0.011)	
7	0.067	0.001	5,933
		(0.013)	
8	0.079	0.002	5,933
		(0.015)	
9	0.093	-0.003	5,933
		(0.017)	
10	0.112	-0.008	5,933
		(0.019)	
11	0.131	-0.004	5,933
		(0.017)	
12	0.148	0.001	4,527
		(0.024)	
13	0.185	0.011	4,527
		(0.030)	
14	0.228	0.002	3,298
		(0.028)	

Notes: This table presents tests for differential attrition based on the administrative data for children. We follow Grogger (2013) and construct a measure of attrition based on terminal runs of zeros for a given outcome in a panel of observations for each child. We do this for In each time period t after demolition, we construct a measure of terminal zeros for employment, social assistance receipt (foodstamps, TANF or Medicaid), arrests and imprisonment. We aggregate across these outcomes to create a single measure of whether an individual in year t has a terminal string of zeros (up to 2009). For example, the first entry of Column 1 shows that 1.1 percent of the non-displaced children began a terminal run of zeros for all outcomes in the first year after demolition (up to 2009). Column 2 tests whether displaced and non-displaced youth have detectably different rates of attrition using Equation 1. Note that the sample size changes in post demolition years 12, 13 and 14 because some children are displaced in 1998 so they only have 11 years of post-demolition data.

Table A4: Spillover Test Results

	(1)	(2)	(3)	(4)
	Control Mean	Diff.: Displaced-Far, Within Est.	Diff: Near-Far, Within Est.	N
Voting:				
Ever Voted, General	0.292	0.034*	0.007	5,933
		(0.018)	(0.015)	
Ever Voted, Primary	0.148	0.014	-0.006	5,933
		(0.014)	(0.012)	
Voted General, 2016	0.173	0.029*	0.012	5,933
		(0.016)	(0.015)	
Voted General, 2012	0.180	0.031*	0.004	5,933
		(0.017)	(0.016)	
Voted General, 2008	0.171	0.026	0.006	5,933
		(0.017)	(0.014)	
Voted General, 2004	0.135	0.037*	0.012	3,364
		(0.019)	(0.016)	
Share of Pres. Elections Voted	0.161	0.031**	0.008	5,933
		(0.014)	(0.012)	
Registration:				
Registered	0.392	0.029**	0.017	5,933
		(0.014)	(0.014)	
Registered, Non-partisan	0.242	0.015	0.018	5,933
		(0.016)	(0.016)	
Registered, Republican	0.005	-0.001	0.000	5,933
		(0.002)	(0.002)	
Registered, Democrat	0.146	0.015	-0.000	5,933
		(0.013)	(0.011)	

Notes: This table presents tests of spillovers using Equation C2 from Section C. The independent variables in the regression model include an indicator for treatment (displaced) status, an indicator for living in a public housing building that borders (is adjacent to) a demolition targetted building, a set of project fixed effects, and controls for sex and race. The omitted group in the regression is the set of children living in stable buildings located in the "far" buildings that were not adjacent to demolished buildings. The control mean statistics in Column 1 refer to the averages for non-displaced individuals living in the group of far buildings. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A5: Impacts of Demolition and Relocation on Incarceration

	(1)	(2)	(3)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Ever Incarcerated (Up to 2012)	0.155	-0.025**	5,933
		[0.012]	
Incarcerated, 2010	0.043	-0.008	5,933
		[0.007]	
Incarcerated, 2008	0.049	-0.007	5,933
		[0.008]	
Incarcerated, 2004	0.044	-0.010	3,364
		[0.009]	
Share of Pres. Election Years in Jail	0.047	-0.005	5,933
		[0.006]	

Notes: This table analyzes incarceration outcomes for displaced (treated) and non-displaced (control) children. The control mean statistics in Column 1 refer to averages for non-displaced children. The mean difference between displaced and non-displaced 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 *** $p < 0.01$.

Table A6: Impacts of Demolition and Relocation on Voting of Parents

	(1)	(2)	(3)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Voting:			
Ever Voted, General	0.371	0.010	4,290
		(0.011)	
Ever Voted, Primary	0.257	0.008	4,290
		(0.012)	
Share of Pres. Elections Voted	0.249	0.000	4,290
		(0.010)	
Voted General, 2016	0.277	0.002	4,290
		(0.015)	
Voted General, 2012	0.276	0.001	4,290
		(0.013)	
Voted General, 2008	0.264	0.008	4,290
		(0.011)	
Voted General, 2004	0.235	-0.006	4,290
		(0.013)	
Registration:			
Registered	0.414	0.028**	4,290
		(0.011)	
Registered, Non-partisan	0.143	0.017	4,290
		(0.011)	
Registered, Republican	0.004	0.002	4,290
		(0.002)	
Registered, Democrat	0.267	0.009	4,290
		(0.012)	

Notes: This table analyzes adult voting outcomes for displaced (treated) and non-displaced (control) parents. The control mean statistics in Column 1 refer to averages for non-displaced parents. The mean difference between displaced and non-displaced 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 *** $p < 0.01$.

Table A7: Impacts of Demolition and Relocation on Voting by Subgroup

	(1)	(2)	(3)	(4)
Subgroup	Fraction of All Children	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
All	1.00	0.301	0.029**	5,933
			(0.014)	
Sex				
Male	0.49	0.201	0.003	2,939
			(0.015)	
Female	0.51	0.400	0.055**	2,994
			(0.020)	
Age at Baseline				
5-11	0.58	0.281	0.036*	3,464
			(0.019)	
12-18	0.42	0.331	0.016	2,469
			(0.019)	
Poverty Rate				
Higher	0.80	0.296	0.043**	4,760
			(0.015)	
Lower	0.20	0.320	-0.019	1,173
			(0.023)	
HH Adult Employment				
No Working Adults	0.83	0.300	0.021	4,887
			(0.014)	
>0 Working Adults	0.17	0.306	0.056	1,046
			(0.035)	
HH Past Arrests				
No Adults with Arrest(s)	0.69	0.295	0.039**	4,061
			(0.017)	
>0 Adults with Arrest(s)	0.31	0.314	0.009	1,872
			(0.023)	

Notes: Subgroups are based on baseline (the year prior to relocation due to demolition) characteristics. The control mean statistics in Column 2 refer to averages for non-displaced individuals. The specification includes indicators for treatment interacted with subgroup membership indicators and project fixed effects. Results by baseline neighborhood poverty rate are based on dividing the sample into a group of children residing in "higher poverty projects" where the poverty rate was 87 percent and a group of children residing in "lower poverty projects" where the poverty rate was 66 percent. Robust standard errors are clustered at the public housing building level. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A8: Impacts of Demolition and Relocation on Distance to Polling Paces During Adulthood

	(1)	(2)	(3)
Fraction of Adult Years with Address	0.599	-0.007	5,933
		[0.021]	
Has Address at Age 18	0.756	0.014	5,617
		[0.023]	
Only Persons with Adulthood Address			
Distance to Nearest Polling (Miles)	0.204	-0.017*	4,646
		[0.009]	
Distance to Nearest Polling (Miles) at Age 18	0.207	-0.010	4,018
		[0.010]	
Voting Rate	0.666	0.000	4,344
		[0.005]	
Voting Rate at Age 18	0.662	-0.000	3,327
		[0.006]	

Notes: The table reports analysis of measures for the distance to nearest polling places and neighborhood voter turnout rates. Location is measured using address data (up to 2009) from IDHS social assistance files. We focus on addresses for displaced and non-displaced children during their adulthood (age > 18). We geocode the address and calculate the distance to the nearest polling station. The polling place data are for 2016 locations in Illinois from Chen et al. (2019). Voter turnout rates are based on data from the Public Mapping Project (Altman and McDonald, 2021). The control mean statistics in Columns 1 refers to averages for non-displaced households. Using the model from Equation 1, the mean difference between displaced and non-displaced households three years after building demolition is reported in Columns 2. Robust standard errors are clustered at the public housing building level. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

B Description of Data, Sample and Linking Process

The analysis in this paper is based on studying a sample of public housing residents that have been linked to the 2019 Illinois (IL) voterfile purchased from the vendor L2, Inc., a non-partisan firm that supplies voter data to candidates, political parties, and others. The process for creating this data consists of two main steps:

1. **Cleaning the sample of public housing residents:** Chyn (2018) created a sample of public housing residents (adults and children) to study long-run impacts of public housing demolition on labor market and criminal justice outcomes. As summarized in Section 3 and Chyn (2018), this sample was created by matching the street addresses of 53 demolition-affected public housing buildings (including demolished and non-demolished buildings) with social assistance case files from the Illinois Department of Human Services (IDHS). This IDHS data is a list of all Cook County (which includes Chicago) cases for beneficiaries who received social assistance services (TANF, Foodstamps or Medicaid) from 1994 to 1997. These case files are associated with 992,729 individuals (463,542 are recipients ("grantees") while 529,187 are individuals living in the same household). With the merged data, we look for social assistance cases where the household (grantees and the other individuals listed on a case) had an address matched to a demolition affected public housing address in the year prior to building closure. Note that this process includes identifying individuals living in the set of non-demolished buildings in the year before a building closure for demolition occurs in their housing project. This focus on the year before building closure ensures the definition for the sample of child households is unrelated to any impact that demolition has on public assistance participation. We estimate that the assistance records covers at least 73 percent of the households living in the demolition sample of buildings.[B1] The process results in a preliminary sample that contains 6,135 children ages 5 to 18 that lived in public housing in the year before demolition.[B2] To link this data to voting records, we define a matching set of variables as first name, last name and date of birth. There are 202 children (3.3 percent) who have non-unique or missing information in terms of the matching set of variables. We drop these children with duplicated matching variable information. The remaining 5,933 children are the main sample for the voting analysis in this paper.

2. **Linking public housing residents to the L2 voter records:** We link the sample of 5,933 public housing children to voter records provided by L2, Inc. Voting data from L2 has been used in prior research (Velez and Newman, 2019; Yoder, 2019; Enamorado et al., 2019). We obtained state-specific voting records from Illinois and neighboring states (i.e., Indiana, Iowa, Kentucky, Michigan, Missouri, and Wisconsin). L2 obtains a snapshot of voting records directly from state voting authorities (e.g., the Illinois Secretary of State office). All L2 voter files include voter registration information for the full state, as well as voter turnout in the 2000-2018 general and primary elections. In addition to the voter file, L2 supplements this data with additional commercial fields, though for the purposes of this paper we just use fields

[B1] The social assistance data contain 5,677 distinct households (including those without children ages 5-18) who lived in public housing in the year before building closure. Since the sample of public housing buildings contains 7,770 individual apartments, this suggests that the assistance sample covers at least 73 percent of the households living in the demolition sample of buildings. Note that this estimate is likely a lower bound as the calculation assumes that there are no vacant apartments.

[B2] The sample of 6,135 children ages 5 to 18 living in public housing was originally created for Chyn (2018). Records for children less than age 5 were not retained because these children were too young to be in the labor market in 2009, which was the latest year contained in the employment data used in Chyn (2018).

provided by the State (as well as an L2-modeled party affiliation variable). Specifically, we use the full name, date of birth, indicators for whether the individual voted in each national election, and a modeled variable indicating the party of the voter.[B3] Both L2 and the State routinely clean the voter file and remove individuals who are either deceased or moved (based on the National Change of Address). As an example, we will not observe an individual in the records from Illinois if they voted in Illinois in 2014 but moved between 2015 to 2019 (provided that their move was recorded in the National Change of Address). However, if they moved to and voted in one of the six bordering states, they will show up in our voter file sample. To link the sample of public housing children and the voting records, we use first name, last name and date of birth. Prior studies also use name and date of birth to link administrative records and voting records. For example, Baicker and Finkelstein (2018) use full name, date of birth and gender to link data from the Oregon Health Experiment to voting records. Akee et al. (2018) use first name, last name and date of birth to link the Great Smoky Mountains Study survey data to voting records. Holbein (2017) use first name, last name and birthday to match individuals who participated in the Fast Track intervention to voter records. Our linking based on first name, last name and date of birth results in 2,227 public housing children (41 percent) that can be linked to the L2 voting records.[B4,B5] There are 9 children who link to two distinct registration records in the voter files from Illinois and the surrounding border states. For these 9 cases, we randomly selected one of the two linked voter records to retain for our analysis.

Finally, the remainder of this section provides additional background information for our analysis. Specifically, Tables B1 and B2 below summarize the data sources and key variables used in this study. In addition, Tables B3 and B4 list the demolition (treated) and comparison group buildings used in this paper. The date of building closure for each treated building is taken from Jacob (2004). After the initial wave of public housing demolitions (1995-1998), the CHA subsequently demolished comparison group buildings. Note that Table B4 provides dates of later demolition for the comparison group buildings based on CHA administrative data.

[B3] Since Illinois (and many other states) do not record the party of a voter, L2 provides a modeled field based on voting in partisan primaries. Specifically, they use the most recent even year primary in which a voter cast a partisan ballot. For example, if an individual voted in the Democrat primary in 2018 and the Republican primary in 2016, they will be recorded as a Democrat. If the voter has participated in no primaries (or most recently voted in a primary outside the Democratic or Republican parties), then she will be recorded as Non-Partisan. See Appendix Table B2 for more detail on this partisanship variable in Illinois as well as the other states.

[B4] The sample of public housing children are ages 29 to 42 in 2019. To provide some relevant points of comparison, the Current Population Survey November Supplement provides self-reported registration rates by background characteristics. This data shows the self-reported registration rate for persons between 25-34 in Illinois is 55.5 percent (63.1 percent for U.S. citizens specifically) (U.S. Census, 2018*a*). While our match rate of 41 percent is lower than the statewide self-reported voter registration rate, it is also important to note our sample is composed of individuals from particularly disadvantaged backgrounds. Prior research shows that voting and political behavior in the U.S. is strongly related with income (Erikson, 2015). Moreover, black voters living poor neighborhoods are much less likely to be politically active relative to similar poor residents of more affluent neighborhoods (Cohen and Dawson, 1993; Alex-Assensoh, 1998). The CPS data shows registration rates for US citizens between the ages of 25-44 with family incomes under \$10,000 at 54.3 percent and for incomes of \$10,000 to \$14,999 at 49.1 percent (U.S. Census, 2018*b*). Finally, one key caveat to the CPS registration rates – similarly noted by Akee et al. (2018) – is that they may be inflated by social desirability bias, as they are based on survey reports.

[B5] We also explored an alternative linking based on probabilistic matching methods. Reassuringly, we obtain similar results when we study a sample constructed using probabilistic matching methods. See Section B.1 for details.

Table B1: List of Original and Intermediate Data Files

#	File Name	Notes
1	Chicago Housing Authority: Building Address and Occupancy Files	Building addresses for all buildings in the Chicago Housing Authority inventory during the 1990s. Obtained from Brian Jacob.
2	Sample of Demolished and Non-Demolished Public Housing Building Addresses	Created from File #1 based on Jacob (2004) sample definition. Details on construction described in the main text.
3	IDHS Social Assistance Case Files from 1994-1997	List of all recipients (grantees and household members) of social assistance services (TANF, SNAP or Medicaid) from 1994 to 1997 in Cook County.
4	Sample of IDHS Recipients Living in Demolished and Non-demolished Public Housing Addresses	Created from File #3. Note that the sample is defined based on public housing demolitions that occurred from 1995-1998.
5	ISP Crime Records	Comprehensive criminal justice data (recorded at the person and date level) up to 2010. Type of offense details included.
6	IDES Unemployment Insurance Records	Quarterly earnings data from 1995-2009.
7	IDHS Social Assistance Files	Monthly (TANF, SNAP, Medicaid) participation from 1989-2010 for Cook County residents on social assistance at some point during 1994-1997.
8	L2 Voting Files	Records are from state voting authorities in Illinois, Indiana, Iowa, Kentucky, Michigan, Missouri, and Wisconsin. These files include voter registration information for each state, as well as voter turnout in the 2000-2018 general and primary elections.
9	Main Analysis File	Person-level observations for the sample of displaced and non-displaced public housing persons (i.e., File #4). Each observation includes post-demolition voting-related measures from the files listed in #8.

Table B2: List of Key Variables

#	Variable	Details
1	Ever Voted, General	Indicator equal to 1 if the individual ever voted in any general election from 2000-2018.
2	Ever Voted, Primary	Indicator equal to 1 if the individual ever voted in any primary election from 2000-2018.
3	Voted General, 2016	Indicator equal to 1 if the individual voted in the 2016 general election.
4	Voted General, 2012	Indicator equal to 1 if the individual voted in the 2012 general election.
5	Voted General, 2008	Indicator equal to 1 if the individual voted in the 2008 general election.
6	Voted General, 2004	Indicator equal to 1 if the individual voted in the 2004 general election.
7	Share Pres. Elections Voted	Share of the presidential elections that the individual voted in during the period 2000-2016. Note that the measure only consider elections in which an individual was at least 18 years old. For example, if an individual was born in 1990, they would not have been eligible to vote in the 2004 presidential election, and we do not include this election in the measure.
8	Registered	Indicator equal to 1 if the individual was listed as a registered to vote in any of the L2 records available for our study.
9	Registered, Democrat	Indicator equal to 1 if the individual was coded by L2 as being affiliated with the Democratic Party. In Iowa and Kentucky, this party affiliation is directly self-reported on the voter registration form. In Missouri and Wisconsin, L2 models this based "on a great many public and private data sources including demographics available through the voter file, exit polling from presidential elections, commercial lifestyle indicators, census data, self-reported party preferences from private polling and more." In Michigan, L2 uses similar modeling as in Missouri, but also uses the measurement of partisan ballots in the 2016, 2012, and 2008 Presidential primary elections (before these were deleted from the state voter file). In Indiana and Illinois, affiliation is based on the most recent even year primary where a voter cast a partisan ballot (i.e., it equals 1 if the individual voted in the Democratic primary).
10	Registered, Republican	Indicator equal to 1 if the individual was coded by L2 as being affiliated with the Republican Party (same methodology as "Registered, Democrat").
11	Registered, Non-partisan	Indicator equal to 1 if the individual is a registered voter and was not coded by L2 as a Democrat or Republican.

Table B3: Treated Demolition Buildings and Dates of Building Closure

Project Name	Building #	Closure due to Demolition Date (Jacob, 2004)
Ida B. Wells Homes	1	1-Sep-95
Ida B. Wells Homes	3	1-Sep-95
Madden Park	10	1-Sep-95
Madden Park	11	1-Sep-98
Robert Taylor Homes	28	1-Sep-98
Robert Taylor Homes	10	1-Sep-98
Robert Taylor Homes	11	1-Sep-98
Robert Taylor Homes	21	1-Sep-98
Robert Taylor Homes	1	1-Sep-95
Robert Taylor Homes	4	1-Sep-98
Robert Taylor Homes	25	1-Sep-98
Robert Taylor Homes	16	1-Sep-98
Robert Taylor Homes	17	1-Sep-98
Robert Taylor Homes	20	1-Sep-98
Rockwell Gardens	1	1-Sep-98
Rockwell Gardens	2	1-Sep-98
Stateway	4	1-Sep-96
Washington Park	26	1-Sep-95
Washington Park	85	1-Sep-95
Washington Park	44	1-Sep-95

Notes: Building closure dates come from Jacob (2004) and are based on CHA administrative records.

Table B4: Comparison Group Buildings and Subsequent Demolition Dates

Project Name	Building #	Demolition Date
Ida B. Wells Homes	4	7-Jul-09
Ida B. Wells Homes	5	7-Jul-09
Ida B. Wells Homes	7	7-Jul-09
Ida B. Wells Homes	8	7-Jul-09
Ida B. Wells Homes	9	7-Jul-09
Ida B. Wells Homes	10	7-Jul-09
Madden Park	9	14-Sep-02
Robert Taylor Homes	6	30-Apr-03
Robert Taylor Homes	7	30-Apr-03
Robert Taylor Homes	5	30-Apr-06
Robert Taylor Homes	27	27-Aug-01
Robert Taylor Homes	9	30-Apr-05
Robert Taylor Homes	13	30-Sep-02
Robert Taylor Homes	14	30-Apr-02
Robert Taylor Homes	2	30-May-04
Robert Taylor Homes	3	30-May-03
Robert Taylor Homes	24	15-Oct-02
Robert Taylor Homes	26	30-May-03
Robert Taylor Homes	18	5-Apr-04
Robert Taylor Homes	19	30-Apr-03
Robert Taylor Homes	12	26-Apr-07
Rockwell Gardens	4	2-Jun-06
Rockwell Gardens	6	12-Jul-06
Stateway	5	11-Sep-07
Stateway	6	30-Sep-02
Stateway	7	30-Sep-02
Stateway	8	30-May-03
Stateway	9	5-Apr-04
Stateway	1	23-Jul-02
Stateway	3	30-May-03
Washington Park	35	30-Apr-07
Washington Park	42	15-Oct-02
Washington Park	65	30-Apr-03

Notes: Date of demolition taken from CHA administrative records.

B.1 Robustness to Alternative Linking for the Sample Construction

For our main analysis, we rely on a sample that was created based on linking children in social assistance records to the voting records using first name, last name and exact date of birth. In this subsection, we demonstrate that we obtain similar results when we study a sample constructed using an alternative linking process based on probabilistic matching methods. One concern for this approach is that we could potentially obtain false matches due to using a less restrictive matching criteria. Using this alternative linking process, we create a sample with a match rate of 45 percent. Table B5 reports the results for our main voting outcome analysis using this alternative sample. Reassuringly, we find similar results for the impacts on all voting outcomes. For example, we find that the impact of demolition and relocation is a positive 3.5 percentage points impact on the likelihood of ever voting in a general election (2000-2018). In our main estimates (based on exact matching), the corresponding point estimate is 2.9 percentage points.

Table B5: Alternative Sample: Impacts of Demolition & Relocation on Long-run Voting of Children

	(1)	(2)	(3)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Voting:			
Ever Voted, General	0.348	0.035**	5,933
		(0.015)	
Ever Voted, Primary	0.168	0.023*	5,933
		(0.013)	
Voted General, 2016	0.217	0.022*	5,933
		(0.012)	
Voted General, 2012	0.214	0.035**	5,933
		(0.014)	
Voted General, 2008	0.207	0.020	5,933
		(0.012)	
Share of Pres. Elections Voted	0.198	0.027**	5,933
		(0.010)	
Share of General Elections Voted	0.135	0.016**	5,933
		(0.008)	
Registration:			
Registered	0.448	0.023*	5,933
		(0.012)	
Registered, Non-partisan	0.272	0.009	5,933
		(0.013)	
Registered, Republican	0.005	-0.000	5,933
		(0.002)	
Registered, Democrat	0.171	0.016	5,933
		(0.014)	

Notes: This table presents results based on an alternative sample where we link records using probabilistic matching methods. The table analyzes adult voting outcomes for displaced (treated) and non-displaced (control) children. The control mean statistics in Column 1 refer to averages for non-displaced children. The mean difference between displaced and non-displaced 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 *** $p < 0.01$.

B.2 Robustness to Using Historical Voting Records (Illinois Only)

For our main analysis, we rely on a sample that was created based on linking children in social assistance records to the voting records as of 2019. In this subsection, we demonstrate that our main conclusions remain when we study a sample constructed by linking our data to voting records from 2013. Note that due to data limitations, we can only produce this analysis for Illinois. In this way, this section differs from our main analysis which is based on using voting records from Illinois *and* its six bordering states.

Table B6 provides a comparison of the results from analyzing the two samples created by linking to the 2013 and 2019 Illinois voting records, respectively. Reassuringly, we find similar results for the impacts on all voting outcomes regardless of the vintage of voting records. For example, we find that the impact of demolition and relocation is a positive 4.2 percentage points impact on the likelihood of ever voting in a general election (2000-2018) using the 2013 records. In the sample based on 2019 records, the corresponding point estimate is 3.3 percentage points.

Table B6: Alternative Voting Records from Illinois: Impacts of Demolition & Relocation on Long-run Voting of Children

	2013 Illinois Voter File			**2019 Illinois Voter File**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N	Control Mean	Diff.: Displaced-Non-displaced, Within Est.	N
Voting:						
Ever Voted, General	0.286	0.042**	5,933	0.283	0.033**	5,933
		(0.018)			(0.014)	
Ever Voted, Primary	0.069	0.013*	5,933	0.142	0.020*	5,933
		(0.008)			(0.010)	
Voted General, 2012	0.225	0.040**	5,933	0.177	0.028**	5,933
		(0.016)			(0.012)	
Voted General, 2008	0.180	0.022	5,933	0.172	0.022*	5,933
		(0.014)			(0.011)	
Share of Pres. Elections Voted	0.178	0.036***	5,933	0.162	0.027***	5,933
		(0.013)			(0.010)	
Share of General Elections Voted	0.123	0.023**	5,933	0.110	0.016**	5,933
		(0.009)			(0.007)	
Registration:						
Registered	0.383	0.025	5,933	0.371	0.020*	5,933
		(0.017)			(0.012)	
Registered, Non-partisan	0.319	0.012	5,933	0.231	0.005	5,933
		(0.017)			(0.012)	
Registered, Republican	0.000	0.001	5,933	0.004	-0.002	5,933
		(0.001)			(0.001)	
Registered, Democrat	0.065	0.012*	5,933	0.136	0.016*	5,933
		(0.007)			(0.009)	

Notes: This table presents results based on two alternative samples that we created by linking the main sample of children to 2013 (Columns 1-3) and 2019 (Column 4-6) voting records from Illinois. The table analyzes adult voting outcomes for displaced (treated) and non-displaced (control) children. The control mean statistics in Columns 1 and 4 refer to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Columns 2 and 5. 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 *** $p < 0.01$.

C Attrition and Spillovers

As discussed in Section 3, it is important to examine two possible threats to the identification and the interpretation of our estimates. First, one potential issue is sample attrition. The data that we use allows us to observe individual registration and voting outcomes as long as a person lives in Illinois or one of the six bordering states. A concern is that our estimates will be biased if displaced children are more likely to move to a state that is not captured in the voting records we use. In this case, our data would suffer from a missing data problem: an individual will be recorded as not being registered or having voted even if they are politically active in a new state of residence.

There are at least two reasons why attrition due to moving out of Illinois may not be a concern for our analysis. First, as documented in Chyn (2018), an analysis of National Student Clearinghouse data shows that 3.5 percent of the demolition sample ever attends a two- or four-year out of state university. There are no detectable differences between displaced and non-displaced children in the out-of-state attendance rate. Second, Jacob et al. (2015) study children in Chicago whose households lived in private market housing and won a housing voucher. As part of their analysis, they used address data from the National Change of Address (NCOA) registry and national credit bureau checks. They found that 86 percent of children and their households were still living in Illinois after about 15 years.

In addition to this evidence, we follow a standard approach in the literature to study attrition due to migration out of Illinois. Specifically, we follow Grogger (2013) and impute attrition A using various administrative sources. This measure of attrition is straightforward and is based on observing terminal runs of zeros. Permanent attrition at time t implies that an outcome is zero subsequently (i.e. $Y_{i,t+j} = 0 \; \forall \; j \quad \in \{1, \ldots, T-t\}$, where Y is an administrative data outcome and T denotes the last unit of time in the data). For a single outcome k, we measure attrition by creating a binary indicator of a d-period run of zeros as:

$$a^k_{i,t}(d) = \mathbf{1}\left(\sum_{j=0}^{d-1} Y^k_{i,t+j} = 0\right).$$

Administrative data for the K-many outcomes available across administrative sources can be pooled and attrition can measured as:

$$a_{i,t}(d) = \mathbf{1}\left(\sum_{j=1}^{K} a^k_d = K\right).$$

In what follows, we use the following compact notation: $a^k_{i,t} \equiv a^k_{i,t}(d)$ and $a_{i,t}(d) \equiv a_{i,t}$.

Appendix Table A3 reports our analysis of attrition. In summary, we find no evidence that displaced children are more likely to move out of Illinois.[C1] The attrition measure that we use is based on pooling separate measures of attrition using data on employment, social assistance receipt (foodstamps, TANF or Medicaid), arrests and imprisonment. We measure attrition in each year t after demolition (up to 2009). For example, the first entry of Column 1 shows that 1.1 percent of the non-displaced children began a terminal run of zeros for all outcomes in the first year after demolition (up to 2009). Column 2 tests whether displaced and non-displaced youth have detectably different rates of attrition using Equation 1. Note that the sample size changes in post demolition years 12, 13 and 14 because some children are displaced in 1998 so they only have 11 years of

[C1] Note that we can also use the voting records from states bordering Illinois to assess whether displaced children are more likely to move out of Illinois. We find no evidence that demolition and relocation has a detectable impact on the likelihood of matching to a border state voting record.

post-demolition data.

Second, another plausible concern is that the demolition could have affected the long-term political participation of the control group. This spillover threat seems particularly plausible if the mechanism by which demolition affects participation is a psychological channel such as conveying a message about the quality of government services or encouraging a feeling of powerlessness (Baicker and Finkelstein, 2018; Soss, 1999; Schneider and Ingram, 1993). Such an "interpretive effect" may operate, as the control group would see homes similar to their own demolished, but would be unable to move through the housing vouchers made available to their neighbors.[C2]

To test for this threat, we assume that social interactions between buildings (and thus, spillovers) are decreasing with distance, and compare children who lived in control group buildings close to the demolished ones with those in the control group who lived further away. Formally, we implement this test by augmenting Equation 1 with additional indicators for living in a control building that is adjacent to a demolition building:

$$y_i = \alpha + \beta' D_{b(i)} + \pi N_{b(i)} + X_i' \theta + \psi_{p(i)} + \epsilon_i, \tag{C2}$$

where $N_{b(i)}$ is an indicator that a public housing building borders (is adjacent to) a demolition-targeted building. The omitted group in the regression is the set of children living in stable buildings located farther away from a demolished building. Appendix Table A4 reports results that show that we find no evidence of spillovers given that the analysis fails to reject the null $\pi = 0$ across voting outcomes, and the estimates of $\widehat{\pi}$ are generally small in magnitude.[C3]

[C2] Prior work in political science has considered the potential role of spatial spillovers. Sinclair et al. (2012) find no evidence of spatial spillovers due to interpersonal communication in a large-scale voter mobilization experiment.

[C3] Moreover, we find the treatment effects of demolition and relocation (i.e., estimates of β) on voting are similar to the main results that we obtain with Equation 1.

D Voting Access and Participation Related Mechanisms

In this section, we explore two potential mechanisms related to voter access and participation rates in one's adulthood neighborhood. First, we consider a proxy for the ease of voting. In particular, prior research suggests that distance to polling places causally reduces voting participation (Cantoni, 2020). As in the neighborhood characteristics analysis, we use an annual panel of addresses (up to 2009) from social assistance records. We geocoded these addresses and compute distance to polling places using 2016 polling locations from Chen et al. (2019). Our main outcomes are the distance to the nearest polling station at age 18 and the average distance to the nearest polling place during adulthood (up to 2009). Appendix Table A8 shows that displaced and non-displaced children generally live similar distances to polling stations during adulthood. While the point estimate for average distance during adulthood years is negative and statistically significant at the 10 percent level, the magnitude indicates that displaced children live just 0.017 miles closer to the nearest polling station. Focusing on distance at age 18, the results show no statistically significant impacts of demolition and relocation. Relative to prior studies, this effect on average distance is too small to explain the voting impacts. The largest estimate in Cantoni (2020) suggests that a one-mile increase in distance to polling stations decreased turnout by 14.5 percentage points in the 2012 Presidential Election. Based on this prior evidence and our point estimate, we would expect that this could explain at most 9 percent ($= 0.017 \times 14.5 = 0.25$ percentage points) of the 2.9 percentage point effect.

Second, we consider a mechanism related to social networks. Moving to higher opportunity neighborhoods likely has an effect on the composition of one's social network later in life.[D1] If, for example, one's friends are more likely to vote, then this peer effect could directly affect one's voting propensity. Note that while this is related to the socialization channel discussed in Section 5.5, it's possible that peer effects could operate over a shorter time horizon (e.g., even if one's preferences around voting are unchanged by the slow process of socialization, having friends who vote on Election Day could change turnout by, for example, allowing one to carpool to the polling place). We unfortunately do not have access to social network data; however, we can investigate a proxy based on the voter turnout rate for one's neighborhood in adulthood. Specifically, we use a voting rate measure based on data from the Public Mapping Project (Altman and McDonald, 2021). Note that the measure combines totals from the 2008 Presidential Election precinct-level voter turnout data and estimates for the Voting Age Population from the Census.[D2] As shown in Table A8, we find no significant treatment effect on this measure of voter engagement.

It bears noting that both mechanisms in this section are potentially noisy proxies of the underlying constructs of interest. For example, the voting rate in one's neighborhood might be a poor measure of the voting rate in one's social network. Thus, while this section is suggestive that these mechanisms are not primary, the conclusion remains tentative.

[D1] Gay (2012) also explores social networks as an explanation for their results. She finds evidence that the moves in adulthood could have disrupted one's social ties and thereby diminished one's civic engagement. The effects of moves in childhood on the strength of social ties in adulthood, however, is less clear. While the strength of ties may be ambiguous, it is plausible that the (demographic or otherwise) composition of one's eventual adult network are affected by the displacement.

[D2] Specifically, the Public Mapping Project constructed block-level measures of the numerator and denominator of this voting rate. Given that vote totals are observed at the precinct level, the authors first apportioned vote totals among blocks according to the share of voting-age population of each block within each precinct. We sum these block-level measures of the numerator (vote total) and the denominator (voting-age population) to the Census Tract and then divide to produce a Census Tract-level voting rate measure. We do this to match the definition of neighborhood used throughout the paper (Census Tract).